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The Economy of Peirce's Abduction

It was long thought that induction was a process of discovery. Bacon seemed often to think so; and later even Mill, in his occasional flights of fancy, suggests that "induction may be defined, the operation of discovering and proving general propositions."[1] Mill, however, could always rely on Whewell's excesses to bring him back to his senses: "Dr. Whewell," he notes, "calls nothing induction where there is not a new mental conception introduced, and everything induction where there is. But this is to confound two very different things, Invention and Proof."[2] "Success," he concluded, "is here dependent on natural and acquired sagacity, aided by knowledge of the particular subject allied to it. Invention, though it can be cultivated, cannot be reduced to rule; there is no science which will enable a man to bethink himself of that which will suit his purpose."[3]

This distinction between invention and proof, or between the "context of discovery" and the "context of justification" as Reichenbach called it, has persisted and in many respects has become a central dogma of those philosophers of science who follow in the empiricist tradition.[4] The proper role of philosophy, on this view, is the logical reconstruction of the warranty, justification or test of scientific hypotheses and a careful, logical analysis of the language and method by which this justification is achieved. Induction, at best, is seen as a weighing of positive evidential support for hypotheses, and, at worst, vanishes in favor of a principle of falsification according to which successful hypotheses are those which no data have yet shown to be false. But the principal logical relationship between hypotheses and positive and negative instances of them is seen as deductive. Certain singular statements are shown to be deductive consequences of the general hypotheses and statements of initial conditions, the latter thus explaining the events described by the former; or, more decisively, the statement purporting to describe some event, proves not to, and thus falsifies the premises containing the hypothesis under consideration. Mill and Whewell both had versions of this hypothetico-deductive method, as it has come to be called, and it admits of greater sophistication than I have indicated here. It received its classical statement in Karl Popper's, *The Logic of Scientific Discovery*. This is an ironical title, since Popper made quite clear that the discovery of hypotheses was not a matter of logic at all; it was, as it had been for Mill, a matter of "natural and acquired sagacity." "The initial stage," writes Popper, "the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it. The question how it happens that a new idea occurs to a man may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge."[5]

The trouble with this view is not with its account of the articulation and testing of scientific hypotheses once they are proposed and part of current, on-going research. It is rather with what it leaves out. Recent work in the history of science has offered a strikingly different picture of the procedures and character of scientific inquiry in which the initial stages of proposing new hypotheses for consideration occupy a prominent place. This has raised anew the question whether it is possible to give a philosophical account of the beginning phases of hypothesis formation analogous to that of the later stages of hypothesis testing. In addition, the reconstruction of Popper and others seems to leave entirely unexplained why certain hypotheses come to be proposed (and perhaps accepted) when, as a matter of logic, an indefinitely large number of them, equally adequate from the point of view of the available data, seems possible. (A favorite example is the possibility of drawing an indefinitely large number of lines through a finite set of points.) In fact, at various important junctures in the history of science very few alternative hypotheses or theories have ever been available, or seemed at all plausible or worthy of commitment by the scientists whose energies and reputations were involved.

What I would like to discuss in this paper, then, is the question to what extent it is possible to give a philosophical account of the context of discovery, that is, to account for the initial formulation and proposal of hypotheses as well as the later stage of hypothesis justification. My approach will be to consider the efforts of C. S. Peirce to do just this, namely, to formulate a logic of discovery, which he called a logic of abduction, as opposed to inductive procedures of testing.

Peirce begins by characterizing what he understands a hypothesis to be.

