

# GOODMAN AND THE DEMISE OF SYNTACTIC AND SEMANTIC MODELS

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## 1 HISTORICAL BACKGROUND

The “problem of induction” in its modern form is most often traced back to David Hume [2006]; (See 2, 3.<sup>1</sup>) To put the issue in terms we will make use of later, consider these two arguments:

- I. 1. All emeralds are green  
2.  $a$  is an emerald  
∴  $a$  is green.
  
- II. 1. Emeralds 1, 2, 3, . . . , 999 are green  
∴ All emeralds are green

Both seem to be good arguments, although there are significant differences between them. In Argument I, the conclusion follows deductively from its premises. If the premises are true so must be the conclusion. Rules of logic justify inferring one from the other. Not so with Argument II. No matter how many emeralds are examined and found to be green, it does not follow logically that all are. The question then is what sanctions or justifies concluding “All emeralds are green” in Argument II?

One might claim that the conclusion is warranted on the grounds that it is an *a priori* truth; it states a relation of ideas. “Green” is part of the meaning of “emerald”, so of necessity all such gems are green. In that case, though, premise 1 in argument II plays no real role. There is no need to appeal to any empirical evidence to underwrite the conclusion, and there is no problem of induction. If, however, “All emeralds are green.” is taken to state an empirical fact, then

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<sup>1</sup>Chapter numbers to articles appearing in this volume are in italics. Two anthologies, *The Philosophy of Nelson Goodman: Nelson Goodman's New Riddle of Induction* [Elgin 1997] and *Grue: the New Riddle of Induction* [Stalker 1994] present both early and more contemporary papers on the topics to be discussed. *Grue* is particularly helpful in that it ends with a 316-entry annotated bibliography, many in detail. A good number of the papers cited in this essay can be found in these anthologies, and most are abstracted in the bibliography of *Grue*. The bibliography for Part III of Scheffler [1963] is also useful.

questions do arise about the status and validity of Argument II. What is the relationship between the premise and the conclusion that entitles inferring the latter from the former?<sup>2</sup>

Unlike Argument I, the conclusion does not follow from its premises on the basis of logic alone. At the same time, the conclusion of Argument II does seem to be reasonable, given the evidence. The problem is to account for the soundness of this inference. One widely accepted solution was thought to lie in making explicit an assumed yet unstated, premise, the principle of the Uniformity of Nature. Since Nature is uniform, not chaotic, it can be assumed that past regularities will continue into the future. By adding this uniformity principle as a premise to Argument II the argument can be turned into a valid deductive inference. Hume does not deny this. Rather he questions our right to assume the principle as a premise.

Hume argues that the Uniformity of Nature principle is not an *a priori* truth. It is not a relation of ideas, and it is possible to doubt. There is no way to establish the principle by thought or reason alone. Hume then argues that it is not possible to justify the principle on empirical grounds either. Any such justification will be circular. It will depend on assuming the uniformity principle or something equivalent to it. By their very nature inductive conclusions are under-determined by their premises. There is no guarantee that past regularities will continue to hold. With respect to empirical matters we can never be certain. Our best-supported hypotheses are always subject to refutation by new evidence. Hume concludes that ultimately induction is founded on habit, our habit of predicting future regularities on the basis of those experienced in the past. There is no further ground or firmer warrant for inductive practice.

For Hume the problem of induction also raises issues concerning the justification of causal claims. Hume maintains that it is a mistake to believe that we actually observe causes. We can, for example, observe one billiard ball striking another and the latter moving away, but we do not observe any causal connection *per se*. There is no such thing to observe. All we can perceive is *A* followed by *B*. Hume goes on to point out, however, that simply observing *B* to follow upon the occurrence of *A* is not enough to sanction the claim that *A* caused *B*. That it starts to rain immediately after Claire sneezes does not justify the claim that her sneeze caused the rain. The difference between these cases cannot lie in a failure to observe the causal connection between sneeze and rain. It is not possible to observe any such connection between the billiard balls either. Nevertheless, it seems correct to say that the one billiard ball striking the other causes it to move. According to Hume what distinguishes the cases are facts about previous observations. A record of the past observations indicates that whenever billiard balls or similar items collide the stationary item moves. This constant conjunction instills a habit, the expectation that the next collision observed will be followed by movement. We have not observed comparable sneezes followed by rain regularities.

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<sup>2</sup>Recently Harman and Kulkarni [2006] question whether the rules of induction and rules of deduction should both be characterized as rules of inference.

Hence no habit is established that engenders the expectation that rain will appear on the heels of local sneezes.

The crucial feature of Hume's analysis of causation is its reliance on past regularities to underwrite causal claims. The cause/effect relation is not a necessary connection. No amount of reason and thought about *A* entails that *B* will follow upon its occurrence. Nor can causal connections themselves be observed. Without the experience of past regularities there is no basis for expecting events to turn out one way or another. Hume's account of causation is usually referred to as a "regularity" theory. The relation between Hume's problem of induction and his analysis of causation is on the surface. Valid judgments of causation depend on there being relevant regularities to back them up. Justification for believing there are such regularities depends on empirical evidence. But as Hume's analysis of the problem of induction shows, trust that observed regularities will continue into the future can not be further grounded. Thus judgments of causation, too, are never certain. They are only as hardy as the observed regularities that support them. The bedrock warrant for both inductive and causal claims is habit.<sup>3</sup>

Scholars debate whether Hume's analysis shows that he is a skeptic. Many think Hume is. Since Hume maintains that inductive judgments are neither *a priori* nor empirically certain they cannot "really" be known. Moreover, it is argued, on Hume's account of induction the confidence we do have in such judgments is not objectively grounded. The confidence rests on habit, and habits are subjective. Habit might explain a practice, but habit cannot justify or provide norms for the practice. Other scholars argue that it is not correct to assume that Hume is a skeptic. Hume thinks that inductive judgments based on observed regularities are justified. Objective inquiry requires there be sufficient evidence to back up our statements, not that the evidence makes them certain. If a belief is well supported by the empirical evidence, it has all the justification that is possible and all that is needed to warrant its acceptance.

Hume's analysis of inductive reasoning met resistance from the start, and efforts to explain his "real" position, explain away the problem, criticize his solution or propose new ones have continued. (See [Swinburne, 1974; Foster and Martin, 1966].) I think it fair to say that the majority opinion is that Hume's core ideas about the problem of induction have survived these challenges. His account of causality, although still influential, has not remained as firm.

## 2 DEVELOPMENTS IN THE TWENTIETH CENTURY

For the purposes of this entry it is possible to skip ahead and pick up the story in the early twentieth century. (See 4, 5, 6 to fill in the period between.) In light of exciting progress in symbolic logic and the related development of formal tools of analysis there was much interest in constructing a syntactic theory of inductive

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<sup>3</sup>Lewis [1973] claims that Hume actually offered a second account of causation in terms of counterfactuals.

logic, comparable to that developed for deductive inference. Much of this work sought to formulate a quantitative measure of confirmation in terms of probability. The most detailed and influential work on this task was that of R. Carnap. [See 9.] In his monumental *Logical Foundations of Probability*, Carnap [1950] laid out the basics for a formal system of inductive logic. Others joined Carnap in the project, criticizing, altering and expanding on what he accomplished. Carnap's ideas and approach remain topics of discussion.

By mid-century work had begun on a related, more modest program. The aim here was neither a quantitative nor a comparative measure of confirmation, but a qualitative theory. It was to spell out the relation between a body of evidence ( $E$ ) and a hypothesis ( $H$ ) such that  $E$  confirms  $S$ . The initial thought was that the relation could be defined syntactically. (See [Hempel, 1943; 1945; Achinstein, 1983].) The first step was to lay down a set of conditions of adequacy that any satisfactory qualitative theory of confirmation must meet. C. Hempel (*op cit*) with input especially from N. Goodman and P. Oppenheim produced the most discussed work on this topic. The task turned out to be more complicated and elusive than supposed. Firm intuitions about obviously correct conditions of adequacy were put in doubt when they were shown to sanction counter-intuitive results. It became increasingly clear that it was not possible to design a system that adopts all the conditions of adequacy that intuition deems necessary. Such a system would be inconsistent.<sup>4</sup> A few examples of particular relevance to the issues to be taken up later can give the flavor of the problems encountered. [See 8.]

