14. Joseph Ullian and Nelson Goodman, "Bad Company: A Reply to Mr. Zabludowski and Others," *J. Philosophy 72,* 5 (March 13, 1975), 142.

15. Israel Scheffler, *The Anatomy of Inquiry* (Indianapolis: Hackett, 1981; first published 1963), 320.

Natural Kinds

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What tends to confirm an induction? This question has been aggravated on the one hand by Hempel's puzzle of the nonblack nonravens,[1] and exacerbated on the other by Goodman's puzzle of the grue emeralds.[2] I shall begin my remarks by relating the one puzzle to the other, and the other to an innate flair that we have for natural kinds. Then I shall devote the rest of the paper to reflections on the nature of this notion of natural kinds and its relation to science.

Hempel's puzzle is that just as each black raven tends to confirm the law that all ravens are black, so each green leaf, being a nonblack nonraven, should tend to confirm the law that all nonblack things are nonravens, that is, again, that all ravens are black. What is paradoxical is that a green leaf should count toward the law that all ravens are black.

Goodman propounds his puzzle by requiring us to imagine that emeralds, having been identified by some criterion other than color, are now being examined one after another and all up to now are found to be green. Then he proposes to call anything 'grue' that is examined today or earlier and found to be green or is not examined before tomorrow and is blue. Should we expect the first one examined tomorrow to be green, because all examined up to now were green? But all examined up to now were also grue; so why not expect the first one tomorrow to be grue, and therefore blue?

The predicate 'green,' Goodman says,[3] is *projectible;* 'grue' is not. He says this by way of putting a name to the problem. His step toward solution is his doctrine of what he calls entrenchment,[4] which I shall touch on later. Meanwhile the terminological point is simply that projectible predicates are predicates ζ and η whose shared instances all do count, for whatever reason, toward confirmation of $\ulcorner$All ζ are $\eta\urcorner$.

Now I propose assimilating Hempel's puzzle to Goodman's by inferring

From *Essays in Honor of Carl G. Hempel,* edited by Nicholas Rescher et al., 1–23, Dordrecht: D. Reidel, 1970.

from Hempel's that the complement of a projectible predicate need not be projectible. 'Raven' and 'black' are projectible; a black raven does count toward 'All ravens are black.' Hence a black raven counts also, indirectly, toward 'All nonblack things are nonravens,' since this says the same thing. But a green leaf does not count toward 'All nonblack things are nonravens,' nor, therefore, toward 'All ravens are black'; 'nonblack' and 'nonraven' are not projectible. 'Green' and 'leaf' are projectible, and the green leaf counts toward 'All leaves are green' and 'All green things are leaves'; but only a black raven can confirm 'All ravens are black,' the complements not being projectible.

If we see the matter in this way, we must guard against saying that a statement $\ulcorner$All ζ are $\eta\urcorner$ is lawlike only if ζ and η are projectible. 'All nonblack things are nonravens' is a law despite its nonprojectible terms, since it is equivalent to 'All ravens are black.' Any statement is lawlike that is logically *equivalent* to $\ulcorner$All ζ are $\eta\urcorner$ for some projectible ζ and η.[5]

Having concluded that the complement of a projectible predicate need not be projectible, we may ask further whether there is *any* projectible predicate whose complement is projectible. I can conceive that there is not, when complements are taken strictly. We must not be misled by limited or relative complementation; 'male human' and 'nonmale human' are indeed both projectible.

To get back now to the emeralds, why do we expect the next one to be green rather than grue? The intuitive answer lies in similarity, however subjective. Two green emeralds are more similar than two grue ones would be if only one were green. Green things, or at least green emeralds, are a kind.[6] A projectible predicate is one that is true of all and only the things of a kind. What makes Goodman's example a puzzle, however, is the dubious scientific standing of a general notion of similarity, or of kind.

The dubiousness of this notion is itself a remarkable fact. For surely there is nothing more basic to thought and language than our sense of similarity; our sorting of things into kinds. The usual general term, whether a common noun or a verb or an adjective, owes its generality to some resemblance among the things referred to. Indeed, learning to use a word depends on a double resemblance: first, a resemblance between the present circumstances and past circumstances in which the word was used, and second, a phonetic resemblance between the present utterance of the word and past utterances of it. And every reasonable expectation depends on resemblance of circumstances, together with our tendency to expect similar causes to have similar effects.

The notion of a kind and the notion of similarity or resemblance seem to be variants or adaptations of a single notion. Similarity is immediately definable in terms of kind; for things are similar when they are two of a kind. The very words for 'kind' and 'similar' tend to run in etymologically

cognate pairs. Cognate with 'kind' we have 'akin' and 'kindred.' Cognate with 'like' we have 'ilk.' Cognate with 'similar' and 'same' and 'resemble' there are '*sammeln*' and 'assemble,' suggesting a gathering into kinds.

