leagues at Notre Dame—faculty and students alike—for making my visit there a thoroughly pleasant experience.

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1 Peirce's Theory of the Self-correctiveness of Science

1. Peirce's Position and the Critical Onslaught upon It

Everyone realizes that we must turn to science to understand the ways of the world; to describe, explain, and predict the phenomena of nature. But how do we know that science is really efficacious and that the scientific method of inquiry is successful in getting at the real truth of things?

For Peirce, the inductive method used in the sciences leads inevitably to truth; its justification lies in being self-corrective. It has the capacity to yield the correct result in the long run, whatever transitory errors and missteps may occur along the way. Peirce saw self-correction as the definitive characteristic of induction, which in its very nature "is a method of reaching conclusions which, if persisted in long enough, will assuredly correct any error concerning future experience into which it may temporarily lead us."[1] Peirce put the matter forcefully in his essay, "The Logic of Drawing History from Ancient Documents":

> Induction . . . is not justified by any relation between the facts stated in the premisses and the fact stated in the conclusion; and it does not infer that the latter fact is either necessary or objectively probable. But the justification of its conclusion is that that conclusion is reached by *a method which, steadily persisted in, must lead to true knowledge in the long run of cases of its application, whether to the existing world or to any*

> *imaginable world whatsoever*. (CP, 7.207 [1901]; italics added)[2]

The operational adequacy of our inductive reasonings can be checked, and indeed improved, by inductive means. Induction is thus self-monitoring. It has the characteristic, crucial to its justification as a rational resource, that its continued use will in the long run uncover any mistakes to which its previous use may have led. Its nature is such that science, if persistent in its use, is predestined to reach the truth eventually. And thus, science—not science in our own day, but ultimate-long-range-science—is infallible.[3]

No part of Peirce's philosophy of science has been more severely criticized, even by his most sympathetic commentators, than this attempted validation of inductive methodology on the basis of its purported self-correctiveness. Even dedicated Peirceans, let along his critics, incline to meet his claims in this area with a mixture of incredulity and dismay. This book, however, will endeavor to rehabilitate this aspect of Peirce's theory of scientific method, arguing that his views on the inductive corrigibility of our inductive practices are both coherent and cogent.

Let us begin at the beginning and consider the structure of Peirce's theory of induction. In his Lowell Lectures of 1903, and in many other places, Peirce distinguished three modes of inductive reasoning: *crude* (or *rudimentary*) induction, *qualitative* induction, and *quantitative* (or *statistical*) induction.[4]

Crude induction is concerned with the projection of universal claims on the basis of a uniform experience of conforming instances. It relies on "the *absence* of [any encountered] instances to the contrary."[5] The merely empirical generalizations of everyday life—"All swans are white," "Thunder is always preceded by lightning"—are illustrations. Such generalizations simply dismiss the prospect that familiar things might fall outside familiar patterns. This crude sort of induction will not concern us much here; Peirce rightly views it as a primitive instrumentality of everyday life that plays little or no role in scientific inquiry.

Qualitative induction is regarded by Peirce as a powerful instrument of very general utility in inquiry. In its essentials it is simply equivalent to the *hypothetico-deductive method*. Phenomena are observed. A series of explanatory hypotheses—$H_1, H_2, \ldots, H_n$—is imaginatively projected to account for these (by the process of conjectural hypothesis-proliferation that Peirce called *abduction*). These hypotheses are then tested by the familiar process of exploiting them as a basis for predictions, which are then checked against the actual course of developments. The hypothesis that fares best under such trial is tentatively adopted over the alternatives until it is itself overthrown by a further sequence of projection and testing of hypotheses. Peirce gave the name *retroduction* to this process of eliminating hypotheses by experiential/experimental testing. Qualitative-induction is thus the collaborative meshing of abduction and retroduction, of hypothesis conjecture and hypothesis testing.

The third mode of induction, quantitative induction is in effect the methodology of *statistics*, the mathematically guided process of sampling and sample-analysis applied in the context of scientific reasoning. Its mode of operation is largely based on the precept that later writers on inductive inference have called "the straight rule" of induction, the principle that the observed frequency of some target-property in a sample may be taken (if the sampling process is appropriately designed) as an index for its actual frequency in the population at large.[6] In quantitative induction, *observed* frequencies are taken as indicators of *actual* frequencies.

The pivotal feature of the process of quantitative induction is, according to Peirce, that it is *automatically* self-monitoring or self-corrective. If the frequency with which some target-property is distributed over the individuals of a sample does not correspond to its frequency of distribution over the population, the discrepancy is certain to become apparent as the sampling process is extended over the long run. By constantly readjusting the estimate of the population-frequency in light of the actual sample-frequency, the scientist is bound eventually to get things right.

As Peirce sees it, quantitative induction is effective as a matter of abstract mathematical principle, independent of any factual presuppositions or dubious metaphysical assumptions:

> [An] endless series must have some character; and it would be absurd to say that experience has a character which is never manifested. But there is no other way in which the character of that series can manifest itself than while the endless series is still incomplete. Therefore, if the character manifested by the series up to a certain point is not that character which the entire series possesses, still, as the series goes on, it must eventually tend, however irregularly, towards becoming so; and all the rest of the reasoner's life will be a continuation of this inferential process. This inference does not depend upon any assumption that the series will be endless, or that the future will be like the past, or that nature is uniform, nor upon any material assumption whatever. (CP, 2.784 [c. 1905])

Quantitative induction, Peirce maintains, is unfailingly successful in the long run and "always makes a gradual approach to the truth, though not a uniform approach."[7]

The close analogy of Peirce's position here to Hans Reichenbach's well-known pragmatic justification of induction is striking (indeed, it eventually struck Reichenbach himself). Most modern writers on Peirce's theory of induction, influenced no doubt by Reichenbach's spirited defense of a closely similar position, are inclined to concede that Peirce's doctrine of the self-correctiveness of quantitative induction is a theory of, at any rate, some discussable merit. They view it not, to be sure, as decisively established, but at any rate as a controversial position for whose tenability (duly construed and qualified) a reasonable case can be made.[8]

Here the commentators reach a sticking point, however, for Peirce's own explicit and considered position is not merely that *quantitative* induction is self-corrective but that induction as it figures in scientific practice in general is self-corrective. In sum, induction at large is self-corrective. How, the critics ask, can Peirce possibly maintain this self-correctiveness, since scientific induction is preeminently based on *qualitative* induction? On what grounds can Peirce hold that the hypothetico-deductive method in general is self-corrective, even in the case of nonstatistical hypotheses? As G. H. von Wright puts it:

> The Peircean idea of induction as a self-correcting approximation of the truth has no immediate significance . . . for other types of inductive reasoning than statistical generalization [i.e., specifically quantitative induction]. (*The Logical Problem of Induction*, 2nd ed. [Oxford, 1965], p. 226)

Abner Shimony objects as follows:

> [T]he only clear example of an infallible asymptotic approach which he [Peirce] offers is the simple one which is at the heart of Reichenbach's treatment of scientific inference: the evaluation of the limit of relative frequencies in infinite sequences of events (for example 2.650, 6.100, 7.111, 7.120). Since this kind of inference ("statistical" or "quantitative" induction) is only one of the three kinds of induction which he recognizes . . . even sympathetic commentators on Peirce have found that his demonstrations [of the self-correctiveness of science] fall far short of realizing his general program. (Shimony 1970, p. 127)

Laurens Laudan appears to speak for all of Peirce's critics here:

> [Peirce] says quite plainly that all forms of induction are self-corrective. . . . 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 [indeed, all apart from quantitative induction] are self-corrective. . . . Seemingly unwilling to admit, even to himself, that he has failed in his original intention to establish . . . [self-correction] for all the methods of science, Peirce acts as if his argument about quantitative induction shows all the other species of induction to be self-corrective as well. (Laudan 1973, p. 293)[9]

Virtually to a man, Peirce's expositors, even the more sympathetic, are prepared to desert him on this issue.[10]

Such complaints and objections set the stage for the present inquiry, whose focus is indicated by the following seemingly incongruous triad:

1. Peirce maintains that the inductive methodology of science is internally complex and specifically includes not only *quantitative* but also *qualitative* induction.
2. Peirce establishes (with at least some degree of cogency) only the self-correctiveness of quantitative induction.
3. Peirce maintains self-correctiveness to be a crucial and characteristic aspect of scientific methodology in general.

