

Fact, Fiction, *and* Forecast

NELSON GOODMAN

Harvard University Press
Cambridge, Massachusetts
and London, England

PREDICAMENT

1946

The chapter to follow was originally delivered as a lecture at the New York Philosophical Circle on May 11, 1946; and published with some revisions in the Journal of Philosophy in February 1947, volume xlv, pages 113-28. Only minor changes have been made in the present text.

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Printed in the United States of America

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Library of Congress Cataloging in Publication Data

Goodman, Nelson.

Fact, fiction, and forecast.

Includes bibliographical references and index.

1. Induction (Logic)—Addresses, essays, lectures.
2. Conditionals (Logic)—Addresses, essays, lectures.
3. Possibility—Addresses, essays, lectures. 4. Fore-
casting—Addresses, essays, lectures. 5. Science—
Philosophy—Addresses, essays, lectures. I. Title.

BC91.G66 1983

161

82-15764

ISBN 0-674-29070-4

ISBN 0-674-29071-2 (pbk.)

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by Hilary Putnam

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1946

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FOREWORD TO
THE FOURTH EDITION

Fact, Fiction, and Forecast has achieved the paradoxical status of a contemporary classic. It is a classic by virtue of being one of the few books that every serious student of philosophy in our time *has* to have read; it is contemporary not just because it is by a contemporary philosopher but because it speaks to what are still among the most widely discussed issues in philosophy.

Goodman totally recasts the traditional problem of induction. For him the problem is not to guarantee that induction will succeed in the future—we have no such guarantee—but to characterize what induction *is* in a way that is neither too permissive nor too vague. The central difficulty, which Goodman was the first to highlight, is the projection problem: what distinguishes the properties one can inductively project from a sample to a population from the properties that are more or less resistant to such projection?

Goodman's celebrated argument, which he uses to show that all predicates are not equally projectible, depends on his invention of the strange predicate "grue". He defines something as grue if it is either observed before a certain date and is green, or is not observed before that date and is blue. There is something very much like a

work of art about this piece of philosophical invention, but why? It isn't just that it has the aesthetic virtues of elegance, novelty, and simplicity. Perhaps what makes the argument so stunning is the rarity in philosophy of proofs that really are proofs. However, Goodman doesn't present his argument as a proof, but rather as a puzzle. Perhaps *that* is the artistry—that, and the fact that an elegant proof is conveyed by means of a simple example.

What did Goodman show? In his contribution to a widely read discussion, Jerry Fodor claimed it was that an innate ordering of hypotheses is needed for induction.¹ But that *isn't* what he showed; in fact, it isn't even right. There are models for induction in which no innate ordering of hypotheses or predicates is presupposed; Goodman's own model is one such. Hypotheses are ordered in a way that changes in the course of cultural and scientific history in his model. Even the principles Goodman uses to order hypotheses in the light of past inductive practice, for example, the principle of 'entrenchment', aren't innate in his view but are arrived at by philosophical reflection on the practice of our community.

Catherine Elgin has recently suggested to me a strong resemblance between Goodman's views and those of the later Wittgenstein, at least on one reading.² Such a comparison is more to the point than any attempt to relate Goodman's ideas to those of Noam Chomsky. Like Witt-

¹ See Fodor's and Chomsky's comments in Massimo Piattelli-Palmarini, ed., *Language and Learning* (Cambridge, Mass.: Harvard University Press, 1980), pp. 259–261, for example.

² The reading Elgin has in mind is due to Saul Kripke; see his *Wittgenstein on Rules and Private Language* (Cambridge, Mass.: Harvard University Press, 1982).

genstein, Goodman doesn't believe in looking for guarantees, foundations, or the 'furniture of the universe'. (He goes even farther than Wittgenstein in his rejection of traditional philosophy, describing himself in his most recent writing as a 'relativist' and an 'irrealist'.) What we have in Goodman's view, as, perhaps, in Wittgenstein's, are practices, which are right or wrong depending on how they square with our standards. And our standards are right or wrong depending on how they square with our practices. This is a circle, or better a spiral, but one that Goodman, like John Dewey, regards as virtuous.

I referred to Goodman's celebrated argument as a proof. What he proved, even if he did not put it that way, is that inductive logic isn't formal in the sense that deductive logic is. The *form* of an inference, in the sense familiar from deductive logic, cannot tell one whether that inference is inductively valid.

