

11 Beware of Syllogism: Statistical Reasoning and Conjecturing According to Peirce

I. PROBABLE DEDUCTION

Peirce wrote extensively on deduction, induction, and hypothesis beginning with the Harvard Lectures of 1865 and Lowell Lectures of 1866. The ideas that he examined in those early discussions were re-worked over nearly two decades until the comprehensive statement of his view contained in "A Theory of Probable Inference" of 1883 that was included in the *Studies in Logic, by the Members of the Johns Hopkins University* and is reprinted in W 4, 408–450. This remarkable paper developed a version of the Neyman–Pearson account of confidence interval estimation that incorporated the main elements of the rationale offered for its adoption in the early 1930s and presented it as an account of inductive inference.

In his retrospective reflection on the question of induction in 1902 (CP 2.102), Peirce revealed satisfaction with the views on induction advanced in 1883 and this attitude is confirmed in other remarks from that period. However, Peirce did express dissatisfaction concerning his notion of "Hypothetic Inference." Although Peirce called it Hypothetic Inference or Hypothesis from 1865 to 1883 and later, in 1902, Peirce replaced the term "Hypothesis" with "Abduction."

In what I said about "Hypothetic Inference" I was an explorer upon untrodden ground. I committed, though I half corrected, a slight positive error, which is easily set right without essentially altering my position. But my capital error was a negative one, in not perceiving that, according to my own principles, the reasoning with which I was there dealing could not be the reasoning by which we are led to adopt a hypothesis, although I all but stated as much. But I was too much taken up in considering syllogistic forms and the doctrine logical extension and comprehension, both of which I made more

fundamental than they really are. As long as I held that opinion, my conceptions of Abduction necessarily confused two different kinds of reasoning. (CP 2.102)

Peirce's description of the situation seems reasonably accurate, as I hope to show.

Peirce's writings on logic in the period from 1865 to 1870 propose an account of a formal "unpsychologicistic logic" that, unlike Frege's later discussion, applies to inductive and hypothetic inferences as well as to deductive inferences.¹ The account of deduction, induction, and hypothesis Peirce offered in these three series of lectures starts with Aristotle's theory of the categorical syllogism as improved by Peirce.

Following Aristotle, Peirce understood induction to exhibit the formal structure of a transposition of one of the premises and conclusion of a valid categorical syllogism (the "explaining syllogism"). In the case considered by Aristotle where the explaining syllogism is figure 1, mood AAA in Barbara, the conclusion of the induction is the major premiss (the "rule") of the "explaining" syllogism and the conclusion (the "result") of that syllogism replaces the major premiss. The minor premiss (the "case") of the explaining syllogism remains a premiss of the induction. Peirce claimed that this form of argument characterizes induction by "simple enumeration." A favorite example of Peirce's is the inference from information that a selection of cloven-hoofed animals that turn out to be neat, swine, sheep, and deer are also herbivorous to the conclusion that all cloven-hoofed animals are herbivorous. The explaining syllogism is: All cloven-hoofed animals are herbivorous (Rule); a sample of neat, swine, sheep, and deer is selected from the cloven-hoofed. (Case). Hence, the neat, swine, sheep, and deer selected are herbivorous. (Result).

Peirce clearly understood, of course, that the minor premiss of a syllogism in Barbara could be a universal affirmative (A) proposition such as "All neat, swine, sheep, and deer are cloven-hoofed" rather than singular statements such as "this neat, this sheep, etc., are cloven-hoofed." But the minor premiss of the "explaining syllogism" is retained as a premiss of the induction when Rule and Result of the syllogism are transposed to yield the form of an inductive argument. And this minor premiss or Case is intended to convey the information that a sample S of individuals have been selected from the population characterized by the middle term M.

According to Peirce's reading, however, the Case or minor premise of the explaining syllogism in Barbara reports more than that each of a given set of individuals is a member of the class characterized by the middle term *M*. A sample is taken from the class represented by the middle term (in our example, the cloven-hoofed). The report describes the specimens taken from the population characterized by the middle term by the minor term. In our example, it is reported that the specimens selected from the cloven-hoofed are particular specimens of neat, swine, sheep, and deer.

Moreover, in the syllogisms in Barbara eligible to be explaining syllogisms for inductions, the method of selection from the middle term *M* must be such that the specimens are consciously selected only on the basis of whether they exhibit the characteristics represented by the middle term (*W* 1, 264–5). In the example, the neat, the swine, the sheep, and the deer are not selected because they belong to one of these four species but solely on the basis of whether they are cloven-hoofed. This is the way Peirce characterized the method of selection in 1865. He used essentially the same characterization in the Lowell Lectures of 1866 but called the method of selection "random." In the 1869 "Grounds of Validity of the Laws of Logic," Peirce was assuming that the long-run relative frequency or statistical probability with which a member of a set will be selected given that it is selected at random is the same for all members of the set (*W* 2, 268).²

Peirce explicitly acknowledged that a psychological or epistemic constraint should be satisfied by the method of sampling.

When we say that neat swine sheep and deer are a sample taken at random of cloven-hoofed animals, we do not mean to say that the choice depended upon no other condition than that all should be cloven-hoofed; we can not know *that* and the presumption is the other way since there is a certain limitation of that class indicated by our having taken so few instances. What we mean, then, in saying that neat swine sheep and deer are taken at random from among the cloven-hoofed animals, is that being cloven-hoofed was the only condition that consciously guided us in the selection of these animals. (*W* 1, 433).

Peirce admitted throughout that he could not give a purely formal and, hence, logical (in the sense of unpsychological logic) characterization of the strength of inductive arguments. In this respect, they differed fundamentally from deductive arguments. There is no

notion of inductive consequence to correspond to deductive consequence that belongs properly to formal logic. But inferences, can be classified as inductive on the basis of formal considerations alone. In the late 1860s, his idea was to appeal to the transpositions of premises and conclusions of categorical syllogisms to provide the characterization.³ This formal classification constituted the basis for Peirce's claim that induction and hypothesis could be objects of study under the rubric of an unpsychologicistic formal logic.

Peirce abhorred vacuums in logical space. There is obviously another kind of transposition of the explaining syllogism in Barbara where the minor premise or case and conclusion or result of that syllogism replace each other. Peirce proposed to think of the resulting argument as instantiating hypothetical inference.

Hypothetic inferences, like inductive inferences, can be classified by purely formal and, hence "unpsychological" criteria. But the strength of such inferences, like the strength of inductive inferences, takes into account considerations that are not purely formal.

One of the problems internal to Peirce's approach is that at least from the time of the Lowell Lectures of 1866, he wished to regard inference from sample frequencies to population frequencies or, more generally, to statistical hypotheses as paradigmatic of inductive inference. The difficulty is that statistical claims such as "Most cloven-hoofed animals are herbivorous" or "90% of cloven-hoofed animals are herbivorous" are not categorical propositions and are difficult to construe as categorical propositions for the purpose of integration into syllogistic argument. But if they could not be so integrated, the forms of inductive inferences whose conclusions are statistical claims could not be obtained by transposing the major premises and conclusions of *categorical* syllogisms.

