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A Survey of Inductive Generalization

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Inductive generalization asserts that what obtains in known instances can be generalized to all. Its original form is enumerative induction, the earliest form of inductive inference, and it has been elaborated in various ways, largely with the goal of extending its reach. Its principal problem is that it supplies no intrinsic notion of strength of support so that one cannot tell if the generalization has weak or strong support .

Accounts of inductive inference can generally be divided into the three families described in my “Little Survey of Induction” (Norton, 2005): inductive generalization, hypothetical induction and probabilistic induction. This document presents a lengthier survey of the accounts of induction found in the first family. [1]

The basic principle of inductive generalization is that what obtains of known instances can be generalized to all. Its best-know form is the venerable induction by simple enumeration, or, more briefly, enumerative induction. We know that some A's are B; from it we infer that all A's are B. As we shall see below, in many logic treatises even within the last century or so, "induction" just meant induction by simple enumeration. It will be the topic of the early sections.

A difficulty of this form of inductive inference is its narrow scope. If the evidence does not present itself as the sentence "A is B," we cannot proceed; if the result to be supported is not of the form "All A's are B." we cannot proceed. There have been many attempts to extend the reach of this form of inductive inference. These attempts have generated a family of forms of inductive inference that I label "inductive generalization." That family will be the subject of the remaining sections. The principle governing this family is the notion that what obtains of known instances can be generalized to all. Hence, we are licensed to infer from instances to their generalizations.

What generates the family is the use of more expansive ways of characterizing and describing instances and their generalizations. Mill's methods allows us to introduce talk of causes. If we have many cases of A’s followed by B’s, we are licensed to infer not just that A’s are always followed by B’s, but that A’s *cause* B’s.

These accounts are still largely limited to the sorts of sentences found in syllogistic logic: “Socrates is mortal.” “All men are mortal.” “Some men are mortal.” etc. Hempel's satisfaction criterion draws on the vastly greater expressive power of first order predicate logic. We can consider sentences like “If someone is a god or descended from a god, then that someone is

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[1] The survey is not exhaustive. I have not discussed nonmonotonic logics. These are logics in which the set of conclusions derivable may be reduced by the addition of new premises, something that cannot happen with traditional deductive logic. In this aspect, nonmonotonic logics behave like inductive inference in which the acquisition of new evidence may lead us to retract earlier generalizations.

immortal." Hempel offered a precise account of what it is to be an instance of such a universal sentence.

In one aspect, however, Mill's methods outstripped those of Hempel. For Mill's methods allow an instance like "These A's are followed by B's." to confirm a generalization that A's cause B's. The crucial novelty is the new term "cause" that did not appear in the instance sentence. Hempel's account does not allow instances to confirm generalizations that contain new terms. This is especially awkward if we are using sentences that describe observations ("This more distantly sourced light is redder.") to confirm theoretical sentences expressed in a different vocabulary ("The universe is expanding."). Glymour's bootstrap approach to confirmation is an attempt to repair this defect. His account allows us to use sentences from the theory under investigation as an interpretive device for introducing new theoretical vocabulary into the sentences connected by confirmation relations. Demonstrative induction makes this interpretive use of theoretical sentences the full content of the induction. Its inductions become "demonstrative," that is deductive

In all these forms of inductive inference, a single problem remains unsolved. Sometimes an instance supplies strong evidential support for an hypothesis. Just one instance may be enough to establish it. On other occasions, many instances may supply only slender support. While we may have strong intuitions about which case is which—somehow we just know—that knowledge does not come from the accounts of inductive generalization. Two inductive generalizations that look essentially similar in form may provide very different strengths of support; nothing in the accounts to be developed below gives us a systematic way to distinguish.

1 Enumerative Induction.

Enumerative induction is the reluctant offspring of traditional syllogistic logic. It exists partly because something like it is pervasive and partly because the framework of syllogistic logic allows it to be defined.

The Pervasiveness of Enumerative Induction

An enumerative induction or, to use its more formal name, an induction by simple enumeration has the form

Some As are B

Therefore, All As are B.

It is the simplest form of inductive inference, even the most ancient ancestor of all inductive inference. But it is not a venerated ancestor. As we shall see, it is routinely approached with hesitation and mistrust. That seems to me to be undeserved.[2] Whatever may be its weaknesses, its use is quite pervasive in ordinary life and in science. Our investigations in all areas would be greatly impoverished if it was denied us. The argument form is typically introduced with somewhat frivolous examples that mask its importance: some squirrels have bushy tails; therefore all squirrels have bushy tails. These examples make it easy to display its form, but mask its importance.

We can find examples in all areas of the sciences. Euclid based his geometry on the postulate that we can always draw a straight line between any two points. Euclid's geometry and his postulate hold true of the space of our common experience and that of much of science, at

[2] It is hard to find anyone in the literature willing to defend enumerative induction. The exception may be Nicod (1930, pp. 203-205)

least to a level of accuracy of measurement that transcends our instruments.[3] How do we know the truth of the postulate, at least at the level of virtually all observational error? We know by enumerative induction. We have massive experience that tells us that we can always connect two points with a straight line. Perhaps we use a ruler of a stretched string to connect the points. Or, recalling that light moves in straight lines, we affirm that a pair of points is connected by a straight line when we sight from one to the other. The light reaching our eye traverses the straight line. We know that some—many—pairs of points can be connected by a straight line. We conclude that all pairs of points can be connected by a straight line. I know of no more fundamental justification for Euclid's postulate.[4]

In fundamental particle physics, many properties of particles can be recovered from the theory. But others are simple known to us a brute facts. We have no account of why the particles have those properties. We just have always found that particular instances do. The simplest example is the electron. It has a mass of 9.1 x 10^{-28}g and a charge of –1.6 x 10^{19} coulombs. We know this because every time we measure reliably we get these results. Similarly we know there are no magnetic monopoles and no paraparticles (particles that superpose integer and non-inter spin) simply because every individual we check has the property of not being a monopole or paraparticle. This is not to say that theories may not emerge that will explain why the electron has its particular charge; and so on. But when and if they do, we will already have had a long history of belief in the generality on the basis of induction by simple enumeration.

We can proceed through the sciences in this way, listing generalizations whose sole support was originally enumerative induction and may still be its sole support today. Thus we

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[3] Einstein's general theory of relativity finally informs us that Euclid's geometry holds only approximately in our space. The deviations are only discernible by the most refined of observations. A ray of starlight grazes the edge of the sun and is momentarily visible in photographic plates taken at the time of a solar eclipse. The photos reveal that the ray is slightly deflected as the light falls into the sun. Only half the deflection is due to that fall. The remainder is due to the slight deviation of the geometry of space from Euclidean near the sun.

[4] Typically induction by simple enumeration is the only way we can establish our most fundamental laws. So Newton was assured that all masses attract one another gravitationally by an inverse square law simply because every pair he could check attracted in this way.

knew through enumerative induction that cats cannot mate with dogs long before we had any account of genes and DNA. We knew that green plants need light to grow before we had accounts of photosynthesis and chlorophyll. We knew that drying or salting preserves food long before we knew of the micro-organisms these processes destroyed. We knew that intentional maggot infestation is an effective way of cleaning a wound and promoting its healing well before we knew that they acted in part by inhibiting bacterial infection. We knew that gold does not tarnish and that all pieces of wood burn before we had an elaborated theory of chemistry. We knew that all samples of diamond are the hardest of minerals before we knew of diamond's unique molecular structure. After every set of numbers we tried conformed to Fermat's last theorem, we knew pretty surely that is was true, long we found a proof. The examples multiply without apparent limit. Science without enumerative induction could not have become what it is today and could not continue.

The Connection to Traditional Syllogistic Logic

When we consult the literature, the description given of enumerative induction (or what amounts to the same thing) may appear diffuse. The Port Royal Logic merely asserts "When from the examination of many particular instances we conclude to a general statement, we have made an induction." (Arnaud, 1662, p. 264) This, or something close to it, is the standard formula. The induction consists in an inference from particulars to a generality.[5] Other accounts

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[5] This standard formula is usually given by first applying it to perfect or complete induction (described below) and then apologetically extending it to the incomplete case. So Keynes (1921, p.274), as part of an historical survey of the use of the term induction, attributes to Aristotle a notion of induction "in which we generalize after the complete enumeration and assertion of *all* particulars which the generalisation embraces." Induction by simple enumeration "approximates" this sense if the enumeration is incomplete "as the number of instances is increased." Eaton (1931, pp.486-87) in related discussion also speaks of induction in terms of the passage from particulars to a generalization. Joseph (1916, p.378) in initiating his survey of the historical use of the term induction allows that: "…and induction meant primarily to Aristotle, proving a proposition to be true universally, by showing empirically that it was true in each particular case or kind of case…" This becomes the familiar (incomplete) enumerative induction as long as not

appear a little more precise in naming a class of individuals. Whately (1827, p336) defines induction as "A kind of argument which infers, respecting a whole class, what has been ascertained respecting one or more individuals of that class."[6] Or, less commonly, the assertion is vaguer. Jevons (1870, p.211) merely informs us that "[I]n induction … we proceed from less general, or even from individual facts, to more general propositions, truths, or, as we shall often call them, Laws of Nature."[7]

Just what is a "particular" and the associated "generalization"? Unless they can be given precise meanings, the standard formula does not give us a clear notion of enumerative induction. Excepting possibly Jevon's formulation, the vagueness is far less than it seems. The context is traditional syllogistic logic, which deals, in its simplest form, with categorical propositions of the form "Some X's are Y's" and "All X's are Y's". (See Box 1 Traditional Syllogistic Logic) This context supplies precise meaning for the terms--or at least precision at the level of traditional syllogistic logic. The labels "particular" and "generalization" in this context refer precisely to these last two assertions, "Some X's are Y's" and "All X's are Y's," respectively. So the standard formula merely tells us that enumerative induction licenses the inference from "Some X's are Y's" to "All X's are Y's." This presence of this context protects the notion from a charge of vagueness, but I will suggest below that the relative poverty of this context is the notion's ultimate undoing.

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all cases are enumerated. This is given later (p. 528) as “Induction by simple enumeration consists in arguing that what is true of several instances of a kind is true universally in that kind.”

[6] Joyce (1936, pp,231-32) asserts "When we draw a universal conclusion [about the class as a whole] after enumerating some members only of a class [and affirming a given attribute of those individuals], we are said to employ **imperfect induction**." (Joyce's boldface) Salmon (1973 , p. 83) defines "induction by enumeration" as "…a conclusion about *all* the members of a class is drawn from premises which refer to *observed* members of that class." (Salmon's emphasis.)

[7] A minority tradition that I will not pursue characterizes induction by simple enumeration as the inference from many instances of a generalization to the obtaining of the *next* instance of that generalization. See for example Fowler (1883, pp. 122-23) Rescher (1964, pp. 284-85). Elsewhere this variant is known as "Example". See Whately (1827, p. 213).

Perfect versus Imperfect Induction.

Because enumerative induction is developed within the context of syllogistic logic, it is most commonly treated there as a defective deductive syllogism. Most developments define "perfect induction," in which we check that each individual has the requisite property and thereby deduce that all do. That is, we learn that "All X's are Y." by checking that every individual X is a Y. This is a valid deductive argument. It can be codified as syllogism, known as the "inductive syllogism."[8] Jevons' (1870, pp. 214-15) example is

> Mercury, Venus, the Earth &c. all move round the sun from West to East.
> Mercury, Venus, the Earth &c. are all the known planets.
> Therefore all the known planets move round the sun from West to East.

This is the perfected form of induction to which all well-meaning enumerative inductions should aspire.

Since it brings deductive certainty akin to the traditional syllogism, perfect induction is praised. We might doubt its value, in comparison to the imperfect form described below, since it merely summarizes what we already know. Jevons leaps to its defense

> It would be a great mistake, however, to suppose that Perfect Induction is in itself useless. Even when the enumeration of objects belonging to any class is complete, and admits of no inference to unexamined objects, the statement of our knowledge in a general proposition is a process of so much importance that we may consider it necessary.

"Imperfect induction" arises when we do not have a complete enumeration of all cases. This is just what is also known as enumerative induction. As the label "imperfect" suggests, it is regarded as lesser and more dangerous. We are to be warned against it. "[I]t must be carefully remembered also that no Imperfect Induction can give a certain conclusion," Jevons (1870, p.

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[8] See Joyce (1936, pp. 228-31) for the general form. As Jevons and Joyce point out, this syllogism extends the standard framework. The minor premise (Mercury, Venus, the Earth &c. are all the known planets.) is not one of the four forms A, E, I or O or a singular proposition S. It does not just assert that Mercury, *etc*. are known planets. It asserts that Mercury, *etc*. are the known planets *and that there are no others known*.

213) somberly counsels. Perhaps we are to eschew its bounty as somehow ill-gotten, for (Jevons, 1874, p. 149) intones with biblical solemnity "We reap where we have never sown."

Since perfect induction is deductive, does it count as an induction at all? The literature generally seems to accept it or at least to allow the use of the term "induction" to pass without comment. The use is legitimate as long as we take induction to refer to an inference from particulars to generalities. Why should it be any less inductive because all particulars happen to be exhausted? It would be otherwise if we regard induction to be necessarily ampliative. So Mill (1872, Bk. 3, Ch. 2, Sect. 1) demands that, in his sense, induction is "an inference from facts known to facts unknown." As a result, perfect induction fails to count as induction.

The confused Aristotelian pedigree

It is commonplace for treatises to trace the origin of enumerative induction to Aristotle. This is understandable since the standard context for discussion of enumerative induction are treatises on traditional logic.[9] (For example Joseph, 1916, Ch. XVIII; Eaton, 1931, Part IV; Joyce, 1936, Ch. XIV) Aristotle is the canonical source for such works. So it is natural to fall back to Aristotle in discussions of enumerative induction; he certainly talks about it. The difficulty, however, is that it is far from clear that Aristotle's notion of induction was the simple notion of enumerative induction described above.

If one is determined to find it in Aristotle, one finds it in two forms; the imperfect and perfect forms described above. The clearest is in the *Prior Analytics* Book II.23 68b15-20 (McKeon, 1941, p.102.). Aristotle offers the "syllogism that springs from induction" as the following (in my synopsis):

All of man, horse, mule *etc*. (C) are long-lived (A).
All of man, horse, mule *etc*. (C) are bile-less (B).
Therefore, all the bile-less (B) are long-lived (A).

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[9] Whately (1827, pp. 207-210) argues energetically against unnamed writers who have portrayed induction as lying outside syllogistic logic. Induction is not an argument form distinct from syllogism, he urges. See also Keynes (1921, p. 274) to find that even writers in the probabilistic tradition feel the need to mention Aristotle.

That much is a deductive fallacy, as imperfect induction always is. Aristotle, however, restores validity to the syllogism by adding the requirement that C is convertible with B. That is:

> All of man, horse, mule *etc*. (C) *and only them* are bile-less (B).

The new demand converts the syllogism into one that is deductively valid. It is now an example of perfect induction. Indeed it is the classic example in the literature. We know all the bile-less are long-lived since we exhaustively checked that it is so for each of the bile-less.

Is imperfect induction to be found in Aristotle? It can be found. The frequently cited example is in *Topics* Book I, Ch. 12 105a10-19 (McKeon, 1941, p. 198)

> …induction is the passage from individuals to universals, e.g. the argument that supposing the skilled pilot is the most effective, and likewise the skilled charioteer, then in general the skilled man is the best at his particular task.

Thus, if one is determined, one can find enumerative induction in Aristotle's writing. It is nearly universally agreed, however, that Aristotle's notion of induction is not simply the one that enters later texts under the banner of enumerative induction. What remains a subject of significant debate is precisely how it differs and how Aristotle himself saw induction figuring into his system of thought. Here I refer the reader to a copious literature. (For an unsystematic entry, see Joseph (1916, Ch. XVIII); Milton (1987, pp. 51-53); McKirahan (1992, pp. 250-57), Barnes (1994, pp. 82-83).) A persistent theme in this literature is that Aristotle understood induction as more than the simple-minded generalization we see in the later versions of enumerative induction. Through induction the soul comes to see a universal embodied in a few particulars. This is far more than routine generalization, the mechanical overwriting of "Some A's are…" with "All A's are…" come what may. It involves the application of a highly selective insight that, in most of Aristotle's examples, does not proceed by routine generalization.[10] This view of induction is perhaps best embodied in the modern literature in Johnson's notion of "intuitive induction" described below.

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[10] Whatever may have been Aristotle's general notions about the role of induction in the methodology of science, he seems not to have worn these notions on his sleeve when it came to the development of the sciences that interested him most. Lennox's (2001) recent study of Aristotle's philosophy of biology does not even contain an entry for "induction" in its index. (I am also grateful to Jim Lennox for helpful discussion in the writing of this section.)

If not already in Aristotle's writing, a fairly straightforward notion of enumerative induction entered into ancient writing. Much later in that tradition, for example, Sextus Empiricus, dismisses induction in his *Outlines of Scepticism* (Book II, XV, 204):[11]

> It is easy, I think, to reject the method of induction. For since by way of it they want to make universals convincing on the basis of particulars, they will do this by surveying either all the particulars or some of them. But if some, the induction will be infirm, it being possible that some of the particulars omitted in the induction should be contrary to the universal; and if all, they will labor at an impossible task, since the particulars are infinite and indeterminate. Thus in either case it results, I think, that induction totters.

While the goal was to dismiss induction, what this passage contains is a quite serviceable and uncomplicated characterization of what we now call imperfect and perfect induction. We arrive at the generalization either by reviewing some instances (imperfect) or all (perfect).[12]

Intuitive Induction

There are a few variant forms of enumerative induction. The most interesting is abstractive or intuitive induction. An intuitive induction proceeds from instances to a generalization, but with a new twist. There must in addition be a felt certainty that the generalization is warranted to the level of certainty. We just know that what is true of one instance will be true of them all. Johnson (1922, p. 192) puts it so:

> The term intuitive is taken to imply felt certainty on the part of the thinker; and it is characteristic of propositions established by means of intuitive induction that an accumulation of instances does not affect the rational certainty of such intuitive generalizations.

At first this idea seems positively strange. Johnson, however, manages to give it some plausibility and even familiarity by leading us through a series of examples. He starts with ones that arise as immediate judgments of experience. Having seen in one case that yellow is further

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[11] Annas and Barnes (1994, p.123)

[12] For a somber appraisal of the weak state of medieval writing on induction, see Milton (1987, p. 57).

from red than orange, we feel a certainty that all our experience of these three colors will replicate the result. Or once we see that one equilateral triangle has equal angles, we are sure than any others we might look at will as well.[13] Johnson proceeds through more elaborate examples, concluding with intuitively apprehended formal mathematical truths. From the one instance (p. 195)

3 times 2 ft. + 3 times 5 ft. = 3 times (2 ft. + 5 ft.)

we apprehend with a felt certainty[14] the generality of the distributive law embodied in the symbolic expression

n times P + n times Q = n times (P + Q)

There is surely much right about this notion of felt certainty in so far as it describes the psychological experience of those engaged in enumerative induction. In some cases we just know that our few instances are unassailable; in others we can never erase our doubt, no matter how often we multiply successful instances. My fear, however, is that these psychological observations become the end of the analysis. If we are to have a serviceable account of induction, we must do better than to say that sometimes enumerative induction works; sometimes it doesn't; and that you will intuitively sense the difference when you see it. We need some objective and explicit way of deciding which cases are which. Otherwise how can we ever hope to adjudicate disputes over evidence? Once we hand over to that intuitive sense of certainty, we may as well forgo any objective account of evidence and its import. You may see in each day's newspaper ever more instances of extraterrestrial alien interference in world affairs and sense it with

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[13] Natural as these examples are, they are easily challenged. It is only our richly tutored understanding of Euclidean geometry that allows the example with the equilateral triangles to succeed. If we draw equilateral triangles in a non-Euclidean geometry, the geometry associated with surfaces of variable curvature, then some will have equal angles and others not. If our experience and education had been of this latter geometry, we would have no sense of certainty upon seeing one equilateral triangle with equal angles. This suggests that the sense of certainty Johnson reports has origins in far more than mere exposure to the one instance. Whatever the induction may be, its structure is far more complex than intuitive induction allows.

[14] As before, this sort of certainty is fragile. It is far weaker for those who have experience of non-distributive lattices, mathematical systems that do not obey the distributive law.

complete certainty. I may just see chance and coincidence and with an equal sense of certainty. Obviously, further explicit considerations are able to decide which of us has the correct warrant.[15]

I remark below that the weakness of enumerative induction is its incompleteness. One could conceive of intuitive induction as an attempt to solve the problem. However the augmentation to intuitive induction is more an acknowledgement of the incompleteness of enumerative induction than a successful way of completing it.

Eduction, Example, Analogy and Things Like It

Further forms of inductive inference are expressible within the framework of syllogistic logic, although little of fundamentally new importance comes with them. So I will pass over them with just a few words. Enumerative induction is the inference from instances--"Some As are B."--to generalities--"All As are B." We may proceed less ambitiously, from instances to instances. "This A is B; therefore that A is B." Johnson's (1924, p.45) illustration is:

Mars is is a solar planet.
The earth is a solar planet.
The earth is inhabited.
Therefore, mars is inhabited.

Johnson (1924, Ch.IV) calls this form of induction "eduction." More commonly it is called "example."[16]

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[15] The same would appear to be true even in examples very similar to Johnson's favorites. We might infer from the one case 2x3=3x2 to the general commutative law for natural numbers, mxn = nxm. But if we happened upon two matrices **A** and **B** that commute under multiplication, we would not infer instantly to a commutative law for matrix multiplication. Notoriously it fails for matrices: $\mathbf{C}\cdot\mathbf{D}\neq\mathbf{D}\cdot\mathbf{C}$ in general. In both examples we have one case of commutation. In one but not the other we feel the certainty. We would not dream of stopping the analysis there and just insisting that we *know* it in one case but not the other. We would explain the difference by introducing our background knowledge of multiplication of natural numbers and of matrices.

[16] Whately (1827, p. 213) writes "…Example, which differs only from it [Induction] in having a singular instead of a general Conclusion…"

Analogy has attracted considerable discussion. In enumerative induction, we generalize over a set of *individuals*. We note many individual instances of As that are B; so we infer that all instances of A will be B. In analogy, we generalize over *properties*. We note (Rescher, 1964, p. 277) that two individuals a and b share properties P, Q, R, *etc*.; so when we learn that the first also has property F, we infer by analogy that the second will too. Schematically, Rescher gives it as

Pa & Qa & Ra & *etc*.

Pb & Qb & Rb & *etc*.

Fa

Therefore Fb

This is sometimes expressed in the language of proportion, arising when we have four terms related by a:b::c:d (read "a is to b as c is to d). For an example, Joseph (1916, p. 533) gives:[17]

> It might be said that the relation of his patients to a doctor is the same as that of his customers to a tradesman, and that therefore as a customer is at liberty to deal at once with rival tradesmen, so a man may put himself at once in the hands of several doctors.

As with enumerative induction, analogy has occasioned much concern;[18] some are obviously inductions by analogy are strong and others weak. To distinguish the cases, one might compare the number of properties the two individuals share (the "positive analogy") with those they do not share (the "negative analogy"). It is probably better not to use a simple head count as a way of gauging the strength of the argument, although some authors cannot resist (Fowler, 1883, p. 224):

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[17] It is by no means clear that this version of analogy is the same as the former. Where the former has two individuals: a and b, this version has four: a, b, c and d. However the latter can be forced into the guise of the former by the usual tricks: Individual a employs services (of a tradesman) and may employ many. Individual b employs services (of a doctor). By analogy, b may also employ many.

[18] In addition to the places cited, see Keynes (1921, Ch. XIX) and Eaton (1931, 550-56) for discussion.

> If, for instance, the phenomenon A is known to resemble the phenomenon B in four points, whereas the known points of difference between them are three, and it is discovered that some new property belongs to A but it is uncertain whether it also belongs to B, the value of the analogical argument that it does belong to B will be represented by 4:3.

Example and analogy are, in principle, distinct; there is no necessity that an inference from an instance to an instance need be governed by analogies. In practice, example is governed routinely by an analogy of properties, so that example and analogy are often not distinguished and are taken to be synonymous. (Joyce, 1936, p. 259 does this.) The tradition is venerable; there is one classic illustration in Aristotle (*Prior Analytics*, Bk. II, Ch. 24) that is cited as the canonical illustration for what is later reported as both example and analogy. In Joyce's gloss, the illustration is:

> The war of the Thebans against the Phocians proved calamitous.
> War between Athens and Thebes resembles the war of the Thebans with the Phocians in being a war with a neighboring state.
> War between Athens and Thebes will prove calamitous.

Finally there is another category of inductive inference that appears to belong to this tradition. It has many variant forms. As "proportional induction" (Black, 1967, p. 169) or "statistical inductive generalization" (Maher, 1998, p.756), it licenses an inference from the frequency of some characteristic in a sample population to that frequency in the whole population. The inference scheme may require specification of the procedure used to arrive at the sample and restrict the derived population frequency to an interval or by vague qualifiers ("approximately").

The connection to the traditional induction literature is superficial. These inferences cannot be stated in the standard language of syllogistic logic, which does not talk of frequencies. So they were never part of that literature. They are prototypes of standard inferences in the literature of statistics, or, perhaps stripped down versions of standard statistical procedures. They have little value outside that statistical literature. If the procedure used to arrive at the sample is known, the statistical literature provides elaborate and powerful methods for locating the corresponding population frequency, typically expressed as a probability distribution over possible values. In serious applications, we have no good reason to truncate these results. If the procedure is unknown, those techniques generally cannot be applied; but correspondingly one would be loath to carry out the proportional induction.[19]

[19] The commercial may assure us that 3 of 4 dentists surveyed recommend Oh-So-Brite toothpaste. But since we have serious doubts over how the sample of dentists was selected for the survey, we are loath to infer that about 75% of dentists overall would concur.

Box 1: Traditional Syllogistic Logic

Traditional syllogistic logic develops the logical relations between propositions of such forms as "All X's are Y's." It forms the background context for enumerative induction.

At its introductory level, traditional syllogistic logic deals with the logical relations between categorical propositions. These are propositions like "All squirrels have bushy tails." They have a subject term (squirrels) and a predicate term (bushy tails). The four forms traditionally dealt with are:

(A) All X's are Y's.

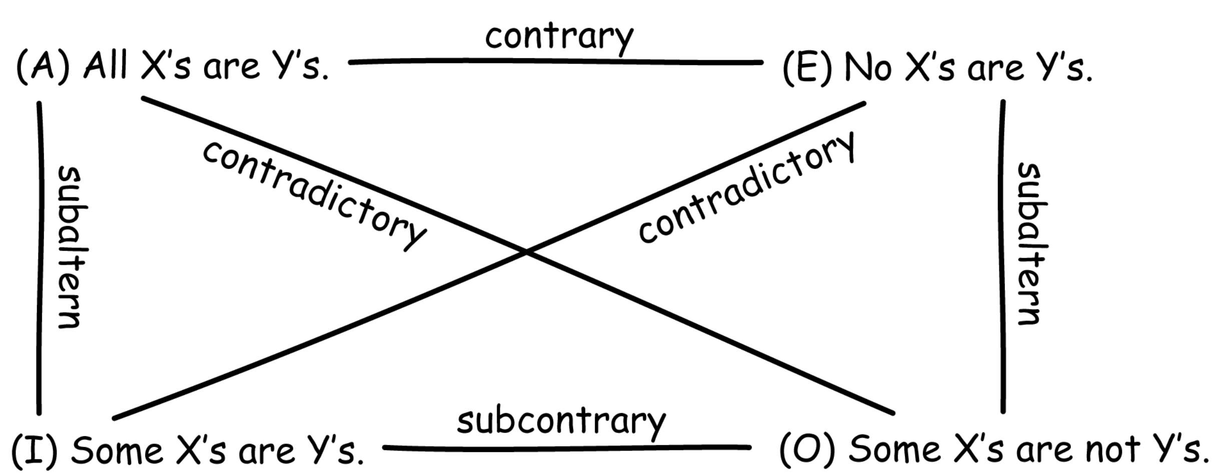
(I) Some X's are Y's.

(E) No X's are Y's.

(O) Some X's are not Y's.

The first two, A and I, are the universal and particular affirmative; the A and I are read from ***AffIrmo***, the Latin for "I affirm." The last two, E and O, are the universal and particular negation; the E and O are read from ***nEgO***, the Latin for "I deny."

Their relations are traditionally represented in the square of opposition



We read from the square that A and O are contradictories, for example.

A typical categorical syllogism is

All animals are mortal.

All men are animals.

Therefore, all men are mortal.

It consists in the deduction of a categorical proposition from two others by the sharing of a term, the so-called "middle" term ("M"), which in this case is "animals." The minor term ("S") is "men" and the major term ("P") is "mortal."

One of the first burdens of the logic of syllogisms is the determination of which are the valid syllogisms of all the ones we might write down. To do this, the logician must check both "figure" and "mood". The figure determines how the middle term is distributed between the two premises. There are four figures:

Figure 1	Figure 2	Figure 3	Figure 4
M - P	P - M	M - P	P - M
S - M	S - M	M - S	M - S
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S - P	S - P	S - P	S - P

In the above syllogism, the middle term is distributed according to Figure 1. The mood is determined by which of the four forms A, I, E and O are used. All three propositions in this syllogism are universal affirmatives: A-A-A.

Mediaeval scholarship reduced the discerning of valid from invalid syllogisms to the memorization of a Latin mnemonic:

Barabara, Celarent, Darii, Ferioque prioris;

Cesare, Camestres, Festino, Baroco secundae;

Tertia *Darapti, Disamis, Datisi, Felapton,*

Bocardo, Ferison habet. Quarta insuper addit

Bramantip, Camenes, Dimaris, Fesapo, Fresison.

We read from the vowels of the italicized words which are the valid moods; we read from the markers *prioris* (first), *secundae* (second), *tertia* (third) and *quarta* (fourth) which are the associated figures. So the A-A-A syllogism is B**A**rb**A**b**A** of the first form. From ***BOcArdO*****,** we read that O-A-O is a valid syllogism of the third (*tertia*) figure:

Some A's are not B's

All A's are C's

Therefore, some C's are not B's.