It is, he says, "any proposition added to observed facts, tending to make them applicable in any way to other circumstances than those under which they were observed" (6.524).[6] Clearly what Peirce wants to consider are general law-like statements whose scope is greater than their known positive instances. He is excluding hypotheses which are merely *ad hoc* or face saving. Second, he notes that "a hypothesis ought at first to be entertained interrogatively. Thereupon, it ought to be tested by experiment as far as practicable . . . . But it is the first process, that of entertaining the question, which will here be of foremost importance" (6.524). Here then, is the distinction between discovery and justification; first, we may entertain a hypothesis interrogatively; then later we may subject it to experimental test. It is the former process, "the first starting of a hypothesis and the entertaining of it" which he calls *abduction*. "This will include," he continues, "a preference for any one hypothesis over others which would equally explain the facts, so long as this preference is not based upon any previous knowledge bearing on the truth of the hypothesis, nor on any testing of any of the hypotheses, after having admitted them on probation" (6.524). The second process, "the operation of testing a hypothesis by experiment," he calls *induction*. "Induction," he specifies, "must mean the operation that induces assent . . . to a proposition already put forward . . . . Abduction must cover all the operations by which theories and conceptions are engendered" (5.590).

The problem, then, of giving a philosophical account of discovery would seem to be for Peirce to give the rules or principles of the logic of abduction. He appears to have three suggestions or "conditions" in this regard, though his account here is very elusive.

His first suggestion is that "the hypothesis cannot be admitted even as a hypothesis, unless it be supposed that it would account for the facts or some of them. The form of inference, therefore, is this:

> The surprising fact, C, is observed;
> But if A were true, C would be a matter of course,
> Hence, there is reason to suspect that A is true. (5.189)

We should, of course, recognize here an ancestor, perhaps the most important one, of the positivist principle of verification. It is quite clear, too, that Peirce believes this to be not only a characterization of abduction, but of pragmatism in general. "If you carefully consider the question of pragmatism, you will see that it is nothing else than the question of the logic of abduction" (5.196).

What are we to make of this condition? First, note that in a sense we must already have what might be called a hypothesis-candidate or a putative hypothesis before we can consider whether or not it is a *bona fide* hypothesis, i.e., a general proposition involving a "conception of conceivable possible effects" (5.196) which go beyond already-observed facts. Peirce's earlier claims are misleading if they are interpreted to imply that a method of inference is available according to which we can generate such hypothesis-candidates.

Second, the schema of abduction just given is misleading in that it suggests that the hypothesis need only account for already observed facts, presumably by showing descriptions of these facts to be deducible from the hypothesis, "taken in connection with other conceptions and intentions" (5.196). This may be a necessary condition which establishes a hypothesis-candidate as relevant, but it is not sufficient to characterize it as a full-blown hypothesis.

Third, Peirce doesn't seem to exclude the possibility that the hypothesis-candidate might conflict with hypotheses and principles already accepted. It seems to me that this is right and important, although it is not clear how much Peirce wants to include in the "conceivable possible effects" of the hypothesis. I'll return to this point in a moment.

Finally, this proposal must appear rather disappointing. It is, after all, essentially the hypothetico-deductive method. And it conflicts to that extent with Peirce's claims to have found a process for the "first starting of a hypothesis" which he contrasts with hypotheses "in regard to which knowledge already in our possession may, at once, quite justifiably either raise them to the rank of opinions, or even positive beliefs, or cause their immediate rejection" (6.524).

All of this suggests a revision of our problem, then. It was before to explain the "first starting of a hypothesis and the entertaining of it." Now it is how are we to judge a hypothesis-candidate to be a genuine hypothesis concerning the facts at hand. So far the answer Peirce suggests is that it must be relevant, i.e., capable of explaining the data

available; and also, that it has conceivable practical effects not yet tested, i.e., that we can use it to make predictions. (Of course, determining that the hypothesis-candidate accounts for the available data is already a process of initial testing and hence in Peirce's terms an initial *inductive* step. Insofar, then, abduction would appear to be an initial instance of induction.)

What now about Peirce's second suggestion? In some respects it is a continuation of the first, especially insofar as he considers the "logic of abduction" to be covered by the "maxim of pragmatism." Still his first sugestion fails to distinguish among innumerable possible hypotheses all accounting for the data at hand.