In order to 'solve' Goodman's problem one has therefore to provide some principle capable of selecting among inferences that do not differ in logical form, that is, on the basis of the particular predicates those inferences contain. Philosophers who dislike Goodman's proposal, because of its dependence on the actual history of past inductive projections in the culture, have come up with a number of 'solutions' that *don't* work. For example, some philosophers think a valid inductive inference must not contain any disjunctive predicates. However, this fails because, from the point of view of logic, being disjunctive is a *relational* attribute of predicates: whether a predicate is disjunctive is relative to the choice of a language. If one takes the familiar color predicates as primitive, then Goodman's predicate "grue" is a disjunctive predicate; if

one takes the unfamiliar predicates *grue* and *bleen* as primitive, however, then being green may be defined as being *grue* and observed prior to time *t* or being *bleen* and not observed prior to time *t*. Thus the predicate *grue* is disjunctive in a language with normal color predicates as primitive, while the normal color predicates are disjunctive in a language having as primitive the nonstandard predicates (call them “gruller” predicates) Goodman invented. No predicate is disjunctive or nondisjunctive in itself.

What I have just described is the situation as it looks to a logician. Rudolf Carnap proposed that over and above this way in which a predicate can be disjunctive or nondisjunctive, that is, relative to a language or a choice of primitives, a predicate can be intrinsically disjunctive or nondisjunctive. In effect, he postulates a metaphysical pointer that singles out, we know not how, certain predicates as qualitative, that is, as kosher from the point of view of induction. However, even if we rule out predicates like *grue*, which are, in Carnap’s view, nonqualitative, problems still remain, at least in his systems of inductive logic. For example, we will get abnormal degrees of confirmation for certain hypotheses if we take the magnitude “the square of the length” as primitive instead of the magnitude “length”.³ Yet both “length” and “length squared” are qualitative, according to Carnap. To

³ For example, in Carnap’s systems, relative to the evidence “*x* has length between 0 and 1”, the degree of confirmation of the hypothesis “*x* has length between 0 and $\frac{1}{2}$ ” is 0.5 if “length” is primitive, but 0.25 if “length squared” is primitive. This is so because the hypothesis can be rewritten as “the square of the length of *x* is between 0 and $\frac{1}{4}$ ”.

justify the choice of the standard primitive magnitude, length, he therefore postulated that some qualitative magnitudes, including length, are intrinsically fundamental. Logical Heaven itself tells us which predicates to take as primitive in our theories! These Carnapian views do not solve Goodman’s problem; they merely turn logic into metaphysics.

A more radical solution proposed by Wesley Salmon—and several other philosophers have made similar proposals—is that *ostensively defined* primitive predicates are what is needed for inductive logic. “Ostensive definability is the basis for distinguishing normal from pathological predicates”.⁴ However, ostensively definable predicates are all *observational* predicates, and the proposal to rule out all nonobservational predicates is unmotivated and too severe.

Unmotivated: Call a bacillus “S-shaped” if it looks so under a microscope. Then “is an S-shaped bacillus” isn’t observational but perfectly projectible. If one weakens “ostensively definable” by allowing oneself to use instruments, then, as Goodman points out, *grue* is ostensively definable: all one has to do is build a measuring instrument that flashes a red light if the time is before *t* (imagine that the measuring instrument contains an internal clock) and the instrument is scanning something green or if the time is later than *t* and the instrument is scanning something blue.⁵ Using such an instrument, one can tell whether or

⁴ From Salmon, “Russell on Scientific Inference”, in *Bertrand Russell’s Philosophy*, ed. G. Nakhnikian (New York: Barnes and Noble, 1974), p. 190.

⁵ I have here shifted from Goodman’s definition of *grue* to one proposed by Stephen Barker and Peter Achinstein, which can be used to make the same points.

not something is grue without knowing what time it is, by seeing whether or not the red light is flashing. Critics might object that such an instrument is really measuring the time, but there is a sense in which any measuring instrument that contains internal moving parts and whose correct functioning depends on those parts moving at the appropriate rate may be said to contain an internal clock. The point is that unless we rule out the use of mechanical aids to observation altogether, then we cannot rule out grue for the reason given.⁶

Too severe: If only ostensibly definable predicates are projectible, then how do we make inferences to the unobservable? One strength of Goodman's account is that it includes a mechanism by which new predicates, including nonobservation predicates, can acquire projectibility. These mechanisms, which are similar to what Hans Reichenbach called "cross induction", depend upon a relation between one hypothesis and another, called by Goodman an "overhypothesis", that contains higher-level predicates than the first. For example, "all the marbles in any bag are the same color" is an overhypothesis of "all the marbles in this bag are red". But if the higher-level predicates we are allowed to use are all ostensibly definable (as on Salmon's proposal), then an underhypothesis of a projectible hypothesis will always be about observables because the overhypothesis is, so the objector can't use Goodman's mechanisms to transfer projectibility from projectible observation predicates to nonobservation predicates, and Goodman's critics have failed to come up with any alternative mechanisms to do the job.