In 1883, Peirce clearly and explicitly recognized that statistical claims that he took often to be major premises of the explaining syllogisms for induction in the earlier papers are not categorical propositions. He emphasized that the analogy between syllogisms of the form "All *M* are *P*, *S* is *M*, therefore *S* is *P*" and probable deductions of the form "The proportion ρ of the *M*s are *P*s; *S* is an *M*; therefore it follows, with probability ρ , that *S* is *P*" and probable deduction is certainly genuine and important, there are four significant differences between these modes of inference (W 4, 409–12)⁴:

- (1) Probable deduction is related to syllogism as the quantitative branch is to the qualitative branch.
- (2) All that is requisite for ordinary syllogism is that the three terms of the syllogism stand in some transitive relation such as inclusion, better than, etc. Probable deduction presupposes quantitative ratios. "For that there must be counting and consequently units must exist, preserving their identity and variously grouped together."
- (3) In syllogism, the conclusion follows from the premises according to a formal relation of logical consequence. In probable deduction where the rule or surrogate major premise asserts that the proportion ρ of the *Ms* are *Ps*, the information that *S* is an *M* does not render the conclusion that *S* is *P* even probable. It is necessary that *S* be selected at random from the *Ms*. According to Peirce, the requirement of randomness takes into account various subjective circumstances such as the manner in which the premise has been obtained, the absence of countervailing considerations; "in short, good faith and honesty are essential to good logic in probable reasoning." "In choosing the instance *S*, the general intention should be to select an *M*, but beyond that there should be no preference; and the act of choice should be such that if it were repeated many times with the same intention, the result would be that among the totality of selections the different sorts of *Ms* would occur with the same relative frequencies as in experiences in which volition does not intermeddle at all." In other words, random selection is (a) selection without *deliberate* or conscious bias and (b) selection from the *Ms* that yields *Ps* with probability (according to the frequency interpretation favored by Peirce) equal to the proportion ρ in the *Ms*. Random selection, however, is not enough. "A card being drawn at random from a picquet pack, the chance is one-eighth that it is an ace, if we have no other knowledge of it. But after we have looked at the card, we can no longer reason that way."
- (4) The conclusion of a syllogism is a necessary consequence. The conclusion of a probable deduction is only probable. That is to say, the rule or major premise of syllogism is such that the inference from Case (minor premise) to Result

(conclusion) is truth-preserving in all applications. In probable deduction, the inference preserves truth with a relative frequency ρ . Peirce then asserts it is "useful" that we should have a stronger feeling of confidence the higher the value of ρ . Although Peirce would never call this degree of confidence a subjective or belief probability, it is clear in all his writing that it has all the earmarks of what many contemporaries would call a subjective probability including the disposition to take risks. For this reason, I am inclined to reconstrue Peirce's view as one that admits that inquirers may assign subjective degrees of probabilistic belief to hypotheses provided those degrees of belief can be grounded or justified by knowledge of objective, statistical, or frequency probability. His objection is to taking numerically determinate judgments of subjective probability seriously in scientific inquiry when they lack such grounding.

Peirce extended his account of probable deduction to inferences from samples of n *Ms* to the proportions of *Ps* among the n *Ms*. He first invoked the binomial distribution to determine the probability (long-run relative frequency) of n -fold samples exhibiting any given relative frequency of *Ps* among n *Ms*.

The Weak Law of Large Numbers implies that as n increases, the probability of obtaining a relative frequency whose absolute difference from ρ is less than some specific value d converges to 1. Peirce then used the normal approximation of the binomial distribution (supported by the Central Limit Theorem) to sustain the following argument he called "Statistical Deduction": The proportion r of the *Ms* are *Ps* (Rule). S' , S'' , S''' , etc. are a numerous set, taken at random from among the *Ms*. Hence, probably and approximately, the proportion r of the *Ss* are *Ps*.

In keeping with his account of probable deduction, the "subjective" condition that the sample is taken at random should not be part of the form of this argument. Peirce's concern to distill out an unpsychologistic component of the reasoning involved precluded this. Similarly "probably" qualifies the "mode" of the conclusion and is not part of its content. "Approximately" here is intended to suggest that the conclusion is not that the relative frequency is exactly r but that it falls in some more or less vaguely specified interval around r . Peirce used the normal approximation to the binomial

distribution to illustrate how bounds can be roughly specified for the interval depending on the sample size n .

The Harvard Lectures of 1865 do not explicitly mention direct inference from statistical hypotheses to the outcome of sampling exemplified by probable and by statistical deduction or the inverse inductive inference from the outcome of sampling to a statistical hypotheses. The Lowell Lectures introduce induction (in Lecture III) as inference of just this kind. However, in discussion of induction and hypothesis as transpositions of Premise and Conclusion of Syllogisms there is no discussion of how induction to statistical hypothesis is to be represented as such a transposition. Yet Peirce did recognize the main points of difference between syllogism and statistical deduction mentioned above in the 1866 Lectures. Apparently Peirce was thinking in 1866 of extending his 1865 account of induction (and hypothesis) to statistical cases but did not have answers of the sort he offers later on concerning the relations between propositions of the form " $r\%$ of M s are P s" and "All M s are P s" and gradually thought these matters through in the next decade.⁵ Thus, the lectures and papers starting in 1866 seem to be attempts to adjust Peirce's engagement with the idea that induction and hypothesis are, formally speaking, transpositions of premises and conclusions of categorical syllogisms to transpositions of probable and statistical deductions. The 1878 and 1883 papers represent the culmination of this effort.⁶

2. TRANSPOSING PREMISES AND CONCLUSIONS OF EXPLAINING SYLLOGISMS AND APOGOGIC INVERSION

In the *Prior Analytics*, Aristotle characterized induction as the product of transposing the major premise and conclusion of a categorical syllogism in Barbara. The minor premise is retained and is spelled out as asserting that the individuals covered by the minor term S are members of the set characterized by the middle term M . But Aristotle also insisted on premising the converse of the minor premise, positing that the S s are the only M s. Like most commentators, Peirce took this to mean that Aristotle's induction is induction by complete enumeration and is thus, strictly speaking, a species of necessary or deductive inference (W 1, 263).

Peirce rejected this attempt to reduce induction to deduction. Neither induction nor hypothesis is reducible to necessary inference.

One could not come to know that the sample exhausts the population without some sort of induction. The information conveyed by the sample of M s observed – to wit, the S s – cannot warrant such a judgment.

This much Peirce explicitly stated in 1865. Yet Peirce did construe inductions to be transpositions of the major premise and conclusion of a categorical syllogism. In 1883, he explicitly abandoned the notion that an inductive inference is a transposition of the major premise and conclusion of a categorical syllogism. It is rather the transposition of the major premise or Rule of a statistical deduction and the conclusion or Result.

The statistical deduction has the following form: Statistical Deduction

Rule: The proportion r of the M 's are P 's.

Case: s_1, s_2, \dots, s_n belong to a numerous set S of objects taken at random from among the M 's.

Result: The proportion r of the S 's are P 's (probably and approximately).

Induction is obtained by transposing the Rule and Result.

Induction

Result: The proportion r of the S 's are P 's.

Case: s_1, s_2, \dots, s_n belong to a numerous set S of objects taken at random from among the M 's.

Rule: The proportion r of the M 's are P 's (probably and approximately).

