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Scientific Inference: The Myth and the Reality

Prologue: “Abduction”

Some years ago, after a period of transitions for both William Stephenson and myself which had left me uncertain of his whereabouts, I was pleased to recover a sense of his reality from the published appearance of his “Scientific Creed” (Stephenson, 1961). My pleasure turned to delight upon discovery that his “creed” centered upon a little-recognized but scientifically basic principle of human reason on which I had just completed what I still think is perhaps my finest paper to date (Rozeboom, 1961). I promptly sent Stephenson a copy of my manuscript with a note that read in part, “I suspect that you may now entertain doubts that your claim, ‘philosophers have been quite unable to do anything with Peirce’s ideas about abduction’, is still altogether true.” To this he replied, “You will agree that I had good reason to say that philosophers have done little to help us,” and went on to express a graciously warm appreciation for my efforts in this regard.

Having since made some effort to learn just what Peirce actually said about “abduction,” I prefer to retain a modicum of distance between this and what I find similar to it in my own analysis of scientific inference. For Peirce, “abduction” seems to have been a rather protean concept that most consistently emphasized the initial creation of hypotheses regardless of their epistemic character. (Thus, “Abduction must cover all the operations by which theories and conceptions are engendered” Peirce, 1934, p. 414.) Even so, a strong subsidiary theme in Peirce’s notion was, as Stephenson has nicely put it, that abduction is “inference, like induction, but concerned with *explanation*, whereas induction was descriptive—one proceeded from a sample to the whole in induction, but from the whole to an explanation or interpretation in abduction ... The concern is with *causes*, not with the discovery of regularities” (Stephenson, 1961, p. 11). It is this mode of reasoning-from observations to conceptions of their explanatory sources that can most usefully be emphasized by a modern revival of the term ”abduction,” for this is where the present gulf between scientific practice and metascientific theory gapes most abysmally. For Peirce himself occasionally to the contrary notwithstanding, there is a determinate *logic* to abduction in this sense, a logic whose primitive forms are just as intuitively compelling as are any commonsense patterns of deduction or statistical generalization, and whose advanced manifestations have become increasingly prominent in technical data analysis of the past few decades. Not merely have standard textbook accounts of science failed to acknowledge and appraise this logic, they have instead promulgated fantasies about the nature of scientific inference which, if taken seriously in research practice, would degrade the thrust of scientific progress into a random walk. In what follows, I shall try to make evident that the methodological Emperor paraded in the standard textbook fable is quite naked, but that unbeknown to his Court followers his working-class subjects have prepared for him a wardrobe of homespun which, although still clumsily tailored, is more than adequate to restore his comfort and dignity.

The Myth: Hypothetico-Deductivism

Let us suppose that in your professional capacity as a research scientist you have made the considerable effort needed to determine with high confidence and precision that a certain finite set s of entities has manifested such-and-such properties under thus-and-so circumstances. Call this observed data configuration $D(s)$. Now that you know that $D(s)$ is the case, what next? Clearly you will want to publish your findings, if only to convince your grant agency that your research merits continued support. But few journals accept raw data protocols, so to get any professional mileage out of your information $D(s)$ you must summarize and interpret it. For that matter, even if you could publish $D(s)$ undigested, a general (*contra* historical) science such as psychology cares about specific dated events only insofar as these reveal something about the principles that govern particular cases. One way or another, then, your professional responsibilities regarding data $D(s)$ are not properly discharged until you draw some conclusions from this evidence. It is also professionally obligatory that these conclusions be epistemically *justified* (warranted, rational), not just imaginative speculations about what is logically possible. Thus if from data $D(s)$ you infer that C may well be the case, you must attempt not only to communicate a thoughtfully appropriate *degree* of confidence in C—neither more nor less than is warranted by $D(s)$[1]—but also to insure that this degree of C-belief given $D(s)$ is rational according to standards of sound judgment shared with you by those persons with whom you wish to communicate. Your problem of what to make of $D(s)$ is thus: What are the conclusions you can justifiably infer from $D(s)$, and by what theory of inference can you argue that these are rational interpretations of your data?

[1] It is widely recognized that it is generally appropriate for scientific conclusions to be hedged about with cautions, qualifications, and tentativeness. But it is also important to appreciate that *under*confidence in one's less-than-certain conclusions can be just as epistemically irrational as an excess of assurance. For example, although the evidence is not conclusive that Thalidomide tends to cause birth defects, or cigarette smoking lung cancer, it would be idiocy for one's degree of uncertainty about these causal hypotheses to be a complete lack of conviction one way or the other.

Inferences from data in the behavioral sciences today basically fall into two methodological categories, (1) statistical generalizations, and (2) anything else. Very crudely, the former consists of noting that data configuration $D(s)$ entails that observed population s has a certain array $P_1, \ldots, P_n$ of distributional properties (e.g., that the data variables' means, variances, and intercorrelations have whatever values are computed for them in this sample) and from this inferring that properties $P_1, \ldots, P_n$ or reasonably close approximations thereto, are also possessed by the larger population from which s is a more or less random sample. As every successful graduate student well knows, there now exists a large, revered, and technically demanding literature on the theory of such generalizations. Although the foundations of this theory are much less secure than most of its users realize, and there still exist differences of educated opinion on how the precise credibilities of statistical conclusions should be assessed, operational disagreements nowadays about the statistical interpretation of sample data become asymptotically negligible with increasing sample size. All in all, modern statistical theory is one of mankind's truly remarkable intellectual achievements. Although some of its important practical facets still remain largely intuitive (e.g., what are the populations to which a sample's statistics can be generalized?), this is the one sector of scientific data interpretation that is now relatively unproblematic—which is why statistical reasoning is virtually the only kind of inference in which graduate science education today provides any formal training.

