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THE RELATIONSHIP BETWEEN SCIENTIFIC INDUCTION AND MATHEMATICAL INDUCTION

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Abstract: It is often maintained in conversation and print alike that scientific induction, a method of arriving at scientific generalities which may eventually acquire the status of „natural laws", is completely distinct from the method of proof commonly used in mathematics known as „mathematical induction". We examine the literature to demonstrate this unfortunate *lack* of conflation and explain the embarrassingly simple way in which the two methods ultimately converge.

Keywords: scientific induction, mathematical induction, methods of proof.

I. THE TWO METHODS

Both the literature of scientific induction and the literature on mathematical induction go way back. The scientific method (in its systematic form) goes back to Roger Bacon (*c.* 1220-1292 C.E.) and was further systematized by the polymath John Stuart Mill (1806-1873 C.E.). A good part of what is meant by scientific induction is going from the particular to the general via observation – the observations can be in the field, in the laboratory, or arrived at via deductive or mathematical methods from such observations (which is, more or less, how astronomy developed). Of course, the inductive method so described cannot produce certainty, because the next observation might contradict the generality arrived at from prior observations. This, of course, need not prove fatal to a scientific theory; it is not quite true that Einstein „disproved" Newton; rather, he qualified Newton's results by restricting the range to which they applied, and showed how a more general theory still could account for Newton's results *as a special case*. The more complicated the

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mathematics involved in deducing generalities from observations, the less certain one can even be of what has already been found even in this limited sense. This explains, to a large extent, the hotly contested areas of science such as particle physics and cosmology, where differences of opinion are as sharp, perhaps, as in many of the traditionally „softer" disciplines where mathematics is often not of much assistance. This extended prolegomena explains, in large part, the resistance by almost everyone to see the obvious connection between what, on the one hand, is merely the best we can presently do to sort natural phenomena out and, on the other hand, an undisputed method of proof taught in secondary schools and beyond.

In 1908, Professor A. Voss of Munich produced a 98-page monograph on the nature of mathematics, reviewed in Cajori (1909), the most extensive treatment of the development of mathematical induction I was able to locate. Cajori's reading of Voss is that „[o]n the authority of Moritz Cantor, Voss ascribes the introduction of the inference by mathematical induction to Pascal (before 1654) and its independent re-introduction to Jacob Bernoulli (1680)." Cajori goes on to write – and it is not clear[1] whether the sentence is also an ascription to Voss or not – „This important process is called by Max Simon the «Bernoullian» or the «Kästnerian» inference. Charles S. Peirce ascribes it to Fermat and calls it the «Fermatian» inference." (Cajori 1909, 407)

Cajori continues his review of Voss (now speaking clearly in his own voice) by discounting any claim of Kästner, because he (and others who are not mentioned by name) wrote so much later – „the second half of the eighteenth century" (Cajori 1909, *loc. cit.*) He says likewise that priority cannot be given to Bernoulli, coming as he did after Pascal, of whom he writes, „This process [*viz.*, the process described in Pascal's complete works, Vol. 3, p. 248, published in 1866 in Paris] is *precisely* what is now designated by mathematical induction." [italics added] (Cajori, 1909, *loc. cit.*). While he does concede that Fermat came first – as early, says Cajori, as 1636 or 1637 – he denies him the status of discoverer for four reasons. The first is that „it is not mathematical induction in its purity." (Cajori, 1909, 408) To me, this hardly matters; no idea springs forth from anywhere in its „purity". This is not how either science or mathematics, or for that matter any other discipline, develops. In this regard, I quote Ernest (1982, 120): „Like many of the concepts of mathematics, proof by mathematical induction is not the invention [I would say „discovery"] of a particular individual at a fixed date. Rather, the method is implicit in the work of mathematicians and slowly emerges until at last a satisfactory, fully explicit formulation is given." Second, and more compelling, the selfsame impure method was used by Campanus in 1260 „in his edition of

[1] The reason it is not clear who is speaking is that the subsequent claims are footnoted, and whether the references were taken from Voss or are, in fact, due to Cajori, himself an expert on the subject, is unclear.

Euclid". (Cajori, 1909, *loc. cit.*) Ernest (1982, *loc. cit.*) goes a good deal further, by showing that „[m]athematical induction is implicit in some of Euclid's proofs, for example, in the proof that there exist infinitely many primes." (This is Proposition 20 in Book IX of Euclid.) The third reason strikes me as extremely weak. Cajori writes, „Even if this term [mathematical induction] be used in a sense broad enough to include the mode of inference described by Fermat ... his Relation did not become the general property of mathematicians until 1879. [Cajori previously tells us, on p 407, that the document was discovered then among the papers of Huygens in that year.] Fermat's leaning was toward keeping his methods secret. He influenced mathematicians in the theory of numbers by the *results* he reached, rather than by the methods he made known." (Cajori, 1909, *loc. cit.*) Different people influence a discipline in different ways, and to apply today's publication standards to a figure who lived so many centuries ago and on that account deny him any part of the credit due him is at least ahistorical and perhaps simply absurd.[2] The fourth reason is by far more compelling – and reminiscent of the later observation of Ernest previously quoted – *viz.*, that „[t]he recurrent modes of inference of the Hindus and the Greeks are more nearly the modern process of mathematical induction than is the mode of inference used by Fermat," although also not „free from entanglement with other processes." (both from Cajori 1909, 409). To this effect, he cites work from Bhaskara, Theon of Symrna, and Proclus, by name. His conclusion is that the modern method ought to be ascribed as originated by Pascal.

