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Historical Note: Paul Carus

Paul Carus, the first editor of THE MONIST, was born in Ilsenberg am Harz on July 18, 1852, and died in LaSalle, Illinois, on February 11, 1919. After receiving his Ph.D. degree in philosophy and classical philology from Tübingen University in 1876, he taught briefly at the State Military Academy at Dresden. In search of freedom for expression of his independent views, he migrated first to England and then to the United States. In 1887 he accepted the invitation of Edward C. Hegeler (who later became his father-in-law) to edit the Open Court Magazine, a monthly journal devoted primarily to comparative religion. In 1888 the Monist was established as a quarterly journal of the philosophy of science, and Paul Carus served as editor of both journals and as editor of the Open Court Publishing Company until his death in 1919.

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GENERAL TOPIC: *The Relevance of Charles Peirce*

ABDUCTION AND THE NEW RIDDLE OF INDUCTION

Although the relevance and importance of his work has been recognized only belatedly, Charles Sanders Peirce was, throughout his life, a careful student and significant contributor to the development of logic, scientific theory, and philosophy generally. Occasionally, complete appreciation of Peirce's efforts has been hampered because his work is often unique and, at times, highly idiosnycratic. Yet, we hope to show in this paper that for one aspect of his work in logic Peirce did not abandon the ordinary without purpose. Only relatively recently have philosophers of science become interested in the logic of discovery—that is, in the logic of the *selection* of hypotheses to be tested rather than simply in the ways of testing hypotheses which have already been selected.

We intend to show that there are logical problems in the selection of hypotheses, that Peirce was very clear-headed about what these problems are, and that he provided us with some of the best suggestions yet available on how to deal with these problems.

It is well known that Peirce eschews the traditional division of inference into deductive and inductive. Instead, he divides all inference into explicative and ampliative. While the former is simply equivalent to deduction, he subdivides the latter into abduction (also called hypothesis, retroduction, and presumption) and induction, giving three kinds of inference in all. Peirce's view of deduction is traditional in that it includes mathematics and formal logic;[1] however, in the case of *abduction*, Peirce singles out as an independent form of inference the formulation of hypotheses for inductive testing. All this is well known, but, we fear, too much ignored outside the constricted space of Peirce scholarship. Unfortunately, the notion of abductive inference, which is peculiarly Peirce's, has not exerted an influence proportionate to the significance of its insight.

Peirce, however, leaves no doubt about what he thinks of the importance of his distinction. He writes,

> Nothing has so much contributed to present chaotic or erroneous ideas in the logic of science as failure to distinguish the essentially different characters of different elements of scientific reasoning; and one of the worst of these confusions, as well as one of the commonest, consists in regarding abduction and induction taken together . . . as a simple argument (7.218).

Nevertheless, most current discussions of the problem of induction proceed without reference to Peirce's critical distinction—all to their loss, we believe.

We hope to take a small step toward rectifying this state of affairs by demonstrating the relevance of Peirce's analysis of inference to a significant problem in contemporary philosophy of science—specifically, to Nelson Goodman's "new riddle of induction." However, before proceeding to the central part of our discussion, a brief summary of the relevant features of Peirce's analysis of inference may prove helpful.

I

Unlike many philosophers of science, Peirce was a scientist by training and vocation. Perhaps, for this reason his investigations often take a broader, less rarefied view of the actual processes of scientific inquiry. While not simply formal, Peirce's analysis is grounded in general considerations of epistemology—especially in a prior analysis of the role of belief and doubt in the reasoning process. In fact, Peirce defines inference as "the process by which one belief determines another." "A belief," he continues, "is itself a habit of mind by virtue of which one idea gives rise to another" (7.354). A habit is a general rule or disposition; therefore, not only is a belief a certain disposition; but beliefs can also be determined by a general rule or habit (*CP*, 5.411). Peirce designates those general rules which determine inferences "leading" or "guiding" principles. Critical logic is largely an examination of these principles.