Unhappily, however, what scientists no less than common mortals crave to learn about the world is seldom just the statistical parameters to which sample frequencies converge. Drawing whatever conclusions we can about our data variables' distribution in the population sampled is but prologue to the deep problems of data interpretation. For suppose that we have observed so large a sample s of events from class C that no practical uncertainty remains in concluding that the distributional properties $P_1, \ldots, P_n$ found in s also hold for C as a whole—what then? One important further implication of course is that we can then expect properties $P_1, \ldots, P_n$ to be approximately true of any other random selection s' of events in C as well, where the larger is s' the better will be the degree of approximation. But is that all? Cannot we rationally infer anything from data configuration $D(s)$ more intellectually satisfying than that additional data obtained like s will be rather like s in other respects as well? Published methodological doctrine on this has largely polarized between two extremes. On one hand, the most vocal of empiricists usually insist that observational generalities are the only knowledge to which a genuine science can aspire—beyond that lie speculative amusements which may rejoice the creative inner man but lay no serious claim to rational conviction. So constrictive an outlook on the reach of human reason has never been popular even among professional experimentalists, however, so most scientists whose theoretical yearnings have not been totally extinguished by their graduate training gratefully subscribe to an alternative orthodoxy that has been the unquestioned dogma of virtually all modern philosophers of science, namely, that we come to understand the causes of observed events by constructing hypotheses about their underlying sources and then testing the observable consequences of these hypotheses. Accordingly, it will conform to prevailing custom, albeit to the intense disapproval of some empiricists, if your research report on data $D(s)$ concludes by discussing *why* the population to which you generalize your sample statistics might have these properties.

The controversy to which I have just referred is of course an old and familiar one, often waged under the battleflag "Science can only describe, not explain." Its two traditional factions have each fused strength with weakness, the weakness of one corresponding to the strength of the other. The heart of the empiricist argument is that insomuch as statistical induction is the only known inference form whose extrapolations from hard data can attain high degrees of plausibility, descriptive regularities are all a science can learn with any confidence about the world. Alternatively, apologists for the more philosophically sophisticated "theoretic" or "explanationist" outlook in effect contend that since the most prestigious sciences often do develop convincing if never completely certain explanations for observed regularities, and since hypothetico-deductive (H-D) reasoning is the only acknowledged way to support theoretical/explanatory conclusions,[2] H-D arguments must be a legitimate and indeed indispensable form of scientific inference. Each of these positions is nourished by an important epistemic reality—for the explanationist, that we do in fact somehow manage to work out plausible explanations for observed events; for the radical empircist, that no heretofore identified pattern of ampliative inference beyond statistical induction (the H-D schema especially not excluded) carries much conviction for tough-minded thinkers—but mistakenly sets itself in opposition to the other through a simplistic equating of scientific inferential practice with extant philosophic reconstructions thereof. I will show later that radical empiricism is wrong to think that we have no way to reach highly plausible conclusions about the underlying sources of observed events. But first I want to make overwhelmingly evident that the radical empiricist's scorn for H-D thinking is well merited—that the H-D model as conventionally propounded simply has *no relevance whatsoever* to the logic of rational inference.

What is this "hypothetico-deductive" orientation that has so dominated modern theories of scientific inference? Review of the methodological literature reveals a remarkable fact: Although the H-D *prescription*—i.e., what must be done to

[2]It might be wondered why analogical inference should not also be recognized here. Despite the fact that philosophers have at times construed analogical arguments to have some rational force, however, the use of analogy in *de facto* scientific practice has been almost entirely heuristic, namely, as a stimulus to conception of hypotheses. Confirmation of these analogy-inspired hypotheses is then sought along other lines.

get an H-D argument under way—has been abundantly publicized, it is virtually impossible to find any articulate discussion of what specific sorts of conclusions are supposedly the epistemic pay-off of this procedure. The prescription, of course, is that if H is some conjecture whose truth or falsity cannot be settled by direct observation, then we are to pass judgment on H by deducing its empirical implications and testing whether these do, in fact, turn out as predicted. That is, we are to derive an observable consequence C of H and assess H according to whether C is the case[3] But assess H how? The inference schemata that come obviously to mind are (1) and (2) of Chart 1,

Chart 1

(1)	(2)	(3)
H entails C	H entails C	H entails both C_1 and C_2
C proves false	C is verified	C_1 is verified
H is false	H is confirmed	C_2 is confirmed

(4)

If hypothesis H is logically equivalent to the conjunction of two or more logically independent subhypotheses H_1 and H_2 such that H_1 alone entails C_1 and H_2 alone entails C_2, then the bare fact that (verification of) C_1 confirms H as a whole gives us no reason to think that C_1 also confirms C2.

in which "confirmed" means an increase in credibility generally less than complete verification. H-D enthusiasts have in fact always been quick to laud schema (1), and although (2) is less often acknowledged publicly (probably because, unlike (1), it has no basis in deductive logic and hence lies beyond the technical skill of most writers to justify), schema (2) describes the standard H-D interpretation—covert when not explicit—of a theory's successful predictions. And to be sure, there would be little to protest here were the main flow of H-D reasoning to be channeled through (1) and (2) alone, for the validity of (1) is uncontroversial and