> Proposals for hypotheses inundate us in an overwhelming flood, while the process of verification to which each one must be subjected before it can count as at all an item, even of likely knowledge, is so very costly in time, energy, and money – and consequently in ideas which might have been had for that time, energy, and money, that Economy would override every other consideration even if there were any other serious considerations. In fact there are no others. For abduction commits us to nothing. It merely causes a hypothesis to be set down upon our docket of cases to be tried. (5.602)

This is a striking suggestion, namely that a principle of parsimony, or "Economy of money, time, thought and energy" should guide our entertaining of hypotheses. There is no doubt that Peirce understands this to be a fully practical issue: "The question is what theories and conceptions we *ought* to entertain" (5.594). The suggestion, then, is this. As a matter of social policy, or of individual practice and personal commitment, one ought not to bother entertaining a hypothesis if the "money, time, thought, and energy" which are likely to be needed to test it, exceed that needed by any other competing hypotheses. Alternatively, we should entertain that hypothesis (or those hypotheses) whose testing is likely to involve the least expenditure of "money, time, thought and energy." "The whole question," Peirce concludes, "of what one out of a number of possible hypotheses ought to be entertained becomes purely a question of economy" (6.529). Apparently this principle would supplement the first suggestion, not replace it, and would also require that the hypothesis-candidates already be made initially available by some other means. It would not therefore help to explain the origin or initial discovery of plausible hypothesis-candidates.

In at least one respect, Peirce is giving expression here to a venerable and widely acclaimed principle of choice which he expresses as economy of thought and which is more familiar as a principle of simplicity: all other things being equal, we should entertain the simplest hypothesis. Here, too, Peirce anticipates one of the most persuasive of current views on simplicity (Popper's), namely, (to use Peirce's words) that a hypothesis is simpler than another one if it is "*more verifiable*, that is to say, would predict more, and could be put more thoroughly to the test" (5.598). The difficulties with the notion of simplicity are notorious and there is no characterization of it, I think, which most philosophers today would agree on. Still, it does correctly express our intuitions and, presumably, if a difference of simplicity were notable, we would use it as a criterion of choice. But how are we to recognize it? Peirce, alas, is of little help.

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> Modern science has been builded after the model of Galileo, who founded it, on *il lume naturale*. That truly inspired prophet had said that, of two hypotheses, the *simpler* is to be preferred; but I was formerly one of those who, in our dull self-conceit fancying ourselves more sly than he, twisted the maxim to mean the *logically* simpler, the one that adds the least to what has been observed, in spite of three obvious objections: first, that so there was no support for any hypothesis; secondly, that by the same token we ought to content ourselves with simply formulating the special observations already made; and thirdly, that every advance of science that further opens the truth to our view discloses a world of unexpected complications. It was not until long experience forced me to realize that subsequent discoveries were every time showing I had been wrong, while those who understood the maxim as Galileo had done, early unlocked the secret, that the scales fell from my eyes and my mind awoke to the broad and flaming daylight that it is the simpler Hypothesis in the sense of the more facile and natural, the one that instinct suggests, that must be preferred; for the reason that, unless man have a natural bent in accordance with nature's, he has no chance of understanding nature at all. (6.477)

This doesn't help with the core problem of characterizing simplicity; but it does hint at justification for valuing it, namely, that the laws of nature are simple and our minds are naturally attuned to them. Here again is Mill's "natural or acquired sagacity." Of course, such a belief in the simplicity of the laws of nature is equally problematic as the principle of simplicity itself, and the burden, in any case, is thrown on the intuitions of the scientist rather than on a characterization of hypotheses, which is what we wanted. As we'll see, this idea is closely connected with Peirce's third suggestion.

But what about the notion of economy in general? In one respect Peirce is surely right. Given two or more hypothesis-candidates which account for the available data, we surely *do* budget our "money, time, thought and energy" in the most economical way. Anything else would be foolish, unless we had other kinds of reasons, kinds which we have yet to consider, such as metaphysical or aesthetic axes to grind, or broader theoretical beliefs to acknowledge – though these might somehow be worked into the notion of economy. But what reason have we to believe that the more economical hypothesis in this sense is likely to be the true one? Isn't that, after all, what should prompt us in the first place to entertain a hypothesis interrogatively?