Presumably, evidence that boosts the credibility of a hypothesis should be taken to confirm it. If this principle is adopted, though, evidence that appears irrelevant to a hypothesis's full content will count as confirming it. For example, let  $H^*$  be the conjunction of  $H1$ , "All emeralds are green," and any arbitrary, independent  $H2$ , say, "Snow is white." Take the observational evidence to be that emeralds 1–999 are green. This evidence appears to count in favor of  $H1$ , while irrelevant to an evaluation of  $H2$ . Nevertheless, if this evidence makes  $H1$  more credible it should also increase somewhat confidence in  $H^*$ . Yet this result runs counter to another intuition, namely the intuition that confirming evidence for a hypothesis should spread its support to all of its instances. In the case being considered, although the 999 observed emeralds do favor  $H1$ , they do not spread their support to all items that fall within the scope of  $H^*$ . The evidence does not speak one way or the other about the color of snow. Adopting the intuitively sound condition of adequacy, "Evidence that confirms  $H$  confirms all consequences of  $H$ , however, would require that if the evidence of 999 green emeralds confirms  $H^*$ , then it must confirm  $H2$  itself.

Problems of this sort suggest drawing a distinction between evidence that supports or makes a hypothesis more credible and evidence that spreads its support to *all* instances of the hypothesis in question. Only a body of evidence that spreads its support throughout the hypothesis will count as *confirming* evidence. Obser-

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<sup>4</sup>For argument that any of the proposed conditions of adequacy can be given up, see Hanen [1971].

vation of green emeralds, therefore, does confirm  $H1$ , but it does not confirm  $H^*$ . It does not spread its support to the instances of  $H2$  that fall within the full scope of  $H^*$ . Although the 999 green emeralds speak in  $H^*$ 's favor, lending it additional credibility, they do not confirm  $H^*$ .

Another troubling conflict of intuitions arises from the so-called "Raven's Paradox". It seems obvious that a confirming instance of a hypothesis should count as confirming logical equivalents of that hypothesis. This condition of adequacy is usually labeled the "equivalence condition". Now evidence of a black raven is surely a paradigm case of a confirming instance of the hypothesis "All ravens are black". By parity of reason, evidence of a non-black object that is not a raven should count as a positive instance of the hypothesis "All non-black items are non-ravens" which is logically equivalent to "All ravens are black". It would follow then that an observation of a red herring (i.e. something that is non-black and not a raven), in confirming "All non-black things are non-ravens." confirms the hypothesis "All ravens are black". At first blush it strikes us as highly implausible that we should be able to confirm a hypothesis about the color of ravens by examining herrings for color.<sup>5</sup>

### 3 THE NEW RIDDLE OF INDUCTION

While attempts to resolve these issues were in full swing, Nelson Goodman [1946] pointed out a problem for both quantitative and qualitative theories of confirmation. He noted that the same observational data could support *conflicting* hypotheses, depending on how the evidence is described.<sup>6</sup> Suppose a hitherto unknown machine were to toss up a total of 999 marbles in sequences of 2 red and 1 black. Calculating the odds that the next toss is red will differ if the evidence is described as simply a total of 666 red and 333 black marbles or if it is described in more detail as 333 sequences of 2 red and 1 black marbles.

Another example of the problem can be constructed if instead of a total of 666 red and 333 black tosses, the machine were to toss 999 marbles that were all red. The intuition is that this evidence supports the hypothesis that marble 1000<sup>th</sup> will be red, not some other color. Goodman argued, though, that there is another characterization of the evidence that predicts the 1000<sup>th</sup> marble is black. The trick depends on introducing a new, "peculiar" predicate,  $S$ .  $S$  is defined as applying to marbles  $M1$  &  $M2$  & ...  $M999$  and red, or not  $M1$ – $M999$  and black. Describing the evidence as consisting of all and only  $S$ s is no less true than describing each of the evidence instances as "red". Hence, the hypothesis "All marbles are  $S$ " has the same number of confirming instances as the hypothesis "All marbles are red". Projecting  $S$  leads to the prediction that ball 1000<sup>th</sup> and those encountered thereafter will be black, but this conflicts with the seemingly better prediction

<sup>5</sup>In addition to Hempel's work there is a vast and increasing literature concerning the proper analysis of this paradox. Of special relevance to the issues to be considered, see [Quine, 1970; Scheffler, 1963; Scheffler and Goodman, 1972].

<sup>6</sup>To simplify presentation here and elsewhere I have slightly altered several of Goodman's particular examples.

that future cases will be red.

Actual inductive practice, of course, sanctions the projection of red and not the projection of black. The evidence of 999 red marbles does spread its support to all instances of the red-hypothesis, it does not do so in the case of the *S*-hypothesis. Projecting the *S*-hypothesis is not warranted, the evidence does not confirm it. Projecting the *S*-hypothesis would be a sign of some sort of inductive irrationality.

Carnap [1947] recognized the force of Goodman's examples. He also recognized that this meant there could not be a strictly *syntactic* inductive logic, as many had hoped. Semantic features of the predicates employed had to be taken into consideration. Carnap proposed that a sound logic of confirmation should rule out the use of Goodman's peculiar predicate and others like it. The challenge then was to specify a principle for determining those predicates that should be allowed in and those that should be excluded. Carnap's answer was that predicates mentioning particular objects, times and places were to be discounted. Only non-positional, purely qualitative predicates were to be employed in a theory of induction.

In a brief reply Goodman [1947a] argued that Carnap's solution had serious shortcomings. Carnap's plan to divide the good predicates from the inadmissible ones depends on there being a set properties that are absolutely simple into which all other acceptable predicates can be analyzed. Goodman countered that all analysis is relative to the categories or concepts a system takes as its base. Properties or predicates analyzable in one system may be primitive or unanalyzable in another. Goodman mentions, too, that various "good" predicates do in fact mention particulars (e.g. "arctic", "solar", "Sung") and are used in making projections.<sup>7</sup>

Goodman later elaborated and defended his position in his book, *Fact, Fiction and Forecast*. Here Goodman labels his puzzle the "New Riddle of Induction". It is the version of the puzzle laid out in this book that has become the primary focus of discussion. To explain the riddle, Goodman introduces the predicate "grue". The definition of "grue" is:  $x$  is *grue* =  $x$  is examined before (a future time)  $t$  and is green, or not so examined (before  $t$ ) and is blue. Suppose again that all emeralds examined before  $t$  (e.g. emeralds 1– 999) are observed to be green. Then they will each be grue as well. The hypotheses "All emeralds are green." and "All emeralds are grue." have equal support, 999 positive instances. Yet their predictions about the color of emeralds examined after  $t$  conflict. Goodman does not deny that the evidence warrants projecting the green-hypothesis and not the grue-hypothesis. The New Riddle of Induction is to explain and justify the choice. Why do we predict that emeralds that will be examined after  $t$  are green rather than blue, and what warrants our doing so?

In *Fact, Fiction and Forecast* Goodman introduces several more "peculiar" predicates, "bleen" and "emeruby" among others. "Bleen" is defined as:  $x$  is *bleen* =  $x$  is examined before  $t$  and is blue, or not so examined and is green. "Emeruby" is defined as:  $x$  is an *emeruby* =  $x$  is an emerald examined before  $t$  or a ruby not so examined. These additional predicates are used to flesh out Goodman's arguments and highlight the variety of ways the New Riddle can arise.

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<sup>7</sup>Carnap [1948] is a response.