How can Peirce (or a sympathetic Peircean) possibly reconcile these three points? The commentators think that it cannot be done. The ensuing discussion will endeavor to show that a reconciliation is indeed possible.

2. An Interpretative Reconstruction of Peirce's Position

To begin with, let the cards be spread on the table, presenting an interpretative reconstruction of a Peircean position that straightforwardly reconciles the three seemingly discordant theses under consideration. This interpretation is based on a critical distinction. If a productive process P is composed of several constituent subprocesses P_1, P_2, etc., then the idea of self-corrective monitoring of performance can be construed in two ways.

1. *Distributively*. Each constitutive cognitive process P_i monitors *its own* performance: for every P_i belonging to P, P_i monitors the performance of *itself*.
2. *Collectively*. Some one (or, conceivably, several of the processes P_i monitors the performance *of P as a whole*, and this unit is in turn *self*-corrective on its own account; the overall performance of P is thus monitored by certain of its constituent components, which are themselves self-monitoring.

The difference can be illustrated graphically as follows.

1. *Distributive Self-correctiveness*

$$\begin{array}{lccccc} \text{Corrected unit} & \boxed{P_1} & \boxed{P_2} & \boxed{P_3} & & \\ & \uparrow & \uparrow & \uparrow & & \text{etc.} \\ \text{Correcting unit} & P_1 & P_2 & P_3 & & \end{array}$$

2. *Collective Self-correctiveness*

$$\begin{array}{lc} \text{Corrected unit} \left\{ \vphantom{\begin{array}{c} x \\ x \\ x \end{array}} \right. & \boxed{P = P_1 + P_2 + P_3 + \text{etc.}} \\ & \uparrow \\ & \boxed{P_j} \\ \text{Correcting Unit} \left\{ \vphantom{\begin{array}{c} x \\ x \end{array}} \right. & \uparrow \\ & P_j \end{array}$$

Both of these constructions represent perfectly good, although differing, modes of "self-correction."

Now the crucial fact is that the previously cited criticisms of Peirce's theory of the self-correctiveness of induction are all based on the specifically *distributive* construction of self-correction. The critics, in effect, complain to Peirce, "You say that scientific methodology has several components (preeminently qualitative and quantitative induction). But you only argue explicitly for the self-correctiveness of quantitative induction. How can you go on to claim that science as a whole is self-corrective?"

This complaint may be met on Peirce's behalf by pointing out that what is at issue in his theory of scientific method is the self-correctiveness of scientific induction in the *collective* sense, with quantitative induction in the controlling role. The crux of Peirce's view is that the performance of scientific induction as a whole, qualitative induction specifically included, can be monitored by statistical techniques, which in their turn are self-corrective in the narrower sense.

Exactly what must be established to support the view that scientific induction at large is self-corrective? Two theses are clearly needed:

1. Quantitative induction is (individually) self-corrective.
2. Qualitative induction can be correctively monitored by quantitative induction.

The first of these has already been discussed. It is certainly clear that Peirce does indeed maintain this thesis, and does so on grounds whose tenability—or at any rate arguability—is generally conceded. We may thus concentrate on the second thesis, the contention that the performance of qualitative induction can be monitored by statistical means.

3. How Can Statistical Methods Monitor the Performance of Qualitative Induction?

For Peirce, the scientific method is not a small list of rules (à la "Mill's Methods") but an internally complex and highly sophisticated organon, an intellectual discipline acquired by years of study and apprenticeship in the actual practice of theorizing and experimenting.

Peirce sees qualitative induction as an evolutionary process of variation and selection. Two component processes are involved here, as we have seen:

1. Hypothesis-projection or *abduction*: the purely conjectural proliferation of a whole gamut of alternative explanatory hypotheses that are relatively plausible, a proliferation based on guesswork—though not "mere" guesswork, but guesswork guided by a scientifically trained intuition. The aim of this enterprise is to identify those hypotheses that merit detailed scrutiny.
2. Hypothesis-testing or *retroduction*: the elimination of hypotheses on the basis of observational data, generally secured by suitably contrived experimental trials.

The result of the operation of the overall process is that the progress of science proceeds by the repeated eliminations of rival hypotheses in favor of one preferred candidate, each stage of the abduction-retroduction cycle reducing a cluster of conjectural hypotheses to an accepted theory. The systematic operation of the scientific method thus results at every stage in a diversified family of accepted hypotheses (that is, theories) that collectively constitute "the (current) scientific view of the way in which things work in the world."

The crucial feature of Peirce's theory of scientific methodology is that this process of theory and acceptance can be monitored statistically in terms of the *applications* of theories. As Peirce puts it, in a letter to Paul Carus, in qualitative induction the inquirer "may turn to the consideration of the hypothesis, study it thoroughly, and deduce miscellaneous observable consequences, and *then* return to the phenomena to find out how nearly these consequences agree with the actual facts" (CP, 8.232 [c. 1910]). Each time we employ a theory for prediction or for actual control (using it to guide intervention in the course of natural events to produce a desired outcome), we contribute to the statistical sample population by which its credentials are monitored. Its adequacy is thus controlled by what might be called its "success ratio":

$$\frac{\text{Number of successful applications (in prediction or control)}}{\text{Total number of such applications.}^{11}}$$

To put the matter in Peirce's own words:

> [T]he only sound procedure for induction, whose business consists in testing a hypothesis . . . is to receive its suggestions from the hypotheses first, to take up the predictions of experience which it conditionally makes, and then try the experiment. . . . [W]hen we get to the inductive stage *what we are about is finding out how much like the truth our hypothesis is, that is, what proportion of its anticipations will be verified*. (CP, 2.755 [c. 1905], italics added)[12]

The effectiveness of this success-ratio as a control of theorizing about the world is a key aspect of Peirce's understanding of the very idea of physical *reality* itself:

> [T]he distinction of reality and fiction depends on the supposition that sufficient investigation would cause one opinion to be universally received and all others to be rejected. . . . [I]n the long run, there is a real fact which corresponds to the idea . . . that a given mode of inference sometimes proves successful and sometimes not, and that in a ratio ultimately fixed. As we go on drawing inference after inference of the given kind, during the first ten or hundred cases the ratio of successes may be expected to show considerably fluctuations; but when we come into the thousands and millions, these fluctuations become less and less; and if we continue long enough, the ratio will approximate toward a fixed limit. We may, therefore,

