CHAPTER 14

PEIRCE AND THE TRIVIALIZATION OF THE SELF-CORRECTIVE THESIS*

If science lead us astray, more science will set us straight.1
– E. V. DAVIS (1914)

The aims of this chapter are two-fold: first and primarily, to identify and to summarize the development of an important but hitherto unnoticed tradition in 19th-century methodological thought, and secondly, to suggest that certain aspects of the history of this tradition give us a new perspective from which to assess certain strains in contemporary philosophy of science. In Part I below, I attempt to define this tradition, to document its existence, and to note some features of its evolution. In Part II, I briefly indicate the manner in which this history may shed new light on some recent trends in inductive logic.

I

As the title of the chapter suggests, the tradition that interests me is connected with the view of scientific inference as self-corrective, and the work of Charles Sanders Peirce looms large in the story.2 It has been customary to see Peirce as the founder and first promulgator of the view that the methods of scientific inference are self-corrective.3 This historical claim is simply incorrect. The doctrine that scientific methods are self-corrective, that science in its development is inexorably moving closer to the truth by a process of successive approximation, has a pedigree extending back at least a century before Peirce’s time. And, in my view, Peirce’s importance resides not in the creation of this doctrine but in his transformation of it in subtle but significant ways. As I shall argue below, Peirce is the crucial logical and historical link between 19th- and 20th-century discussions of self-correction and progress towards the truth. Moreover, he is responsible for effecting a major metamorphosis in the self-correcting doctrine as it had been understood by his predecessors. To get some sense of the magnitude of that mutation, we must go back to the middle of the 18th century to see how and why the idea of self-correcting modes of inference arose.

Beginning in the 1730s and 1740s, a number of philosophers and scientists
began to claim that science, as a result of the methods it employs, is a self-corrective enterprise. (Hereafter I shall refer to this view as the self-corrective thesis or simply SCT.)

Most early versions of SCT — like their more recent counterparts — were closely connected with a theory of scientific progress (SCT asserting, in effect, that science does “progress”) and it is, therefore, not surprising that the Enlightenment view of intellectual progress first provided a leitmotif and rationale for SCT. That eclectic theory of knowledge, unique to the *philosophes*, which identified the growth of the mind with the moral improvement of mankind, was certainly related to the doctrine of self-correction. But it is important not to be too beguiled by facile historical plausibilities. That the Enlightenment theory of progress produced fertile ground for the growth of the self-corrective view is quite likely; but we must look beyond the ethos of the Enlightenment to find the initial stimulus for theories of self-correction. Specifically, we must look to certain tensions and problems latent in the history of methodology itself. For instance, it is crucial to realize that the self-corrective thesis was itself a weakened form of a still more sweeping thesis which had dominated metascientific thought from antiquity.

According to this more general thesis, which we might call the *thesis of instant, certain truth* (TICT), science — in so far as it is genuine science — utilizes a method of investigation which infallibly produces true theories. Virtually every theorist of method in the 17th century (including Bacon, Descartes, Locke, and Newton) subscribed to TICT. The proponents of TICT believed that science could dispense with conjectures and hypotheses since there was, ready at hand, an “engine of discovery” (as Hooke called it) which could infallibly (and usually mechanically) produce true theories. The concept of progress, within the framework of TICT, was clear and unambiguous. Progress, on this view, could only consist in the *accumulation of new truths*. The replacement of one partial truth by another simply made no sense in this context. Growth, in so far as it occurred, was by accretion rather than by attrition and modification.

By the middle of the 18th century, however, many methodologists were convinced that TICT was untenable. Difficulties in articulating a coherent logic of discovery, along with sceptical arguments about the inability of empirical evidence to prove a theory conclusively, conspired to chasten the scientist’s confidence in the undisputed truth of his mental creations and to make (merely probable) hypotheses respectable, for the first time since the euphoria of the Scientific Revolution had made them unfashionable.

There were two major arguments which seemed to undermine TICT: one
was directed against the method of “proof *a posteriori*” (as Descartes had called it); the other, against eliminative induction. The main argument of the first kind was an application of the so-called “fallacy of affirming the consequent” to scientific inference. As surprising as it might seem, several methodologists and scientists in the 17th and 18th centuries had argued that the ability of a theory to predict successfully an experimental result was prima facie evidence that the theory was a proven truth. Cartesians (e.g., Jacques Rohault) and Newtonians (e.g., Bryan Robinson) alike often slipped into this sloppy mode of reasoning. By 1750, however, the inconclusive character of this form of inference had been pointed out by Leibniz, Condillac, and David Hartley, among others.

Similarly, the method of proof by eliminative induction (associated with Bacon and Hooke) had been discredited by the arguments of Condillac, Newton, and LeSage against the possibility of exhaustively enumerating all the conceivable hypotheses which might be invoked to explain a class of events. These three all asserted that (in light of the impossibility of knowing that we have thought of all the appropriate hypotheses which might explain facts in a given domain) we can never be sure that the hypotheses which have survived systematic attempts at refutation are true.

(The third major candidate for a model of scientific inference, *enumerative* induction, had long since been discredited; in antiquity by Aristotle and Sextus Empiricus, and in early modern times by Bacon, Newton, and Hume, among others.)

Since none of the known modes of “empirical inference” were valid, methodologists of science in the late 18th century were no longer able to speak, with a clear conscience, about the certainty and truth of scientific theories. (The notable exceptions to this generalization are the “a priorists” e.g., Lambert, Wolff, and Kant; but theirs was a minority viewpoint.)

Prepared to concede that the theories of the day might eventually be refuted, convinced moreover that TICT was too ambitious, several late 18th-century methodologists produced a compromise. If, they reasoned, there is no instant, immediate truth, we can at least hope to reach truth *in the long run*. Even if the scientist’s methods do not guarantee that he can get the truth on the first attempt, perhaps he can at least hope to get ever closer to it. Even if the methods of science are not foolproof, perhaps they are capable of correcting any errors the scientist may fall prey to. Thus was born SCT. In some ways, it was a face-saving ploy, for it permitted the scientist to imagine that his ultimate goal was, as TICT had suggested, the Truth; although the
scientist now had to be satisfied with the quest for ever-closer approximations rather than the truth itself.\textsuperscript{7}

At the same time that SCT was emerging (and this was no coincidence) some methodologists were moving away from a Baconian inductive model of scientific inference towards something like a model of conjectures and refutations.\textsuperscript{8} Science was seen, not as a discipline where theories were somehow extracted or deduced from experiment, but rather as one where theories were formulated, tested, rejected, and replaced by other theories. When SCT was stated within the context of such a model of scientific inquiry, it generally amounted to the following claims:

(1) Scientific method is such that, in the long run, its use will refute a false theory;

(2) Science possesses a method for finding an alternative $T$ which is closer to the truth than a refuted theory.\textsuperscript{9}

On this view, which is as much an historiography as a philosophy of science, the temporal sequence of theories, in any genuinely scientific domain is a series of ever-closer approximations to the truth (provided of course, that science uses the method(s) which insure(s) self-correction). And there was a certain amount of intuitive plausibility to this picture. Even today, it is common to hear that Ptolemy's system was closer to the truth than Aristotle's system of concentric spheres; that Copernicus' heliocentric system was 'more nearly true' than Ptolemy's; and that Kepler's elliptical system is a still closer approximation.