## 4 SOME MISUNDERSTANDINGS

Attempts to resolve the New Riddle have often taken a wrong turn because their proponents misread how “grue” and other predicates introduced apply. To label an emerald “grue”, before or after  $t$ , is not to claim that the emerald or any emerald ever changes its color from green to blue. An emerald is tenselessly green, tenselessly grue or both if examined before  $t$  and green. An emerald examined after  $t$  is (tenselessly) blue, if it is grue and green if it is green. The role “grue” plays in the puzzle is to pick out or separate two subsets of emeralds, the examined from the then unexamined. It is not meant to suggest a worry over whether any emerald ever turns from green to blue.

Another point that is often unappreciated is that the riddle does not depend essentially on considerations *time*. The version of Goodman’s predicate,  $S$ , mentioned above, employs names for each examined case and makes no reference to time in separating items in the evidence class from those in the projected class. In fact, pretty much any property or way of characterizing the subsets will do. (See [Elgin, 1993; Scheffler, 1963].)

A variant of the riddle can also be demonstrated with the use of graphs. (See [Hempel, 1960; Hullett and Schwartz, 1967].)

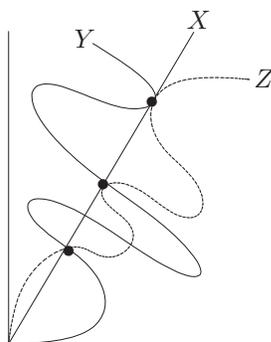


Figure 1.

Each curve in Figure 1 can be taken to represent a distinct hypothesis. Indeed, an unbounded number of conflicting curves could be drawn that pass through the data points. All of these curves/hypotheses take into account the body of available evidence, and no curve is a more accurate characterization of the data than the others. Each is a *true* description of the evidence. The different curves, however, make conflicting predictions about items/values not as yet determined. Again, time need not enter the picture.

Another questionable response to Goodman’s puzzle is the argument that the predicate “grue” and others raised in generating the New Riddle are not really relevant to science. When the puzzle is formulated in terms of actual scientific properties and theories, it is held that the riddle can be readily solved or dissolved.

It is undoubtedly true that little science goes on at the level of the hypotheses, “All emeralds are green.” or “All rubies are red”, and discussion of the New Riddle in the context of high-level theory can be profitable. (See [Earman, 1985; Wilson, 1979].) Unfortunately, many of the attempts to escape the puzzle by citing its failure to be scientific enough tend to lose sight of the fundamental issue at stake. The real significance of the New Riddle is that it forces an examination of the way everyday and scientific concepts and vocabularies shape practices of inquiry. In particular, it focuses attention on how the choice of concepts and vocabularies play a role in warranting the acceptance of some hypotheses and rejecting others. There is another reason why simply setting the problem in the context of more theoretical concepts and vocabularies of science cannot be the whole solution. These loftier predicates and properties can themselves be “grue-ified”, and the problem then reappears with respect to high-level laws and generalizations. (See below.)

Over the years it has been periodically maintained that there is nothing new in the New Riddle. It is said that Goodman’s puzzle is essentially no different from Hume’s problem of induction.<sup>8</sup> All it does is reaffirm Hume’s point that inductive claims are never certain, and that such inductive indeterminacy cannot be eliminated. The introduction of “grue” it is said merely presents an old story in a new guise. Collapsing Hume’s problem and Goodman’s problem in this way is to misunderstand the significance of both. Give or take some alterations and modernization, Goodman accepts Hume’s analyses of induction and causality. He contends, though, that Hume did not go far enough. The problem of induction runs much deeper.

Hume argued that inductive reasoning cannot be ground in a principle of the uniformity of nature, and that attempts to do so end in circularity. Goodman wishes to show that the uniformity principle by itself is in a sense vacuous. Regularities are free for the asking. There are an unbounded number of past regularities that can be projected and the projections conflict. The hypothesis “All emeralds are green.” projects the *regularity* of observed cases of green emeralds into the future. The hypothesis “All emeralds are grue.” projects the *regularity* of observed cases of grue emeralds into the future.

The New Riddle is *not* meant to restate Hume’s thesis of inductive indeterminacy. The New Riddle asks instead how is it that we project some past regularities and not others, and what guides and justifies such practices. This difference between the New Riddle and the old problem of induction can be explicated with the help of Figure 1. Hume’s claim is that although we project curve *X* on the basis of past regularities, we have no logical guarantee that its predictions will be born out. In empirical matters there is always risk. Goodman asks why we project curve *X* over the rest of the curves, given that there is equal evidence for them all. Granted inductive indeterminacy is a factor with any projected curve, what is it about curve *X* that warrants its projection, rather than the other curves that conflict with *X*.

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<sup>8</sup>There have been many different arguments for this claim. For a quite recent one see Norton [2006].

Hume's problem does not raise worries about the need to distinguish between justified predictions based on past regularities and conflicting predictions equally supported by past regularities. He is concerned with the justification for believing that observed regularities will continue to hold in the future. The New Riddle asks instead why based on the same evidence we project some past regularities and not others. Goodman and Hume both ask what warrants inductive practices. But they are concerned to explain and justify two different aspects of these practices.

## 5 PROPOSED ASYMMETRIES

Most of those who have grappled with the New Riddle have looked for a semantic/epistemic solution. Like Carnap they argue that there is a semantic asymmetry between "green" and "grue" that accounts for the difference in the projectibility of the two predicates. In *Fact, Fiction and Forecast* Goodman makes an effort to turn back several such solutions he thinks people might propose. I will mention a few of the most prominent.

Carnap we have seen claims that his inductive logic is not to be applied to hypotheses that contain explicit reference to particulars (e.g. times, places, or individuals). Goodman's earlier response was that whether or not a predicate refers to particulars depends on the system of analysis adopted. Now Goodman fleshes out this argument with an example. Relative to a system that takes "green" and "blue" as primitives the definition of "grue" makes explicit reference to a particular, i.e. time  $t$ . However, if "grue" and "bleen" are taken as primitives of the system and "green" and "blue" are defined using them, green and blue will be the predicates that make explicit reference to time  $t$ .

Another response Goodman considers concerns the use of higher-level properties to draw distinctions that could not be made in terms of less general properties. Granted, for example, "All emeralds are grue." does not assert that any emerald changes its color, it does project blue to the unexamined emeralds, and blue is a different color from that of the observed emeralds. By contrast, the hypothesis, "All emeralds are green." predicts that newly examined emeralds will be the same color as those in the evidence base. There are several problems with this sort of solution. First, it assumes that grue and bleen are not colors. Second, even if there is reason not to consider them colors, it is possible to introduce a new predicate, color\*, that results from grue-ifying the general predicate "color". Then the grue hypothesis projects that new emeralds are the same color\* as those in the past, and the green hypothesis projects that unexamined emeralds are different in color\* from those found in the evidence. (See [Scheffler, 1963].)

This, of course, does not show that there is no way to distinguish "grue" from "green". For example, instances of green perceptually match, while grue items do not. The problem, though, is to explain how this perceptual distinction in color can be used to solve the riddle. Why should failure of instances of a predicate to match perceptually rule out its use in projection? Sapphires and diamonds come in a variety of un-matching colors, but the predicates "sapphire" and "diamond" do

find their way into sound inductions. More significantly, an asymmetry based on perceptual matching is of very limited use. It cannot be employed with respect to an overwhelming number of scientific predicates (e.g. conducts electricity, gravity, soluble). Thus solutions to the New Riddle that depend on features peculiar to color, like those that depend particularly on time, are not adequate for the task.

I have switched back and forth between talking of properties and talking of predicates and have made no attempt to distinguish between them. In light of the many problems with the notion "property" Goodman, prefers to speak of "predicates." Some think, however, it is a mistake not to separate the two. [See: Armstrong 1978 Shoemaker 1980.] Predicates are linguistic entities. Properties are abstract objects whose existence or non-existence is not a matter of language. Predicates are ours for the making; properties are mind-independent.