Belief, which plays a crucial role in his theory of inference, has, Peirce observes, the important property that it appeases doubt relative to the same matter. While the fact that feelings and states of mind are not necessary to the analysis of belief, it does suggest that what is necessary and distinctive is that belief leads to action given the appropriate circumstances, while doubt inhibits it (2.242). This suggests that the logical relationships of belief and doubt are the important aspects for Peirce.

Doubt exists only relative to belief. The source of doubt for Peirce is surprise (5.512, 5.543). Generally, however, Peirce reduces doubt to an awareness of any inconsistency in one's beliefs. Inconsistencies are characterized by the following logical conditions of doubting: A person A doubts p (a proposition expressing a belief) if, and only if, A is led to believe p and some other proposition q and A is aware of an inconsistency between p and q.[2] That is, that taken with the set of one's other beliefs such inconsistencies would lead to contradictory expectations or anticipated actions. Hence doubt exists only if there is belief.

"Logic," Peirce writes, is "the theory of the conditions which determine reasonings to be secure" (2.1). Inferences "fix belief" in that their conclusions are secure against doubt. As already observed, Peirce distinguishes three

varieties of inference—deduction, abduction and induction. Let us now consider in detail the latter two sorts.

Abduction is the process of initially setting up or entertaining a hypothesis likely in itself (7.202). The form of an abductive inference is as follows: "The surprising fact, C, is observed; but if A were true, C would be a matter of course. Hence, there is reason to suspect A is true" (5.189, also cf. 2.624). Abductions carry no guarantee of their truth; they must be tested before anyone is justified in accepting them. Except for two considerations, they should hardly seem to deserve the name "inference."

Most relevant to our present purpose, Peirce holds that there is an *icon relationship* between the facts of the hypothesis and the facts observed. He cites as an example the likeness between the hypothesis of an elliptical orbit and the data about Mars which led Kepler to adopt the elliptical orbit in his theory of planetary motion. Peirce writes that "Presumption (i.e., abduction) is the only kind of reasoning which supplies new ideas" (2.777). Not only then is abduction fundamental to any increase in our knowledge, but it also has definite relational structures and leading principles which can be treated by critical logic.[3]

The third type of inference which Peirce identifies is induction. He holds that induction is nothing more than the testing of a hypothesis or theory by checking the occurence of lack thereof of a prediction deduced from it.[4] Although Peirce claims that conflating abduction and induction is the greatest source of confusion in science (7.218), neither he nor his commentators are completely free of this confusion themselves (e.g., cf. 2.759 and 5.170). The reason is not difficult to find.

The process, at first glance, is straightforward enough. An *abduction* yields a hypothesis; various consequences are derived from it by deduction; and these expected consequences are tested through *induction*. The three types of inference are most easily confused when the hypothesis is contradicted by induction, but only small modifications in the hypothesis are required to adjust it to account for new evidence. Peirce clearly believes that this process of adjustment is abductive, although it is often mistaken for induction (7.114). In scientific inquiry, the three types of inference are closely integrated (though distinct nonetheless) into one unified process. It is, therefore, understandable if occasionally the two are confused, especially when the question is not simply the validity of each type of inference, but rather the justification of the whole of scientific inquiry.

As an inference, induction has the psychological effect of fixing belief in a certain hypothesis. It becomes ever more difficult to doubt the truth of a hypothesis that is confirmed in ever more instances (2.96, 7.218). Logically, on the other hand, induction has a less powerful role. Every induction tests

the conclusion of some abduction, and provides the grounds for supporting it. Because abduction suggests a hypothesis only in response to some surprising fact, there is, for any properly drawn hypothesis, already some positive support. The lack of any positive support, then, provides grounds *ipso facto* on which to doubt the hypothesis. Nevertheless, the role of induction is only to give an opportunity for nature to refute or falsify a hypothesis. No number of positive instances can demonstrably prove that a hypothesis is true (2.663).

II

Unfortunately, inference in scientific theory and ampliative inference generally have not often been discussed in Peirce's terms. The practice of treating all amplicative inferences as induction, which has dominated philosophical discussion of scientific theory to date, is particularly fraught with problems which, we suggest, Peirce's theory goes some way towards resolving.