Peirce justified the transposition that yields Induction from Statistical Deduction by arguing that the principle of statistical deduction is that two proportions, that of the P s among the M s and that of the P s among the S s, are approximately equal. That is to say, as long as the methods of sampling and the available evidence warrant the applicability of the calculus of probability, the weak law of large numbers and the central limit theorem insure that this probable and approximate equality holds. Peirce then wrote:

If then, this principle [of statistical induction] justifies our inferring the value of the second proportion from the known value of the first, it equally justifies our inferring the first from that of the second, if the first is unknown but the second has been observed (W 4, 416)

The phrase “equally justifies” calls for closer scrutiny, as Peirce well understood. In the passage cited from 1883, he claimed that

he could specify conditions under which this claim could be made good. In the Harvard Lectures of 1865 and Lowell Lectures of 1866, he adopted a different rationale for claiming that there is equal justification for inferring population frequency from sample frequency as there is for inferring sample frequency from population frequency. He then became silent on the issue until 1878 when he came out with the new rationalization – one he expressed with great clarity in 1883. But in spite of an important change in his views of induction between the late 1860s and the late 1870s, throughout the entire period Peirce thought he could achieve legitimately what Aristotle attempted to achieve illegitimately by the illicit conversion of the minor premise of a categorical syllogism.

I believe that Peirce already endorsed many elements of his view in 1878 and 1883 in Lecture IV of the 1866 Lowell Lectures when criticizing J. S. Mill's contention that inductive inference is "grounded" on a Principle of the Uniformity of Nature (W 1, 420–1). Construing him this way provides a way to understand the argument of the Lowell Lectures even though those lectures often fail to be explicit about the various transitions in the argument.⁷

By using contraposition and conversion, one can transform valid first figure syllogisms into valid second and third figure syllogisms. To obtain the second figure the negation of the Case of a syllogism in Barbara becomes the conclusion and the negation of the Result a premise. To obtain a third figure, the negation of the Rule becomes the conclusion and the negation of the Result a premise. Peirce seemed to have been interested in these "apogogic inversions" because they look similar in form to hypothetic and inductive inference. But if the syllogisms are proper categorical syllogisms this is not strictly speaking so. In 1883, Peirce suggested that if the rule says that the proportion of *Ps* among the *Ms* is ρ , the negation of the rule is that the set of *Ps* among the *Ms* is one of the real values between 0 and 1 other than ρ . Using this and relying upon the weak law of large numbers, Peirce showed that the apogogic inversion of a statistical deduction is an induction. However, the statistical deduction of which it is an apogogic inversion is not the "explaining" statistical deduction whose rule and result are transposed to obtain the induction. And in the course of making the case for this view, Peirce wrote that it is necessary if an induction is to have any validity at all that the explanatory syllogism should be a valid statistical deduction.

"Its conclusion must not merely follow from the premises but follow from them upon the principle of probability. The inversion of *ordinary* syllogism does not give rise to an induction or hypothesis." That is because the inversion of an ordinary syllogism relies on an illegitimate conversion (W 4, 424–7).

Peirce expressed a similar view in "Deduction, Induction and Hypothesis" in the famous 1878 series published *Popular Scientific Monthly* (W 3, 328).

In "On the Natural Classification of Arguments" of 1867 (W 2, 48) Peirce pointed to "a resemblance between the transposition of propositions by which the forms of probable inference are derived and the contraposition by which the indirect figures are derived." Peirce does not, however, elaborate the point. Still this remark does appear to be an allusion to the idea contained in the 1878 and 1883 papers. Confidence in this interpretation is rendered insecure by the fact that nowhere in the 1867 essay did Peirce require explicitly that the explaining syllogism for an induction have a statistical premise as the Rule. That requirement is present in both 1878 and 1883. Nonetheless, the 1867 essay does suggest the possibility that induction (and hypothesis) could be construed as "apogogic inversions" of statistical deductions.

The Lowell Lecture Series of 1866 does not even recognize the analogy between transposing the Rule and the Conclusion of a syllogism in Barbara and obtaining a third figure syllogism by contraposition from the same syllogism. Yet the 1866 lectures do recognize inverse inference from sample to population frequencies as paradigmatic of induction and recognize the importance of the probable and approximate similarity between sample and population in this respect. Thus, there is a progression from the virtual absence of statistical considerations in Peirce's account of induction (and hypothesis) in 1865 to the irreducibly statistical account of induction offered in 1878 and 1883.

3. FREQUENTISM AND INSUFFICIENT REASON

Even in 1865, Peirce insisted that the Case or minor premise of an explaining syllogism reports that the Ss (minor term of the explaining syllogism) are a selection from the class characterized by the middle term according to a method that took into account as a principle of

selection whether the item is an M or not without regard to other considerations. This method of selection is called random selection in 1866 and thereafter. Again Peirce is more explicit in the later papers that selecting an item at random from a population licenses the assumption that the statistical probability of selecting one item from the population is the same as selecting any other item. This method of random selection is the guarantor of the validity of the probable deduction to the outcome of a single selection and of the statistical deduction to the probable and approximate equality of the sample frequency with the population frequency in a statistical deduction.

I have suggested that Peirce held that there are occasions when one may legitimately judge degrees of belief construed as dispositions to take risks of certain kinds. When the inquirer can justify the result of a statistical deduction by appeal to the statistical rule and the premise that the method of selection is a random one, then *ceteris paribus*, according a degree of belief that the result will be the true equal to the statistical probability of such a result happening due to such selection is legitimate. The problem that Peirce sought to address is how to show that the inverse inference obtained by transposing Rule and Result that is an induction entitles us equally to infer that approximately $r\%$ of the population of M s are P s from the data that in a large random sample of M s, $r\%$ are P s. Success in this required that no degree of belief be assigned to the conclusion unless it could be derived via equation with a statistical probability deduced according to a proper statistical deduction.

Peirce stood opposed to one approach that many students of induction including Bayes, Laplace, and DeMorgan had favored. Peirce enthusiastically endorsed George Boole's criticism of the use of insufficient reason arguments to form priors in order to obtain posteriors as early as 1865 and even more explicitly in 1866. If the proportion of red balls in the bag is unknown, it is still the case that by statistical deduction based on the weak law of large numbers and the central limit theorem one could claim that no matter what the percentage ρ of red balls in the urn is the true one, the relative frequency of red balls selected at random is with great probability (is almost certainly) approximately equal to ρ . If via the principle of insufficient reason, one assigns equal prior probability to each conjecture regarding the true value of the population frequency as Bayes, Laplace, and De Morgan

recommended, then, according to Bayes' Theorem, the posterior probability conditional on evidence that the proportion of red balls in the large sample selected from the urn is r is going to be as high as approximately r of the balls in the urn from which the sample is taken are going to be red.

