When Induction Was About Concepts

John P. McCaskey
Stanford University

The philosophy professor says, “This swan is white. That swan is white. And so is the other,” and asks, “May we legitimately infer that all swans are white?” In this way the students are introduced to induction or, as we seem always to call it, the “problem” of induction. Deduction presents no real problem, we say, but induction brings no end of trouble. It is stubborn, recalcitrant, rebellious. We can almost hear Henry Higgins singing, “Why can’t a woman be more like a man?” or “Why can’t induction be more like deduction? / Deduction is so honest, so thoroughly square / eternally noble, historically fair. / Why can’t induction be more like deduction ?” Higgins does not sing that, but Nicholas Rescher, for example, says, “An inductive inference can always be looked upon as *an aspiring but failed deductive inference.*”[1] Rescher speaks for virtually all epistemologists of the last hundred years. But this position was not always so common. There were times when induction was not thought to be a problematic sort of inference. In fact, there were times induction was not thought to be primarily about propositional inference at all. Induction was about forming sound concepts and good definitions. This essay recalls those times.

Recalling them is important for three reasons. First, doing so will help check our temptation to think our predecessors were too naïve, careless, or stupid to see the obvious. “How could Francis Bacon have thought his method of induction would lead to certainty?!” “How could William Whewell not have appreciated Hume’s challenge to induction?!” “How could Aristotle have thought all induction reduces to complete enumeration?!” But these men were not stupid. Indeed, I claim, if they had simply conceived of induction as we do, what is obvious to us would

[1] Rescher, *Induction*, p. 10. Emphasis in original.

have been obvious to them. But they did not so conceive it. To understand what these philosophers wrote about induction, we need to read them on their terms, not ours.

Second, we need to appreciate that many remarkably successful scientists, from William Harvey and Robert Boyle to John Herschel and Charles Darwin, claimed to be following an inductive method that would not be recognized as one today. When discussing Darwin, David Hull wrote, "Philosophers of science [today] commonly concede that early philosophers of science were hopelessly confused."[2] But those successful early scientists were enthusiastic and careful students of those early philosophers. If the philosophers were hopelessly confused about method then the scientists were hopelessly confused about how they discovered what they discovered. That would be strange indeed. If we want to understand the methodology of scientists from Bacon's day to Whewell's, we need to understand the inductive philosophy they claimed to practice—and understand it as they did, not through a lens that makes it look hopelessly confused.

Third, maybe induction is a vexing problem to us because we took a wrong turn somewhere. Maybe there is something good in the old view. Consider: Our professor continues the lecture by putting up his photo of a black Australian swan. "Even if all the swans you've ever seen are white, what happens when you then go to Australia and see this, a black swan? With induction, you can never be certain." A student replies, "But that black thing is not a swan." "Of course it's a swan." "No, it's not." "Yes, it is." "No, it's not." There is simply no way to know whether all swans are white without criteria for knowing what is and is not a swan. There is something right in the old idea that, at bottom, induction is about forming good, well-defined concepts; that if your concepts are ill-delimited, no universal proposition using them will be possible; and that if you get the concepts right the universal statements will follow straightaway.

Let us review the history and then look at three examples of old-style induction in scientific practice.

[2] Hull, *Darwin and His Critics*, p. 29.

Antiquity: Aristotelian/Socratic Induction

Aristotle's view of induction (*epagōgē* in Greek, then *inductio* in Latin) dominated antiquity, but what exactly Aristotle's view was can be difficult to discern. We need to look back beyond the Neoplatonic, Alexandrian interpretation of late antiquity that was bequeathed to both Arabic and Latin Scholastics and that colors our view of Aristotelian induction even today. That means we need to set aside the belief that Aristotle's famous chapter on induction, *Prior Analytics* B 23, says that induction is a kind of propositional inference made good by complete enumeration. Aristotle did not say this, and if he had it would have been wholly out of character. For what he says in the rest of the surviving corpus indicates a much different conception of induction.

Aristotle's most frequent use of the noun "induction" *(epagōgē)* or the adjective "inductive" (*epaktikos*, as in *epaktikos logos,* "inductive reasoning") is in the *Topics*. Early in the first book, in a short chapter introducing induction, Aristotle gives this definition: "Induction . . . is a proceeding from particulars to a universal."[3] The definition has been conventional ever since, but there is an ambiguity here. Did Aristotle mean proceeding from the observation of particular things to formation of a universal concept? Or did he mean proceeding from particular propositions to the statement of a universal proposition? That is, is induction primarily a process for forming *universal concepts* or for forming *universal propositions*? Is it a process of conceptual abstraction, the result of which can then ground universal statements, or is it at bottom a process of propositional inference? Using modern terminology: In human cognition does ampliation take place fundamentally at the conceptual level or at the propositional level?

Aristotle does not say. But one of the best hints to what he thought comes from what he did not say. He regularly introduces induction to help explain some other, presumably less clear, issue. He generally introduces the term without preface or explanation. He never provides, as he does with so many other subjects, a list of competing views. He never says that his understanding

[3] Aristotle, *Topics,* 1.12 105a13–14.

of induction is new, controversial, or contested. In one passage, he mentions induction as third in a list of three. After explaining the first two, Aristotle passes over the third by saying only, "What sort of thing induction is, is obvious."[4] Aristotle expected his students to already know what induction is and to assume his own view of it was conventional.

What conception of induction would an Athenian student of the late fourth-century have had? In the *Metaphysics*, Aristotle attributes the introduction of "inductive reasoning" to Socrates,[5] again without suggesting the term is unfamiliar. In the *Topics*,[6] Aristotle says that effective use of comparisons *(parabolai)* is characteristic of a good inductive reasoner and in the *Rhetoric*,[7] that the use of such *parabolai* was Socrates' distinctive method. At the same place in the *Rhetoric*, Aristotle gives an example of what he says is a Socratic induction,[8] and it is comparable to many of Aristotle's own, including this one, given immediately after the definition cited earlier: "For instance, if the pilot who has knowledge is the best pilot, and so with a charioteer, then generally the person who has knowledge about anything is the best."[9] The similarity between this example and the countless portrayals in Plato's early dialogues of Socrates' search for universal definition is undeniable. To one of Aristotle's students, Socrates would have been a figure of the recent past and his distinctive style of arguing well enough known. When Aristotle used the word *epagōgē*, he expected his students to understand it to be none other than what they would have known to be Socratic induction.