The "traditional" problem of induction is usually described as the attempt to justify inferences concerning *un*observed events or phenomena on the basis of observed events or phenomena. What justification is there in inferring that what we have found to be true in observed cases will continue to be true in yet unobserved cases? This query is usually traced to David Hume and his claim that inductive reasoning cannot be justified. Various philosophers have tried to defend induction against Hume's skeptical attack; however, none of these attempts to defend induction has gained universal approval by the philosophical community, and the Humean stigma has remained with induction.

There, matters have stood until relatively recently when Nelson Goodman raised what he calls "the new riddle of induction."[5] Hume's analysis rules out certainty in ampliative inference and also any notions of truth or knowledge which require such certainty. Goodman expresses agreement with this consequence: "If the problem is to *find* some way of distinguishing antecedently between true and false predictions, we are asking for prevision rather than philosophical explanation" (*FFF*, p. 62). Hume was on the right tract but did not go far enough. He overlooked the fact, according to Goodman, that not *all* regularities produce beliefs or habits. For Hume, the problem of induction involved trying to justify how any set of observations *ever* justifies a claim about unobserved events. Goodman's new riddle of induction recasts the problem with the even more disturbing results that *every* claim about unobserved events based on *any* set of observations is *equally justified*.

Let us now examine Goodman's argument. The problem of "justification" for Goodman is really a problem of determining which inferences are valid and which ones are not. According to Goodman, a deductive argument is justified "by showing that it conforms to the general rules of deductive inference" (*FFF*, p. 63). Analogously, he suggests, the problem of justifying an inductive argument really amounts to showing that it conforms to the general rules of inductive inference (*FFF*, p. 63). The general rules, in turn, are "justified" because they yield "acceptable" inferences. The circularity, Goodman claims, is virtuous rather than vicious.

Such an approach dissolves Hume's way of putting the problem because we will no longer be concerned with attempting to answer such spurious questions as how a certain kind of knowledge is possible. The "new" problem becomes one of distinguishing between valid and invalid predictions and of *defining* the difference between them (*FFF*, p. 65). Such a task will amount to explicitly stating the principles and canons of induction which will define the confirmation relation in an analagous manner to the way in which the laws of deduction define the consequence relation (*FFF*, p. 67).

Goodman's argument is illustrated by his well known "grue paradox." Consider a new predicate, 'grue', which is defined as applying to "all things examined before [some specified time] t just in the case that they are green and to other things just in the case they are blue" (*FFF*, 74).[6] 'Green' and 'blue' are completely inter-definable with 'grue' and 'bleen' (blue before t or green thereafter). Thus, 'green' is whatever is grue before t and bleen afterwards (*FFF*, 79,80). The problem, says Goodman, is that if certain emeralds are examined before t they are both green and grue. Why should the hypothesis 'all emeralds are green' be chosen over the hypothesis 'all emeralds are grue'? Why should an emerald after t be expected to be green rather than grue (and, therefore, blue)? Both predications cannot be correct because they lead to contradictory expectations. The problem is why the one hypothesis is *lawlike* and the other not, or to put it in other terms, why the one is projectible and the other not.

The way out of the paradox according to Goodman is one of distinguishing "lawlike" hypotheses from others, because only "lawlike" hypothese are confirmed by their positive instances and hence justify projection. In order to define "lawlikeness" and resolve the "new riddle," Goodman offers the theory of entrenchment. He begins by defining *actual projection*. A hypothesis is actually projected if it is adopted after some of its instances have been determined true and the rest are yet to be determined (*FFF*, 87). Positive instances constitute the evidence class; undetermined instances, the projected class. A hypothesis with some positive instances is sup-

ported; with some negative instances violated; and with no undetermined instances, exhausted. Adoption of a hypothesis is actual projection when the hypothesis is supported, unviolated and unexhausted (*FFF*, 90).