The system is readily extended to deal with singular propositions, such as (famously) "Socrates is Mortal." They can be divided into affirmative singular propositions ("S"), "[individual] X is Y"; and negative singular propositions ("N"), "[individual] X is not Y." Then we have additional valid syllogisms of ASS and ESN for Figure 1; ANN and ESN for Figure 2; and SSI and NSO for Figure 3.

2 Complaints about Enumerative Induction.

Enumerative induction has been unfairly judged inadequate because it may fail; and it has been fairly judged so, because the characterization spawned by syllogistic logic is too weak. There is no consensus on how to repair it.

The Vilification of Enumerative Induction

Enumerative induction has become the flatulence of philosophy of science. Everyone has it; everyone does it; and everyone apologizes for it. It will not go away and cannot go away without compromising the vital function of science. The most celebrated jibe is Francis Bacon's (1620, First Book, §105)

> 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.

Reading earlier in the work, we see that Bacon's reservations about enumerative induction are coupled with a denunciation of its practitioners: they are slothful and intellectually dishonest. So, earlier (§19), Bacon described the then current practice of enumerative induction as one that "…hurries on rapidly from the senses and particulars to the most general axioms…" He then lamented (§20) that this haste is natural to the understanding "when left to itself…[;] for the mind is fond of starting off to generalities, that it may avoid labor, and after dwelling a little on the subject is fatigued by experiment." The result is poorly grounded science, defended by sophistry (§25):

> The axioms now in use are derived from a scanty handful, as it were, of experience, and a few particulars of frequent occurrence, whence they are of much the same dimensions or extent as their origin. And if any neglected or unknown instance occurs, the axiom is saved by some frivolous distinction, when it would be more consistent with truth to amend it.

Is it just enumerative induction that has aroused his fury. Or is it enumerative induction as he believes it is practiced? Or does Bacon hold enumerative induction to be so flawed that no responsible practice is possible? The last would seem to be the case. For Bacon's remedy is not merely to call for more care and honesty; it is to replace enumerative induction by his new method.

In any case, the idea of enumerative induction as defective has maintained a persistent place in the literature. The Port Royal Logic (Arnaud, 1662, p. 264) includes a discussion of enumerative induction in a chapter "Sophisms: the Different Ways of Reasoning Incorrectly." Jevon's (1874, p. 149), having warned us of enumerative induction that "[W]e reap where we have never sown," proceeds with an elaborate denunciation:

> …I venture to assert that it never makes any real addition to our knowledge, in the meaning of the expression sometimes accepted. As in other cases of inference, it merely unfolds the information contained in past observations; it merely renders explicit what was implicit in previous experience. It transmutes but does not create knowledge.

A more measured Johnson (1924, Ch. II) captures it best in my view, in labeling enumerative induction "problematic induction." That represents at once that enumerative induction does present a problem but that it is a problem worthy of solution.

Counterexamples to Enumerative Induction…

Why is this most common form of inductive inference so vilified? The most common complaint against enumerative induction is that it admits counterexamples. This concern is ancient; it seems to be the essence of Sextus Empiricus' complaint against imperfect induction quoted above. ("…the induction will be infirm, it being possible that some of the particulars omitted in the induction should be contrary to the universal;…") In almost every era the counterexamples have been duly paraded. Indeed the goal of the parade is often to display the most striking exception drawn from the latest science. The message implicit in the examples--whether intended or otherwise--is not just that enumerative induction may fail. It is that the failure would obscure whatever the latest breakthrough is in science; or it would have, had we been so foolish as to rely solely on enumerative induction. I have collected a sampling of these counterexamples in Box 3: All Swans Are Not While.

…are Not a Telling Objection

It is now hard to understand why these counterexamples were regarded as fatal defects of enumerative induction. Certainly many of them merely illustrate an enumerative induction conducted with imprudent haste. Obviously, as Arnaud suggests in the Box below, the occasional failure of a skilled physician is no warrant for the conclusion that "medicine is completely useless and is a craft of charlatans." Others are quite forthright. An enumerative induction was a bad induction if a counterexample is subsequently found. Mill (1872, p. 205) is clear:

> That all swans are white could not have been a good induction, since the conclusion has turned out erroneous.

Mill's expectation for a good induction is that its conclusion must be true if based on true premises. He continues to elucidate the failure of the swans induction.

> The experience, however, on which the conclusion rested was genuine. From the earliest records, the testimony of the inhabitants of the known world was unanimous on the point. The uniform experience, therefore, of the inhabitants of the known world, agreeing in a common result, without one known instance of deviation from that result, is not always sufficient to establish a general conclusion.

If we understand inductive inference to be ampliative inference, then no inductive inference can supply a guarantee of truth of a conclusion on the strength of the truth of the premises. That is the function of deductive inference. Ampliative inference seeks to go beyond. That we find conclusions of some inductive inference scheme failing from time to time is merely a manifestation of this greater reach. Indeed failures are to be expected and were none ever found our suspicion would properly be raised. A false conclusion on true premises shows that a *deduction* was not good; it is fatal. A false conclusion on true premises does not show the same for an induction; it is to be expected.

Beneath many of these objections is a tacit supposition that a good induction should supply conclusion not just of some likelihood but of sufficiently great likelihood that we can accept them without qualification. If the point of the parade of counterexamples is that enumerative induction does not meet this desideratum, they still fail. Even if an inductive scheme delivers conclusions of nearly complete certainty, as long as the certainty is only *nearly* complete, counterexamples are possible and, as we repeat our inductions, to be expected. The only difference is that they should arise less frequently. However this concern of how likely the conclusion is, does come close to what I think is the real problem of enumerative induction, to be explained shortly.

Motives

We might wonder whether this critical literature is driven by somewhat transparent motives. Typically, a critic of enumerative induction has another scheme of inductive inference waiting in the wings and enumerative induction is identified as its chief competitor. This is certainly the case with Bacon, who, as we shall see below, proceeds to develop his method of tables as a tonic for the defects of enumerative induction. Mill similarly proceeds to his very similar eliminative methods. These methods clearly aspire to deliver greater reliability than mere enumerative induction. Similarly Arnauld writes in a Cartesian tradition in criticizing enumerative induction. For example, we do not base the certainty of geometry by examination of instances (1662, p.264)

> I should perhaps never have been led to consider the nature of a triangle had I not seen a particular triangle which furnished me the occasion for thinking about the nature of triangularity. And yet what leads me to conclude with certainty that the area of any triangle is equal to one half the product of the base and the altitude is not an examination of every triangle--that would be impossible--but simply a consideration of what is contained in the idea of a triangle I have in my mind.

The real guarantor of the certainty is the clarity and distinctness of the idea. That is the first of what Arnaud (p.323) later calls "Some important axioms which can serve as the basis for great truths." His Axiom 1 is "All that is contained in the clear and distinct idea of a thing can be truly affirmed of the idea of that thing."

Finally, we saw above that Jevons (1874, p. 149) complained that enumerative induction ("imperfect induction") "never makes any real addition to our knowledge." This appraisal is profoundly pessimistic. Are we to allow this of Euclid's geometry, which is founded so completely on enumerative induction? Surely not. Perhaps his pessimism stems from a desire to make attractive his own candidate for the right account: hypothetico-deductivism, founded on Laplace's inverse probabilities. Unlike Arnaud, Jevons cannot tout his candidate as supplying certainty; it merely delivers probability. But that is better than the mere summary Jevons sees delivered by enumerative induction.

The Real Flaw in Enumerative Induction: its Incompleteness

There is no question that there is something wrong with enumerative induction. The mere fact that it can fail to yield true conclusions is not it. The real flaw is closely related. Take two formally identical enumerative inductions--that is, two enumerative inductions that look identical as long as we do not concern ourselves with what the terms mean. They can have very different successes in so far as the induction might give us the highest confidence in the conclusion in one case but only the most slender confidence in the other.

Mill (1872, pp. 205-206) gives a statement of this problem upon which I cannot improve:

> …there are cases in which we reckon with the most unfailing confidence upon uniformity, and other cases in which we do not count upon it at all. In some we feel complete assurance that the future will resemble the past, the unknown be precisely similar to the known. In others, however invariable may be the result obtained from the instances which have been observed, we draw from them no more than a very feeble presumption that the like result will hold in all other cases. That a straight line is the shortest distance between two points we do not doubt to be true even in the region of the fixed stars.[20] When a chemist announces the existence and properties of a newly-discovered substance, if we confide in his accuracy, we feel assured that the conclusions he has arrived at will hold universally, though the induction be founded but on a single instance. We do not withhold our assent, waiting for a repetition of the experiment; or if we do, it is from a doubt whether the one experiment was properly made, not whether if properly made it would be conclusive. Here, then, is a general law of nature inferred without hesitation from a single instance, a universal proposition from a singular one. Now mark another case, and contrast it with this. Not all the instances which have been observed since the beginning of the world in support of the general proposition that all crows are black

[20] Mill's footnote: "In strictness, wherever the present constitution of space exists, which we have ample reason to believe that it does in the region of the fixed stars." Mill was exactly right, if not presciently so, to allow that this result is an empirical one to be learned by induction. The notions of straightness (loosely, sameness of direction) and being shortest are not the same. One can contrive geometries in which straight lines are not the shortest and conversely.

would be deemed a sufficient presumption of the truth of the proposition to outweigh the testimony of one unexceptionable witness who should affirm that, in some region of the earth not fully explored, he had caught and examined a crow and found it to be gray.

Why is a single instance, in some cases, sufficient for a complete induction, while in others, myriads of concurring instances, without a single exception known or presumed, go such a very little way towards establishing a universal proposition? *Whoever can answer this question knows more of the philosophy of logic than the wisest of the ancients and has solved the problem of induction*. [my emphasis]

Something quite essential is missing from the standard characterization of enumerative induction. We are to infer from "Some As are B." to "All As are B." But we may not always make the inference in the same way. Sometimes we make it with the highest of confidence; in others, we do it with very little confidence.

Another Example of the Flaw

Our premise is

"Our solar system has nine planets"

From it we deduce

"Our solar system has more than one planet."

By enumerative induction, we would arrive at the conclusion

"All solar systems have nine planets."

Few could feel comfortable with this application of enumerative induction. In so far as we have any warrant to the last conclusion, it is weak. Now take a case that is formally identical. Our premise is:

"This uncharged fluorine atom is orbited by nine electrons."

From it we deduce

"This uncharged fluorine atom is orbited by more than one electron."

By enumerative induction, we arrive at the conclusion

"All uncharged fluorine atoms are orbited by nine electrons."

The license for the last inductive conclusion is significantly stronger.

In both cases the deductions are equally secure. But the security of the inductions vary significantly. The reader will have little doubt over why there is such a difference between the inductions. We know a great deal about the properties of atoms and solar systems. We know that the number of electrons orbiting an uncharged atom is equal to its atomic number and thus must be the same for all atoms of the same element. So, while we may never have checked fluorine directly, we do expect that if one uncharged fluorine atom has nine electrons, then they all will. We know of nothing that restricts the number of planets orbiting a star. That our sun has nine planets is happenstance and nothing I know in celestial mechanics forces the same result onto other solar systems.

Is it somehow unfair to contaminate the enumerative induction with outside knowledge about atoms and suns? I do not think so. The same contamination has no effect on the deductions. Whether we recall or forget all we know about atoms and suns, the deductions succeed with equal force. Should we nonetheless restrict our consideration to enumerative induction in a context that does eschew all further knowledge? Perhaps that would be an interesting technical exercise. But it would not resemble the inductive practice of science or even of everyday life. Those inductions are carried out with a full awareness of relevant background knowledge. Since that background knowledge can make a difference, it would be perverse to forgo it. If an account of enumerative induction is to be relevant to our practices, it cannot be an account of an idealized induction that somehow eschews that further knowledge.

The familiar schema of deductive inference are complete in the sense that nothing further has to be added to determine whether the truth of the premises assures the truth of the conclusion. The flaw in enumerative induction is that we have no assurance of the strength of the induction unless we bring in background information in some way that is not specified in the schema. Only then do we know if our enumerative induction supplies strong or weak support or perhaps virtually none at all.

The easy diagnosis is to suppose that the constraint of syllogistic logic has undone enumerative induction. If enumerative induction is to be defined within the context of syllogistic logic, then it is restricted to the use of simple sentence forms like "Some As are B." and "All As are B." There is certainly something right about that diagnosis. However one would be wrong to suppose that a simple cure lies in an expansion of the sentence forms permitted, so that the original schema could be made wholly reliable by merely adding a further restriction to the types of sentences employed. As we shall see, there have been efforts along these lines, but they have met with universal assent. The notion of intuitive induction above tries to solve the problem by a different method. In effect it asserts that there is a subset of enumerative inductions that are the truly legitimate ones and that we just know them when we see them. I have already explained my discomfort with this approach.

My own resolution of this problem comes in the material theory of induction (Norton 2003, 3005). I urge that there are no universal inductive inference schemes. Inductive inferences are licensed by facts and, since none is true universally, there is no universal logic of induction. The inductions we identify as enumerative are typically warranted by different facts, so that their similarity is superficial. These differing facts—or their absence—is what gives the inductions their differing strengths. The inference from one fluorine atom to all is warranted by a fact concerning the uniformity of electron configuration of uncharged atoms of each element. The security of that fact makes the inference strong. There is no corresponding fact concerning the number of planets that orbit stars. Hence the superficially similar induction on stars and planets fails in that it has no underlying warranting fact.

The upshot

Enumerative induction seems never to have been intended by Aristotle in the form that it took hold in the logic literature. It took that form largely because it could be defined using the simple vocabulary of traditional syllogistic logic and because it approximated something that was and will continue to be used in science and everyday life. Virtually everyone writing on enumerative induction senses that there is something missing. The logicians contrast its unreliability to the sterile but deductively secure "perfect induction." The more philosophically minded seek to find the extra secret ingredient that would repair it, such as the felt certainty of Johnson's intuitive induction. None of these repairs have met with universal acceptance. Enumerative induction then poses a problem, as does the archetype of each of the three families of induction that I describe. There is clearly something right and important about it. It does embody a sense in which we can make natural generalizations. But it needs to be repaired by something a little more systematic than merely tacking on an extra condition.

Box 2: All Swans Are Not White

A fixture in the literature on enumerative induction is a display of counterexamples. They are typically chosen as illustrations of remarkable obtuseness of proponents of enumerative induction or of their overlooking major advances in science. Whether intended on not, they suggest that proponents of enumerative induction are foolish or benighted.

Sextus Empiricus, in the passage quoted above, already enunciated in antiquity[21] the simple objection that enumerative induction is infirm since some unchecked instance may contradict the conclusion. Elsewhere in the same work (Book II, 195), he contributed to the literature of surprising counterexamples. Horses, hogs, humans and hordes of other animals that spring naturally to mind all move their lower jaw. The enumerative induction to all animals is instinctive, but…:[22]

> Now this proposition--Everything human is an animal--is confirmed inductively from the particulars; for from the fact that Socrates, being human, is also an animal, and similarly with Plato and Dio and each of the particulars, it is thought possible to affirm that everything human is an animal. For even if one of the particulars were to appear contrary to the others, the universal proposition is not sound--e.g. since most animals move their lower jaw but the crocodile alone moves its upper jaw, the proposition 'Every animal moves its lower jaw' is not true.

Francis Bacon's assault on enumerative induction is rich in confident, polemical denunciation, but rather lean in concrete example. There is one quite memorable counterexample

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[21] See Franklin (2001, pp. 201-201) for Stoic examples that draw heavily on reports of freaks "…the man in Alexandria half a cubit high with a colossal head that could be beaten with a hammer, who used to be exhibited by the embalmers…" (Reported by the Eupicurean, Philodemus, *On Signs*.)

[22] Annas and Barnes (1994, p. 120)

that illustrates not so much the failure of enumerative induction but of the frailty of the human understanding that he elsewhere described as supporting overly hasty enumerative induction. It is always ready to add more support to propositions once laid down and slow to seek counterexamples.

> It was well answered by him [Footnote: Diagoras] who was shown in a temple the votive tablets suspended by such as had escaped the peril of shipwreck, and was pressed as to whether he would then recognize the power of the gods, by an inquiry. But where are those who have perished in spite of their vows? (Bacon, 1620, Book 1, §46)

Like Bacon, the *Port Royal Logic* complains forcefully that enumerative induction fails if conducted rashly and founded on too few instances. Arnaud (1662, pp. 284-85) writes:

> There are diseases which escape the detection of the most skilled physicians, and prescribed cures are not always successful. Rash minds, therefore, conclude that medicine is completely useless and is a craft of charlatans. There are women of easy virtue; this is warrant enough for the jealous to entertain unjust suspicions against the most upright women and for irresponsible writers to condemn all women. Great vices are often concealed beneath a façade of piety; hence, infers the freethinker, all devotion is but hypocrisy. Some things are difficult and obscure, and often we are grossly deceived. Therefore, say the ancient and modern Pyrrhonists, all things are obscure and uncertain--we cannot know the truth of anything with certainty.

The litany continues through improper generalization of some human failure or of a weakness of reason to the *finale*:

> From a few repeated actions we conclude to a custom; from three of four faults, a habit. What happens once a month or once every year is said to occur every day, every hour, even every moment. Men take so little pains to keep within the limits of truth and justice!

Where Bacon was not so clear, the *Port Royal Logic* makes clear that it has little sympathy for an enumerative induction even when conducted thoroughly and based on many instances, for it often leads to error. Arnaud (1662, pp. 264-65) continues:[23]

> …We cite but one noteworthy example.
>
> Until recently all philosophers held two things indubitable truths: (a) A tight-fitting piston cannot be removed without bursting the pump; (b) suction pumps can raise water to any height. The philosophers came to hold fast to these generalizations in virtue of inductions based on a great many experiments. But new experiments have disclosed that both generalizations are false; the piston of a suction pump, however tight fitting, may be withdrawn if we use a force equal to the weight of a column of water whose cross section is the diameter of the pump and whose height is more that thirty feet; but a suction pump cannot raise water to a height of more than thirty-two or thirty-three feet.

Arnaud is referring here to what modern readers would call the discovery that a vacuum is possible after all; removing a piston without collapsing the cylinder forms a vacuum. Arnaud artfully avoids reporting the new results that way. With his Cartesian inclinations, presumably he does not want to challenge Descartes' deduction that space and matter are identical so that vacua--space without matter--really are impossible.

Jevons liked also to draw on recent science for illustrations of the failure of enumerative induction. One example came from astronomy. (Jevons, 1870, p.215)

> …there was a widespread and unbroken induction tending to show that all the Satellites in the planetary system went in one uniform direction round their planets. Nevertheless the Satellites of Uranus when discovered were found to move in a

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[23] Arnaud (1662, p.319) later gives another example drawn from the then recent breakthroughs in fluid dynamics:

> Not long ago we were quite certain that were water poured into a dish one end of which was much narrower than the other, the water would be at the same depth everywhere in the dish. Our certainty was derived from observation. Still, quite recently it was discovered that if one end of an irregularly shaped dish is very narrow, the water stands higher there than in the wider end.

> *retrograde* direction, or in an opposite direction to all Satellites previously known, and the same peculiarity attaches to the Satellites of Neptune more lately discovered.

Other examples came from chemistry. (Jevons, 1874, p. 238)

> Lavoisier, when laying the foundations of chemistry, met with so many instances tending to show the existence of oxygen in all acids, that he adopted a general conclusion to that effect, and devised the name oxygen accordingly. He entertained no appreciable doubt that the acid existing in sea salt also contained oxygen;[24] yet subsequent experience falsified his expectations.

This unreliability of natural patterns in chemistry was reinforced by the observation that many of the then newly discovered elements had simply been mistaken for others, until their distinct identity was discovered. (Jevons, 1870, p. 224; 1874, p. 237) So the recently discovered caesium and rubidium were mistaken for the chemically similar potassium. Other chemically similar elements were confused: tantalum and niobium, selenium and sulfur, and so on. His goal apparently was to shake the faith we may have in continuing any discovered regularity. Substances we presume of the same kind may later turn out not to be.

Yet more examples came from mathematics. Fermat believed, Jevons (1870, p. 222) reports, that $2^{2^x}+1$ is always prime, for any natural number x, presumably from the weight of positive instances. The regularity fails when x=4294967297, with the resulting number divisible by 641. Similarly (Jevons, 1870, p. 221; see also 1874, p. 230) "at one time it was believed"[25] that the formula x^2+x+41 yields primes, since it certainly does so for x=1, 2, 3 and many more values. "This was believed solely on the ground of trial and experience." It fails finally for x=40, for which the formula gives 40x40+40 +41 = 41x40 + 41 = 41x41 and that is not a prime number!

Jevons (1874, pp. 229-30) also invented simpler mathematical examples that a reader could easily see through. 5, 15, 35, 45, 65, 95 all end in the digit 5 and are divisible by 5. Does that allow us to infer that all numbers ending in the digit 5 are divisible by 5? The result is true

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[24] Jevon's footnote: "Lavoisier's *Chemistry*, translated by Kerr. 3rd ed., pp. 114, 121, 123."

[25] The failure at x=40 is so obvious that it is hard to see how this formula, attributed to Euler, could ever have caused serious confusion.

but cannot be founded on the enumerative induction. Otherwise, we might note that 7, 17, 37, 47, 67, 97 all end in the digit 7 and are primes. We would then infer to the falsity that all numbers ending in the digit 7 are prime.

Mill (1872, p.205) reports the venerable counterexample: all swans are not white. Black swans were found in Western Australia.[26] This counterexample has become such a familiar cliché that one rarely needs to complete the sentence beginning with the words "black swans…" for one's point to be all too apparent. Presumably its novelty had not yet dissipated when Mill invoked it, for he could readily classify it with other possible failures of enumerative induction that would be quite shocking were they now affirmed:

> But let us now turn to an instance apparently not very dissimilar to this [case of the swans]. Mankind were wrong, it seems, in concluding that all swans were white; are we also wrong when we conclude that all men's heads grow above their shoulders and never below, in spite of the conflicting testimony of the naturalist Pliny? As there were black swans, though civilized people had existed for three thousand years on the earth without meeting them, may there not also be "men whose heads do grow beneath their shoulders," notwithstanding a rather less perfect unanimity of negative testimony from observers? Most persons would answer, No…

I have my own contribution to this literature of counterexamples.[27] Fundamental theories of physics have long treated left and right as equivalent. If some system or process is possible, then so is its mirror image. If we encounter the left handed process and not the right handed mirror image, that is mere happenstance. *Apriori* there seems no reason to expect such even

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[26] The Western Australian government website dates the first report of these black swans to January 1697 by the Dutch navigator Vlamingh. Mill's mention of it comes a century and a half later.

[27] Let us not forget two commonly overlooked counterexamples. We routinely accept that the sun will rise tomorrow and that any account of induction must somehow deliver that result--that is unless we live beyond the Arctic or Antarctic circles. Then, as winter approaches, we eventually encounter a sunset not followed by a sunrise, at least for months. (I am grateful to Eric Angner, who is Swedish, for reminding me of this!) Also, everything expands on being heated--except ice, that most familiar of counterexamples.

handedness in the fundamentals of nature. The human form, for example, is only superficially the same as its mirror image; we all have hearts on the left side. What led us to believe in the equivalence of left and right was an enumerative induction. As we uncovered more and more of the laws of fundamental physics, each new law respected this equivalence. There was no deeper justification. By the 1950s, with not a single counterexample known, the expectation of this equivalence was massive. Thus, even with suspicions set in motion in 1955, it came as a shock when processes governed by the so-called "weak" force of particle physics were found to treat left and right differently. This is the famous violation of parity in particle physics. Lee and Yang found no one had checked whether parity was violated by weak interactions. Experiments soon revealed the violation, such as Wu's experiments on the beta decay of radioactive cobalt.[28]

My award for the best contribution to this genre, however, is less elevated scientifically and goes to Russell (1912, pp. 97-98):

> Domestic animals expect food when they see the person who usually feeds them. We know that all these rather crude expectations of uniformity are liable to be misleading. The man who has fed the chickens every day throughout its life at last wrings its neck instead, showing that more refined views as to the uniformity of nature would have been useful to the chicken.

[28] For a brief account, see Segrè (1980, pp. 258-63).

3 Inferring to Causes: Bacon's Tables and Mill's Methods

According to Mill's methods, we identify the cause of an effect by checking for what is present in every instance in which the effect arises; for the removal of which circumstances assures the elimination of the effect; and for which circumstance grows and diminishes in concert with the effect. These simple methods continue to be of enormous value in science.

The Common Sense Inference to Causes

If science depended only on enumerative induction, its power to learn would not be great. For about four centuries the literature has acknowledged another form of inductive inference that promises to take us beyond the products of enumerative induction and, in a quite methodical way, to fundamental laws and causes. The strategies it embodies are not esoteric. They are a part of reflective common sense. For example, after each summer's day at the beach, I notice my skin is burnt. I conclude that this will happen after all days spent at the beach. That is just enumerative induction and it does not really tell us enough. What I really want to know is why I burned. What caused it? Knowing that would give me some hope of preventing burns in the future. Enumerative induction on the instances of my burning cannot answer. But the answer is easy to find. Even the most casual of reflection leaves me certain that sunlight is the cause. How do I know this? I know that the cause is present when I am at the beach. But is it the sun, the open air, the wind, the sand or what? I never burn at night, even if I am in the open air or on the sand, so it is not the open air or sand. I won't burn wherever I keep myself covered; and I don't burn under my swimsuit or on my head if I wear a hat. Is that because those parts are shielded from the sun or from the wind? It is not the wind, since I can still burn on calm days; or I can avoid a burn on windy days by staying in the shade. So the sun is the cause. If there were any doubts, I might notice that the severity of a burn increases with the length of exposure to sunlight.

These sorts of inferences are a standard part of the repertoire of common sense. We routinely use them without explicitly formulating the principles that govern them. We can identify a cause by looking for what is always present when the effect arises; by checking that the effect is eradicated when we selectively suppress just our favored candidate; and by noting that the magnitude of the effect increases with that of the cause. These simple notions are invaluable in ordinary life and they are of comparable use in science. They underwrite the single most important method in science, the controlled study. We wish to determine whether some agent has a particular effect. Does this diet predispose us to heart disease? Does this drug cure that ailment? We contrive two populations that, as far as we are able, differ only in the agent of interest. The test group has it; the control group does not. We then attribute systematic differences between the two groups to the agent. If the test group sickens but not the control group, we blame the difference between the groups. If the test group is cured but not the control group, we credit the drug given only to the test group.

The design of the controlled study is just one very obvious manifestation of the use of these principles in science. There use is pervasive. See Box 3: Cholera and Gravitational Waves.

Mill's Methods of Experimental Inquiry

There have been numerous efforts to codify the principles that underlie this common sense approach to revealing causes. The best-known codification is by John Stuart Mill in his *System of Logic* (1872, Book III, Ch. 7). Mill's methods have now fallen from favor as a topic in the literature. If they are discussed, it is usually with an awkward admission of their supposed naivety or as part of some historical survey.[29] From the time of Mill's writing to the mid twentieth century, however, an account of Mill's methods was a staple both of logic books that wished to cover induction and also of accounts of what was then called "scientific method."

Mill describes "Four Methods of Experimental Inquiry," captured in five canons.

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[29] There are exceptions: Salmon (1984, Ch. 4).

Method of Agreement (First canon):

"If two or more instances of the phenomenon under investigation have only one circumstance in common, the circumstance in which alone all the instances agree is the cause (or effect) of the given phenomenon."

The content of this and the following methods is readily displayed in abstract examples used by Mill. He represented possible causes by upper case letters A, B, C, … and their possible effects by lower case letters a, b, c, … He imagined, for example, "…that A is tried along B and C, and that the effect is abc." I will write this in compact form as "ABCabc". To illustrate the method of agreement, Mill supposed we have

ABCabc

ADEade

______________________(agreement)

A is the cause of a.

We infer to A as the cause because all instances agree in A in the antecedents that yield the effect a. So I infer to sunlight (A) as the cause of my burn (a) because all instances with the burn agree in exposure to sunlight.

Method of Difference (Second canon):

"If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have every circumstance in common save one, that one occurring only in the former; the circumstance in which alone the two instances differ is the effect, or the cause, or an indispensable part of the cause, of the phenomenon."

In comparable compact notation, we can represent Mill's illustration as:[30]

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[30] Mill (1872, p. 256) characterized these two methods as methods of elimination. For completeness, I mention that there is no close connection with what is elsewhere called "eliminative induction." Mill drew the term "elimination" from "…the theory of equations [where it] denote[s] the process by which one after another of the elements of a question is excluded, and the solution made to depend on the relation between the remaining elements only." Thus in Mill's example for the method of agreement, B, C, D and E are eliminated; in the method difference, B and C are eliminated.

ABCabc

BCbc

___________________(difference)

A is the cause of a.

Again I infer to sunlight as the cause of my burn since the burn does not arise in case I eliminate the sunlight.

Joint Method of Agreement and Difference (Third canon):

> "If two or more instances in which the phenomenon occurs have only one circumstance in common, while two or more instances in which it does not occur have nothing in common save the absence of that circumstance, the circumstance in which alone the two sets of instances differ is the effect, or the cause, or an indispensable part of the cause, of the phenomenon."

This method is a variant of the method of difference. In the method of difference, we infer to A as the cause of a by noticing that the only change in our instance needed to eradicate a is to eliminate A. Cases may arise in which we cannot locate instances in which just the possible cause A of interest is eradicated. Nonetheless we may notice in a mix of heterogeneous instances that whenever A is present so is a; whenever A is absent so is a. The joint method of agreement and difference then licenses the conclusion that A is the cause of a. A schematic example (not given by Mill) is:

ABCabc

ADEade

FGfg

XYxy

___________________(joint method)

A is the cause of a.

To illustrate, Mill seeks the cause of Iceland spar's power of double refraction on light. We suspect its crystalline nature, but the method of difference cannot help us, since we cannot find Iceland spar that is not crystalline. It is an essential property. However we do notice that all of the many substances that are doubly refracting are crystalline; and all non-crystalline substances lack the property. So the joint method of agreement and difference allows us to ascribe Iceland spars power of double refraction to its crystalline nature.

Method of Residues (Fourth canon):

"Subduct from any phenomenon such part as is known by previous inductions to be the effect of certain antecedents, and the residue of the phenomenon is the effect of the remaining antecedents."