> define the probability of a mode of argument as the proportion of cases in which it carries truth with it. (CP, 2.650 [1878])

Thus a true theory is one whose success ratio throughout the range of its inferential applications is 100 percent, one whose probability of correctness is 1, one that applies effectively throughout the whole range of the contexts of its inferential applications.[13] And a theory will more closely approximate the truth as its success ratio is higher, that is, closer to 1. It is against this background that Peirce's validation of induction should be seen:

> Thus, an argument [i.e., a "probable deductive" argument] that out of a certain set of sixty throws of a pair of dice about to be thrown, about ten will probably be doublets, is rendered valid by the fact that if a great number of just such arguments were made, the immense majority of the conclusions would be true, and indeed ten would be indefinitely near the actual average number in the long run. The validity of induction is entirely different; for it is by no means certain that the conclusion actually drawn in any given case would turn out true in the majority of cases where precisely such a method was followed; but what is certain is that, in the majority of cases, the method would lead to *some* conclusion that was true, and that in the individual case in hand, if there is any error in the conclusion, that error will get corrected by simply persisting in the employment of the same method. The validity of an inductive argument consists, then, in the fact that it pursues a method which, if duly persisted in, must, in the very nature of things, lead to a result indefinitely approximating to the truth in the long run. (CP, 2.781 [1902])

The primacy of quantitative induction lies in its ability to monitor the functioning of induction in general:

> The true guarantee of the validity of induction is that it 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 temporarily lead us. This it will do

> not by virtue of any deductive necessity (since it never uses all the facts of experience, even of the past), but because it is manifestly adequate . . . to discovering any *regularity* there may be among experiences, while *utter irregularity is not surpassed in regularity by any other relation of parts to whole*, and is thus readily discovered by induction to exist where it does exist, and the amount of departure therefrom to be mathematically determinable from observation where it is imperfect. (CP, 2.769 [c. 1905])

The use of statistical data to compile success ratios in order to assess the acceptability of theories provides the critical link between the qualitative and quantitative modes of induction. It becomes possible to use specifically quantitative induction to monitor the adequacy of inductive procedures as a whole, since every inductive generalization is correlated with the quantitative second-order thesis that it successfully accommodates in 100 percent of its applicative instances.

For Peirce, the only true test of the correctness of a theory is whether the inferences, applications, and predictions based on it prove successful. Purely intellectual factors such as explanatory power, parsimony, intuitive appeal, antecedent probability resulting from concordance with previously accepted theories, etc., are, for him, considerations relevant to the abductive process of selecting theories and hypotheses for testing. But these purely intellectual factors have no place in the specifically retroductive process of verifying the truth of theories and assessing their acceptability.

For Peirce, successful utilization affords the only strictly objective and nonintellectual standard for evaluating theories. In his Harvard lectures on pragmatism, he is explicit on its crucial controlling role:

> The justification for believing that an experiential theory which has been subjected to a number of experimental tests will be in the near future sustained about as well by further such tests as it has hitherto been, is that by steadily pursuing that method we must in the long run find out how the matter really stands. The reason that we must do so is that our theory,

> if it be admissible even as a theory, simply consists in supposing that such experiments will in the long run have results of a certain character. (CP, 5.170 [c. 1903])

It is clear how, for Peirce, quantitative induction validates induction in general—qualitative theory-projection included. Even a theory that has been completely successful in all tests to date may still prove inadequate in the long run, but further testing—that is, further quantitative inductions on as yet untested applications—will expose any error in our assessment and allow it to be corrected. We need assume only that the series of experiments by which we test a theory will reflect the nature of "reality" in the long run—an assumption which, for Peirce, is true by the very definition of the term, "reality."

Peirce's general position is exactly that of Hans Reichenbach, who writes:

> We thus come to the result that the rule of induction can by no means be maintained to be the best method of approximation. But with its help we can find better methods of approximation. Scientific method makes use of this fact to a great extent. . . . The rule of induction, or one of its equivalents, is the only method that can be used in the test of other methods of approximation, because it is the only method *of which we know* that it represents a method of approximation.[14]

Peirce, like Reichenbach, argues that if the experiential sector of nature has a "character," that is, if there is a reality in Peirce's sense (and this is something that cannot ever be fully established), then naive (straight rule) induction is bound to bring it out in the indefinitely prolonged long run.[15]

From this perspective, it is clear that even qualitative induction can be corrected through a quantitative monitoring of its products. Scientific progress is preeminently the change of adopted theories, and statistical controls can be used both (1) to determine that an improvement is necessary at this level (because the old theories are no longer 100 percent effective), and (2) to determine whether a proposed or supposed improvement is a real improvement (actually or probably). Accordingly, the process of qualitative induction itself can be correctively monitored by quantitative induction.[16] It can be subjected to quality control by statistical means. Peirce is thus at one with Sir Ronald Fischer in declaring that the theory of statistical design, and statistical inference in general, make key contributions to the theory of scientific induction.[17]

4. What Does Self-correctiveness Involve? Improving Our Theories versus Monitoring Them

Just how can an essentially statistical approach to the quality control of theorizing correct erroneous science so as to render the scientific enterprise self-corrective?

To begin with, it is necessary to note something it does not do. If we are incompetent in our theorizing, if the theories we accept are relatively inadequate, statistical checks will doubtless tell us that something is amiss, but they will certainly not do us the service of providing a better theory or theories to be adopted instead. Quantitative induction reveals *that* an improvement is necessary, but clearly not *which* improvement is needed.

The notion that science is self-corrective can be traced to several eighteenth-century writers (especially David Hartley [1705–1757], Georges Le Sage [1724–1803], and Joseph Priestley [1733–1804]), who took as their model the various mathematical methods of successive approximation, exemplified by such procedures as the well-known processes for determining n-th roots.[18] These methods, the rule of false position for example, operate in such a way that, given an initial position based on guesswork (however wild), there is an automatic procedure for successively revising this wrong answer into one that approximates stage by stage more closely to the correct one. As Priestley put it:

> 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. . . .[19]

Le Sage drew an analogy between the scientist at work and an arithmetician solving a problem in long division, producing at each stage a quotient more accurate than the one before.[20] Implicit throughout this eighteenth-century view is the conception that science possesses an *automatic and mechanically routine method* for continually improving on its older incorrect theories. This thesis—that science proceeds by routine steps of successive approximation inexorably closer to the truth, that scientific progress is a matter of convergence upon "the correct answer"—is the initial form of the theory of self-correction.

This early view of the self-correctiveness of science in terms of an automatic algorithm for improving its claims once their deficiency becomes apparent, is of course untenable. When the progress of science indicates that an accepted hypothesis is not warranted, then neither "the scientific method" nor any other cognitive device affords any automatic way of producing a new and more appropriate replacement.