[3] In practice, of course, the situation tends to be a bit more complicated than this. For one, H's "observable" consequences are likely to be statistical generalities whose truth or falsity is never conclusively determined by finite sample data. Secondly, in order to deduce C from H we often need some auxiliary assumption K, so that the hypothesis tested by C is actually the conjunction of H and K. And thirdly, C is usually of the form "If A, then B" whereas the outcome of a test of this is most properly expressed as "A was brought about and B did (did not) occur" and is not entirely equivalent to the former in its bearing on H (cf. Rozeboom, 1968). These are merely complications in the circumstances under which H-D reasoning gets attempted, however; they do not affect the logical character of the latter or ameliorate its to-be-shown vacuity.

it is hard to conceive of a coherent theory of inference which countenances violations of (2). (In particular, (2) is a simple theorem in the conditional probability model of credibility relations.) Where the intuitive reasonableness of hypothetico-deductivism begins to curdle is in one's inclination to suppose further that this holistic confirmation or disconfirmation of H has epistemic relevance for other consequences of H as well.

It is easily seen and widely recognized that disproving a hypothesis H does not generally impugn everything which follows from H (since if H is conjunctively complex the falsity of any one of its premises suffices to make their conjunction false as a whole).[4] Yet H-D partisans have to a man overlooked that this limitation has a mirror image on the side of confirmation. It is strongly tempting to think that verifying a consequence of hypothesis H increases the plausibility not merely of H itself but of its additional untested consequences as well—i.e., to assume that schema (3) of Chart 1 is a general principle of ampliative inference that holds for any logically consistent hypothesis H and consequence C_2 not already certain prior to verification of C_1. For example, one might attempt to justify his conviction that the sun will rise tomorrow morning by arguing that the hypothesis "The sun rises every morning" has been overwhelmingly confirmed by the accuracy of its implications for all previously observed mornings, whereas nothing so persuades us to stake our real-life welfare on a theory's still-unverified consequences (e.g., for the design of nuclear reactors and governmental fiscal policies) as for the theory to have triumphed at previous predictions, especially implausible ones. I think it can safely be said that if hypothetico-deductivists did not find arguments of form (3) convincing they would be hard pressed to find any practical value in theory confirmation. Inference patterns (1) and (2) are what give the H-D model its aura of philosophical respectability, but its operational, gut-level force for persons who actually reason hypothetico-deductively is carried by pattern (3).

Yet with but a single exception that merely verbalizes the writer's naked intuition (namely, Hempel, 1945, on the "special consequence condition"; see also Hempel, 1968, p. 275), I am aware of no argument in the vast literature on scientific inference that explicitly makes a case for some version of (3). The simple brute fact of the matter is that (3) is *not* a defensible inference pattern.[5] A complex

[4] This is the basis of the "Duhemian argument" (much discussed in the recent philosophy-of-science literature) that if, as is almost always the case in practice, we must make some auxiliary assumption K in order to derive a testable consequence C from theory H, then disproof of C refutes not H but only its conjunction with K.

[5] Except as a weak statistical enthymeme whose suppressed premises are (a) that hypotheses which arise naturally in scientific practice tend to have a special character for which (3) holds, and (b) that H is a hypothesis which has arisen naturally in scientific practice. Although the basic objection to (3) has been long known (cf. Hempel, 1945, n.39), it is only within the last few years that philosophers of science have begun to think seriously about its confirmation-theoretical significance (cf. Hesse, 1970).

hypothesis H can easily couple premises entailing C with additional assumptions to which C is irrelevant or even disconfirmatory. It requires no particular model of credibility relations to demonstrate this, for given any two logically compatible propositions C_1 and C_2, the conjunction of C_1 and C_2 is a logically consistent hypothesis that entails both C_1 and C_2; hence, if (3) were generically sound under the conditions stipulated, any datum would confirm every still-uncertain proposition with which it is logically compatible. It is important to appreciate that this general failure of (3) in no way undercuts the acceptability of confirmation principle (2); rather, it points up the latter's epistemic triviality. For no matter how arbitrarily or implausibly H may conjoin C with other speculations, part of H's initial uncertainty resides in the uncertainty of C; hence when C becomes verified, this much of the total doubt about H is dispelled even when C warrants no increase of confidence in whatever H proposes over and above C.

It would be of the utmost foolishness to shrug aside the point just raised as pedantic, artificial, degenerate, unlikely, or otherwise unworthy of serious methodological concern. What it shows is that hypothetico-deductive exploitation of fanciful theories can easily convert hard data into alleged support for any conjecture the data do not logically contradict. It does not help the H-D traditionalist in the slightest to counter that even if inference schema (3) may not be acceptable for all arbitrarily constructed hypotheses, it surely holds for most theories that arise in the natural course of scientific inquiry. Empirically, this is just not so—working theories in scientific practice often (I would guess virtually always) contain many assumptions which careful analysis can show to be quite gratuitous relative to those portions of the theory that extant data genuinely support.[6] This is especially common in turbulent areas where hypotheses spew forth in sufficient profusion to anticipate nearly all the possible outcomes of portending research, but it results whenever undisciplined theorizing grafts imaginative speculations onto plausible extrapolation from known phenomena. *It is for precisely this reason that tough-minded empiricists have always been so obdurately hostile to grandiose theories and the hypothetico-deductive view of scientific inference.* That verifying some of a particular theory's consequences often strengthens our confidence in parts of its remainder is an ineluctable fact of human reason. But it still remains to identify the logic by which this properly occurs, to make explicit what sorts of internal structure confer this epistemically vital inferential cohesiveness upon a hypothesis—and not only is the H-D model totally uninformative about this, it does not even recognize that any such account is needed.