This method allows us to interpret new instances in the light of results gleaned from earlier applications of the methods to other instances. Mill's example is

ABCabc

A causes a (from earlier applications)

B causes b (from earlier applications)

(residues)

C is the cause of c.

Method of Concomitant Variations (Fifth canon):

"Whatever phenomenon varies in any manner whenever another phenomenon varies in some particular manner, is either a cause or an effect of that phenomenon, or is connected with it through some fact of causation."

This last method allows us to identify a cause when we cannot eliminate it or other terms from our instances. The circumstance we seek is that the possible cause can vary and we find the effect varied correspondingly in the resulting instances. If we denote varied terms with ' and *, the example (not given by Mill) would be:

ABCabc

A'Bca'bc

A*Bca*bc

(concomitant variations)

A is the cause of a.

This method allows us to infer the effect of ineliminable causes: of a nearby mountain on the period of a pendulum (we vary the distance to the mountain); of heat on the volume of a gas (we vary the temperature); and of the moon on the earth's tidal water (by waiting as the position of the moon changes). Or I can convinced myself that sunlight caused my burn since the longer my exposure to the sun, the greater the burn.

Mill's Examples

Mill leaves his readers in no doubt of the range of applicability of his methods. He interleaves examples (some mentioned above) throughout his development of the methods in Book III, Ch. VIII and in an examples chapter Book III, Ch. IX. This last chapter has examples given in some detail that span the universe of 19th century science. He applies his methods to toxicity of arsenic compounds; to the phenomenon of induced electricity; to the physiological investigation of "the relations between muscular irritability, cadaveric rigidity, and putrefaction"; to unaccounted for residual motions of heavenly bodies (when a comet fails to return to the position predicted by Newtonian theory, the residual is evidence of a slight resisting medium); and more.

A quite endearing example is drawn from what he names as John Herschel's *Discourse on the Study of Natural Philosophy*. Indeed the exposition draws heavily on direct quotation with Mill elaborating and tightening Herschel's argument. What is the cause of formation of dew? Mill gives a quite thorough answer. There are many analogous phenomena that agree in the formation of films of water: dew, moisture on the surface of a glass of cool water, water inside windows when the weather suddenly cools, *etc*. All agree in the coldness of the surface upon which the water is deposited. So, by the method of agreement, that coldness is the cause. Or is the dew the cause of the coldness? Mill proceeds to investigate further. Through reports on a series of experiments, he concludes that dew forms on substances that either conduct heat slowly or radiate it away quickly, so that the substances lose heat on their surfaces faster than conduction from within can replace it. He arrives at this by consideration of substances with different properties, such as different powers of heat conduction, so that the conclusion is supported by the method of concomitant variations. Finally, Mill observes that the only property shared by bodies that form dew is that their surfaces lose heat faster than it is replenished; and bodies that do not form dew lack the property. This observation ranging over many substances then allows him to infer to that thermal property as the cause by the joint method of agreement and difference.

Curiously, in spite of his admiration for the methods and this example, Mill needs to supplement it by "the Deductive Method." He recalls the limited power of air to hold water and how condensate forms when the air is sufficiently cooled. Taking this as his starting point, he deduces from it that dew will form in the circumstances generated by the earlier analysis, when the surface of the body cools sufficiently in moist air. Mill offers this embellishment as a "corroboration." Rather it appear to be more: it is an essential part of the full causal account of dew formation and the need to generate it by another method can hardly count in favor of the comprehensiveness of Mill's canons.

4 Mill's Predecessors

The essential idea of Mill's methods were clearly articulated centuries before within Francis Bacon's methods of tables. They were used to find forms. The methods were adapted to the search for efficient causation by later thinkers, notably John Herschel.

Mill's exposition of his canons was part of a well-established tradition in methodological writing that extended back through John Herschel and Isaac Newton to Francis Bacon. As we shall see, Mill owed to Bacon the idea that one discerns underlying processes by collecting instances and seeking the appropriate element present or lacking in instances with the effect, without it and with it in varying degrees. Bacon, however, used the technique for seeking forms in an Aristotelian mode. Newton focussed attention on efficient causes--what we now ordinarily think of causes--and Herschel focussed Bacon's method on the search for them. Mill's contribution was to bring a simple, disciplined order to the resulting corpus.

John F. W. Herschel

Mill certainly did not pretend that he was doing anything more than adding to this established tradition. He acknowledged Herschel's priority, quoting extensively from Herschel's examples. Both the example of comets and dew are given with extensive quotes from Herschel. I have followed the tradition in giving Mill's presentation of the methods, since it is the most thorough and careful[31] and the one commonly known to modern readers. His immediate predecessor is Herschel's (1830) *A Preliminary Discourse on the Study of Natural Philosophy*. Herschel lays out (p.152-59) ten "rules of philosophizing." These rules do contain the essentials of Mill's methods but without Mill's legalistic clarity. Indeed some of the rules are quite obscure. The fourth merely asserts:

> "4th, That contrary or opposing facts are equally instructive for the discovery of causes with favorable ones."

What saves the pronouncement from complete obscurity is the example that follows immediately. Herschel reports on air whose power to support combustion or life has been exhausted after it is exposed to iron filings over water. That the air *fails* to support combustion or life allows us to conclude that the part of air that supports combustion and life has been extracted by the iron. The example is clear, but how are we to translate it into a method of generality comparable to Mill's? This rule and others go beyond Mill's carefully refined scope. The third rule merely enjoins us not to deny causes inferred to by analogy, even if it is unclear how the analogous process would work. So, by analogy with terrestrial light sources, we conclude the sun must be hot, although we have no idea of the processes that could maintain its heat.[32] Or the sixth rule enjoins us to allow for the confounding effect of additional unperceived causes.

Within the remaining rules, one can find more or less serviceable versions of Mill's methods of agreement, difference, concomitant variations and residues. The seventh rule amounts to the method of difference. It begins (Herschel's emphasis):

> 7th, If we can either find produced by nature, or produce designedly for ourselves, two instances which agree *exactly* in all but one particular, and differ in that one, its influence in producing the phenomenon, if it have any, *must* thereby be rendered sensible. If that particular be present in one instance and wanting altogether in the

[31] Here I concur with Mill's acknowledgment of Herschel's priority (Mill, 1872, p.271): "…of all books which I have met with, the four methods of induction are distinctly recognised…" while preserving his own primacy "…though not so clearly characterised and defined…"

[32] The example is the same as Newton invokes in support of Newton's second rule of reasoning. However Newton does not use it to illustrate the power of analogy but to illustrate how we should assign the same causes to the same effects, a not unrelated notion.

other, the production or non-production of the phenomenon will decide whether it be or be not the only cause:…

Newton's Rules

Herschel in turn draws directly on the work of Isaac Newton and Francis Bacon. Indeed a bust of Bacon appears on his title page and, on the page facing the table of contents, is the quotation of an aphorism of Bacon. More directly, Herschel makes clear that the causes he seeks are those named by Newton as the real causes--*verae causa*: "…causes recognized as having a real existence in nature, and not being mere hypotheses or figments of the mind." (Herschel, 1830, p. 144). Newton himself offered no methods comparable to Herschel and Mill's. However Herschel's expression "rules of philosophizing" is a natural English translation of Newton's own label, *Regualae Philosophandi*, for the four rules Newton laid down at the beginning of Book III of his *Principia*. Those rules amount to directions on the conduct of inductive inferences:

RULES OF REASONING IN PHILOSOPHY

RULE 1

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.

RULE II

Therefore to the same natural effects we must, as far as possible, assign the same causes.

As to respiration in a man and in a beast; the descent of stones in Europe and in America; the light of our culinary fire and of the sun; the reflection of light in the earth, and in the planets.

RULE III

The qualities of bodies, which admit neither intensification nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever.

…

RULE IV

In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate, or liable to exception.

This rule we must follow, that the argument of induction may not be evaded by hypotheses.

It is quite clear why Newton includes these rules. He is concerned over the fragility of the very inductions he is about to advance in his *Principia*. He may find that all bodies he examines possess inertia. But how secure is the enumerative induction that concludes all bodies, examined or not, possess inertia? He may find that all systems of bodies he can test gravitate towards one another: the moon towards the earth, the earth's ocean towards the moon, the planets towards one another; and so on. But how secure is the enumerative induction that all bodies gravitate? Newton resolves the problem by announcing Rule III and appealing to it at appropriate places (Newton, 1729, p. 399, 413).

The other rules serve analogous purposes. While the fall of bodies close to the earth's surface and the fall of the moon towards the earth could be accounted for by the same cause, should we? Newton appeals to Rules I and II to justify it (p.409); for we then invoke just one cause and not many. Should we ascribe the centripetal motion of all planets to this same cause? Newton calls on Rules I, II and IV jointly (p. 410) to license the ascription.

Their Basis

While Newton's intent is clear, we must ask what enforces his rules. Are they merely exercises in legislation? Is Newton eradicating a concern over the fragility of induction by the simple expedient of stipulating its soundness? Or does he have some deeper grounding? There seems to be more of the former stipulation than latter grounding. The discussions following Rules I, II and IV seem largely devoted to establishing the naturalness and desirability of the Rules. The extensive discussion following Rule III (omitted above in quote) does have something more of an argument. To support the Rule he appeals to our preference for "evidence of experiments" over "dreams and vain fictions." Presumably the point is that we have no reason to believe that unexamined bodes differ from those examined other than untrustworthy dreams and fictions. Newton also recalls our supposed common belief that the properties of the whole of a body are recoverable from those of its undivided particles: the hardness of the whole from the hardness of those parts, and so on. Presumably Newton is seeking to support Rule III through an atomic theory of matter; atoms have certain immutable properties that can then be conferred on gross bodies. So when we find such immutable properties in gross bodies, we have good reason to think that we have found the properties of the atoms which are thus universal. This would explain why Newton would limit Rule III to invariant properties of bodies not properties in general, for those invariant properties are most likely to be secured by the atoms.

In comparison to Mill's canons or Herschel's rules, Newton's are weaker, in so far as they enjoin us to seek fewer causes (Rules I and II) and trust what induction delivers (Rule IV). And they are different, in so far as they enjoin us to be especially ready to generalize invariant properties of a body (Rule III), apparently on the strength of an atomic theory of matter.

Francis Bacon

Francis Bacon remains a fascinating and controversial figure in the history of the philosophy of science. He assembled writings on the methods of science in a major, incomplete work, *The Great Instauration*. The literature on Bacon is expansive. (For an entry into it, see Blake, Ducasse and Madden, 1960, Ch.3; Urbach, 1994.) We shall be concerned just with one part, the *New Organon* (*Novum Organum*), which includes his famous assault on enumerative induction. The core of his positive proposal in the work is his method of tables, described in Book II. Its goal is to reveal forms, the true natures of things. Bacon presents these forms (Book II, Aphorisms 2-3) as one of Aristotle's famous four causes: the material, efficient, formal and final cause.[33] It is the one he is most concerned with for if one knows it one "embraces the unity

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[33] The four causes, in Aristotle's account (*Metaphysics* I.3) specified what one needed to know if a process was to be understood. The material cause is the substance undergoing change; the marble being sculpted, for example, if the process is the creation of a marble statue. The efficient cause is that which initiates the change; the chisel striking the marble and shaping it. The final cause is the goal of the process; the completed sculpture. The formal cause is the structure that rules the process; the abstract shape that it to be realized in the final sculpture. The efficient cause corresponds to our ordinary notion of cause; they are the causes sought by Mill's methods.

of Nature in substances utterly different from each other." (1620a, p.135) Or later (1620a, Book II, Aphorism 17, p. 170): "…when I speak of forms I mean nothing but those laws and definitions of pure actuality, which govern and constitute any simple nature, such as heat, light, weight, in every kind of material and subject that is capable of receiving them."

His Tables …

To arrive at the forms, Bacon enjoins us to compile exhaustive tables of instances from which the forms can be read. He demonstrates with the lengthy example of the form of heat. First he lists 28 instances, all agreeing in the nature of heat, his "Table of Existence and Presence." They are quite a diverse--indeed motley--collection. They include (1620a, Book II, Aphorism 11):

> 1. The rays of the sun, especially in summer and at midday…3. Fiery meteors…6. All flame…13. All hairy substances…22. Strong oil of sulfur and of vitriol [sulfuric acid] has the effect heat in burning linen…27. Even keen and hard frosts bring a certain burning feeling--'nor burns the North Wind's piercing cold.'…28. Other instances.

They are contrasted with instances in a second table, which are closely akin to those of the first, but differ in lacking heat. They comprise the "Table of Deviation or Absence in Proximity." In contrast to 1., Bacon offers "1. The rays of the moon and of stars." In contrast to 6., Bacon lists numerous examples of luminous effects that have little heat, such as sparks produced by scraping sugar or the electrical effect of St. Elmo's fire. To 22., Bacon calls to mind that acrid liquids are not initially hot to the touch. Bacon then offers 41 instances in which heat occurs to greater or lesser degree; these comprise his "Table of Degrees" or "Table of Comparison."

…Yield the First Vintage: Heat is Motion

Bacon presents these tables to the understanding. He is immediately able to eliminate fourteen candidates for the form of heat since they fail to be present invariably. For example, the form cannot be elemental--presumably he means a solely terrestrial element--since heat arises in rays of the celestial sun. Correspondingly it cannot be celestial, since heat can be found under the earth. He then announces his conclusion, albeit a provisional and hesitant one, his "First Vintage concerning the Form of Heat" (1620a, Book II, Aphorism 20, p. 174):

> Throughout each and every instance the nature of which heat is a limitation appears to be motion.

He proceeds to support this by observing that motion is indeed obviously present in many cases of heat, such as the flickering of a flame; that heat is enhanced by motion, such as the action of bellows on a fire; and that it is reduced by compression that reins in motion and extinguishes fires. He then takes pains to clarify exactly what sorts of motions are involved: they are expansive and upward motions; they are not uniform motions so that the body "takes on a vibrating motion, always trembling, straining and struggling, irritated by its rebuff; whence arises the fury of fire and heat." (p. 177) Finally the motion "should take place through minute particles; not, however, the very smallest, but ones that are just a little larger." (p. 178)[34]

Appraisal

What are we to make of this? I do not know whether to rejoice or lament. To begin, the *method* Bacon outlines seems quite unobjectionable, even prescient. One can see just how close his tables come to the highly controlled methods of agreement, difference and concomitant variants described by Mill--although Bacon seeks forms, where Mill seeks causes. In other applications, one could imagine Bacon's method proceeding without hitch. Had he, for example, sought the form that constitutes not heat, but the power to conduct heat readily, he might well have succeeded in deciding that this form coincided largely with a metallic form. Or had he sought the form of buoyant materials, he might have found that it coincided with low density.

But there is something amiss in Bacon's execution. Let us see if we can find it. Bacon wrote centuries ago, so it would be unfair, indeed historiographically mischievous, to hold Bacon to standards informed by modern knowledge. Nonetheless I will do this briefly merely to deflect a possible distraction. Heat is motion, Bacon concludes, and it is a little hard not be amazed that he makes the very same claim as the celebrated kinetic theory of heat. Don't be amazed. Bacon's theory is not the kinetic theory of heat. First that theory pertains only to particular forms of heat, such as in gases, liquids and solids, where it associates heat at least in part with the rapid motion

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[34] There is a great deal more to Bacon's scheme that I now pass over. His presentation of the method of tables is then followed by the description of 27 "prerogatives of instances," allowing one to improve and refine one's investigations.

of molecules. Even in this case, not all the heat is motion; in a solid, depending on the internal structure, much of the energy we might call heat is stored in the potential energy of atoms momentarily displaced from equilibrium configurations. In other cases, the heat is not energy of motion at all; the heat of sunlight does not reside in motion of anything, but in the energy of an electromagnetic field.

We should not hold against Bacon that his account does not square with what we now know. That is too high a demand to place on anyone from the past. But we can ask that he be consistent with his own demands. Here I think Bacon fails. Bacon's tables of instances are the most motley collection--the few quoted above do not begin to capture how haphazard they are. Bacon presents them to his understanding and extracts motion as the first vintage. When I review the instances, I see *no* common element that meets the requirements of being present in all instances of the Tables of Presence and absent in all in the Tables of Deviation. Motion certainly is present in some instances of the Tables of Presence. But it is absent in many others. Where is the motion in oil of vitriol? Or in hairy substances? Bacon can only find motion in these and many other cases by the greatest of indulgences. Perhaps there is motion when oil of vitriol acts on substances; or there is motion in the beasts that grew the hairy substances. How does Bacon find motion in "the rays of the sun" (an instance of heat) but not in "the rays of the moon…" (the corresponding entry in his Table of Deviation)?

This self-defeating indulgence manifests in another way. The motion that Bacon identifies is gross motion, such as the turbulence of a flame. Yet Bacon's conclusion is that heat is motion right down to some vaguely specified level of "minute particles" (but "not the very smallest"). While Bacon may properly find gross motion in many cases of heat, what in his method licenses the inference that there is invisible motion in minute particles of hot bodies even when no gross motions are visible? It may be justifiable as some kind of comfortable intuitive leap, but he has now surrendered his moral high ground. He is as guilty of hasty generalization as those he criticizes. As an index of how unsuccessful Bacon's analysis was, recall that a material theory of heat persisted well into the nineteenth century. The hallmark of the method outlined is that it produces quite compelling results if executed properly; apparently Bacon's analysis was not sufficiently compelling to settle the matter.

Box 3: Cholera and Gravitational Waves

The methods described by Bacon, Herschel and Mill are used nearly universally in the sciences. As illustrations I will describe a famous application in epidemiology and a lesser known one in gravitational physics.

We shall look at how John Snow identified contaminated water as the cause of a cholera epidemic in mid 19th century London and the design of 20th century experiments intended to reveal gravitational waves. Neither account reveals conscious implementation of the method outlined by a Bacon, Herschel or Mill. However both embody the common sense of someone seeking causes by these methods. In the description below, so that the connection to these methods are their, I will make their tacit methodological assumptions explicit, using the language of Mill's methods.

Snow on Cholera

The most celebrated illustration of the use of these methods lies in John Snow's famous substantiation of the cause of an outbreak of cholera in London in 1854, as recounted in Snow (1855). Cholera is an illness characterized by massive diarrhea. Its victims often die from loss of fluids and salts. We now know that it is caused by a bacterium that is often transmitted in water contaminated by the fecal matter. The bacterial cause was not know in 1854, although Snow and others thought that contamination of the water supply a likely cause of cholera. When a "most terrible" (p. 38) outbreak of cholera arose in London in the area he described as "Broad Street, Golden Square, and the adjoining streets," Snow was on the spot and ready to investigate.

Water was supplied to the area by a pump at Broad Street, so he immediately suspected its water (p. 39): "…but on examining the water, on the evening of 3rd September, I found so little impurity in it of an organic nature, that I hesitated to come to a conclusion." He continued to explain what is, for our purposes, most interesting: he sought that factor common to the cases of cholera, just as, we might note, the method of agreement requires. He convinced himself it must be the water.

> …Further inquiry, however, showed me that there was no other circumstance or agent common to the circumscribed locality in which this sudden increase of cholera occurred, and not extending beyond it, except the water of the above mentioned pump.

On proceeding with his investigation, Snow found most of the deaths from cholera to be in the vicinity of the pump. Might we infer from the method of agreement that proximity to the pump is the cause of cholera? Not quite. While nearly all deaths were in proximity to the pump, there were ten deaths in houses nearer another street pump. While small in comparison with the more than 300 deaths that had already occurred by September 3, they showed that mere proximity was not necessary for cholera. An attempt to apply the method of difference fails to establish that proximity is the cause; cholera arises for cases both near and far from the pump.

Snow proceeded to show that most of the ten anomalous cases ceased to be so if we asked not after proximity to the pump, but if the victims drank its water. This proved to be the case. In eight of the ten cases, Snow could establish that the victims drank Broad Street pump water or were likely to do so. Either their families preferred that pump's water and sent out for it (5 victims) or they were children who went to a school on Broad street and were known to or may have drunk the pump's water (3 victims). We might be tempted at this point to call on the joint method of agreement and difference. Cholera victims all agree in having drunk Broad Street pump water; the difference between those victims and those who did not fall ill is that the latter did not drink the pump's water. There is a complication that Snow mentions only in passing (p. 45): "There were many persons who drank the water from Broad Street pump about the time of outbreak, without being attacked with cholera…" So we must modify the method's application. The effect is not cholera, but the possibility of cholera.

Snow continued in this vein, showing that the possibility of cholera largely[35] existed when people drank Broad Street pump water and was absent when they did not. He listed case after case. The workers in a brewery on Broad Street escaped the epidemic. It turned out they had other sources of water in the brewery and that the workers were supplied with malt liquor, so

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[35] Another complication: Snow knew that cholera could also arise if, for example, someone tending to a cholera victim directly ingested the fecal matter through not washing their hands and then eating food.

they were thought not to drink water at all! Or there was the puzzling case of a woman who died of cholera in Hampstead, having not visited the Broad Street area for months. It turned out she had bottled Broad Street pump water brought to her every day.

Snow also plotted the deaths on a street map. He found (p. 47) "that deaths are most numerous near to the pump where the water could be more readily obtained." We easily identify this as an application of the method of concomitant variations: the greater the proximity to the pump (and presumably the more water consumed), the greater the possibility of cholera. There may be a second application of this method. On September 8, Snow was able to arrange for the handle of the pump to be removed, to prevent water being drawn. The number of new attacks of cholera drops precipitously at this time. The reduction in the availability of the water is associated with a reduction in new attacks. The application is problematic, however, since the number of new attacks was *already* starting to fall rapidly on the day the pump handle was removed.[36] Snow expected an incubation period of one to two days (p.16) and we would need to wait for supplies of water already drawn to be used up, so an effect of the pump handle's removal should not be seen so rapidly. We may still apply the method of concomitant variations, but our further knowledge suggests that the result it delivers is spurious. The removal of the pump handle did not cause the drop in mortality but accidentally coincided with it.[37]

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[36] The relevant portion of the figures reported by Snow is (p. 49):

Date	Aug. 27	28	29	30	31	Sep. 1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
No.	1	1	1	8	56	143	116	54	46	36	20	28	12	11	5	5	1	3	0	1

where "No." designates "No. [number] of [new] Fatal Attacks." The pump handle was removed on September 8.

[37] Snow himself is more cautious in dismissing the possibility that the stopping of the operation of the pump halted the outbreak, remarking (pp.51-52): "There is no doubt that the mortality was much diminished … by the flights of the population, which commenced soon after the outbreak; but the attacks had so far diminished before the use of the water was stopped, that it is impossible to decide whether the well still contained the cholera poison in an active state, or whether, from some cause, the water had become free from it."

Snow's second hand report on an earlier cholera outbreak in Manchester in 1848 and 1849 displays in briefest form the sort of agreement and difference used to establish cause. As with the later outbreak in London, a particular well whose water came to be contaminated with sewage, is the suspect. Snow (p. 31) quotes from the "Report of the General Board of Health on the Cholera of 1848 and 1849":

> The inhabitants of thirty houses used the water from this well; among them there occurred nineteen cases of diarrhea, twenty-six cases of cholera, and twenty-five deaths. The inhabitants of sixty houses in the same immediate neighborhood used other water; among these there occurred eleven cases of diarrhea, but not a single case of cholera, nor one of death.

Attempts to Detect Gravitational Waves…

Relativistic theories of gravitation—most notably Einstein's general theory of relativity—predict the existence of waves in the gravitational field. They arise whenever masses are moved; their displacement sends out ripples in the gravitational field at the speed of light. In principle, the ripples are easy to detect. As they speed past, they set masses in motion, much as a water wave makes a buoy bob up and down as it passes. The practice is much different. Gravitational waves are vastly weaker in strength than other forms of waves known to us. While waving your arm about might create gravitational waves, they are of such minute amplitude that no one has any idea how to detect them. Detectable waves are only likely to be produced by events involving huge amounts of mass and energy. So efforts to detect gravitational waves have concentrated on waves produced extraterrestrially by cataclysmic cosmic events, such as the explosions of stars.

The best known of these efforts are due to Joseph Weber. His detector consists of a large aluminum bar, weighing roughly a ton, and suspended by wires in a vacuum. A gravitational wave passing through will excite oscillations in the bar, much as a gentle tap on a xylophone tube produces a musical note. These oscillations are then revealed by strain gauges attached to the bar. They can sense tiny changes in distance between neighboring points on the surface. (See Misner, Thorne and Wheeler, 1973, p. 1016.) The resulting detector is extremely sensitive and capable of revealing quite minute oscillations.

Gravitational waves are not the only source of oscillation in the detector. There are others. The mere fact that the bar is not at the absolute zero of temperature means that its atoms are dancing about in random thermal jiggles that in turn excite tiny oscillations in the bar. In spite of efforts to isolate the bar from local disturbances, there is always the possibility that sounds or electric and magnetic fields or seismic disturbances at the laboratory may get through and excite oscillations. All these oscillations are likely to be very small. The problem is that the strength of the gravitational waves sought is so weak that the oscillations they produce will be of comparable size. How are they to be distinguished from the noise produced by these other sources?

Weber's experiment incorporated an ingenious solution to the problem. All credible sources of noise are local; they arise from events in the spatial vicinity of the laboratory. Gravitational waves produced by distant cosmic events are spread out. As the wave expands from its distant source, it forms a huge sphere in space. When that sphere impinges on the earth, it strikes all parts of the earth at the same moment, much as an expanding balloon might strike a speck of dust. To exploit this difference, Weber set up two detectors: one at the University of Maryland near Washington D.C. and the other at Argonne National Laboratory near Chicago. Each detector reports a stream of small oscillations, some bigger, some smaller, but virtually all of them excited by local noise. Occasionally there will be a larger peak. Might this be caused by a gravitational wave, the fading reverberation of some distant cosmic cataclysm gently passing by? If the cause is local, then we would not expect the peak at one detector to be replicated *at precisely the same moment* at the other detector some 600 miles distant. If the cause is a gravitational wave, we would expect precisely such a coincidence. So Weber's experiment compares the two data streams reported by each detector, looking for correlations between them.

One easily recognizes the technique as an application of the method of agreement. The method does not directly identify the cause of a correlated peak as a gravitational wave. Rather it identifies the common time as the only common factor in the correlated peak, so that local causes in either Washington or Chicago are ruled out. Schematically, if we say the correlated peak occurs at time T, we have

> Local cause 1 at Washington, Local cause 2 at Washington, … , time T; correlated peak at Washington at time T

Local cause 1 at Chicago, Local cause 2 at Chicago, … , time T; correlated peak at Chicago at time T

where "local cause 1" might be some local acoustic source of noise; *etc*. The sets of candidate causes agree only in the time T. So we infer to it as the cause. Further argumentation that is not a part of the method of agreement leads us to conclude that a gravitational wave is the only possible way that the time alone can be the cause.

…and the Calibration of the Detectors

There is a second use of the methods in the operation of Weber's experiment. It is most desirable to have some independent check that the signals reported by each detector are indeed good measures of some external disturbance. In standard laboratory practice, one checks that the instruments are functioning correctly with a calibration. In my own experience, one checks that a thermometer is reading correctly by plunging it into a bath of ice water and bath of boiling water and checking that the thermometer correctly reports 0ºC and 100ºC respectively. Or one might check a scale weighs correctly by putting an object of known weight on it. A similar strategy was used in the case of Weber's experiment and attempts to replicate it. An artificial disturbance of known magnitude is allowed to impinge on the detector and the experimenter checks that there is a corresponding peak in the signal. The source of the disturbance varied. In one approach, it was a pulse of electrical energy of known size; in another it was the effect of a rotating mass nearby—the latter disturbance was not caused by gravitational waves but merely the variation in gravitational force as the mass approached and receded. (For further discussion of the calibration of this and of other experiments, see Franklin, 1994, pp. 469, 485-87; 1986, pp. 175-177.)

This use of calibration is readily identifiable as an application of the method of concomitant variations. We have the normally prevailing causes producing the effect, the standard signal; we then augment these causes with a known disturbance and check for the corresponding alteration in the effect, a peak in the signal. We conclude that the disturbance is the cause of the peak and thus that the detector is functioning correctly.

For all the brilliance of its design and the patience of its designers, the efforts to detect gravitational waves produced no universally accepted successful detections. In the early 1970s about a dozen groups worked on detection of gravitational waves. By 1975 none but Weber were still working on detecting the waves and only he was reporting successful detection. The other researchers in the field felt that his reports were based on incorrect analysis of his data. Different methods were available for comparing the signals from the two detectors and the criticism was that Weber's method was incorrect and likely to produce spurious correlations. (For review and analysis see Franklin 1994.)

5 What Sort of Induction is Embodied in the Methods?[38]

The methods identify necessary and sufficient conditions of various types within the instances presented. They are first interpreted as causes or forms. Then the induction consists in supposing that the relations so identified in these instances will persist in all other instances.

Necessary and Sufficient Conditions

The methods described have been used in widely differing circumstances. Bacon used them to find forms; Herschel and Mill used the same methods to find causes. What makes the methods applicable for these difference goals is that the forms or causes sought are identified through a shared feature: relations of necessity and sufficiency in the phenomena. In their simplest form, these relations are:

> *Sufficiency*: Given background B, if condition C obtains, then so must effect E.
>
> *Necessity*: Given background B, if condition C fails to obtain, the effect E fails to obtain.

When both necessity and sufficiency obtain we have:

> *Necessity and Sufficiency*: whenever condition C arises so does effect E, given the background B.

Necessity and simplicity provide the simplest case of relations of interdependence.

The method of concomitant variations introduces a more complicated relation of interdependence. The presumption is that the condition C and effect E are variable magnitudes. Generally the magnitudes can be measured by a single real number: masses, temperatures, concentrations of chemicals, velocities or accelerations in some direction, displacements in some direction, and so on. But they need not be so simple. They might include things that cannot be

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[38] There have been numerous analyses of the sort of inference embodied in these methods. See for example Johnson (1922, pp. 217-39) and Skyrms (1975, pp. 89- 116)

measured by single real numbers, such as the distributions of matter or fields in space; or the nutritive power of a diet; or the health of a patient. All that matters is that that the conditions and effects admit some gauging as a variable magnitude. The relation sought is that the condition C is necessary and sufficient for the effect E and, moreover, that the magnitude of the effect E varies with the magnitude of the condition C so that fixing C fixes E. This is summarized as:

> *Functional Dependence*: Condition C is a necessary and sufficient condition for effect E given the background B; and the magnitude of effect E is fixed by that of effect C.