We cannot easily imagine a more familiar or fundamental notion than this, or a notion more ubiquitous in its applications. On this score it is like the notions of logic: like identity, negation, alternation, and the rest. And yet, strangely, there is something logically repugnant about it. For we are baffled when we try to relate the general notion of similarity significantly to logical terms. One's first hasty suggestion might be to say that things are similar when they have all, or most, or many properties in common. Or, trying to be less vague, one might try defining comparative similarity—'*a* is more similar to *b* than to *c*'—as meaning that *a* shares more properties with *b* than with *c*. But any such course only reduces our problem to the unpromising task of settling what to count as a property.

The nature of the problem of what to count as a property can be seen by turning for a moment to set theory. Things are viewed as going together into sets in any and every combination, describable and indescribable. Any two things are joint members of any number of sets. Certainly then we cannot define '*a* is more similar to *b* than to *c*' to mean that *a* and *b* belong jointly to more sets than *a* and *c* do. If properties are to support this line of definition where sets do not, it must be because properties do not, like sets, take things in every random combination. It must be that properties are shared only by things that are significantly similar. But properties in such a sense are no clearer than kinds. To start with such a notion of property, and define similarity on that basis, is no better than accepting similarity as undefined.

The contrast between properties and sets which I suggested just now must not be confused with the more basic and familiar contrast between properties, as intensional, and sets as extensional. Properties are intensional in that they may be counted as distinct properties even though wholly coinciding in respect of the things that have them. There is no call to reckon kinds as intensional. Kinds can be seen as sets, determined by their members. It is just that not all sets are kinds.

If similarity is taken simple-mindedly as a yes-or-no affair, with no degrees, then there is no containing of kinds within broader kinds. For, as remarked, similarity now simply means belonging to some one same kind. If all colored things comprise a kind, then all colored things count as similar, and the set of all red things is too narrow to count as a kind. If on the other hand the set of all red things counts as a kind, then colored things do not all count as similar, and the set of all colored things is too broad to count as a kind. We cannot have it both ways. Kinds can, however, overlap; the red things can comprise one kind, the round another.

When we move up from the simple dyadic relation of similarity to the more serious and useful triadic relation of comparative similarity, a correlative change takes place in the notion of kind. Kinds come to admit now not only of overlapping but also of containment one in another. The set of all red things and the set of all colored things can now both count as kinds; for all colored things can now be counted as resembling one another more than some things do, even though less, on the whole, than red ones do.

At this point, of course, our trivial definition of similarity as sameness of kind breaks down; for almost any two things could count now as common members of some broad kind or other, and anyway we now want to define comparative or triadic similarity. A definition that suggests itself is this: *a* is more similar to *b* than to *c* when *a* and *b* belong jointly to more kinds than *a* and *c* do. But even this works only for finite systems of kinds.

The notion of kind and the notion of similarity seemed to be substantially one notion. We observed further that they resist reduction to less dubious notions, as of logic or set theory. That they at any rate be definable each in terms of the other seems little enough to ask. We just saw a somewhat limping definition of comparative similarity in terms of kinds. What now of the converse project, definition of kind in terms of similarity?

One may be tempted to picture a kind, suitable to a comparative similarity relation, as any set which is 'qualitatively spherical' in this sense: it takes in exactly the things that differ less than so-and-so much from some central norm. If without serious loss of accuracy we can assume that there are one or more actual things (*paradigm cases*) that nicely exemplify the desired norm, and one or more actual things (*foils*) that deviate just barely too much to be counted into the desired kind at all, then our definition is easy: ***the kind with paradigm a and foil b*** is the set of all the things to which *a* is more similar than *a* is to *b*. More generally, then, a set may be said to be a ***kind*** if and only if there are *a* and *b*, known or unknown, such that the set is the kind with paradigm *a* and foil *b*.

If we consider examples, however, we see that this definition does not give us what we want as kinds. Thus take red. Let us grant that a central shade of red can be picked as norm. The trouble is that the paradigm cases, objects in just that shade of red, can come in all sorts of shapes, weights, sizes, and smells. Mere degree of overall similarity to any one such paradigm case will afford little evidence of degree of redness, since it will depend also on shape, weight, and the rest. If our assumed relation of comparative similarity were just comparative chromatic similarity, then our paradigm-and-foil definition of kind would indeed accommodate redkind. What the definition will not do is distill purely chromatic kinds from mixed similarity.