So far as I can see, Peirce's quite general sense of economy is not justified as a principle of choice by anything but wishful thinking and convenience. However, if there is no reason to believe that the more economical hypothesis in this broad sense is more likely to be true, there's no reason *not* to suppose so. Short of further testing, this seems to me to be an excellent criterion of choice; but it is, it should be noted, accepted *faute de mieux*.

Now, before going on to Peirce's third suggestion, I would like to assess the general problem of analyzing the context of discovery. First, it seems reasonable to distinguish between two processes which Peirce tends to lump together. One is "the first starting" of a hypothesis, its initial presentation as a candidate for consideration. About this, in spite of his claims, as we have seen, Peirce has little to say. And it is clear that, in part, this aspect of discovery is the one which philosophers since Mill have generally relegated to the domain of psychology. The other process is the appraisal of the hypothesis-candidate as worthy of further consideration, i.e., as worthy of inductive testing, in Peirce's language. It is this which Peirce labels the "entertaining" of a hypothesis, "whether as a simple interrogation or with any degree of confidence" (6.525). The issue here is therefore less one of discovery that such-and-such is the case, the discovery, say, that the orbit of Mars is elliptical, or the discovery of such-and-such a general law, such as the law of universal gravitation. Rather it is the discovery that a certain hypothesis or set of hypotheses are *plausible*, that *they well may be true*, that it is worthwhile to investigate them further. Of course, this is a modest achievement, perhaps, compared to discovery that such-and such *is* the case, which is a matter, at least, of continuing investigation. One may discover that it is plausible that such-and-such is the case even though, as it turns out, it is false that such-and-such is the case. To discover a plausible hypothesis is not the same as discovering a true one. Indeed one is tempted to say that whereas plausible hypotheses are invented, true ones (or that certain ones are true) are discovered. And this may well be an origin of the persistent view that hypotheses and theories in science are of instrumental value only: no hypothesis ever passes out of the category of invention; for all science does is to increase plausibility, or decrease implausibility, which in Peirce's terms is a matter of induction or confirmation. It is, after all, worth noting this distinction carefully. Clearly there is no reason to suppose that a logic of discovery can pick out those hypotheses which will sustain prolonged testing or generate correct predictions, *prior* to our having made and checked the predictions and carried out the testing. This would be prophesy, not scientific argument.

Second, we might note that plausibility, unlike truth, is relative to a particular stage of inquiry. If Peirce is right the relevance of a hypothesis is determined in part by what sort of phenomena we are trying to account for, and its general acceptability is relative to the current state of our budget, of time, money and energy.

It would seem, then, that the philosophical problem of a context of discovery is the problem of characterizing the notion of plausibility. Plausibility, in turn, is clearly related to the notion of truth. An account is plausible if there is reason to suppose that it is true. But this may be too strong. For what are we to say about a hypothesis, on the assumption of which, to follow Peirce, some surprising fact C would be a matter of course, which nevertheless on *other* grounds we might well suppose to be false? (For such a reason, to use a classical example, the planetary system of Tycho Brahe should have been given clear preference by everyone over the system of Nicholas Copernicus.) As I will indicate in a moment this raises an ethical question for the conduct of science which forces us once again to Peirce's consideration of economy. (N)