It cannot be said too emphatically that Peirce does not hold the ill-advised view that the method of science is "self-corrective" in providing some sort of automatic, cookbook procedure for devising good theories to put in place of bad ones.[21] Nothing in his teaching requires him to take this very dubious stance. The complaint that the statistical process of quantitative induction fails to establish the "self-correctiveness" of science in the specifically abductive sense of theory improvement (finding a new and better theory to put in place of one that has been impugned by statistical quality controls) is simply beside the point and does nothing to invalidate Peirce's theory of self-correctiveness.[22] To maintain that the methods of scientific inquiry will—or can—in the long run determine the truth of proposed answers to scientific questions, is not to say that science has a routine method (or anything approaching an automatically effective procedure) that enables it to ferret out the correct answers to its questions. Analogously, when it is said of the calculating prodigy that his genius enables him with a routine *procedure* (codifiable and transmittable to others) for solving such problems. Or again, when someone maintains that the free market provides a mechanism for pricing commodities, he does not thereby imply that it provides some explicit routine procedure for determining the market price of commodities.

Peirce's theory of self-correctiveness is simply that the methods of science in general provide the tools of theory-improvement, not that quantitative induction in particular does. Induction as a whole—the scientific method in general and not each of the several parts of induction—is "self-corrective," in the sense of self-monitoring.[23] The job of producing new theories is done by the scientific method at large, and not specifically and particularly by quantitative induction. Self-correctiveness in the sense of *performance-monitoring* does not proceed by induction as a whole, but by a part of induction (quantitative induction) for induction as a whole. Theory improvement is accomplished not by quantitative induction (or by any one part of scientific method separately), but by scientific (or inductive) methodology as a whole.

The crucial point is that the "self-correctiveness" of scientific inquiry has two aspects: (1) quality control or performance-monitoring, and (2) theory improvement. Quantitative induction takes care of the former and does so routinely in a mechanical and automatic way; the scientific method in general (induction as a whole) takes care of the latter, albeit in a way that is certainly not routine. With these distinctions understood, the resulting Peircean position is safe against the sorts of changes launched by the host of critics mentioned above.

5. Self-correctiveness Reexamined

But is there not still a serious problem here? Surely no Cartesian deity is at work issuing *a priori* guarantees that science will ultimately ferret out the real truth of a matter. Peirce's own beliefs to the contrary notwithstanding, science surely may, even in the eventual long run, arrive at a false position.[24] How then can it possibly be claimed to be self-corrective? The really crucial core of the claim that science is self-corrective is the profound Hegelian thesis that it is certainly not corrigible by anything else.

Science is autonomous. Corrections to science must come from science. Shortcomings in scientific work can be discovered only by further scientific work. The mistaken results of science can be improved or corrected only by further results of science. There can be no recourse at this point to tealeaf reading, numerology, the Delphic oracle, or the like. The self-correctiveness of science is best understood to lie in the final analysis simply in the autonomy of science, in its not falling subject to any external standard of correctness. Scientific claims must, whenever corrected at all, be corrected by further scientific claims. A "science" subject to external standards of correctness simply does not deserve the name of science: the truthfulness of scientific claims must be settled wholly within the scientific enterprise itself. This fundamental fact is the rock bottom on which the doctrine of the self-correctiveness of science must find its foothold.

The Peircean theory of science may be controversial in many respects, but the core of its doctrine that science is self-corrective (that is, autonomous and not admitting of any external correction) is a view that is inexorably pressed upon us by the groundrules of cognitive rationality, and one that is surely right.

6. Conclusion

Peirce's theory of the self-correctiveness of science is predicated on the far-reaching idea that the adequacy of our theorizing can be monitored by a key component of the scientific method itself; namely, by statistical controls applied to the results by putting our theories to work in various ways, especially in spectatorial (that is, predictive) and manipulative control over nature. By these standards

our current theories are still far from perfect and science yet has a long way to go. But the superiority of, for example, the Pasteurian germ theory of disease over its earlier Galenic humor-imbalance counterpart does not rest on the evidence of its inner harmony, or intuitive appeal, systematic simplicity, or any other strictly intellectual factor. It has carried the day because of its statistical record of superior results in prediction and control over nature—in short, because of its greater applicative success. This idea—of a statistical monitoring of the success or failure of a theory's applications—is at the core of Peirce's conception of the self-corrective performance monitoring (that is, self-monitoring) of science. It provides an implicit and immanent standard of scientific progress, and a very reasonable one at that.[25]

2 Peirce on Scientific Progress and the Completability of Science

1. Peirce's Scientific Realism

Peirce propounded an ingenious theory regarding the relationship between the results of scientific inquiry and the nature of ''the real truth,'' in factual matters that deal with actual existence in the world. In the face of the philosophic sceptic's agnosticism as to the very possibility of attaining ''the real truth'' about nature, Peirce proposed that *the truth is simply ''the limit of inquiry,'' that is, what the scientific enterprise will discover in the idealized long run*, or would discover if the efforts were so extended.[26] Once scientific progress reaches a point at which a question is answered in a certain way and that answer is thereafter maintained without change within the ongoing community of inquirers, then it is indeed the true answer to the question in hand.[27] At issue is a Copernican inversion reminiscent of Kant: It is not that ''rational inquiry'' is appropriate because what it ultimately arrives at is the actual truth, but that ''the actual truth'' qualifies as such because rational inquiry ultimately arrives at it. As Peirce put it, the truth simply *is* ''the opinion which is fated to be ultimately agreed to by all who investigate.''[28] Accordingly, he maintained:

> Reality is that mode of being by virtue of which the real thing is as it is, irrespectively of what any mind or any definite collection of minds may represent it to be. The truth of the

> proposition that Caesar crossed the Rubicon consists in the fact that the further we push our archaeological and other studies, the more strongly will that conclusion force itself on our minds forever—or would do so, if study were to go on forever. (CP, 5.565 [1901])

Truth is *adaequatio ad rem,* and reality answers to the view of things lying at the *focus imaginarius* of the end of inquiry. As Peirce put it:

> [R]eality, the fact that there is such a thing as a true answer to a question, consists in this; that human inquiries,—human reasoning and observation,—tend toward the settlement of disputes and ultimate agreement in definite conclusions which are independent of the particular stand-points from which the different inquirers may have set out; so that the real is that which any man would believe in, and be ready to act upon, if his investigations were to be pushed sufficiently far. (CP, 8.41 [c. 1885])

By this ingenious doctrine of Hegelian inspiration, Peirce was able to bridge the Kantian gap between reality as it is *an sich* and *our reality* (our scientific conception of reality) by holding that the former is simply the long-range projection of the latter.[29]

Specifically, Peirce's theory was predicated on two contentions:

1. *Ultimate Correctness*. Whatever science will come to maintain over the theoretical long run is indeed true. (Over the TLR, science maintains *only* the truth.)
2. *Ultimate Completeness*. *All* truth regarding the world will be realized by science in the theoretical long run. (Over the TLR, science maintains *all* the truth.)

The first of these theses stipulates the accuracy of theoretical-long-run science, its ability to get at the actual truth of things.[30] The second stipulates its comprehensiveness (nay, potential omniscience), that no general truths about the world are in principle beyond its ken, that nature contains no ultimately occult compartments of inaccessible truth.[31] Like a good witness, theoretical-long-run science presents the truth, the whole truth, and nothing but the truth.