To make clear that a fundamental logical problem is at issue here, one that can no more be handled within the conceptual framework of traditional hypothetico-deductivism than a meat axe will do for heart surgery, consider the most obvious

[6]For elaborations on this theme, see Rozeboom, 1970.

restriction on (3) by which one might think to resuscitate its intuitive force. The big trouble we have found with (3) is that verifying one consequence, C_1, of a hypothesis H cannot be trusted to confirm another, C_2, because H may arbitrarily conjoin C_2 with whatever in H is genuinely relevant to C_1. Since this objection would be irrelevant were we sure that C_2 follows from the same premises in H needed to deduce C_1, all might seem well again with (3) if cases falling under the disclaimer (4) in Chart 1 were excluded from its scope. (Note that (4) does not deny that C_1 may confirm C_2 in particular cases; it merely insists that *if* C_1 confirms C_2, it does so on grounds other than H's joint entailment of C_1 and C_2.) But if principle (4) is accepted, it prevents us from *ever* construing verification of one consequence of a theory as good reason, on traditional hypothetico-deductive grounds, for increased confidence in another. For given any two different consequences C_1 and C_2 of any hypothesis H, we can always rewrite H to have the form presupposed by (4). Specifically, if H entails both C_1 and C_2 and 'R' abbreviates the proposition 'Either H or not-C_1 or not-C_2', it is easily seen that H is logically equivalent to the conjunction C_1 and C_2 and R, in which R is the residual of H over and above C_1 and C_2, and all three premises, C_1, C_2, and R, in this factoring of H are logically independent of one another.[7] This construction is, of course, an unnatural one; but it shows that no matter how tightly integrated a theory H may appear in its initial wording, it can always be reaxiomatized so that any finite set $C_1, \ldots, C_n$ of its logically distinct consequences are formally decoupled therein, i.e., so that the subset of H's independent premises needed to derive C_i is entirely disjoint from the subset needed to derive $C_j, (i \neq j)$. Hence, if we characterize the logical relations among a hypothesis and its consequences only in terms of which subsets of premises entail which consequences, we will never find a basis for thinking that a theory's various consequences are anything more than arbitrary conjuncts therein with little or no confirmational relevance to one another.

Lest my protests against hypothetico-deductivism here seem tainted by an excess of polemical zeal, I want to deny any intent to disparage the soft, liberal interpretation of "hypothetico-deductive method" that advocates under this label no more than that we should try to conceive of explanations for phenomena that interest us and then assess the credibilities of these theories *through whatever considerations are epistemically relevant* from tests of their empirical consequences. My attack is directed specifically at the simplistic, unthinking presupposition of H-D orthodoxy that the epistemically relevant relation between hypotheses and data is mere logical entailment. This traditional view of the *way* in which observations confirm the theories that predict them, despite its total inability to withstand critical examination, has thoroughly occluded our access to the problem of *significant* theory confirmation, namely, to decipher what a theory must be like in order to

[7]Except for the degenerate cases in which C_1 entails C_2, C_2 entails C_1, or C_1 and C_2 jointly entail H. In the last case, R is tautological and can be deleted.

transmit credibility from one of its consequences to another.

To be sure, hypothetico-deductive arguments by no means cover all features of scientific credibility which philosophers of science have expressly sought to explicate. For example, a theory's "simplicity" has long been acknowledged as an important, if still poorly understood, determinant of its intuitive merit, and recent years have seen increasingly sophisticated attempts to quantify confirmation theory within the framework of the probability calculus. But neither by deliberate intent nor by inadvertent implication have these probes illuminated the specific logical properties which give inferential muscle to theories that arise in serious science—nor indeed, considering the distance of most abstract philosophy of science from the operational realities of scientific data processing, could they reasonably be expected to do so. Just the same, a determinate logic of trans-statistical scientific inference does in fact exist, and to a large degree it can be formalized.

The Reality: Explanatory Induction

What comes next here is importantly incomplete in at least two ways. I have been ruthlessly destructive in my criticism of the H-D inference model for the same reason that a gardener hoes down the weed stand on soil he intends to cultivate: Within this clearance we can now begin domestication and controlled evolution of those hitherto unattended species of inference which, unbeknown to epistemic horticulturists, are the real roots of scientific knowledge. It is not to be expected, however, that the first exploratory plantings of this stock will reveal all its natural varieties or optimal growth conditions. That is (to abandon a rapidly deteriorating metaphor), not only is there no reason to think that the patterns of explanatory induction described below are exhaustive, neither can I yet formalize all the conditions on which the intuitive force of these arguments is dependent. The latter deficiency is the same sort of incompleteness which still blemishes our formalizations of statistical induction, namely, that artificial albeit true premises can be contrived from which the induction form derives grotesque conclusions.[8] I shall not speak further of this complication on this occasion, but it should be acknowledged at the outset lest astute readers who note the possibility of such contrivances take them to discredit the generic force of the inference forms they counterinstance. We do not, however, consider statistical induction to be

[8]Thus to embarrass the abstract generality of statistical induction, let S be a random sample from class C, and for any arbitrary property P, P^* is the disjunctive property of belonging either to S or to the class of P-things. Then P^* has relative frequency of 1 in S (i.e., all Ss are P^*s), and generalization of this sample frequency to C as a whole yields the conclusion that all members of C not in S have property P. The methodological distress occasioned by such constructions has been most actively exacerbated of late by philosophers of science (cf. Goodman's celebrated green/grue paradox), but has not been unknown in serious science as well, notably, in the indeterminacy of trend extrapolations.

generically irrational just because its plausibility in particular cases is contingent upon these being "natural" in a sense still much in need of clarification, and neither is it any epistemic stigma on the forms of explanatory induction that these too require an intuitive "naturalness" restriction (cf. Rozeboom, 1961, p. 366f *57f*). It merely shows that the whole subject of ampliative inference still contains major mysteries, cutting across specific inference patterns, which insightful analysis has scarcely begun to breach.