Let me suggest an analogy at this point which I think can help to clarify the problem of finding criteria of plausibility. Surely the same question can be raised in the case of works of literary art. I don't want to get into the whole question of truth in literature, although it seems to me to be one of the central issues in philosophy of art and criticism. But it is clear enough that one can raise the question, concerning a novel, say, whether it is plausible. The point of doing so, of course, is not to ask, is the work not a novel after all, but, say, a biography or historical account (though that involves some interesting questions). We may be quite satisfied that the work is fiction (for reasons other than the line on the title page, which reads, "A novel by so-and-so"). Rather the point is to establish that the work is consistent in broad outlines with what we hold to be true, for example, about human nature; and our standards here may be (and usually are) general, common sense notions as well as educated conceptions of human psychology. The issue may arise in several ways. We may ask whether a character is portrayed in a psychologically consistent manner, or whether the characterization as a whole is sufficiently similar to known personality types or adequately exemplifies such personality types. The rub here, of course, is that much of the greatest literature succeeds precisely in upsetting our presuppositions of plausibility. Dostoevski, Kafka, Genet, enlarge our literary as well as our non-literary sensibilities and expectations; they force a revision in our criteria of plausibility. (This issue arises again, in a way quite close to our problem, in the case of fantasy and science fiction.)

This analogy reveals some striking limitations in Peirce's account of (N) the logic of discovery, or of abduction, and helps to further our characterization of it. Clearly what is missing so far is reference to other hypotheses, principles and methodological rules which form, at the time we are considering a novel hypothesis, the accepted canon of scientific knowlege. And our appraisal of alternative hypothesis-candidates will initially be in terms of their compatibility with other principles we are reluctant to give up or modify. Which ones are relevant here will depend on where the "surprising fact C" is located in our conceptual scheme, that is, where in our previously adequate account a surprise has forced us to consider revision and amendment. All that may be required is an addition of a new parameter to make a descriptive generalization more adequate to the data, as when Charles modified Boyle's law. But much more may be at stake. The significance of the suggestion of Copernicus that the planetary system might include the earth and have the sun at its center was that though it accounted for available facts of planetary movements, it was radically implausible on other grounds – it was totally incompatible with the physics of his day. We think of Copernicus, of course, as having been right. But what would prompt anyone to commit himself to the Copernican scheme a hundred years before the publication of Newton's *Principia*? The problem here is that new hypotheses (like new works of literature) may introduce radical conceptual changes in what we think of as science and accordingly change the framework within which we judge the plausibility of hypothesis-candidates of lesser scope.

But what about our problem up to the point of major theoretical revision: have we adequately characterized the limits of plausibility by indicating that the currently accepted scientific theories and principles of greater scope act as standards which guide scientific research at other levels? I suspect we have. But we have done so only on pain of showing the "context of discovery" at this level to be continuous with and scarcely distinguishable from the "context of justification." In fact, our principle of plausibility becomes an extremely conservative principle which prejudices the acceptability of any hypothesis-candidate which requires an enlargement of the conceptual scope of current science. It is, to use Thomas Kuhn's phrase, a principle for puzzle solving within the confines of normal science.[7]

What about major theoretical revisions, then? Well, again, an answer

depends on how major a revision is in question. There seems some reason to require that any new suggestion accord with, for example, the cconservation principles, such as those of energy, momenta, or spin, and perhaps with the "so-called *extrema principles* . . . which express the a priori view that nature is economical in its expenditure of energy."[8] But it is difficult to see what *empirical* value such principles would confer on a novel hypothesis or even how they differ much from what may be called metaphysical principles. And it is well known that these latter, such as principles of constancy, conservation, quantification, atomicity, potency or force, etc., have been adduced by scientists in *support* of revolutionary hypotheses when currently accepted theories weigh against them. And, presumably, the accepted theories satisfy such criteria, too, as well as being in general superbly adapted to the current grab-bag of facts. (And how, after all, are we to judge between competing metaphysical principles?) Even if the accepted theories are currently troubled by what Kuhn calls "anomalies" and Quine labels "racalcitrant facts" which are not fully expected given present presuppositions, we are not entitled to make too much of them as a matter of principle. After all, the new candidate inevitably has its own troubles, and who is to say that the accepted theory won't surmount its difficulty with simplicity preserving modification. In short, it does not seem to me possible to formulate a *general* criterion which would separate out what Martin Gardner all too quickly dismisses as the fads and fallacies of cranks. We can, however, demand of any such seemingly absurd candidate for status of scientific theory (as, of course, Copernicus' contemporaries viewed his hypothesis) that it be extended, articulated, tested and further developed before we begin to give it serious consideration: so much, however, a serious theorizer would surely expect of himself, and so much we can explicate in terms of Peirce's notion of induction.