On such a view, the truth about factual matters at the level of generality simply *coincides* with what science will maintain to be so in the long run. Genuine factual knowledge, our attainment of "the real truth" about the world, can thus be construed as being what Peirce characterized as the "final irreversible opinion" of the scientific community.[32] Espousal in the long run within an ideally projected community of scientific inquirers is both a necessary and sufficient condition for the truthfulness of generalizations in the factual (world-oriented) domain. For Peirce, the decisive consideration in the determination of truth is stability; Peirce himself used the word "fixity." According to him, the mark of a true belief, one constrained by an external and independent reality, is that it is destined or, as he put it, "fated" to be underwritten by the operation of scientific method.[33]

2. The Historical Background of Peirce's Doctrine: The *Fin de Siècle* View of Natural Science

To clarify Peirce's position, let us examine it in its historical context.

One acute contemporary analyst of physics speculates as follows about the ultimate completion of his field:

> It is possible to think of fundamental physics as eventually becoming complete. There is only one universe to investigate, and physics, unlike mathematics, cannot be indefinitely spun out purely by inventions of the mind. The logical relation of physics to chemistry and the other sciences it underlies is such that physics should be the first chapter to be completed. No one can say exactly what completed should mean in that context, which may be sufficient evidence that the end is at least not imminent. But some sequence such as the following might be vaguely imagined: The nature of the elementary particles becomes knwon in self-evident totality, turning out by its very structure to preclude the existence of hidden features. Meanwhile, gravitation becomes well understood and its relation to

> the stronger forces elucidated. No mysteries remain in the hierarchy of forces, which stands revealed as the different aspects of one logically consistent pattern. In that imagined ideal state of knowledge, no conceivable experiment could give a surprising result. At least no experiment could that tested only fundamental physical laws. Some unsolved problems might remain in the domain earlier characterized as organized complexity, but these would become the responsibility of the biophysicist or the astrophysicist. Basic physics would be complete; not only that, it would be manifestly complete, rather like the present state of Euclidean geometry.[34]

Extended from physics to natural science in general, such a position holds that the realm of potential discovery is effectively exhaustible. To be sure, it is never wholly and completely exhausted: the total exploration of nature is achieved only gradually and "in the limit," because the upper limit of potential scientific discovery is never actually reached but only approached asymptotically over the long run of scientific progress. Nevertheless, this perspective encourages a situation in which really big surprises remain. To be sure, there is no final *Gotterdämmerung*, and natural science does not come to a stop, but really important innovation fades away for all practical purposes.

A position of just this sort was maintained by Peirce who, in effect, saw the history of science as progressing by way of asymptotic approximation to a finally adequate picture of the world. He wrote that we can expect "in the progress of science its error will indefinitely diminish, just as the error of 3.14159, the value given for π, will indefinitely diminish as the calculation is carried to more and more places of decimals."[35]

As Peirce saw it, the current stage of scientific knowledge is such that further scientific progress is solely a matter of increasing accuracy, of filling in with increasing refinements and ever more accurate determination the details of a picture whose shape becomes increasingly clear and well-defined. Future progress is a matter of providing increasingly fine-grained detail within a context whose course-grained structure has already been determined. Thus in about 1896, Peirce wrote, that it is not implausible to think, given the content of current science, that "the universe is now entirely explained in all its leading features; and that it is only here and there that the fabric of scientific knowledge betrays any rents."[36]

Peirce was by no means alone in regarding such a view as tenable. In physics above all, the conviction was widespread in the 1875–1905 era that the days of major innovations were over, that all the really big discoveries had been made. Some of the most able physicists of the day shared this sense, that the discipline had reached its more or less completed form and that little remained to be done, apart from work on relatively minor issues.

In a dedication address delivered at the Ryerson Physical Laboratory at the University of Chicago in 1894, A. A. Michelson, America's first Nobel Laureate in science, remarked:

> While it is never safe to affirm that the future of Physical Science has no marvels in store even more astonishing than those of the past, it seems probable that most of the grand underlying principles have been firmly established and that further advances are to be sought chiefly in the rigorous application of these principles to all the phenomena which come under our notice.
>
> It is here that the science of measurement shows its importance—where quantitative results are more to be desired than qualitative work. An eminent physicist has remarked that the future truths of Physical Science are to be looked for in the sixth place of decimals.[37]

Michelson's gloomy forecast was echoed by T. C. Mendenhall, formerly a physics professor, at the time a college president, and soon to be both president of the American Association for the Advancement of Science and Superintendent of the United States Coast and Geodetic Survey. In his popular text on electricity (1887), he maintained:

> More than ever before in the history of science and invention, it is safe now to say what is possible and what is impossible. No one would claim for a moment that during the next five

> hundred years the accumulated stock of knowledge of geography will increase as it has during the last five hundred. . . . In the same way it may safely be affirmed that in electricity the past hundred years is not likely to be duplicated in the next, at least as to great, original, and far-reaching discoveries, or novel and almost revolutionary applications.[38]

This *fin de siècle* sentiment that the great heroic deeds of physical science were over was by no means confined to a few eccentric notables.[39] In his 1956 presidential address to the American Physical Association, R. T. Birge recalled his first physics teacher at the University of Wisconsin in 1906:

> To him physics was an incomparably beautiful, but *closed* subject. There was nothing in his lectures to suggest that there were things still to be discovered in physics, and hence no incentive to enter the field except to become a teacher and in turn show these same beautiful experiments to one's own students.[40]

An even more remarkable instance of the same phenomenon is given by Max Planck:

> As I was beginning to study physics [in 1875] and sought advice regarding the conditions and prospects of my studies from my eminent teacher Phillip von Jolly, he depicted physics as a highly developed and virtually full-grown science, which—since the discovery of the principle of the conservation of energy had in a certain sense put the keystone in place—would soon assume its finally stable form. Perhaps in this or that corner there would still be some minor detail to check out and coordinate, but the system as a whole stood relatively secure, and theoretical physics was markedly approaching that degree of completeness which geometry, for example, had already achieved for hundreds of years. Fifty years ago [as of 1924] this was the view of a physicist who stood at the pinnacle of the times.[41]

Contemplation of the enormous strides being made all across the scientific frontier—in biology, medicine, chemistry, and on and on—led to the widespread if not typical belief that science stood pretty much at the last frontiers and that the course of progress in scientific knowledge—so dramatically explosive since its first great flourishing in the seventeenth century—was not approaching completion. From the teacher of Planck in the 1870s to the teacher of Birge in the 1900s, a substantial group among those working physicists who thought about the issue at all took the view that the potential range of physical knowledge is finite and moreover held that the proportion of the known to the unknown sector of this finite range was relatively large.

In this regard, Peirce was a child of his time. But the predominant ethos was one of success and self-congratulation, decidedly not of failure. To this extent, to speak of *fin de siècle*, with its overtones of weariness, exhaustion and failure of nerve, is not appropriate. The dominant sentiment in metascientific theory was elation, even pride, in the immense strides taken, a sense of power approaching *hubris* that the intellectual conquest of nature was virtually complete.[42] As Peirce wrote in 1878: "the new phenomena which now remain to be discovered are probably only of secondary importance."[43]

3. The Foundation of Peirce's Theory: The Geographic Exploration Model

Peirce's equation of "the real truth" about nature with the ultimate conclusions of science in the idealized long run immediately encounters an apparent difficulty. It seems plausible to suppose *a priori* that the physical world does have a nature of some sort, that something or other is bound to be generally true of it. And if we assume, with Peirce, that all truth is (ultimately) discovered truth stably maintained, then problems arise. For how can we possibly know in advance that science finally will settle down to a stable teaching? And so various critics take umbrage when Peirce insists that "there is a general *drift* in the history of human thought which will lead it to one general agreement, one catholic consent."[44] Let Bertrand Russell speak for them all:

> Is this an empirical generalization from the history of research? Or is it an optimistic belief in the perfectability of man? Does it contain an element of prophecy or is it a merely hypothetical statement of what would happen if men of science grew continually cleverer? Whatever interpretation we adopt, we seem committed to some very rash assertion.[45]

What guarantee have we that scientific progress will ultimately lead to a stable result, that scientific opinion will not eventually simply diverge or oscillate or go off on a long random walk? Peirce thinks that we do have such an assurance, and his position on this question is indeed crucial to his metaphysics of knowledge.