On this account it is possible that two hypotheses could both be actually projected, conflicting in their prediction, but both supported and unviolated. For example, any evidence t which supports "all emeralds are green" also supports "all emeralds are grue." In order to decide between them, Goodman suggests that the one with the better entrenched predicates be projected. A predicate becomes better entrenched than another by having been used in the past in more actually projected hypotheses. Green is the "veteran" of many past projections and grue of very few, if any; hence, "all emeralds are green" should be projected (*FFF*, 95).

By its very nature, projection must be made without regard to the truth of the prediction. Goodman observes: "The criterion for the legitimacy of projection cannot be truth that is yet undetermined. Failure to recognize this was responsible . . . for some of the worst misconceptions of the problem of induction" (*FFF*, 99). The projected class consists of instances whose truth is undetermined, otherwise there would be no call for projection at all.

Entrenchment, then, is not a matter of truth, but of linguistic practice. Goodman writes: "The reason why only the right predicates happen so luckily to have become well entrenched is just that the well entrenched predicates have thereby become the right ones" (*FFF*, 98). His claim here only expresses one side of a circular notion. On the other side, if hypotheses using a particular predicate are continually violated, then that predicate loses entrenchment relative to others. So it is equally true that the right predicates (in the sense of those conforming to experience) become the entrenched ones or, at least, the wrong predicates do not become entrenched.

In posing his new riddle of induction, Goodman has conflated the two very distinct processes of scientific theory which Peirce was so intent upon distinguishing—namely, the formulation of hypotheses (i.e., abduction), and their testing (i.e., induction). We hope in the following section to demonstrate that Peirce's theory of inference anticipates Goodman's "new riddle," analyzes it more adequately, and resolves it more completely. We intend to show that the "new riddle of induction" is neither new nor a riddle of induction.

III

Considering Goodman's account of induction from Peirce's point of view, it seems obvious that Goodman confuses induction with abduction and

also confuses the logical and psychological aspects of confirmation and testing. Nevertheless, there are points of contact between Goodman's and Peirce's accounts: Goodman's notion of projectibility involves the same problems as Peirce's account of abduction.

It is obvious that if Peirce's division of inference is accepted, the problem of selecting between competing hypotheses (e.g., the predication of 'green' or 'grue' to emeralds) or of projecting or not projecting a single hypothesis (e.g., "all men in this room are third sons") is a question of abduction—not of induction. Induction involves only the testing of hypotheses once projected, while abduction is the first setting up or entertaining of any hypothesis as likely in itself. Let us use Peirce's analysis of inference (especially of induction), to examine Goodman's theory.

Goodman's approach emphasizes language. Entrenchment accrues against the entire background of linguistic practice. In contrast, Peirce emphasizes belief in his pragmatic approach to inference, but the difference is really more one of vantage point than of substance. Just as Goodman recognizes that we intuitively avoid projection of 'grue', Peirce notes that our instinctive logical practice mitigates the need for critical logic in ordinary affairs (5.368). This instinctive logical practice is part of that important body of indubitable (i.e., never actually criticized) beliefs, which, he says, are indubitable only insofar as they apply to the primitive mode of life (5.445). These beliefs have withstood numerous potentially doubt inducing experiences and have not yielded to them. So long as present experience resembles this past experience, there will be no cause to doubt these beliefs. But when inference in science or any non-ordinary situation goes beyond common experience, the beliefs are no longer indubitable and critical logic is required (5.511, and 5.368). The process of continual adjustment between beliefs, inference and experience in Peirce's presentation parallels that in Goodman's treatment of the entrenchment of predicates. Using Peirce, Goodman's analysis of entrenchment can be reformulated: The right predicates have become entrenched because they are the ones which reflect our beliefs about the world.

In his theory of entrenchment, Goodman provides for the possibility of change in the relative entrenchment of predicates in the face of greater experience (*FFF*, 64, 98, 106ff). This parallels closely Peirce's suggestion that one's indubitable beliefs change as one's mode of life becomes more sophisticated (5.545). These same observations apply equally to the general requirements for validity. Goodman states: "A rule is amended if it yields an inference we are unwilling to accept; an inference is rejected if it violated a rule we are unwilling to amend (*FFF*, 64). Such recalcitrance can be expres-

sed in Peirce's terms as the conflict between a belief that something is the case and a belief in the inference process, on the one hand, and a belief in the validity of the rule of inference and in the truth of the prediction, on the other.