It is important to be clear about the set of problems which SCT was presumed to resolve. Like TICT before it, SCT was designed to provide an epistemic solution to the problem of scientific knowledge. That problem can be put in various forms: Why should we take science seriously as a cognitive pursuit? What justification is there for the methods which science employs? Why should we prefer science to quackery or pseudo-science? Whatever our views about SCT, we must at least concede that it was an attempt to resolve what are perhaps the central problems of the philosophy of science; namely, the justification of both the knowledge-claims and the methods of the natural sciences.

Adherents of SCT provided what was, in its time, a highly original approach to this perennial problem. For them, the justification of science as a cognitive, intellectual pursuit was sought — not in the certainty or even the truth of its conclusions — but in its progressive evolution towards the truth. As I shall show below, in the course of the later evolution of SCT, there was
an increasing tendency to lose sight of this justificational problem in its full
generality, a tendency to see the self-corrective thesis as the solution to rather
different problems, of far less significance. But more of that below.

If the conditions I have spelled out indicate roughly what SCT amounts to, what was its rationale? What reason had Enlightenment philosophers to believe that science uses methods which satisfy conditions (1) and (2) above? The early proponents of SCT provided an answer, but not a very satisfactory one. Pursuing analogies between certain methods of mathematical inference and the methods of science, they claimed that just as the mathematician finds the roots of an equation by posing incorrect guesses, and then refines those guesses via mechanical tests, so the scientist can formulate an incorrect hypothesis and subsequently improve on it by comparing its results with observation, altering the hypothesis where necessary to bring it into closer agreement with fact. Clearly, the analogy is incomplete. After all, it does not prove that the methods of science are self-corrective to compare them with self-corrective mathematical techniques unless the analogies between the two cases are very strong in appropriate respects. Unfortunately, they are not (or, at least, they were not shown to be) strongly analogous. Although it is relatively easy to show that the method of hypothesis satisfies condition (1) above, there is no machinery for insuring that such a method satisfies condition (2) or even (2').10 Indeed, no methodological procedure was suggested in this period for replacing a refuted hypothesis by one which could be known (or reasonably presumed) to be closer to the truth. This perhaps can be made clear by discussing a pair of representative early defenders of SCT. Among the first philosophers11 to address themselves to this problem were David Hartley (1705–57) and Georges LeSage (1724–1803), who, although working independently, arrived at almost identical results. I shall consider them in turn.

In a chapter, “Of Propositions, and the Nature of Assent”, in his Observations on Man (1749), Hartley analyzed the sorts of methods which the scientist has at his disposal. Hartley insisted that only in mathematics can one develop theories which can be rigorously demonstrated.12 In science, however, we must be content with something less than certainty. However, taking his cue from the mathematicians, Hartley believed that the scientist can utilize certain methods which, if they do not yield the truth immediately, will gradually bring the scientist to a true theory in the long run. He proposed
two different methods, both based on mathematical techniques, both of which are self-correcting, and both of which are, in the long run, supposed to lead the scientist to the truth:

(1) The rule of false position. This approximative technique, known as the *regula falsa* among Renaissance mathematicians, was characterized by Hartley as follows:

Just as the arithmetician supposes a certain number to be that which is sought for; treats it as if it was that; and finding the deficiency or over-plus in the conclusion, rectifies the error of his first position by a proportional addition or subtraction, and thus solves the problem; so it is useful in all kinds of inquiries, to try all such suppositions as occur with any appearance of probability, to endeavour to deduce the real phenomena from them; and if they do not answer in some tolerable measure, to reject them at once; or if they do, to add, expunge, correct, and improve, till we have brought the hypothesis as near as we can to an agreement with nature. After this it must be left to be further corrected and improved, or entirely disproved . . . .

Two centuries earlier, the mathematician Robert Recorde had, like Hartley, been impressed and amazed at the capacity of the rule of false position to generate truth from error, as this delightful piece of doggerel verse indicates:

Gesse at this woorke as happe doth leade  
By chaunce to truthe you may procede  
And first woorke by the question,  
Although no truthe therein be don.  
Such falsehode is so good a grounde,  
That truthe by it will soone be founde.

Hartley took Recorde's point one important step farther, however, by arguing that this sort of method works in natural philosophy as well as in algebra.

(2) The method of approximating to the roots of an equation. Like the rule of false position, this Newtonian technique was seen by Hartley as a means of generating a theory "which though not accurate, approaches however, to the truth". Here, the scientist begins by a guess at the root of the equation. From such a guess, applied to the equation, "a second position is deduced, which approaches nearer to the truth than the first; from the second, a third, etc." Hartley insists that the use of such self-corrective methods "is indeed the way, in which all advances in science are carried on".

There are, I believe, two important points to note about each of these methods. In the first place, both involve the inquirer in making posits (viz., hypotheses) which, if false, can be eventually falsified. Much more importantly, they both provide a method, having once refuted an hypothesis, for
mechanically finding a replacement for it which is closer to the truth than the original hypothesis. These two characteristics together constitute the necessary and sufficient conditions for what I shall call a strong self-correcting method (or SSCM). A method is an SSCM if and only if (a) it specifies a procedure for refuting a suitable hypothesis, and (b) it specifies a technique for replacing the refuted hypothesis by another which is closer to the truth than the refuted hypothesis. Much of this paper will be concerned with post-Hartleyan accounts of SSCMs.

Unfortunately, Hartley himself did not indicate how we can apply such mathematical methods to the natural sciences. While it is easy enough to imagine that scientific hypotheses are refutable (neglecting Duhemian considerations), it is more difficult to guess what rule he had in mind for replacing a refuted scientific hypothesis by one which was more nearly true. Hartley simply took it for granted that one can, in a more or less straightforward fashion, import these mathematical techniques into the logic of the natural sciences.

Hartley’s contemporary, Georges LeSage, though drawing on slightly different mathematical analogies, made an argument very similar to Hartley’s. LeSage compared the procedure of the scientist to that of a clerk solving a long-division exercise. At each stage in the division, we produce in the quotient a number which is more accurate than the number appearing as the quotient in the preceding stage. At each stage, we multiply the divisor by the assumed quotient and see if it corresponds to the dividend. If it does not, we know that there is an error in the quotient, and we have a mechanical process for correcting the error, i.e., for replacing the erroneous quotient by one which is closer to the true value. Going beyond such fanciful examples, LeSage, like Hartley, suggested that there are other approximative techniques which the scientist can borrow from the mathematician, including “the extraction of roots, the search for the rational divisors of an equation and several other arithmetical operations”. Beyond this, LeSage’s views are, even to their ambiguity, sufficiently similar to Hartley’s not to require separate consideration.