Shortly after publication of his review, he was contacted by Dr. G. Vacca, who had also worked on the history of this method of proof, whose work shows that it was discovered earlier by the Italian, Franciscus Maurolycus (1494-1575 C.E.). Vacca, and probably Cajori, find it strange that Pascal could be unacquainted with Maurolycus' method and made no mention of it in his works. Regardless, we now know that the modern method in its purity ought to be attributed to Maurolycus sometime in the sixteenth century. (Vacca, with an introductory note (and perhaps editing; again this cannot be told) by Cajori, 1909: 70-73).[3] Incidentally, the modern method has two variants, known as „weak" and „strong" mathematical induction (Ernest, 1982: 123-124), and it is rather easy to prove that the „stronger" method is not really stronger (although it appears that way at first blush), but is rather logi-

[2] See the following note regarding Pascal's use of a pseudonym as some support for this.

[3] Moreover, the claim is incorrect, although this is no fault of either Vacca or Cajori. Bussey (1917, 203) informs us that Pascal expressly attributes the method to Maurolycus; however, he does so in a letter written under a pseudonym. Regardless, the point made above that applying today's publication standards to centuries ago is at least ahistorical and perhaps absurd stands. Cantor, too, from whom Cajori obtained his information, corrected his claim that Pascal originated the method in favor of Maurolycus, also subsequent to a communication from Vacca. (Bussey, 1917, 200).

cally equivalent to the „weaker" method.[4] These can be further generalized into the „well-ordering principle" as discussed in Ernest (1982, 124); there are also further induction principles such as the principle of „transfinite induction", but these do not concern us here.

II. THE NAME „INDUCTION" AND THE CONTRAST SEEN

Cajori (1918) writes that „no one, to [his] knowledge has before this time traced historically the origin of the *name* 'mathematical induction'." It seems clear from this source and others that the earliest occurrence of the name „mathematical induction" is due to the famous logician Augustus De Morgan in an article in the *Penny Cyclopedia* published in London in 1838.

Cajori (1918) goes on at length contrasting scientific induction and mathematical induction, the former as a method of discovery and the latter as a method of proof. This contrast is clearer is other sources, which we now cite. Bussey (1917) writes, „One method of clinching an argument by ordinary induction is what has been called *mathematical induction*." He goes on to add, incorrectly as we will show at this article's close, „It [meaning *mathematical induction*] is not a method of discovery but a method of proving what has *already* been discovered." [emphasis added]. In between these two sentences, he adds the crucial sentence, „A more significant name and one that is being used more and more is *complete induction*."[5] Young (1908, 145) writes „He [meaning the pupil] will frequently find that the course of development is from the particular to the general, and in such cases he will often find em-

[4] Cohen (1991, 315) claims that this is not so easy to prove and is a source of „scepticism, confusion, or both", and so devises multiple conditionals and a new logic to simplify matters! However, this hardly simplifies matters; it adds layers of unnecessary complexity. Cohen points to four sources (he lists three, but his second bundles two concerns together) for the scepticism and confusion, the „most serious" of which is „the 'negative paradox' property of the horseshoe of material implication (that a false antecedent makes the entirety 'vacuously' true) ... in order to circumvent the need for a base case." (Cohen 1991, 316). But there is no good reason to formulate „strong induction" without a base case at all. (And, moreover, the negative paradox referred to is paradoxical only at first glance – to develop a new logic and three separate implicative connectives in the formulation of, and in order to formulate, a *single* schema is surely overkill.) Ernest (1982) also presents „strong induction" without an *explicit* base case, but notes on p. 124 that „[t]he fact that $P(1)$ must hold is concealed in the statement '$P(i)$ for $I < 1$ imply $P(1)$'." Most textbooks, however, not written for those specializing in mathematics do include an explicit base case. This rids the equivalent methods of „weak induction" and „strong induction" for any need for reliance on vacuous truth, even if one *does* regard that as paradoxical. In fairness to Cohen, none of the implicative connectives or the logic devised using them originate with him or were originally devised for this purpose; that is merely *his* purpose for the lot of them in *this* article.

[5] The name „complete induction" comes from the German, and is by no means unique in the writing of Bussey; it is his placement of that sentence between the other two that I find telling, but not as telling as some other sources soon to be quoted. Likewise the name „incomplete induction" is treated at length by Cajori (1918), but the contrast followed by what we term an unfortunate lack of conflation and ultimately convergence is clearer elsewhere, as we will explain.

ployed a method of reasoning called *mathematical induction*, which shares with non-mathematical induction the peculiarity of generalizing from particular instances, but which nevertheless, like other mathematics, produces that unhesitating confidence that absolute accuracy of the result which is not felt as to the results of non-mathematical reasoning."