A different attempt, adapted from Carnap, is this: a set is a kind if all its members are more similar to one another than they all are to any one thing outside the set. In other words, each nonmember differs more from some member than that member differs from any member. However, as Goodman showed in a criticism of Carnap,[7] this construction succumbs to what Goodman calls the difficulty of imperfect community. Thus consider the set of all red round things, red wooden things, and round wooden things. Each member of this set resembles each other member somehow: at least in being red, or in being round, or in being wooden, and perhaps in two or all three of these respects or others. Conceivably, moreover, there is no one thing outside the set that resembles every member of the set to even the least of these degrees. The set then meets the proposed definition of kind. Yet surely it is not what anyone means by a kind. It admits yellow croquet balls and red rubber balls while excluding yellow rubber balls.

The relation between similarity and kind, then, is less clear and neat than could be wished. Definition of similarity in terms of kind is halting, and definition of kind in terms of similarity is unknown. Still the two notions are in an important sense correlative. They vary together. If we reassess something *a* as less similar to *b* than to *c*, where it had counted as more similar to *b* than to *c*, surely we will correspondingly permute *a*, *b*, and *c* in respect of their assignment to kinds; and conversely.

I have stressed how fundamental the notion of similarity or of kind is to our thinking, and how alien to logic and set theory. I want to go on now to say more about how fundamental these notions are to our thinking, and something also about their nonlogical roots. Afterward I want to bring out how the notion of similarity or of kind changes as science progresses. I shall suggest that it is a mark of maturity of a branch of science that the notion of similarity or kind finally dissolves, so far as it is relevant to that branch of science. That is, it ultimately submits to analysis in the special terms of that branch of science and logic.

For deeper appreciation of how fundamental similarity is, let us observe more closely how it figures in the learning of language. One learns by *ostension* what presentations to call yellow; that is, one learns by hearing the word applied to samples. All he has to go on, of course, is the similarity of further cases to the samples. Similarity being a matter of degree, one has to learn by trial and error how reddish or brownish or greenish a thing can be and still be counted yellow. When he finds he has applied the word too far out, he can use the false cases as samples to the contrary; and then he can proceed to guess whether further cases are yellow or not by considering whether they are more similar to the in-group or the out-group. What one thus uses, even at this primitive stage of learning, is a fully functioning sense of similarity, and relative similarity at that: *a* is more similar to *b* than to *c*.

All these delicate comparisons and shrewd inferences about what to call yellow are, in Sherlock Holmes's terminology, elementary. Mostly the process is unconscious. It is the same process by which an animal learns to respond in distinctive ways to his master's commands or other discriminated stimulations.

The primitive sense of similarity that underlies such learning has, we saw, a certain complexity of structure: *a* is more similar to *b* than to *c*. Some people have thought that it has to be much more complex still: that it depends irreducibly on *respects,* thus similarity in color, similarity in shape, and so on. According to this view, our learning of yellow by ostension would have depended on our first having been told or somehow apprised that it was going to be a question of color. Now hints of this kind are a great help, and in our learning we often do depend on them. Still one would like to be able to show that a single general standard of similarity, but of course comparative similarity, is all we need, and that respects can be abstracted afterward. For instance, suppose the child has learned of a yellow ball and block that they count as yellow, and of a red ball and block that they do not, and now he has to decide about a yellow cloth. Presumably he will find the cloth more similar to the yellow ball and to the yellow block than to the red ball or red block; and he will not have needed any prior schooling in colors and respects. Carnap undertook to show long ago how some respects, such as color, could by an ingenious construction be derived from a general similarity notion;[8] however, this development is challenged, again, by Goodman's difficulty of imperfect community.

A standard of similarity is in some sense innate. This point is not against empiricism; it is a commonplace of behavioral psychology. A response to a red circle, if it is rewarded, will be elicited again by a pink ellipse more readily than by a blue triangle; the red circle resembles the pink ellipse more than the blue triangle. Without some such prior spacing of qualities, we could never acquire a habit; all stimuli would be equally alike and equally different. These spacings of qualities, on the part of men and other animals, can be explored and mapped in the laboratory by experiments in conditioning and extinction.[9] Needed as they are for all learning, these distinctive spacings cannot themselves all be learned; some must be innate.

If then I say that there is an innate standard of similarity, I am making a condensed statement that can be interpreted, and truly interpreted, in behavioral terms. Moreover, in this behavioral sense it can be said equally of other animals that they have an innate standard of similarity too. It is part of our animal birthright. And, interestingly enough, it is characteristically animal in its lack of intellectual status. At any rate we noticed earlier how alien the notion is to mathematics and logic.