As I hinted before, the thesis that science is self-corrective and thereby progressive lends itself neatly to the 18th-century view of progress, for the sequence of theories of ever greater verisimilitude was the mirror image on the intellectual level of man’s progressive perfection on the moral level. Joseph Priestley, who was in these matters a self-avowed disciple of Hartley, made explicit the link between the self-corrective character of science and his theory of scientific progress. He wrote:
Hypotheses, while they are considered merely as such, lead persons to try a variety of experiments, in order to ascertain them. These new facts serve to correct the hypothesis which gave occasion to them. The theory, thus corrected, serves to discover more new facts, which, as before, bring the theory still nearer to the truth. In this progressive state, or method of approximation, things continue . . . .

Clearly, the weakness with all these programmatic statements is that they simply insist that scientific methods are self-corrective, without indicating precisely the manner in which they are so. Without a persuasive reason for believing that the methods of science are self-corrective, we have no rational grounds for speaking of scientific progress, a point which the logician and physiologist Jean Senebier was quick to emphasize: “Often we move imperceptibly away from the truth, and do so even whilst we believe that we are working towards it.”

The case against the vagueness of SCT as developed by Hartley and LeSage was put convincingly by Pierre Prevost in 1805. He insisted that scientific procedures necessarily differ from such mathematical techniques as the rule of false position. He observed that we do not generally have the knowledge in science to be able to satisfy the conditions of the rule of false position, and that we therefore cannot expect much from that method in science. Prevost argued specifically against the self-correcting character of the method of hypothesis. All that method permits us to do, in his view, is verify or refute an hypothesis; it provides no machinery for replacing a refuted hypothesis with a better one:

Thus when Kepler, beginning with the circular hypothesis, tried out various eccentricities for the orbit of Mars, these false suppositions could (and indeed should) never have led him to a solution. When afterwards he recognized the weakness in the circular hypothesis, if he had tried other curves entirely by chance, he would have been using another method which could well have never brought him to his goal.

I hope these few texts have made it reasonably clear that by the early years of the 19th century, the problem of justifying scientific knowledge (i.e., as infallible, indubitable truth) had been replaced — at least among some writers — by a program for justifying science by claiming that it pursues a method which will lead it ever closer to the truth. The extent to which this kind of approach quickly came to dominate methodological thought is illustrated by the fact that the philosophies of science of Herschel, Comte, and Whewell were all concerned overtly with the progress of science and its gradual approach to the truth.

Among 19th-century scientists as well as methodologists, the view persisted of science as an enterprise moving inexorably closer to a final truth. Claude
Bernard, among others, conceived science in this approximative way. Thus, Ernest Renan wrote about Bernard:

Truth was his religion: he never had any disillusionment or weakness, for not a moment did he doubt science ... The results of modern science are not less valuable for being acquired by successive oscillations. These delicate approximations, this successive refining, which leads us to modes of understanding ever closer to the truth are [for Bernard] the very condition of the human mind.  

Similarly, that fervent Darwinian T. H. Huxley believed that “the historical progress of every science depends on the criticism of hypotheses — on the gradual stripping off, that is, of their untrue or superfluous parts . . . .”  

The key to the progressiveness of science was thought to reside in the fact that it utilized a method which was essentially self-corrective in character. Given time and sufficient experience, science could be perfected to any stage desired. In the middle years of the 19th century, especially with Comte and Whewell, the doctrine of progress through self-correction became, in many ways, the central concern of the philosophy of science. Science was seen as a growing, dynamic enterprise and, accordingly, philosophers of science were prone to stress such dynamic, growth-oriented parameters as increasing scope and generality, greater accuracy and systematicity and, above all, progress towards truth. However, throughout much of the 19th century, the self-corrective character of scientific method, while regularly invoked and persistently praised, remained as unestablished as it had been with LeSage and Hartley. Everyone assumed that science is self-corrective (and thereby progressive), but no one bothered to show that any of the methods actually being proposed by methodologists are, in fact, self-corrective methods.

The focus of the self-correcting thesis had always been on conceptual change, on the progressive succession of one theory by another. What self-correctionists had sometimes ignored was that sort of “progress” which comes from increasing the probability of theories (most often by successful confirmations), without any change in the theories themselves. This second type of progress, which we might call “progress by probabilification”, received much attention in the 19th century. Herschel, Brown, Whewell, Jevons, and Apelt (to name only a few) discussed at length the methods by which we can gain confidence in our theories, without necessarily altering them. Partisans of progress through probabilification, tended to stress the continuity of scientific theory; for them, experiments with high confirming potential were emphasized rather than the falsifying experiments which the self-correctionists stressed. If the advice of the self-correctionists to experimental
scientists was "Devise experiments which will indicate weaknesses in your theories," the corresponding advice from the probabilifications was "Devise experiments which, if their outcome is favorable, will do most to contribute to the likelihood of your theories." Impressed by Laplace's rule of succession and the application of probability theory to induction, the "probabilists" argued that every valid theory goes through all the degrees of certainty from extreme improbability to great likelihood. (Writers like Thomas Brown and John Herschel identified that transition as one from "hypothesis" to "theory" or "law".)

It would be wrong to give the impression that these two alternative theories of scientific progress, one by self-correction and the other by probabilification, were mutually exclusive. On the contrary, several of the best-known methodologists of the period (e.g., Whewell and Bernard) adopted both, arguing that "local" progress occurred by probabilification, while "cross-theoretical" progress was governed by a self-corrective method.29 These two approaches did, however, represent different emphases, and were to give rise in the 20th century to two very different strains in philosophy of science (Carnap and Keynes being the descendants of the progress by probabilification school, and Popper and Reichenbach focussing primarily on progress by self-correction).

A third theory of scientific progress prominent in the 19th century was that endorsed by Mill and Bain. Mill adopts a theory of progress by elimination. An hypothesis is entertained, tested, refuted and replaced by another one. This perhaps seems but another version of the standard method of hypothesis. But it receives an interpretation by Mill very different from that of the self-correctionists. Mill does not believe we have any good ground for believing that a replaced hypothesis is any more true than its refuted predecessor. Indeed, it may be "more false". But the sequence of hypotheses is a progressive one, according to Mill, because the last remaining member of the series is true. Adhering to a principle of limited variety, Mill maintained that there was only a finite number of candidates for the status of a scientific law and the false contenders could be eliminated by a judicious use of the five canons of induction. Clearly, Mill's account of scientific progress differs substantially from that of both the self-correctionists and the probabilificationists.

All three of these theories of scientific progress found their followers in the second half of the 19th century. Nonetheless, the self-correctionists predominate, and it is late 19th-century developments in the self-corrective tradition which I want to examine now.
II

As we have seen, for more than a century after Hartley and LeSage, methodologists almost to a man (Mill being the most noteworthy exception) endorsed SCT and, ignoring the doubts voiced by Senebier and Prevost, assumed without much argument that the methods of science in general, and the method of hypothesis in particular, were genuinely self-corrective. The discussion of this question was given an entirely new slant, however, by the work of Charles Sanders Peirce, whose approach to this question I wish to discuss in some detail.