Herschel's and Mill's Characterization of Cause and Effect…

The first step of Herschel and Mill's application of the methods is to interpret relations of necessity and sufficiency in the instances as causal relations. This reinterpretation is highly non-trivial. The notion of cause has been a subject of longstanding philosophical scrutiny with no clear consensus established on the true explication. We must set this concern aside since an analysis of the notion of causation lies outside the present scope. For our purposes, all that matters is that Mill and Herschel have a definite notion of causation; causes are identified in terms of necessary and sufficient conditions. That this is so is clearest in Herschel's exposition. In order to generate his "rules of philosophizing," he describes "the characters of that relation which we intend by cause and effect." These he describes as leading to his rules. The characters are (Herschel, 1830, pp. 151-52, Herschel's emphasis):

> 1st, Invariable connection, and, in particular, invariable antecedence of the cause and consequence of the effect, unless prevented by some counteracting cause…[39]

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[39] For clarity I omitted a portion of the Herschel's statement since it raises only incidental issues. The omitted text reads:

But it must be observed, that, in a great number of natural phenomena, the effect is produced gradually, while the cause often goes on increasing in intensity; so that the antecedence of the one and the consequence of the other becomes difficult to trace, though it really exists. On the other hand, the effect often follows the cause so instantaneously, that the interval cannot be perceived. In consequence of this, it is sometimes difficult to decide, of two phenomena constantly accompanying one another, which is cause or which effect.

2d, Invariable negation of the effect with absence of the cause, unless some other cause be capable of producing the same effect.

3d, Increase or diminution of the effect, with the increased or diminished intensity of the cause, in cases which admit of increase and diminution.

4d, Proportionality of the effect to its cause in all cases of *direct unimpeded* action.

These characters amount to requiring just the necessity, sufficiency and functional dependence described above.[40]

While I believe Mill's intent essentially identical, I could not find a single, similarly compact statement in his writings, although he does labor to specify what he intends by the notion of cause. He asserts, for example (1872, Bk III, Ch, V, §6, p. 222, Mill's emphasis): "We may define…the cause of a phenomenon to be the antecedents on which it is invariably and *unconditionally* consequent." While "invariable" clearly betokens sufficiency of the cause, it is not clear to me that he intends necessity in this statement. The requirement of being unconditional is introduced to preclude the possibility that the invariance is merely an accident of a few cases at hand. It must be so in all possible cases.

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[40] I will not quibble over slight differences. Herschel states only that alterations in the cause produce corresponding alterations in the effect; in functional dependence we demand that the full magnitude of the effect be determined by that of the cause (given the background). Given the invariable character Herschel assumes for the connection, I am willing to presume that he intended the stronger reading. Once the cause and its magnitude are fixed, he must surely imagine the effect fixed as well and that would include its magnitude. Herschel's 4th condition of proportionality appears stronger than mere functional dependence, as does also the assumption in the 3rd clause that the change of magnitude of cause and effect move in the same direction. Much of these differences can be absorbed into the definitions. For example, if further increase of a cause reverses the direction of change of an effect, we could imagine that we have entered a new domain with a different causal process and thereby restore Herschel's 3rd clause.

…and Bacon on Forms

Bacon must also reinterpret what he sees in his instances in terms of forms. This he does also by presuming that forms are manifested in terms of necessary and sufficient conditions. He writes (Bacon, 1620a)

> For the form of any nature is such that when it is there, the given nature infallibly follows. It is therefore always present when that nature is present, and universally implies it, and is it in all cases. Again, the form is such that, once it is removed, the given nature invariably flees also. It is therefore always absent when that nature is absent, and always implies its absence, and is in it alone.

These descriptions do not exhaust what these authors take forms or causes to be. Herschel also clearly intends (as can be seen from the additional text I give in a footnote for the first clause) that the cause cannot come after the effect in time. Bacon also continues in the above passage to require that the form "derives the given nature from some source of being that is inherent in more natures and is (as they say) better known to Nature." These additional characteristics would be used as a filter on the products of the method. A candidate cause would be dismissed if it comes after its effect; a candidate form would be dismissed if judged to be less well known to Nature.

The Induction from Instances: The Joint Method

This reinterpretation of the instances in terms of causes or forms is the first step. The second is the one characteristic of inductive generalization: it is assumed that whatever obtains for the instances at hand will obtain for all other instances. The different methods arise according to how completely the conditions of necessity and sufficiency are realized in the instances. To complete the analysis, I will just consider Mill's methods. Nothing essential is lost in the restriction.

If we concern ourselves with necessity and sufficiency alone, the richest induction is supplied by Mill's joint method of agreement and difference. It was illustrated schematically above by the example

ABCabc

ADEade

FGfg

XYxy

from which we infer that A is the cause of a. The first two instances ABCabc and ADEade manifest the sufficiency of A for a: whenever A arises so does a. The last two instances FGfg and XYxy manifest the necessity of A for a: whenever A fails to arise so also does a fail to arise. Finally the background is supplied by the remaining BCbcDEde… They supply a varying background. Thus, using Mill's interpretation of cause, we are licensed to infer to the premise of an inductive generalization:

For the instances given, A is necessary and sufficient for a with varying background;
A is the cause for a.

(inductive generalization)

In all instances, A is necessary and sufficient for a; A is the cause for a.

The inference is then completed as shown with an induction entirely within the inductive generalization family: what holds of the instances given is inferred to hold of all.

The Methods of Agreement and of Difference

In the joint method all of necessity, sufficiency and variability of the background are manifested in the instances given. If we do not have all of these so that the variation in the instances is more limited, we arrive at the separate methods of agreement and of difference. Referring to Mill's illustration of the method of agreement, we have

ABCabc
ADEade

as the basis for the inference to A as the cause of a. This illustration manifests the sufficiency of A for a (whenever A arises so does a) and also the variability of the background, but it does not manifest the sufficiency of A for a. In Mill's illustration of the method of difference,

ABCabc
BCbc

is the basis for the inference to A as the cause of a. Here the illustration manifests both the sufficiency of A for a and the necessity of A for a, but there is no variation in the background. In both cases the conclusion that A is the cause of a is generalized from the instances at hand to all instances.

Methods of Residues and of Concomitant Variations

These methods do not alter the essential inductive strategy seen so far. We look at the given instances and identify a causal relation as obtaining in them; we then generalize that all instances will manifest the causal relation. The difference is that richer methods are now used in the first step of identifying the causal relation. In the method of residues, we use causal information gleaned from earlier applications of the methods to infer to a residual causal relation in the instances, which is then generalized. Mill's formula for the method does not make clear just how much of the relation of necessity and sufficiency must be manifest to license the causal relation in the residue. His example (given above) suggests that sufficiency is enough.

In the method of concomitant variations, causal relations are not merely identified through necessary and sufficient conditions. They are identified by their manifesting the stronger condition of functional dependence. The notion of functional dependence presumes sufficiency of the cause for the effect: to see that the magnitude of the effect varies with that of the cause, the effect must be present whenever the cause is. That is, the cause is sufficient for the effect. Presumably, this dependence extends to the absence of the cause leading to the absence of the effect; I have assumed so in the definition of functional dependence given above.

In both cases, the second step remains the same; all unseen instances are presumed to comport themselves as the seen instances have, so that the judgment of a causal relation is universal. The differing conditions manifested are summarized in the table:

Method	Minimum Elements present in instances that manifest a causal relation
Agreement	Sufficiency Variation of background
Difference	Sufficiency Necessity
Joint Method of Agreement and Difference	Sufficiency Necessity Variation of background
Residues	Necessity Other causal relations known prior
Concomitant Variations	Functional dependence

The Weaknesses of the Methods

With the above two step characterization of the methods, it is now fairly easy for us to catalog their strengths and weaknesses. The strengths are quite obvious. The methods have reduced profound tasks—the identification of causes and of forms—to a far simpler task, the identification of necessary and sufficient conditions. The methods are successful in such a huge array of cases that they are truly a standard device in any inductive repertoire. They fail, however, whenever either step fails. Each step can fail.

Failure to Identify the Form (that is not there)…

Bacon assumes that the phenomena of nature are governed by laws. The major weakness of his method is a risky presumption over the character of those laws. He presumes the laws appear as governing forms and, crucially, that these forms are manifested in the instances Bacon catalogues. In some cases, this presumption is correct. As I suggested above, if Bacon were to investigate buoyancy, he would soon find that buoyant material are less dense; their lower density would be manifested in his histories and he would identify low density as the form of buoyancy. The presumption fails in other cases. Heat is energy randomly distributed over the very many components of a larger system. That energy can be in any form, from electromagnetic energy (in heat radiation) to potential energy (part of the thermal energy of solids) to kinetic energy of motion (the thermal energy of ideal gases). In the last case, the kinetic energy is of invisible, submicroscopic molecules and not the energy of gross motion visible to Bacon in his instances and reported in his histories. In these cases, Bacon's method must fail; there is no form for even the most careful and exhaustive history to identify.

…And Failure to Identify the Cause

Similarly Herschel and Mill's version of the methods can fail if identification of the cause in the first stage fails. Such failure can come about in many ways. The cause may not be discernible in the description of the instances. How can we ascertain the cause of the internal heat of the earth or the cause of the heat of the sun if all we have are routine observations of surface geology and telescopic astronomy? Neither can supply direct observation of the internal radioactivity of the earth or the hydrogen in the sun undergoing fusion to helium. The method must be supplemented by further analysis. In the case of gravitational waves described in Box 3: Cholera and Gravitational Waves, the waves themselves are not catalogued in the phenomena. The methods tell us that the correlated pulses agree only in their time. We must then convince ourselves that a gravitational wave is the only credible cause of the agreement.

More seriously, the notion of cause employed is a very simple one and may just be ill suited to describe the inner working of the process at hand. The presumed necessity of the cause for the effect must surely give us some pause since we are quite familiar with many cases of an effect brought about by different causes. (In how many distinct ways can we cause the soufflé to be ruined?!) So the method of agreement must fail to find causes that are not necessary. We might imagine that the analysis is restricted to processes so narrowly construed as to have unique causes; or we might imagine that many distinct causes, C_1, C_2, C_3, … are merely disjoined to form a single disjunctive cause C_1 or C_2 or C_3 or …

Such simple tricks will not help us with a deeper problem. Our judgment that C is the cause of E always includes some background. When we are not proceeding cautiously, we simply ignore the background and affirm C as the cause of E. When we are cautious we may only ascribe some causal role to C in the production of E. Mill, in the statement of his methods, does this by conceding that the cause revealed may merely be "an indispensable part of the cause." If causes have parts, some indispensable and thus presumably some dispensable, there would seem to be a richer notion of causal process at hand than merely relations of necessity, sufficiency and functional dependence. There certainly seem to be cases in which the one cause succeeds in bringing about the effect in many distinct backgrounds, so that no great harm is done in ignoring the background. In other cases that will cause trouble, the power of the cause is dependent on the presence of a particular background. This sort of dependence arises in very complicated systems with many interactions. We might ask after the causes of good health, for example. Beyond the well known truisms, it is notoriously difficult to pick apart the causes. Or we might ask after the cause a patient's bout of influenza. While infection with the relevant virus is obviously necessary, it is certainly not sufficient. Many people have been exposed to the virus without becoming infected. Is it merely their immune systems recognize the virus? Or must we resort to vaguer notions of the robustness of their health, so that a satisfactory account simply cannot be given in terms of a list of distinct causes?

Failure of the Inductive Generalization

There are many ways this second step can fail. The causal claim sought is generally one of necessity and sufficiency of the cause and relative independence from the background. The inductive generalization projects these conditions from the instances given to all instances. There is an obvious risk in cases in which not all the features listed are present in the instances given. The instances given in the method of agreement do not display necessity for example; or those in the method of difference do not display variation of the background. So use of these methods exposes one to an unwarranted presumption of necessity of the cause or of independence from the background.

I remarked earlier that enumerative induction suffered a major weakness: we do not know which of two formally identical inductions will be strong. Since the present methods employ an essentially similar inductive generalization, they suffer precisely the same weakness. Each planet supplies an instance of an object that orbits the sun in an elliptical orbit. The method of agreement would locate the cause of the elliptical orbit in the sun. This is a straightforward and strong inference. Each planet also supplies us with an instance of an object that orbits the sun in the same direction as the Earth. An analogous application of the method of agreement locates the cause of this sameness in the sun. The result is by no means as secure. The sameness is either happenstance or to be explained by facts in the history of the formation of the planetary system—presumably their formation from a single spinning cloud of dust.

Stochastic Causes

For practical purposes, one of the most important ways that the methods fail is when we are dealing with stochastic causal processes. These are processes in which the cause does not assuredly bring about the effect, but only brings it about with at some level of probability. The methods are ideally suited to identifying the cause of an infectious disease, for example. However their literal application must give misleading results if the cause—some infectious agent—only brings about the disease at some level of probability below certainty. Then we will find instances in which exposure to the agent does not lead to the disease. This can defeat the methods at either level. They may fail at the first level if we gather enough instances so the lack of sufficiency becomes evident at that level. Then we must conclude that the agent does not cause the disease. The methods may fail at the second level if the lack of sufficiency was not evident in the instances and we generalize to the incorrect result that the infectious agent is always sufficient to produce the disease.

The methods can be generalized to accommodate stochastic causes. One might begin by looking not at individual cases but at rates within populations and rates of associations with populations. If the associations are strong, one can suppress stochastic fluctuations as noise or redescribe the causal connection in a way that accommodates its stochastic character. We saw both in Snow's analysis of the cause of a cholera outbreak (Box 3: Cholera and Gravitational Waves). Drinking contaminated water was not sufficient for someone to contract cholera in the epidemic; many drank the water without succumbing. So we described the effect not as succumbing to cholera, but its possibility. What justified this was that virtually all cases in the epidemic did arise from drinking contaminated water. We must say "*virtually* all cases" since drinking the contaminated water was also not necessary to succumb to cholera. However other causes, such as direct ingestion of fecal matter from another cholera victim, were rarer and discounted as noise.

These rough and ready techniques can only be used reliably when the associations are very strong. When they are not, we must exercise considerable care not to confuse an accidental agreement with a stochastic cause.[41] If there is a slight increase in cancer among people living under electrical power lines, how do we distinguish whether the power lines caused the cancers, whether some other cause associated with the power lines is the culprit or whether the increase is merely a random fluctuation with no systematic cause at all? In the case of gravitational waves, we seek the coincidence in time of two peaks at widely separated detectors. Peaks arise all the time in the data as a result of random noise. If we wait long enough, two such peaks will coincide by pure chance. How do we discriminate this case from a real detection?[42] To answer such questions, we resort to more sophisticated statistical analyses. Using standard statistical techniques, for example, we may show that, if the effect arises purely by chance, the agreement seen is very improbable. These analyses involve different approaches to induction.

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[41] Accidental agreement can arise in more general contexts as well. Snow's removal of the pump handle coincided with a dramatic drop in new cholera cases; but it was quite likely that the agreement was coincidental.

[42] As Franklin (1994) describes, the locus of disagreement in the debate over the existence of gravitational waves turned out to be over the methods used to discern whether coincident peaks were chance phenomena.

6 Hempel's Satisfaction Criterion of Confirmation

Hempel's satisfaction criterion is a generalization of enumerative induction to the richer language of predicate logic. Its core concept is a generalized notion of the instance of an hypothesis.

Accounts of Inductive Generalization Depend on the Context

Inductive generalization is driven by the simple idea that there is a natural generalization for an item of evidence. What that generalization might be is determined to very great extent by the context in which the evidence is expressed. In enumerative induction, the evidence is expressed as a particular within the context of syllogistic logic. From it we infer to the corresponding universal generalization. In Mill's methods, we note certain necessary or sufficient conditions obtaining in a few cases. We infer that they obtain universally. These contexts limit the scope of the associated inductions to those expressible within syllogistic logic or as necessary and sufficient conditions. The account of inductive generalization developed by Carl Hempel works within the context of predicate logic. The greater expressive power of that logic leads to a much more general notion of an instance of an hypothesis and its inductive generalization.

The Need for a Richer Account of Inductive Generalization

Do we need a richer account? We do. The accounts we have seen so far can only accommodate evidence in a special form. There are many more interesting cases that cannot be fitted into either, or at least not without uncomfortable contortions. For example, a beam balance can compare the two bodies. When the two bodies are placed in the pans, if one pan falls, we say the first overbalances the second.

Imagine that we undertake an investigation of these overbalancings from scratch. What general laws might we arrive at? One that we would probably quickly learn is this:

> Transitivity: For any three masses, if the first overbalances the second and the second overbalances the third, then the first overbalances the third.

This general law is of great importance. It must hold if we are to proceed to the usual account of the beam balance: each body has an intrinsic property called "mass." One body overbalances a second just in case the first has the greater mass. We can see quite quickly that transitivity is necessary for such a notion of mass. We need merely consider what would happen if transitivity fails. Then we might have a 10 gram mass overbalancing a 5 gram mass; the 5 gram mass overbalancing a 1 gram mass; but the 10 gram mass *not* overbalancing the 1 gram mass. That would contradict the properties now routinely assumed for mass.

There is no logical necessity in transitivity. It is a contingent result that may or may not hold. In some crazy world, the 1 gram mass might over balance the 10 gram mass. Presumably we can learn inductively that transitivity holds in our world from our experience with the balance. Repeated balancings ought to present us with instances of transitivity that we generalize inductively to all cases. If we are experimenting with bodies a, b, c and d, that evidence may look like:

> a overbalances b.
> b overbalances c.
> a overbalances c.
> a overbalances d.
> b overbalances d.
> c overbalances d.

It is intuitively clear that that this evidence presents us with an instance or particular case of transitivity and that it inductively supports transitivity. But what general account of induction can we call upon to vindicate the intuition? This evidence and the transitivity hypothesis do not fit the scheme of enumerative induction.

We could contrive to make it fit. We might say that the first three items of evidence can be summarized as "a, b and c obey transitivity." and that this is now the particular associated with the universal generalization "All triples of objects obey transitivity." We could do this, but the cost would be great. Our most primitive property has become "…obeys transitivity." The original evidence is given in terms of the more primitive property "...overbalances..." The real work in seeing that the evidence supports transitivity is to check how the various balancings combine and whether they do so in a way that conforms to transitivity. If we begin with the property "...obeys transitivity" we presume that checking has already been done and that we already know that the balancings of a, b and c conform to transitivity. Is there an extended account of inductive generalization in which this first step is covered explicitly?

Bringing the Power of Symbolic Logic to Bear

The goal of Hempel's (1943, 1945) work on confirmation was to give precise expression to the intuitions outlined above. First he acquired the additional expressive power needed for examples such as the transitivity of balancings. This he did by developing his account in a branch of symbolic logic, predicate logic. (See Box 4: Predicate Logic.) In that logic, transitivity and the evidence listed above can be expressed symbolically quite easily. If the predicate "O(x,y)" is read as "x overbalances y" then the transitivity hypothesis can be expressed symbolically as

$$(x)(y)(z)(O(x,y)\&O(y,z))\supset O(x,z)) \qquad (T)$$

which we read as saying "For all x, y and z, if x overbalances y and y overbalances z, then x overbalances z." Our evidence can be represented as:

$$O(a,b)\ \&\ O(b,c)\ \&\ O(c,d)\ \&\ O(a,c)\ \&\ O(a,d)\ \&\ O(b,d) \qquad (E_1)$$

Somehow we now need to find in the evidence E_1 a particular instance of the transitivity hypothesis T. Hempel saw a very natural way of doing this.

The Development of An Hypothesis

The core of Hempel's account is a novel definition of the notion of an instance of an hypothesis, embodied by his notion of the "development" of any hypothesis. Assume we have some hypothesis H replete with universal and existential quantifiers: it may say "for all ..." and "there exists..." in any combination allowed by predicate logic. Now imagine that we have some finite class of individuals, say {a, b, c, ...} then (informally):

> The *development of the hypothesis H with respect to the class of individuals {a, b, c, ...}* is a sentence that asserts what H would say if a, b, c, ... were the only individuals in the world.

A few examples quickly show Hempel's intent. Take the universal hypothesis (x)Mass(x)—"All things have mass." and the class of individuals {a,b}. The development of the hypothesis for this class would simply be Mass(a) & Mass(b), for that is what the hypothesis would be saying if the only individuals were a and b. Or take the hypothesis (Ex)Mass(x)—"There exists something with mass." The development of this hypothesis for the class of individuals {a, b} is just Mass(a) v Mass(b), for that is what the existential hypothesis would be asserting were a and b the only individuals.

What do we do with more complicated hypotheses like (x)((y)Knows(x,y) ⊃ (Ez)Likes(x,z))—"If everyone knows everyone then everyone likes someone."? What about something even messier like (x)((Ey)((z)P(x,y,z) v (u)(Ew)(Q(x,u,w) ⊃ R(x,y,w))))? It turns out that the highly ordered structure of predicate logic makes it possible to handle all these cases, no matter how complicated, with a few mechanical rules. All sentences in predicate logic are put together with a few simple rules of formation. For example, we can form new sentences by using v, & and ⊃ to combine two old sentences. Or we may take any formula with an x in it and append the quantifiers (x) or (Ex). Any sentence in predicate logic is simply a massive nestling together of a few simple forms like this, just as a complicated mechanical clock turns out to be the fitting together of just a few different types of gears at many scales. At any level we will always find one of these few simple structures. This rigidity enables:[43]

> *Recipe for forming the development of any hypothesis H with respect to the class of individuals {a, b, c, …}.*
>
> 1. Go to the deepest level of nestling of the sentences and apply rule A to universally quantified sentences and rule E to existentially quantified sentences.
>
> > A. Replace (x)Formula(x) by (Formula(a) & Formula(b) & …)
> >
> > E. Replace (Ex)Formula(x) by (Formula(a) v Formula(b) v …)
>
> 2. Repeat at successively higher levels of the nestling until all quantification is eliminated.

For example, in the hypothesis

(x)((y)Knows(x,y) ⊃ (Ez)Likes(x,z)),

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[43] This is as simplification of the recursive definition of Hempel's (1943) more technical paper. The simpler but more widely read Hempel (1945) does not give the full definition.

at the innermost level we find the two sentences (y)Knows(x,y) and (Ez)Likes(x,z). For the domain {a,b}, we apply A and E and recover

$$(x)((\text{Knows}(x,a)\&\text{Knows}(x,b)) \supset (\text{Likes}(x,a)\text{vLikes}(x,b)),$$

We now step up one level and apply A to recover

$$((\text{Knows}(a,a)\&\text{Knows}(a,b)) \supset (\text{Likes}(a,a)\text{vLikes}(a,b)) \;\&$$
$$((\text{Knows}(b,a)\&\text{Knows}(b,b)) \supset (\text{Likes}(b,a)\text{vLikes}(b,b))$$

The quantifiers have been eliminated and we now have the development of the hypothesis with respect to the class of individuals {a, b}.

The Satisfaction Criterion of Confirmation

The notion of the development of an hypothesis gives us precise expression in predicate logic of the notion of an instance of an hypothesis. With that definition in hand, the real work is done. The core claim of Hempel's account of confirmation is merely that an hypothesis is confirmed by its developments. Excepting formal clothing, the notion is just that invoked in enumerative induction when we infer from a particular to a generalization.

Hempel's (1945, p.37) statement of the "Satisfaction Criterion on Confirmation" defines two notions, "*direct* confirmation" and "confirmation":

> Omitting minor details, we may summarize the two definitions as follows:
> (9.1 Df) An observation report B directly confirms a hypothesis H if B entails the development of H for the class of those objects which are mentioned in B.
> (9.2 Df) An observation report B confirms a hypothesis H if H is entailed by a class of sentences each of which is directly confirmed by B.

Hempel's definition is a little more complicated than the obvious "developments confirm hypotheses."[44] Although they complicate the statement of the criterion, they extend it in two

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[44] It is also specialized unnecessarily by the mention just of "observation reports." They are defined (Hempel, 1945, p.22) as sentences that assert or deny that particular individuals have certain observable properties. The latter are given by a specified observational vocabulary that must only include observable attributes such as "black" or "taller than" but not theoretical attributes such as "[is] circularly polarized light." The specialization to observation reports is not essential to the criterion; it is not mentioned in the technically more detailed Hempel (1943).

useful ways. To use the classic example, the hypothesis (x)(Raven(x)⊃Black(x))—All ravens are black.—is confirmed by its development (Raven(a)⊃Black(a)). But that evidence sentence is not the one we expected to confirm the hypothesis. We expected the report of a black raven, (Black(a)&Raven(a)). This is no real failure of the criterion. (Black(a)&Raven(a)) logically entails[45] (Raven(a)⊃Black(a)) So, informally we might say the development of the hypothesis is already in the expected evidence sentence. Formally this is accommodated in Hempel's clause (9.1 Df) when he calls for the evidence ("observation report") to entail the relevant development. (Note the simplest case: the development entails itself.)

The first clause (9.1 Df) extended the compass of the criterion to include sentences that logically entail the development. The second clause (9.2 Df) extends the reach of the criterion in the other direction, to sentences entailed by the hypothesis confirmed. We see quickly that the extension is welcome. If we allow that (Black(a)&Raven(a)) confirms (x)(Raven(x)⊃Black(x)) then presumably we also expect it to confirm the assertion that the next raven we see—say raven b—is black. That is, we expect (Black(a)&Raven(a)) to confirm (Black(b)&Raven(b)). But (Black(a)&Raven(a)) is not the development of (Black(b)&Raven(b)) and does not entail the development, However (Black(b)&Raven(b)) is already in implicit in (x)(Raven(x)⊃Black(x)), which is directly confirmed by (Black(a)&Raven(a)). More precisely, (x)(Raven(x)⊃Black(x)) entails (Black(b)&Raven(b)). So through (9.2 Df), we can say that (Black(a)&Raven(a)) confirms (Black(b)&Raven(b)). For a more elaborate illustration of the application of the criterion see Box 5: The Satisfaction Criterion and the Beam Balance.

Complications

While these additions to the criterion are useful, they have the unfortunate effect of obscuring a very simple idea behind a screen of logical legalisms. As Hempel's ominous

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[45] In predicate logic A&B entails A⊃B. That is because A⊃B is logically equivalent to (~A)vB, which means that the "if…then…" is somewhat weaker than the "if…then…" of ordinary talk. So A⊃B will come out as true when A is false, whatever the truth value of B. In this case, there is no real harm in the weakening. Since (~A)vB is equivalent to ~(A&~B), (Raven(a)⊃Black(a)) precludes Raven(a) being true without Black(a) also being true. However (Raven(a)⊃Black(a)) will still come out as true if a is not a raven, no matter what its color.

"Omitting minor details…" suggests, the Hempel (1945) version of the criterion is itself a simplified version of the full criterion given in Hempel (1943, p. 142). The full version addresses complications that cause trouble for the simplified version. For example, the development of any hypothesis can turn out to be a tautology—an assured truth—even if the hypothesis is not. Hempel's (1943, p.141) example is (x)P(x) v (x)~P(x)—either everything is P or everything is not P. The development of the hypothesis for a class {a} is P(a) v ~P(a). This is a tautology and thus entailed by any evidence sentence at all—say Q(a). The odd result is that Q(a) directly confirms (x)P(x) v (x)~P(x). The full version of the criterion explicitly and somewhat clumsily rules this out by requiring that tautological developments can only be used in the criterion if they come from tautological hypotheses.

There are many other complications that can be handled by similar additional, tailor-made clauses, all adding to the screen of legalisms. Hempel's (1945, p. 38) describes how extra individuals can be introduced spuriously into the evidence sentence thereby defeating the satisfaction criterion.

A more interesting and serious problem is described by Hempel (1945, p.49) in the included 1965 Postscript. There are some hypotheses that require infinitely many individuals if they are to be true. As a result we are defeated if we ask what they would say if there were only some finite class of individuals in the world. They require that not to be the case. So we cannot form the development of such hypotheses for any finite class of individuals. The development turns out to be self-contradictory. The simplest example is afforded by numbers. The hypothesis is that for every number there is a bigger number. No finite class of numbers can have this property. Every such class has a greatest member for which there is no bigger member. This means that an examination of the arithmetic properties of any finite class of numbers, no matter how big, cannot confirm by the satisfaction criterion that for every number there is a greater number.

Hempel's Contracted Ambitions: A Purely Qualitative Notion…

Hempel's account of confirmation is a natural continuation of the old literature in enumerative induction. The core inductive relation is still between a particular and a generalization. However Hempel's development of the tradition came with a very significant contraction in ambitions.

The first was the notion of strength or degree. As we saw above, the older tradition held that at least in some cases a few favorable particulars could provide strong support to the generalization, although it was defeated in its attempts to give a systematic account of when the support would be weaker or stronger. Otherwise there seemed to be some agreement that an increase in the number of instances would increase the degree of support. Hempel's account forgoes the notion of degree. In the limited compass of Hempel's satisfaction criterion, confirmation is a yes-no matter. If the evidence confirms the hypotheses, whether extremely weakly or very strongly, then the relation obtains. It is, as Hempel (1943, p. 123) explains, "the more elementary, non-quantitative relation of confirmation." This relation, explicitly stripped of all notion of strength is the one that has survived prominently in the later literature on Hempel's work.

Hempel and his colleagues, Olaf Helmer and Paul Oppenheim clearly did not intend the investigation to halt there without further investigation of the degree of confirmation. The latter was to be represented by probabilities and those probabilities, they believed, could be also recovered syntactically from the form of the sentences, in the same way that the satisfaction criterion used relations between the forms to sentences to determine whether any relation of confirmation existed at all. Their Helmer and Oppenheim (1945) and Hempel and Oppenheim (1945) described in more and less technical detail how one could graft probabilities onto sentences within the language L in a way that was compatible with the satisfaction criterion of confirmation. Stripped of massive layers of formalism, the proposal reduced to a quite simple-minded counting of cases. If we are in an idealized universe in which objects are blue or not blue, what is the probability that the next object we inspect will be blue? The probability is simply given by counting the frequency of blue objects among all those examined so far. If five of ten were blue, the probability is just 5/10 = 0.5. Their principal burden was the extend this intuition to more complicated cases.