Other, more detailed parallels between projection and abduction arise from Goodman's definition of "actual projection." According to Goodman, a hypothesis can be actually projected only if it is supported, unviolated and unexhausted. The requirement that the hypothesis must have some support prior to its projection is exactly the same as Peirce's requirement that an abduction explain some surprising fact.

Goodman's other requirements—that the hypothesis be unviolated and unexhausted—are very similar to Peirce's claim that predictions of a theory be designated in advance of testing (2.790, and *FFF*, 90). Both claims are based on the fact that the truth of any genuine ampliative inference is not yet determined (5.584). A hypothesis should pose a question to nature, not merely a catechism.

As already observed, Goodman accuses Hume of having overlooked the fact that not all regularities produce belief or habits. Similarly, Peirce observes that ". . . any two things resemble one another just as strongly as any two others, if recondite resemblances are admitted" (2.634). Both Peirce and Goodman hold, then, that not only must the existence of regularity be asserted, but also the relevant regularity must be specified (*FFF*, 77, 2.790, also cf. *FFF*, 61). This is Peirce's rule of predesignation, and it is what makes it possible in Goodman's theory to speak of support, violation and exhaustion.

A rule of predesignation, however, is no more than the claim that, for Peirce, an *abduction* must precede an induction. The failure to recognize this produces the same confusion which Goodman recognizes with the grue paradox. Peirce uses the following example:

> A chemist notices a surprising phenomenon. Now if he has a high admiration of Mill's *Logic*, as many chemists have, he will remember that Mill tells him that he must work on the principle that, under precisely the same circumstances, like phenomena are produced. Why does he then not note that this phenomenon was produced on such a day of the week, the planets presenting a certain configuration, his daughter having on a blue dress, the milkman being late that morning and so on?

But Peirce's solution makes no formal mechanistic appeal to entrenchment indexes; rather he writes:

> The answer will be that in early days chemists did use to attend to some such circumstances, but that they have learned better (6.413).

As the alchemists's abductions were falsified by formal or informal inductions, the more current beliefs of today—upon which the modern chemist's abductions are based—have replaced them.

That ampliative inference is best thought of in terms of abduction (hypothesis) and induction (testing) rather than under a single rubric of induction is never explicitly admitted by Goodman. Yet, it suits his theory: The old riddle of induction is truly a question about induction; but the "new riddle" is better understood as a problem of abduction. Goodman does admit—indeed emphasizes—that the "traditional" theory of induction cannot solve the grue paradox. This failure of induction to handle the paradox is the reason for Goodman's theory of projectibility. However, the whole theory of projectibility and the choice between a grue theory and a green theory all boils down to a choice between hypotheses. Here, it seems, we have a clearly identifiable issue in the logic of discovery. That Goodman does not separate projection as part of an abductive stage from confirmation as part of an inductive stage of inquiry has its roots in his confusion of the psychology and the logic of belief. In this regard, Peirce's analysis aids in understanding the deficiencies of Goodman's account.

The primary area in which Goodman makes this crucial confusion is his notion of confirmation. His view is explicitly stated when he writes: ". . . affirmation [of a hypothesis] as certainly true is not demanded, but rather something like affirmation as sufficiently more *credible* than alternative hypotheses" (*FFF*, 88, our emphasis). The role of credibility in the process of projection is reflected in three claims which Goodman makes: First, that a greater number of positive instances makes a projectible hypothesis more likely; secondly, that entrenchment can theoretically be quantified and used as an efficacious tool for the selection of hypotheses; and finally, that confirmation like consequence is a logical relation (*FFF*, 67–72, 84, 108, 118, 119).