According to Peirce, the probability of a hypothesis conditional on evidence is the long-run relative frequency of obtaining a true conclusion from true data in inferences of that kind. The inverse inference appealing to insufficient reason would be acceptable to Peirce if the equal prior probability distribution over hypotheses concerning the proportion of red balls in the bag could be obtained on the ground that the bag from which the ball was selected was in turn selected at random from a bag of bags in which all possible proportions were present in equal proportions. Without some such assumption, the appeal to uniform prior probability and to Bayes' Theorem cannot yield a posterior construed in frequentist terms. In 1865 and 1866, Peirce expressed admiration for Boole's algebraic way of expressing the indeterminacy of the posterior probability derived from Bayes' Theorem when no prior is given (W 1, 238–9, 404–5). He also sharply criticized the principle of insufficient reason as a means for relieving the indeterminacy (W 1, 401–3). From the very beginning of his career, Peirce registered unwavering opposition to using insufficient reason and Bayes' Theorem to rationalize induction as Laplace had done.⁸ In 1878, Peirce famously wrote:

The relative probability of this or that arrangement of Nature is something which we should have a right to talk about if universes were as plenty as blackberries, if we could put a quantity of them in a bag, shake them up, draw out a sample and examine them to see what proportion of them had one arrangement and what proportion another. But even in that case, a higher universe would contain us, in regard to whose arrangements the conception of probability could have no applicability (W 3, 300–01).

Peirce reinforced this objection to the use of numerically determinate prior credal or belief probabilities without grounding in knowledge of statistical probability with a more specific objection to insufficient reason. In 1866, he complained that if one is ignorant with respect to the truth of h and $\sim h$, insufficient reason assigns h a probability of $1/2$. However, one may be thus ignorant and also ignorant concerning the truth of h , h^* , and h^{**} , where $h^* \vee h^{**} = \sim h$. Insufficient

reason recommends assigning h a prior probability of $1/3$. This verdict is inconsistent with the other one. But there is no principled way to resolve the conflict (W 1, 402).

4. UNIFORMITY OF NATURE

Peirce thought judgments of numerically determinate credal probability ought to be grounded in full belief that some statistical probability is true. Mill maintained that inductions ought to be grounded somehow in appeal to a major premise that, if true, would convert the induction into a categorical syllogism. Ultimately there would be a hierarchy of syllogisms where the fundamental major premise would be some principle of uniformity of nature. In 1865, Peirce complained that this demand for converting an induction into a syllogism threatens to yield an infinite regress of uniformity principles.

In 1866, Peirce added an additional more interesting cluster of complaints. The notion of uniformity is unclear. According to one of Mill's formulations, the universe "is so constituted, that whatever is true in one case, is true in all cases of a certain description" (*System of Logic*, v.1, Bk.3, Ch.3, §1). Peirce forcefully called into question the empirical warrant of this claim.

Every student of physics knows that a law which is exactly conformed to in nature without interference from other laws is almost if not quite unknown. Every law that is discovered therefore is found after a few years not to be exact. What do we say? Why that it is true in all cases of a certain description; but we haven't found of what description. . . .

There is still another sense in which we might speak of the uniformity of nature. If we select a good many objects on the principle that they shall belong to a certain class and then find that they all have some common character, pretty much the whole class will generally be found to have that character. Or if we take a good many of the characters of a thing at random and afterwards find a thing which has all these characters, we shall generally find that the second thing is pretty near the same as the first.

It seems to me that it is this pair of facts rather than any others which are properly expressed by saying that nature is uniform. We shall see that it is they which are the leading principles of scientific inference. (W 1, 420.)

Peirce distinguished between formal laws that would hold no matter what the "state of things" might be and material laws that do not.

He argued that the uniformity principle he endorsed will hold as long as there are any laws whatsoever. Since the existence of some laws is a precondition of knowledge, the uniformity he favors is a formal condition of all knowledge.

Peirce then went on to state that the prediction of the approximate equality of the proportion of *Ps* in the total population of *Ms* and the proportion found in a random sample of *Ms* corresponds to a special uniformity in the world. "It is the type of all uniformity and all induction. *Statistics*; that is induction" (W I, 423).

Peirce clearly thought that adopting this notion of Uniformity of Nature did not adequately account for inductive – i.e., statistical – inference, but merely rebaptised the challenge to do so.

The crucial point is that by the end of this discussion, Peirce had explicitly acknowledged even in 1866 that the conclusion of an induction did not have to be universal or approximately universal. Any proposition stating that the proportion of *Ms* that are *Ps* is approximately *r* can be the conclusion of an induction. In cases like estimation of a statistical parameter such as the proportion of red balls in the bag, all that is required is that the proportion be one of the possible relative frequencies.⁹ This is an important departure from Mill's understanding of uniformity.

5. NUISANCE INFORMATION

The progression in the emphasis Peirce placed on statistical considerations in his discussion of induction in 1878 and 1883 pivots on his appeal to the Weak Law of Large Numbers and the Central Limit Theorem in the calculus of probabilities to derive the validity of statistical deductions. Given the rule that states that the proportion *r* of *Ms* are *Ps* and the information that s_1, s_2, \dots, s_n are a numerous random sample from the *Ms*, statistical deduction warrants the conclusion that probably and approximately the proportion *r* of *Ss* are *Ps*. That is to say, probably the sample will be representative of the population when the proportion *r* of *Ss* are *Ps*. Moreover, for large *n*, this claim will hold for all proportions between 0 and 1 inclusive. Consequently, the inquirer who knows nothing about the outcome of the random sample to be taken other, perhaps, than that it will be taken can use statistical deduction and the calculus of probabilities to

conclude that the outcome of random sampling will be representative with very high probability concerning the proportion of *Ps* among the *Ms*. Hence, the inquirer is entitled before sampling to be nearly certain that he or she will obtain such a representative sample.

Once the sample is taken and the proportion of *Ps* among the *Ss* ascertained, the inquirer must reassess his subjective probabilities in the light of the new information available.

Suppose, for example, that an inquirer *X* intends to sample at random from cloven-hoofed animals, take note of the percentage of herbivorous animals among them, and then estimate that the percentage of cloven-hoofed animals that are herbivorous is approximately equal to the percentage in the sample. Prior to sampling, statistical deduction will warrant almost certainly that the approximate estimate of the population frequency will be true. The inquirer engages in the sampling and obtains the following information:

- (1) *S* is the random sample of *Ms*. Specimens of neat, swine, sheep, deer have been selected.
- (2) The percentage of *Ps* among the *Ss* is *r*.

Can the inquirer conclude via statistical deduction that it is almost certain that the proportion of *Ms* that are *Ps* is approximately *r* as he or she could prior to obtaining the information specified under (1) and (2)? I believe that Peirce gave one answer to this question in 1865 and 1866 and another answer in 1878 and 1883.

Peirce clearly recognized, as is evident from his review of John Venn's *The Logic of Chance* in 1867, that the desired inductive conclusion cannot be derived via statistical deduction from the information contained in (1) and (2). He had already rejected using Bayes' Theorem to secure high probability for that conclusion. So he insisted that no grade of probability grounded in frequencies could be assigned to the conclusion.

What is the problem with continuing to judge it highly probable that the sample is representative? Take first the information about the sample frequency contained in (2). Given the information that the sample frequency is *r*, the probability that the sample is representative is 1 given a population frequency near *r* and is 0 otherwise. We can no longer argue that it is almost certain that the sample is

representative no matter what the proportion of *Ps* in the population of *Ms* is.

Consider now the information given in (1). Even if the information about the sample frequency were unknown to *X*, knowing that the random sample contained specimens of neat, sheep, swine, deer *might*, as far as *X* knew, be relevant to being herbivorous in the sense that the frequency of being herbivorous among these kinds of animals is not typical of the entire cloven-hoofed population. To do a proper statistical deduction, *X* would need to have established that the information that the specimens are of neat, sheep, swine, and deer is irrelevant on the basis of information about frequencies. (This is the burden of the criticism of Venn in Peirce's 1867 review.) Typically this could not be done either.