That passage in the *Metaphysics* reads, more fully, "Socrates was occupying himself with the excellences of character, and in connection with them became the first to raise the problem of universal definition. . . . For two things may be fairly ascribed to Socrates—inductive reasoning

[4] Aristotle, *Topics*, 8.1 157a8.
[5] Aristotle, *Metaphysics*, M 4 1078b28.
[6] Aristotle, *Topics*, 8.14 164a16.
[7] Aristotle, *Rhetoric*, 2.20 1393b5.
[8] Aristotle, *Rhetoric*, 2.20 1393b4–8. The example is actually of a paradigm, which Aristotle explains is a "rhetorical induction" (*Rhetoric*, 1.2 1356b5). In such an induction, the universal is left unstated but silently presumed in subsequent arguments.
[9] Aristotle, *Topics*, 1.12 105a13–19.

and universal definition."[10] Two examples can illustrate Socratic induction. In the *Ion*, Socrates observes that the excellent pilot is one who knows best what to do at sea. He makes similar observations about a doctor, cowherd, wool-maker, and military man. Socrates makes the universal statement that the master of any craft "is the one who knows best matters falling within its subject-matter."[11] But this is not an inferential conclusion drawn from premises. Nor is Socrates simply giving examples. The instances are components of an exploratory process to find the properties that define a class. In the *Euthyphro,* Socrates and Euthyphro search for the definition of piety. Euthyphro proposes a definition. Socrates claims the proposal covers only a subset. Euthyphro concedes and offers an alternative. Socrates says the new proposal is ambiguous. Euthyphro alters it, but to no avail. A mere listing of examples is not succeeding. They need a more structured approach. Socrates proposes they first identify a wider category, a genus, of which piety is a species. Then they can narrow the genus with differentiae. The "inductive reasoning" and "universal definition" that Aristotle attributed to Socrates are not two unrelated things. For Socrates, inductive reasoning is the iterative, compare-and-contrast process by which one identifies appropriate genus and differentiae and thus comes to a universal definition.

But for Aristotle (maybe for Socrates too; we don't know) induction can serve a broader end also. For Aristotle, induction is used to find all properties, not just defining ones, that characterize all and only the members of a class. Aristotle called each such property an *idion kata hauto*, a property that distinguishes by the nature of the thing, or a *proton katholou*, a primitive universal.[12] Such a property counter-predicates and converts with the class just as a

[10] Aristotle, *Metaphysics,* M4 1078b24–29. Ross's translation, slightly modified. Cf. a similar passage in *Nicomachean Ethics,* 6.3 1139b26–33.

[11] The observations here about Socratic induction are drawn from Gregory Vlastos, "Editor's Introduction," in *Protagoras*, xxix, n 18 and *Socrates: Ironist and Moral Philosopher*, p. 267–8. The quote is from p. 268 of the latter.

[12] Because of its importance, later commentary ignored the other types and used *idion* for *idion kata hauto*. Latin *proprium* is regularly said to be the same as Aristotle's *idion* but is in fact the same as Aristotle's *idion kata hauto.* Aristotle also uses *idion haplos* (distinguishing without qualification) for *idion kata hauto*.

definition does, but might not be part of the definition. Having three angles, having three sides, and having angles that sum to 180° are all such properties of triangles. Anything that has one of these properties has the other and is a triangle. Being a rational animal, a featherless biped with broad nails, an animal by nature civilized, an animal that cooks its food, and an animal that laughs all counter-predicate with man, though only the first makes a good definition. While Socrates seems to have offered nothing beyond frustrating interrogation to someone in search of counter-predicating properties, Aristotle offered a whole book of guidelines, book 5 of the *Topics.*

In *Prior Analytics* B 23, Aristotle does not say that the deduction he presents about bileless and long-lived animals is an induction made good by pretending there has been a complete enumeration.[13] The induction is not the syllogistic inference. It is rather a separate compare-and-contrast process that was performed outside of this chapter and followed, presumably, the guidelines in book 5 of the *Topics* and by which Aristotle discovers that being bileless and long-lived are both *idion kata hauto* properties of a certain group of animals. Once that induction is complete, a deduction from induction *(ho ex epagōgē sullogismos)* can be formulated in which the minor premise operates just as one obtained by complete enumeration would operate.

Even if Socrates did not, Aristotle used induction to solve Meno's paradox. To find a definition we need to know what group we are defining, but to identify the group we need a definition. Aristotle's solution to the apparent chicken-and-egg problem is to distinguish two ways of knowing. He says, "Before the induction . . . you should perhaps be said to understand in a way—but in another way not. . . . Otherwise the puzzle in the *Meno* will result."[14] Aristotle's solution works like this: At first we group particulars based on observable similarities and differences. Some things are alive, walk on two feet, move by their own initiative, go about in the marketplace, live with others, are able to hear and see, have five fingers, are able to learn, can

[13] McCaskey, "Freeing Epagōgē."
[14] Aristotle, *Posterior Analytics,* 71a25–31 (Barnes' first translation).

be deceived. (These are attributes used in examples in *Topics* 5). We find it useful to group these individuals together and call each a "man." We have a definition, but it is merely a descriptive or nominal one. And sometimes, when we use the description, we run into difficulties. For example, if the being is blind or has only four fingers is it still a man? But the description is good enough for us to compare and contrast this group with others and in so doing identify one property which is the very essence of each group. The cognitive product, the mental integration that is a concept, matures as we proceed from having a descriptive definition to having an essential one. We conclude, "Man is the rational animal."

That is, we first understand our concepts in an immature, descriptive way and then in a mature, essentialized way. The iterative compare-and-contrast process that gets us through this process is Aristotelian, or Socratic, induction.

Scholasticism: A Syllogism Formed By Complete Enumeration

That is how *epagōgē* and then, after Cicero coined the term, *inductio* were understood in the ancient world. To the same extent induction is now associated with David Hume, it was in antiquity associated with Socrates and the process Socrates tried using to obtain universal concepts and good definitions.

In late antiquity this was changed, little by little, primarily by Neoplatonists, starting with Clement (fl. c. 150 AD). Clement says, "Induction leads to the universal and the definition,"[15] and in this he is wholly conventional. But his understanding of the process is new. Clement takes material from *Posterior Analytics* B 5–7, and casts it as saying that a definition is the "summation resulting from Division,"[16] that is, that the whole is defined by a complete enumeration of its parts. Now this is a terrible distortion of Aristotle, but not the first that would come in the following centuries, as philosophers sought to reconcile Plato and Aristotle. By the early sixth century, the time of Simplicius, Philoponus, and Boethius, understanding of induction

[15] Clement, *Stromata* 8.6.
[16] Clement, *Stromata* 8.6.

had fully changed. Induction became a kind of propositional inference, inferior to deduction. It had force only to the extent it could be rendered, by complete enumeration, as a deduction. That is what *Prior Analytics* B 23 was taken to say.

That chapter and its Neoplatonic interpretation were nearly all that was passed on of Aristotle's writings on induction. In both East and West, Aristotle's scientific works, where readers could have seen Aristotle use what he claimed was induction, and the *Topics* and *Posterior Analytics,* where however fleetingly Aristotle described induction, fell out of use. In the West, the *Topics* and the *Analytics* got replaced by Boethius's treatments, *De Topiciis Differentiis* and *On Categorical Syllogisms*. These were supposedly presentations of Aristotle, but their interpretation of Aristotle was that of late antiquity, and that interpretation got passed on. Virtually all of those countless textbooks on "Aristotelian logic" in the thirteenth through seventeenth centuries were modeled not on Aristotle's own writings but on Peter of Spain's *Tractatus,* and Peter's textbook was based on Boethius's tracts. What medieval, Renaissance, and even later philosophers thought to be Aristotelian logic was really Aristotle's logic as recast by Neoplatonic writers of late antiquity such as Simplicius, Philoponus, and Boethius. And these writers took induction to be a kind of propositional inference made good by complete enumeration. Arabic writers including al-Farabi, Avicenna, and Averroës did the same. The reintroduction of the full Aristotelian corpus into Europe in the thirteenth century had no immediate effect on induction theory.[17]

Humanism: Rediscovery of Socratic Induction

Occasionally Scholastic commentators, faced with the discrepancies between the canonical interpretation and the scattered references found in the *Posterior Analytics* and the *Topics,* would attempt a reconciliation, but mostly the discrepancies were passed over with little comment. Jean Buridan was the first to give the discrepancies serious attention and to consider the possibility

[17] For an account of the transmission of induction theory from late antiquity to the Renaissance, see McCaskey, "Regular Socratis," ch. 2.

that the conventional view of *Prior Analytics* B 23 was misleading and that the picture one forms from the scattered references was Aristotle's actual position on induction. But Buridan's notes on this received little circulation.[18] The big change came in the fifteenth century with the rise of humanism. A big factor was new interest in the subject matter of the *Topics* and a desire to return to treatments that predated Boethius's. This got readers back to Cicero's *Topics* and Aristotle's. These works cast induction as Socrates' process of forming concepts and definitions.