The credibility of a hypothesis is a subjective psychological standard which may or may not be correlated with the future facts predicted by it. As Goodman himself noted, absolute certainty in prediction would be prevision not hypothesis. Peirce objects that not only is the likelihood or plausibility of a hypothesis not a certain indicator of its truth, it is also not possible to give it a valid numerical measure (2.662, 2.663). "Likelihood" is merely an indicator of the agreement or disagreement of preconceived ideas with the hypothesis (7.220). As such, it may serve to guide research but not to determine precisely which hypotheses ought to be projected.[7] Validity for Peirce is a logical not a psychological matter. Since positive instances impart no logical necessity that an inference will turn out true, it is only negative instances in the class of available evidence which affect the logical question of

validity. A rule of inference is not necessarily true simply because it is supported, but it is certainly false if it is violated. It is disappointed expectation which provides the logical grounds to doubt an accepted inference.

The confusion of the psychology and logic of belief and the emphasis of positive over negative instances leads Goodman to believe that theoretically, a mechanical procedure would permit selection between rival hypotheses. Each hypothesis could be given, in theory, a comparative index of projectibility based upon the entrenchment of the predicates it employs and the comparative projectibility of hypotheses related to it heirarchically (e.g., "all bagfuls of marbles in Utah are uniform in color," if it is otherwise "actually projectible" contributes to the comparative projectibility of "all bagfuls of marbles in stacks S are uniformly of some warm color," if S is in Utah [*FFF*, 111]). The hypothesis with the highest comparative projectibility would be projected over its rivals supported by the same evidence. That this cannot be done in practice, requires that actually projectible hypotheses be distinguished by the following rule: "A hypothesis is *projectible* if all conflicting hypotheses are overridden, *unprojectible* if overridden, and *nonprojectible* if in conflict with another hypothesis and neither is overridden."[8] A hypothesis is overridden by a contrary overhypothesis which employs better entrenched predicates (*FFF*, 110–18, esp. 117).

Peirce does not see the simple collection of ever more positive instances as increasing the likelihood of the hypothesis. In fact, he observes that inductive certainty may be very similar to deductive certainty. One test may establish a qualitative result for a chemist, just as one proof is enough for a mathematician. On the other hand, even with a deduction a student may check his work several times (5.580). The degree of belief in an inference does not follow from any simple mechanical procedure. So it is unlikely that even with more careful development that an exact, quantified index of entrenchment could be usefully assigned to predicates (cf. *FFF*, 119). Rather, it is economy of money, time, thought and energy instead of formal rules or entrenchment indexes which should guide the abductive/inductive procedure, says Peirce (5.6000, 2.780).

Projection of a hypothesis, for Peirce, could only mean taking it up for inductive testing. Not only, then, is it possible on Peirce's account to be unconcerned about fine distinctions of entrenchment, but it may be the wisest procedure. Using Peirce's method, a scientist would check only those hypotheses which were consistent with his current beliefs which could be tested in a reasonable time, within his means, and so forth. Since within the scientific community, other scientists would be working on the same problem from various perspectives and under differing circumstances, a greater range of possible hypotheses would be examined. This is true in the forefront of

science, where no body of shared beliefs is commonly agreed upon. It is even more true in a situation in which many unaccountable results have led one or more mavericks to suggest that some agreed upon beliefs be reexamined—the situation of Copernicus, Newton, Darwin or Einstein.

Goodman does not claim that a projected hypothesis must be correct. Although he often seems to forget it, a hypothesis must be subjected to some testing to be even logically acceptable and probably to a great deal of testing to be psychologically satisfying (5.599). Yet, if Goodman's rules were followed, each scientist would test the most projectible hypothesis and move on to the next one only if it were violated. In an ideal scientific community, in which every member had complete information on the operations of the other members, exact indexes of entrenchment and projectibility would suggest that the same hypothesis be projected and tested by each investigator at a given time. Such a conservative procedure would militate against radical changes of belief that have often advanced science, as well as restrict normal scientific progress. Even in a real scientific community such a procedure would severely restrict the scope of investigation.

Suppose a scientist faces a choice between two otherwise actually projectible hypotheses, H_1 and H_2, where H_1 overrides H_2 (i.e., H_1 employs better entrenched predicates than H_2), then according to Goodman, the scientist should project and proceed to test H_1. But suppose also that H_1 is of the nature that it will take a long time, say ten years, and a great deal of expense to test whereas H_2 is of the nature that it will take a relatively much shorter time, say a few days, and much less expense to test. Peirce's dominating concern with economy would require that H_2 be tested first even though it is overridden.