6. INDUCTION AND SEMEIOTIC

Peirce's initial proposal for addressing these difficulties called for an appeal to a theory of signs. The 1865 and 1866 papers, as well as the series of papers given to the American Academy of Arts and Sciences in 1867, presented the first versions of what later became Peirce's semeiotic. He introduced these ideas into these papers, I contend, in order to provide a basis for confronting the problem of nuisance information in the reference class that faced his account of induction.

Space does not permit an extended discussion of Peirce's early theory of representations. But a brief summary is in order. There are three types of representations: signs (later indices), copies (later icons), and symbols. Neither signs nor copies convey information. Signs have denotation, extension, or breadth but no connotation, comprehension, or depth. Copies have depth but no breadth. Symbols have depth and breadth and carry information. More important, however, is Peirce's insistence that the depth and breadth of a symbol are determined by the information or belief state of the inquirer. According to an inquirer *X*, the "informed breadth" of a term *S* is the set of objects of which *S* might be true as far as *X* knows. The informed depth of *S* is the set of traits attributable in the light of what *X* knows to an object of which *S* is predicated. If the inquirer's state of information is held constant, term *M* has more informed breadth than term *S* if and only if it has less informed depth than *S*. Both

informed breadth and informed depth can increase, however, if the inquirer's state of information increases.

Observe that the claim that S is a random sample from M is the minor premise of the "explaining syllogism." That is the information conveyed by (2). That the proportion of P s among the S s is r conveyed by (1) is the conclusion of the explaining syllogism or, more accurately according to the later work, the probable deduction.

The inductive conclusion is obtained by substituting M for S in premise (1). To come to believe that approximately $r\%$ of M s are P s involves an increase in information. Peirce characterizes this situation in the following manner.

The extension or breadth of M is at least as great as that of S both relative to the information before and after the shift (typically it is known to be greater). The substitution, as a consequence, incurs a risk of error that may act as a deterrent to the inquirer X . To overcome this risk, the inquirer needs an incentive. That is provided by the fact that the replacement of S by M in (1) increases the informed depth of M due to the addition of the predicate P to its informed depth. And this increase yields an increase in information by adopting the conclusion of the induction. Whether the increase is sufficient to justify the induction depends on the "preferences" of the inquirer. It reflects the extent to which he or she is prepared to risk error to obtain new information.

There is one setting in which the trade-off automatically favors making the substitution of M for S . That is in the case where S is a list of the specimens (the neat, swine, sheep, and deer) selected at random from the M s. According to Peirce, even if the descriptions of the individuals selected are symbols, the disjunctive term S (is a neat or a swine or a sheep or a deer) is not, according to Peirce, a symbol because it does not characterize any common feature of the alternative species and, hence, carries no information. Peirce then argued that there is an "absolute" preference for " $r\%$ of M s are P s" (where M is a symbol) over " $r\%$ of S s are P s."

According to Peirce's view, an inquirer is not entitled to assess risk in terms of degrees of belief unless the degrees of belief can be grounded in knowledge of probabilities as frequency in the long run. Peirce denied that this was feasible even in the 1865 Harvard Lectures. Hence, the acknowledged tension between risk of error and value of information gained is not representable as a weighted

average of probability of error and the value of information. In the absence of a quantitative representation, Peirce understood the matter to be one of a comparative "preference." And it is possible for one inquirer to refuse to draw the inductive inference whereas the other endorses it.

In the case of induction by simple enumeration, however, the pressure is much stronger – or so insisted Peirce. The nuisance information contained in (1) is no information at all. The disjunctive symbol *S* has no informed depth. The claim that *S* is a sample selected at random from the *M*s is as informative as the claim that a random sample was selected from the *M*s. Hence, the claim that *S* is *P* is no more informative than the addendum that the sample randomly selected is included in the *P*s. Peirce seemed to think that as long as the term *M* had more informed depth than *S* (according to the state of information before reaching the inductive conclusion), the inquirer ought to have a marked preference for "All *M* are *P*" then for "All *S* are *P*" and, hence, should take the inductive leap. In effect, the inquirer is entitled to ignore the nuisance information contained in *S* because there is none.

Even if one endorses the distinction between real properties and mere predicates that so many authors advocate these days and denies that disjunctions of real properties are real properties, the strong claims Peirce makes about absolute preference do not appear to follow from the concession. Peirce abandoned this proposal silently, but I believe fairly quickly, in the late 1860s and early 1870s. He explicitly rejected it at the turn of the twentieth century. And by 1878 he had offered an alternative approach to replace this method of finessing the nuisance information.

In spite of its brief duration as Peirce's method of dissolving the problem of nuisance information, this idea is important to the understanding of Peirce's thought for two reasons: (a) it establishes that Peirce's semeiotic was initially used as a means of dissolving some difficulties in Peirce's account of induction and hypothesis and (b) it contributes to our understanding of the reasons why Peirce eventually abandoned the notion of hypothetic inference as distinct from induction and replaced it by a rather different idea at the beginning of the twentieth century.

In addition, it is worth noticing that in 1865 and 1866, Peirce toyed with the idea of rationalizing ampliative reasoning as a quest to seek

truth while shunning error in a context where seeking truth and avoiding error means seeking more information while avoiding error and where this objective is seen as a characterization of common features of proximate goals of specific inquiries.

Before turning to the reasons that led to replacing hypothetical with abductive inference, it is time to explain how Peirce came to think of induction in 1878 and 1883.

7. PREDESIGNATION AND SELF-CORRECTION

An inquirer X intends to sample at random from cloven-hoofed animals, take note of the percentage of herbivorous animals among them, and then estimate that the percentage of cloven-hoofed animals that are herbivorous is approximately equal to the percentage in the sample.

Prior to sampling, statistical deduction will warrant almost certainly that the policy the inquirer intends to adopt for making estimates of the population frequency will be true. At that stage, for example, the inquirer will not know what the specimens selected at random from the cloven-hoofed animals are. *Retrospectively*, however, the statistical deduction is undermined by the information that the sample selected is a sample of *Ss* and that the proportion of *Ps* in the sample is *r*.

Moreover, those who seek to use Bayes' Theorem to derive a posterior probability on the data for a statistical conjecture or who seek to use Peirce's early approach to exploit the information obtained retrospectively from the data to make a judgment seem to be in some difficulty.

In 1878 and 1883, Peirce proposed to think of the inquirer as adopting a program *prospectively* for using the data to make statistical estimates. The inquirer chooses a program suited to his or her objectives and then subsequently implements it. Implementation will require collecting data; but the inquirer is committed to using the data as input into the program adopted beforehand.

Before implementing the program, the inquirer can use statistical deduction to argue that the policy of estimating the population frequency to be approximately equal to the sample frequency is almost certainly true. The inquirer at that point lacks the sort of nuisance information that could interfere with statistical deduction.