Briefly stated, Peirce's doctrine amounts to a *cumulative-convergence theory* of scientific progress.

Peirce, in effect, saw the history of science as progressing through two stages: an initial or preliminary (noncumulative) phase of groping for the general structure of the *qualitative* relations among the parameters of nature, and a secondary (cumulative) phase of *quantitative* refinement, where the second phase would determine with increasing precision the exact values of parameters that figured in equations whose general configuration was determined in the initial one. In the first stage, there is the qualitative change of a succession of substantively discordant theories. In the second stage, the main qualitative issues (we might say, the equation-forms) are settled, and it becomes a matter of refining knowledge of the constants at issue. Once the first qualitative phase is completed—a stage Peirce believed to have been realized by his own day, at least in the physical sciences[46]—then ongoing scientific progress is just a matter of increasing detail and exactness, of determining the ever more minute decimal-place values of quantities whose approximate value is already well established. The increasingly sophisticated radiotransmission of photographs provides an apt analogy. There is indeed change,

but such change always preserves the rough outline of the old while filling in its details.[47]

On this view, science in due course enters into a condition where further progress is cumulative (always preserving and merely improving on what has gone before) and convergent (further improvements come to be smaller and smaller in scope).[48] Indeed, convergence presupposes a cumulation of sorts. For if there is to be convergence, then the previous position must always be preserved in its essentials and then improved in some relative detail. Convergence implies reaching a stage after which further change becomes a matter of filling in yet another decimal place to lend added refinement to an already largely fixed picture. Accordingly, Peirce insists:

> [S]cience does not advance by revolutions, warfare, and cataclysms, but by coöperation, by each researcher's taking advantage of his predecessors' achievements, and by his joining his own work in one continuous piece to that already done. (CP, 2.157 [1902])

On such a view, science undeniably "has a future" since there will always be worthwhile discoveries to be made. But discoveries take place within a context of overall limits, and the magnitude of their intrinsic importance becomes ever smaller. New discoveries will not, nay cannot, modify the overall understanding of processes of nature in any fundamental way. They serve to increase the accuracy or sophistication of a basically determinate view of the world, making adjustments and refinements which, however difficult and important the work of discovery itself might be, still produce only marginal adjustments in our intellectual world picture. To be sure, scientific progress will not ever quite reach a completed, final, static, and unchanging state, because the realm of potential discovery is never completely mapped. Rather, it moves toward this position by way of asymptotic approximation to a finally adequate picture of the world.

The analogy of geographic exploration is suggestive here. First an entire hemisphere is added to the globe. Then oceans and continents are explored. After that, the source of the Nile and the depths of the Arabian desert are brought to light. Eventually the North Pole is visited and Everest is scaled. The latter are great, perhaps monumental, *achievements*, but as *discoveries* their magnitude shrinks to comparative insignificance.[49]

This geographic-exploration model is not a historical curiosity that flourished in Peirce's day and died thereafter. It continues to thrive today. Recently, it was given a clear and eloquent formulation by the microbiologist, Gunther S. Stent:

> I think everyone will readily agree that there are *some* scientific disciplines which, by reason of the phenomena to which they purport to address themselves, are *bounded*. Geography, for instance, is bounded because its goal of describing the features of the Earth is clearly limited. . . . And, as I hope to have shown in the preceding chapters, genetics is not only bounded, but its goal of understanding the mechanism of transmission of hereidtary information *has*, in fact, been all but reached. Indeed, and here I will probably part company with some who might have granted me the preceding example, even such much more broadly conceived scientific taxa as chemistry and biology are also bounded. For in the last analysis, there is immanent in their aim to understand the behavior of molecules and of ''living'' molecular aggregates a definite, circumscribed goal. Thus, though the total number of possible chemical molecules is very great and the variety of reactions they can undergo vast, the goal of chemistry of understanding the principles governing the behavior of such molecules is, like the goal of geography, clearly limited. . . . [T]here is immanent in the evolution of a bounded scientific discipline a point of diminishing returns; after the great insights have been made and brought the discipline close to its goal, further efforts are necessarily of ever-decreasing significance.[50]

We have here an accretional view of the progress of science, with each successive accretion inevitably making a relatively smaller contribution to what has come before. Progress, on this view, consists in

driving questions down to lesser and lesser magnitudes, providing increasingly enhanced detail of increasingly diminished significance.[51] This at bottom is the Peircean vision of ultimate convergence in scientific inquiry.[52]

4. A Critique of the Geographic Exploration Model

The plausible analogy of geographic exploration is nevertheless fundamentally mistaken. It views scientific progress as a whole on the basis of one particular (and by no means typical) sort of progress, the sequential filling in of an established framework with greater and greater detail, lending additional refinement to a fundamentally fixed result. This view combines two gravely erroneous ideas: (1) that science progresses by cumulative accretion (like the growth of a coral reef), and (2) that the magnitude of these additions is steadily decreasing.

If the first of these ideas collapses, so does the position as a whole. And collapse it does, for science progresses not just *additively* but in large measure also *subtractively*. As Thomas Kuhn and others have persuasively argued, today's most significant discoveries always represent an overthrow of yesterday's: the big findings of science inevitably take a form that contradicts its earlier big findings and involve not just supplementation but replacement. Substantial headway is made preeminently through conceptual and theoretical innovation. The preservationist stance, that the old views were accpetable as far as they went and merely need supplementation, will not serve. Significant scientific progress is genuinely revolutionary in that there is a fundamental change of mind about how things happen in the world.

The medicine of Pasteur and Lister does not add to that of Galen or of Paracelsus, but replaces them. The creative scientist is as much a demolition expert as a master builder. Significant scientific progress is generally a matter not of adding further facts on the order of filling in of a crossword puzzle but of changing the framework itself. Science in the main develops not by addition but by substitution and replacement.[53] Progress lies not in a monotonic accretion of more information but in superior performance in prediction and control over nature.[54]

The Peircean doctrine of convergent cumulation must, accordingly, be abandoned. Nevertheless, it must be recognized that Peirce contributed an insight of immense value in this sphere. This insight concerns the economic aspect of the matter. Let us look at it more closely.

5. Cost-Escalation versus Yield-Diminution

In his pioneering 1879 essay on ''Economy of Research,'' Peirce addressed the issue of the increasing difficulty of scientific progress:

> We thus see that when an investigation is commenced, after the initial expenses are once paid, at little cost we improve our knowledge, and improvement then is especially valuable; but as the investigation goes on, additions to our knowledge cost more and more, and, at the same time, are of less and less worth. Thus, when chemistry sprang into being, Dr. Wollaston, with a few test tubes and phials on a tea-tray, was able to make new discoveries of the greatest moment. In our day, a thousand chemists, with the most elaborate appliances, are not able to reach results which are comparable in interest with those early ones. All the sciences exhibit the same phenomenon, and so does the course of life. At first we learn very easily, and the interest of experience is very great; but it becomes harder and harder, and less and less worthwhile. . . . (CP, 7.144)

As this passage makes clear, Peirce sees natural science as subject to (1) rising costs and (2) diminishing returns.