Aristotle had said that what induction is, is obvious, but in a commentary published in 1542 on the chapter in which Aristotle gave his definition of induction, Agostino Nifo said that there was now great uncertainty about what induction is.[19] The debate affected English textbooks. In the first edition of the first logic textbook published in English, Thomas Wilson's *Rule of Reason* of 1551, the author gave a thoroughly conventional Scholastic treatment.[20] But in the second edition, "newly corrected," published only one year later, Wilson added a section, longer than the first, discussing what he explained is another kind of induction, "called . . . Socrates['] Induction."[21] John Seton's *Dialectica*, first published in 1545, jumbled the two kinds together. Both textbooks were popular in the school days of Francis Bacon.

More engaged with Aristotelian and Renaissance philosophy than generally recognized nowadays,[22] Bacon (1561–1626) was the first to explicitly make induction the centerpiece of an epistemological system, and his conception of induction was essentially that of Socrates and (properly understood) of Aristotle. It is to Socratic induction that Bacon refers when he says, "[the correct procedure] has not yet been done, nor even tried except only by Plato, who certainly

[18] The important text is Buridan, *Summulae de Dialectica*, 6.1.3, 6.1.5, and 8.5.4. On Buridan's induction, see Thijssen, "Buridan and Nicholas on Causality and Induction." In all Renaissance printings of Buridan's *Summulae*, his innovative commentary on induction got replaced with more conventional remarks by one John Dorp (fl. 1393–1405).

[19] Nifo, *Topicorum*, f. 18r.

[20] Wilson, *Rule of Reason*, ff. H5v–H6v.

[21] f. 66r in the 1552 edition; f. 32v in later editions.

[22] Here is just one bit of evidence. In about a page of introductory remarks to the *Novum Organum* (in the *Distributio Operis*), forty-one times Bacon uses or cites technical terms or issues in recent Aristotelian scholarship. He knew his Aristotle, or at least Scholastic Aristotelianism, and presumed his readers did too.

makes use of this form of induction to some extent in settling on definitions and ideas."[23] Bacon's induction is a codification of Socrates'. But Bacon also stressed something that Socrates and Aristotle let pass without comment. Bacon directly illustrated how well-formed concepts could ground universal propositions.

Bacon came to induction late and indirectly. His interest was not primarily epistemological but practical. He sought some method by which someone could confidently effect a property or feature where it had never existed before. The method would need to ensure two things, what he called certainty and liberty.[24] The first, Bacon thought, was easy—if one ignored the second. It takes no great genius or much method to know that heat melts butter (my example, not Bacon's). The next dollop thrown on the skillet will melt. We can continue doing what we have always done, and we know what will happen. But what of cheese? What about wax? Clay? What about a new artificial material, envisioned but not yet produced? As we exercise our liberty, as we try things increasingly dissimilar, we lose our certainty—at least without a proper method. Bacon wanted a method that would allow liberty without sacrificing certainty.

To solve his problem Bacon turned to Aristotle's concepts of *kata pantos* and *katholou proton*.[25] A property that is true *kata pantos* is true for *all* members of a class. But a property that is true *katholou proton*, recall, is true of *all and only all* members of a class. Thus a proposition predicating a *katholou proton* property is convertible; that is, subject and predicate can be swapped. But for practical purposes, identifying properties that are *katholou proton* is not enough. Having a flat surface, feeling smooth, appearing bright, and being able to see your face in it may all be *katholou proton* properties of a metal, but to make a metal all of these, you need

[23] Bacon, *Novum Organum* 1.105.

[24] Bacon, *Valerius Terminus* 11, p. 235 in Spedding.

[25] Bacon, *Valerius Terminus* 11, p. 236 in Spedding ("This notion Aristotle had in light, though not in use"); *Advancement of Learning* 2.17.12; *De Augmentis Scientiarum* 6.2. In Latin these went by the names *de omni* and *universaliter*, respectively, but Bacon preferred either the Greek, as in his published works, or the Ramist forms "rule of truth" and "rule of prudence," as in *Valerius Terminus*.

to know which causes the others, or, to use Bacon's term, which is more "original."[26] It is not enough to know that properties "cluster and concur";[27] it is important to identify which property is the cause. But which cause? Bacon dismisses the final cause as inapplicable in cases outside of human actions. And he thought knowing just the material and efficient causes could provide certainty but not liberty. Knowledge of these causes can only help to "achieve new discoveries in material which is fairly similar."[28] What is needed, Bacon says, is to identify the "form or formal cause."[29] The formal cause is what makes something the kind of thing it is. Knowing the formal cause would allow man to be a creator, to effect properties in substances that have never had the property before. This is the kind of knowledge that is power. Bacon says "Aristotle's school" was right "that there is no true knowledge but by causes, no true cause but the form, no true form known except one."[30] But, he says, Aristotle's school misunderstood the form and consequently—and wrongly—regarded its discovery as hopeless.[31]

A form, Bacon says, is merely a certain arrangement and motion of (frequently imperceptible) physical components. In Book 2 of the *Novum Organum,* Bacon presents a method for identifying that arrangement and motion. He says the search should begin with a comprehensive inventory of observed instances, related absences, and related variations. One must then begin an iterative process of comparison, and he describes twenty-seven kinds of observations ("prerogatives") that make particularly useful comparisons. The first goal is to identify the genus of whatever one is analyzing. Bacon's example is heat, and he concludes that heat is a kind of motion. "Motion . . . is . . . the genus of which heat is a species."[32] The next goal is to use the same process to identify the differentiae. The result will be a definition, the identification of what makes something what it is, the identification of the formal cause.

[26] Bacon, *Valerius Terminus* 11, pp. 240–1 in Spedding.
[27] Bacon, *Valerius Terminus* 11, p. 240 in Spedding.
[28] Bacon, *Novum Organum* 2.3.
[29] Bacon, *Valerius Terminus* 11, p. 241 in Spedding; *Novum Organum* 2.2.
[30] Bacon, *Valerius Terminus* 11, p. 239 in Spedding.
[31] Bacon, *Novum Organum* 2.2.
[32] Bacon, *Novum Organum* 2.20. Spedding translation.