It is well known that Peirce was a persistent defender of SCT. Unlike most of his predecessors, however, Peirce (usually) realized that SCT was not self-evidently true, and felt that one of the tasks of the logician of science was to show how and why science is a self-corrective enterprise which, in its historical development, gradually but inexorably comes closer and closer to a true representation of natural phenomena.  

Peirce's most crucial claim in this regard is his insistence that all scientific inquiry is self-corrective in nature. "This marvelous self-correcting property of Reason", he wrote, "belongs to every sort of science . . ." and every branch of scientific inquiry exhibits "the vital power of self-correction". The reason the sciences are self-corrective is that (in Peirce's view) they utilize methods which are self-corrective. It was thus incumbent on Peirce to show that all the methods of science exhibit self-correction and thereby guarantee progress towards the truth. Those methods for Peirce are threefold: deduction, induction and abduction.

It is at this point that the first of Peirce's serious problem slides occurs. Although he was presumably obliged to show that all three methods of science are self-corrective, he ignores deduction and less excusably, abduction, and limits his discussion almost entirely to induction. There is, nonetheless, a certain rationale for this since, in Peirce's view, inductive methods are operative in every appraisal we make of a theory. So long as the inductive step is self-corrective, any failure of self-correction in deduction and abduction may be ameliorated. Thus, Peirce's problem is changed from that of showing that scientific methods generally are SCMs, to demonstrating that the various methods of induction are self-corrective. The "induction" in question refers to the entire machinery for the testing of a scientific hypothesis. Although the precise significance of the term "induction" undergoes several notorious shifts in his long career, this very general sense of the term is a persistent feature of almost all his discussions of the question.
Thus, in about 1901, Peirce wrote that “the operation of testing a hypothesis by experiment . . . I call induction”\(^{33}\). In 1903 he virtually repeated this definition,\(^{34}\) and in his later, important essay on “The Varieties and Validity of Induction” (c. 1905) he made substantially the same point:

The only sound procedure for induction, whose business consists in testing hypotheses . . . is to receive its suggestions from the hypothesis first, to take up the predictions of experience which it conditionally makes, and then try the experiment . . . When we get to the inductive stage what we are about is finding out how much like the truth our hypothesis is . . . .\(^{35}\)

Peirce asserts on a number of occasions that induction conceived in this broad sense is self-corrective in nature. As early as 1883, he observed that: “We [must not] lose sight of the constant tendency of the inductive method to correct itself. This is of its essence, this is the marvel of it.”\(^{36}\) He reiterated this point twenty years later: “[Induction] is a method of reaching conclusions which, if it be persisted in long enough, will assuredly correct any error concerning future experience into which it may lead us.”\(^{37}\) Between these two temporal extremes, Peirce regularly returns to SCT. About 1896 for instance, he remarked that “Induction is that mode of reasoning which adopts a conclusion as approximative [i.e., approximately true], because it results from a method of inference which must generally lead to the truth in the long run.”\(^{38}\) And two years later he smugly claimed that the fact “that induction tends to correct itself, is obvious enough”.\(^{39}\) To this point, the Peircean texts I have cited could have been written by LeSage, Hartley, Whewell or any of a dozen other methodologists living in the century before Peirce.

What Peirce usually perceived, which his predecessors had not, was that it was not all that obvious that induction, defined as the testing of an hypothesis, is, or tends to be, self-correcting. He saw this as a genuine problem and one which he attempted to resolve on several occasions, most notably in the Lowell Lectures of 1903, and in the famous manuscript “G” (c. 1905). In his classic essay of 1903, Peirce distinguished three varieties of induction: crude induction, qualitative induction, and quantitative induction. Crude induction is concerned with universal (as opposed to statistical) hypotheses, the evidential base for which is flimsy and precarious in that they are merely empirical generalizations of the type “all swans are white” or “all Germans drink beer”. What typifies crude inductions is not so much the logical form of their conclusions as the nature of the evidential base on which they rest. The only license required for making a crude induction of the form “All A are B” is “the absence of [any known] instances to the contrary”.\(^{40}\) Such inductions
may be indispensable to daily life but, on Peirce’s view, they play no significant role in science. *Quantitative* induction, on the other hand, is an argument from the observed distribution of certain properties in a sample to an hypothesis about the relative distribution of those properties in a larger population. Quantitative induction is induction by simple enumeration in its most literal sense. The conclusion of a quantitative induction is always a statement concerning the probability “that an individual member of a certain experiential class, say the S’s, will have a certain character, say that of being P”.

Unlike crude induction, quantitative induction is (according to Peirce) used in the sciences, if only to a limited extent.

“Of a more general utility” is the remaining variety of induction, *qualitative* induction. This corresponds, more or less, to what is usually called the hypothetico-deductive method. Here, the scientist formulates an hypothesis, deduces predictions from it, and performs experiments to check the predictions. If all of the tested predictions are confirmed, this hypothesis should be tentatively adopted; while if any of the predictions are refuted, the scientist modifies the hypothesis, or abandons it and tries another.

Peirce then argues that one of these species of induction, namely the quantitative variant, is genuinely and demonstrably self-corrective. “Quantitative induction”, he insists, “always makes a gradual approach to the truth, though not a uniform approach”. Peirce’s argument for the self-correcting character of quantitative induction is a crude version of the arguments advanced more recently by Reichenbach and Salmon. Provided that our sampling procedures are fair and that our long run is long enough, the estimates which quantitative inductions lead us to posit will in time approximate ever more closely to the true value. (In developing this argument, Peirce tells us that he was impressed, as Hartley and LeSage had been 150 years earlier, by the fact that “certain methods of mathematical computation correct themselves”.)

Ignoring the familiar technical difficulties with this argument, let us concede that Peirce came close to showing that quantitative inductions are self-corrective. At all events, quantitative induction does satisfy two conditions for a self-correcting method; namely, it is a method which not only allows for the refutation of an hypothesis but which also mechanically specifies a technique for finding a replacement for the refuted hypothesis (provided, and it is a crucial proviso, that the hypothesis is taken as a probability statement).

But what of that scientifically more significant species of induction, the method of hypothesis? Such qualitative inductions clearly satisfy the first condition for an SCM, insofar as persistent application of the method of
hypothesis will eventually reveal that a false hypothesis is, in fact, false. But the method of qualitative inductions provides no machinery whatever for satisfying the second necessary condition for an SCM; given that an hypothesis has been refuted, qualitative induction specifies no technique for generating an alternative which is (or is likely to be) closer to the truth than the refuted hypothesis. Nor does it even provide a criterion for determining whether an alternative is closer to the truth. Peirce, in short, gives no persuasive arguments to establish that qualitative induction is either strongly or weakly self-corrective.48

At a certain level of consciousness, Peirce was fully aware of the fact that he had not shown qualitative inductions to be self-corrective. He remarks that while quantitative induction “always makes a gradual approach to the truth . . . qualitative induction is not so elastic. Usually either this kind of induction confirms the hypothesis or else the facts show that some alteration must be made in the hypothesis.”49 What the facts do not show, of course, is how the hypothesis is to be altered so as to bring it closer to the truth. While “the results of [qualitative] induction may help to suggest a better hypothesis”, there is no guarantee they will yield a better one.50

In one especially candid lecture (1898) on the “Methods for Attaining Truth”, Peirce confesses that in “the Explanatory Sciences”, we have no sure way of knowing whether the outcome of any confrontation between competing theories is “logical or just”.51 Peirce had evidently landed himself in a situation in which he is pursuing a rapidly degenerating problem. Where before he had answered the question “Are the methods of science self-corrective?” by replying that at least all the inductive methods of science are self-corrective, he is here reduced to saying that even of the various methods of induction, only one is known to be genuinely self-corrective.