…with No Associated Method of Discovery

The second contraction in ambitions was that Hempel explicitly disavowed all hope that his account of confirmation would also comprise a logic of discovery. That latter had traditionally figured to lesser and greater degree in accounts of induction. It is present most strongly in Bacon's and Mill's methods. They were obviously intended as methods of discovery.

They coached us in how to organize our evidence so that the sought form or cause could be read from it. While the literature on enumerative induction was less clear, there is always a single, natural generalization for a particular: "Some As are B." naturally generalizes to "All As are B." Hempel's break with the tradition is decisive. Citing Popper and Einstein as his authorities (Hempel, 1945, pp. 5-6) he denounces the process of invention of scientific hypotheses as a matter of psychology lying outside rules of logic.

> …the quest for rules of induction in the original sense of canons of scientific discovery has to be replaced, in the logic of science, by the quest for general objective criteria determining (A) whether, and—if possible—even (B) to what degree, a hypothesis H may be said to be corroborated by a given body of evidence E. This approach differs essentially from the inductivist conception of the problem in that it presupposes not only E, but also H as given, and then seeks to determine a certain logical relation between them.

I would guess that the notion that one starts with the E and H given and inquires after the confirmation relation obtaining has been imported from the probabilistic explications of confirmation.

We might accept some prudent skepticism concerning the existence of a single explicit method capable of generating all our science. However the complete renunciation of methods of discovery seems unreasonably pessimistic. Mill's methods may have limited scope, but nothing in Hempel's argumentation vitiates our confidence in them as methods of discovery. Aside from citation to his authorities, the principal argument Hempel offers (1945, pp.5-6) is that evidence is expressed in the vocabulary of observation and hypothesis in the vocabulary of theory. So, he asserts, any general method must be able to bridge the two vocabularies in all conceivable circumstances, which he regards as manifestly impossible. Whether that is correct is beside the point in assessing Mill's methods. They provide the means for introducing just one theoretical term: "cause". Aside from the inductive move, they consist largely of rules for determining how to translate observation (expressed in terms of necessary and sufficient conditions) into causal claims. Their scope may be limited, but, if they work, their success is notable. Hempel's objection is also ironic. We shall shortly see that the major defect of Hempel's satisfaction criterion is that it fails to allow evidence expressed in the vocabulary of observation to confirm hypothesis expressed in the language of theory.

Nicod's Criterion and its Difficulties

In setting up the satisfaction criterion, Hempel considered others. One was inspired by Jean Nicod's writing and was a slight adjustment of enumerative induction. The case of something with property P that also turns out to have property Q confirms that having property P entails having property Q. In symbols

Nicod's Criterion:[46] $P(a)$ & $Q(a)$ confirms $(x)(P(x) \supset Q(x))$

Hempel immediately listed difficulties with the criterion. It was restricted to the confirmation of hypotheses of the form $(x)(P(x) \supset Q(x))$. Worse it could be easily fooled by a simple rearrangment of the hypothesis. $(x)(P(x) \supset Q(x))$ is logically equivalent to its contrapositive form $(x)(\sim Q(x) \supset \sim P(x))$—saying property P entails property Q is just the same as saying failure of property Q entails failure of property P. However, according to Nicod's criterion, $P(a)$ & $Q(a)$ confirms the original hypothesis but not its logically equivalent contrapositive form. That is a fatal failing. The two forms have the same content and should be confirmed or disconfirmed equally by the same evidence.

Hempel's Solution: "Conditions of Adequacy for Any Definition of Confirmation"

Hempel's remedy was to reverse course. Rather than look at existing criteria, he proceeded to list requirements that any adequate definition of confirmation must satisfy. The preceding failure of Nicod's requirement immediately suggested one condition (Hempel, 1945, p.13):

> *Equivalence Condition*: Whatever confirms (disconfirms) one of two equivalent sentences, also confirms (disconfirms) the other.

Once the idea of proceeding this way had been raised, it was easy for Hempel to collect a list of three conditions and their associated special cases (1945, pp.30-35, with similar statements in 1943, pp. 127-28). His ultimate goal was a definition of confirmation that satisfied these conditions. The general conditions were:

> ENTAILMENT CONDITION. Any sentence which is entailed by an observation report is confirmed by it.
>
> …

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[46] Correspondingly, $P(a)\&\sim Q(a)$ disconfirms $(x)(P(x) \supset Q(x))$,

CONSEQUENCE CONDITION. If an observation report confirms every one of a class K of sentences, then it also confirms any sentence which is a logical consequence of K.

…

CONSISTENCY CONDITION. Every logically consistent observation report is logically consistent with the class of all hypotheses which it confirms.

The second and third conditions admitted several special cases of interest. The most important was the equivalence condition as a special case of the consequence condition. Consider two logically equivalent forms H and H' of the same hypothesis. Their logical equivalence just means that each is a logical consequence of the other. Thus the consequence condition requires that if evidence confirms one of H and H', then it must also confirm the other. This is just the equivalence condition.

Hempel (1943) expended considerable effort to fine tune versions of his definition of confirmation until they satisfied these conditions of adequacy and this was demonstrated in a series of careful proofs.

Unexpected Legacies

Hempel's program of research was to clarify the qualitative notion of confirmation with his satisfaction criterion and use it to underpin a syntactically based, probabilistic account of confirmation. A deeper unspoken motivation lay in a tacit tenet of the dominant strands of logical positivist philosophy: we understand better if we translate into the language of symbolic logic. The legacy of his work over half a century later is quite different. His conditions of adequacy must have appeared to him as convenient, intermediate devices to propel him towards his goal. Instead they became major topics of discussion in their own right.

To begin Hempel's conditions appear entirely acceptable when considered in the abstract. Perhaps what makes them appear so is that they are a subset of the properties of logical entailment. It soon became clear that many of them are violated in natural, probability based accounts of confirmation and this became a subject of great interest. The consequence condition requires that if E confirms hypothesis H_1 and also hypothesis H_2, then it confirms their conjunction, H_1 & H_2. It turns out that we can contrive cases in the probabilistic context in which this E *contradicts* H_1 & H_2.

Hempel regarded Nicod's criterion and the equivalence condition merely as foils to ease introduction of the satisfaction criterion. Jointly, however, they already entailed a counterintuitive aspect of this theory that aroused considerable discussion. This is the famous paradox of the Ravens. What made it especially seductive is that the result would arise in any theory compatible with Nicod's criterion and the equivalence condition. Finally Hempel has assumed that a purely syntactic definition of confirmation was possible and the assumption scarcely seemed to need any discussion. Whether it was possible became a subject of long standing debate when Nelson Goodman mounted his famous paradox of the "grue" emeralds.

Taking Stock

The enduring attainment of Hempel's satisfaction criterion is that it supplies a much richer but still precise notion of the instance of a general hypothesis than found in traditional enumerative induction. It also alerted us to the series of enduring problems in induction listed above as the unexpected legacy. However it gave up on the notion of a logic of discovery; and it made no advance on what Mill had singled out as "the problem of induction"—why some are strong and some weak. Indeed Hempel and his colleagues had sought to solve the problem by combining his syntactic account with a probabilistic approach to induction, but the satisfaction criterion endured independently of that combined approach. Finally, in the next section, we shall take up what proves to be the most important limitation of both Hempel's criterion and enumerative induction as far as real examples in science are concerned: the cannot supply confirmation of hypotheses that use a vocabulary different from that of the evidence.

Box 4: Predicate Logic

Predicate logic supplies a precise symbolic language in which to express the logic of sentences that contain individuals and predicates and quantification over them.

Hempel's account of confirmation is set within a branch of symbolic logic known as predicate logic. Predicates are properties, such as "…is a planet" and "…has a mass". These are of degree one. Higher degree predicates include "…is more massive than…" (degree two) and "…is the greatest of …[list of nine numbers]…" (degree ten). Predicate logic allows for quantification over such properties—"for all individuals…" and "there exists an individual..." So it allows:

Jupiter is a planet.

Jupiter has mass.

For all x, if x is a planet then x has mass.

For all x, either it is not a planet or it has mass.

There does not exist a planet that has more mass than Jupiter.

The goal of the logic is to provide a completely unambiguous symbolic representation of sentences such as these and to reduce all questions about the logical relations between such sentences to questions asked of the rules that govern the manipulations of the symbols.

Hempel's Language L

It is beyond the present scope to develop a logic. However we will take the first step of laying out the syntax of the particular formal logic Hempel used in his theory of confirmation. This is his "Language L" of Hempel (1943, Section 2). In that language, the sentences above would be written respectively as:

Planet(Jupiter)

Mass(Jupiter)

(x)(Planet(x) ⊃ Mass(x))

(x)(~Planet(x) v Mass(x))

~(Ex)(Planet(x) & MoreMassive(x, Jupiter))

In these examples, "Jupiter" is an individual constant that we informally recognize as representing a fixed individual. More generally lower case letters early in the alphabet—"a", "b", "c" *etc*.—are used for individual constants. "Planet(_)" and "Mass(_)" are predicates of the first degree and "MoreMassive(_,_)" is a predicate of the second degree. They are more usually represented by single capital letters with number subscripts: $P_1, P_2, \ldots, R_1, R_2, \ldots$ *etc*. Individual variables are usually lower case letters from the end of the alphabet, such as x, y, … *etc*. They enter into the two quantifiers "(x)"—"For all x"—and "(Ex)"—"There exists an x." Finally there are the four sentential connectives: "~" (not), "v" (or), & (and) and ⊃ (if…then). Parentheses "(" and ")" are used to disambiguate the combinations of symbols. The import of ~, v and & is just as you might expect. ~A is true when A is false and conversely. A&B is true just in case both A and B are true. AvB is true just when at least one of A or B are true. The material implication ⊃ is a little more complicated since one might expect the "if…then" that it represents to carry some weight of necessitation. It is so defined that the truth value of A⊃B, for sentences A and B, is determined solely by the truth values of A and B individually. The outcome has been that A⊃B has been defined as synonymous with (~AvB). As a result (x)(Planet(x)⊃Mass(x)) and (x)(~Planet(x)vMass(x)) are logically equivalent.

This logic is a "first order predicate logic" since quantification extends only over individual variables. We may say "For all individuals x, …" but *not* "For all properties P, …" The latter requires a second order logic.

Box 5: The Satisfaction Criterion and the Beam Balance

The evidence of balancings does directly confirm transitivity according to the satisfaction criterion, but only when tacit assumptions in that evidence are made explicit. This shows that the criterion is a demanding taskmaster.

A Bigger Development than Expected…

As an illustration of the satisfaction criterion, we will complete the example of the beam balance introduced above. We will check whether the evidence sentence (E_1), reporting outcomes of tests with a beam balance, confirms the general hypothesis of transitivity, expressed in the sentence (T), according to the satisfaction criterion. To recapitulate, these two sentences were

$$O(a,b) \,\&\, O(b,c) \,\&\, O(c,d) \,\&\, O(a,c) \,\&\, O(a,d) \,\&\, O(b,d) \qquad (E_1)$$

$$(x)(y)(z)(O(x,y)\&O(y,z))\supset O(x,z)) \qquad (T)$$

First we must find the development of T for the class of individuals {a, b, c, d} mentioned in E_1. One might expect that development to be

$$(O(a,b)\&O(b,c))\supset O(a,c) \quad \& \quad (O(a,c)\&O(c,d))\supset O(a,d) \quad \& \quad (O(b,c)\&O(c,d))\supset O(b,d) \qquad (E_2)$$

For this sentence lists the three occasions in the evidence in which transitivity is manifested. As we would also hope, E_2 is a logical consequence[47] of E_1. Unfortunately E_2 is not the development of T for the class {a, b, c, d}. A mechanical application of the recipe for forming developments shows that it is a much larger expression. It is the conjunction of many terms of the form $(O(x,y)\&O(y,z))\supset O(x,z))$ in which the triple (x, y, z) has been replaced by all possible triples that can be formed from (a,b,c,d). There are many more triples than used in forming E_2. The triple, (a, c, b) translates into the sentence

$$(O(a,c)\&O(c,b))\supset O(a,b)$$

[47] As noted above, in predicate logic A&B entails A⊃B.

The triple (a, a, b), translates into the sentence[48]

$$(O(a,a)\&O(a,b))\supset O(a,b).$$

The development must accommodate all those extra triples. It will be a long list of 64 conjuncts since there will be 4x4x4=64 triples. With ellipses it looks like

$$(O(a,a)\&O(a,a))\supset O(a,a) \quad \& \quad (O(a,a)\&O(a,b))\supset O(a,b) \quad \&$$
$$\ldots \quad \& \quad (O(d,d)\&O(d,c))\supset O(d,c) \quad \& \quad (O(d,d)\&O(d,d))\supset O(d,d) \qquad (E_3)$$

For E_1 to directly confirm T, we must have that E_1 entails this development E_3 of T. It is quite easy to see that E_1 does *not* entail E_3. For E_1 must entail each of the new conjuncts added to it in forming E_3 from E_2; and that it cannot do. For example we added

$$(O(c,b)\&O(b,a))\supset O(c,a)$$

in forming E_3 and it is logically equivalent to

$$\sim O(c,b) \vee \sim O(b,a) \vee O(c,a).$$

This sentence is not entailed by E_1, for E_1 makes no mention of any of its three term, O(c,b), O(b,a) and O(c,a).

…is Entailed by an Augmented Evidence Sentence

An augmentation of E_1, strengthened in a natural way, does entail E_3. We simply assume that E_1 lists every overbalancing that can be asserted; all the rest fail. That is, O(a,a) is not included in E_1. Therefore its negation ~O(a,a) is the case and should be added to the evidence sentence; and so on for all other pairs.

The strengthening can be motivated by our knowledge of a beam balance. We know there are three *mutually exclusive* options when we compare objects a and b: a overbalances b, b overbalances a or they balance exactly. Thus if we find that a overbalances b, we know that it is not the case that b overbalances a. That is, if our evidence contains O(a,b), then it should also

[48] It turns out that this particular sentence is entailed by E_1, since this sentence is a tautology (i.e. always true) and tautologies are entailed by every sentence. To see that it is a tautology, note that $(O(a,a)\&O(a,b))\supset O(a,b)$ is equivalent to $\sim(O(a,a)\&O(a,b)) \vee O(a,b)$ which is equivalent to $\sim O(a,a) \vee \sim O(a,b) \vee O(a,b)$. Its second and third disjuncts, $\sim O(a,b) \vee O(a,b)$, form a tautology, as, therefore, does the entire sentence.

contain ~O(b,a). Similarly the evidence could allow that a does not overbalance itself. We might imagine object a being compared with a clone object a. By symmetry, a does not overbalance the clone. Or we might simply assert that there is no meaningful way for a to be balanced against itself. In either case, we end up adding ~O(a,a) to the evidence.

The augmented evidence sentence now reads:

$$\begin{aligned} &O(a,b) \;\&\; O(b,c) \;\&\; O(c,d) \;\&\; O(a,c) \;\&\; O(a,d) \;\&\; O(b,d) \\ &\;\&\; {\sim}O(b,a) \;\&\; {\sim}O(c,b) \;\&\; {\sim}O(d,c) \;\&\; {\sim}O(c,a) \;\&\; {\sim}O(d,a) \;\&\; {\sim}O(d,b) \\ &\;\&\; {\sim}O(a,a) \;\&\; {\sim}O(b,b) \;\&\; {\sim}O(c,c) \;\&\; {\sim}O(d,d) \end{aligned} \qquad (E^{aug}{}_1)$$

This augmented evidence sentence $E^{aug}{}_1$ does entail the development E_3. For $E^{aug}{}_1$ entails each of the new conjuncts added in the transition from E_2 to E_3. To see this, note that each added conjunct is of the form

$$((\text{antecedent}_1 \;\&\; \text{antecedent}_2) \supset \text{consequent})$$

which is logically equivalent to

$$({\sim}\text{antecedent}_1 \vee {\sim}\text{antecedent}_2 \vee \text{consequent}).$$

So to show that $E^{aug}{}_1$ entails a conjunct we need only show that $E^{aug}{}_1$ entails at least one of ${\sim}\text{antecedent}_1$ or ${\sim}\text{antecedent}_2$ for each of the new conjuncts added in forming E_3. The antecedents of each added conjunct will have a report of a self-balancing such as O(a,a) or of a balancing in reverse lexical order such as O(b,a) or O(d,b). $E^{aug}{}_1$ entails the negation of each of these and hence the truth of each added conjunct.

In sum, the augmented evidence, expressed by $E^{aug}{}_1$, entails the development E_3 of the transitivity hypothesis T with respect to the class of individuals {a, b, c, d} mentioned in $E^{aug}{}_1$. Therefore $E^{aug}{}_1$ directly confirms T by Hempel's satisfaction criterion.

7 Glymour’s Bootstrap

This condition extends Hempel's criterion in allowing assistance of a theory T in inferring an instance of an hypothesis H from some item of evidence E. The condition allows evidence expressed in an observation language to confirm hypotheses expressed in theoretical language. Its weakness is a threat of vicious circularity.

Limitations of Enumerative Induction and the Satisfaction Criterion

While Hempel succeeded in expanding the scope of enumerative induction greatly, the expansion proves still to fall far short of the way instances are used to confirm hypotheses in practical science. The difficulty lies in the narrower character of evidence, as it practically arises in science, in comparison with the hypotheses they support. We use evidence of spectrograms of sunlight—statements about the pattern of lines on a photographic plate—to support hypotheses about the chemical composition of the sun. Or we support hypotheses about the evolution of biological species by evidence of particular patterns of marks in rocks. In both cases we interpret the evidence in such a way that it is transformed from statements about observed lines or marks to statements at a more elevated level of theory: to the spectral signature of helium or to the fossilized remains of a transitional species.

Such interpretation lies outside the scope of enumerative induction. It can lead us from the assertion that "Some stars contain helium." to "All stars contain helium." But it cannot allow us to assert that this pattern of lines supports the presence of helium in the sun. Hempel's satisfaction criterion suffers the same problem. Unless "contains helium" is a predicate used in the sentence that states the evidence, it cannot confirm an hypothesis like "All stars contain helium." or the simpler "Our sun contains helium." This difficulty is usually stated in the confirmation literature by distinguishing a narrow vocabulary of observation terms such as may be used in reports of evidence: "is crescent shaped," "is reddened" *etc*. These are contrasted with terms in the theoretical vocabulary that cannot arise in reports of evidence: "is an electron," "has lower frequency," *etc*. I will set this particular distinction aside since it is not so easy to know where to set the boundary. In one context, "has lower frequency" might be deemed observational, such as when we notice a reddening in the color of light and infer directly to a velocity of recession courtesy of the Doppler effect; in another, it might be deemed theoretical, such as when we explain why noticing a reddening in the color of light led us to declare that the light has a lower frequency. For our purposes, all that matters is the unavoidable fact that evidence using one vocabulary is often taken as evidence for hypotheses that use another.

In practical science, the jump between them is routine. To the paleontologist, the particular pattern of marks that comprise Archaeopteryx are interpreted to reveal that it is the fossilized remains of an animal with both feathers and teeth, a transitional form in the evolution of modern birds. The interpretation is effected by our knowledge of the structure of teeth and feathers and the marks they leave when they are fossilized. Similarly, marks in the spectrogram of light from the sun are interpreted by our knowledge of how light emitted by various elements form distinctive patterns of lines when passed through a spectrograph.

Using Theory to Interpret Evidence: Glymour's "Bootstrap"

We have already seen in the established inductive generalization tradition, that is in Mill's methods, how some elements of theory can be used to interpret evidence. The example is narrow since it allows us to add just one notion to our vocabulary, that of cause. The central notion in Mill's methods is to interpret various patterns of presence and absence in evidence as necessary and sufficient conditions and then as causal relations. We notice that drinking tainted water is strongly associated with cholera; Mill's methods use a simple theory of causation to allow us to reinterpret that evidence in the new vocabulary of causation.

Clark Glymour's (1975, 1983) "bootstrap" account of confirmation generalizes in allowing all theories to be used in interpreting the evidence, including the very theory under test. This last element has a circular if not paradoxical air to it. The theory itself is somehow presumed in confirming it. This conjured up the familiar expression to Glymour of someone trying to lift themselves off the ground by hauling on their own bootstraps.

The Informal Bootstrap

Glymour's full statement of the bootstrap condition is very elaborate and specialized to a particular context. For our purposes and for its application to most real examples in science, a greatly simplified version will serve all our needs, as well as giving a clear understanding of the idea behind the bootstrap condition.

> *The Informal Bootstrap Condition*. Evidence E confirms hypothesis H with respect to theory T if
>
> (a) E and T entail an instance of H;
>
> (b) there exists alternative evidence E' such that E' and T entail ~H in an inference analogous[49] to that of (a).

The first clause (a) merely summarizes the extension of Hempel's account of confirmation. Hempel required that evidence E entail an instance of the hypothesis H to be confirmed. We are now allowed to use a body of theory T to aid in deducing the instance; this body provides the translation of the observational content of E into the theoretical language of H.

Further clauses are needed. Since T may contain the hypothesis H itself, the very hypothesis under test, there is a risk that the entire condition is trivialized. The inference of (a) may merely amount to

T entails H, since H is contained in T; and

H entails an instance of H, since hypotheses entail their instances.

The evidence E would play no role at all in this supposed confirmation of H. The role of clause (b) is to attempt to preclude such trivial satisfaction of the condition by requiring that the confirmation of H is not assured. The idea is that the particular content of E must play an essential role in the confirmation. If the evidence E were otherwise—say E'—then the inference of (a) ought no longer to yield an instance of the hypothesis H.

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[49] Through the vague requirement that it be analogous, it is intended that the alternative evidence E' be of the same character as E so that the resulting inference is formally similar. If E consists of statements of planetary positions used to confirm H, Newton's law of gravitation, then E' should consist of possible but unrealized positions and not, for example the negation of H itself!

The Formal Version

Glymour's (1980, pp. 130-31)[50] official version of the bootstrap condition comprises almost a page of closely written, heavily qualified text that has made it notoriously difficult to penetrate. Before proceeding to examples of the informal bootstrap condition, let us take just a moment to review how the formal version improves the informal version. It does it in two ways, but at the cost of narrowing the scope of the informal version and obscuring its content.

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[50] The central text reads:

"*Bootstrap Condition:* Let E and H be sentences, T a consistent, deductively closed collection of sentences. E confirms H with respect to T if

i. T is consistent with H&E.

ii. There is a collection of quantities $\{P_i\}$ and a set C of computations from E of values for those quantities, such that every predicate occurring nonvacuously in H occurs among the $\{P_i\}$, and all hypotheses used in the computation in C are in T, and $\{P_i\}$ is not included in any larger set of quantities that can be computed from E using T.

iii. E together with the values of $\{P_i\}$ computed from E, confirms H according to the satisfaction* condition [a version of Hempel's condition].

iv. There is a sentence E' containing only vocabulary occurring in E, and a set of quantities $\{S_i\} \subseteq \{P_i\}$, including all quantities occurring nonvacuously in H, such that the computations in C include computations of all quantities in $\{S_i\}$ from E', and no set of quantities properly including $\{S_i\}$ can be computed from E' using computations in C, and E' is consistent with the conjunction of all hypotheses (save possibly H) used in the computation of the quantities in $\{S_i\}$; and E' and the values of the quantities in $\{S_i\}$ so computed confirm ~H according to the satisfaction* condition.

v. For any two sentences H' and H^* whose nonlogical vocabulary is included in that of H and such that [it is demonstrable that] H [is logically equivalent to] H^*&H' but [it is not demonstrable that] H [is logically equivalent to] H' and [it is not demonstrable that] H [is logically equivalent to] H^*, condition i-iv are satisfied if H is replaced in them by H' and H^*."

First, it provides a precise reading of the requirement that the inference in (b) be "analogous" to that of (a). The formal version is set within first order predicate logic. Evidence is given as what Glymour calls "values of quantities." They are simple atomic sentences like P(a) and Q(b) that attribute properties like P and Q to individuals a and b, or their negation, ~P(a), *etc*. One can then use sentences deduced from theory T to compute further quantities. For example, from P(a) and the sentence (x)(P(x) ⊃ R(x)), we arrive at R(a). The sequence of inferences is called a "computation" and must have a tree structure, which precludes closed loops. The computation is required to generate a set of quantities that then entails an instance of the hypothesis H. The vague demand of an analogous inference in clause (b) is replaced by what amounts to the precise demand for an inference in which the same computation are used but with evidence that differs in replacing some quantities by their negations.

Second, additional clauses block candidate bootstrap confirmations that are defective on intuitive grounds. For example, they require that E be consistent with T&H, for if they are not, the contradiction arising entails, in standard logic, all propositions and thus make it possible for evidence E that contradicts T to confirm any H within T. Similarly the counterevidence E' may succeed is disconfirming the hypothesis merely because it contradicts the interpreting theory T, so consistency of E' and T must also be demanded (excluding consistency with H alone). One can also tack on irrelevant hypotheses. If E (informally) bootstrap confirms H, then it will also (informally) bootstrap confirm H&K, where K is some suitably chosen irrelevant hypothesis that we might add into T. This irrelevant tacking on is blocked by the fifth condition that in effect requires any such K to be independently bootstrap confirmed.

Adding a few such clauses to block obvious problems is certainly worthwhile as long as there is a natural limit. Hempel, for example, augmented his satisfaction criterion with extra clauses until it provably satisfied his list of desiderata. With the formal version of the bootstrap condition, there seems to be no such limit. New clauses have been added as *ad hoc* responses each time a troubling counterexamples has been thought up. There is no assurance that the additions will stop. Yet with each new clause the bootstrap condition become more restricted in its scope and unacceptably so, as I will suggest below.

Instances of Hypotheses as Values that Satisfy Equations

Clause (a) mentions an "instance of H." All that is intended is our informal notion; roughly speaking, an instance of H is what H asserts when specialized to some narrow domain, such as a restricted class of individuals. In the precise context of the full definition of the condition, this notion of instance is explicated by Hempel's theory. An instance of an hypothesis is its development for some class of individuals. While the informal notion is vaguer, there is one clear and important case that commonly arises in bootstrap confirmation. An equation in a scientific theory can usually be read as a universal generalization and the particular value sets that satisfy it are its instances.

For example, modern cosmology proceeds from an observationally based result introduced by Edwin Hubble and now known as Hubble's law. On cosmic scales, stars are grouped into galaxies and these galaxies are observed to recede from us. Hubble's law tells us that this velocity of recession is directly proportional to the distance to the galaxy. The equation that expresses this law is

$$v = H\,d$$

where v is the velocity of recession of the galaxy (km/sec), d is the distance to the galaxy (Mparsec)[51] and H is the famous Hubble constant. Hubble's original value for the constant was around 500 km/sec Mparsec. One can read the equation as a universal generalization over all galaxies. In words

For all galaxies g, the velocity v of recession of g
is equal to H times the distance d of galaxy g from us.

So an instance of the law will be a set of values for v, H and d that satisfy the law pertaining to some galaxy. For example, we might have some galaxy 2 Mparsec distant with velocity of recession 1000 km/sec. Combined with a value of the Hubble constant of 500 km/sec Mparsec, we now have an instance of the Hubble law

v = 1000 km/sec   H = 500 km/sec Mparsec   d = 2 Mparsec

[51] A parsec is an common unit of distance used in astronomy. It is the distance from which the mean radius of the earth's orbit of the sun subtends one second of arc; that is, about 3 1/4 light years. A megaparsec (Mparsec) is a million parsecs.

Illustration of the Bootstrap: Hubble's Law

Hubble's law provides a simple illustration of the application of the bootstrap condition. (I have patterned it after the introductory example pertaining to the ideal gas law that Glymour (1975; 1980, Ch. V) uses.) Hubble's law is based on a growing body of observation of the velocities and distances to galaxies. Here is a sample of the data derived from one of the earliest papers on the law, Hubble and Humason (1931).[52] These authors grouped galaxies according to the clusters in which they belonged and reported averages for each cluster.

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[52] This paper greatly extended the number and distance of galaxies used to underpin the relation beyond those of the earlier Hubble (1929). The velocities of recession were easier to determine, being read from red shifts in the light from the galaxies. The determination of the distances to the galaxies was far harder. It employed a problematic estimation of the intrinsic brightness of a galaxy, so that its distance could be inferred from a comparison with its apparent brightness. The data in the text were derived from Hubble and Humason (1931, p. 74, Table IX). Hubble and Humason did not report the distances. Instead they reported the apparent magnitude m for each average of galaxies, presuming that the intrinsic brightness corresponded to an absolute magnitude of –13.8 (e.g. see p. 56, p. 76). Presuming the same absolute magnitude for all cases, I converted the apparent magnitudes directly into distances d via the formula $\log_{10}d = 0.2m - 0.2M + 1$, which turns out merely to express an inverse square dilution of apparent brightness with distance. Hubble did not employ the statistically naive procedure described in the text. He fitted a straight line by least squares regression to the data presented as logarithms of distances and apparent brightnesses and demonstrated that the curve fitted corresponded to the linear relation.

Galaxies in cluster	Velocity v of recession (km/sec)	Distance d (Mparsecs)	Computed H = v/d (km/sec per Mparsec)
Virgo	890	1.820	489.1
Pegasus	3810	7.244	525.9
Pisces	4630	6.918	669.2
Cancer	4820	9.120	528.5
Perseus	5230	10.965	477.0
Coma	7500	14.454	518.9
Ursa Major	11800	22.909	515.1
Leo	19600	36.308	539.8
Isolated nebulae I	2350	3.311	709.7
Isolated nebulae II	630	1.202	524.0

The evidence E lies in the ten reported pairs of values of velocity v and distance d. The final column displays a natural way that we might use the data to check Hubble's law. We compute the value of H that would correspond to each pair of velocity-distance data. If Hubble's law is true we expect to get the same result for each pair, somewhere around 500 km/sec Mparsec. Within reasonably limits of error we do find the expected values. Hubble's law is confirmed. (The weight of the data favored a value greater than 500 and Hubble and Humason revised their best estimate of H up to 560 km/sec Mparsec.)