The practical result will be a power that gives both certainty and liberty. Bacon makes this remarkably universal claim:

> *If in any body you can arouse a motion . . . [of this certain kind], you will certainly generate heat.* It is irrelevant whether the body is elementary (so-called) or imbued with heavenly substances; whether luminous or opaque; whether rare or dense; whether spatially expanded or contained within the bounds of its first size; whether tending toward dissolution or in a steady state; whether animal, vegetable or mineral, or water, oil or air, or any other substance whatsoever.[33]

Bacon stresses that this particular kind of motion is not the efficient cause of heat, that is, it is not the case that there is one thing, the motion, that produces another thing, the heat. Rather the particular kind of motion is what heat is. So, naturally enough, if you can produce this kind of motion, you produce heat. The motion and the heat are the same thing.

Looked at from the perspective of Scholastic logic, Bacon makes an important shift. A conventional Scholastic induction would set out particulars like this:

Exploding French gunpowder is hot.

Exploding German gunpowder is hot.

Exploding English gunpowder is hot.

and then ask: Have we surveyed enough explosions to know if all exploding gunpowders are hot? Bacon is saying that is the wrong question. The right question is: What is heat? Bacon shifts attention from the subject to the predicate and looks at the same problem this way:

Exploding French gunpowder is hot.

Exploding German gunpowder is hot.

Exploding English gunpowder is hot.

[33] Bacon, *Novum Organum* 2.21. Italics in original.

And then he insists that you can learn nothing from positive instances only. You must have a wide range of contrasts. Consider unignited French gunpowder. Consider burning charcoal. In fact, consider everything from the heat of burning leaves, of sunshine, and of horse dung to the lack of heat or varying grades of heat in moonlight and starlight, different animals, natural springs, and rotting vegetables. For Bacon an induction is performed not by positive instances that confirm, but by comparisons that contribute to the definition of the predicate. And, of course, finding that definition by this process of compare-and-contrast is the essence of Socratic induction.

For two centuries after Bacon, any substantive work on induction was done along this Socratic/Humanist line and, more generally, the whole business of epistemology was different than it had been. Major works on human reasoning were no longer logic treatises but works with titles such as *An Essay Concerning Human Understanding* (Locke), *An Enquiry Concerning Human Understanding* (Hume), and *Essays on the Intellectual Powers of Man* (Reid), that is, tracts about how man comes to abstract knowledge from sense perception. The most important work on induction *per se* was that of William Whewell (1794–1866).

Whewell explored even deeper than Bacon had the interrelationships between universal propositions, concept-formation, and definitions. Bacon had approached inductive inquiry the way Socrates and Aristotle had: We already have a concept, we can identify its instances, and we have a description (or overlapping descriptions) that provisionally function as a definition. Now, to remove ambiguity, add precision to our knowledge, and raise it to the level of scientific understanding, we use induction to identify the essence, the formal cause, of what we are studying. Once we have identified that essence, we can legitimately make certain kinds of unqualified universal statements.

Whewell drew attention to the fact that in much scientific inquiry, the researcher in fact does not begin with a ready-formed concept. The forming of the concept (or "conception" in Whewell's terminology) itself can be a crucial part of scientific discovery. For example, Newton's integration of facts about falling apples, revolving moons, planetary orbits, tides, comets, and so on did not merely result in better definitions of old concepts, such as *gravitas*, but more importantly the formation of the new concept *mass*. Newton had at hand some facts, but the facts were not cognitively held as a single unit. They were expressed in statements, paragraphs, lists, tables, even whole chapters and books. Newton's inductive breakthrough, Whewell says was to integrate ("colligate") a variety of facts into a single cognitive unit, assign to it a term ("technical term"), and identify its definition. For example, the conception of universal gravitation, says Whewell, *includes* the fact of heliocentric motion, *includes* the fact of the precession of the equinoxes, *includes* the conception of terrestrial weight, and so on.[34] A conception is not merely a binding of multiple instances of a common attribute. It is rather a binding of the facts themselves—not just the common attributes, not just what is in the definition, but indeed all the attributes and even propositions associated therewith. Induction, for Whewell, "is a term applied to describe the process of a true Colligation of Facts by means of an exact and appropriate Conception." The definition of the concept then "gives the last stamp of distinctness to the Conception; and enables us to express, in a compact and lucid form, the . . . propositions into which the . . . Conception enters."[35] Consequently, scientific laws obtain their warrant from the strength of the definition, that is, from the quality of the induction. For a large class of universal scientific statements, then, " true by induction" and " true by definition" mean the same thing.

Before we consider a few scientific developments that exemplify this approach to induction, a two brief asides are required, one involving David Hume and one that brings our narrative up to date.

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[34] Whewell, *Philosophy*, bk. 11, ch. 6, § 1 (p. 162 in Butts). Whewell himself uses such italics when making this point.

[35] Whewell, "Of Induction," p. 284 in Butts edition. See also *Philosophy*, bk. 11, ch. 2, § 6–10 (pp. 110–114 in Butts).

Eventually there was a turn away from Baconian induction, but it did not start with David Hume. Hume wrote virtually nothing, or at least nothing critical, on what in his day was called induction. He said he intends to base the *Treatise of Human Nature* on the philosophy Bacon introduced. He uses the term *induction* there only twice, in both cases to defend some argument he is making.[36] In none of the instances does Hume suggest he sees any problem with induction or has anything new to say about it. A century later, Whewell and his contemporary, John Stuart Mill, managed to write, between them, at least seven volumes on the history, theory, and practice of induction without finding it necessary to give Hume significant attention. As we will see, the association between Hume and induction is a result of developments in the late nineteenth century.

Whately's Revolution: Introduction of the Uniformity Principle

It was Richard Whately (1787–1863) who led us back to the Scholastic notion that induction is about propositional inference and not about formation of concepts and definitions. The change began with an article he published in 1823 and was completed by the time Stanley Jevons (b. 1835) was writing about logic in the 1870s.

Whately was a logician at Oxford in the 1810s. He and his teacher Edward Copleston lamented the abandonment of Scholastic logic in university curricula, and they set out to bring it back. Some things, however, would need to be different. Given the prominence induction had acquired over the previous two centuries, the revival would need to give a central role to induction. But the old Scholastic understanding of induction as a syllogism made good by complete enumeration always did have some problems. Copleston recognized this and introduced a profound change. The Scholastics had taken induction to be a kind of inference

[36] *A Treatise of Human Nature* 1.2.1.2, 1.3.7.7. The word does not appear in *An Enquiry Concerning Human Understanding* or *An Abstract of a Book Lately Published, Entitled A Treatise of Human Nature*. It appears once in *An Enquiry Concerning the Principles of Morals,* at the very beginning, again in a passage indicating Hume saw no problem with induction. For the reference to Bacon, see Hume's full title, *A Treatise of Human Nature: Being an Attempt to Introduce the Experimental Method of Reasoning into Moral Subjects,* and then Hume's description of the "experimental method" in the introduction to Book 1.

made good by conversion to a deductive syllogism through the introduction of the implicit premise that all instances had been enumerated or could be treated as if they had been. In the Scholastic syllogism, this implicit, or "suppressed," premise was the minor, as in this example:

(maj) This magnet, that magnet, and the other magnet attract iron.