Peirce must have sensed the awkwardness of the position in which he found himself. Having set out to show that science is a progressive, self-corrective enterprise, moving ever closer to the truth — and there can be no doubt that this was his initial problem, since both the tradition he was in and his early writings make this clear — Peirce finds himself able to show only that one of the methods of science (and that, by Peirce’s admission, a relatively insignificant one) was self-corrective.

I cannot stress too strongly how important it is to be clear about Peirce’s intentions. Virtually all Peirce’s recent commentators have seen him as setting out to answer Hume’s doubts about induction; and have, accordingly, discussed his accounts of SCT and enumerative induction as if they were intended only or primarily as an answer to Hume. Unless my analysis is
completely wrong-headed, this is to judge Peirce by an inappropriate yardstick. It was not enumerative induction, but science which Peirce set out to justify; it was not Hume but the cynical critics of science whom Peirce set out to answer. (I might generalize this point by adding, parenthetically, that it is one of the wilder travesties of our age that we have allowed the myth to develop that 19th-century philosophers of science were as preoccupied with Hume as we are. As far as I have been able to determine, none of the classic figures of 19th-century methodology — neither Comte, Herschel, Whewell, Bernard, Mill, Jevons, nor Peirce — regarded Hume's arguments about induction as much more than the musings of an historian. This claim is borne out by the fact that in Peirce's thirty-two papers on induction and scientific method — papers teeming with historical references — there is only one reference to Hume; and that is not in connection with the problem of induction but with the problem of miracles.) 52

As it turned out, Peirce attempted to bridge the gap between intention and performance by a combination of bluster and repetition. Just as LeSage and Hartley could, a century earlier, gloss over their failure to demonstrate an analogy between approximative techniques in mathematics and the methods of science, so Peirce conveniently ignores his painstaking discrimination between the various forms of induction, and pretends (as the quotations above make clear) that his argument has established that all forms of induction (and, by implication, all scientific inferences) are SCMs. In his later writings, 53 he will generally assert that qualitative inductions (or, as he sometimes calls them, 'Inductions of the Second Order') are progressive and self-corrective; but he never goes further than asserting that such methods are SCMs, without even the pretense of an argument for that assertion.

Lenz has charitably said that Peirce's "remarks on the self-correcting nature of the broader form[s] of induction are extremely hard to comprehend". 54 I think we must lay a more serious charge at Peirce's feet than that of obscurity. Peirce's remarks in themselves are not difficult to comprehend; he says quite plainly that all forms of induction are self-corrective. What is hard to comprehend is Peirce's reason for making such a general assertion. And I think it would be less than candid not to say that Peirce offers no cogent reasons, not even mildly convincing ones, for believing that most inductive methods are self-corrective. I suspect that the explanation of this glaring oversight may be found by recalling Peirce's original motivation. 55

Peirce began, as I claimed before, with a very general and a very interesting problem: that of justifying scientific inference by showing that the methods of science (including all species of induction) are self-corrective. This was, as
I have shown, one of the standard problems of philosophy of science by Peirce's time. Unable to find a general solution to that problem, Peirce tackles the more limited task of showing that one family of inductive arguments, quantitative inductions, are self-corrective. Having shown, at least to his own satisfaction, that quantitative induction is self-corrective, Peirce then, without even the hint of a compelling argument, makes the crucially serious slide. Seemingly unwilling to admit, even to himself, that he has failed in his original intention to establish SCT for all the methods of science, Peirce acts as if his arguments about quantitative induction show all the other species of induction to be self-corrective as well.

His dilemma was genuine. Having discovered that he could show only quantitative induction to be self-corrective, he could have gone the way of Reichenbach and argued that quantitative induction was the only species of scientific inference, to which all other legitimate methods could be reduced. But Peirce did not share Reichenbach's belief that complex inference was a composite of simple inductions by enumeration. Alternatively, he could have abandoned SCT altogether, conceding that science uses methods which are not, so far as we know, self-corrective. But that would have meant taking much of the flesh out of his philosophy of science. Faced with two such debilitating alternatives, Peirce conveniently ignored the restricted scope of his argument and (perhaps unconsciously) slid from the self-corrective character of the straight rule to SCT as a general thesis. The extent to which Peirce was prepared to make this leap is illustrated by such remarks as his claim that "inquiry of every type, fully carried out, has the vital power of self-correction and growth".56

At one point, his bedrock commitment to SCT, even in the absence of any methodological rationale for it, becomes clear:

It is certain that the only hope of retroductive reasoning [viz., qualitative induction] ever reaching the truth is that there may be some natural tendency toward an agreement between the ideas which suggest themselves to the human mind and those which are concerned in the laws of nature.57

Unable to find a rational justification for his intuition that science is self-corrective, the otherwise tough-minded Peirce had to fall back on Galileo's *il lume naturale*, on an inarticulate faith in the ability of the mind somehow to ferret out the truth, or a reasonable facsimile thereof:

We shall do better to abandon the whole attempt to learn the truth ... unless we can trust to the human mind's having such a power of guessing right that before many hypotheses shall have been tried, intelligent guessing may be expected to lead us to the one which will support all test ...58
A similar belief was shared by Peirce's contemporary Pierre Duhem, who argued for SCT in terms of an approach to "the natural classification". In a more explicit manner than Peirce, Duhem concedes that he can produce no logically compelling grounds for believing that the history of science brings us closer and closer to a genuine representation of natural relations. Nonetheless, he is convinced that this occurs and that every scientist knows that SCT is true:

Thus, physical theory never gives us the explanation of experimental laws . . . but the more complete it becomes the more we apprehend that the logical order in which theory orders experimental laws is the reflection of an ontological order, the more we suspect that the relations it establishes among the data of observation correspond to real relations among things . . . . The physicist cannot take account of this conviction . . . . But while the physicist is powerless to rid his reason of it . . . yielding to an intuition which Pascal would have recognized as one of those reasons of the heart "that reason does not know", he asserts his faith in a real order reflected in his theories more clearly and more faithfully as time goes on. 59

Less optimistic than Peirce about the possibility of finding a methodological rationale for the view that science moves ever closer to the truth, Duhem maintains that the methodologist cannot justify SCT, and that its only defense lay in what Duhem calls a "metaphysical assertion". 60