Now this is no place to examine the pro's and con's of the ontological, metaphysical and epistemic difficulties associated with properties. Be that as it may, merely countenancing abstract properties will not provide a solution to the New Riddle. Properties may be eternal occupants of Plato's heaven, but the rules for gaining entrance into this paradise are not clear. If "green" and "blue" have a place, what about "grue" and "bleen"? And if they too are properties, the New Riddle is reintroduced. Simply declaring that grue and bleen are peculiar and therefore cannot be "real" properties begs the question. Efforts to distinguish real and faux properties in terms of "what can be analyzed into simples" or on the basis of time, color and perceptual matching run into many of the same obstacles encountered in formulating criteria for distinguishing between projectible and unprojectible predicates.

One response to the difficulty posed by the possible overpopulation of Platonic heaven is to distinguish between properties and universals. [See: Lewis 1983] Only some properties are universals, and universals are projectible. Right off this move does not solve the New Riddle. Settling the riddle now requires explaining why green and blue are universals, but grue and bleen are not. So unless there are acceptable criteria for determining which properties are universals and which not, this proposal by itself does not significantly advance matters.

There has also been a spate of attempts to establish an asymmetry between projectible and unprojectible hypotheses by appealing to counterfactuals. The details of these solutions differ, and I can give only the flavor of the approach. [See: Jackson 1975; Godfried-Smith 2003] On this account, the difference between "grue" and "green" is to be drawn along the following lines. We generally believe that observing an object does not change its properties. In the case of "All emeralds are green." it seems true to say that if an emerald in the evidence base, say emerald 12, had been first observed after  $t$  it would, nonetheless, be green. Not so with "All emeralds are grue". We are not inclined to assert that had emerald 12 been initially observed after  $t$ , it would be grue. To be grue it would have to be blue. But we do not believe the color an object possesses depends on when it is first observed.

This approach, too, is not without difficulties. [See: Schwartz 2005] As discussed, to the extent any solution depends crucially on supposed time or color features of grue it is not general enough. The New Riddle can be raised without employing these features. Some versions of counterfactual solutions also seem to conflict with essentialist assumptions about identity when applied to our old example, “All emeralds are S”, where members of the evidence class are specified by name. And it must not be forgotten that these types of solutions depend on the analysis of counterfactual conditionals adopted. For many, especially Goodman, an analysis of counterfactuals involves an appeal to laws. In Chapter I of *Fact, Fiction and Forecast*, Goodman argues that true counterfactuals are those that have laws to back them up.<sup>9</sup> Accidental generalizations do not offer such support. Goodman then goes on to claim that distinguishing lawlike generalizations from accidental generalizations requires a distinction on par with that between projectible from unprojectible hypotheses. (More on this issue soon.)

There have been numerous attempts to recast the New Riddle and the other paradoxes of induction along Bayesian lines (see [Good, 1975; Salmon, 1970; Jeffrey, 1983]).<sup>10</sup> Examining the plusses and minuses of a Bayesian approach in general and its treatment of projectibility in particular are beyond the boundaries of this essay. A main feature of most such Bayesian analyses of the New Riddle is assigning the grue-hypothesis a prior probability that is clearly much less than the prior probability assigned the green-hypothesis. The question then is to account for this asymmetry in probability assignments. Depending on how this issue is settled there may be no incompatibility with Goodman’s statement and solution of the riddle.

Many other proposed solutions to the New Riddle go wrong because they too depend on factors peculiar to “grue”, or because they fail to heed Goodman’s warnings about the “obvious” semantic solutions, or because they depend on tools of analysis Goodman eschews. Over the years more careful semantic/epistemic analyses and critiques of the New Riddle emerged, and Goodman and his colleagues have made efforts to respond. There is now a very extensive literature discussing these solutions and their fate (see [Goodman, 1972; Stalker, 1994; Elgin, 1997; papers in *Journal of Philosophy* 1966, 1967<sup>11</sup>; and new solutions crop up on a regular basis).

## 6 THE ENTRENCHMENT SOLUTION

In *Fact, Fiction, and Forecast* Goodman offers his own solution to the New Riddle. It is neither syntactic nor semantic. It is *pragmatic*. According to Goodman projectible predicates are just those that have a history of past use. “Green”, for

<sup>9</sup>This chapter is based on Goodman [1947].

<sup>10</sup>Kyburg [1964] reviews related issues and provides an extensive bibliography of work on inductive logic.

<sup>11</sup>The *Journal of Philosophy* 1966 and 1967 each had a large section of papers, replies, etc by a variety of people on the subject. Please refer to Volume 63, 1966 and Volume 64, 1967.

example, has played a role in many past projections. “Grue” has no such history of gainful employment. In Goodman’s term, “green” has become *entrenched* as a result of actual inductive practice, “grue” has not. Such differences in entrenchment he claims are a major, but not the only, factor that distinguishes projectible from unprojectible hypotheses.

To fasten on entrenchment as a key to the New Riddle is one thing, to formulate a set of rules using entrenchment to separate projectible from unprojectible hypotheses is another. In the first edition of *Fact, Fiction, and Forecast*, Goodman defined a number of technical concepts and used them to formulate a system of rules intended to handle both the sorts of cases discussed above, as well as deal with various complications that I have ignored in laying out the New Riddle. In the book, Goodman also extends and clarifies his initial account of entrenchment. Predicates earn entrenchment by both their own use and by the projection of co-extensive predicates. They can also inherit entrenchment from over-hypotheses. “Copper” and “aluminum” can gain entrenchment from projections of “metal”. A notion of presumptive projectibility is introduced, offering additional routes for enhancing entrenchment.

These rules were revised, simplified and made more intuitive in “An Improvement in the Theory of Projectibility” [Schwartz, Scheffler and Goodman, 1970]. Goodman’s three original rules were reduced to one:

A hypothesis is projectible if all conflicting hypotheses are overridden, unprojectible if overridden and non-projectible if in conflict with another hypothesis and neither are overridden. An hypothesis H is said to override an hypothesis, if they conflict and H is the better entrenched.<sup>12</sup>

Adoption of these new rules showed that it was necessary to rethink several earlier issues and claims. For instance, on the new version of the rules the assumed connection between confirmation, projectibility and lawlikeness is less direct (see [Schwartz, 1971]). In addition, alternative responses to earlier criticisms become available (see [Davidson, 1966]);

The new rules like the old are given extensional formulations. Entrenchment is understood in terms of the entrenchment of extensions. The projection of co-extensive predicates lifts the degree of entrenchment of them all. Hypotheses are assumed to conflict, when it is thought that they assign an object to incompatible extensions, and so forth. Remaining within extensional boundaries does have its costs, but those who doubt that intensional notions are clear enough to use have little option. What is more, in the context of a theory of projection, extensionality can make sense independent of qualms over “meanings”, “modalities” and “essences”. If, for instance, the evidence warrants the projection of a hypothesis, it would seem safe to project a hypothesis that simply replaces one of its predicates with another predicate *assumed* to be coextensive. A similar case can be made for the propriety of the extensional definitions of the concepts “conflict,” “positive

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<sup>12</sup>I have omitted here additional complications.

instance” and the others employed.<sup>13</sup>

Continued efforts either to defend or criticize the details of the proposed rules of projection, however, are likely to be unproductive (see [Schwartz, 1999]). The rules are at most tentative and of limited scope. They are formulated with respect to a simple language and apply strictly only to hypotheses of the form  $(x)(Px \rightarrow Qx)$ . The rules and definitions are not geared to deal with relational predicates and statistical hypotheses. Formulating rules that do take into account such richer realms of hypotheses awaits further development, and it would not be surprising if this results in a need to rework or to drop earlier principles and definitions. The current limitations on the applicability of the projection rules also means that over-hypotheses not in the canonical form cannot be taken into account. This can have a significant affect on entrenchment values, and also make it especially hard to justify the introduction of more complex new predicates that have no entrenchment of their own.