If in sampling any class, say the M's, we first decide what the character P is for which we propose to sample that class, and also how many instances we propose to draw, our inference is really made before these latter are drawn, that the proportion of P's in the whole class is probably about the same as among the instances that are to be drawn, and the only thing we have to do is to draw them and observe the ratio. But suppose we were to draw our inferences without predesignation of the character P; then we might in every case find some recondite character in which these instances would agree. (W 4, 434)

The approach expressed here is a clear formulation of the approach advocated nearly 50 years later by J. Neyman and E. S. Pearson and elaborated by A. Wald. Indeed, Peirce, like these authors, required not only that the "characters" whose frequency in the population is to be estimated be specified in advance but also that the size of the sample (as the passage clearly indicates) be stipulated beforehand. The injunction against "optional stopping" is a key methodological marker distinguishing advocates of the Neyman–Pearson–Wald approach to statistics from advocates of the Bayesian approach and their cousins, advocates of the likelihood view.

The innovation is not merely that calculations of probability are relative to the information available to the inquirer before the experiment is instituted but that a plan for implementing a program for taking decisions or reaching conclusions is adopted relative to the pre-experiment information. Moreover, steps are taken to somehow "bind" the experimenter to following that program even after collecting the data necessary to implementing the program.

Peirce did not entirely appreciate the force of his own invention. He continued to think of induction as "inference" from data taken to be premises to an inductive conclusion even in 1878 and 1883. Inference calls for using the data reports as premises judged true, on the basis of which the conclusion is added to the body of beliefs. The data are not used as premises, according to the Peirce–Neyman–Pearson–Wald account. Using data as premises is precisely the source of the difficulties Peirce, Neyman, and Pearson all sought to circumvent. Data are used as "input" into a program that determines what is to be added to an inquirer's beliefs.

In my judgment there is a form of inference from premises recognizable as inductive; but implementation of the kind of preplanned program for adding new information envisaged by Peirce does not

fill the bill. Indeed, Peirce's view of induction after 1878 and 1883 and in all subsequent writing stands opposed to any evaluation of hypotheses on the basis of data after the data are collected that may be called inference from premises to conclusion.¹⁰

Nonetheless, Peirce did worry about retrospective assessment of hypotheses. In 1878, Peirce commented on an inference from a sample of Cretans all of whom are liars to "pretty much" all Cretans are liars: "whether there may be any special probability in that, I do not know" (W 3, 303). On the other hand, in cases where samples of *M*s are all *P*s, the long-run frequency of claims of the type "Nearly all *M*s are *P*" is very close to 1.

Peirce's remark in 1883 on statistical deduction and induction is both revealing and more interesting:

These two forms of inference, statistical deduction and induction, plainly depend upon the same principle of equality of ratios, so that their validity is the same. Yet, the nature of the probability in the two cases is very different. In the statistical deduction, we know that among the whole body of *M*'s the proportion of *P*'s is ρ ; we say, then, that the *S*'s being random drawings of *M*'s are probably *P*'s in about the same proportion, – and though this may happen not to be so, yet at any rate, on continuing the drawing sufficiently, our prediction of the ratio will be vindicated at last. On the other hand, in induction we say that the proportion ρ of the sample being *P*'s, probably here is about the same proportion in the whole lot; or at least, if this happens not to be so, then on continuing the drawings the inference will be not *vindicated* as in the other case, but *modified* so as to become true. The deduction, then, is probable in this sense, that though its conclusion may in a particular case be falsified, yet similar conclusions (with the same ratio ρ) would generally prove approximately true; while the induction is probable in this sense, that though it may happen to give a false conclusion, yet in most cases in which the same precept of inference was followed, a different and approximately true inference (with the right value of ρ) would be drawn. (W 4, 416–7)

As I understand this passage, in making the estimate that the proportion of *M*s that are *P*s is ρ , the inquirer may fail to add the estimate to his or her stock of full beliefs. In that case, the inquirer might declare that obtaining more data and making a fresh estimate (using the new data as input) would either "vindicate" the first estimate or "correct" it. Alternatively the inquirer might actually come to full belief that the first estimate is true. The inquirer might then think

that there is little point in an additional test because given the truth of the first estimate the probability of vindication by repeat performance would be very high indeed. Nonetheless, the inquirer should still admit that if counter to fact the first estimate were false, the new estimate would uncover the mistake.

As the passage just quoted indicates, Peirce thought that claiming that the “conclusion” is probable meant something different than it did in the case of probable deduction. Thus, he stood in disagreement with Bayesians and with advocates of Fisher’s fiducial argument. What the claim that the conclusion is probable means is that the conclusion was reached by a highly reliable procedure that if reapplied would uncover any mistake in the estimate. Induction is in this sense *self-correcting*.

Peirce did not offer this account of induction as his contribution to solving Hume’s problem of induction. He did think that his account avoids presupposing any principle of uniformity of nature or universal causation. Even so, the inquirer needs to assume beforehand not only the truth of the statistical model (or, at least, of some alternative very similar to that model) but also that the method of sampling implements the requirements of randomness necessary for the acquisition of the kind of data demanded by the model. So induction relies on “matters of fact” that go well beyond the testimony of the senses and the records of our memory. Since Peirce did not demand a justification of current beliefs not subject to serious doubt, this circumstance did not appear a problem to him. For the same reason, Peirce did not seek to avoid Hume’s problem by pressing for vindication rather than validation in the sense of Reichenbach, Feigl, and Salmon. He did not think that induction is “self-correcting” in the rather laughable sense promoted by Reichenbach according to which the inquirer interested in positing a limit of relative frequency keeps “correcting” his or her estimates with the acquisition of new data *ad infinitum*.

There is one serious problem with the account of Peirce’s view of induction I have just presented. If data are used as input and not as premises, then induction can no longer be considered as inference from premises to conclusion. To be sure, we sometimes speak of drawing inferences from suppositions taken as true for the sake of the argument, but the premises of an inference are otherwise claims judged to be true and used as evidence in justifying the addition of the

conclusion to the stock of beliefs. When data are used as input, they are neither suppositions nor premises judged to be true. Since Peirce continued in 1883 to think of inductive inference as an argument from premises to conclusion exhibiting the form of a transposition of the major premise of a statistical deduction and its conclusion, his views seem to contain elements in some tension.

I doubt very much that Peirce succeeded in completely removing this tension in his ideas. But he took a large step in that direction when at the turn of the century he explicitly abandoned two key elements of his earlier account of induction:

- (a) The appeal to semeiotic considerations of informed breadth, depth, and information.
- (b) The emphasis on taking induction as a transposition of premises and conclusion of a syllogism.

Feature (a) had already been abandoned silently by 1878. Feature (b) had been explicitly modified in 1878 and 1883. At the turn of the century, Peirce realized he had to retrench still more. His reasons had to do with his misclassification of the types of inference he called "hypothetic" as distinct from inductive inference.

8. INDUCTION, HYPOTHESIS, AND ABDUCTION

When Peirce introduced hypothetic inference in 1865 and 1866, he characterized it in terms of its form and contrasted this form with the form of its explaining syllogism and the inductive inference that shared the same explaining syllogism. Peirce attached some importance to this point. Logic is unpsychologistic because it investigates forms. In the case of deductive logic, validity is itself a purely formal matter. By way of contrast, inductive and hypothetic validity are not characterizable entirely in terms of formal considerations. But the forms of inference eligible for evaluation with respect to their strength as inductive or hypothetic arguments can be identified.