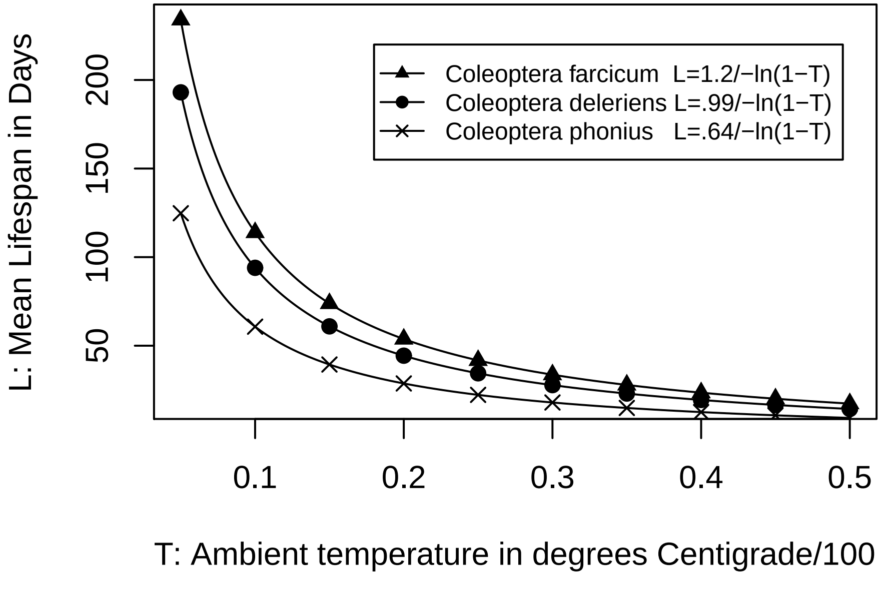
By *explanatory induction* I mean inference patterns that algorithmically transform datum premises of an appropriate kind into conclusions that say *why* the data are this way even when the inference's intuitive strength may well approach total conviction. The qualifier "algorithmically" is important here, for the inference forms to which I refer are vehicles of discovery as well as of justification—not merely do their premises confer evidential support upon their conclusions, if necessary they also bring the latter to mind in the first place. Over and above statistical induction (which is not commonly thought to yield explanatory conclusions although a case can be made to the contrary), there exists at least one broad family of such forms that contribute massively to data processing in both technical science and practical everyday life. I have previously called these "ontological" inductions (Rozeboom, 1961, 1966b) because, by generating conceptions of entities to which we have epistemic access only through their effects on our data variables, they yield awareness of the world's nonperceptual furnishings.

The formal nature of explanatory induction lies in its reworking of distinctive features of restricted data regularities into empirical indices of the underlying determinants nomically responsible for the particular character of these local regularities. The logical details of such transformations seem rather sensitive to the specifics of their data base, but at least two major types can be distinguished, corresponding to whether the data patterns at issue are between-variable or within-variable consistencies. I shall call the inferential algorithms at work in these two cases *parameter conversion* and *factorial decomposition*, respectively.

The data patterns upon which parameter conversion operates are those dealt with by traditional multivariate analysis, namely, where we have a set of two or more data variables $\mathbf{X}_1, \ldots, \mathbf{X}_n$ which have a particular constellation of values for each individual in a population P. (See Rozeboom, 1961, p. 340ff. *30ff.* and Rozeboom, 1966b, p. 176ff. *132ff.*, for details on the logical nature of "variables" in the scientific sense of this term.) Some of these value constellations (i.e., conjunction of properties) generally occur in P more frequently than do others, and we describe the pattern of these co-occurrences by various parameters (e.g., means, variances, correlation coefficients) of the joint distribution of $\mathbf{X}_1, \ldots, \mathbf{X}_n$ in P. Now, when a given distributional parameter takes a particular value for a given population P, this is logically a property of P as a whole, not of the individual members of P. (E.g., a high correlation between Educational Level and Annual Income may well hold in the population of all U.S. taxpayers, but cannot be meaningfully ascribed to individual taxpayer John Smyth.) Yet when we observe that different populations P_i $(i = 1, 2, \ldots)$ of a given natural kind are characterized by different values of the same distributional parameter for the same variables, we often find ourselves treating the value observed for this parameter in each P_i as an estimate of where *each member* of P_i individually stands on an underlying determinant of that individual's position in data space (see Rozeboom, 1961, p.362ff. *53ff.*). In particular, when the parameter expresses a statistical dependency, its conversion into a theoretical source of the observed contingency is often so unhesitantly automatic that we are unaware of having made any inference at all. (E.g., most of the "operational definitions" positivistic methodologists have extolled as the ultimate in observational security turn out under careful inspection to be theoretical concepts introduced in exactly this way.)