When all is said and done, though, the underlying idea, made more apparent in the new rules, seems to point in the right direction. Entrenchment is only one among many properties (e.g. evidential support, conservatism, scope and simplicity) that contribute to a hypothesis’s status. Current opinion is that any useful account of hypothesis acceptance must pay attention to a range of good-making properties and cannot rely only on observational support. All these values/virtues have a say in determining which among competing hypotheses to adopt. A hypothesis having sufficiently more of these “good-making” properties than its rivals will win out. Precise measurements of all these values, though, are notoriously hard to come by. And even when rough and ready assessments can be made, the virtues often compete. The more you have of one virtue the less you can have of the other. Trade-offs are required. As a result, a hypothesis that comes out on top according to one weighting scheme may not be ranked as high by another. Choice among hypotheses relies as much on good scientific sense as on rules and calculation. The hope of accounting for hypothesis choice solely in terms of the evidence has not seemed a viable goal for some time (see [Hempel, 1966; Kuhn, 1977; Quine and Ullian, 1978]).

## 7 IMPLICATIONS

The issues raised by the New Riddle have significant implications well beyond the problem of induction and the development of a logic of confirmation. Goodman discussed several such issues in Chapter I of *Fact, Fiction and Forecast* before introducing the New Riddle. The first concerns the proper analysis of counterfactual conditionals. The statements, “Had this piece of butter been heated to 100 F it would have melted.” and “Were this piece of butter to be heated to 100 F it would melt.” are true. The statements “Had this rock been heated to 100

<sup>13</sup>An extensive debate related to this issue began with a paper by Zabłudowski [1974] and a reply by Ullian and Goodman [1975]. It continued on for a number of years, primarily in the *Journal of Philosophy* with others joining in (e.g. [Scheffler, 1982; Kennedy and Chihara, 1975]).

degrees it would have melted.” and “Were this rock to be heated to 100 degrees it would melt.” are false. Wherein lies the difference? Goodman’s answer, in part, is that in the first case there are accepted generalizations underpinning the counterfactual claims, while in the second case there are no comparable established generalizations on hand.

But not all true generalizations are capable of supporting counterfactuals. Although all objects on desk *d* have weighed less than a half pound, we are not inclined on the basis of this observed regularity to maintain that any item that *would have been* or *were to be* on the desk would have/will weigh no more. The fact that all the items on *d* have and may in the future turn out to be under a half pound is thought to be an “accident”, and true accidental generalizations, are incapable of supporting counterfactuals. By contrast, Goodman’s example of a true counterfactual, “Had this piece of butter been heated to above 100 F it would have melted.” is backed up by a law, “All butter melts when heated above 100 F.” The generalization “All items on *d* are under a half pound.” may be true of the evidence cases and may turn out to be true of all objects ever to be on *d*. This generalization, however, is not lawlike, and it takes laws to support counterfactuals.

This raises the problem of explaining what accounts for some general hypotheses being lawlike and others not. One clue is that although the so far observed instances of items on *d* lend some support for the tenseless hypothesis “All objects on *d* are under a half pound”, the evidence does not *confirm* it. Positive instances do not spread their support to all items that could have or might fall under the hypothesis. On this score, accidental generalizations are similar to the grue-hypothesis. The evidence that all so far encountered emeralds are grue does not make it credible that all the unexamined instances will be grue. We are not inclined to project “All emeralds are grue.” even though it has as many positive instances as “All emeralds are green”. As Goodman presents it, an evaluation of counterfactuals presupposes a distinction between lawlike hypotheses and accidental generalizations. And this distinction itself is intimately connected with the distinction between projectible and unprojectible hypotheses. In turn, these considerations impinge on the analysis of “causation”. A regularity theory of causation depends on generalizations that are lawlike, not accidental. The main alternative to a regularity account is one that explicates the notion of “cause” in terms of counterfactuals, and on Goodman’s analysis support for counterfactuals comes from laws.

Possible world analyses of counterfactuals are thought to offer a wedge into these interrelations. One difficulty with this approach is its dependence on the existence of possible worlds, an ontological commitment many are leery to take on. Another difficulty is that possible world analyses usually depend on an ordering, similarity or accessibility relation among possible worlds. Typically worlds that violate the laws of the actual world are thought to be further apart than worlds that violate accidental generalizations. And as we have seen Goodman argues that drawing this latter distinction rests on the distinguishing projectible from unprojectible

hypotheses.

For many, any *pragmatic* solution to the New Riddle and related issues like that of “lawlikeness” are in principle unsatisfactory. Critics want and seek a firm, fixed foundation. To many the idea of “natural kinds” has seemed well-suited to the task. Natural kinds as opposed to made-up or artifactual kinds are said to be objective; their boundaries are mind- *independent*. “Grue,” “bleen,” “emerubies” along with the kinds employed in accidental generalizations (e.g. all things on desk *d*) are peculiar and defective, because the kinds they pick out are not natural. That the properties/predicates “grue,” “bleen” and “emerubies” are peculiar, defective and have little scientific import is undeniable. The question that awaits an answer is how to characterize or distinguish natural kinds from other kinds. There does not seem to be any purely syntactic or semantic means to accomplish the division. Nor does the world come ready-made with its ontological joints delineated.

A pragmatic answer is that natural kinds are those that have been used and found useful in practice. The naturalness of kinds and the predicates that denote them is not intrinsic. Rather, natural kinds are kinds we rely on in projections, and they become natural as a result of use/entrenchment. This solution is, of course, unpalatable to those who believe that pragmatic explanations are by nature *subjective*. They look for an objective, “naturalistic” account of natural kinds. It has been thought, for example, that it is possible to explain the naturalness of natural kinds in terms of the inherent *similarity* among the members of the kind. Members of a natural kind are similar one to another; not so with the kinds picked out by “grue” “emerubies,” “items on desk *d*” and other peculiar predicates.

The immediate problem then is to specify satisfactorily what “similarity” means in this context. The items in the extension of “green” are similar in being green, and those in the extension of “emerald” are similar in their emerald-ness. But the same can be said of “grue” and “emerubies”. Grue objects are similar in being grue, and emerubies are similar in being emerubies. Drawing a useful distinction between natural and unnatural kinds, therefore, requires a narrower, more restrictive notion of “similarity”.

One proposal is to define “similarity” in terms of perceptual matching. Green objects perceptually match one another, while items that are grue do not (see [Shoemaker, 1975]). Matching differences then can be used to explain why it is correct to claim that “green” picks out a natural kind and “grue” does not. Several things count against this solution to the projectibility puzzle. The most obvious is that instead of being too inclusive, a perceptual matching criterion is too exclusive. Once again this sort of solution is overly dependent on features special to grue. A perceptual matching test for similarity and natural kinds is not applicable to the full range of kinds found in science and in everyday use.

The difficulty with a similarity solution, however, runs much deeper. There is something wrong with the very idea of an absolute notion of similarity [Goodman, 1970]. Similarity is no more an intrinsic feature of the world than is the naturalness of kinds. To be asked to group a set of items according to their similarity pure and simple does not make much sense. Similarity judgments are relative

to the task at hand; they are not absolute. There is no single correct way, say, to group a collection of pills on the basis of similarity. With equal justice, the pills may be grouped with respect to color, shape, chemical structure, diseases used for, manufacturer, price range or whether they have been prescribed to Mrs. Smith. Judgments of similarity are relative to context, interests, past experience, perceptual skill and purpose in mind.