There is a marvelous story illustrating these points told by Freeman Dyson about a lecture given by Wolfgang Pauli outlining an unorthodox theory suggested by him and Heisenberg. In the discussion after the lecture, many of the younger scientists were sharply critical of Pauli, and at one point, Neils Bohr, who was also present, rose to speak. "We are all agreed," he said to Pauli, "that your theory is crazy. The question which divides us is whether it is crazy enough to have a chance of being correct. My own feeling is that it is not crazy enough." Dyson then comments: "The objection that they are not crazy enough applies to all the attempts which have so far been launched at a radically new theory of elementary particles. It applies especially to crackpots. Most of the crackpot papers which are submitted to *The Physical Review* are rejected, not because it is impossible to understand them, but because it is possible. Those which are impossible to understand are usually published. When the great innovation appears, it will almost certainly be in a muddled, incomplete, and confusing form. To the discoverer himself it will be only half-understood; to everybody else it will be a mystery. For any speculation which does not at first glance look crazy, there is no hope."[9]

But suppose, now that such a theory-candidate is proposed and developed and is shown to be capable of accounting for a wide range of phenomena currently explained by the accepted theory while conflicting with it over some other range of phenomena including some not yet tested for either theory? Wouldn't Peirce's criterion of economy still rule out the upstart? Perhaps. But I know of no general procedure here for weighing the economical burden in time, money and energy of encouraging the development of some new theory. It is possible that the development of new theories in some areas of science might be extremely costly, and I don't see how in general one would decide when they become respectable enough to warrant at least partial commitment. On the other hand, current data are cheap and unless a new theory can in a general way account for them we needn't worry about economy. However, if it can do this much, it no doubt can already begin to pay its own way, namely, by providing a detailed challenge to the accepted theory in those areas where the competing theories predict different experimental results. The importance of such challenges is very great; for they may well lead to the development of new research and instrumentation to resolve the conflicting predictions, or even lead to uncovering of phenomena long ignored by the accepted theory which reveal in it inherent limitations so far unnoticed. All of which justifies a fundamenal principle of tolerance which should modify the conservative effect of any principle of plausibility or economy.

We started out with the problem of characterizing the logic of discovery which seemed also to be Peirce's object in outlining a logic of abduction. The issues seemed better stated, however, as explaining not how hypotheses first get proposed, but how they may be evaluated initially as plausible or implausible. As far as they go, Peirce's suggestions here are quite reasonable; but they don't help in cases of major theoretical revision. Plausibility arguments are possible only within some antecedently accepted framework of beliefs, and major changes in conceptual scheme are precisely changes in such frameworks. Still, the entrenchment of such major theoretical views is likely not always to encourage the discovery of whatever difficulties or limitations they may harbor. And this is a sufficient reason for encouraging the development of broadly competing alternatives.

But now a version of our original question occurs again. Whence come such alternatives? Is it only a matter of the spontaneous or chance occurrences of the novel ideas of genius – a field of investigation for empirical psychology? I have no doubt that the answer is at least partly, yes. On the other hand, in spite of the fact that there seems no logical limit to the number of hypotheses which might have been proposed at various moments in the history of science, in fact their supply has always seemed remarkably scarce. Peirce's third suggestion, which I have delayed discussing until now, addresses itself to this problem in an extremely interesting way which makes it worth quoting him at length.