This innate qualitative spacing of stimulations was seen to have one of its human uses in the ostensive learning of words like 'yellow.' I should add as a cautionary remark that this is not the only way of learning words, nor the commonest; it is merely the most rudimentary way. It works when the question of the reference of a word is a simple question of spread: how much of our surroundings counts as yellow, how much counts as water, and so on. Learning a word like 'apple' or 'square' is more complicated, because here we have to learn also where to say that one apple or square leaves off and another begins. The complication is that apples do not add up to an apple, nor squares, generally, to a square. 'Yellow' and 'water' are mass terms, concerned only with spread; 'apple' and 'square' are terms of divided reference, concerned with both spread and individuation. Ostension figures in the learning of terms of this latter kind too, but the process is more complex.[10] And then there are all the other sorts of words, all those abstract and neutral connectives and adverbs and all the recondite terms of scientific theory; and there are also the grammatical constructions themselves to be mastered. The learning of these things is less direct and more complex still. There are deep problems in this domain, but they lie aside from the present topic.

Our way of learning 'yellow,' then, gives less than a full picture of how we learn language. Yet more emphatically, it gives less than a full picture of the human use of an innate standard of similarity, or innate spacing of qualities. For, as remarked, every reasonable expectation depends on similarity. Again on this score, other animals are like man. Their expectations, if we choose so to conceptualize their avoidance movements and salivation and pressing of levers and the like, are clearly dependent on their appreciation of similarity. Or, to put matters in their methodological order, these avoidance movements and salivation and pressing of levers and the like are typical of what we have to go on in mapping the animals' appreciation of similarity, their spacing of qualities.

Induction itself is essentially only more of the same: animal expectation or habit formation. And the ostensive learning of words is an implicit case of induction. Implicitly the learner of 'yellow' is working inductively toward a general law of English verbal behavior, though a law that he will never try to state; he is working up to where he can in general judge when an English speaker would assent to 'yellow' and when not.

Not only is ostensive learning a case of induction; it is a curiously comfortable case of induction, a game of chance with loaded dice. At any rate this is so if, as seems plausible, each man's spacing of qualities is enough like his neighbor's. For the learner is generalizing on his yellow samples by similarity considerations, and his neighbors have themselves acquired the use of the word 'yellow,' in their day, by the same similarity considerations. The learner of 'yellow' is thus making his induction in a friendly world. Always, induction expresses our hope that similar causes will have similar effects; but when the induction is the ostensive learning of a word, that pious hope blossoms into a foregone conclusion. The uniformity of people's quality spaces virtually assures that similar presentations will elicit similar verdicts.

It makes one wonder the more about other inductions, where what is sought is a generalization not about our neighbor's verbal behavior but about the harsh impersonal world. It is reasonable that our quality space should match our neighbor's, we being birds of a feather; and so the general trustworthiness of induction in the ostensive learning of words was a put-up job. To trust induction as a way of access to the truths of nature, on the other hand, is to suppose, more nearly, that our quality space matches that of the cosmos. The brute irrationality of our sense of similarity, its irrelevance to anything in logic and mathematics, offers little reason to expect that this sense is somehow in tune with the world—a world which, unlike language, we never made. Why induction should be trusted, apart from special cases such as the ostensive learning of words, is the perennial philosophical problem of induction.

One part of the problem of induction, the part that asks why there should be regularities in nature at all, can, I think, be dismissed. *That* there are or have been regularities, for whatever reason, is an established fact of science; and we cannot ask better than that. *Why* there have been regularities is an obscure question, for it is hard to see what would count as an answer. What does make clear sense is this other part of the problem of induction: why does our innate subjective spacing of qualities accord so well with the functionally relevant groupings in nature as to make our inductions tend to come out right? Why should our subjective spacing of qualities have a special purchase on nature and a lien on the future?

There is some encouragement in Darwin. If people's innate spacing of qualities is a gene-linked trait, then the spacing that has made for the most successful inductions will have tended to predominate through natural selection.[11] Creatures inveterately wrong in their inductions have a pathetic but praiseworthy tendency to die before reproducing their kind.

At this point let me say that I shall not be impressed by protests that I am using inductive generalizations, Darwin's and others, to justify induction, and thus reasoning in a circle. The reason I shall not be impressed by this is that my position is a naturalistic one; I see philosophy not as an a priori propaedeutic or groundwork for science, but as continuous with science. I see philosophy and science as in the same boat—a boat which, to revert to Neurath's figure as I so often do, we can rebuild only at sea while staying afloat in it. There is no external vantage point, no first philosophy. All scientific findings, all scientific conjectures that are at present plausible, are therefore in my view as welcome for use in philosophy as elsewhere. For me, then, the problem of induction is a problem about the world: a problem of how we, as we now are (by our present scientific lights), in a world we never

made, should stand better than random or coin-tossing chances of coming out right when we predict by inductions which are based on our innate, scientifically unjustified similarity standards. Darwin's natural selection is a plausible partial explanation.