To return to Peirce only briefly, I suspect that there is another important sense in which he takes much of the force out of the SCT tradition. As I have tried to make clear, that tradition was committed to the view (among others) that the replacement of one non-statistical hypothesis by another was the basic unit of progress and self-correction. Peirce, at least on some occasions, abandons that view altogether. In its place, he argues that, although we have no way of correcting our hypotheses, what we can correct are the assignments of probability which we give to those hypotheses. When arguing in this vein, Peirce sees the process of assigning probabilities to hypotheses as self-corrective, while the process of replacing one hypothesis by another no longer remains even a candidate for consideration as a self-corrective process. In the course of time, it is not our theories which get closer to the truth, but rather, the probabilities which we assign to theories exhibit progress and self-correction. Where all previous discussions of the question had been concerned to show that a sequence of hypotheses of the form:

\[
A \text{ is } B, \\
A \text{ is } C, \\
A \text{ is } D, \\
etc.,
\]
is progressive and self-corrective, Peirce’s quantitative induction goes for the “cheapest” form of self-correction, arguing that a sequence of the following kind:

\[
\begin{align*}
\text{The probability that } A & \text{ is } B \text{ is } m/n, \\
\text{The probability that } A & \text{ is } B' \text{ is } m'/n', \\
\text{The probability that } A & \text{ is } B'' \text{ is } m''/n'',
\end{align*}
\]

etc.,

is (or can be) self-corrective. Peirce simply cannot handle a case where an hypothesis (of the form “A is B”) is replaced by a conceptually different one (say “A is C”).

It would not be appropriate in this volume to discuss at length the views of more recent methodologists about SCT, since that would take us well into the 20th century. Nonetheless, I think a few words are in order about more recent developments in so far as they link up rather closely to the tradition I have been discussing here. As everyone knows, Hans Reichenbach took up SCT, most notable in his *Wahrscheinlichkeitslehre* and his *Experience and Prediction*. In both works, Reichenbach, like Peirce, set out to show that the straight rule is self-corrective, that induction by simple enumeration is a SCM. Like Peirce, Reichenbach then goes on to assume, with only the flimsiest of arguments, that science is a self-correcting enterprise because (and here Reichenbach differs from Peirce) all the methods of science can be reduced to enumerative induction. Unfortunately, however, Reichenbach’s attempts to reduce most scientific methods to convoluted species of the straight rule are at best, programmatic; at worst, unconvincing. As a result Reichenbach, like Peirce, found himself unable to prove SCT generally, and was forced to be content with the comparatively insignificant consolation that enumerative induction is self-corrective.

All the same, it must be said on Reichenbach’s behalf that he takes up the banner of the SCT tradition in a less half-hearted way than Peirce had. Reichenbach sensed the object of the exercise, and understood that exploration of the self-correcting properties of the straight rule was only of crucial import in so far as one could establish the relevance of the straight rule to more subtle forms of scientific reasoning. That Reichenbach’s program did not come off, that he never quite managed to achieve the reduction of scientific methods to enumerative induction, does not diminish the soundness of his intuitions as to the nature of the problem.

In the last two decades, however, there seems to have been a tendency to return to a Peircean rather than a Reichenbachian treatment of the question.
Many contemporary philosophers of science, perhaps forgetting that self-correction was originally a thesis about science rather than a putative answer to Hume, have explored at length the question whether the method of enumerative induction is self-corrective without seriously considering whether the methods of science are enumerative. Reichenbach’s most distinguished disciple, Wesley Salmon, similarly skirts this particular issue on many occasions. One has the impression (perhaps unjustifiably) that such philosophers have become so involved with the technical and formal aspects of Peirce’s solution that they have lost sight of the problem to which it was a solution. We are, I suspect, sometimes repeating Peirce’s mistake of thinking that so long as we establish that any ampliative inference is self-corrective, we can easily show that most of them are.

Criticisms of the type I have offered here, however well intentioned, are always open to the charge of being premature and philistine. After all, one might say, a break-through could come at any moment and in that event the work of Reichenbach and Salmon might become to the foundations of scientific inference what Russell, Frege or Cantor were to the foundations of mathematics. Moreover, it might be pointed out that foundational studies, especially in their preliminary stages, always have only tenuous connections to what they purport to be foundations of. But, granting all that, one has a right to insist that putative “foundational studies” must satisfy some canons of adequacy, and be subject to certain standards of criticism.

Precisely what these standards are I do not pretend to know. (This in itself is a major philosophical problem.) But there are several seemingly relevant points to make about the so-called pragmatic justification of induction and scientific inference. The first point is that distinguished philosophers have been exploring this approach for almost a century. In that time, they are no closer to exhibiting a connection between the straight rule and other modes of inference than Peirce was in 1872. While promissory notes are not dated, there is a presumption that payments will be made at respectable intervals. Secondly, and more disturbingly, the ‘pragmatic’ approach has, at least since the 1930s, tended to concern itself less and less with the one thesis which originally made that approach interesting, viz., the thesis that scientific inference could be reduced to enumerative inference. The centrality of that thesis in the pragmatic tradition has been replaced by a preoccupation with enumerative induction itself. In an unnoticed sleight of hand, the problem of the justification of science has been displaced by the problem of justifying induction. And, in the absence of any established link between the former and the latter, this portion of the
"philosophy of science" has surrendered any convincing claims to being the philosophy of science.

If we believe, with Peirce, LeSage, Hartley, Whewell, and Duhem that science is a self-corrective, progressive enterprise, then we should presumably be seeking to show how and why it is so. If we further believe with Peirce and Reichenbach that the exploration of enumerative induction will provide the answer, then we ought to be exploring more assiduously the role of enumerative induction in real science. What we must avoid is falling into the Peircean pit by assuming without argument that the grand old problem of the progress of science is apt to be clarified by technical investigations of the straight rule. We have accepted Peirce's *ersatz* self-correction — a self-correction which only can deal with changes in probabilities rather than changes in theories — without openly discussing whether full-bodied self-correction in the traditional sense is beyond our powers of explication. It is the self-correcting nature of science, not the self-corrective nature of a "puerile" rule, which should be our main concern.65

NOTES

* Since this chapter first appeared, it has been discussed at length by Nicholas Rescher and Iikka Niiniluoto. It should be evaluated in the light of the constructive criticisms they have made of its central thesis.

1 *Mid-West Quarterly* 2 (1914), 49.


3 See, for instance, Burks, 'Peirce's Theory'. Even Peirce himself tries to give the impression that he was the first to enunciate the view that scientific reasoning is self-corrective. For instance, he wrote in 1893 that "you will search in vain for any mention in any book I can think of" of the view "that reasoning tends to correct itself". C. S. Peirce, *Collected Papers*, ed. Hartshorne, Weiss et al., 8 vols. (Cambridge: Harvard University Press, 1931–58) Vol. 5, p. 579. Without questioning Peirce's integrity, we do have some grounds for doubting his memory. Peirce makes numerous references to the works of many of the writers whom I cite below as Peirce's predecessors in this matter. (See, for
example, ibid., Vol. 5, p. 276 n., where he writes knowledgeably of the philosophies of science of both LeSage and Hartley, who had stressed the self-correcting aspects of scientific reasoning.)