⁶ See Goodman's discussion in *Languages of Art*, 2nd ed. (Indianapolis: Hackett, 1976), pp. 100-101.

In any case, we don't want to rule out grue completely. Sometimes it is projectible, and his discussion allows for this explicitly.

The failure of these attempts to evade Goodman's problem does not show that our ordering of predicates must be based on entrenchment, but his choice of entrenchment accords with his metaphilosophy. Entrenchment depends on the frequency with which we have actually inductively projected a predicate in the past; whether Goodman is writing about art or induction, what he prizes is congruence with actual practice as it has developed in history. This may seem paradoxical in a philosopher who also prizes novelty and who is a friend of modernity, but Goodman sees no conflict here. In his view, what makes it possible to value and operate within both inherited traditions and novel activities and versions is the truth of pluralism. This pluralism is only hinted at in the present work, for instance, in the clear statement that which predicates are projectible is a matter of the contingent history of the culture, but it has become the dominant theme in his most recent work.⁷ Even if the choice of entrenchment as the primary source of projectibility is congruent with Goodman's metaphilosophy, that does not mean he excludes the possibility of any other solution to the projectibility problem a priori. Few philosophers are less aprioristic than Goodman. What he insists upon, and all that he insists upon in this connection, is that *any* proposed solution be judged by its ability to systematize what we actually do.

In this connection as in others, it is important to recog-

⁷ See especially his *Ways of Worldmaking* (Indianapolis: Hackett, 1978).

nize that Goodman is not interested in formalisms that we can't use. This pragmatism, in the best sense of the term, is apparent in his work on counterfactuals—another vexed area of contemporary philosophy in which Goodman's work, although negative in its upshot, has set the agenda for the subsequent discussion. Recent workers on the problem, for example, David Lewis, have produced formalistic schemes that presuppose a given totality of 'possible worlds' and a 'similarity metric' that measures their similarity.⁸ Such 'solutions' to the problem of counterfactuals are not solutions at all in Goodman's view, since we are not given any principles for telling which possible worlds are more or less similar to the actual world. Relying on intuition for the answer is no improvement on relying on intuition to tell us that the counterfactual we are interested in is right or wrong in the first place. Also, there aren't any 'possible but not actual' worlds. Carnap's formalized inductive logics, mentioned earlier, are in the same boat. Goodman respects formal logic but not when it dresses up a problem in a way that has no payoff in practice. He deplores the current love of formalism for formalism's sake.

This brings me to perhaps my most important remark about Goodman's philosophical methods and attitudes. Although he starts as, say, Rorty does, by rejecting certainty and by rejecting the idea of an ontological ground floor independent of our theorizing and, even more like Rorty, by rejecting the most fashionable problems of philosophy, he is totally free of the "now philosophy is over"

⁸ See David Lewis, *Counterfactuals* (Cambridge, Mass.: Harvard University Press, 1973).

mood that haunts much of twentieth-century philosophy.⁹ If there isn't a ready-made world, then let's construct worlds, says Goodman. If there aren't objective standards, then let's construct standards! Nothing is ready-made, but everything is to be made.

Goodman's prodigious output and enormous breadth of interest—he has written on the theory of constructional systems, on nominalistic foundations for mathematics, on the general theory of signs, on the philosophy of psychology, as well as on aesthetics and on the tasks of philosophy today—illustrates how far he is from sharing the view that philosophy is over. So does the constructive nature of much of his writing. Most philosophers are people with theses to defend; Goodman is a man with methods and concepts to sell (his word). But, he would remark, if there is no ready-made world, the line between a thesis and a construction dissolves.

As I already remarked, it's a mistake to see Goodman as providing support for any doctrine of innate ideas. It is not that he is uninterested in psychology; he has worked in it most of his life. The real problem, in his view, isn't what is innate; the real problem has to do with cultural evolution. We are world-makers; we are constantly making 'new worlds out of old ones'. What we see, perceive, touch, is all in flux—a flux of our own creation. The real psychological question is how we shape this flux and how we maneuver in it. In thinking about Goodman, I keep coming back to his optimism, or perhaps I should say his energy. He doesn't believe in progress in any sense that

⁹ See Richard Rorty, *Philosophy and the Mirror of Nature* (Princeton: Princeton University Press, 1979).