By 1883, Peirce had broken with the idea that induction and hypothesis are transpositions of *categorical* explaining syllogisms. Inductive inferences are obtained by transposing the major premise of a statistical deduction with the conclusion after the fashion already explained. Hypothetic inference is also obtained by transpositions of premises and conclusions of statistical deductions. And they

differ from inductions in that the transposition is of the minor premise with the conclusion. But the statistical deduction whose premise and conclusion are transposed has a different structure from the explaining statistical deduction for induction. It is a "statistical deduction in depth." The idea seems to be this.

The major premise of the statistical deduction in depth asserts that all *Ms* are P_1s, P_2s, \dots, P_ns . *S* has an *r*-likeness to the *Ms*. Hence, probably and approximately, *S* resembles an *M* in *r* out of the *n* respects P_i .

There are several unexplained features of this mode of inference. One concerns the interpretation of "*r*-likeness." Peirce couched matters this way to guarantee that the explaining deduction is transformed into a hypothetic inference by transposing the minor premise and the conclusion. Immediately after presenting his idea, Peirce conceded that in the "extended sense" he had given to the term induction, the argument "is simply an induction respecting qualities instead of respecting things." The P_i s are "a random sample of the characters of *M* and the ratio *r* of them being found to belong to *S*, the same ratio of all the characters are concluded to belong to *S*" (W 4, 419). This suggests an alternative explaining statistical deduction for the so-called hypothetic inference. The major premise asserts that object *S* has proportion *r* of the (relevant) characters of *Ms*. A random sample has been taken of the characters of *Ms*. Hence, probably and approximately, *S* has the proportion *r* of the characters thus selected. By transposing the major premise and conclusion of this probable deduction, the same inference initially classified as hypothetic is exhibited in the form of an induction.

Peirce preferred presenting the inference as hypothetic because of the impossibility of "simply counting qualities as individual things are counted. Characters have to be weighed rather than counted" (W 4, 419). But he clearly had appreciated that the inference could be classified one way or the other. And, in any case, both induction and hypothesis were recognized by Peirce to be ampliative rather than explicative.

Apparently the awkwardness of his system of classification eventually led Peirce to abandon the basic template for classifying scientific inferences that he had used from 1865 to 1883 when he made his most important contribution to the foundations of statistics. By the beginning of the twentieth century Peirce acknowledged not only

that his use of his semeiotic to assess the acceptability of inductive and hypothetic inference was overblown but also that so was his use of permutations of syllogisms to classify ampliative inferences into inductive and hypothetic inferences.

Peirce replaced his earlier classification with a new, more ramified account. A division between deduction, induction, and abduction (or retroduction) replaced the trichotomy between deduction, induction, and hypothesis. The change was more than terminological and cosmetic. As already noted, deduction included not only categorical syllogism, propositional logic, the logic of relations, and quantification but also probable and statistical deduction. Both probable (and statistical) deduction are used in scientific inquiry to explicate the consequences (more crucially the testable consequences) of conjectures proposed as answers to a question or problem under investigation. Abduction yields the conjectures. Induction invokes the data obtained by observation or test on the basis of which conjectured answers to the given question are eliminated.

Although Peirce insisted throughout that this classification is of different types of inference, it seems to me better to think of it as focused on different *tasks* that are undertaken in problem-solving inquiry. One needs to identify potential answers to the question under investigation. That is the task of abduction. Deduction focuses on deriving (either apodictically or probabilistically) consequences of conjectures that were obtained via abduction. Induction evaluates the status of conjectures as a result of testing.

The model is neither the hypotheticodeductive model nor a falsificationist model. According to Peirce, the “deductive” component concerns deduction broadly conceived to include statistical deduction. And Peirce appreciated (as Braithwaite later did but Popper apparently did not) that the rejection of a statistical conjecture because the predictions derived via statistical deduction are observed to be false is an ampliative inductive inference.

In his later years, Peirce could understand induction in this way because induction included all ampliative reasoning. In a draft of a letter to Paul Carus from 1910, published as CP 8.214–239, induction so conceived included (a) quantitative induction, induction from a sample whose representativeness of the population from which it is sampled is secured by random sampling; (b) qualitative induction,

which Peirce admitted he classified as hypothetical reasoning in 1883; and (c) crude induction, which appears to be induction by simple enumeration.¹¹

Throughout this discussion the appeal to transpositions of syllogisms is completely forgotten, as is the semeiotic. There is no effort to offer an account of the difference between deduction, induction, and abduction as a distinction between inferences of different forms. Peirce's early attempts to understand some aspects of scientific reasoning other than deductive logic as part of formal logic are abandoned. The distinction between deduction, induction, and abduction focuses on the differences between the *tasks* that an inquirer will have to address in problem-solving inquiry.

Consider, for example, the task of identifying potential solutions or answers to a given question or problem. That is the task of abductive reasoning. Abductive inference is from the puzzling phenomenon to a conjectured potential answer. Once a potential answer has been proposed, one might bring general critical principles to bear in ascertaining whether the putative potential answer is one that is relevant to the question under study. Peirce thought that there are general principles for evaluating such abductive claims.

The pragmatic or pragmaticist principle is the fundamental principle of abduction. It is not a general principle for distinguishing truth-value-bearing judgments from other kinds of judgments. It distinguishes between propositions that qualify as potential answers to the problem under investigation and propositions that do not by reference to their testable consequences. Here the consequences are consequences of the potential answers given the initial settled body of background knowledge already taken for granted as perfectly free from doubt (though vulnerable to further scrutiny if the results of subsequent inquiry warrant raising new doubts). Since the background knowledge can change, the conditions for being a potential answer can change. Peirce's pragmatic principle is not a verificationist surrogate for truth.

Deduction (which includes both necessary and probable deduction) is used to elaborate the testable consequences of the conjectures formed via abduction. Induction (whether quantitative, qualitative, or crude) contributes to confirming or disconfirming these conjectures. Disconfirming a conjecture, like confirming it, is, in general,

an ampliative induction because, for Peirce, conjectures will typically involve some sort of statistical element.

Thus, Peirce's mature view was (a) strongly opposed to the sort of probabilism so widely endorsed by philosophers who endorse Bayesianism, (b) supportive of the importance of forming testable conjectures, as Popper insisted, but (c) in opposition to both the probabilists and the Popperians, strongly in favor of ampliative induction.

It is also clear that by the end of his career, Peirce had jettisoned the narrow antipsychologistic view of the logic of scientific inference with which he had begun. Unlike Carnap and Popper, who sought to keep logic free of context-sensitive normative components, Peirce came to acknowledge the centrality of normativity and context to logic. This must be so once the distinction between the three kinds of "inference" is based on the difference in the tasks and, hence, the aims of these kinds of reasoning. I believe that this heightened sensitivity to context derived from the lessons he learned while attempting to address statistical reasoning – a topic that remained at the core of his philosophical interest from the beginning of his career to its end.

Early on (W I, Lecture VI), Peirce warned of the dangers of too much reliance on syllogistic reasoning. But Peirce himself actually sought to build his account of induction and hypothesis on the formal relations between these kinds of reasoning and deduction – especially syllogism. He came to appreciate fully the dangers of such reliance only in the last decade of his career.