It is important to realize that two substantially different ideas are conjoined in this Peircean picture of the economics of scientific progress. The first of these is the ideal of *cost-escalation*: the conception that with the progress of science, it becomes increasingly expensive (in both material resources and human effort) to achieve worthwhile new results. The second is *yield-diminution*: the conception that with the progress of science the later findings are inevitably of an increasingly diminished significance. Diagramatically, the contrast stands as follows:

1. later → harder (more difficult, more expensive)
2. later → lesser (less significant)

To make use of a homely apple-picking analogy, the first thesis says that the later apples are harder to get off the tree than the earlier; the second says that the later apples are smaller than the earlier.[55] These two ideas move in quite different directions. Though conjoined in Peirce's thought, they are certainly separable.

The point to be stressed in this connection is that the preceding critique of Peirce's position relates solely to the second member of this pair, his conception of yield-diminution. The strictures presented above to not touch the thesis of cost-escalation, an idea which seems to me profoundly insightful and entirely right. This important point warrants closer scrutiny.

6. Cost-Escalation

Evidence of the escalating costs of scientific progress is provided by some suggestive statistics:[56]

1. In the U.S.A. the total (real) expenditure per scientist has been increasing at a rate of roughly 7 percent per year (thus doubling roughly every ten years) throughout recent history,[57] while the per-capita productivity of scientists (measured by contributions to the literature) has remained relatively constant.
2. The number of scientists working in the U.S.A. has been increasing at roughly 6 percent per year (doubling every twelve years or so), whereas (a) the number of ''eminent'' men (those selected for listing in the standard biographical handbooks or other registers that select only a limited elite of scientific contributors) has been increasing at a rate of only

> about 3 percent per year (thus doubling in around twenty years),[58] and (b) the number of relatively significant findings (those cited in the references of synoptic monographs, handbooks, and textbooks) has to all appearances been growing at a virtually *linear* rate.[59]

The historic situation regarding the costs of American science was carefully delineated by Raymond Ewell in 1954.[60] His study of research and development expenditures in the U.S. showed that growth has been exponential; from 1776 to 1954 nearly $40 billion was spent, and half of that was spent after 1948. Research and development expenses were found to be increasing at a rate of 10 percent per year.[61] Projected at this rate, Ewell saw the total climbing to what he viewed as an astronomical $6.5 billion by 1965, a figure that turned out to be too conservative.[62] By the mid-1960s, America was spending an amount on research and development that was more than the whole of the federal budget before Pearl Harbor.

The proliferation of scientific facilities has proceeded at an impressive pace over the past hundred years. In the early 1870s there were only eleven physics laboratories in the British Isles; by the mid-1930s there were more than three hundred;[63] today there are several thousand. The scale of activities in these laboratories has also expanded vastly. It is perhaps unnecessary to dwell on the immense cost of the research equipment of contemporary science. Even large organizations can hardly keep pace with rising levels of research expenditures.[64] Radiotelescopic observatories, low-temperature physics, research hospitals, and lunar geology all involve outlays on a scale that require the funding support of national governments, sometimes even consortia of governments. Science has increasingly become a very expensive undertaking. Alvin M. Weinberg, former Director of the Oak Ridge National Laboratory, has written:

> When history looks at the 20th century, she will see science and technology as its theme; she will find in the monuments of Big Science—the huge rockets, the high-energy accelerators, the high-flux research reactors—symbols of our time just as surely as she finds in Notre Dame a symbol of the Middle Ages.[65]

Every recent statistical study that has been made of the costs of scientific research projects in private industry, in government, and in academic institutions yields the uniform result that the per project cost (in real dollars) has grown at a doubling time of less than ten years throughout recent decades. Unless we are prepared to accept the farfetched view that the average unit yield of scientific research has been improving impressively, we are driven to a picture of steadily increasing human and material costs of high-level scientific results. There are, it seems, substantial grounds for agreement with Max Planck's appraisal:

> To be sure, *with every advance* [in science] *the difficulty of the task is increased; ever larger demands are made on the achievements of researchers,* and the need for a suitable division of labor becomes constantly more pressing.[66]

It emerges that the economic sector of Peirce's theory of scientific progress involves two, distinct, though for him interrelated, contentions, one of which, yield-diminution, deserves to be scrapped, but the other of which, cost-escalation, represents an important insight into the structure of scientific research.

7. An Economic Critique of Peirce's Theory of the Truth-Science Relationship

It is somewhat ironic that Peirce's scientific realism can be seen to be flawed from a perspective that is in fact highly congenial to it—an economic one.

A zero-growth world is upon us in science as elsewhere. The resources at our disposal are limited, and we shall not be able to continue to exploit them at exponentially increasing levels as we have done in the past. There is, for example, a limit—a fundamentally economic limit—to the size of the particle accelerators, radio telescopes, high-flux reactors, etc., that can be constructed. These limits inexorably circumscribe our cognitive access to the real world. There are interactions with nature of such a scale (measured in such parameters as energy, pressure, temperature, particle velocities, etc.)

whose realization would require the use of resources on so vast a scale that we could never realize them.

But if there are interactions to which we have no access, then there are (presumably) phenomena that we cannot discern. It would be unreasonable to expect that nature has confined the distribution of phenomena of potential cognitive significance to ranges that lie within our ken.

Where there are inaccessible phenomena, there must be cognitive incompleteness. To this extent, at any rate, the empiricists were surely right. Moreover, if certain phenomena are not just undetected but by their very nature are inaccessible to us (even if only for the merely economic reasons noted above), then our theoretical knowledge of nature must presumably be incompletable. Only the most hidebound rationalists could uphold the capacity of sheer intellect to compensate for the lack of data. Where there are unobserved phenomena, we must reckon with the prospect that our theoretical systematizations are incomplete.

Thus, there is a limit, ultimately an economic limit, to the questions about the phenomena of nature that we can ever answer. Many questions of transcending scientific importance will remain unresolved because their resolution would demand a greater concurrent deployment of resources than will ever be marshalled at any one time in a zero-growth world. There will thus be truths that science cannot attain, so that we shall not (even in the infinite long run) achieve "the whole truth." The economic requisites of scientific work are such that our knowledge of nature must finally remain incomplete.

Think once again of the main contentions of the preceding discussion: (1) in science, yield-diminution must be rejected because major new scientific innovations—revolutionary discoveries—are always possible in principle, and (2) that cost-escalation must be accepted as a fact of scientific life; scientific revolutions, though possible, are increasingly more difficult and expensive to mount. These contentions have dire repercussions for a Peircean scientific realism. Consider again his two key theses:

1. *Correctness*: That whenever science will come to maintain

over the theoretical long run (TLR) is true; over the TLR, science maintains nothing but the truth.
2. *Completeness*: That all truth regarding the world will be realized by science in the theoretical long run; over the TLR, science maintains all the truth.