Suppose, for example, that research on invertebrate physiology has shown average lifespan in beetles to be a decreasing function of environmental temperature, but that some species are more sensitive to this effect than are others. Specifically, experiments in which beetles of various species are maintained in thermally stabilized environments and their observed life-spans plotted for each species against imposed temperature reveal (let us say) a family of curves such as illustrated in Figure 1, each being of the form $\mathbf{L} = c[-\ln(1-\mathbf{T})]^{-1}$ in which $\mathbf{L}$ is mean lifespan

Figure 1



Regressions of lifespan upon ambient temperature for selected species of *beetles maintained in thermally stabilized environments (imaginary data)*

in days and $\mathbf{T}$ is centigrade temperature divided by 100, but with considerable interspecies variation in the numerical value of parameter c. Now clearly the value of this parameter—call it the "thermal sensitivity coefficient"—is a biologically significant property of beetles, both as one of the differentia among species and as a factor in how long a particular beetle will live in an environment of given temperature. Yet the character of a species merely reflects the traits common to its individual members, whereas the determinants of an individual's biological reaction to its environment can scarcely be anything but features of that individual's own constitution. To treat the thermal sensitivity coefficient in the only way that makes good scientific sense, we must thus construe this observed class-property as the trace of an underlying attribute, possessed by each member of the species, which is responsible for an individual beetle's susceptibility to heat.[9] We are not logically *compelled* to make this inference, for it is an inductive leap. Even so, it is a move we habitually and unhesitantly do make. If we did not, there would be hard empirical data we would be at a loss to understand.

The present example of parameter conversion, although make-believe, is completely typical in all significant respects (except perhaps empirical tidiness) of research practice in areas whose objects cluster into relatively homogeneous natural classes. But even more common, especially in the behavioral sciences, are cases wherein a series of observations on the same individual through time reveals some idiographically consistent response to environmental impingements of a certain kind. For example, a distinctive relation between imposed force and resultant deformation can be determined for a chunk of unknown substance; an animal can be repeatedly exposed to a stimulus S to reveal how strongly the presence/absence of S is correlated over this period in that animal's history with emission/nonemission of a certain response R; or we can note for a new acquaintance how strongly and consistently he shows rage behavior in various vexing circumstances. In all such cases, what we observe most directly (with quantitative precision in technical science; with impressionistic vagueness in ordinary life) is

[9] I have deliberately chosen this example to be perspicuous on two crucial methodological points. In the first place, it is clear that the *function* relating Lifespan to Temperature, which is the empirical property expressed by the thermal sensitivity coefficient, cannot be effectively abstracted from Life-span and Temperature observations on a single individual, for the latter has only one lifespan and hence establishes but one data point through which pass an infinitude of potential Lifespan/Temperature curves. Secondly, although the value of the thermal sensitivity coefficient for a given species can be estimated from data on a single member of that species once the function-form common to all species is known, namely, by the computation $\hat{c} = -\mathbf{L} \cdot \ln(1 - \mathbf{T})$ in which $\mathbf{L}$ and $\mathbf{T}$ are Lifespan and Temperature for a single specimen, observation variable $-\mathbf{L} \cdot \ln(1 - \mathbf{T})$ cannot be *identified* with the underlying thermal-sensitivity factor even though by presumption the two are highly correlated, insomuch as a property that is analytically abstracted (in part) from Lifespan cannot be held *causally* responsible (in part) for it.

some parameter of input/output covariation in a class of time-slices of the object's life-history; yet we *interpret* this parameter as assessment of a relatively enduring property—compressibility, *S-R* association strength, and irascibility, respectively, in the examples just given—possessed by this object at each moment throughout this interval and by virtue of which it is disposed to react in the particular way it does to this input variable. Thus as we normally understand it, John Smyth's irascibility is not a property of the class of his moments in time, but a trait he has right now, as well as five minutes ago, and yesterday at 10:28 a.m., etc., independent of his environmental circumstances at any given moment but which, if these are vexing, causes him to react more angrily than would most persons in that situation. Theoretical properties of the sort commonly known as "dispositions," "abilities," "capacities," or (in former days) "powers" all owe our conceptions of them to parameter conversion, and the frequent failure of scientists and philosophers to recognize that these *are*, in fact, hypothesized entities which explain rather than analytically abstract from the data to which they are tied only emphasizes the impelling immediacy of these inductions.

Explanatory inductions of a second main type, factorial decomposition, arise from a structure often detectable in data whose formal character may be described as "polyadic" in contrast to the monadic data of classical multivariate analysis. A "monadic" variable over a population P can be formalized as a function which maps each of its arguments in P into a "value" (typically a number) corresponding to which property in a set of logical alternatives is true of that individual. (E.g., the value of the Weight-in-grams variable for any given physical object is the number of grams it weighs.) Analysis of monadic data then searches for regularities in the joint distribution of several such variables over a common population, most notably for the manner in which an individual's value on one (or more) of these variables makes a difference for his value on another. But monadic variables are logically very much a special case—more generally, an n-adic variable over population P maps each ordered n-tuple of arguments in P into a value demarking which relation out of a set of n-adic alternatives holds for that n-tuple. (E.g., Degree-of-admiration-for is a dyadic variable whose value for a pair of persons expresses how much the first person admires the second.) An n-adic variable for which $n \geq 2$ is "polyadic," and for polyadic data there exist patterning possibilities quite unlike those comprehended by traditional monadic analysis.[10] Although such data have undoubtedly been parsed intuitively throughout mankind's sentient history,