It may, in view of a consideration to which I next turn, be almost explanation enough. This consideration is that induction, after all, has its conspicuous failures. Thus take color. Nothing in experience, surely, is more vivid and conspicuous than color and its contrasts. And the remarkable fact, which has impressed scientists and philosophers as far back at least as Galileo and Descartes, is that the distinctions that matter for basic physical theory are mostly independent of color contrasts. Color impresses man; raven black impresses Hempel; emerald green impresses Goodman. But color is cosmically secondary. Even slight differences in sensory mechanisms from species to species, Smart remarks,[12] can make overwhelming differences in the grouping of things by color. Color is king in our innate quality space, but undistinguished in cosmic circles. Cosmically, colors would not qualify as kinds.

Color is helpful at the food-gathering level. Here it behaves well under induction, and here, no doubt, has been the survival value of our color-slanted quality space. It is just that contrasts that are crucial for such activities can be insignificant for broader and more theoretical science. If man were to live by basic science alone, natural selection would shift its support to the color-blind mutation.

Living as he does by both bread and basic science, man is torn. Things about his innate similarity standards that are helpful in the one sphere can be a hindrance in the other. Credit is due to man's inveterate ingenuity, or human sapience, for having worked around the blinding dazzle of color vision and found the more significant regularities elsewhere. Evidently natural selection has dealt with the conflict by endowing man doubly: with both a color-slanted quality space and the ingenuity to rise above it.

He has risen above it by developing modified systems of kinds, hence modified similarity standards for scientific purposes. By the trial-and-error process of theorizing he has regrouped things into new kinds which prove to lend themselves to many inductions better than the old.

A crude example is the modification of the notion of fish by excluding whales and porpoises. Another taxonomic example is the grouping of kangaroos, opossums, and marsupial mice in a single kind, marsupials, while excluding ordinary mice. By primitive standards the marsupial mouse is more similar to the ordinary mouse than to the kangaroo; by theoretical standards the reverse is true.

A theoretical kind need not be a modification of an intuitive one. It may issue from theory full-blown, without antecedents; for instance the kind which comprises positively charged particles. We revise our standards of similarity or of natural kinds on the strength, as Goodman remarks,[13] of second-order inductions. New groupings, hypothetically adopted at the suggestion of a growing theory, prove favorable to inductions and so become 'entrenched.' We newly establish the projectibility of some predicate, to our satisfaction, by successfully trying to project it. In induction nothing succeeds like success.

Between an innate concept of similarity or spacing of qualities and a scientifically sophisticated one, there are all gradations. Science, after all, differs from common sense only in degree of methodological sophistication. Our experiences from earliest infancy are bound to have overlaid our innate spacing of qualities by modifying and supplementing our grouping habits little by little, inclining us more and more to an appreciation of theoretical kinds and similarities, long before we reach the point of studying science systematically as such. Moreover, the later phases do not wholly supersede the earlier; we retain different similarity standards, different systems of kinds, for use in different contexts. We all still say that a marsupial mouse is more like an ordinary mouse than a kangaroo, except when we are concerned with genetic matters. Something like our innate quality space continues to function alongside the more sophisticated regroupings that have been found by scientific experience to facilitate induction.

We have seen that a sense of similarity or of kinds is fundamental to learning in the widest sense—to language learning, to induction, to expectation. Toward a further appreciation of how utterly this notion permeates our thought, I want now to point out a number of other very familiar and central notions which seem to depend squarely on this one. They are notions that are definable in terms of similarity, or kinds, and further irreducible.

A notable domain of examples is the domain of dispositions, such as Carnap's example of solubility in water. To say of some individual object that it is soluble in water is not to say merely that it always dissolves when in water, because this would be true by default of any object, however insoluble, if it merely happened to be destined never to get into water. It is to say rather that it *would* dissolve if it were in water; but this account brings small comfort, since the device of a subjunctive conditional involves all the perplexities of disposition terms and more. Thus far I simply repeat Carnap.[14] But now I want to point out what could be done in this connection with the notion of kind. Intuitively, what qualifies a thing as soluble though it never gets into water is that it is of the same kind as the things that actually did or will dissolve; it is similar to them. Strictly we cannot simply say '*the* same kind,' nor simply 'similar,' when we have wider and narrower kinds, less and more similarity. Let us then mend our definition by saying that the soluble things are the common members of *all* such kinds. A thing is soluble if *each* kind that is broad enough to embrace all actual victims of solution embraces it too.