> How is it that man ever came by any correct theories about nature? . . . You cannot say that it happened by chance, because the possible theories, if not strictly innumerable, at any rate exceed a trillion – or the third power of a million; and therefore the chances are too overwhelmingly against the single true theory in the twenty or thirty thousand years during which man has been a thinking animal, ever having come into any man's head. If you carefully consider with an unbiased mind all the circumstances of the early history of science and all the other facts bearing on the question, which are far too various to be specifically alluded to in this lecture, I am quite sure that you must be brought to acknowledge that man's mind has a natural adaptation to imagining correct theories of some kinds, and in particular to correct theories about forces . . . . (5.591)

Peirce's suggestion is that there are innate, perhaps instinctual, limitations to human imagining or conceptualizing which are the product of something like evolutionary adaptation. Aside from the very doubtful analogy to animal instinct, however, Peirce's claim rests on an assumption which comes close to begging the whole question, namely, that we have in fact in the last twenty or thirty thousand years imagined a correct theory. The chances would indeed seem slim of the one true out of a trillion theories popping into a man's head, though such, roughly, may be the chances of life at all. But what grounds do we have for supposing with any certitude that our theories are true? Even Peirce considers the true theory to be "the opinion which is fated to be ultimately agreed to by all who investigate" (5.407), and nothing, so far as I can see, precludes the possibility that all investigators should agree on a false theory, no matter how long the investigation continues. It is not, therefore, the truth of our theories that might prompt us to look for limitations to the human imagination, but the lack of any sizable number of them. And even that is suspicious. After all, if we only require our theories to be adequate rather than true, it seems less surprising that we should have formulated a fair number so far (if we choose to be generous), and given their required complexity, a principle of least action or laziness may do as well to explain their meager numbers (if we choose to be stingy). But how many is meager? Less than a trillion and more than one? How many should we expect? One problem here is that no one has tried to count them, or even spelled out clearly what would count as one. And contrary to Peirce's preference for correct theories, surely we must add all those which have been tried and failed. This, of course, brings us back to the question of what counts as a try (or a good, or serious try), which is the general question of a logic of abduction applied to the full range of scientific theorizing.

It may well be that Kant, Whewell, Peirce and Chomsky are right in suggesting that there are specific inherent limitations to human conceptualizing, though they are not, I think, remarkably persuasive. However, the problem is much too important and interesting to leave to the devises of psychology, as Mill and Popper would have us do. After all, the task of characterizing man's "natural and acquired sagacity"[10] is surely the fundamental problem of philosophy.

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NOTES

1. John Stuart Mill, *A System of Logic Ratiocinative and Inductive*, edited by J. M. Robson (Toronto: University of Toronto Press, 1973), p. 284.
2. *Ibid.*, pp. 304-5.
3. *Ibid.*, p. 285.
4. Hans Reichenbach, *Experience and Prediction* (Chicago: University of Chicago Press, 1938), pp. 6-7.
5. Karl Popper, *The Logic of Scientific Discovery* (New York: Basic Books, 1959), p. 31.
6. The references following quotations from Peirce's writings are to the *Collected Papers of Charles Sanders Peirce*, edited by C. Hartshorne and P. Weiss (Cambridge, Massachusetts: Harvard University Press, 1931-35). The selections referred to in this essay are conveniently collected in Justus Buchler (ed.) *Philosophical Writings of Peirce* (New York: Dover, 1955), Ch. 11, and in Vincent Tomas (ed.) *Peirce's Essays in the Philosophy of Science* (New York: Liberal Arts Press, 1957), Ch. XIII.
7. See Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1970).
8. George L. Farre, "On the Linguistic Foundations of the Problem of Scientific Discovery," *Journal of Philosophy* LXV (1968), p. 794.
9. Freeman J. Dyson, "Innovation in Physics," *Scientific American* 199 (September, 1958), pp. 79-80.
10. Mill would not have agreed entirely with Peirce's emphasis on "instincts." "I disclaim, as strongly as Dr. Whewell can do," Mill writes, "the application of such terms as induction, inference, or reasoning, to operations performed by mere instinct, that is, from an animal impulse, without the exertion of any intelligence." *Op. cit.*, p. 287.