• FOREWORD •

implies things are getting better, or must in the future. But he does believe that novelty can be exciting and good as well as boring and bad; he finds construction and creation exciting and challenging. He believes, in short, that there is much, much we can do, and he prefers concrete and partial progress to grand and ultimately empty visions.

Hilary Putnam

INTRODUCTORY NOTES

INTRODUCTION TO THE FIRST EDITION, 1954

The chapters to follow were all originally delivered as lectures. Although seven years and a few thousand miles separated the delivery of the first from the delivery of the remaining three, the four represent a consecutive effort of thought on a closely integrated group of problems. Only the first has been published before.

In the summer of 1944, I had nearly completed a manuscript entitled 'Two Essays on Not-Being'. The first essay explained the counterfactual conditional, and the second made use of this explanation in dealing with potentiality, possibility, and dispositions. Some minor difficulties in the first essay still needed attention, however, and these led to less minor ones until, a few weeks later, my two essays were instances rather than treatments of not-being.

Grasping at the scientist's slim straw of solace, that failure is as significant as success, I used the detailed history of this frustration as the subject for a talk given in New York in May of 1946. It was published a few months later in the *Journal of Philosophy* as 'The Problem of Counterfactual Conditionals'.

The scores of articles that have been published since then have made so little progress towards settling the matter that current opinion varies all the way from the view that the problem is no problem at all to the view

that it is insoluble. Neither of these extremes is very well substantiated. The former is usually supported by the claim that we can, theoretically at least, get along without counterfactuals in the sciences. But however that may be, we do not yet by any means know how to get along without them (or transparent substitutes for them) in philosophy. The view that the problem is insoluble is sometimes supported by the citation of paradoxical counterfactuals that confound commonsense. But such cases do not argue insolubility; for if we can provide an interpretation that handles the clear cases successfully, we can let the unclear ones fall where they may.

The urge to dispose of the problem as spurious or insoluble is understandable, of course, in view of the repeated failures to find a solution. The trouble is, though, that what confronts us is not a single isolated problem but a closely knit family of problems. If we set one of them aside, we usually encounter much the same difficulties when we try to deal with the others. And if we set aside all the problems of dispositions, possibility, scientific law, confirmation, and the like, we virtually abandon the philosophy of science.

For some years, work on a book dealing with other matters took most of my time; but after *The Structure of Appearance* was published in 1951, I turned again to the problem of counterfactuals and kindred problems—and began to travel in all the old circles. When, in 1952, the University of London invited me to give some lectures the following year, I set to work with a conviction that some new approach must be found. The results of that work were reported in three lectures delivered in Lon-

don in May of 1953 under the general title *Fact, Fiction, and Forecast*.

In the present book, the first part 'Predicament—1946' consists of 'The Problem of Counterfactual Conditionals', reprinted without major changes. The second part 'Project—1953' consists of the three London lectures now printed for the first time. These have been somewhat revised, and rather extensive notes have been added. The greatest change, involving many additions and improvements, has occurred in the expansion of the last quarter of the final lecture into the last half of the fourth chapter. I am indebted to C. G. Hempel for many useful suggestions, and to Elizabeth F. Flower for valuable editorial assistance.

The two parts of the book are intimately related to each other in the ways I have described; but no attempt has been made to revise them to make a more continuous whole. The occasional duplications and minor disparities between the work of 1946 and the work of 1953 have been left untouched. Thus readers familiar with the article on counterfactuals or unready for its technicalities will find the second part a more or less self-contained unit, while other readers will find in the first part an essentially unaltered description of the state of affairs from which the London lectures took their departure. The layman and the beginning student may well read the second part first.

Throughout I have used commonplace and even trivial illustrations rather than more intriguing ones drawn from the sciences; for I feel that examples that attract the least interest to themselves are the least likely to divert attention from the problem or principle being explained. Once

the reader has grasped a point he can make his own more consequential applications. Thus although I talk of the freezing of radiators and the color of marbles, which are seldom discussed in books on chemistry or physics, what I am saying falls squarely within the philosophy of science.

As yet we are able to deal with only a few aspects of a few problems. We have to isolate for study a few simple aspects of science just as science has to isolate a few simple aspects of the world; and we are at an even more rudimentary stage in philosophy than in science. This, admittedly, is over-simplification. But conscious and cautious over-simplification, far from being an intellectual sin, is a prerequisite for investigation. We can hardly study at once all the ways in which everything is related to everything else.