Graphically the idea is this: we make a set of all the sometime victims, all the things that actually did or will dissolve in water, and then we add just enough other things to round the set out into a kind. This is the water-soluble kind.

If this definition covers just the desired things, the things that are really soluble in water, it owes its success to a circumstance that could be otherwise. The needed circumstance is that a sufficient variety of things actually get dissolved in water to assure their not all falling under any one kind narrower than the desired water-soluble kind itself. But it is a plausible circumstance, and I am not sure that its accidental character is a drawback. If the trend of events had been otherwise, perhaps the solubility concept would not have been wanted.

However, if I seem to be defending this definition, I must now hasten to add that of course it has much the same fault as the definition which used the subjunctive conditional. This definition uses the unreduced notion of kind, which is certainly not a notion we want to rest with either; neither theoretical kind nor intuitive kind. My purpose in giving the definition is only to show the link between the problem of dispositions and the problem of kinds.

As between theoretical and intuitive kinds, certainly the theoretical ones are the ones wanted for purposes of defining solubility and other dispositions of scientific concern. Perhaps 'amiable' and 'reprehensible' are disposition terms whose definitions should draw rather on intuitive kinds.

Another dim notion, which has intimate connections with dispositions and subjunctive conditionals, is the notion of cause; and we shall see that it too turns on the notion of kinds. Hume explained cause as invariable succession, and this makes sense as long as the cause and effect are referred to by general terms. We can say that fire causes heat, and we can mean thereby, as Hume would have it, that each event classifiable under the head of fire is followed by an event classifiable under the head of heat, or heating up. But this account, whatever its virtues for these general causal statements, leaves singular causal statements unexplained.

What does it mean to say that the kicking over of a lamp in Mrs. O'Leary's barn caused the Chicago fire? It cannot mean merely that the event at Mrs. O'Leary's belongs to a set, and the Chicago fire belongs to a set, such that there is invariable succession between the two sets: every member of the one set is followed by a member of the other. This paraphrase is trivially true and too weak. Always, if one event happens to be followed by another, the two belong to certain sets between which there is invariable succession. We can rig the sets arbitrarily. Just put any arbitrary events in the first set, including the first of the two events we are interested in; and then in the other set put the second of those two events, together with other events that happen to have occurred just after the other members of the first set.

Because of this way of trivialization, a singular causal statement says no more than that the one event was followed by the other. That is, it says no more if we use the definition just now contemplated; which, therefore, we must not. The trouble with that definition is clear enough: it is the familiar old trouble of the promiscuity of sets. Here, as usual, kinds, being more discriminate, enable us to draw distinctions where sets do not. To say that one event caused another is to say that the two events are of *kinds* between which there is invariable succession. If this correction does not yet take care of Mrs. O'Leary's cow, the fault is only with invariable succession itself, as affording too simple a definition of general causal statements; we need to hedge it around with provisions for partial or contributing causes and a good deal else. That aspect of the causality problem is not my concern. What I wanted to bring out is just the relevance of the notion of kinds, as the needed link between singular and general causal statements.

We have noticed that the notion of kind, or similarity, is crucially relevant to the notion of disposition, to the subjunctive conditional, and to singular causal statements. From a scientific point of view these are a pretty disreputable lot. The notion of kind, or similarity, is equally disreputable. Yet some such notion, some similarity sense, was seen to be crucial to all learning, and central in particular to the processes of inductive generalization and prediction which are the very life of science. It appears that science is rotten to the core.

Yet there may be claimed for this rot a certain undeniable fecundity. Science reveals hidden mysteries, predicts successfully, and works technological wonders. If this is the way of rot, then rot is rather to be prized and praised than patronized.

Rot, actually, is not the best model here. A better model is human progress. A sense of comparative similarity, I remarked earlier, is one of man's animal endowments. Insofar as it fits in with regularities of nature, so as to afford us reasonable success in our primitive inductions and expectations, it is presumably an evolutionary product of natural selection. Secondly, as remarked, one's sense of similarity or one's system of kinds develops and changes and even turns multiple as one matures, making perhaps for increasingly dependable prediction. And at length standards of similarity set in which are geared to theoretical science. This development is a development away from the immediate, subjective, animal sense of similarity to the remoter objectivity of a similarity determined by scientific hypotheses and posits and constructs. Things are similar in the later or theoretical sense to the degree that they are interchangeable parts of the cosmic machine revealed by science.

This progress of similarity standards, in the course of each individual's

maturing years, is a sort of recapitulation in the individual of the race's progress from muddy savagery. But the similarity notion even in its theoretical phase is itself a muddy notion still. We have offered no definition of it in satisfactory scientific terms. We of course have a behavioral definition of what counts, for a given individual, as similar to what, or as more similar to what than to what; we have this for similarity old and new, human and animal. But it is no definition of what it means really for *a* to be more similar to *b* than to *c*; really, and quite apart from this or that psychological subject.