4 This point requires some qualification. As is well known, passages can be adduced from all these authors where they seem to abandon the infallibilism of TICT and to replace it by a more modest "probabilism". (Many of the relevant texts are discussed in Chapter 4.) However, it would be a serious error of judgment to let these concessions to fallibilism obscure the fact that all of these figures shared the classical view that science at its best is demonstrated knowledge from true principles. Bacon, Descartes, Locke, and Boyle all see it as a goal that science become infallible; until that goal is realized they are willing to settle — but only temporarily — for merely probable belief. Their long-term aim, however, is to replace such mere opinion by genuine knowledge.


6 See Chapter 4.

7 A century and a half later Max Planck gave eloquent expression to this quintessentially 18th-century viewpoint: "Nicht der Besitz der Wahrheit, sondern dass erfolgreiche Suchen nach ihr befruchtet und beglückte den Froscher". (Wege zur physikalischen Erkenntnis, 4th ed. [Leipzig, 1944], p. 208.)

8 Some, but by no means all. As late as the 1790s, philosophers such as Thomas Reid were still arguing for a strictly inductive methodology. (Cf. Chapter 7.)

9 To be faithful to the historical situation, it is important to point out that some 18th- and early 19th-century methodologists, while accepting SCT as a general thesis, were not altogether happy with the idea expressed in (2) above. As formulated there, SCT is committed to the view that there is a mechanical process for finding alternatives. Some methodologists denied this. What they did insist on, however, was that: (2') Science possess techniques for determining unambiguously whether an alternative $T'$ is closer to the truth than a refuted $T$. William Whewell, for instance, denied the claim implicit in (2) that the scientist possessed any algorithm for automatically correcting an hypothesis. Nonetheless, he was convinced that it was generally possible, given a (refuted) theory and an alternative to it, to determine which of the two was (in Whewell's language) "nearer to the truth". Hereafter, I shall refer to the pair (1) and (2) as the strong self-correction thesis (or SSCT) and to the pair (1) and (2') as the weak thesis of self-correction (WSCT).

There is another important qualification to make here. Although all the figures I discuss talk about "getting closer to the truth", "moving nearer to the truth", etc., it is not altogether clear that there is a shared conception of what truth consists in. With some writers, for instance, the notion of truth seems to be an instrumental one (viz., that is true which adequately "saves the phenomena"); with others, the concept of truth is a correspondence one. Nonetheless, most discussions of self-correction and proximity to the truth seem to be conducted independently of various conceptions of, and criteria for, the truth.

10 See note 9 above.

11 This claim for the priority of LeSage and Hartley is, like all claims for historical priority, necessarily tentative. R. V. Sampson, in his Progress in the Age of Reason (London, 1956), asserts that Blaise Pascal conceived of science as "cumulative, self-corrective" and progressive. I have been unable to find such an argument in Pascal and
(unfortunately) Sampson offers no evidence for his interpretation. Similarly, Charles Frankel (The Faith of Reason, [New York, 1948]) argues likewise without evidence, that “For Pascal ... scientific method was progressive because it was public, cumulative, and self-corrective” (p. 35). Until more substantive evidence is produced, I believe the available historical evidence supports my priority claims for Hartley and leSage. However, the argument in the body of the essay does not depend on the priority issue.

13 Ibid., I: 345–6. Basically, the rule of false position worked as follows: If one sought the solution to an equation of the form \( ax + b = 0 \), one made a conjecture, \( m \), as to the value of \( x \). The result, \( n \), of substituting \( m \) for \( x \) in the left-hand side of the equation is given by \( am + b \). The correct value of \( x \) was then determined by the formula

\[
x = \frac{mb}{b-n}
\]

The rule of false position was one of the earliest known rules for the solution of simple equations.

It should be added that during the 18th century, the term “rule of false position” normally referred, not specifically to the rule given above, but rather to what we call the rule of double position, which involves two conjectures rather than one. An interesting discussion of this latter rule may be found in Robert Hooke’s Philosophical Experiments and Observations, ed. Durham (London, 1726), pp. 84–6.

15 Observations on Man, n12, 1: 349.
16 Ibid., 1: 349. It is perhaps worth observing that Hartley adhered to a theory of moral progress and self-improvement which paralleled the progress and self-correction of science. “We have”, he writes, “a Power of suiting our Frame of Mind to our Circumstances, of correcting what is amiss, and improving what is right” (Ibid., 1: 463).
17 Ibid., 1: 349. There is, we should observe, a very great difference in the results which these various “approximative methods” yield. Some of these methods – such as the Newtonian method of approximation to the roots of a general equation – do not necessarily ever yield a true result. We can, by their use, constantly improve our estimate, but there is no guarantee that we will ever determine precisely the correct answer. However, other methods Hartley mentions, especially the rule of false position, not only correct a false guess, but immediately replace it by the correct solution. These differences become very significant when applied to a scientific context. If our model for scientific method is the rule of false position, then one can imagine science rapidly reaching a stage where all the false theories have been replaced by true ones, and where scientific knowledge would be both static and non-conjectural. If, on the other hand, our model for inquiry is the search by approximation for the roots of an equation, then science would seem to be perhaps perennially in a state of change and flux, with no guarantee whatever that it could ever reach the final truth.

Hartley, as well as most of his 19th-century successors, seems to vacillate between these two very different models.
18 Talk of a ‘self-correcting method’ is, of course, slightly misleading since the method does not correct itself, but rather it allegedly corrects those statements which an earlier application of the method produced. However, since linguistic traditions sanctify all manner of confusions, and since it is de rigueur to speak of methods with these properties as self-corrective methods, I will do so, hoping the reader will bear this caveat in mind.
A method will be weakly self-corrective (WSCM) if (a) above and if (b) without itself specifying a ‘truer’ alternative, it can determine for certain whether a given alternative is truer. (See also Note 6 above).

Precisely this criticism was raised by Condillac in 1749 against the view that science can borrow the approximative methods of the mathematician. (Cf. his *Traité des Systèmes* [Paris, 1749], pp. 329–31.) It was also raised by J. Senebier a generation later. (Cf. his *Essai sur l'Art d'Observer et de Faire des Expériences* [Gêneve, 1802], 2: 215–6.)

As LeSage puts it: “The corrections made of these particular suppositions, resulting from the small multiplications which serve to test their validity, have as their sole aim to bring closer together these suppositions and the [true] number; with the exception of the last partial division, which must be performed rigorously because it is here that one finally rejects the inaccurecies one has permitted oneself in the previous operations.” G. H. leSage, ‘Quelques Opuscules relatifs a la Méthode’, posthumously published by Pierre Prevost in his *Essais de Philosophie* (Paris, 1804), 2: 253–35. The passage in question dates from the 1750s, and appears on p. 261. (I discuss LeSage’s work at much greater length in Chapter 8.)


“Souvent on s’écarte du vrai, sans douter, et on le fuit en croyant le poursuivre” (*Essai*, 2: 220).

Prevost, *Essais de Philosophie* (Paris, 1804), 2: 196. Prevost nevertheless believes that there are self-corrective methods which the scientist can use.