Similarly, if a particular hypothesis, e.g., H_2, accounts for more of a greater variety of phenomena than another hypothesis, H_1, then again economy in the scientific process requires us to test H_2 first to "save repetitious work" (7.221). In place of Goodman's mechanical procedure for choosing hypotheses for inductive testing Peirce suggests a much looser alternative. His dictum is, "Don't block the path of research." (1.135).

Goodman's "new riddle of induction" has justifiably provoked as much excitement and critical response from philosophers of science and epistemologists as any other single development in theory construction in the last quarter of a century. It is a measure of Peirce's greatness that he anticipated, in a very general way, Goodman's problem and dealt with it in a more illuminating way by clearly separating Goodman's question of the selection of hypotheses (abduction) from Hume's problem of the confirmation of hypotheses (induction). Once Goodman's riddle is properly understood as a problem of abduction, theory construction and the logic of scien-

tific discovery, its significance becomes clear. What Goodman has valuably demonstrated is that abduction is, as Peirce had claimed, a different form of reasoning. The important result is to shift our out attention from problems of confirmation, induction, and David Hume to problems of theory construction, abduction and Charles Sanders Peirce.

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NOTES

1. References are to Charles S. Peirce, *Collected Papers*, Volumes I–VIII, edited by Charles Hartsborne, Paul Weiss and Arthur Burks, (Cambridge: Harvard University Press, 1931–58). All references to the *Collected Papers* are in the standard form, citing only the volume number, decimal point and paragraph number. Peirce also includes the categories of necessary inference, probable and statistical deductions. These are simply necessary deductions with probabilities as their subject matter (2.785, 2.694).

2. Formulated this way by Robert G. Meyers, "Peirce on Cartesian Doubt," *Transactions of the Charles S. Peirce Society*, 3 (Spring 1967): 16–17.

3. A second reason for treating abduction as a type of inference is that it has a degree of determinate force over our beliefs. This is especially true when it operates in the guise of sense perception which Peirce classifies as a limiting case of abduction.

4. This is Peirce's fully developed view. In earlier writings, he held that induction has a different but equally creative role as abduction (cf. 2.263, 2.264, 2.640, 6.709). We cannot trace here the development of Peirce's thought. For reference, see Arthur Burks, "Peirce's Theory of Abduction," *Philosophy of Science*, 12 (1946); Harry Frankfurt, "Peirce's Notion of Abduction," *Journal of Philosophy*, 45, 1958; and K. T. Fann, *Peirce's Theory of Abduction*, (The Hague: Martinus Nijhoff, 1970), pp. 20ff.

5. Nelson Goodman, *Fact, Fiction and Forecast*, Second Edition, (New York: Bobbs-Merrill, 1965). Chapter III. Hereafter *FFF* followed by page number.

6. Most of Goodman's critics have used various restatements of this definition. Many of them have unwittingly employed definitions which alter the meaning of 'grue,' Because the grue paradox is but an illustration of the new riddle of induction (of which other illustrations can be constructed), this issue is of relatively minor interest in the present context. Whether or not the supposed untoward results follow if Goodman's definition is taken strictly is debatable. On both points, see Frank Jackson, "Grue," *Journal of Philosophy*, 62 (March, 1975).

7. In 1878, Peirce claimed that probability ought to be connected with the psychological feeling of belief in a hypothesis (2.676). In 1902, however, he pointed out that probability has only a doubly indirect relation to the validity of abduction (2.102).

8. Nelson Goodman, Robert Schwartz and Israel Scheffer, "An Improvement in the Theory of Projectibility," in Nelson Goodman, *Problems and Projects* (New York: Bobbs-Merrill, 1972), p. 390. This is Goodman's latest revision of what had been three rules in the first and two rules in the second edition of *FFF*. Each revision, he claims, only removes redundancy and adds clarity.