Did I already suggest a definition to this purpose, metaphorically, when I said that things are similar to the extent that they are interchangeable parts of the cosmic machine? More literally, could things be said to be similar in proportion to how much of scientific theory would remain true on interchanging those things as objects of reference in the theory? This only hints a direction; consider for instance the dimness of 'how much theory.' Anyway the direction itself is not a good one; for it would make similarity depend in the wrong way on theory. A man's judgments of similarity do and should depend on his theory, on his beliefs; but similarity itself, what the man's judgments purport to be judgments of, purports to be an objective relation in the world. It belongs in the subject matter not of our theory of theorizing about the world, but of our theory of the world itself. Such would be the acceptable and reputable sort of similarity concept, if it could be defined.

It does get defined in bits: bits suited to special branches of science. It is in this way, on many limited fronts, that man continues his rise from savagery, sloughing off the muddy old notion of kind or similarity piecemeal, a vestige here and a vestige there. Chemistry, the home science of water-solubility itself, is one branch that has reached this stage. Comparative similarity of the sort that matters for chemistry can be stated outright in chemical terms, that is, in terms of chemical composition. Molecules will be said to *match* if they contain atoms of the same elements in the same topological combinations. Then, in principle, we might get at the comparative similarity of objects *a* and *b* by considering how many pairs of matching molecules there are, one molecule from *a* and one from *b* each time, and how many unmatching pairs. The ratio gives even a theoretical measure of relative similarity, and thus abundantly explains what it is for *a* to be more similar to *b* than to *c*. Or we might prefer to complicate our definition by allowing also for degrees in the matching of molecules; molecules having almost equally many atoms, or having atoms whose atomic numbers or atomic weights are almost equal, could be reckoned as matching better than others. At any rate a lusty chemical similarity concept is assured.

From it, moreover, an equally acceptable concept of kinds is derivable, by the paradigm-and-foil definition noted earlier in this paper. For it is a question now only of distilling purely chemical kinds from purely chemical similarity; no admixture of other respects of similarity interferes. We thus exonerate water-solubility, which, the last time around, we had reduced no farther than to an unexplained notion of kind. Therewith also the associated subjunctive conditional, 'If this were in water, it would dissolve,' gets its bill of health.

The same scientific advances that have thus provided a solid underpinning for the definition of solubility in terms of kinds, have also, ironically enough, made that line of definition pointless by providing a full understanding of the mechanism of solution. One can redefine water-solubility by simply describing the structural conditions of that mechanism. This embarrassment of riches is, I suspect, a characteristic outcome. That is, once we can legitimize a disposition term by defining the relevant similarity standard, we are apt to know the mechanism of the disposition, and so by-pass the similarity. Not but that the similarity standard is worth clarifying too, for its own sake or for other purposes.

Philosophical or broadly scientific motives can impel us to seek still a basic and absolute concept of similarity, along with such fragmentary similarity concepts as suit special branches of science. This drive for a cosmic similarity concept is perhaps identifiable with the age-old drive to reduce things to their elements. It epitomizes the scientific spirit, though dating back to the Pre-Socratics: to Empedocles with his theory of four elements, and above all to Democritus with his atoms. The modern physics of elementary particles, or of hills in space-time, is a more notable effort in this direction.

This idea of rationalizing a single notion of relative similarity, throughout its cosmic sweep, has its metaphysical attractions. But there would still remain need also to rationalize the similarity notion more locally and superficially, so as to capture only such similarity as is relevant to some special science. Our chemistry example is already a case of this, since it stops short of full analysis into neutrons, electrons, and the other elementary particles.

A more striking example of superficiality, in this good sense, is afforded by taxonomy, say in zoology. Since learning about the evolution of species, we are in a position to define comparative similarity suitably for this science by consideration of family trees. For a theoretical measure of the degree of similarity of two individual animals we can devise some suitable function that depends on proximity and frequency of their common ancestors. Or a more significant concept of degree of similarity might be devised in terms of genes. When kind is construed in terms of any such similarity concept, 'fishes' in the corrected, whale-free sense of the word qualify as a kind while 'fishes' in the more inclusive sense do not.