This use of the data is a bootstrap confirmation. Our original data were velocity-distance pairs. We used an element of theory to compute a theoretical term, the Hubble constant H, for each pair. That element of theory is Hubble's law itself, written as H=v/d. We have then inferred to a set of instances of Hubble's law, where H has a value of roughly 560:

$$v = 890 \quad H = 489.1 \quad d = 1.820$$

$$v = 3810 \quad H = 525.9 \quad d = 7.244$$

etc.

Clause (a) is met: the evidence (pairs of d,v data) in conjunction with theory (the law H=v/d) entail instances of the hypothesis. Clause (b) is satisfied since almost any other data set would not conform to the linear relationship; that is, it would contradict the hypothesis.

Benign Circularity: the Hypothesis under Test is Not used to Interpret the Evidence

This rather simple example illustrates the novel features of the bootstrap condition. The original data alone cannot supply instances of the hypothesis until they are interpreted with the help of theory. That interpretation introduces theoretical terms, or at least terms more distant from observation than the original data. The original data pertain to what we see of individual galaxies. The Hubble constant H is a property of the universe as a whole and one of fundamental importance. In big bang cosmology, the Hubble age is a good estimate of the time elapsed since the big bang and it given simply by 1/H.

The example also illustrates the circularity that earns the bootstrap its name. We take the success of the bootstrap to confirm the very law used in the interpretation of the data. The fear is that there is some kind of harmful circularity in this procedure. We presume the very law that we confirm. While such presumption can certainly vitiate claims of confirmation, in this case it does not and that it does not is intuitively clear. The procedure of checking that the various pairs of v and d yield the same ratio v/d is a natural if statistically unsophisticated way of testing Hubble's law. The reason it is unobjectionable is that the linearity supposed by Hubble's law is not imposed upon the data when the law was used to interpret the data. All that we use is a part of the law, that part that tells us that the Hubble constant H is *defined* as v/d. We do not use the part of the law that asserts that v/d will have the same constant value for all galaxies. So the interpretation of the data via Hubble's law does not transfer the essential empirical content of the law to the data. It merely transfers a definition. The empirical content of the law, its linearity and the value of H, then emerges when we find that our values of H are roughly the same.

A viciously circular use of the law would arise if our data consisted merely of distances and we used Hubble's law to compute the velocity of recession and then H. This case of vicious circularity would be ruled out by clause (b), for we cannot find a set of distances to be used in an analogous inference that would end up contradicting the law.

The success of clause (b) in this one case is comforting. However it is far from clear at this point that the clause is strong enough to preclude all cases of harmful circularity. We will see a case below in which the essential content of the hypothesis under test is used to interpret the evidence in a bootstrap confirmation. I will urge, however, that the circularity is excusable.

Inferring Forces from Motions

A more substantial illustration of the bootstrap criterion is afforded by one of the most famous arguments from evidence in all science, Newton's argument for the inverse square law of gravitation in Book III of his *Principia*. Newton argues from the evidence of the orbital *motion* of planets and moons to the *forces* that maintain these motions. The evidence consists in a summary of the observed positions of planets and moons. The hypothesis concerns the direction of gravitational force—towards a single center; and how the gravitational force dilutes with distance—with the inverse square of distance from the center of attraction. The principal problem is to reinterpret the motions in terms of forces. That proves easy to do. Once one has the trajectories given as a formula, it is relatively straightforward to infer to the velocities and then accelerations. Newton's second law of motion—"force = mass times acceleration" in modern language—allows the final step, the reinterpretation of accelerations as forces.

We can look to Galileo's law of fall for a simple illustration of the reinterpretation. That law tells us that the distance bodies fall terrestrially from rest is proportional to the square of the time of the fall. Generations of students have found in their laboratory assignments that a free falling body will conform roughly to the relation:

$$\text{distance fallen (feet)} = 16 \text{ time}^2 \text{ (seconds}^2\text{)}$$

If a motion obeys this relation, we readily recover that[53]

$$\text{velocity} = 32 \text{ time}$$

$$\text{acceleration} = 32$$

using the definitions that velocity is the time rate of change of distance and acceleration time rate of change of velocity for reinterpretation. The notable simplification is that time has been eliminated. The acceleration is a constant independent of time. Finally we use Newton's second law to reinterpret the result for acceleration. The law in inverted form tells us that "acceleration = force/mass." Thus the body is acted on by a constant force per unit mass of g = 32 ft/sec^2. We

[53] These results are most easily recovered from the calculus. If $s = 16t^2$, then $v = ds/dt = 32t$ and $a = d^2s/dt^2 = 32$, where s, t, v and a are distance, time, velocity and acceleration.

have inferred to an instance of the Newtonian law that a terrestrial falling body of mass m is acted on by a uniform downward force of mg.

Newton's Argument for Central Forces…

Newton's overall strategy is to carry out the analogous argument to proceed from the motions of planets and moons to the forces. What complicates the argument is that planets and moons move in a two dimensional plane, where Galileo's bodies fell in a one dimensional straight line. Their accelerations vary from place to place in their orbits. He must determine both the magnitude and direction of the force acting.

It turns out the two problems can be addressed separately. First Newton shows that each of the celestial bodies under consideration move under the action of a central force, a force directed to a single point. Newton's inference is from the motions of known celestial bodies to an instance of the hypothesis that all celestial bodies move under the action of a central force. It is an *instance* of the hypothesis since the full hypothesis is taken to apply to other celestial bodies not mentioned in the evidence, such as comets, and also to hypothetical further planets and moon that could occupy vacancies in the solar system. The induction is a bootstrap induction because Newton must use elements of his broader theory to derive the instances. His derivation depends upon his identifying the characteristic signature of a central force in the motions of the planets and the moons of Jupiter, Saturn and the Earth: the motions conform to the "area law." (See Box 6: Newton's Arguments in *Principa*.) So Newton's argument is:

Evidence: Motions of the planets and moons of Jupiter, Saturn and the Earth obey the area law.

Theory: Area law holds if and only if the motion is governed by a central force.

Instance of hypothesis: All celestial bodies are governed by central forces.

Clause (b) is satisfied since any motion not satisfying the area law would contradict the hypothesis.

…that Obey an Inverse Square Law

Newton's demonstration of the magnitude of the force has a similar structure.

To reduce the complexity of the argument, Newton avails himself of a convenience in planetary and lunar orbits: it turns out that they are all very nearly circular.[54] In so far as this approximation holds, the acceleration of each planet and moon is of constant magnitude and always directed towards the center of force. Newton reports that the planets and the moons of Jupiter and Saturn all obey what we now know as the harmonic law or Kepler's third law. This proves to be the signature in the motions of a force that dilutes with the inverse square of distance.

Let us consider Newton's argument as it applies to the planets. According to the harmonic law, their radii R and periods T are related by the proportionality $R^3 \propto T^2$. Newton had shown that this harmonic law was the characteristic signature in the motions of an inverse square law. (See Box 6: Newton's Arguments in *Principa*.) So he could infer that forces acting on the planets dilute with the inverse square of distance. Once again he has inferred to an instance of the general hypothesis since he deduced only what the forces are for the few planets orbiting the sun and not those acting on possible but non-existent planets. Newton's argument is:

Evidence: Motions of the planets obey the harmonic law (approximately)

Theory: Harmonic law holds if and only if the motion is governed by an inverse square force.

Instance of hypothesis: All objects orbiting the sun are governed by an inverse square force law. (approximately)

Once again clause (b) is satisfied since any other near circular motion not satisfying the harmonic law would contradict the inverse square law.

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[54] Newton has been unfairly criticized by unsympathetic commentators for using the approximation. There is no problem in using it, as long as one realizes that the immediate result is only an approximation. The real burden is to determine how closely the result approximates the real result. There, as we shall see later, Newton had an ingenious and sensitive method for affirming that the approximation is extremely good. See Box 7: Newton's Arguments in *Principia*: From the Approximate to the Exact Inverse Square Law

Newton's Moon Test

There is a second famous example that turns out to be a case of bootstrap confirmation. (See Box 6: Newton's Arguments in *Principa*.) In his celebrated "moon test," Newton contemplates the terrestrial force of gravity such as causes apples to fall from trees; and the force directed to the Earth's center that maintains our moon in orbit. His claim is that the two forces are the same. This sameness entails the hypothesis that the acceleration due to gravity and due to the astronomical force must be the same at all altitudes. His goal is to confirm this last hypothesis and thereby his original claim. To do it, Newton infers to an instance of the equality of the two accelerations. He computes the acceleration due to gravity at the Earth's surface and the acceleration due to the astronomical force at the Earth's surface. To get the latter, he takes the acceleration due to the astronomical force at the orbit of the moon and, assuming that the force is diluted by an inverse square law with distance, computes its strength at the Earth's surface. The result is a quite good agreement in the two accelerations; that is, an instance of the hypothesis that the accelerations agree at all altitudes. In sum clause (a) is satisfied by the argument:

> Evidence: acceleration of bodies at Earth's surface and of the moon
> Theory: Inverse square dilution of astronomical force on the moon
>
> ---
>
> Instance of hypothesis: Equality of gravitational and astronomical accelerations

Clause (b) is satisfied since clearly almost any other combination of accelerations would not yield the equality.

Much more can be said of the bootstrap condition in the context of Newton's arguments. See Glymour (1980, Ch. VI) and Laymon (1983).

The Circularity of the Moon Test…

This example, more strongly that the others, illustrates the circularity admitted by the bootstrap condition. The theory employed is that the astronomical force acting on the moon dilutes with distance as an inverse square law *and that this law persists all the way to the Earth's surface*. While Newton had weighty arguments for his inverse square law, they depended upon an examination of the motion of bodies in the celestial realm. We might now automatically presume that a law that holds in the celestial realm would also hold terrestrially. However the older tradition of a radical difference between terrestrial and celestial physics had persisted even within the century prior to Newton's *Principia*. Newton, along with Galileo, Descartes and others, were in the process of dismantling this tradition.

The success of Newton's moon test depends upon this presumption that the inverse square law could reach from the celestial realm of the moon down to the terrestrial realm of falling bodies. Newton has shown his evidence bootstrap confirms his hypothesis with respect to theory—but this is all for naught if we have no reason to believe this most relevant part of the theory. Newton clearly thought the moon test a great success and that judgment surely accords with modern intuitions, or at least it does with my modern intuitions. So presumably we do have some reason to believe the presumption. Indeed I would say that the moon test itself supplies them. The very fact that the test works so well suggests that it was entirely appropriate to project the inverse square dependence down to the surface of the Earth. Therein lies the circularity. The moon test needs the presumption of the inverse square law to allow interpretation of the evidence. The success of the moon test itself provides reason to accept the presumption.

…is Expressed in a Second, Hidden Bootstrap Confirmation

How can the moon test itself supply those reasons? We need look no further than the bootstrap condition. The same evidence provides a bootstrap confirmation of the crucial presumption. To see how, let us label the hypothesis of equality of acceleration due to gravitational and astronomical forces the "equality hypothesis," H_{eq}; let us label the hypothesis that the inverse square law extends from the moon to the Earth the "inverse square hypothesis," $H_{inv\ sq}$; and let us label the known accelerations of terrestrial bodies and the moon the "accelerations," Acc. We have seen so far that:

Acc bootstrap confirms H_{eq} with respect to $H_{inv\ sq}$.

I claim that in addition we have closely analogous confirmation[55]

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[55] In brief, here is how it works. The evidence for this bootstrap is as before: in one minute the moon falls 15.0934 Paris feet towards the Earth and in one second bodies above the surface of the Earth fall 15.0957 Paris feet. If we assume the hypothesis of equality H_{eq}, it now follows that the moon, relocated on the Earth's surface, would fall 15.0957 Paris feet in one second, which (using Galileo's law of fall) would become 60^2x15.0957 Paris feet in one minute. So, to very good approximation, the acceleration of the moon dilutes by a factor of $(1/60)^2$ as it moves from

Acc bootstrap confirms $H_{inv\ sq}$ with respect to H_{eq}.

...and Excused

This pair of bootstrap confirmations express more formally the circularity one intuitively senses in the moon test. I do not think this is a harmful circularity. Rather what this suggests is that the two hypotheses stand together. The success of one brings that of the other—this directly through the bootstrap confirmation relations indicated. And the failure of one brings the failure of the other—this because a failure of one blocks the bootstrap confirmation of the other. This means the evidence confirms the pair. As with all induction, the possibility remains that the thing confirmed is false. But if it is, we would expect both hypotheses in the pair to be false.

Once this coupled structure is made apparent, one might fear that other competing hypotheses might also enjoy bootstrap confirmation in quite analogous inferences. This is the case. We could replace the two hypotheses above by alternatives: the "inequality hypothesis" H_{ineq} that acceleration due to gravitational forces is 60^2 times the acceleration due to astronomical forces at the same place; and the "constant hypothesis" H_{const} that the astronomical force has a constant magnitude in the realm that extends from the moons orbit to the surface of the earth. One can see very quickly that Newton's evidence for the moon test bootstrap confirms H_{ineq} with respect to H_{const}; and that it bootstrap confirms H_{const} with respect to H_{ineq}. So by formally identical bootstrap confirmations, Newton's moon test evidence confirms the pair of hypotheses H_{eq} and $H_{inv\ sq}$; and it confirms the pair incompatible with it H_{ineq} and H_{const}.

While this seems troubling at first, it is really a quite common feature of inductive generalization. The one item of evidence can often confirm hypotheses that are incompatible. Perhaps the most familiar example lies in curve fitting. A number of data points can be taken to confirm any curve that passes through them.

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the earth's surface to its true orbit. Since that orbit is roughly 60 Earth radii away from the center of the Earth, we have inferred to an instance of the hypothesis that the inverse square law extends to the surface of the Earth; that is, we used H_{eq} to infer from the same evidence to an instance of $H_{inv\ sq}$.

Nonetheless we do prefer Newton's induction and not the alternative. There are many reasons for this. One is that we have prior inclinations that favor H_{eq} over H_{ineq}; and that favor $H_{inv\ sq}$ over H_{const}. For H_{eq} allows us to replace quite plausibly two separate forces that appear to act in very similar ways by just one; and $H_{inv\ sq}$ just says that the force of gravity depends on distance near the Earth in the same way as Newton showed it did in the realms of planetary orbits and of the moons of Jupiter and Saturn. Moreover the success of the moon test with Newton's hypotheses would be surprising if they were false. The two coupled bootstrap confirmations of H_{eq} and $H_{inv\ sqt}$ allows only two possibilities, that either both are true or both are false. If they are both false, then we must be astonished that the numerical test numbers still worked out so perfectly. So by elimination the truth of both seems the only remaining viable option. We cannot run a similar argument with H_{ineq} and H_{const}. There is no surprise that the confirmations succeeded. We assured the success by setting the value of the multiplier in H_{const} to 60^2. If we had different data, we would have used a different multiplier.

While these two considerations might lead us to prefer Newton's induction to the alternative, the bootstrap condition itself cannot underwrite the preference, for the bootstrap confirmations of the pairs are formally identical. The first consideration just is that we have *other* evidence favoring Newton's hypotheses. The second returns to the perennial problem of enumerative induction and the inductive relations modeled on it: we have no grounds within the accounts given for judging one induction to be strong and another other weak (beyond simple counting of instances). While the intuition that Newton's induction is stronger is clear, it is not vindicated by the bootstrap condition, just as the original scheme of enumerative induction cannot tell us why some enumerative inductions are strong and others weak.

Circularity in its Strongest Form

The two coupled bootstrap confirmations of the moon test can be put into their most provocative form by combining them into one:

Acc bootstrap confirms (H_{eq}& $H_{inv\ sq}$) with respect to (H_{eq}& $H_{inv\ sq}$).

We now have an admissible confirmation in which an hypothesis is bootstrap confirmed by means of itself as the interpreting theory and in which the interpretation seems to fully exploit the content of the hypothesis in the interpretation. Compare this with the earlier illustration of benign circularity in the Hubble law in which the empirical content of the Hubble law was not used at all in the bootstrap confirmation.

Circularity Again: Spurious Relevance

We have now seen two examples of circularity in bootstrap confirmation. The first, in relation to Hubble's law was benign; the second in relation to Newton's moon test was excusable. A third form of circularity proved to be inexcusable and has dogged the bootstrap condition from its early days. In these last cases, the problem is not that success of the confirmation is assured. It is that the wrong thing is confirmed. In Glymour's original developments of the bootstrap condition, one of its most attractive features was that it allowed an item of evidence to support a quite particular hypothesis buried deeply within a theory. Thus the account promised to refute the holistic view of evidence, which holds that evidence can only confront the entirety of a theory. One consequence of this view is a pessimistic limitation on the reach of evidence known as the "Duhem-Quine thesis". If evidence can only confront theories as a whole, a particular hypothesis can always escape refutation by evidence if other parts of the theory are appropriately adjusted.

Glymour proceeded from strong intuitions that evidence can support individual hypotheses in a theory and is irrelevant to others. So the evidence that planets obey an area law supports the hypothesis of a central force but is not relevant to the inverse square law. In a series of counterexamples initiated by Christensen (1983), the evidence was shown to confirm irrelevant hypotheses under the bootstrap condition.

An Example

The following example is quite representative of the bootstrap confirmation of irrelevant hypotheses. The theory T will consist of two hypotheses

$$H_{WE}: (x)\ (W(x) \supset E(x))$$

$$H_{WM}: (x)\ (W(x) \supset M(x))$$

The evidence E will be just

$$E: W(a)\ \&\ E(a)$$

To begin, E confirms H_{WE}. It does this even without using the bootstrap condition, for E entails $W(a) \supset E(a)$, which is just an instance of H_{WE}. On quick reflection one sees that E also bootstrap confirms H_{WE}; any vacuous theory T is sufficient to meet the demands of the two clauses. What we would *not* expect is that this evidence E would also confirm H_{WM}. That would violate our simplest intuitions on relevance. Finding an individual that has both W and E might confirm that all W's are E's; but how could it possibly confirm all W's are M's? This last irrelevant confirmation is just what the bootstrap condition allows. To see how, notice that theory T contains the hypothesis H_{WEM} as a consequence of H_{WE} and H_{WM}, where[56]

$$H_{WEM}: (x)\ (W(x) \supset (E(x) \equiv M(x))\ )$$

We now find E bootstrap confirms H_{WM} through the inference[57]

Evidence E: $W(a)\ \&\ E(a)$

Theory H_{WEM}: $(x)\ (W(x) \supset (E(x) \equiv M(x))\ )$

Instance of H_{WM}: $W(a) \supset M(a)$

Clause (b) is satisfied since alternative evidence $W(b)\ \&\ {\sim}E(b)$ would refute H_{WM}.

No Easy Escape

The natural reaction is to try to block such irrelevant confirmation by adding extra clauses to the conditions a bootstrap confirmation must satisfy. All such efforts face quite substantial difficulties. There are a myriad of ways that irrelevant confirmation can arise and proposals for tightening the conditions tend to respond in an *ad hoc* way to one or other example of irrelevant confirmation. More seriously there is a real danger of compromising the bootstrap condition with these extra clauses. The point of the bootstrap condition is to allow evidence in one vocabulary to confirm hypotheses expressed in another, so we must expect the interpreting theory to introduce terms not present in the evidence. They may be introduced in an entirely

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[56] For $\equiv$ read "if and only if." $A \equiv B$ is defined as $(A \supset B)\ \&\ (B \supset A)$. The derivation of H_{WEM} is simple. $H_{WE}\ \&\ H_{WM}$ entails $(x)\ ((W(x) \supset E(x))\ \&\ (W(x) \supset M(x)))$ which entails $(x)\ (W(x) \supset (E(x)\ \&\ M(x)))$. But $E(x)\ \&\ M(x)$ entails $({\sim}M(x) \vee E(x))\ \&\ ({\sim}E(x) \vee M(x))$ which is $(M(x) \supset E(x))\ \&\ (E(x) \supset M(x))$ which is $(E(x) \equiv M(x))$.

[57] The details: from H_{WEM} we have $(W(a) \supset (E(a) \equiv M(a)))$, which, with E, gives us $E(a) \equiv M(a)$ and then $W(a)\ \&\ M(a)$. This finally gives $W(a) \supset M(a)$.

benign way as a definition free of empirical content. More commonly they are introduced through factual hypotheses of more of less complexity that might make it extremely hard to see whether some illicit circularity is buried deep within their structure.

Glymour's initial reaction (1983) to the examples was to find a new clause that precluded hypotheses like H_{WEM}.[58] The problem is that we should not seek to preclude hypotheses like it. For there are clear cases in which such an hypothesis is quite appropriately used in bootstrap confirmation.[59]

To see such a case, we need only put a physical interpretation on the predicates W, E and M:

W(x) = "x is water."

E(x) = "x expands on freezing."

M(x) = "x melts under pressure."

So now the evidence E tells us that individual a is a sample of water that expands on freezing. This is a quite unusual property of water. Virtually universally, other liquids contract on freezing. Finding one sample of water that expands on freezing certainly confirms that all do.

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[58] The new clause is: "For all i, H does not entail that Ti [the theoretical results invoked in the i-th computation used in deriving the instance of the hypothesis] is equivalent to any sentence S whose essential vocabulary is properly included in the essential vocabulary of Ti." (Vocabulary is inessential to a sentence if the sentence is logically equivalent to another that does not have the vocabulary.) The clause rules out the example at hand. The hypothesis under test H_{WM}: (x) (W(x) ⊃ M(x)) entails that the interpreting theory H_{WEM}: (x) (W(x) ⊃ (E(x) ≡ M(x))) is equivalent to H_{WE}: (x) (W(x) ⊃ E(x)), a sentence with smaller essential vocabulary

[59] For another example, take the case of fundamental particles, such as electrons, which may have half integral spins 1/2, 3/2, … or integral spins 0, 1, 2, …; and their statistical-thermal properties may reveal that they are fermions or bosons. To learn that some particular electron has spin 1/2 might confirm all have spin 1/2. But would it confirm that all electrons are fermions? It would if one is allowed theoretical results to interpret the evidence. Pauli's celebrated spin-statistics theorem in quantum field theory tells us that half integral spin particles are fermions and that integral spin particles are bosons.

But can it also confirm that samples of water melt under pressure? If one knows no thermodynamics, the prospect seems dubious, an example of irrelevant confirmation if it is admitted. However James Thomson (brother of the famous William Thomson, Lord Kelvin) discovered an early form of a remarkable result in 1849.[60] If a substance has the unusual property of expanding on freezing then it must also have the equally unusual property of melting under pressure, on pain of violation of the second law of thermodynamics. Thus if we believe the thermodynamics, finding a sample of water that expands on freezing entails that it also melts under pressure and thereby confirms the hypothesis that all samples of (frozen) water melt under pressure. That is, what first looked like an example of irrelevant confirmation turns out to be entirely relevant. James Thomson's result in thermodynamics is just $(x)(E(x) \equiv M(x))$ which entails the hypothesis H_{WEM} used in as the interpreting theory in the above bootstrap confirmation.

The Range of Circularity: Restrictions Balance Security against Scope

Perhaps such restrictive conditions are the only escape. However the cost will be to preclude important and interesting bootstrap confirmations. We have seen how the degree of circularity can vary from the benign to the quite aggressive. At one extreme we have cases such as the Hubble law above in which the circularity proved to reduce to the introduction of a mere definition: that $H(v,d)=v/d$. Definitions need not always look like definitions. We might merely write $v=Hd$, which will amount to a definition of H as long as we do not demand its constancy.

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[60] James Thomson's (1849) version of the result was given within the caloric based form of thermodynamics as originally developed by Carnot and in which there was no second law of thermodynamics. When translated into the modern form of thermodynamics developed by his brother William and by Clausius, the result is a consequence of the second law. It is read most quickly from a form of the Clausius-Clapeyron equation adapted to ice/water. If T is the melting point of ice at pressure P, then $\frac{dT}{dP} = \frac{\Delta V\, T}{H}$, where H is the latent heat of ice and ΔV is the increase in specific volume of ice on melting. We read immediately that if ice contracts on melting so that $\Delta V<0$, then $dT/dP<0$, so that the melting point T of ice decreases as the pressure P increases; and conversely.

Similarly, in examples pertaining to Newton and gravitation, the transition from net acceleration to force acting per unit mass is a definition, although empirical content rapidly enters once we presume any further property for the force. At this same extreme are the many, important cases in which the empirical content of the interpreting theory is independent of the hypothesis under test. The motion of the Earth obeys the area law. Using the interpreting theoretical result that such motion is associated with a central force, Newton infers to the hypothesis that the Earth's motion is governed by a central force, an hypothesis quite independent of the interpreting theory. Again, if we find the characteristic spectral lines of hydrogen in the spectrum of light from the sun, we readily interpret the lines to mean that the emitter contains hydrogen. The atomic theory that allows this interpretation is independent of the hypothesis under examination, that the sun contains hydrogen.

At the other extreme we saw that Newton's celebrated moon test is expressible as a bootstrap confirmation that displays the strongest form of circularity: the essential content of the hypothesis is fully used to interpret the evidence that confirms the hypothesis.

One can easily find intermediate degrees of circularity. Even if the hypothesis and interpreting theory are not independent, there are degrees. In the benign bootstrap, we introduced Hubble's quantity H merely as a definition $H(v,d)=v/d$, without supposing any constancy— $H(v,d)$ could be a function of v and d. We might introduce some of the content of Hubble's law by presuming H is a constant independent only, say, of the value of v, or only, say, of the value of d.

A restrictive condition will divide the range at some point. If the division is admits mostly benign circularity, it is at the cost of eliminating more aggressive but sound bootstrap confirmations; if the division is more permissive, it will admit more aggressive, improper cases. There has been considerable discussion of these and related problems of bootstrap confirmation. See Earman (1983; Part I), Zytkow (1986), Earman and Glymour (1988), Christensen (1990), Mitchell (1995), Madison Culler (1995),

The Final Assessment

The bootstrap condition must remain an important scheme in any account of inductive inference. For better or for worse scientists do use their theories to interpret evidence and thereby arrive at instances of hypotheses. There are many cases that are surely benign, such as when the interpreting theory merely introduces definitions of theoretical terms or it introduces empirical content essentially distinct from the hypothesis under examination. Others are riskier. It is entirely possible that we introduce a vicious circularity so that the confirmation of the hypothesis is an illusion; or that we overinterpret the evidence so that we end up confirming irrelevant hypotheses. We would hope for some formal condition that would shield us from such error. At present we know of none that does not at the same time excessively restrict inductive practice. Until such a condition arises, we will need to proceed as we always have. Scientists will continue in clear conscience to carry out induction successfully without a detailed warrant from philosophers. Generally it is intuitively evident when an illicit circularity arises so that the egregious cases are easy to avoid. These intuitions are strong: they underpin all the above judgments of whether this or that form of circularity is benign or not. My hope is that such strong and apparently systematic intuitions do have a principled foundation and that we will eventually find it.

How poorly does this leave bootstrap confirmation? Its successful implementation requires an check based on intuitive sensibilities with no formal rule. That is not new. The entire tradition of inductive generalization harbors a serious problem of exactly this form already. Sometimes a few instances provide very strong support for an hypothesis; sometimes they supply the weakest support. We have no principled way to decide which. We allow intuitions to decide. This problem remains in bootstrap confirmation. After the theoretical interpretation is over, a bootstrap confirmation delivers an instance of an hypothesis and we must let our intuitions guide us on how strongly that instance supports the hypothesis. In dealing with the problem of circularity, it would be nice if we did not need to call on these intuitions. However to insist that we cannot is to demand more rigor in one part of the condition than we have achieved in another. That is an excessive demand.

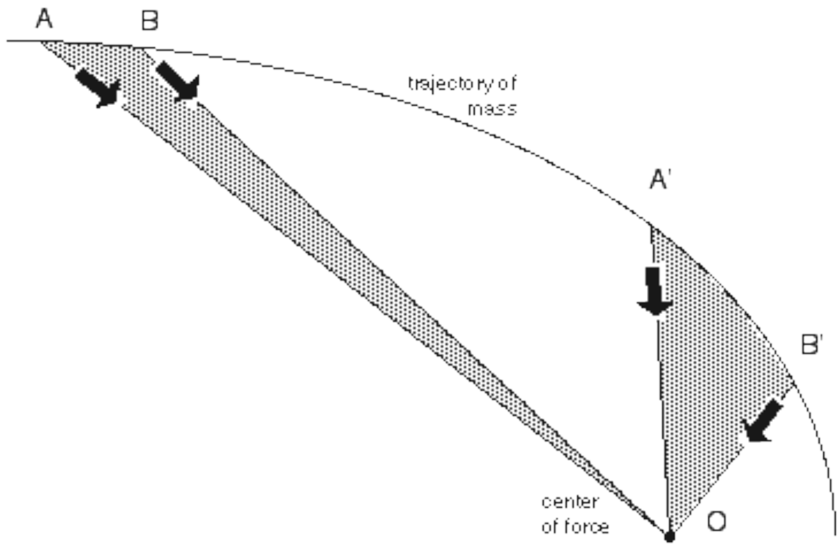
Box 6: Newton's Arguments in *Principia*

Newton develops mathematical theorems in Book I of Principia *that enable him in Book III to infer from the observed motions of the planets and their moons to the inverse square force law that governs them. The moon test demonstrates that force is the same as terrestrial gravity.*

Published in 1687 (3rd edition, 1726), Isaac Newton's *Principia*, his *Philosophiae Naturalis Principia Mathematica* or *Mathematical Principles of Natural Philosophy* is the greatest single scientific work ever written. It recounted his new mechanics that applied equally to celestial and terrestrial realms as well as revealing the amazing simple system of forces responsible for the motion of celestial bodies. It brought the seventeenth century scientific revolution to a close and prevailed for two centuries as the paradigm of sublime scientific achievement. Newton gave a rigid logical structure to his work. It was divided into three books. The first two laid out a series of mathematical results derived from his celebrated laws of motion. They were theorems that said things like "if a body moves in such and such a way then it is governed by such and such a force." The third book, Newton's "System of the World" then applied these theorems. He laid out a list of phenomena, that is, reports of our experience of the motions of celestial bodies. From them he inferred to the forces that governed the motions. He argued, for example, "The moon moves this way; so, by my theorem, it must be driven by force directed to the *Earth*'s center." What follows is the briefest sample of Newton's results.