(min) [Every magnet is this magnet, that magnet, and the other magnet.]

(con) Therefore, every magnet attracts iron.

("This magnet, that magnet, and the other magnet" forms, as a unit, the middle term.)

Copleston and Whately agreed that an induction is basically a propositional inference and forceful only to the extent it can be rendered as a syllogism by introduction of an implicit premise. But for them, that premise was the major, not the minor. Now to make this work required some maneuvering that would never have been acceptable to Scholastics. In the earlier understanding, the minor term is the subject of the conclusion and the major term the predicate—regardless of the grammar of the sentence. Whether one says, "Magnets attract iron," "Attracting iron is a property of magnets," or "The property of attracting iron belongs to magnets," the subject would be magnets and attracting iron the predicate. But Copleston insisted that logic is about language, not things, and so "minor" and "major" refer to grammatical subject and predicate, not ontological subject and predicate. So he ended up with this:

(maj) [A property of the observed magnets is a property of all magnets.]

(min) The property of attracting iron is a property of the observed magnets.

(con) Therefore, the property of attracting iron is a property of all magnets.

(con) Therefore, all magnets attract iron.[37]

This proposal is the introduction into induction theory of a uniformity principle: What is true of the observed is true of all. Once induction is conceived to be a propositional inference made good by supplying an implicit major premise, some sort of uniformity principle becomes

[37] Cf. Copleston, *Examiner Examined*, pp. 35–43 and Whately, *Elements of Logic*, ch. 2 "Synthetical Compendium," pt. 3 "Of Arguments," § 7 "Of Enthymeme, Sorites, etc.", p. 124 in the first edition.

necessary. When induction was not so conceived there was no need for a uniformity principle. There was not one in the induction theories of Aristotle, Cicero, Boethius, Averroës, Aquinas, Buridan, Bacon, Whewell, or anyone else before Copleston and Whately.

In 1826, Whately's article was printed as a book. It became the standard textbook on logic until John Stuart Mill's *System of Logic* supplanted it. Mill himself applauded Whately's new syllogism as "original" and "extremely important." [38] If this one syllogism had been proposed two hundred years earlier, Mill suggested, there would have been no need for that whole era of Baconian induction.[39] William Hamilton, however, who knew much more about logic and its history than either Whately or Mill, warned that there would be no end of trouble if Whately's syllogism were adopted. Such an adoption would be "palpably suicidal,"[40] for there would be no way to justify that suppressed major. It is just another statement of the very question at issue. Mill, undeterred, adopted Whately's syllogism enthusiastically and made it the very foundation of his theory of induction.[41]

Even though Whately preferred the modified Scholastic conception, he believed the Baconians were using the word in the "original and strict sense of the term."[42] This turned around over the next fifty years. In 1847, Augustus De Morgan thought Whately's use was the "original and logical sense"[43] even though people still used the term in the Baconian way. In an 1874 textbook, Mill's disciple Alexander Bain warned his students against the Baconian usage. "By Induction, we arrive at *Propositions*, . . . [It is not Induction] where what we arrive at is a Notion or Definition."[44] In the 1870s works of W. Stanley Jevons, the view Bain warned against

[38] Mill, "Review of *Elements of Logic*," p. 169.

[39] Mill, "Review of *Elements of Logic*," p. 170.

[40] Hamilton, "Recent Publications on Logical Sciences," p. 231.

[41] Mill, "Axiom of the uniformity of the course of nature," *System of Logic,* bk. 3, ch. 3, § 1.

[42] Added in fourth edition. Whately, *Elements of Logic,* 4th ed., bk. 4, ch. 1, § 1, n. 2. In later editions, moved into the body of the text.

[43] De Morgan, "On Induction," *Formal Logic,* ch. 11, p. 215. De Morgan was a friend of Whewell's.

[44] Bain, "Meaning and Scope of Induction," *Logic,* bk. 3, ch. 1, § 1, p. 1. Italics in original. "Notion" was Bacon's technical term for a concept, the cognitive content corresponding to a word.

had wholly and silently vanished. Induction had once again become about propositions, not concept-formation.

It was only in the 1920s, once Whately's uniformity principle got associated with uniform causality, that Hume came to be associated with induction. John Maynard Keynes may have been the first to make the connection. In 1921 he wrote, "Hume's sceptical criticisms are usually associated with causality; but argument by induction—inference from past particulars to future generalizations—was the real object of his attack."[45] Only since the 1930s has it been common to hear induction treated as if its full and formal name were "the Humean problem of induction."

True by Definition

Understanding Socratic induction might help us understand a situation that often arises in the history of science and causes consternation among those who study that history. One group of historians and philosophers sees the development of a scientific law, e.g., Newton's second law of motion, Boyle's law of gases, or Ohm's law of resistance, as model cases of scientific induction, obtained by wide integrations of experimental and experiential data. Others look at the same results and see tautologies, analytic truths. Sure, they say, "F=ma" is true, but only because Newtonian mechanics defines force as the product of mass and acceleration, and once inertia, force, mass, velocity and all the other basic terms get defined, the rest of Newtonian mechanics all follows deductively. Newtonian mechanics, they say, is merely true by definition. Now let me not tackle Newtonian mechanics here but look instead at three smaller cases that suggest how one might reconcile the two opposing views of scientific laws: the cases of cholera, electrical resistance, and tidal science, all products of the nineteenth century, before Socratic/Baconian/ Whewellian methods lost philosophers' respect.

[45] Keynes, *A Treatise on Probability*, p. 272.

Cholera

We first learn of cholera from Celsus (c. 25 BC–50 AD). He defines it symptomatically: a disease of the intestines characterized by diarrhea, vomiting, flatulence, turning of the intestines, and ejection both upwards and downwards of bile (*biles*) that resembles water at first, but then as if meat had been washed in it, sometimes white, sometimes black or variegated.[46] We might expect him to define the malady with reference to the humor *choler*, but he does not; the name of the disease probably derives not from the humor, but from the word for a rain gutter (also *choler*). Be that as it may, when the term again gains currency for the disease, in the mid sixteenth century, it is again defined symptomatically and is distinguished from the humor, with which no particular connection is noted.[47]

Physicians knew little about what caused cholera. Consequently, although they could make general statements about it, they could make very few universal ones, few, that is, that applied with certainty to each and every case. (I distinguish general statements from universal ones. Apples are generally red, but not universally so. It is a universal and not just general truth that the angles of a Euclidean triangle sum to 180°.) One generalization physicians could make is that the different cases could be grouped into categories. By the early nineteenth century, several types were recognized. There was summer cholera, infants' cholera, cholera associated with poisoning, cholera associated with eating something undigestible, cholera with fever and cholera without, cholera that was contagious and that which was not. In the 1820s, reports were coming back to England from its colonies in India of severe cases of cholera, both sporadic and epidemic. Initial studies concluded that although these Indian, or "Asiatic," cases were more

[46] Celsus, *On Medicine*, bk. 4, ch. 11.