Attempts have been made to salvage a fixed, non-relative standard of similarity by appealing to evolution and innateness.<sup>14</sup> This approach, too, runs into obstacles. First, if the innate determinants or biases take the form of innate perceptual *quality* spaces, the solution will lack generality. Positing non-perceptual innate constraints runs into other difficulties. At present, there is no convincing account of how even comparatively low-level everyday and scientific predicates would/could be encoded in our genes. Moreover, one of the hallmarks of human cognition is the ability to forge concepts that pay no heed to innate or “natural” psychological groupings. Most frequently in science kinds gain their importance by cutting across psychologically “primitive” boundaries.

Second, evolutionary pressures, by themselves, cannot explain the survival of “green” projectors over “grue” projectors. Up until time  $t$  (or more generally, until unexamined cases are confronted), there will be no difference in their survival value. Both the green-hypothesis and the grue-hypotheses jibe with the environment thus far encountered and both make the same predictions for the period before  $t$ . The response that other predicates *like* “grue” and “bleen” have turned out to be bad for survival begs the question. For what does it mean to be “like grue” or “like bleen”. What is the property common to “grue” and “bleen” that makes them peculiar and unsuitable for projection? Finally, it cannot be argued that natural, evolutionary favored categories are necessarily better than those not so sanctioned. In retrospect, we can see that schemes that strike us as biologically unlikely could have had important advantages over those we found natural and did employ (see [Putnam, 1983; Schwartz, 1999; Elgin, 1996]).

It is important to realize as well that solutions to the New Riddle that cite similarity judgments or other psychological factors offer *pragmatic*, not syntactic or semantic solutions. Nor do such solutions necessarily challenge an entrenchment account. They might be part of an explanation of entrenchment, rather than a denial of its affects.

## 8 VALUES, VIRTUES AND HYPOTHESIS SELECTION

Entrenchment along with evidential support, conservatism, simplicity and scope were mentioned separately as “good-making” properties that influence choice among competing hypotheses. I wish now to discuss some relations among them.

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<sup>14</sup>Quine [1990] suggests an approach along this line while recognizing its limitations. Some, for example, N. Stemmer [2004] have pursued and continue to pursue this approach. Other theorists have reversed this line of argument and cite the New Riddle to justify innateness claims. See the debates concerning this strategy in Piatelli-Palmarini [1980].

### 8.1 *Evidential support:*

Hume showed that no matter how much evidence there is for an empirical hypothesis there is no guarantee that regularities of the past will carry into the future. The New Riddle grants Hume's point but goes further. Not only are empirical hypotheses always subject to refutation, there will always be an unlimited number of conflicting hypotheses that encompass the data available. "All emeralds are grue." has as many positive instances as "All emeralds are green." The difference is that the positive instances of the latter confirm its projection and positive instances of the former do not. Entrenchment is offered to help account for this disparity in practice.

### 8.2 *Conservatism:*

Pragmatists have long argued that a major problem with Cartesian and other prominent accounts of inquiry is their failure to appreciate the full force of scientific conservatism. Although science continually finds it necessary to break with past commitments, it needs a certain amount of stability of its background assumptions to progress. Inquiry would be stymied if everything were up for grabs at the same time. Inquiry, as we know it, always begins in the middle of things against a corpus of accepted hypotheses. It is constrained by these beliefs and tries to preserve them. Entrenchment is a type of conservatism. Entrenched predicates are just those that make use of concepts and patterns of projection that have been relied upon in the past. Predicates that have occurred frequently in earlier projections are preferred to those that have not. To project "grue" where "green" is compatible with the data is to go against conservatism.

### 8.3 *Scope:*

Conservatism does not entail standing pat. Devising new hypotheses that fruitfully go beyond the old is the job of science and inquiry more generally. But how far it is reasonable to leap? The decision is not obvious, and is complicated by the fact that desiderata for settling on an appropriate stopping place compete. In particular, the less a theory sticks its neck out, the less likely it is to be refuted. Adopting the most conservative, minimal risk strategy, however, would prevent progress. It would countenance inertia or possibly retreat. Alternatively, maximizing the scope of projections maximizes risk. Welcomed gains in coverage are offset by diminished credibility.

A related problem was raised in discussing the support/confirmation distinction. Recall the hypothesis  $H^*$  is the conjunction of  $H1$  and  $H2$ . The evidence favors  $H1$ , but seems to say nothing relevant to  $H2$ .  $H^*$  does have wider scope than  $H1$  and is made more credible by the evidence, nevertheless, the added coverage is not warranted.  $H^*$  overshoots the mark.

Goodman [1961] highlights the trade off between scope and credibility in his paper, "Safety, Strength and Simplicity". Suppose, he says, we have examined a

large, widely distributed number of maple trees and determined that they were all deciduous. The following three hypotheses each incorporate all the evidence.

1. All maple trees, except perhaps those in Eagleville, are deciduous.
2. All maple trees are deciduous.
3. All maples whatsoever, and all sassafras trees in Eagleville are deciduous.

Hypothesis 2 has wider scope than hypothesis 1 and therefore is more susceptible to refutation. Hypothesis 3 goes beyond 2, but its adoption also increases risk. Of the three hypotheses, the safest projection is hypothesis 1 and the strongest projection is hypothesis 3, yet hypothesis 2 appears to be the one that gets the tradeoff right. Why? What determines the correct balance between intellectual bravado and modesty?

Goodman's answer is that the predicate "maple tree" is entrenched while the predicates "maple trees except those in Eagleville" and "maple trees plus sassafras trees in Eagleville" are not. Hypothesis 2 is the simplest, and simplicity is a value or virtue, a good-making property of hypotheses.

#### 8.4 *Simplicity:*

Simplicity can be understood in several distinct ways (see [Goodman, 1951; 1972]). On the one hand, there is *formal* simplicity, as exemplified when the number of primitive predicates or axioms of a system are reduced. Mere counting, though, is not a reliable measure of formal simplicity. Any set of axioms can be reduced to a single axiom by conjunction. The claim that such a conjunctive axiom is really complex, a compound of simpler ideas, encounters the difficulty already noted that such analyses are always relative to the set of predicates taken to be the primitives of a system. Hypotheses are neither intrinsically conjunctive nor non-compound. Relative to one set of primitives, a hypothesis is simple. Start with a different set of primitives and the hypothesis is syntactically and semantically complex. Other proposed measures of *formal* simplicity run into serious roadblocks when they are employed in explanations of our actual practices of induction and hypothesis choice.

Curve fitting can provide an example of some of these issues. The claim that the line plotted by  $X$  in Figure 1 is simpler than the lines of the conflicting hypotheses seems reasonable. Yet how is such simplicity to be characterized and measured? One approach has been to develop a measure of the simplicity of curves on the basis of the complexity of the functions that describe them. Thus it has been proposed that straight-line functions are mathematically simpler than those of curves, and that the functions of periodic curves are simpler than curves that wander in no regular order. But again the problem of system relativity raises its head. Lines that are straight when using rectangular coordinates are not straight when plotted according to polar and other coordinate systems (see [Hempel, 1966]). Thus it

is questionable if and how any absolute measure of geometric or mathematical simplicity can itself solve the New Riddle.