Four lectures do not make a treatise. This is a report of work in process that I hope may prove to be work in progress. It might be thought of as consisting of first thoughts towards a far-off sequel to *The Structure of Appearance*. But no acquaintance with that book, and no knowledge of symbolic logic, is required for an understanding of the present work.

NOTE TO THE THIRD EDITION, 1973

Happily, the three rules set forth in the final chapter of the first edition, reduced to two in the second edition, can now be reduced to one. In the second edition, the second of the three rules was dropped since I had found that the cases this rule was designed to cover were taken care of by the first rule. Now, slight modification of the first rule, together with explicit recognition that a hypothesis may at a given time be neither projectible nor unprojectible but rather nonprojectible, has made the third of the original rules also unnecessary. Accordingly IV.4 has been rewritten, and changes thus required in IV.5 have been made.

For this result and others along the way, I am heavily indebted to Robert Schwartz and Israel Scheffler. Our joint report was published in the *Journal of Philosophy*, volume 67 (1970), pages 605 through 608, under the title "An Improvement in the Theory of Projectibility".

In the rather extensive discussion relating to this book, some interesting points have been made. Scheffler's examination of selective confirmation paved the way for Marsha Hanen's convincing argument that all the familiar so-called adequacy conditions for confirmation are dispensable. Wolfgang Stegmüller has corrected the

• INTRODUCTORY NOTES •

notion that 'anti-inductivists' of the school of Karl Popper escape the new riddle of induction. Elizabeth Shipley has quite justly remarked that along with number of projections, such other factors as the importance, variety, and Humean 'liveliness' of the projections contribute to the entrenchment of a predicate. Other writers have noted additional defects in the attempt to delimit the relevant conditions for counterfactuals; but since that attempt is here (I.2) rejected anyway on other grounds, further flaws are of minor interest. Some brief reservations and clarifications carried over from the second edition resulted from discussions with Scheffler, C. G. Hempel, and Howard Kahane. Kahane indeed deserves special, if left-handed, credit. Ironically, his persistent efforts to demolish the whole theory of projectibility by counterexample have instead shown that this admittedly tentative and fragmentary theory is, with some modifications and simplifications, more nearly adequate and more durable than I had supposed.

Among the most common mistakes in discussions of this book have been failures to recognize (1) that the projectibility status of a hypothesis normally varies from time to time, (2) that even an emerald existing from prehistoric time may be grue while remaining green, (3) that a major obstacle to a nonpragmatic way of ruling out 'grue-like' predicates is the lack of any non-question-begging definition of "grue-like", (4) that the discussion of possibility in Chapter II is concerned not with the question raised when we say that something may or may not be in fact a soandso but with the question raised when we say that something that is not in fact a soandso is nevertheless a possible soandso, (5) that since at any

• INTRODUCTORY NOTES •

time as many supported, unviolated, and unexhausted hypotheses are not projectible as are projectible, projectible hypotheses or predicates cannot be defined in terms of survival of the fittest, and (6) that the analogy I have drawn between justification of induction and justification of deduction is quite independent of the obvious fact that, when valid, deductive but not inductive inference always yields a true conclusion from true premisses.

Some of these matters, both positive and negative, have been discussed a little more fully in my *Problems and Projects* (Hackett, 1976, Chapter VIII), but could not be incorporated in the present text.

In addition to the important improvement mentioned above, several minor revisions have been made in this edition, and a new index has been prepared by Samuel Scheffler.

NOTE TO THE FOURTH EDITION, 1983

That this book not only continues to be widely read by philosophers but also is coming to be recognized as relevant far beyond induction and even beyond philosophy is especially gratifying. The conclusion that projectibility cannot be defined syntactically or semantically has been seen to be highly consequential for psychology and has inspired lively controversy over just what the consequences are. In my own recent work—for example, *Ways of Worldmaking*—the treatment of inductive validity developed here has unexpected ramifications, for rightness of many sorts, including fairness of sample and rightness of representation and design, involves rightness of categorization. And since rightness of categorization is obviously a matter not of discovering 'natural' kinds but of organizing relevant kinds, the role of entrenchment must be taken into account.