See, for instance, the several essays on progress in Whewell’s *Philosophy of Discovery* (London, 1860) and Auguste Comte’s preliminary discourse to the *System of Positive Polity* (4 vols. [London, 1875–7]). Similar, if more vague, sentiments are involved in John Herschel’s discussion (*Preliminary Discourse on the Study of Natural Philosophy* [London, 1831], para. 224 ff.).


My labels are, of course, anachronistic. The concepts they denote are not.

For Peirce’s application of SCT to the history of science, see his *Lessons from the History of Science*, (c. 1896), in *Collected Papers*, 1: 19–49, especially para. 108, p. 44.
must in the long run approximate to the truth" (2: 780). “persistently applied to the problem [induction] must produce a convergence (through irregular) to the truth” (2: 775). “the method of induction must generally approximate to the truth” (6: 100). 1903: “The justification of [induction] is that, although the conclusion at any stage of the investigation may be more or less erroneous, yet the further application of the same method must correct the error” (5: 145). “Suppose we define Inductive reasoning as that reasoning whose conclusion is justified . . . by its being the result of a method which if steadfastly persisted in must bring the reasoner to the truth of the matter or must cause his conclusion in its changes to converge to the truth as its limit” (7: 110). “. . . if this mode of reasoning [viz., induction] leads us away from the truth, yet steadily pursued, it will lead to the truth at last” (7: 111). See also Collected Papers, 2: 709.

40 Ibid., 2: 756.
41 Ibid., 2: 758.
42 Ibid., 2: 77 ff.
43 Ibid., 2: 770.
44 Provided, of course, that there is some limit to the sequence in question; a qualification which Peirce realized to be essential.
45 Ibid., 5: 574. Peirce’s example, that of the extraction of roots, is identical to Hartley’s and LeSage’s. It is perhaps appropriate to add here that Peirce knew Hartley’s Observations on Man first-hand, and makes numerous references to it in his Collected Papers. Moreover, he knew of LeSage’s work, at least second-hand, citing it in volume 5 of his Collected Papers.

I know too little about Peirce’s intellectual biography to assert with any confidence that it was definitely Hartley and LeSage who gave him the idea of a SCM; but, given Peirce’s knowledge of Hartley and the obvious similarities in the initial approaches to the problem, it seems a reasonable conjecture that Hartley may have stirred Peirce to consider the question of self-correction in detail.

46 For references to the vast body of technical literature on the straight rule, cf. the bibliography in Salmon’s The Foundations of Scientific Inference (Pittsburgh, 1967).
47 Whether that replacement is closer to the truth than that which it replaces, is, of course, another matter. But at least quantitative induction can specify a replacement, and is thus (potentially) a strong self-corrective method.
48 This point, viz., that qualitative induction is not (or, at least, has not been shown to be) self-corrective, has gone unnoticed by several of Peirce’s commentators. For instance, Cheng writes: “To say that a qualitative induction is self-correcting is either to say that a given hypothesis is replaceable by a new hypothesis or that the scope of the given hypothesis is modifiable or limitable . . . ” (Peirce’s and Lewis’s Theories, p. 73).

In arguing this point, Cheng has used an unfortunate sense of ‘self-correcting’. That an hypothesis is replaceable or ‘modifiable’ merely means that we have techniques for discarding or altering it. If qualitative induction is to be self-correcting then we need, at a minimum, the further assurance that its replacement or altered expression is an improvement. This assurance Peirce nowhere provides, and on occasion even denies that we can obtain it.

49 Collected Papers, 2: 771.
50 Ibid., 2: 759.
51 Ibid., 5: 578.
52 The titles of these 32 papers are listed in appendix I to Cheng’s Peirce’s and Lewis’s
Theories of Induction. Ironically, Cheng himself discusses Peirce’s work as if it were designed explicitly as a reply to Hume.

55 One could schematically survey the major changes in SCT by looking at three formulations, the first, typically 18th-century, the second, 19th-century, and the third, Peirce’s:

SCT\textsubscript{1}: The methods of science are such that, given a refuted hypothesis $H$, a mechanical procedure exists for generating a ‘truer’ $H'$ . . . . Science is progressive (i.e., getting closer to the truth).

SCT\textsubscript{2}: The methods of science are such that, given a refuted hypothesis $H$, we can always determine whether an alternative $H'$ is ‘truer’ . . . . Science is progressive.

SCT\textsubscript{3}: The method of enumerative induction is such that, given a refuted $H$ (and the available evidence) we can mechanically produce an alternative $H'$ which is likely to be truer than $H$ . . . . Science is progressive.

The sequence SCT\textsubscript{1}—SCT\textsubscript{2}—SCT\textsubscript{3} is one in which the premises become increasingly precise and defensible; but the price paid is that the premises seem to lend less and less inferential support to the conclusion.

56 Collected Papers, 5: 582.
57 Ibid., 1: 81. Peirce insists “that it is a primary hypothesis . . . that the human mind is akin to the truth in the sense that in a finite number of guesses it will light upon the correct hypothesis” (Ibid., 7: 220).
58 Ibid., 6: 531. Cf. also 1: 121.
60 Ibid., p. 297. Duhem summarizes his position when he observes: “To the extent that physical theory makes progress, it becomes more and more similar to a natural classification which is its ideal end. Physical theory is powerless to prove this assertion is warranted, but if it were not, the tendency which directs the development of physics would remain incomprehensible. Thus, in order to find the title to establish its legitimacy [as an SCM], physical theory has to demand it of metaphysics” (Ibid., p. 298).
61 My suspicion is that this ‘cheap’ form of inductive self-correction has its origins in Laplace’s rule of succession, and the discussions that rule engendered in 19th-century probability theory.
62 Reichenbach writes: “The method of scientific inquiry may be considered as a concatenation of [enumerative] inductive inference . . . ” (Experience and Prediction, [Chicago, 1938], p. 364).
63 Cf. G. H. von Wright, The Logical Problem of Induction, 2nd ed. (Oxford, 1965), chap. vii. It is, however, to von Wright’s credit that he, almost alone among Peirce’s commentators, perceives the limited scope of Peirce’s treatment of self-correction. As he puts the point: “the Peircean idea of induction as a self-correcting approximation to the truth has no immediate significance . . . for other types of inductive reasoning than statistical generalization” (Ibid., p. 226).

A very different formulation of SCT has been developed by Karl Popper in his *Conjectures and Refutations* (London, 1963). Popper's approach, unlike that of Peirce, Reichenbach and Salmon, does not attempt to make enumerative induction the cornerstone of scientific inference. It depends, rather, upon showing (unsuccessfully, I believe) that the method of hypothesis is weakly self-corrective in virtue of methodological conventions about increases in content. Popper is perhaps alone among contemporary philosophers of science in facing the issues raised by SCT in their full generality. As inadequate as his discussion of verisimilitude is, he has sensed the magnitude of the problem. In this, as in other ways, Popper is probably closer to the 19th-century methodological tradition than is any other living philosopher.