Theorems: the Area Law reveals Central Forces

Book 1. Theorems 1, 2 and 3; Propositions 1, 2 and 3. It turns out in Newton's system that planets orbit the sun and moons their planets because of attractive forces directed to a single point, the common focus of the orbits. These theorems identify the characteristic signature of motion governed by a force directed to a single point in space, that is, a "central force." If we draw a radial line from that center to the moving body and consider the area it sweeps, then the area will be same in the same times.



For example, if the body takes the same time to move from A to B and from A' to B', then the areas OAB and OA'B' will be the same. This "area law" is shown to be both necessary (Proposition 1) and sufficient (Proposition 2) for the motion to be governed by a central force.[61] In sum we have:

Area law if and only if Central force

This result applies for centers of force at rest or in uniform motion. Proposition 3 extends the result to the case of accelerated centers of force by incorporating the force responsible for the acceleration of the center of force.

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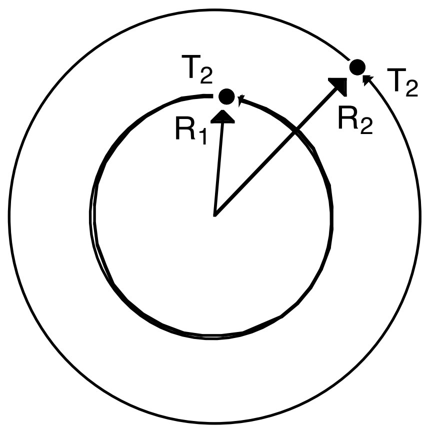
[61] While Newton's method was geometrical, these results are proved rapidly with a little vector analysis. If a point moves with velocity **v** at radial vector position **r** from some point, then the time rate at which the radial vector **r** sweeps area A is $dA/dt = |\mathbf{r} \times \mathbf{v}|$. The "same area in same time" condition is just $d^2A/dt^2=0$. It follows since $d^2A/dt^2=d|\mathbf{r} \times \mathbf{v}|/dt = |\mathbf{v} \times \mathbf{v} + \mathbf{r} \times \mathbf{a}| = |\mathbf{r} \times \mathbf{a}|$ where $\mathbf{a}=d\mathbf{v}/dt$, $\mathbf{v}=d\mathbf{r}/dt$ and $\mathbf{v} \times \mathbf{v}=0$. But $|\mathbf{r} \times \mathbf{a}|=0$ if and only if **r** is parallel to **a**; that is, if the force generating the acceleration **a** is directed along the radial line to the central point. (A zero force is taken to satisfy this condition.) Since the angular momentum of a mass m about that point is given as $m\mathbf{r} \times \mathbf{v}$, we see that Newton's results are now expressed as saying that motion is governed by a central force to a point if and only if the mass' angular momentum about that point is constant with time.

The Harmonic Law Reveals Inverse Square Forces

Book 1. Proposition 4.Theorem 4. Corollary 6. These last theorems give us the characteristic signature of motion governed by a central force. However all they tell us is the *direction* of the force. Newton also developed theorems that allow the identification of the *magnitude* of the force. This corollary is one of the simplest yet most useful. It is specialized to the case of circular motions. If we have masses orbiting uniformly in concentric circular orbits about the same point, then the force governing their motion is (by the earlier theorems or, more simply, by symmetry) directed to that center. The characteristic signature of a inverse square dilution of the magnitude is the "harmonic law": if we have many bodies orbiting in the system, the radii of the orbits R and their periods T are related by the proportionality $R^3 \propto T^2$.



So if there are two bodies orbiting as shown we have $(R_1/R_2)^3 = (T_1/T_2)^2$. Newton's corollary asserts that this harmonic law is both necessary and sufficient for the central force to dilute with the inverse square of radius; that is:

Harmonic law   if and only if   Inverse square force law

Newton's method of proof is geometric. We can arrive at the result more simply with a little algebra. In Book 1, Proposition 1, Theorem 4, Corollary 1, Newton had already developed the now familiar result that the centripetal acceleration A for a body moving with velocity V is V^2/R. Since the velocity V is just the circumference $2\pi R$ divided by the period T, we can write

$$A = \frac{V^2}{R} = \frac{(2\pi R)^2}{T^2 R} = (2\pi)^2 \frac{R^3}{T^2} \cdot \frac{1}{R^2}$$

We read the result directly from this equation. If the harmonic law holds, then R^3/T^2 will be a constant for all bodies in the system; therefore the acceleration and thus force is proportional to

$1/R^2$, as the inverse square law demands. Conversely, if the inverse square law holds so that the acceleration A is proportional to $1/R^2$, then R^3/T^2 must be a constant, so that the harmonic law holds.

From the Phenomena to the Forces

With these purely mathematical theorems in place, Newton turned in Book III to the phenomena. He noted, reporting on observations of astronomers, that the moons of Jupiter obey both the area law and harmonic law (Phenomenon 1); that the moons of Saturn obey both the area law and harmonic law (Phenomenon 2); that the five primary planets orbit the sun and obey the area and harmonic law (Phenomena 3, 4 and 5); and that the Earth's moon obeys the area law (Phenomenon 6). It was now a simple matter to infer to the forces that governed all these motions and Newton proceeded to do this in Book III, Proposition 1. Theorem 1. Proposition 2. Theorem 2. Proposition 3. Theorem 3. Proposition 5. Theorem 5. In all cases, since the area law is obeyed, the motions are governed by a central force, directed to the center of the orbital motions. In those cases in which the harmonic law is verified, the central force law is an inverse square law.

These inferences form the backbone of Newton's recovery of his inverse square law. Clearly many more steps are needed that I pass over. Also there are complications in the first steps. Newton needed to factor out small perturbations in the motion of the Earth's moon due to the sun's attraction. More seriously, the orbits of the planets were then known positively not to be circular but elliptical. However the ellipses have small eccentricities so are close to circles. Thus Newton's inference to an inverse square law for them holds only as correspondingly closely as an approximation. As it turned out, Newton has an ingenious method of showing that the approximation was extremely good, much better than the extent to which planetary orbits approximate circles. See Box 7: Newton's Arguments in *Principia*: From the Approximate to the Exact Inverse Square Law below.

The Moon Test

Early in his inferences, Newton sought to show that the astronomical force that kept the planets and moons in their orbits was the very same force of gravity responsible for the fall of objects on the Earth's surface. To demonstrate this in Book III, Proposition 4, Theorem 4, Newton took the magnitude of the force acting on the Earth's moon and computed how much it would be intensified were it to act on the Earth's surface, presuming an inverse square law for the force. The result was that the force was almost exactly that of gravity. So, calling upon his first and second "Rules of Reasoning in Philosophy," Newton felt justified in concluding that the astronomical force was the same as gravity.

In order to calculate these results, Newton took the radius of the moon's orbit to be roughly 60 times the radius of the Earth. He then computed how far the moon falls towards the Earth in the course of one minute, finding 15 Paris feet 1 inch 1 4/9 lines [twelfths inch]. This fall is directly proportional to the acceleration produced by the force and thus directly proportional to the magnitude of the force itself. If an inverse square law obtains, this same force would be increased in magnitude by a factor of 60^2 on the surface of the Earth, where we are closer by a factor of 60 to the center of the force. Therefore the distance fallen would be 60^2 x 15 Paris feet 1 inch 1 4/9 lines. It is impractical to measure the distance fallen on the surface of the Earth in one minute. So Newton reduced the time of fall from one minute to one second. The distance of fall is reduced correspondingly by a factor[62] of $(1/60)^2$. That is the distance fallen is $(1/60)^2$ x 60^2 x 15 Paris feet 1 inch 1 4/9 lines, which is:

Computed distance of fall under astronomical force in one second at Earth's surface
= 15 Paris feet 1 inch 1 4/9 lines (= 15.0934 Paris feet)

The corresponding distance of fall under gravity for bodies at the Earth's surface is easy to determine from simple experiments such as those Newton describes as conducted by Huygens on pendula. The result is:

Distance of fall under gravity in one second at Earth's surface
= 15 Paris feet 1 inch 1 7/9 lines (=15.0957 Paris feet)

and the agreement between the two results very good.

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[62] Why (1/60) *squared*? Galileo's law of fall tells us that, for bodies in free fall terrestrially, the distance fallen is proportional to the square of the time. So if we reduce the time by factor of 1/60, we reduce the distance fallen by a factor of $(1/60)^2$.

8 Demonstrative Induction

In a demonstrative induction the full inference from evidence to hypothesis is rendered deductive by supplementary hypotheses that in many cases we already have good grounds to believe. Demonstrative induction both continues the old tradition of perfect induction (which are deductions) and has an important role in the actual inductive practices of scientists..

An Atavism of Syllogistic Logic…

We have seen above that perfect induction is routinely covered in traditional accounts of enumerative induction. In perfect induction, we infer *deductively* to "All As are Bs." from a list of premises that exhausts every case of A, affirming in each case that it is a B. While Jevons defended perfect induction as supplying a useful summary of our knowledge, it is hard to see any really useful function for perfect induction. Rather it appears as an unnecessary reminder that ordinary enumerative induction falls short of the standard of deductive inference.

Nonetheless perfect induction has retained its place in logic treatises and has even expanded it. Johnson (1922) devotes full chapters to it (Ch. X, XI). In his expanded treatment, he renames it "demonstrative induction." The inferences are demonstrative is the usual sense of their being deductive: as he says, "the conclusion follows necessarily from the premisses." But they are inductive in the sense that the conclusion is a generalization of one or more of the premises. Johnson codifies demonstrative induction using the template of various syllogisms. For example, *modus ponens* "If P then Q, but P; therefore Q." yields the demonstrative induction (p.211)

If some S is p, then every T is q;

but this S is p,

Therefore every T is q.

The argument is demonstrative since it is deductively valid; it is inductive is so far as the second premise (This S is p.) is generalized by means of the first premise to yield a universal claim (Every T is q.). One may well wonder if such schemes are of any use in science. While most of Johnson's example are perfunctory[63], he gives at least one example that does capture real inference in science

Every specimen of argon has the same atomic weight.

This specimen of argon has the atomic weight 39.9.

Therefore every specimen of argon has atomic weight 39.9.

These forms of demonstrative induction and examples are really just a short introduction to what Johnson (and C. D Broad(1930) writing after him) take to be their real burden. It is to show how the inductive inferences of Mill's methods may be converted into demonstrative inductions. I pass over the details since the reader will surely find the work to be unilluminating formal exercises with a foregone conclusion.

…That Found New Friends

Analyses such as Johnson's and Broad's lay largely neglected in logic treatises. It is obvious that any inductive argument can be turned into a deductive argument by supplying some missing premise. If our induction is from evidence E to hypothesis H, we need only supply "If E then H." So how can such a commonplace of logic be of any use in philosophy of science? The hasty answer is that it cannot. The success of bootstrap confirmation, however, ought to make one hesitate in agreeing with this hasty response. The novel power of bootstrap confirmation comes precisely because it incorporates deduction from evidence E to an instance of hypothesis H. The final inductive step from the instance of the hypothesis H to H itself often seems a lesser

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[63] "If some boy in the school sends up a good answer, then all the boys have been well taught; the boy Smith has sent up a good answer; therefore all the boys have been well taught." (p.213)

part of the analysis. So might a full deduction from evidence to hypothesis have some place in real science? Might scientists really transform ampliative induction to deduction by supplying the missing premises?

In recent decades, philosophers of science who looked at the actual inductive practice of scientists came to a surprising conclusion. Scientists were performing just this transformation to very good effect. They were taking at times troublesome ampliative, inductive inference and supplying missing premises to make them demonstrative. What made the transformation valuable was that the missing premises often proved to be quite innocuous assumptions that were readily accepted. So the inference from evidence to hypothesis ceased to be inductive in the common sense—and at times an apparently quite risky one. The inference became a quite secure deduction; the inductive risk had evaporated.

My favorite example involves the introduction of quantum discontinuity in the early part of the twentieth century. (Norton, 1993, 1994) Once adequate instrumentation was available, it was possible to measure how energy was distributed over the various frequencies of the type of heat radiation called cavity or black-body radiation. That is, if one has a body in a furnace glowing red or white hot, one measured the energy in the different colors of the light emitted. The theory underlying the process was Maxwell's electromagnetism. The light is just electromagnetic radiation emitted by wildly oscillating electric charges in the heated body. It proved difficult to reconcile the measured distribution with the theory. It became apparent from the work of Max Planck that the reconciliation could be achieved if one assumed that the energy associated with heat radiation was discrete.[64] That this assumption allowed one to recover the measured result is the fact widely reported in textbooks on quantum theory and supposedly justifies the assumption that formed the early foundation of quantum mechanics. In the first decade of the twentieth century, the justification was far from compelling. By making a very peculiar assumption one recovers the observations. It was more a curiosity since this

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[64] There has been debate over how clearly Planck himself saw this. The discreteness would lie either in the radiation itself or in the electric resonators emitting the radiation. The discreteness meant the energies of either could not vary smoothly from zero up; the energies must be selected from the discrete set $0, \varepsilon, 2\varepsilon, 3\varepsilon, \ldots$ where the energy $\varepsilon = h\nu$, for h a new fundamental constant of nature and ν the frequency of the radiation or oscillator.

discontinuity seemed impossible to reconcile with then current physics. Are we compelled to make this strange assumption so thoroughly at odds with the fundamentals of our physics? Surely there are less destructive ways of maintaining compatibility of our theories and measurement. In the early 1910s Poincaré and Ehrenfest independently showed that there were not. They showed that one could infer deductively *from* the measured result *to* the discontinuity. One merely needed to supplement the measured results with fairly innocuous additional premises, most notably that thermal systems adopt the most probable distribution of energy. Their inference was a demonstrative induction that showed the power of the evidence to force a definite conclusion.

There are many other examples. Some are drawn from Newton's work in gravitation (Harper, 1990, 1995, 1997; Dorling 1990); a few will be reviewed below. Others are from his work in optics (Worrall, 2000). Various demonstrations of the inverse square law of electrostatics supply further examples. (Dorling 1973, 1974; Laymon, 1994) The same argument form has been found in Einstein's work in relativity and the quantum (Dorling, 1971, 1995); in Bohr's development of his model of the atom (Norton, 2000); in general relativity (Di Salle *et al.*, 1994; Gunn, 1997); and in quantum field theory (Bain, 1999). See also Bonk (1997). The pattern in all is the same. An ampliative inductive inference from evidence to hypothesis is rendered deductive by supplying further premises that are (in most cases) relatively unproblematic.

The General Scheme

These examples employ a relaxation of Johnson's restrictive definition of demonstrative induction. Indeed the scheme is quite loose. It merely requires a deductive argument of the form:

Demonstrative Induction

Premises of greater generality.

Premises of lesser generality

Conclusion of intermediate generality

The way the argument form is used is what makes it interesting in the induction literature. The premises of lesser generality are usually observations, measurements or other forms of evidence. The conclusion of intermediate generality is the hypothesis supported by the evidence. The premises of greater generality are the additional assumptions needed to allow the deduction to proceed.

These premises of greater generality function by limiting the possibilities. It can be fertile to reconceptualize these premises extensionally: they restrict the true theory or hypothesis to one of some class of theories or hypotheses that map out a universe of possibility. So in Johnson's example above, the premise of greater generality is "Every specimen of argon has the same atomic weight." In extensional form, this just says "The universe of possibility is restricted to those chemistries in which all specimens of argon have the same atomic weight." The deduction then allows us to infer to one or more of the hypotheses or theories in the universe of possibility. Since the remainder is eliminated, the associated deduction is called an "eliminative induction." It is, of course, equivalent to demonstrative induction:

Eliminative Induction

Premises that define a universe of possibilities

Premises that enable elimination of members of this universe.

Conclusion: Uneliminated members of this universe.

If the two are equivalent, why bother to define both? Each places emphasis on different aspects of the inference. Demonstrative induction stresses the deductive aspect; there is no inductive risk. Eliminative induction stresses the premise of greater generality; it lets us conceptualize the argument as saying "If any theory in this universe of possibility works, it must be…" and emphasizes that our inductive risk has been relocated in this selection exhausts the possible.

Other terminology is also used for the argument form. As the citations above indicate, it is often called "Newtonian deduction from the phenomena." We shall see shortly that this might not be accurate historically; some of the classic examples from Newton might be as much bootstrap confirmations as demonstrative inductions. Stressing that the universe of possible theories are often indexed by parameters, Gunn (1997) favors the term "test theory methodology."

Replace Deduction by Induction?….Really?

At first glance, demonstrative induction appears to pretend to an implausible miracle, the replacing of induction by deduction. That it cannot do. The goal cannot be to replace all induction in science by demonstrative induction. As long as the evidential base of science is logically weaker than our theories, that goal is impossible. And surely no one imagines that the evidence could ever be so complete as to match our theories in logical content.

The goal is far more modest. There are occasions in science in which we use an inductive inference but we do not need to. We may already be committed to just the premises needed to make the inference deductive. One sample of argon has atomic weight 39.9. We could use an enumerative induction to infer that all samples do. But our inference is actually far more secure than the invocation of enumerative induction suggests. Our accumulated experience of other elements gives us quite strong belief that all samples of one element have the same atomic weight. So we can infer directly from the atomic weight of one sample to that of all. An enumerative induction is not needed. It is replaced by a deduction.

Perhaps more modestly we might just say that the demonstrative induction allows us to localize and assess the inductive risk taken in inferring from one sample to all. We are secure in doing this just in so far as we are convinced that all samples of an element have the same atomic weight. That turns out to be less than completely secure. The discovery of isotopes shows us that different samples of an element may have different atomic weights. In nature we tend to encounter the element with the same portions of the isotopes so the measured atomic weight of naturally occurring samples of a given element will be the same. But that can change if we sample artificially separated isotopes of the same element.

Newton Determines the Inverse Square Law is as Exactly Correct as Measurement Permits…

Newton's determination that the planets are attracted to the sun by an inverse square law of attraction provides an excellent illustration of a demonstrative induction. In the last section we saw how Newton used the harmonic law to infer to an inverse square law. That argument presumed the orbits of the planets are circular, which is only approximately true. Thus the argument from the harmonic law can at best only assure us that the law of attraction is *approximately* an inverse square law. As explained in Box 7: Newton's Arguments in *Principia*: From the Approximate to the Exact Inverse Square Law, Newton found an ingenious way to convert this approximate recovery into an exact one. The actual orbits of the planets are ellipses fixed in space. If the law of attraction differed even slightly from an inverse square law, then that difference would be manifested as a motion of the ellipse traced in space by the orbiting planets, in the case of the near circular orbits of the planets. To make the relation quantitative, Newton considered a family of power laws in which the force of attraction varies with distance r as $1/r^{3-N}$, where N>0. (N=1 corresponds to an inverse square law, $1/r^2$.) He found that the ellipse would rotate so that a complete orbit requires the planet to pass through an arc not of 360^o but of $360^o/\sqrt{N}$. If the ellipse is fixed in space as it is for the planets, a complete orbit requires just 360^o and we conclude that N=1, the case of an inverse square law.

This is a demonstrative induction:

Premises of greater generality: The force of attraction is $1/r^{3-N}$, where N>0.

Premises of lesser generality: For each planet, there is no motion of the ellipse.

Conclusion of intermediate generality: N=1; the force of attraction is $1/r^2$.

There are two ways to read the argument and I am not sure which way Newton would endorse. In the stronger reading, the scope of the premise of greater generality is all space surrounding the sun. In that case, we need run the argument just once for one planet to infer to the inverse square law. Repeating the argument for different planets merely rederives the same result. In the weaker reading, the premise of greater generality just refers to that portion of space visited by the particular planet in question. Then the conclusion is correspondingly weaker since it applies only to that portion of space. Repeating the argument for different planets gives new information as the inverse square law of attraction is found to prevail in different regions of space.

…in a Strong Demonstrative Induction

This demonstrative induction illustrates how the argument form can greatly strengthen the evidential case for a result. If one knew only that an inverse square law generates a fixed elliptical orbit, one might feel some license in the elliptical orbit of the planets to infer to the law of attraction being an inverse square law. But one should reserve some doubt because of the possibility that other force laws can generate fixed elliptical orbits. Newton's demonstrative induction eradicates that doubt by showing that all other plausible candidates (force laws close to an inverse square law) can be eliminated. The strength of the argument depends on the acceptability of the premises. And there Newton stood on solid ground. The fixity of the planetary ellipses was delivered to him as an observation by the astronomers. That the class of force laws be restricted to central forces was also dictated by observation. Since planetary motion obeys the area law, we already saw that Newton showed the force that governed them must be directed centrally to the sun. The class of power laws of form $1/r^{3-N}$ (where N>0) is very broad and includes the inverse square law of special interest. But it is not the broadest. However anyone who has worked through the demonstration of Newton's results would be aware that similar arguments could be mounted for virtually any alternative force law. As explained in Box 7, the rotating ellipse orbit is generated by a force law of the form $A/r^2 + B/r^3$, with A and B constants. In near circular orbits, the planet probes the attractive force over a small segment of the distance from the sun and in such small segments virtually all force laws can approximate the form $A/r^2 + B/r^3$. A deviation from the inverse square law results in a non-zero value for B and that non-zero value would be manifested in the rotation of the orbit's ellipse.

Steady State Cosmology Supplies an Illustration of…

So far we have celebrated successful demonstrative inductions. Since the arguments are deductive, one might well wonder how they could possibly fail. They can fail in the same way that deductive arguments may fail. Excepting fallacies due to errors of logic, they can fail because a premise is false. And that can certainly happen if one builds the demonstrative induction on premises for which one has little warrant.

An interesting example of such a demonstrative induction arose in mid twentieth century cosmology. We saw above that modern cosmology proceeds from the Hubble law, which describes the overall motion of the galaxies. It asserts $v = H d$, where v is the velocity of recession of a galaxy, d the distance to the galaxy and H is Hubble's constant. While the law was introduced by generalization from observation, it turns out that the same law can be generated through a simple argument. We assume that the overall cosmic motion of the galaxies is the same everywhere in the universe and indifferent to direction in space. That is, we assume the motion is homogeneous and isotropic. It turns out that the Hubble law is the only law compatible with these conditions. From isotropy, we conclude that there can be no rotational component to the motion—that would require an axis which would be a preferred direction. So the motion of a galaxy must be in the radial direction. The velocity v must vary linearly with distance d so that an observer on another galaxy sees the same distance dependence.[65] Thus we arrive at v = Hd, where a negative constant H (corresponding to inward motion) and zero H (corresponding to no motion) remain possible.

Simple requirements like homogeneity and isotropy limit possible cosmological theories quite powerfully. The advent of "steady state cosmology" was one of the most adventurous attempts to use such requirements to generate a quite definite cosmology through what amounted to a grand demonstrative induction. The theory was published by Bondi, Gold and Hoyle in 1948 (Bondi and Gold, 1948; Hoyle, 1948; see also Bondi, 1960) It was based on the "perfect cosmological principle," which required that the universe look the same not just to observers at all places in space but at all times as well. With very little observational input, this perfect cosmological principle forced a quite definite cosmology.

To begin, it follows from the principle that the average density of cosmic matter must remain constant with time. Since we observe a cosmic expansion of matter, it follows that this expansion must be universal. One would expect the expansion to dilute the average matter density in contradiction with its constancy in time just inferred. To preclude this, the steady state cosmologists concluded that the law of conservation of matter must be discarded: matter is continually created throughout space at just the rate needed to offset the dilution by the expansion. (Using then current estimates of matter density and rate of expansion, Bondi (1960, p. 143) estimated the rate of matter creation to be equivalent to the mass of one hydrogen atom per liter of space in each 500 billion years—a rate far too small to be detected directly.)

Similar arguments led to a quite definite spacetime structure for the cosmology. Steady state cosmology followed the approach of relativistic cosmology in which the expansion of cosmic matter was accounted for by an expansion of space itself. The rate of expansion was tracked by the time dependence of a cosmic scale factor. In steady state cosmology, that scale factor could be taken as the distance R(t) at some time t to some arbitrarily nominated galaxy that we decide to use as our measuring stick. The velocity of recession of that galaxy will be V(t)

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[65] That is, we seek the vector velocity $\mathbf{v}(\mathbf{d})$ as a function of the vectorial distance $\mathbf{d}$ to the galaxy. An observer on a galaxy as distance $\mathbf{D}$ will see a velocity distribution $\mathbf{v}'(\mathbf{d}') = \mathbf{v}(\mathbf{d}-\mathbf{D}) = \mathbf{v}(\mathbf{d})-\mathbf{v}(\mathbf{D})$. Homogeneity requires that $\mathbf{v}(\mathbf{d})$ be the same function of $\mathbf{d}$ as $\mathbf{v}'(\mathbf{d}')$ is of $\mathbf{d}'$ and this can only obtain if $\mathbf{v}$ is linear in $\mathbf{d}$, so that $\mathbf{v} = \mathrm{H}\mathbf{d}$.

which is just the time rate of change of R(t). Since the Hubble law tells us that the velocity of recession of a galaxy is proportional to distance d, we know that the velocity v of recession of a galaxy at distance d is given as $v = \frac{V(t)}{R(t)} d$, for some fixed time t. From comparison with the Hubble law v = Hd, we conclude that $\frac{V(t)}{R(t)} = H$, where the perfect cosmological principle requires H to be a constant with time. This last equation admits a unique solution[66]

$$R(t) = R(0) \exp(Ht)$$

which tells us that the distance to galaxies is growing exponentially with time. It also proves to be the characteristic property of a de Sitter spacetime whose other properties were recovered from the perfect cosmological principle by analogous arguments.

…A Weak Demonstrative Induction

To recover this spacetime structure, the perfect cosmological principle (with homogeneity and isotropy of the cosmic motion) needed to be supplemented by the most meager of assumptions, most notably that galaxies are receding from the earth. It was not necessary to assume that the recession obeyed Hubble's law—that could be derived from homogeneity and isotropy as well. These inferences can be summarized as a demonstrative induction:

Premises of greater generality: Perfect cosmological principle; homogeneity and isotropy in space of cosmic motion.

Premises of lesser generality: Galaxies recede from us.

Conclusion of intermediate generality: Continual creation of matter; our universe is a de Sitter spacetime.

The steady state cosmologists were willing to reduce the empirical component of this argument even further. They argued, for example, that cosmic matter must be expanding, else we would have arrived at a thermodynamic equilibrium that would even preclude life.

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[66] That is H = V(t)/R(t) = (1/R) dR/dt = d(log R)/dt, so log R(t) = Ht + log R(0), from which the result follows. (For experts) The de Sitter spacetime, in coordinates used by the steady state cosmologists has a line element $ds^2 = dt^2 - R^2(t)\,(dx^2 + dy^2 + dz^2) = dt^2 - \exp(2Ht)\,(dx^2 + dy^2 + dz^2)$. These coordinates only partially cover the spacetime.

This deduction of a cosmology engendered considerable controversy and eventually failed when the cosmology was refuted by astronomical observation, most notably the cosmic background radiation. (See Kragh, 1996) What makes the above demonstrative induction weak is *not* that its conclusion was refuted. With all induction we take a risk that may lead a strong induction to endorse what proves to be a false conclusion. The situation is different here. In so far as we have any means for assessing the strength of a demonstrative induction, that assessment must rest on the warrant for the premises. Here the demonstrative induction fails. The perfect cosmological principle is a very strong assertion from which a great deal can be recovered. The steady state cosmologists did not have correspondingly strong grounds in support of the principle. Rather they sought to extend the widely accepted fixity in time of physical laws to all general physical facts, a plausibility argument too weak to sustain the strength of the conclusion.

Relation of Demonstrative Induction and Bootstrap Confirmation

There is a very close affinity between demonstrative induction and bootstrap confirmation. Both depend on the use of auxiliary hypotheses to carry out deductions from the evidence. In the case of demonstrative induction the deduction is to the hypothesis of interest; in the case of bootstrap confirmation it is to an instance of the hypothesis. Indeed with small rearrangments, the one type of induction can often be converted into the other. To illustrate, consider the example given above of Newton's inference of the inverse square law of attraction from the harmonic law. The two forms of the induction can be compared as in the table:

	Bootstrap Confirmation	**Demonstrative Induction**
Evidence	Known planets that follow (approximately)[67] circular orbits around the sun conform to the harmonic law: radius^3/period^2 is a constant.	
		By enumerative induction:
Generalized Evidence		Planets that follows a circular orbit at any possible distance from the sun will conform to the harmonic law.
Supporting Theory	In circular motion, centripetal acceleration is (velocity)/radius^2; the circumference of a circle is 2π x radius.	
	Deduce:	*Deduce:*
Instance of Hypothesis	An instance of the hypothesis in the case of the known planets	
	By enumerative induction:	
Hypothesis	Planets at all possible distances from the sun are attracted to the sun by an inverse square force.	

In the bootstrap confirmation, we deduce from the evidence of the behavior of known planets to an *instance* of the hypotheses of the inverse square law. We then infer from the instance to the hypothesis in what amounts to an enumerative induction. The evidence and supporting theory are not logically strong enough to allow a demonstrative induction to deduce the hypothesis. However the deduction is possible if the evidence of known planets is first generalized by an enumerative induction. The generalized evidence and supporting theory are strong enough to allow deduction of the hypothesis of the inverse square law. That deduction is the demonstrative induction.

In briefest terms, the bootstrap confirmation consists in a deduction to an instance followed by an enumerative induction. The alternative scheme consists in an enumerative induction first then followed by the demonstrative induction.

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[67] If you are troubled by the "approximately," I would suggest that the evidence be allowed to support the modified evidence: "if the planets' orbits were to be exactly circular they would obey the harmonic law" and that this modified form of the evidence be used in the inductions.

Is one form superior? In this case, the difference likely to count most lies in the relocation of the enumerative induction. Perhaps one might find an enumerative induction on raw evidence more secure than one in the rarefied world of hypotheses; or one might the latter more secure. In my view they are equally secure. The deductive inference shows that the induction from evidence to generalized evidence is equivalent to the induction from the instance of the hypothesis to the hypothesis: if one fails then so will the other and conversely.

In other cases, we might prefer the scheme that includes demonstrative induction if it allows us to circumvent the circularity that arises in bootstrap confirmation. However my expectation is that conversion to a demonstrative induction scheme is most likely to succeed if the bootstrap confirmation involves no circularity. I do not see, for example, how the conversion could be effected for the highly circular bootstrap confirmations of Newton's moon test.

How Good were Newton's Deduction from the Phenomena?