Our present consideration of the economics of scientific progress cast a strong shadow of dubiousness over Peirce's position. His completeness thesis emerges as untenable because, for essentially economic reasons, there will be questions that science will never be able to resolve, even in the theoretical long run.[67] It would thus appear that certain features fundamental to the very structure of man's inquiry into the ways of the world conspire to limit the knowledge that we can attain in this sphere.[68]

Even Peirce's correctness thesis must be rejected, because what science maintains over the indefinitely projected long run could well be false, having defects that could be discovered only on the far side of an economic data-barrier. The effort needed to determine the falsity of claims at issue might make demands on resources that could not be met in a zero-growth world. There is every reason to deny that what we ultimately reach is actually "nothing but the truth" since there is every reason to think that where scientific knowledge is concerned further knowledge does not just supplement but generally corrects our knowledge-in-hand, so that the incompleteness of our information entails its incorrectness as well.

8. The Economic Impediments to Peirce's Scientific Realism

Peirce overlooked a crucial factor when he wrote:

> [T]hought, controlled by a rational experimental logic, tends to the fixation of certain opinions, equally destined, the nature of which will be the same in the end, however the perversity of thought of whole generations may cause the postponement of the ultimate fixation. (CP, 5.430 [1905])

Whatever consensus may arise from "rational experimental logic," in natural science we must work with data. These data stem from interactions with nature, and the realization and cognitive exploitation of these interactions has an economic aspect that limits their completeness. As anyone who has ever confronted a curve-fitting problem well knows (Peirce himself certainly included), incomplete data lend themselves to incompatible extrapolations. In the very nature of the case, an ultimate fixation of opinion is unattainable in such circumstances.

Peirce's theory of the science-truth relationship is defective simply because it ventures too far into abstraction from the human limitations within which scientific work must actually be done. His conception may well be plausible—for a community of powerful disembodied intelligences whose capabilities and efforts at mastering the resistances of nature are unimpeded by any economic constraints, beings whose observations are obtained by cost-free processes, for whom computations and data-processing are to be had for the asking, or whose experimental interactions with nature can be carried out by acts of will alone. But creatures like us, whose scientific efforts are subject to crucial limitations of resources, cannot be confident that their science must in principle ultimately ferret out "the real truth." In the actual circumstances, the Peircean vision of the scientific enterprise is visionary, in the sense of being unrealistic.

Peirce and those scientific realists who follow him hold the ill-advised view that only the limitations of time separate "the-truth-about-reality" from "the teachings of science." They see truth attainment as a one-factor idealization looking to the removal of a single limitation—that of effort maintained over a sufficiently extended course of time. However, the present deliberations indicate that if one wants to move along such a route, then one must resort at least to a two-factor idealization with respect not only to time but (more drastically) also to the availability of resources. And the presence of this second dimension of idealization introduces an unrealistic aspect of far-fetchedness into the scientific realism at issue, rendering the whole thing a very dubious proposition. For while the prospect of intelligent life on other planets may conceivably remove

temporal limitations (cf. footnote 27 above), it cannot remove economic ones.

These considerations indicate that any version of a scientific realism of Peircean stamp is untenable when it proposes to conceptualize reality as "what will eventually be held to be the case by science (over the long run)." If there is reason to think—in line with our theory—that even the ultimate scientific consensus (if there was to be one) would inevitably be an imperfect fragment of some larger, more complex structure that lies further down the road than we can ever actually afford to travel, then the equation:

reality as it actually is = reality as science will ultimately
(and irreversibly) deem it to be

must be abandoned. One cannot venture to close the gap between our putative reality and genuine reality by resort to the contrast between what we think at some juncture and what we are fated to think in the long-run (as Peirce puts it).

To be sure, this critique does not require us to abandon Peirce's conception of the essentially experiential character of "reality." But, as he himself insists, it is not actual but possible experiences that are at issue, not just the *is*s but also the *would-be*s of the thing. And economic limitations of the sort we have noted are crucial here, placing an entire range of experiential *would-be*s outside our effective reach, not just until further notice, but permanently. A sector of (theoretically) experientiable reality becomes *de facto* inaccessible.

On such a view, the progress of science, even at the level of the idealized long run, will never issue in authentic reality, but only in a historical succession of suboptimally putative realities, reality-pictures to which we cannot impute any sort of finality, not even that merely approximate finality envisaged by Peirce.

Accordingly, we seem driven to the conclusion that there is no real justification for equating "the truth about reality" with "the teachings of science," if the science at issue is indeed our human science, carried on by creatures subject to the fundamentally inevitable economic limitation of finite resources.

This upshot is ironic, as we have noted, because Peirce was in many contexts so acutely (and pioneeringly) alive to the economic aspects of science. It was Peirce himself who brought the issue of the economics of science to the forefront and virtually single-handedly laid the foundations for this enterprise in the discipline he characterized as "the economy of research."[69] His basic insights into the economic aspects of science, particularly his views on the cost-escalation of ongoing significant scientific work, are prescient, valid, and highly useful for a proper understanding of the scientific enterprise. Yet these very considerations operate to cast strong doubt upon the tenability of Peirce's metaphysical teaching that science is fated to arrive at the real truth of things in the long run.

The line of objections considered here did not wholly escape Peirce himself, and he came close to dealing with it. In the important unpublished 1884 paper, "Design and Chance" (Ms. 875), Peirce proceeds to "call in question," and indeed to "call into doubt," his own earlier doctrine—the "fundamental axiom of logic" that "every intelligible question whatever is susceptible in its own nature of receiving a definitive and satisfactory answer, if it be sufficiently investigated by observation and reasoning" (p. 7).[70] And in one extremely interesting passage, typical of his thinking on this issue after 1880, he maintained:

> [I]f we think that some questions are never going to get settled, we ought to admit that our conception of nature as absolutely real is only partially correct. Still, we shall have to be governed by it practically; because there is nothing to distinguish the unanswerable questions from the answerable ones. . . . (CP, 8.43 [c. 1885].)[71]

In the context of this passage, Peirce contemplates a retreat from his usual hopeful view that whatever can ultimately be answered by science will ultimately be answered, that "there is an ascertainable true answer to every intelligible question."[72] This withdrawal leads him to the uncomfortable and un-Peircean thesis that perhaps "our conception of nature as absolutely real is only partially correct." To be sure, Peirce continues to hold tight to the central doctrine of his metaphysics that all of reality will ultimately be known,[73] but now the prospect of ultimately unanswered questions about nature opens a disconcerting gap between (natural) reality on one hand and nature itself on the other,[74] a gap between empirical and noumenal "reality" that reopens the Kantian issue that Peirce's theory of truth was designed to close. It is doubtful in the extreme whether we—or indeed Peirce himself, struggle though he might[75]—could long manage to live with such a gap.

We are happily under no compulsion to occupy this uncomfortable position. After all, to vindicate the methods of science as tools of inquiry we do not need to invoke the cumulative-convergence doctrine, and we do not need an *a priori* guarantee—not even a regulative assumption—that science will ultimately "deliver the goods" regarding the real truth of things. All that we do need is a reasonable assurance that by adopting the methods of scientific inquiry we shall do as well as it is possible to do in the epistemic circumstances of the case, that the methodological posture of science is in no way inferior to its contemplatable alternatives. And this sort of defense is in fact one that the whole gamut of Peircean considerations—the appeal to autonomous self-correctiveness, metaphysical soundness, and applicative success—is well suited to provide.[76]