[10] At least on the face of it. Actually, a single n-adic variable can be treated as a set of m^{n-1} monadic variables, where m is the number of individuals in its argument domain, by construing all but one of its n arguments as parameters. The joint distribution of these derived variables can then be analyzed by monadic methods; and in fact, although this approach to polyadic data has not yet to my knowledge been explored, I suspect there may well prove to be much methodological virtue in doing so, especially for freeing polyadic data analysis from its present excessive dependence upon *a priori* model selection.

only within the past two decades has an explicit technical methodology for this begun to appear (although one special polyadic treatment of monadic data, linear factor analysis, has a longer history of its own), most of it under the rather misleading label of "measurement models" and still with little recognition even by its own pioneers much less by the remainder of the scientific community of just how profoundly this development breaks new ground in the theory of data analysis.

Factorial decomposition of an n-adic variable $\mathbf{V}$ consists in discovery of a monadic mapping ϕ of the objects related by $\mathbf{V}$ into some range of (as a rule) numbers or complexes of numbers, together with a function ψ from n-tuples of ϕ-values into values of $\mathbf{V}$, such that if $\mathbf{V}(x_1, \ldots, x_n)$ is the empirical relation of kind $\mathbf{V}$ holding for any individuals $x_1, \ldots, x_n$ in $\mathbf{V}$'s argument domain and ϕx_i is the number (or whatever) into which factorization function maps x_i, then $\mathbf{V}(x_l, \ldots, x_n) = \psi(\phi x_1, \ldots, \phi x_n)$. (For greater detail, see Rozeboom, 1966b, p. 201ff. *152ff.*.) For example, if $\mathbf{P}(t_i, t_j)$ is the observed proportion of times that baseball team t_i beats team t_j this year, it may be possible to assign each team t_i in the league a "skill" rating ϕt_i such that the comparative skills of any two teams determine the probability of one beating the other, say by the law $\mathbf{P}(t_i, t_j) = \phi t_i \div (\phi t_i + \phi t_j)$.[11] Or if $\mathbf{F}(x_i, x_j)$ is the coefficient of attraction empirically determined by torsion balance between any two electrically uncharged chunks of matter x_i and x_2, each chunk x can be assigned a "mass" μx_i such that $\mathbf{F}(x_i x_j) = \mu x_i \times \mu x_j$. Or from beam-balance data telling which aggregates of objects outweigh which other aggregates, we can diagnose for each object a "weight" such that whether one object-aggregate outweighs another is determined by which aggregate has the larger sum of weights. Roughly speaking, the sort of patterning manifest in such data is that given information about how an individual x_i is $\mathbf{V}$-related to a sufficient number of other individuals in the variable's argument domain, we can predict x_i's $\mathbf{V}$-relation to the rest, and factoring $\mathbf{V}$ as the composition of functions ψ and ϕ *could* be construed merely as a descriptive elegance that makes this pattern perspicuous. But in fact we never stop at that if the decomposition is nontrivial.[12] Instead, we find ourselves construing the values of ϕ for objects $x_1, \ldots, x_n$ to represent underlying nonrelational properties of these objects that are *responsible* for their observed $\mathbf{V}$-relations according to a nomic

[11] This is the Bradley-Terry-Luce model (Luce, 1959) for decomposition of probabilistic dominance data. In practice, of course, this and other factorial decomposition models reproduce the empirical data only to some degree of approximation, a discrepancy the model usually attempts to accommodate through such stochastic addenda as the difference (which for simplicity I have not acknowledged in the present example) between probabilities and sample-frequency approximations thereto.

[12] Trivial decompositions reflecting nothing that counts intuitively as data patterning are always possible for polyadic data (see Rozeboom, 1966b, p. 210f. *160f.*). The nature of the trivial/significant dimension here still awaits clarification, but it seems to be intimately connected with how rapidly an accumulation of relational data for a particular object x_i converges upon x_i's value of the factorization function.

principle expressed by ψ. Differences in skill are *why* one baseball team tends to beat another, and the coefficient of attraction has whatever value it does for two objects *because* their masses are whatever they are.

In these sketches of parameter conversion and factorial decomposition, I have stressed the first birthdamp emergence of the explanatory concepts these foal because that has been exactly the point of this essay—to show that some data regularities wear their explanations on the sleeve of their descriptions, manifest to even the most unimaginative onlooker (once the data pattern itself has been perceived) without interpretive intent or controlled surge of intellect.[13] But that is just the opening event in the real-life epic of such concepts; for once born they are swept into a turbulence of inter-phenomenon comparisons and other evolutionary research pressures that either extinguish the nascent concept's explanatory promise or tie it to so many strands of the discipline's "nomological network" (cf. Cronbach & Meehl, 1955) that the particular phenomenon which introduced it is no longer essential to its meaning and indeed may no longer be explained by it in quite the same way as before. For, importantly unlike the theoretical entities envisioned in speculative hypotheses, the source variables identified by parameter conversion and factorial decomposition come tagged from the outset with distinctive operational indices whose empirical behavior in higher-order[14] data regularities mirrors in depth and detail the underlying system of explanatory mechanisms. For example, the coefficient of attraction between two physical objects (see above) is actually a theoretical relation found in a series of observations on the pair, whereas subsequent factorial decomposition of this low level source relation into a product of masses explains it in turn as the result of a deeper monadic source. Again, our hypothetical thermal sensitivity coefficient might be just one of many intraspecies test-condition/test-outcome parameters in beetles (e.g., in the regressions of various adult features upon the concentration of certain chemicals in larval diet) which, when intercorrelated across species and factor-analyzed, show thermal sensitivity to be due, say, to two factors that are finally traced to a particular locus in the species' chromosomal map and the moisture level of the species' preferred egg repositories, respectively. In short, explanatory induction is a recursive process that can build upon its own discoveries at any explanatory level to disclose what,