For two hundred years, Newton's arguments in *Principia* prevailed as the paradigm use of evidence in science; he had deduced the supreme law of the universe from the phenomena. The enthusiasm faded in the twentieth century in large measure due to the success of Einstein's general theory of relativity. Einstein's theory was a new theory of gravitation that replaced Newton's and allowed that Newton's theory was only approximately correct. What now of Newton's simple and bold claim from the General Scholium to Book III of *Principia* (1999, p.943): "In this experimental philosophy, propositions are deduced from the phenomena and are made general by induction." Something had to be wrong.

In my view there was nothing wrong with his claim. Newton did exactly what he said. He inferred, for example, from the motions of planets that they were acted on by an inverse square law and this conclusion was generalized into his law of universal gravitation. The inferences were inductive and therefore always involve risk. Newton had inferred to the perfect correctness of the inverse square law in all domains. He gambled, for example, that the law would still hold in very intense gravitational fields, far stronger than those seen in our solar system. General relativity asserted that he lost the bet. Things fared much better for Newton in the domain of the weaker fields encountered in our solar system. There Newton's perfect correctness need only be replaced by accuracy so close that nearly two hundred years were needed to find the deviations.

Duhem's Objections

A strong tradition of criticism has not been satisfied with the simple diagnosis that good inductive inferences can sometimes fail. It has been determined to find some principled flaw in Newton's method. Most celebrated of these attempts is Pierre Duhem's (1906) critique. Duhem was certainly right to complain that Newton overreached when (according to Duhem, p. 191) "he asserted that in a sound physics every proposition should be drawn from phenomena and generalized by induction." But Duhem fails in his efforts to show that Newton did not himself do this. Duhem has two arguments.

Duhem's First Argument. The first proceeds from the fact that Newton's phenomena, Kepler's laws of planetary motion (which include the area and harmonic law), presume that there is a fixed center of orbital motion located at the center of the sun. Newton's final theory entails however that this is only approximately true; The sun and planets all orbit a common center of mass that turns out to be very close to the center of the sun. Duhem proclaims (p. 193) in emphasized text:

> The principle of universal gravity, very far from being derivable by generalization and induction from the observational laws of Kepler, formally contradicts these laws. If Newton's theory is correct, Kepler's laws are necessarily false.

At first glance the objection seems quite fatal to Newton. How could he infer from phenomena to a law that contradicts them? The objection weakens under further scrutiny, however. That the phenomena contradict the law induced is actually very common in induction and not at all fatal to it. The most familiar example is simple curve fitting. We saw above that Hubble and Humason compared the velocity of recession of galaxies and their distance from us and inferred to a linear relationship: the velocity of recession is 560 times the distance (in suitable units). That conclusion is formally contradicted by *every one* of Hubble and Humason's data! No single galaxy in their data set exactly satisfies the law they induce. Does this mean that we should discard curve fitting? Hardly. In crudest terms, the inference is extremely simple. For each galaxy considered, the velocity distance relation is approximately satisfied. By enumerative induction we infer to the approximate correctness velocity distance relation. That is the major result. It is subject to refinement, of course. By introducing assumptions about the character of the errors within the data, we can compute statistics that assess how good the approximation is. The use of these assumptions may move us beyond enumerative induction; but without further analysis we cannot automatically assume that we have stepped outside the scope of inductive generalization.

The situation is no different with Newton's inferences.[68] From the approximate correctness of the phenomena of Kepler's laws, he infers to the approximate correctness of the inverse square law of attraction. That is a major result, not to be discounted. Newton then converts the approximate correctness of the inverse square law into a correctness as exact as his measurements permits. We have seen here in detail how he does this in one case. In Book III, Proposition 2 Theorem 2, he infers from satisfaction of the harmonic law for the planets to the inverse square law, drawing on the mathematical results of Book I Proposition 4 Theorem 4. Since the analysis assumes circular orbits, the satisfaction of the harmonic law is only approximate and the resulting inverse square law only approximately recovered. Newton completes Proposition 2 of Book III by inferring to the "greatest exactness" of the result through a demonstrative induction from the immobility of the planetary apsides, now drawing on Book I Proposition 45 Corollary 1. The first inference is a bootstrap confirmation and the second correcting inference is a demonstrative induction.

Duhem's second argument. The second arises while Duhem explains how Newtonians show compatibility of Newton's theory with the perturbations observed from Kepler's law. This raises Duhem's belief that evidence cannot confront individual laws but must confront theories as a whole. He notes (p. 194):

> Such a comparison will not only bear on this or that part of the Newtonian principle, but will involve all its parts at the same time; with those it will involve all the principles of dynamics; besides, it will call in the aid of all the propositions of optics, the statics of gases, and the theory of heat, which are necessary to justify the

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[68] One may wonder if the difference is that the errors in Hubble and Humanson's case are random errors, whereas in Newton's case they are systematic, that is errors due to the law itself. There are also systematic errors present in the cosmological case. Under most cosmological models, the linearity of the velocity distance relation obtains only for small distances, just as the center of planetary attraction is only approximately at the sun's center. Yet the observed linear relation of velocity and distance for galaxies is routinely taken as evidence for a cosmological theory that predicts the precise relation to be only approximately linear.

> properties of telescopes in their construction, regulation, and correction, and in the elimination of the errors caused by diurnal or annual aberration and by atmospheric refraction. It is no longer a matter of taking, one by one, laws justified by observation, and raising each of them by induction and generalization to the rank of a principle; it is a matter of comparing the corollaries of a whole group of hypotheses to a whole group of facts.

Duhem is right: if we are to trace out the inductive inferences that lead back from Newton's theory to our raw experience, a complete account would require some sort of entanglement with all these theories. However Duhem is also wrong on two counts. First Duhem's observation mischaracterizes Newton's claim to have inferred his results from the phenomena. Indeed Duhem is guilty of a blatant philosophical "bait and switch." Newton was quite clear. He claimed to infer his inverse square law from the phenomena and what he meant by the term "phenomena" in this context was stated precisely as a list of four numbered phenomena at the beginning of Book III of *Principia*, that include the satisfaction of the area and harmonic law for planetary motion. What Duhem's objection does is to replace these phenomena by others—that at such and such a time this astronomer saw a bright spot in the telescope pointed at such and such a direction. Duhem now complains that Newton did not infer his laws from these phenomena in the manner Newton claimed. How unfair! Newton never pretended his arguments in *Principia* did this. Second, Duhem reads too much into the entanglement of all these branches of science in the full inference to the final result, the inverse square law, for example. The entanglement does not mean that all items of evidence bear equally on it. Evidence of the thermal expansion of gases will only have a quite indirect bearing. Particular planetary motions will have the most direct bearing. As Newton shows, the relation of planetary periods and orbital sizes or the motions of planetary apsides can be translated directly into the law. However we may characterize these inferences, they are much closer to "taking, one by one, laws justified by observation, and raising each of them by induction and generalization to the rank of a principle" than they are to " comparing the corollaries of a whole group of hypotheses to a whole group of facts."

Conclusion; The Neglected Induction

To a systematic epistemologist, demonstrative induction is likely to be a disappointment. The most interesting aspect of induction is its ampliative character—that the conclusion is logically stronger than the premises yet we still somehow find a license for the conclusion. Demonstrative induction eliminates this epistemologically most interesting feature by the artifice of strengthening the evidence logically until the conclusion can be deduced. The mystery of ampliative inference is gone. How dreary! Yet this very dreariness is just what makes demonstrative induction so interesting to practicing scientists. They do not want the grandiose inductive maneuvers that may entertain an epistemologist. These flourishes are as likely to arouse doubts of other scientists. A demonstrative induction is simple, solid and utterly transparent. Accept the evidence and the general hypotheses and one is driven by the force of deductive argumentation to the result. That is what attracted Newton: solidity and security. The epistemologists' neglect of demonstrative induction is also surely exacerbated by the very simple examples of perfect induction that abound and impart a superficial air to demonstrative induction. The non-triviality of real demonstrative inductions in science do not derive from the argument form. The non-triviality derives from the often quite deep and difficult results within the science itself that enable the induction. While a demonstrative induction may be merely an uninteresting deduction to the epistemologist, the scientist may find the very same demonstrative induction to be a brilliant application of a powerful result within the science.

Its theoretical dreariness is, of course, only part of the reason for the neglect of demonstrative induction by epistemologists. It is also that demonstrative induction cannot have any pretensions of being the universal form of induction. That cannot be as long as the totality of our evidence is logically weaker than the theories we seek to induce—a situation that is hardly likely to change. However there are circumstances in which we have already made enough ampliative inductions so that further ampliative inductions are not needed. While these cases are uninteresting to an epistemologist, they are a welcome refuge to the scientist whose interest is strength of the evidential case and not the brilliance of the inductive moves used to assure it. In those cases, the scientist sees that we are already committed to the relevant conclusion once we accept the evidence; combine it with general hypotheses we have already accepted and we deduce the conclusion.

Finally, however its non-ampliative character may disappoint the epistemologist, demonstrative induction does have one highly remarkable feature. One of the long-standing problems of inductive generalization is to find ways of bridging the gap between evidence expressed in one language and theory in another. Demonstrative induction (like the closely related bootstrap scheme) succeeds in bridging that gap. Indeed demonstrative induction reveals a sense in which the evidence univocally has the theory encoded in it. Kepler's harmonic law has the inverse square law of gravity encoded within it; or the immobility of the planetary apsides has the exactness of the same law encoded within it. Or that is what one sees when demonstrative induction shows us how to read the code.

Box 7: Newton's Arguments in *Principia*: From the Approximate to the Exact Inverse Square Law

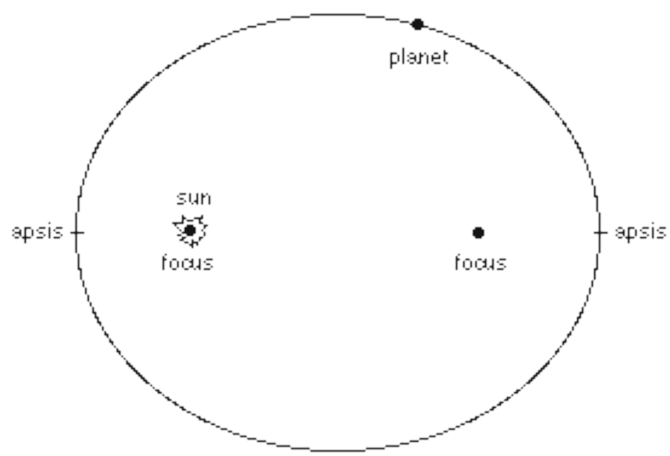
Newton showed that the law of attraction governing planetary motions is as accurately an inverse square law as measurement permits. Any deviations from the inverse square law would be revealed as a rotation of the axis of the ellipse of the orbit and the magnitude of the rotation would have revealed how much the law differed from the inverse square law.

The Problem

We saw in Box 6: Newton's Arguments in *Principa*, how Newton began his argument for his law that the force of attraction to the sun varies with the inverse square of distance. Those arguments were incomplete. Newton had shown, for example, that if planetary periods and radii are related by the harmonic law, then their motions are governed by an inverse square force law. The problem, however, was that this inference depends on an assumption that is only approximately true: he had assumed that planetary orbits are circular. This assumption was made because it greatly simplified the argument. In a circular orbit the planet stays at a constant distance from the sun at the center of the orbit. If the magnitude of the force of attraction to the sun depends only on this distance, then it will be a constant as the planet proceeds in its orbit. Newton's argument needed to consider this constant force only.

The problem, of course, is that planets were then known to orbit in ellipses with the sun at one focus. As a planet proceeds in its orbit, the distance to the sun varies and with it the magnitude of the attractive force to the sun. Now the ellipses of some orbits have small eccentricities so that they are close to circles. The eccentricities for Venus and Earth are 0.007 and 0.017. But for others they are larger; for Mars, Jupiter and Saturn, they are 0.093, 0.048 and 0.055. And for Mercury, it is 0.206. (The eccentricity is defined as the ratio of the distance between the foci and the distance between the apsides of the elliptical orbit. [69]An eccentricity of zero is a circle and eccentricities are always less than one. See figure below which shows an ellipse of eccentricity 0.83.)

In these circumstances, the best Newton can claim for his arguments so far is a weak result. The orbits are only approximately circular, so the force that governs them is only approximately an inverse square force. How can Newton bridge the gap from *approximately* to *exactly*? Newton found an ingenious method that lets him conclude that the force law is *exactly* an inverse square law—or at least as exactly an inverse square law as the available exactness of astronomical observation permitted.

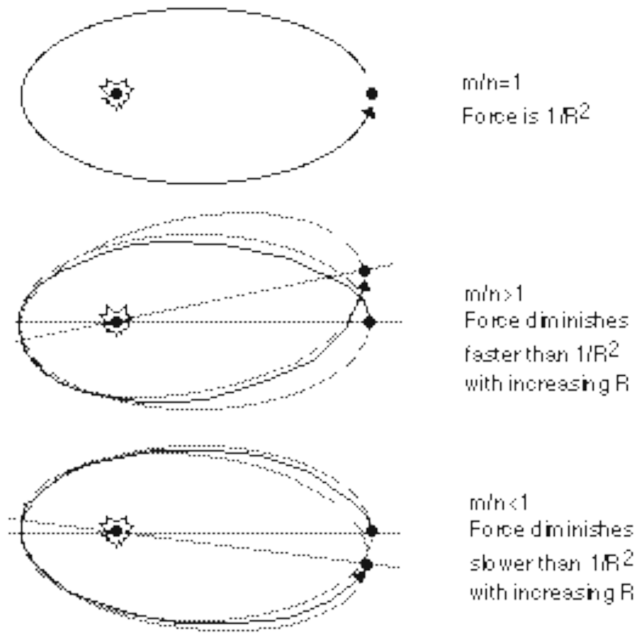


The Motion of the Apsides *of a Planet's Orbit…*

The apsides (plural of apsis) of a planet's orbit are the points of closest approach (lower apsis) and furthest distance (upper apsis) from the center of attraction of a planetary orbit. Their examination was to carry Newton to his conclusion. He had already shown the well known result: if an orbiting body is acted on by an inverse square force, then its orbit is an ellipse. What was special about this result was that the orbit was re-entrant. That means that with each orbit the planet retraces exactly the same ellipse fixed in one position in space. Were the law of attraction to be different, the planet would not return to the same point in space with each orbit. For laws of

[69] The foci are identified as follows: if we sum the distance from the first foci to the planet and the distance for the second foci to the planet we recover the same value for all positions on the ellipse. This property is commonly used as the definition of an ellipse.

attraction that are close to an inverse square law and for near circular orbits, the planet would follow an ellipse that was slowly rotating.



Newton used the ratio m/n to measure the amount of advance or retardation. In one period of its orbit—in the motion from upper apsis to upper apsis—the planet would traverse (m/n)360°. So if m/n>1, the planet advances beyond one complete revolution of 360° in completing its orbit. If m/n<1, the planet is retarded analogously.

...Affirm the Exactness of the Inverse Square Law

What Newton realized was that he could use this motion to detect differences between the real law of attraction and an inverse square law. If the ellipse advanced, that is, rotated in the direction of the planet's orbit, then the force is diluting faster with distance than an inverse square law. If the ellipse is retarded, then the force is diluting slower with distance than an inverse square law. Newton knew of no such advance or retardation. He could thus conclude that the force of attraction acting on the planets was not just approximately inverse square force, but was exactly so, or as exactly as measurement of apsidal motion permitted. Presenting this result as part of Proposition 2. Theorem 2 of Book III, he remarked (1999, p. 802)

> [The inverse square law of attraction for the planets] is proved with the greatest exactness from the fact that the aphelia [upper apsides] are at rest. For the slightest departure from the ratio of the square would (by book 1, prop. 45, corol. 1)

necessarily result in a noticeable motion of the apsides in a single revolution and an immense such motion in many revolutions.

The result is very sensitive to deviations from an inverse square law. A small deviation from the law would yield a corresponding apsidal motion in a single orbit. That motion would then accumulate with each orbit so that a small deviation would yield an immense motion over many orbits. Conversely, we might add, even a very minute deviation would eventually reveal itself as the correspondingly minute motion of the apsides were accumulated over many orbits.

Rotating Ellipses

In qualitative terms, the idea is simple enough: a motion of the apsides reveals a deviation from the inverse square law of attraction. But how much deviation is revealed by a given motion? Answering that question is the principal burden of Propositions 43, 44 and 45 of Book I. These propositions are formidable, even with the assistance of modern interpreters such as Chandrasekhar (1995). However it turns out that their formal content can be presented quite simply, especially if we restrict ourselves to the elliptical orbits generated by inverse square laws as opposed to all reentrant orbits.

Newton's first step was to show that a central force still governs a planet moving in a rotating elliptical orbit. Imagine, for example that we have a planet moving under an inverse square law in an elliptical orbit. Since the motion is governed by a central force, we know it obeys the area law: a radial line from the sun to the planet sweeps equal areas in equal times. (See Proposition 1 Book 1, Box 6: Newton's Arguments in *Principia*.) Now imagine that we add a uniform rotation to the orbital motion as indicated in the figures above. Newton's Proposition 43 assures us that we can find the force needed to generate this motion. The radial line from the sun to the planet will continue to sweep equal areas in equal times, but now at a different rate—slower if the added motion opposes the orbital motion and faster otherwise. Thus by Proposition 2, Book 1, the new motion is also governed by a central force and it is simply a matter of applying Newton's existing techniques to determine the magnitude of the force.

Newton proceeded in Proposition 44 (with additional material in Proposition 45) to determine what the force is. The outcome is a great deal simpler than one would expect. If the original elliptical orbit is generated by an inverse square force

$$f_{\text{inv. square}}(r) = \frac{k}{r^2} \qquad (1)$$

for k a constant and r the radial distance, then a uniform rotation is added to the orbit merely by adding a term in $1/r^3$ to the inverse square law. That is, if the rotation moves from upper apsis to upper apsis in $(m/n)360^o$, then the force that generates the rotating orbit is

$$f_{\text{rotating}}(r) = \frac{k}{r^2} + \frac{k}{r^3} R \left[\left(\frac{m}{n}\right)^2 - 1\right] \qquad (2a)$$

The parameter R is a kind of average radial distance from the sun. More precisely, the planet will move between the upper apsis at radial distance from the sun R_{max} and the lower apsis at R_{min}, then R is defined as $(1/R) = (1/2)(1/R_{max} + 1/R_{min})$ and must always lie somewhere between R_{max} and R_{min}. What is remarkable about this result is that it obtains exactly. There is *no* requirement that the added $1/r^3$ term be small in relation to the $1/r^2$ term or that the orbits be near circular. Once one knows the amount of rotation desired, as expressed by the value of (m/n), one can directly compute the additional force term needed to generate it.[70]

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[70] (For experts.) To see the result of equation (1.2a), note that Newton's equations of motion in the orbital plane are $d^2r/dt^2 - r(d\theta/dt)^2 = -f(r)$ and $(d/dt)[r^2(d\theta/dt)] = 0$ for radial coordinates r, θ and time t and a central force of magnitude f(r). Re-expressed in terms of the new coordinate $u = 1/r$ and integrating the second equation, Newton's equations of motion become

$$d^2u/d\theta^2 + u = f(u)/h^2u^2 \qquad \text{and} \qquad r^2(d\theta/dt) = h,$$

where h is a constant characteristic of the orbit and $f(u) = f(r)$ is the magnitude at $u=1/r$ of the centrally directed force. Since it is twice the rate a radial vector sweeps area, the constancy of $r^2(d\theta/dt)$ expresses satisfaction of the area law. For the case of an inverse square force, $f(u) = ku^2$ and $f(u)/h^2u^2 = k/h^2$. One can see easily that Newton's equations are solved for this inverse square law by the elliptical orbit $u(\theta) = \Delta U\cos\theta + U$, where $U = 1/R = k/h^2$ and $\Delta U = (1/2)(1/R_{max} - 1/R_{min})$. That is, $d^2u/d\theta^2 = -\Delta U\cos\theta$, so that $d^2u/d\theta^2 + u = k/h^2$.

This orbit, set in uniform rotation at m/n, is described by the same equations, but with θ replaced by nθ/m. The rotating ellipse is given by $u_{\text{rotating}}(\theta) = \Delta U\cos(n\theta/m) + U$. Successive upper apsides are at $n\theta/m = 0^o$ and 360^o; that is $\theta = 0^o$ and $(m/n)360^o$. The area law becomes $r^2(d(n\theta/m)/dt) = h$; that is, $r^2(d\theta/dt) = (m/n)h = h'$, from which we see that the rotating orbit

A more convenient form of (1.2a) arises as the ratio

$$\frac{f_{\text{rotating}}(r)}{f_{\text{inv. square}}(r)} = 1 + \left[\left(\frac{m}{n}\right)^2 - 1\right]\frac{R}{r} \tag{2b}$$

Rotation from All Other Force Laws Close to An Inverse Square Law for Near Circular Orbits

In so far as a force law approximates f_{rotating}, it will produce a rotating elliptical orbit. We can now see a quite remarkable result: almost any force law close to an inverse square law will produce an orbital motion that is a rotation of the elliptical orbit in the special case of near circular orbits. To see this, note that the ratio $f(r)/f_{\text{inv. square}}(r)$ measures how much some force law f(r) differs from an inverse square law. If it is constant, then the force law f(r) is an inverse square law. If the ratio varies with r (or equivalently with 1/r), f(r) is some other force law. In a near circular orbit, the value of r will never differ much from some average value R. In that case, the ratio $f(r)/f_{\text{inv. square}}(r)$ can generally be approximated by the linear relation in (1/r) of equation (2b).[71] We conclude immediately that *all force laws close to an inverse square law will produce a uniform rotation of the elliptical orbit in the case of near circular orbits*.

All force laws close to an inverse square law is a large class of laws. To generate more concrete results, Newton considered an important subset, power laws in which the force depends on distance according to $1/r^n$, for a wide range of n. In his Proposition 45, Corollary 1, Newton computes this effect for a power law

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sweeps area at a rate of (m/n) times the rate of the inverse square orbit. To recover the expression for $f_{\text{rotating}}(r)$, substitute $u_{\text{rotating}}(\theta)$ and the new constant $h'=(m/n)h$ into Newton's equations of motion. Since $d^2u_{\text{rotating}}(\theta)/d\theta^2 = -(n/m)^2\Delta U\cos(n\theta/m)$ and $u - U = \Delta U\cos(n\theta/m)$, we recover $f_{\text{rotating}}(u)/u^2 = k + kR((m/n)^2 - 1)u$ from which (1.2a) follows.

[71] To be more precise, not all force laws can approximate f_{rotating}. A sufficient condition for this approximation is that the ratio $f(r)/f_{\text{inv. square}}(r)$ is analytic in 1/r, that is, it can be expressed as a power series in 1/r, and that its first constant term is non-zero. The force law $f(r) = k'/r^3$ fails this condition. It yields a ratio $f(r)/f_{\text{inv. square}}(r) = 0 + \text{constant}/r$. As Newton showed in Proposition 41, Corollary 3, motion under an inverse cube law does not produce a stable orbit. For example, once the planet leaves the upper apsis and descends it enters a motion that spirals into the center.

$$f_{power}(r) = \frac{k'}{r^{3-N}}$$

where N>0. For near circular orbits, r remains close to R so that 1/r – 1/R is small and we can approximate the ratio $f_{power}(r)/f_{inv.\ square}(r)$ by the relation (1.2b), which is linear in 1/r. To match it to the corresponding inverse square law and associated rate of rotation, we need to set the constant k' in the power law as[72]

$$k' = \left(\frac{m}{n}\right)^2 k \left(\frac{1}{R}\right)^{N-1} \tag{3}$$

The linear approximation of the ratio of forces is[73]

$$\frac{f_{power}(r)}{f_{inv.\ square}(r)} = \left(\frac{m}{n}\right)^2 N + \left(\frac{m}{n}\right)^2 (1-N) \left(\frac{R}{r}\right) \tag{4}$$

Comparing equations (1.2b) and (1.4), we arrive at Newton's result

$$\left(\frac{m}{n}\right)^2 = \frac{1}{N} \tag{5a}$$

We read directly from equation (1.5a) how deviations from the inverse square law affect the motion of a near circular orbit. A complete orbit will require $360^o/\sqrt{N}$. Any N other than one, that is any power law other than the inverse square, will yield a motion of the apsides. For N = 1/4, so that that $f_{power}(r) = k'/r^{11/4}$, m/n = 2, which means that the planet moves at twice the speed returning to the upper apsis after $2x360^o = 720^o$. For N = 4, $f_{power}(r) = k'r$ and the force of attraction grows linearly with radial distance r. For this case m/n = 1/2 so that the planet moves at half the speed, returning to its upper apsis after just $(1/2)x360^o = 180^o$. Or one can invert the

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[72] For interpretational purposes, it is best to read this relation backwards. If one has a power law with some given constant k', solve (3) and (5a) to find the value of k in the inverse square law that produces the associated non-rotating elliptical orbit.

[73] That is, $f_{power}(u)/f_{inv.\ square}(r) = k'u^{3-N}/ku^2 = (k'/k)u^{1-N} = (m/n)^2 U^{N-1}u^{1-N}$, using (1.3). Expanding u^{1-N} as a power series in u=1/r about U = 1/R, we recover $u^{1-N} = U^{1-N} + (1-N)U^{-N}(u-U)$ up to linear terms. Therefore $f_{power}(u)/f_{inv.\ square}(r) = (m/n)^2 + (m/n)^2(1-N)R(u-U) = (m/n)^2 N + (m/n)^2(1-N)Ru$ which is (4).

result to give it in Newton's form. If one seeks a motion of m/n, that motion is generated by a force law

$$f_{power}(r) = \frac{k'}{r^{3-(n/m)^2}} \tag{5b}$$

The Problem of the Moon and the Sensitivity of Newton's Method

Newton could apply his method directly to the motion of planets, for which no apsidal motion was then known, and conclude that the attractive force was an inverse square force. The moon was not so obliging. Its apsides advanced by 3°3' per orbit. This, Newton noted in Book III, Proposition 3, could be explained by a power law with radial dependence $1/r^{2^{4/243}}$, a slightly faster dilution than the inverse square. This analysis presumed, however, that there are no forces acting on the moon other than the attraction of the earth. That assumption is incorrect. The moon is also acted on by the sun's attraction and that attraction, Newton determined, was the source of advancing apsides. (See Cohen, 1999, pp. 257-65.)

Once such additional forces were factored out, Newton's method was highly sensitive to deviations from an inverse square law. It is well know that Einstein's general theory of relativity differs only minutely in observable terms from Newton's theory when it comes to planetary motions in our solar system. These deviations were first affirmed in the apsidal motion of Mercury. Its apsidal axis is now known to advance by 574 arc seconds in one century. This advance is minute. An arcsecond is 1/3600th of a degree. In this motion, 531" are due to the perturbing effects of the other planets, leaving a residual advance of 43" per century. Unless another cause can be found, Newton's method assures us that this anomalous advance results from a deviation in the law of attraction from the inverse square law. That is the modern explanation: according to Einstein's general theory of relativity, the attraction of gravity is only approximately governed by an inverse square law. But in the realm of our solar system, the approximation is extremely good. As the 19th century astronomers knew, if the 43" were due to a power law with an exponent other than 2, using Newton's result (5b), that power law would be

$$\frac{1}{r^{2.000\,000\,159}}$$

That minute adjustment is how much Einstein needed to correct Newton in the realm of our planets.

9 Conclusion: The Problems of Inductive Generalization

The family of inductive generalization faces three problems: strength, language *and* reach. *The tradition of elaboration of inductive generalization has really only had success with the problem of* reach.

Three Problems

In all their forms, inductive generalization faces three problems:

Strength. Just how much inductive support does an instance give to its generalization? We have seen that if may be anything from the most slender to the most powerful. Nothing within the schemes themselves indicate what that strength is.

This remains the outstanding problem of inductive generalization. No form of inductive generalization successfully addresses it. Hempel's satisfaction criterion explicitly abandons the problem by merely allowing that the criterion affords support at any level of strength with the strength to be ascertained separately. Perhaps the only form that makes some advance is demonstrative induction since it proceeds with deductive certainty. But that is at the cost of eliminating the ampliative component of induction entirely. In practice, the uncertainty over strength of inductive generalization will reappear in demonstrative induction as an uncertainty in one or more of its premises.

Language. Which is the generalization to be drawn from the known instances? The answer will depend on how we describe the instances. Different forms of inductive generalization use different languages and interpretative moves and may well generate different generalizations.

It is not automatic that this is a problem. One piece of evidence can certainly be evidence for different things. That some person drinking tainted water falls ill can be evidence for the conclusion that all people drinking tainted water fall ill; or it can be evidence that the cause of the illness was the tainted water. However it can be made to be a problem by clever selection of examples, as happens the problem of "grue" (not discussed here). For those who already know of grue, my view is that the choice of language reflects one's background assumptions and that these assumptions are in turn subject to evidential scrutiny.[74]

> *Reach*. The reach of enumerative induction is limited to the replacing of a "some" by an "all." Induction in science is far richer and the reach of enumerative induction must be extended if it is to capture important parts of inductive reasoning in science.

The tradition of work in inductive generalization has had most success with this third problem. The sequence of forms of inductive generalization display an increasing richness in the expressive power used. The strongest extension comes with bootstrap confirmation and demonstrative induction, which allow passage from evidence expressed in the crudest of observational terms to the most aetherial of theoretical heights.

Hand in hand with success has come a failure of sorts. As long as the reach of inductive generalization is limited, the natural generalization of some instance is easy to see. As a result, early forms of inductive generalization could offer themselves simultaneously as methods of discovery. With greater reach, however, it is no longer clear which is the natural generalization of some item of evidence so the tradition of inductive generalization lost its connection with methods of discovery. The transition occurred with the passage from Mill's methods to Hempel's satisfaction criterion.

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[74] Thus these background assumptions may treat "grue" and "green" differently. With plausible background assumptions, only green will come out as a natural kind term, for example, so that one has good reason to project the property green but not grue. What if we "grue-ify everything," including our background assumptions, in an attempt to keep the symmetry of grue and green? In Norton (2006), I argue that it does not help and that, in the end, "grue" presents no worse problems than the familiar difficulty that a pattern can be continued in arbitrarily many different ways.

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