Different similarity measures, or relative similarity notions, best suit different branches of science; for there are wasteful complications in

providing for finer gradations of relative similarity than matter for the phenomena with which the particular science is concerned. Perhaps the branches of science could be revealingly classified by looking to the relative similarity notion that is appropriate to each. Such a plan is reminiscent of Felix Klein's so-called *Erlangerprogramm* in geometry, which involved characterizing the various branches of geometry by what transformations were immaterial to each. But a branch of science would qualify for recognition and classification at all, under such a plan, only when it had matured to the point of clearing up its similarity standards. Such branches of science would qualify further as unified, or integrated into our inclusive systematization of nature, only insofar as their several similarity concepts were *compatible;* capable of meshing, that is, and differing only in the fineness of their discriminations.

Disposition terms and subjunctive conditionals in these areas, where suitable senses of similarity and kind are forthcoming, suddenly turn respectable; respectable and, in principle, superfluous. In other domains they remain disreputable and practically indispensable. They may be seen perhaps as unredeemed notes; the theory that would clear up the unanalyzed underlying similarity notion in such cases is still to come. An example is the disposition called intelligence—the ability, vaguely speaking, to learn quickly and to solve problems. Sometime, whether in terms of proteins, colloids, nerve nets, or overt behavior, the relevant branch of science may reach the stage where a similarity notion can be constructed capable of making even the notion of intelligence respectable—and superfluous.

In general we can take it as a very special mark of the maturity of a branch of science that it no longer needs an irreducible notion of similarity and kind. It is that final stage where the animal vestige is wholly absorbed into the theory. In this career of the similarity notion, starting in its innate phase, developing over the years in the light of accumulated experience, passing then from the intuitive phase into theoretical similarity, and finally disappearing altogether, we have a paradigm of the evolution of unreason into science.[15]

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NOTES

1. C. G. Hempel, *Aspects of Scientific Explanation and Other Essays,* (New York: Free Press, 1965), 15.
2. Nelson Goodman, *Fact, Fiction and Forecast* (London: University of London Press, 1955), 74.
3. *Ibid.*, 82ff.

4. *Ibid.*, 95ff.

5. I mean this only as a sufficient condition of lawlikeness. See Donald Davidson, 'Emeroses by Other Names,' *J. Philosophy 63* (1966), 778–780.

6. This relevance of kind is noted by Goodman, 1st ed., 119 ff.; 2nd ed., 121 ff.

7. Nelson Goodman, *The Structure of Appearance*, 2nd ed. (New York: Bobbs-Merrill, 1966), 163 ff.

8. Rudolf Carnap, *The Logical Structure of the World* (Berkeley, Calif.: University of California Press, 1967), 141–147. (German ed., 1928.)

9. See my *Word and Object* (Cambridge, Mass.: M.I.T. Press, 1960), 83 ff. for further discussion and references.

10. See *ibid.*, 90–95.

11. This was noted by S. Watanabe on the second page of his paper 'Une explication mathématique du classement d'objets,' in *Information and Prediction in Science*, ed. S. Dockx and P. Bernays (New York: Academic Press, 1965).

12. J. J. C. Smart, *Philosophy and Scientific Realism* (London: Routledge and Kegan-Paul, 1963), 68–72.

13. *Fact, Fiction and Forecast, op. cit.*, 95 ff.

14. R. Carnap, 'Testability and Meaning,' *Philosophy of Science 3* (1936), 419–471; 4 (1937), 1–40.

15. I am indebted to Burton Dreben and Nelson Goodman for helpful criticisms of earlier drafts.

Concerning a Fiction About How Facts Are Forecast*

Andrzej Zabludowski

Nelson Goodman's *theory of projection*† is an attempt to answer the following query: What distinguishes credible from incredible inductive generalizations?

The answer it provides is, roughly, this: (1) any supported and unviolated generalization can be given credence unless there is a rival supported and unviolated generalization that wins or at least withstands the competition; (2) among alternative supported and unviolated generalizations, we favor those whose predicates appear *better entrenched*. And a predicate is at a given time better entrenched than another if it has previously been longer and more often used in inductions; or more exactly—for it is not words as such that matter, but the classes they select—a predicate is better entrenched than another if the class it selects has previously been longer and more often mentioned in inductions than the class selected by the other.[1]

But one should not suppose that if, among alternative inductive generalizations, we favor those whose predicates prevail in entrenchment—if, to recall Goodman's favorite example, we credit the hypothesis 'All emeralds are green' rather than an alternative to it such as 'All emeralds are grue [i.e., either thus far observed and green or not as yet observed and blue]'—it is because those predicates become better entrenched which, by themselves, are better fitted for formulating plausible inductions. On the contrary, it is just by becoming better entrenched that a predicate becomes better fitted. A choice, initially made by chance, is subsequently perpetuated by habit (cf. *PP*, 357/8; *FFF*, 98).

I, for one, am not persuaded by Goodman's insight into the roots of our inductive choices. To say that it is just by habit that we project past regularities in greenness rather than those in grueness seems to me as

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