[47] *The [Latin-English] Dictionary of Sir Thomas Elyot* (1538): "[Latin] cholera: an humour callyd choler. Also a sickness called the colyke." *Dictionarium Linguae Latinae et Anglicanae* (1587): "[Latin] cholera: The humor called Choler: also a sickness of the stomach, with a troblous flixe and vomite ioyned with great daunger: the cholerike passion."

severe than common forms, they were indeed cases of cholera; they were "only different degrees of the same disorder."[48]

In October 1831, an epidemic of severe cholera, very similar to if not the same as the Indian kind, hit Britain. There were also epidemics in Continental Europe. In a letter to the *Cholera Gazette*, one Mr. A. Dalmas reported that the "Epidemic Disease now prevailing in London" is "*identical*" with those recently witnessed in Poland and Germany, for "the *causes* (so far as they can be appreciated) [reportedly poverty, damp abodes on rivers, etc.] are the same, the *symptoms* are the same, and the *appearances on dissection* are the same."[49] An article in *The Lancet* discussed whether patients with Indian cholera should be treated the same as patients with the cholera hitherto found in England.[50] The epidemic disease was classed as nothing but a very severe form of cholera, but this rapid-acting, epidemic, Asiatic kind soon became the type most on European's minds when someone said "cholera."

By the mid 1870s, it was widely accepted[51] that Asiatic cholera was spread by a "specific organic poison,"[52] carried in the vomit and stools of infected persons, but the nature of this poison was unknown. There was a suspicion that it might be bacterial, and when there was a cholera outbreak in Egypt in 1883, the Egyptian government sought the aid of European experts in bacteria. Louis Pasteur led a team from France, and Robert Koch a (competing) team from

[48] Ainslie, *Observations on cholera morbus of India*, p. 3. Cf. James Kennedy, *History of Contagious Cholera*, p. 178.

[49] Dalmas, "On the Identity of Cholera," p. 277. Italics in original.

[50] Ayre, "On the Pathology and Treatment of Cholera," p. 105–10.

[51] Though nominally about a US epidemic, Woodworth, *Cholera Epidemic of 1873*, provides a remarkably comprehensive account of what was known at the time about the history and science of cholera. It runs to 1053 pages and draws on American, British, German and French literature. John Snow's conclusion of the mid 1850s that cholera can be spread in contaminated water is treated as decisive and "well-known." Many details in the Woodworth report contradict Brock's assertion that "Snow's work was ignored." (Brock, *Robert Koch*, p. 141.) In general, late-nineteenth-century American medical publications that mention cholera exhibit a remarkable familiarity with the latest British, French and German publications. The *New York Times* covered Koch's researches and the doubts about it without delay. That Klein and Gibbes would raise their doubts in the November 29, 1884 issue of the *Medical Report* was reported in the *New York Times* on November 27.

[52] Woodworth, *Cholera Epidemic of 1873*, p. 8.

Germany. By February, 1884, Koch was confident he had identified the cause, a bacillus that he called the comma bacillus because of its curved shape.

He presented his findings at the First Conference for Discussion of the Cholera Question on July 26, 1884. After his presentation, he responded to sixteen questions.[53] The first, seventh and sixteenth were whether Koch believed that cholera is caused by a specific infectious material coming only from India, that the comma bacillus is indeed this infectious material, and that evidence of the comma bacillus is usable as a diagnostic. These three questions got grouped together and discussed first. Central to the discussion was how the cholera Koch was discussing related to other forms of cholera. The audience was coming to grips with what Koch had already decided: If there were no comma bacillus then, no matter how similar the symptoms, the patient did not have real *(wirklich, echt)* cholera.[54]

Initial reactions were similar in England and America.[55] Koch had claimed the comma bacillus was always and only found in the intestines or intestinal discharges of cholera patients, and moreover that the intensity of the malady corresponded to the number of comma bacilli. Other researchers were simply unable to confirm the perfect correlation Koch had reported.[56] There were cases of cholera where comma bacilli could hardly be found and cases of profuse infestation by comma bacilli but no, or only mild, cholera. Much was made of a claim by Finkler

[53] Transcription of the talk and the question-and-answer period appeared in the *Deutsche Medicinische Wochenschrift*, Nr. 31, 32 and 32a, and is in *Gesammelte Werke*, vol. 2, pt. 1, pp. 61–68. An English translation, without the question-and-answer period, appeared in *The British Medical Journal*, August 30 and September 6, 1884.

[54] E.g., when discussing whether the slightest cases of diarrhea could spread cholera, Koch said, "We can only arrive at positive certainty on this important point, when the slightest cases have proved to be real *(wirklich)* cases of cholera by tracing the presence of comma-bacilli." *British Medical Journal*, Sept. 6, 1884, p. 456.

[55] Reluctance of the English and French to access Koch's theory was surely exacerbated by ethno-nationalist loyalties and potential economic implications, but in fact the doubts did have scientific merit. (See Atalic, "1885 Cholera Controversy"; Ogawa, "Uneasy Bedfellows"; and the literature cited therein.) Reactions in America are particularly informative since there were ethnic sympathies toward all three parties and where there were far fewer economic conflicts of interest. Up-to-date reports were given in general publications such as the *New York Times* and medical periodicals such as *The Weekly Medical Review* (St. Louis Missouri) and *The Medical Age* (Detroit, Michigan).

[56] Klein and Gibbes, *Inquiry*, and the anonymous "Official Refutation."

and Prior in Bonn that they had found in non-Asiatic cholera bacilli that matched Koch's description.[57] Koch examined their cultures and concluded that the bacilli were similar but different. In Detroit, Michigan, physicians concerned that a cholera epidemic could reach America met on June 1, 1885. A paper entitled "The Treatment of Asiatic Cholera" was read. The topic for discussion afterwards was the typical one: What really distinguishes Asiatic cholera from other forms? A recent case of severe cholera, not seemingly part of any epidemic and so presumably not Asiatic, was described. The speaker noted, "Clinically there was no perceptible difference between it and a case of Asiatic cholera."[58] If Koch is correct, he continued, "the presence or absence of the comma bacillus would allay all doubts,"[59] but Koch's claims are still in dispute because of an inability to confirm a perfect correlation. So here again, far from the previous summer's conference in Germany, the initial reaction was the same. To accept Koch's theory, a physician needed first to draw a stronger distinction in his mind between epidemic Asiatic cholera and other kinds of cholera than was provided by the canonical medical taxonomy.

Within a generation, the canonical taxonomy did get redrawn. The benefits of doing so were too good for it not to be. By the early 1890s, reference works were equating "cholera" with "true or genuine Asiatic cholera," identifying its cause as the comma bacillus, and stressing that it is distinct from *cholera morbus* and *cholera infantum*.[60] By the mid 1910s, the cause was not just noted. It was used to define the term: "Cholera—An acute epidemic infectious disease caused by a specific germ, *Spirillum cholerae asiasticae;* it is marked clinically by a profuse watery diarrhea, muscular cramps, vomiting, and collapse. It is also called Asiatic or Indian cholera."[61] The boundaries of the concept were now marked by cause rather than effects. A nominal definition that allowed many general but few universal statements got replaced by a causal,

[57] Burner, "Report."
[58] *The Medical Age*, June, 1885, p. 271, Dr. Shurly speaking.
[59] ibid.
[60] E.g., *Johnson's Universal Cyclopedia*, s.v., "cholera."
[61] Stedman, *Practical Medical Dictionary*, s.v., "cholera."

essential (Aristotelian), formal (Baconian) definition. It became possible to say with complete certainty, without any reservation, that if a person is kept away from *Spirillum cholerae asiasticae* the person positively will not, cannot contract cholera. He may get a bellyache, he may vomit, he may have diarrhea, he may spread his illness to another, he may die of it, but if what he had was not caused by *Spirillum cholerae asiasticae*, then he did not have cholera. Statements about the prevention, diagnosis, and treatment of cholera could now have a certainty they never could have had before, for the statements could be derived from the very definition of the disease. A result of Koch's experimental, inductive inquiry was a new definition of cholera—and thanks in part to it, a dramatic reduction in worldwide cholera mortality.