Peirce should not be faulted for his slowness to see the dangers of flirting with transpositions of the syllogism. Much twentieth century and contemporary thought on induction and probability remains too closely tied to paradigms characteristic of deductive reasoning. To Peirce's credit, he appreciated the serious difficulties confronting those who embrace some form or other of probabilism and those who have been bewitched by the deductivism and falsificationism of Popper. He had an understanding of the distinctive issues that the "nonmonotonicity" of inductive reasoning generates. And he did propose an account of "inductive behavior" that anticipated the ideas of Neyman and Pearson a half century before they presented them. His pragmatic principle, taken as the fundamental principle of abduction, is a sophisticated version of Popper's demarcation criterion. But unlike Popper, who seemed mysteriously to think that producing

testable conjectures via abduction might have a point without the inductive reasoning needed to decide between them, Peirce understood that the testing of conjectures called for more than deductive reasoning. Peirce learned to be wary of syllogisms in a way that neither Carnap, Popper, nor their epigones ever managed to do.

NOTES

1. The documents to which I refer include the Harvard Lectures of 1865 and Lowell Lectures of 1866 (W 1); "The Classification of Arguments" and "Upon Logical Comprehension and Extension" of 1867, the review of Venn's *Logic of Chance* of 1867, and "Grounds of Validity of the Laws of Logic; Further Consequences of Four Incapacities" of 1869 (W 2).
2. I have no doubt that he assumed this in 1865 as well although there is no textual evidence to support this claim. It seems that Peirce at the very outset of his career thought of inductive inference in a statistical setting where probability is understood in terms of long-run or limiting relative frequency. However, his appreciation of the significance of statistical considerations for his view of induction and hypothesis as transpositions of syllogisms seems to have undergone important changes from that time through 1883, as I shall try to explain.
3. Peirce contended (W 1, 265–6) that other forms of inductive argument can be obtained by transposing syllogistic forms in the second and third figure legitimated by contraposition. (Such transformations are "apogogic inversions." Baconian induction combines a transposition of the major premise and conclusion of a syllogism in Barbara, such as the inference to "All cloven-hoofed are herbivorous," with another induction obtained from a syllogism in Camestres (AEE Figure 2), where the A proposition is the Rule in the syllogism in Barbara, E is the negation of the Result of the syllogism in Barbara, and the conclusion is the denial of the Case of the syllogism in Barbara: All Cloven-hoofed are herbivorous. A sample is taken from nonherbivorous animals). Specimens of rats, dogs, and apes. So we have that rats, dogs, and apes are not herbivorous. And it is discovered that these specimens are not cloven-hoofed. This explains the data in an induction whose premises are the denials of the Case and Result in Barbara and whose conclusion is the Rule. Peirce then explored Figure 3 AII, explaining syllogisms in Datisi. He claimed their inversions can give "indirect" support for the conclusions of the inductions whose explaining syllogisms are in Barbara and Camestres.

In the final paragraph of "On the Natural Classification of Arguments" of 1867 and more explicitly in "A Theory of Probable Inference"

of 1883, Peirce argued that in the case of probable deduction, where the Rule or Major Premise is a statistical assumption rather than a categorical one, there is little use for the transpositions in the figure 3 case. They all are virtually reducible to the statistical analogue of figure 1.

4. "Deduction, Induction and Hypothesis" of 1878 distinguishes between "necessary" deductions in Barbara and "probable" deductions "of similar form" (W 3, 329). This may have been Peirce's attitude in the 1866 Lowell Lectures and the discussion in the paper "The Classification of Arguments" of 1867. It is very explicitly the attitude in "Grounds of The Validity of the Laws of Logic" of 1869 (W 2, p. 267). In 1883, Peirce retained the idea that statistical premises are rules but distinguished between statistical rules and categorical propositions as rules. Syllogisms are necessary deductions. Probable deductions are not. So probable and statistical deductions are not syllogisms. Peirce retained an analogy between syllogism in Barbara and statistical deduction; but as noted above, there are some marked and important differences – many of which he recognized in the earlier work but whose significance was more difficult to articulate within the framework of his earlier typology.
5. Peirce may have been stimulated by his involvement with his father, Benjamin Peirce, in compiling and analyzing statistical data pertaining to the authenticity of the signature of Sylvia Ann Howland to an addendum to her will in the Howland Will Case. Benjamin and Charles Peirce gave testimony in 1867. He also wrote a review of Venn's *Logic of Chance* in 1867 that, as we shall see, raised a question about probable deduction that posed issues for induction. I shall argue that he managed to finesse these difficulties only in the 1878 and 1883 essays. Consideration of Venn's ideas may also have contributed to the development of his ideas on induction.
6. See footnote 4.
7. Peirce expressed a common, mistaken prejudice among probabilists when he insisted that probable deduction is the quantitative branch of deduction whereas syllogism is the qualitative branch. By implication, this implied a similar relation between inductive transpositions of statistical deductions and transpositions of syllogisms. In 1883, Peirce explicitly stated that when $r = 1$ or 0 , the induction is an "ordinary induction," by which he meant an inference to a universal generalization. In the earlier discussions, this observation was not made but I think Peirce took it for granted. In 1866, Peirce shifted from discussion of induction, illustrated by inference from data obtained from sampling balls from a bag to conclusions about the proportions of balls of a given color in the bag, to examples of induction by transposing categorical syllogisms. There is only the slimmest of hints as to what the connection

between the two ways of understanding inductive inference might be. Yet Peirce did consider a version of the sampling example where seven balls are selected at random from the bag and all are found to be black and the inference is to "Almost all balls in the bag are black." And it is clear that he thought the difference between "All" and "Almost all" to be important even though he did not explain why he thought so in 1866 as he did in 1883. All of this makes sense if we interpret him as endorsing in 1866 the view of the quantitative and qualitative branches of deduction. In the early years of the twentieth century, Peirce became aware of Borel's work and began to acknowledge that although to say "All balls in the bag are black" warrants claiming that the statistical probability of obtaining a black ball on random selection from the bag is 1, the converse does not hold. How fully he appreciated the ramifications of this point for his views I cannot say.

8. This suggests that even in 1865 Peirce hoped to use his account of induction as transposition of premises and conclusions of an explaining syllogism in order to address statistical inference from sample to population. In that case, the progression in the introduction of statistical issues I have noted in Peirce's papers may indicate the extent to which he was able to address various problems with relating categorical syllogisms to what he later called statistical deductions and, hence, to providing an account of induction and hypothesis as transpositions of premises and conclusions of deductive inferences.
9. If there are n balls in the bag, the number of possible proportions is $n + 1$. Peirce did not address the issue of infinitely many values well; but for countably and even continuumly many values it is possible to address the matter in a coherent fashion. Peirce clearly maintained in 1866 that induction does not presuppose that nature is uniform but that it may instead exhibit statistical regularities.
10. He opposed arguments for balancing arguments essentially rationalized in Bayesian terms both early and late.
11. Peirce was identifying species of induction that are variants of the classification in the draft letter to Carus in 1901 in "The Logic of Drawing History from Ancient Documents." By then Peirce was using the term "abduction" exclusively in the sense of forming conjectured answers to questions that then call for testing (EP 2: 96–106). He further refined quantitative induction to mark off variations where the population from which sampling is done is not finite.