The book's effectiveness as an irritant seems not to lessen through the years. Attacks upon it do not decline in volume or vehemence or futility. All this smoke is no sign of fire. Philosophers not bothering to unravel the specious arguments frequently offered for objections advertised as fatal should be warned against supposing that the theory sketched in Chapter IV has been shown to have the con-

sequence that no hypothesis is projectible or that only false hypotheses are projectible, or that every projection requires countless decisions concerning other hypotheses, or that no new predicates can be projected, or that the new riddle never was any riddle anyway. Occasionally in the course of controversy some point in the book has been clarified or underlined; for example, that one condition upon projectibility is *not* that there is an *assumption of no conflict* with any no-less-well-entrenched hypothesis but that there is *no assumption of conflict* with any such hypothesis. But in most such cases the attentive reader of the book will find his own way well enough without help.

The following papers mentioned in the text—"An Improvement in the Theory of Projectibility" (page xxi), "A Query on Confirmation" (Notes I.16, III.9, III.11), and "Infirmities of Confirmation Theory" (Note III.11)—have been reprinted, along with other relevant material, in Chapter VIII of my *Problems and Projects* (Indianapolis: Hackett, 1976).

Nelson Goodman

Fact, Fiction, and Forecast

Fourth Edition

Other Books by Nelson Goodman

The Structure of Appearance

Problems and Projects

Languages of Art

Ways of Worldmaking

I

THE PROBLEM OF
COUNTERFACTUAL CONDITIONALS¹

1. The Problem in General

The analysis of counterfactual conditionals is no fussy little grammatical exercise. Indeed, if we lack the means for interpreting counterfactual conditionals, we can hardly claim to have any adequate philosophy of science. A satisfactory definition of scientific law, a satisfactory theory of confirmation or of disposition terms (and this includes not only predicates ending in "ible" and "able" but almost every objective predicate, such as "is red"), would solve a large part of the problem of counterfactuals. Conversely, a solution to the problem of counterfactuals would give us the answer to critical questions about law, confirmation, and the meaning of potentiality.

I am not at all contending that the problem of counterfactuals is logically or psychologically the first of these related problems. It makes little difference where we start if we can go ahead. If the study of counterfactuals has up to now failed this pragmatic test, the alternative approaches are little better off.

¹ My indebtedness in several matters to the work of C. I. Lewis has seemed too obvious to call for detailed mention.

• COUNTERFACTUAL CONDITIONALS •

What, then, is the *problem* about counterfactual conditionals? Let us confine ourselves to those in which antecedent and consequent are inalterably false—as, for example, when I say of a piece of butter that was eaten yesterday, and that had never been heated,

If that piece of butter had been heated to 150° F., it would have melted.

Considered as truth-functional compounds, all counterfactuals are of course true, since their antecedents are false. Hence

If that piece of butter had been heated to 150° F., it would not have melted

would also hold. Obviously something different is intended, and the problem is to define the circumstances under which a given counterfactual holds while the opposing conditional with the contradictory consequent fails to hold. And this criterion of truth must be set up in the face of the fact that a counterfactual by its nature can never be subjected to any direct empirical test by realizing its antecedent.

In one sense the name “problem of counterfactuals” is misleading, because the problem is independent of the form in which a given statement happens to be expressed. The problem of counterfactuals is equally a problem of factual conditionals, for any counterfactual can be transposed into a conditional with a true antecedent and consequent; e.g.,

Since that butter did not melt, it wasn't heated to 150° F.

The possibility of such transformation is of no great importance except to clarify the nature of our problem. That

• COUNTERFACTUAL CONDITIONALS •

“since” occurs in the contrapositive shows that what is in question is a certain kind of connection between the two component sentences; and the truth of statements of this kind—whether they have the form of counterfactual or factual conditionals or some other form—depends not upon the truth or falsity of the components but upon whether the intended connection obtains. Recognizing the possibility of transformation serves mainly to focus attention on the central problem and to discourage speculation as to the nature of counterfactuals. Although I shall begin my study by considering counterfactuals as such, it must be borne in mind that a general solution would explain the kind of connection involved irrespective of any assumption as to the truth or falsity of the components.

The effect of transposition upon conditionals of another kind, which I call “semifactuals”, is worth noticing briefly. Should we assert

Even if the match had been scratched, it still would not have lighted,

we would uncompromisingly reject as an equally good expression of our meaning the contrapositive,

Even if the match lighted, it still wasn't scratched.

Our original intention was to affirm not that the non-lighting could be inferred from the scratching, but simply that the lighting could not be inferred from the scratching. Ordinarily a semifactual conditional has the force of denying what is affirmed by the opposite, fully counterfactual conditional. The sentence

Even had that match been scratched, it still wouldn't have lighted