Electrical Resistance

When, in the late 1780s, Luigi Galvani was making frogs' legs move by touching two metals together, there were very few universal statements about the phenomena that could be made with certainty. In fact, it was difficult to make any unambiguous statement at all. The vocabulary simply was not there yet. Galvani himself knew that the regular electricity that could be made by friction and stored in a Leyden jar could make a frog's leg twitch and so he thought he had found a way to manipulate some "animal electricity" naturally occurring in the frog. Others were not convinced that they were studying animal electricity and coined the term "Galvanism." Alessandro Volta concluded the object of study was not electricity endemic to animals but "contact electricity," something like common electricity but which was generated when certain metals come into contact. What Galvani had seen, Volta claimed, was the result of electricity generated when a copper hook made contact with the iron table. Electricity created at the metals' junction was passing through the frog's muscles. Using what he learned, Volta was able to build a "pile" of dissimilar metals that could generate (some sort of) electricity. He announced his success in 1800.

A scramble ensued to figure out in what ways the "Galvanic" electricity generated by Volta's battery was similar to and different from common (i.e., static) electricity. The investigation, of course, was conducted with the existing tools, conceptual framework and vocabulary, and that made things difficult. An electroscope could measure the electricity in a Leyden jar, but a Leyden jar had one wire sticking out. A battery had two. Distinctions between voltage, potential, current, power, charge, charge density and so on were still being worked out.

In the mid 1820s, Georg S. Ohm joined in. Ohm was an admirer of Francis Bacon,[62] a skilled mathematician, and a careful experimentalist. He began with a project other researchers were already working on—how well different wires conduct Galvanic electricity. Ohm passed electricity through different wires and measured the flow for each. The measuring tool was a compass needle aligned north-south and hung over a wire by a thread attached at the other end to a calibrated knob. The stronger the electric flow (whatever exactly that meant) the further the needle would be turned from its north-south axis. The knob was turned until the needle realigned, and the measurement was read off from marks on the dial. Ohm's first attempt was only modestly successful because his battery would weaken during the experiment. Thomas Seebeck had recently discovered that a particular configuration of metals could generate Galvanic electricity when two points of the apparatus were at different temperatures, and Ohm switched from a battery to one of these thermo-electric generators. That resolved his problem of the dying battery.

With his new apparatus he discovered that needle deflection was proportional to $a/(b+x)$, where a is the electromotive force provided by the thermoelectric generator (which Ohm was told by other researchers would be proportional to temperature difference), b is a constant for the configuration, and x is what Ohm called the "restricted length"; x was larger the longer the wire and it varied by type of material. With his discovery, Ohm was able to explain much about Galvanic circuits that was previously unexplained. It is tempting to tell this story by saying that Ohm here discovered "Ohm's Law," the scientific law that current = voltage / resistance. But that

[62] His annotated copy of a Latin edition of *Novum Organum* is item 60 in the Ohm Collection of the Deutsches Museum Library.

description would be anachronistic—those three terms or even associated concepts did not yet exist. Ohm and others worked out that longer wires "resisted" electric flow more, that wires of one material with the same ratio of length to cross section had the same "resisting" effect, that *b* in the equation was the resistance of the generator and test gear, and that resistance of different materials could be compared. So the denominator in Ohm's equation came to be recognized as total resistance in the circuit. Moreover, that there was a correlation between the compass deflection and the intensity with which the electricity flowed was accepted soon enough, even while the nature of that flow was debated. The third parameter, the nominator *a* in Ohm's equation was more troublesome. Was it a force ("electromotive force"), a charge (something measured by an electroscope), a difference in charges, a potential, a "tension", a "mass of electricity"? How was it related to measurements of regular electricity? Some answers proved inconsistent; some used concepts too poorly defined to be effective. It took a couple decades for conceptions of voltage and current to get all sorted out. And as they did, what Michael Faraday called a "beautiful theory" in 1834 became by 1846 "Ohm's Law." It got elevated in part because twenty years of research had confirmed what Ohm had observed but also because its place in a whole supporting conceptual framework got worked out. In 1843, discussing Ohm's theory, Charles Wheatstone wrote, "It will soon be perceived how the *clear ideas of electro-motive forces and resistances, substituted for the vague notions of intensity and quantity* which have been so long prevalent, enable us to give satisfactory explanations of the most important phenomena, the laws of which have hitherto been involved in obscurity and doubt."[63]

A point of some finality was marked by James Clerk Maxwell's 1873 *Treatise on Electricity and Magnetism*. He wrote, "so many conductors have been tested that our assurance of the truth of Ohm's Law is now very high."[64] But let us not misunderstand the statement. That sentence is the ninth of nine sentences in which Maxwell introduced Ohm's law. (I will give essentialized

[63] Wheatstone, *Bakerian Lecture*, p. 304. My italics.
[64] Maxwell, *Treatise*, p. 296.

quotations, leaving out ellipses and adding italics for emphasis.) The first two sentences: "The relations between Electromotive Force, Current and Resistance were investigated by G. S. Ohm. The result was 'Ohm's Law'" Then the law: "The electromotive force is the product of the strength of the current and the Resistance of the circuit." The fourth sentence is remarkable: "Here a new term is introduced, the Resistance of a conductor, which *is defined to be* the ratio of the electromotive force to the strength of the current." The law that voltage is current divided by resistance is now true *by definition*, for resistance is defined to be the ratio of voltage to current. Maxwell continues: "The introduction of this term would have been of no scientific value unless Ohm had shewn, as he did experimentally, that it corresponds to a real physical quantity." Moreover: "The resistance of a conductor is independent of the current. The resistance is independent of the electrical potential. It depends entirely on the nature of the material, the state of aggregation of its parts, and its temperature." So when Maxwell then wraps up by saying, "so many conductors have been tested that our assurance of the truth of Ohm's Law is now very high," he does not mean that a sufficient number of experiments have provided a sufficiently high correlation. He means many conductors have been found that fit the definition. We no longer ask: "The resistances of all measured devices obey Ohm's Law; does the resistance of all devices do the same?" Rather, we ask, "Is this device a resistor?" If what we are measuring does not obey Ohm's Law, then what we are measuring is not resistance. The inductive quest that began with Georg Ohm seeking how Galvanic force and flow are related has ended in a universal law that is true by the very definition of the terms.