[13]Note the parenthetical qualification here, for it absolves me of the charge that my account of explanatory induction leaves no room for the intellectual creativity that major theoretical innovations clearly seem to require. I would argue that cognitive creativity in science lies not primarily in soaring embroideries of speculative fancy, but in conception and detection of inductively significant data patterns. Discerning interpretable features in a raw data array is no mean intellectual/artistic feat, especially when the to-be-discovered pattern is of a kind not previously recognized by that discipline and the data at hand give only a fragmentary approximation to the idealized gestalt.

[14]"Higher-order" in the sense of a hierarchy of logical types (see Rozeboom, 1961, p. 357ff.) wherein the parameters of relatedness within classes define the variables related within a class of classes.

in turn, accounts for these.

Stephenson's own work on Q methodology (Stephenson, 1953) nicely illustrates many of the points I have tried to make here. A single Q-sort observation consists of a graded response made by (1) a particular person at (2) a given time to (3) a specific stimulus item under (4) distinctive interpretive instructions. (E.g., the subject may indicate how strongly he thinks the word "lazy" characterizes his father's opinion of him.) At the lowest level of explanatory induction, each such response is taken to indicate the subject's present underlying propensity to react a particular way to that item under those instructions, our belief in such tendencies (not just in Q methodology but throughout all psychometric research) being the inductive result of parameter conversion from consistencies in how a subject's response covaries with the stimulus variable defining the test.[15] Next, the within-subject correlation across stimulus items for responses to the same item under two different instructional sets is a second-level empirical parameter whose explanatory conversion inductively demarks the similarity in how this person interprets those instructions. (E.g., when the subject is variously instructed to assess how he thinks a specified other person would rate him on an array of evaluative adjectives, such between-instructions correlations reveal how similar the subject feels his mother's view of him is to his father's, how closely it resembles his own self-concept, etc.) Finally, factor analyzing a battery of the subject's between-instructions correlations can indicate what, for him, are the still-deeper determinants of between-instructions similarity.

What is especially instructive to note from this last example is the facility with which explanatory induction can disclose layer after layer of source variables even in data obtained from a single person. Because humanistically oriented personality theorists have been wont to voice their enthusiasm for such idiographic material in a garble of holistic mysticism ("idiography" has only too often deserved translation as "the writings of idiots"), it is perhaps worth a reprise to make clear that although every individual, human or otherwise, is indeed a law—in fact a hierarchy of laws—unto himself, this is in no way abhorrent to a "nomothetic" science which seeks lawfulness across individuals; rather, it is chief counsel to the latter's theoretical development. For as already noted, the regularities that govern a multiplex

[15]On first thought it may seem strange to construe a single test score as a relational parameter. Yet it is only because we think—with good reason—that the test situation makes a difference for the subject's response that we conceive of his performance in that situation as a score on that particular test. In testing practice we usually take only a single point on the test-condition/test-outcome regression, estimated by only a single observation, as the subject's empirically assessed parameter for this relationship, but were it not for practical complications we would learn more by extracting test parameters from multiple observations on the subject under systematic variation in the test conditions (e.g., time limits, precise wording of instructions, etc.). For further discussion of the concept of "test" and the dispositional interpretation of test scores, see Rozeboom, 1966a, Ch. 8.

of observations on a single individual i consist of (1) a *form* of relatedness shared by i for this kind of data with others like him in specifiable respects (namely, in whatever background conditions we have elected to hold constant in our study of these relationships), and (2) a distinctive configuration of parametric specifics therein which make is detailed pattern of idiographic lawfulness generally different from that of anyone else. But between-subject differences in such personal parameters simply define additional data variables of a higher logical type (which is true even if we force ourselves to abstain from their natural interpretation via parameter conversion) and so far as we know these always participate in less local regularities across individuals, i.e., laws that hold "normatively" for all individuals satisfying the relevant boundary conditions. For example, one of the oldest normative principles in that hardest-nosed sector of all psychonomic science, learning theory, a principle believed to be true for all organisms of sufficient biological complexity under suitable conditions of learning, is that an organisms's strength of a given S-R association (or Habit, or Discriminated Operant, or Valenced Expectancy, or etc., depending on one's deeper behavior-theoretic conjectures about the response propensity involved) is an increasing function of the reinforcement value for that organism of stimuli which have previously followed his emission of R in the presence of S. Yet association strengths and reinforcement values are theoretical attributes inferred for a given organism by explanatory induction from his local input/output patterning. Development of this example in honest detail (which is not practical here but may be urged upon the reader as an edifying exercise) would reveal a startling degree of logical complexity (cf. Rozeboom, 1961, p. 374 *66*) together with many more personal parameters than just the two explicitly cited here. But I trust that by now the point is clear enough: Although the "idiographic/normative" distinction is worth retention for methodological classification of data structures, it has nothing to do with the essence of individuality or the scope of uniformity in nature. Regardless of how normatively impeccable may or may not be the system of source regularities from whose surface eddies we skim the data of empirical science, idiographic parameters are the operational peepholes through which we shape our vision of the machinery below.

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