The alternative to formal simplicity is *psychological* simplicity. Some tasks are easier to do than others, some conceptual schemes easier to apply than others and some tools easier to use than others. The five logical connectives found in most introductory logic textbooks can be reduced to a single one, the Sheffer stroke, and this reduction may be taken to be a gain in formal simplicity. From a psychological standpoint, however, it is easier to state and prove arguments using the five standard logical connectives. Goodman argues that there is an intimate relationship between psychological simplicity and entrenchment. The more a concept is employed, the more natural and simpler it is to use. Innate biases surely have some affect on psychological simplicity, but once again their influence is limited. As science progresses the concepts found most useful tend to cut across boundaries that are plausibly imposed by innate constraints. This does not mean that entrenchment and simplicity amount to the same thing, but it would seem to indicate that they cannot be easily separated. Psychological simplicity depends on entrenchment and vice versa.<sup>15</sup>

Although Goodman's notion of "entrenchment" is spelled out with respect to languages, it suggests a natural extension to non-linguistic systems of representation. Perhaps curves plotted in Cartesian coordinates may be more entrenched than those using non-Cartesian coordinates. At the same time, curves in different systems that are assumed "co-extensive" in content will have the same entrenchment. At present there has been little work attempting to develop a theory of projectibility for non-linguistic systems.<sup>16</sup>

I maintained earlier that it probably did not pay to worry much at present about the details of the old or new proposed rules of induction. They only sketch an approach to the problem of projection. As they stand the rules are inadequate to deal with a good deal of science and everyday practices. When the affects of other recognized good-making properties of hypotheses (e.g. evidential support, conservatism, scope and simplicity) are added in, the inadequacy of the rules of projection to settle inductive conflicts on its own is even more apparent. Goodman's work offers a pragmatic picture of a particular and important virtue of hypotheses, one that seems central to an analysis of inductive practice. There is no reason to assume future developments will accord entrenchment as crucial a role as it is now assigned in resolving the New Riddle and related problems. Nevertheless, as things now stand taking heed of entrenchment does seem to provide more insight into a range of topics than the available alternatives.

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<sup>15</sup>There is an extensive literature on the notion of "simplicity" and its uses. See A. Zellner *et al.*, [2001].

<sup>16</sup>See [Goodman, 1976; Elgin, 1997] for use of an entrenchment approach to a wide range of issues, well beyond those mentioned here.

### 8.5 *Justification and Norms:*

Granted entrenchment may help explain our inductive practices, there remains the knotty problem of justifying these practices. Here Goodman borrows a page from Hume. Habits not only explain the practice, they are also the basis for its justification. Predicates are projected because they are entrenched, and they are entrenched because they have been projected. There is no important metaphysical or epistemic difference between the two versions. Likewise it is equally correct to say:

- (i) a hypothesis is lawlike, because it is projected and projected because it is taken to be lawlike
- (ii) a hypothesis is projectible, because it is confirmable by its instances, and it is confirmable by its instances, because it is projectible
- (iii) kinds are projectible, because they are natural, and they are natural, because they have been used in past projections
- (iv) hypotheses are entrenched, because they are simple, and they are simple, because they are entrenched.

Although these pairs of notions are not identical, they are inseparably linked. One props up the other, and together they provide a solid enough base to support weighty normative claims.

According to Goodman, as the rules of deductive logic aim to capture and articulate the norms of deductive practice, the rules of inductive logic aim to capture and articulate the norms of inductive practice. As we criticize arguments for failing to come up to the deductive standards the rules of logic lay down, so we criticize inductive arguments that violate the normative rules implicit in accepted inductive practices (*pace* [Stich and Nisbett, 1980; see Elgin, 1996]). There is no deeper foundation. There is no stepping out of the circle, and were it possible to do so, nothing helpful will be found.

The idea that a primary justification for projecting “All emeralds are green” rather than “All emeralds are grue.” is entrenchment remains very hard to swallow. Entrenchment it is felt is simply the wrong kind of thing to underwrite norms. Habit is too subjective a factor to ground or warrant inductive policies. Justification of inductive practice cannot rest on the practice of induction itself, for then there would be no objective justification for the accepted practice. There would be nothing unassailably fixed to keep inquiry from spinning out of control. Pragmatic solutions, such as Goodman’s, blur the difference between describing what a practice *is* and principles that determine what a practice should be. A firm, mind-independent, non-pragmatic foundation is required. Unless constraints are imposed from outside actual practice, whatever standards are adopted will merely express temporary subjective human commitments and preferences.<sup>17</sup>

<sup>17</sup>Likewise it is thought that there must be something more than practice and habit to *justify* the distinctions between primitive ideas and those that are derived, between laws and accidental

Goodman and other pragmatists, of course, do not deny that there is an important difference between the *is* and the *ought* of practice. What they do claim is that norms emerge from critical reflection on accepted practice. There is neither an *a priori* nor a neutral perspective outside of practice from which to impose norms on practice. Norms, however, are not mere descriptions. They aim to capture and advance our understanding of best practices. The principles that emerge from reflection on practice are not absolute, necessary or eternal. If a principle's ruling on cases is out of sync with informed intuitions and commitments, there will be pressure to rethink the authority of the principle. If the principle has served well and holds promise for continued fruitful application, its authority may exert enough force to withstand the discomfort provoked by some of its rulings. A constant need to strike a balance between the push of new cases and the pull of principle is to be expected, and it is most unlikely that there are algorithms for determining where to locate the equilibrium point. It is always possible, as well, that in setting norms more than one resolution of the trade-offs is acceptable.

Goodman's stance is that the norms of inductive logic are objective and justified in the only way it can make sense for them to be objective and justified. Hume was on solid ground in his appeal to habit both to explain and justify inductive practice. Pragmatic factors cannot be eliminated, and there is no good reason to seek their removal. Pluralism and the impermanence of norms does not entail anything goes. At any given time there is usually a reasonable amount of consensus around which practitioners can rally, if not completely agree. That the consensus may shift is not only possible but is welcome. What was good practice yesterday may not be acceptable today. Still standards can be adopted and a practice can be praised or criticized accordingly.

Goodman's bootstrap approach to justification has gained many more supporters since Rawls [1971] proposed a "reflective equilibrium" rationale for his principles of a just society. In the *Theory of Justice* (p. 48) Rawls, in fact, notes that his views have much in common with Goodman's views on the justification of inductive practices. This, perhaps, is another indication of how far afield problems and issues related to the New Riddle extend.

#### BIBLIOGRAPHY

- [Achinstein, 1983] P. Achinstein, ed. *The Concept of Evidence*. Oxford: Oxford University Press, 1983.
- [Armstrong, 1978] D. M. Armstrong. *Universals and Scientific Realism*, Vol. II of *A Theory of Universals*. Cambridge: Cambridge University Press, 1978.
- [Carnap, 1947] R. Carnap. On the Application of Inductive Logic. *Philosophy and Phenomenological Research* 8: 133-148, 1947.
- [Carnap, 1948] R. Carnap. Reply to Nelson Goodman. *Philosophy and Phenomenological Research* 8: 461-462, 1948.

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generalizations, between natural and unnatural kinds, between items that are similar and those that are not and between simple and complex hypotheses. Descriptions of practice lack epistemic status.