Tides

European languages have long had words for tides, but often the word also meant time (preserved in "winter tide" or "noontide"). For "tides," writers in the sixteenth-century frequently used "flux and reflux of the sea."[65] Mariners were familiar with this flux and reflux and could

[65] Newton used *fluxum et refluxum maris* in chapter titles and wherever else his emphasis was on the general phenomena of the tides; when he referred to a particular increase or decrease in the water level, as a large tide or a

make many general statements about them, especially for the waters they frequently traveled. Various regular patterns had been observed, including ones that involved phases of the moon. But universal, certain, exceptionless statements were ~~had~~ hard to come by. No one knew exactly what caused the tides.

In 1687, Isaac Newton explained that tides were caused by gravitational attraction of the moon and sun. He could explain why there were high tides approximately, but not exactly, twice a day. He could calculate the precise frequency. He could explain the difference between neap and spring tides and the seasonality of tides. His theory promised new precision and accuracy in predicting the tides.

For a long time, however, the promise went unfulfilled, for several reasons. First, to model tidal behavior for an actual point on a particular, real, limited body of water required more advanced understanding of fluid dynamics and more advanced mathematical models than Newton provided in the *Principia*. Laplace provided the needed model, the *Laplace Tidal Equations*, in 1776. Then, solving these equations for particular aquatic bodies remained a challenge. In 1845, George Biddell Airy published a solution for tides in canals. Lord Kelvin (William Thomson) extended Airy's approach and worked out a simplified solution that involved planar instead of spherical coordinates. These and other solutions of the Laplace Tidal Equations treat the result as a superposition of waves of varying frequency, phase, and amplitude. The frequencies and phases are mathematically derived from the celestial mechanics. But for practical predictions of high and low water times and heights the amplitudes are derived empirically. This combination of celestial mathematics and empirical curve-fitting has now led to highly valuable predictions of the rise and fall of the sea.

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small tide, he used *aestus.* In 1701 the *Académie Royale des Sciences* issued a *Mémoire de la Manière d'observer dans les ports le Flux et le Reflux de la Mar* and in 1738 offered prizes for the best essays on *Le flux et le réflux de la mar.* Pond's 1809 translation of Laplace's *System of the World* entitled a chapter "On the ebbing and flowing of the sea" in the table of contents and "Of the Flux and Reflux of the Ocean" on the chapter's title page and then described the chapter's topic as the "regular and periodic oscillations, which are know by the name of tides."

The predictions are highly valuable, but not always highly accurate. For another problem has presented itself. Many non-celestial factors can cause bodies of water to rise and fall. There are daily temperature variations, barometric cycles, seasonal rain patterns, seiches, even man-made causes like ships' passages or industrial releases of water. On August 25, 1882, Kelvin, who had by this time done so much to promote the harmonic analysis of tides, began an evening lecture by saying, "The subject on which I have to speak this evening is the tides, and at the outset I feel in a curiously difficult position. If I were asked to tell what I mean by the Tides I should feel it exceedingly difficult to answer the question. The tides have something to do with motion of the sea. Rise and fall of the sea is sometimes called a tide; but . . . ."[66] He proceeded to cite many problems with this definition—with what we may call a nominal definition. Kelvin was here reflecting on the development of tidal science in the two hundred years since Newton proposed what causes the sea to rise and fall and Newton's successors worked out the physics, mathematics, and data-gathering techniques to make it possible to predict such risings and fallings. And Kelvin has to acknowledge that all that science left him unable to tell the sea-captain for sure where the water level will be at a certain time, because all that tidal science has left temperature variations, barometric cycles, and the coming and going of ships out of the equations. Kelvin returns to his theme, "What are the Tides?" and answers, "I will make a short cut, and assuming the cause without proving it, define the thing by the cause. I shall therefore define tides thus: Tides are motions of water on the earth, due to the attractions of the sun and of the moon."[67]

All that work, from the ancients to Pliny to circumnavigators to Galileo to Newton, all that gathering data, comparing and contrasting, sorting out one thing from another, considering the moon and the sun and oceans and the seas and the canals, developing harmonic analyses and inventing machines that solve equations and comparing predictions to reports—all that to figure

[66] Kelvin, *The Tides*.
[67] ibid.

out what causes the tides. And now Kelvin announces the result: A tide is, *by definition*, caused by attractions of the sun and of the moon. The sea may flux; the sea may reflux; but if some particular fluxing and refluxing has some other cause, it is by definition not a tide. Statements about tides need no longer be just generalizations. They could be certain and universal. For they could be deduced from the very definition of a tide.

Moreover, once a tide is defined as Kelvin defined it, it becomes clear that there are not just tides in water, but tides in the atmosphere, tides in the very shape of the earth. Boaters in Green Bay, Wisconsin may talk of the tides there, just as the Europeans who first arrived there reported. But the scientists have done the calculations and are certain: What the boaters and the explorers have reported are not really tides. Yet a small part of that small daily change in the shape of Sahara sand dunes—those are tides.

Ampliation

The central premise of Socratic/Aristotelian/Baconian/Whewellian induction is that in human cognition, ampliation takes place, fundamentally, at the conceptual, not the propositional level. Propositional ampliation is derivative. An aspect of this premise is that formation of concepts and definitions is a normative process. We need standards by which to judge whether we are doing it correctly or not. Aristotle proposed some standards in the *Topics*. Bacon gave twenty-seven in book 2 of the *Novum Organum*. Whewell had his list.

A second premise is that definitions mature—or, because that makes it sound like the concepts just develop on their own—we improve our definitions. We begin with generalizations. Apples are generally red, bananas yellow, swans white. People who vomit, have gurgling in the belly, and have diarrhea are sick with cholera. The rising and falling of water in a sea is a tide. We then refine our definitions. We compare and contrast choleric patients having high fevers and those without, those who remain ill for a long time and those who die, those who ate a heavy meal and those who drank from the city well. We time the tides. We distinguish different kinds of cholera. We distinguish tides by their amplitude and frequency. We seek what gives things the properties they have. We seek what makes something the kind of thing it is. We decide that what makes cholera cholera is the action of a certain bacteria. What makes a tide a tide is the attraction of celestial bodies. Resistance is the ratio of voltage to current. And then we make an important decision. We decide that the cause will now be definitive. We will bound our concept not by observed effects but by causes. Once we do so, we can make scientific statements that are not just general, but universal.

Speaking from a Baconian perspective: When deciding how to treat a case of severe vomiting and diarrhea, we do not now ask, "Will the treatment that was effective before be effective again? Have enough experiments been run to establish a high correlation?" Instead we ask, "Is this a case of cholera?" Is so, we know what treatment will be effective, viz., destroy or evacuate the *Spirillum cholerae asiasticae*. In Kantian language: What began as synthetic knowledge has become analytic. In the language of Socratic induction: What has been shown true by a comprehensive process of comparing and contrasting is true by induction and thus true by definition. In colloquial terms: Good things—such as universal statements—come to those who define by causes.

The pattern I have described may not be the only way certain and universal statements can be formed, and the process may not be as binary as my contrast between nominal and essential suggests. But the pattern exemplified above is very common in science. It is the way we have formed countless universal scientific laws, statements that seem to be both true by induction and true by definition. The understudied history of Socratic induction offers a wealth of information about how there can be such statements and about how we may go about forming more of them—and maybe even about that stubborn creature we professors keep calling the "problem of induction."

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