III. THE LACK OF CONFLATION OR THE MISSING LINK

Carl Boyer, the eminent historian of mathematics, writes in his *History*, „In fact, mathematical induction, or reasoning by recurrence [the name Pascal used], sometimes is referred to as 'Fermatian induction', to distinguish it from scientific or 'Baconian' induction" (388). This is the common view and the received view. But he adds, in parentheses, on the same page, „(Today the former is sometimes known as „complete induction", the latter as „incomplete induction".)" The matter comes to a head in the work of Cohen and Nagel (1934, 147) who write that, „The reader has doubtless been ensnared by a word. There is indeed a method of *mathematical induction*, but the name is unfortunate, since it suggests some kinship with the methods of experimentation and verification of hypotheses employed in the natural sciences. But there is no such kinship and mathematical induction is a purely demonstrative method." Cajori (1918, 200) tells us that mathematical induction was given the name „demonstrative induction" by George Peacock, but that it „has become obsolete".

The clincher comes in the same work, where Cohen and Nagel (1934, 275) write:

> „Thus it is clear that we must get to know the primary premises by induction; for the method by which even sense-perception implants the universal is inductive." [This he gets from Aristotle.] „This process is an important stage in our getting knowledge. Induction, so understood, has been called by W. E. Johnson *intuitive induction*. Nevertheless, this process cannot be called an inference by any stretch of the term. It is not a *type of argument* analyzable into a premise and conclusion. It is a perception of relations and not subject to any rules of validity, and represents the gropings and tentative guessings [today one would probably say 'conjectures'] of a mind aiming at knowledge." A few sentences later they write, „Aristotle, and others after him, have employed 'induction' in another sense. Suppose we wish to establish that *All Presidents of the United States have been Protestants*.[6] We may offer as evidence the proposition *Washington, Adams, Jefferson, and so on were Protestants* and *Washington, Adams, and Jefferson, and so on were Presidents of the United States*. The evidence is not conclusive unless we know the

[6] Recall that the book was published many years before John F. Kennedy became President of the United States.

> converse of the second proposition is also true: unless we know, that is, that *All the Presidents of the United States are Washington, Adams, Jefferson, and so on*. In that case, the arguments may be presented as follows: *Washington, and so on, were Protestants; all the Presidents of the United States are Washington, and so on; therefore all the Presidents of the United States have been Protestants*."

Induction, in this sense, means establishing a universal proposition by exhaustive enumeration of *all* the instances which are subsumable under it. It has been called *perfect* or *complete induction*. Perfect induction is not antithetical to deduction. As we have just seen, *perfect induction is an example of a deductive argument*. .... It is evident that a perfect induction is possible only when all the instances of the universal proposition are already known to conform to it. But if general propositions could be employed only if they were the conclusions of a perfect induction, they would be utterly worthless for inferring anything about *unexamined instances*.

Now, while none of this is outright incorrect, it all seems to miss the point. Scientific induction is, indeed, a method of conjecture, as much so in mathematics as in the empirical sciences. Nothing is proven by examining less than the totality of instances. But the oft-used (at least formerly) names „incomplete" and „complete" induction indicate full well the unacknowledged, but almost obvious connection between the two methods, by which I do *not* mean that conjectures arrived at by scientific induction can then be proven (if true) by mathematical induction (at times). (This has been observed repeatedly and is by no means a new observation.) I rather mean that the latter method is the *completion* of the former method and contrary to Cohen and Nagel is not at all sterile, *in the case of infinite sets*, where, of course, direct exhaustive examination or enumeration (Baconian induction is also called „enumerative induction") is completely impossible. Indeed, Cohen and Nagel's complaint is reminiscent of the complaint about deduction more generally. If the conclusion is already implicit in the premises, what can possibly be gained by what is simply a mere exercise? Well, it may be an exercise, but as human beings are not „logically omniscient" (the phrase is due to Hintikka), that is that they are not by any means aware of all the logical consequences of their beliefs, mathematics is a highly creative discipline, because figuring out just how something follows from something in which it is *admittedly* implicit is very often no easy matter.

This is perhaps why Poincairé called mathematical induction (then termed reasoning by recurrence) „mathematical reasoning *par excellence*". (Bell 1920, 414) Young (1908, 146) tells us expressly that Poincairé claimed of mathematical knowledge that „mathematical induction ... alone can teach us anything new".

In summary, the most common method of exhaustive enumeration for infinite sets is the completion of the very method by which conjectures about such sets are arrived at as is indicated by their early names. The two methods are, of course, different, but far from being unrelated or only marginally related, they converge in a particular and fruitful way and while they ought not to be confused, there is an element of conflation present in the two methods, which has somehow gone unnoticed. The purpose of this article is to bring this connection to the fore.

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