- [Carnap, 1950] R. Carnap. *Logical Foundations of Probability*. Chicago: University of Chicago Press, 1950.
- [Davidson, 1966] D. Davidson. Emeroses By Other Names. *Journal of Philosophy* 63: 778-780, 1966.
- [Earman, 1985] J. Earman. Concepts of Projectibility and the Problems of Induction. *Nous* 19: 521-535, 1985.
- [Elgin, 1993] C. Elgin. Outstanding Problems: Replies to ZiF Critics. *Synthese* 95: 129-140, 1993.
- [Elgin, 1997] C. Elgin, ed. *The Philosophy of Nelson Goodman* V. 2 New York: Garland Publishing, 1997.
- [Elgin, 1996] C. Elgin. *Considered Judgment*. Princeton: Princeton University Press, 1996.
- [Elgin, 1997] C. Elgin. *Between the Absolute and the Arbitrary*. Ithaca: Cornell University Press, 1997.
- [Foster and Martin, 1966] M. H. Foster and M. L. Martin, eds. *Probability, Confirmation, and Simplicity: Readings in the Philosophy of Inductive Logic*. New York: The Odyssey Press, Inc., 1966.
- [Godfried-Smith, 2003] P. Godfried-Smith. Goodman's Problem and Scientific Methodology. *Journal of Philosophy* 100: 573-90, 2003.
- [Good, 1975] I. J. Good. Explicativity, Corroboration and the Relative Odds of Hypotheses. *Synthese* 30: 39-73, 1975.
- [Goodman, 1946] N. Goodman. A Query on Confirmation. *Journal of Philosophy* 43: 383-385, 1946.
- [Goodman, 1947a] N. Goodman. On Infirmities of Confirmation Theory. *Philosophy and Phenomenological Research* 8: 149-151, 1947.
- [Goodman, 1947b] N. Goodman. The Problem of Counterfactual Conditionals. *Journal of Philosophy* 44: 113-128, 1947.
- [Goodman, 1951] N. Goodman. *The Structure of Appearance*. Cambridge: Harvard University Press, 1951.
- [Goodman, 1983] N. Goodman. *Fact, Fiction, and Forecast*. Cambridge: Harvard University Press, 1<sup>st</sup> edition 1955, 4<sup>th</sup> edition 1983.
- [Goodman, 1961] N. Goodman. Safety, Strength, Simplicity. *Philosophy of Science*, 28: 150-151, 1961.
- [Goodman, 1970] N. Goodman. Seven Strictures on Similarity. In *Experience and Theory*, L. Foster and J. Swanson, eds., pp. 19-29. Boston: University of Massachusetts Press, 1970.
- [Goodman, 1972] N. Goodman. *Problems and Projects*. Indianapolis: Bobbs Merrill Co., 1972
- [Goodman, 1976] N. Goodman. *Languages of Art* (2<sup>nd</sup> ed.) Indianapolis: Hackett 1976.
- [Hanen, 1971] M. Hanen. Confirmation and Adequacy Conditions. *Philosophy of Science* 38: 361-368, 1971.
- [Harman and Kulkarni, 2006] G. Harman and S. Kulkarni. The Problem of Induction. *Philosophy and Phenomenological Research* LXXII: 559-575, 2006.
- [Hempel, 1943] C. G. Hempel. A Purely Syntactical Definition of Confirmation. *Journal of Symbolic Logic* 8: 122-143, 1943.
- [Hempel, 1945] C. G. Hempel. Studies in the Logic of Confirmation. *Mind* 54: 1-26 and 97-121, 1945.
- [Hempel, 1960] C. G. Hempel. Inductive Inconsistencies. *Synthese* 12: 439-469, 1960.
- [Hempel, 1966] C. G. Hempel. *Philosophy of Natural Science*. Edgewood Cliffs: Prentice Hall, 1966.
- [Hullet and Schwartz, 1967] J. Hullet and R. Schwartz. Grue: Some Remarks. *Journal of Philosophy* 64: 259-271, 1967.
- [Hume, 2006] D. Hume. *An Enquiry Concerning Human Understanding*, (ed.) T. Beauchamp, Oxford: Oxford University Press, 2006.
- [Jackson, 1975] F. Jackson. Grue. *Journal of Philosophy* 72: 113-131, 1975.
- [Jeffrey, 1983] R. Jeffrey. *The Logic of Decision* 2<sup>nd</sup> ed. Chicago: University of Chicago Press, 1983.
- [Kennedy and Chihara, 1978] R. Kennedy and C. Chihara. Beyond Zabludowski and Competitors. *Philosophical Studies* 33: 229-53, 1978.
- [Kuhn, 1977] T. Kuhn. Objectivity, Value Judgment and Theory Choice. In *The Essential Tension*, Chicago: University of Chicago Press, 320-39, 1977.

- [Kyburg, 1964] H. E. Kyburg. Recent Work in Inductive Logic. *American Philosophical Quarterly* 4: 249-287, 1964.
- [Lewis, 1973] D. Lewis. Causation. *Journal of Philosophy* 70: 556-567, 1973.
- [Lewis, 1983] D. Lewis. New Work for a Theory of Universals. *Australasian Journal of Philosophy* 61: 343-377, 1983.
- [Norton, 2006] J. Norton. How Formal Equivalence of Grue and Green Defeats What is New in the New Riddle of Induction. *Synthese* 150: 185-207, 2006.
- [Piatelli-Palmarini, 1980] M. Piatelli-Palmarini, ed. *Language and Learning: the Debate between J. Piaget and N. Chomsky*. Cambridge : Harvard University Press, 1980.
- [Putnam, 1983] H. Putnam. Foreword to the Fourth Edition *Fact, Fiction, and Forecast*. Cambridge: Harvard University Press, vii-xvi, 1983.
- [Quine, 1970] W. V. Quine. Natural Kinds. In *Essays in Honor of Carl G. Hempel*, (ed) Nicholas Rescher et al., pp. 5-23, 1970.
- [Quine and Ullian, 1978] W. V. Quine and J. Ullian. *The Web of Belief*. 2<sup>nd</sup> ed. New York: Random House, 1978.
- [Rawls, 1971] J. Rawls. *A Theory of Justice*. Cambridge: Harvard University Press, 1971.
- [Salmon, 1970] W. C. Salmon. Bayes's Theorem and the History of Science. In *Historical and Philosophical Perspectives on Science*. In Minnesota Studies in the Philosophy of Science V, (ed) R. Stuewer. Minneapolis: University of Minneapolis Press, 68-86, 1970.
- [Scheffler, 1963] I. Scheffler. *The Anatomy of Inquiry: Philosophical Studies in the Theory of Science*. New York: Alfred A. Knopf, Inc., 1963.
- [Scheffler and Goodman, 1972] I. Scheffler and N. Goodman. Selective Confirmation and the Ravens. *Journal of Philosophy* 69: 78-83, 1972.
- [Scheffler, 1982] I. Scheffler. Projectibility: A Postscript. *Journal of Philosophy* 79: 334-36, 1982.
- [Schwartz, Scheffler and Goodman, 1970] R. Schwartz, I. Scheffler, and N. Goodman. An Improvement in the Theory of Projectibility. *Journal of Philosophy* 67: 605-608, 1970.
- [Schwartz, 1971] R. Schwartz. Confirmation and Conflict. *Journal of Philosophy* 68: 483-487, 1971.
- [Schwartz, 1999] R. Schwartz. Reflections on Projection. *Protosociology: After the Received View*. (eds.) G. Preyer & A. Ulfag, 1999.
- [Schwartz, 2005] R. Schwartz. Note on Goodman's Problem. *Journal of Philosophy*, 375-79, 2005.
- [Shoemaker, 1975] S. Shoemaker. On Projecting the Unprojectible. *Philosophical Review* 84: 178-219, 1975.
- [Shoemaker, 1980] S. Shoemaker. Properties, Causation and Projectibility. In *Applications of Inductive Logic* (eds.) L. J. Cohen & M. Hesse, Oxford: Oxford University Press, 291-312, 1980.
- [Stalker, 1994] D. Stalker, ed. *Grue: The New Riddle of Induction*. Chicago: Open Court, 1994.
- [Stemmer, 2004] N. Stemmer. The Goodman Paradox: Three Different Problems and a Naturalistic Solution of Two. *Journal for General Philosophy of Science* 35: 351-370, 2004.
- [Stich and Nisbett, 1980] S. Stich and R. E. Nisbett. Justification and the Psychology of Reasoning. *Philosophy of Science* 47: 188-202, 1980.
- [Swinburn, 1974] R. Swinburne, ed. *The Justification of Induction*. Oxford: Oxford University Press, 1974.
- [Ullian and Goodman, 1975] J. Ullian and N. Goodman. Bad Company: A reply to Mr. Zabudowski and Others. *Journal of Philosophy* 72: 142-145, 1975.
- [Wilson, 1979] M. Wilson. Maxwell's Condition - Goodman's Problem. *British Journal for the Philosophy of Science* 30: 107-123, 1979.
- [Zabudowski, 1974] A. Zabudowski. Concerning a Fiction about How Facts are Forecast. *Journal of Philosophy* 71: 97-112, 1974.
- [Zellner, Keuzenkamp and McAleer, 2001] A. Zellner, H. Keuzenkamp, and M. McAleer, eds. *Simplicity, Inference and Modeling*. Cambridge: Cambridge University Press, 2001.