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Ontological Induction and the Logical Typology of Scientific Variables

Abstract

It is widely agreed among philosophers of science today that no formal pattern can possibly be found in the origins of scientific theory. There is no such thing as a "logic of discovery," insists this view—a scientific hypothesis is susceptible to methodological critique only in its relation to empirical consequences derived *after* the hypothesis itself has emerged through a spontaneous creative inspiration. Yet confronted with the tautly directed thrust of theory-building as actually practiced at the cutting edge of scientific research, this romantic denial of method in the genesis of ideas takes on the appearance of myth.

It is the contention of this article that as empirical data ramify in logical complexity, they deposit a hard sediment of theory according to a standard inductive pattern so primitively compelling that it must be recognized as one of the primary forms of inferential thought. This process, here called "ontological induction," is a distillation out of unwieldly observed regularities of more conceptually tractable states hypothesized to underlie them, and is the wellspring of our beliefs in theoretical entities. Previous failure to recognize this pattern of induction has undoubtedly been in substantial measure a result of inadequate attention to the structural details of scientific propositions; for in order to exhibit the nature of ontological induction clearly, it is first necessary to make extended forays through sparsely explored methodological terrain—notably, the nature of scientific "variables," the logical form of "laws," and the type hierarchy of scientific concepts.

1. Introduction

Despite the interest displayed by many modern philosophers in the natural sciences, surprisingly little serious attention has been given to the formal details of scientific propositions and cognitive strategies as they arise in actual practice. On the whole, philosophy-of-science writings with enough conceptual precision to be genuinely illuminating have tended to focus on specific problematic concepts and the epistemic status of theories, analyzed in terms of simplified reconstructions which, however adequate for their purpose, are considerably removed from the living reality of scientific procedure. In particular, scientific operations at the data-gathering and data-collating level are usually disposed of in a few broad generalities with the implication that the formal details are technical issues strictly indigenous to the particular scientific discipline and best tended to therein.

Now it may well be true that the primary responsibility for a methodology lies with its users. The fact remains, however, that the empirical sciences have evolved a formal methodology which is considerably more complex in logical structure than has generally been recognized, yet which, apart from some statistical machinery and aspects of experimental design, remains largely inarticulate. This failure of research scientists to show awareness of the details of their own conceptual methods is not overly surprising, perhaps, in view of the logical sophistication demanded, but this does not ameliorate the unfortunate consequence that many supposedly empirical problems and uncertainties of direction at the working front of science are actually in large measure due to confusions and inadvertent biases introjected by methodological naivete.[1] More than one hard-headed (and quite possibly anti-methodological) experimentalist who prides himself on his ability to stick by the facts and forego the theory has arrived at wildly unwarranted, assumption-packed conclusions which seemed to him in all innocence to be hardly more than summaries of his data. I submit, in other words, that much scientific methodology, particularly that of propositional forms and inferential techniques at the research level, still remains largely intuitive with its users and only dimly appreciated by philosophers. Perhaps no better illustration of this can be found than in the obscurity which currently surrounds the origins of theoretical constructs. It is by now generally recognized that a science which attains any degree of empirical success inevitably develops systematizing theories which make use of concepts that resist explicit definition within the data language of the science. But while the meanings and ontological significance of such concepts have been subjected to intense philosophical scrutiny within recent years (e.g., Carnap, 1956; Feigl, 1950; Hempel, 1958; Rozeboom, 1962), little or nothing has been said about how they arise in the first place. The traditional account of the "Hypothetico-deductive" method of doing science portrays a theory as a free, imaginative creation whose chief virtue lies in its bold entailment of previously unsuspected empirical phenomena and whose justification lies in the subsequent testing of these consequences. But this is clearly at odds with the actual development of theory in science. For one, it is difficult (I do not say impossible) to reconcile the Hypothetico-deductive method, so formulated, with the Principle of Parsimony, a canon of good scientizing for centuries, which discourages fanciful speculation by insisting on the simplest explanation for the available data. The fact that in the actual professional practice of reputable scientists, some theoretical proposals—those which go just a *little* bit beyond the data in suitable ways—receive serious consideration while others (e.g., Dianetics) are ignored as irresponsible fantasies, confutes a Popperian *mystique* of scientific theorizing. Again, the traditional account of the Hypothetico-deductive method conceives a theory as an organic whole, crystallizing outward from a germinal core

[1] For two quite different examples of how linguistic subtleties have prejudiced important empirical issues in behavioral psychology, see Rozeboom, 1958b and 1960 or 1961.

of its most empirically remote theoretical constructs until it spans the gulf between numerous previously disconnected items of experience, and which also, therefore, would be expected to vanish more or less in its entirety when disconfirmed. The fact of the matter is, however, that the theoretical structure of a science grows for the most part by small, piecemeal accretions along the edge of what has already been firmly established, like ice freezing on a pond, in which local segments can be modified or scrapped with little threat to the remaining growth, and where the more spectacular unifications which may appear in the mature science flesh out and tie together theoretical notions which have already taken form in lower levels of theory. Then too, there is the remarkable formal similarity so frequently found among rival theories erected over the same empirical foundation. The conclusion is unmistakable that far from being spontaneous, untrammeled inspirations, the workaday theories around which the no-nonsense research scientist organizes his professional activities are actually pre-formed to a high degree by the corpus of data already accumulated.

In short, just as finite observations automatically urge statistical generalizations (e.g., concluding from the fact that 98% of observed As have been Bs, that probably about 98% of *all* As are Bs), so does it seem possible that empirical phenomena cast a penumbra of theoretical inferences according to a standard inductive pattern which demands little or no creative ingenuity on the part of the individual scientist. In particular, since theoretical concepts which purportedly refer to unobserved entities rear their (as viewed in some quarters) ugly heads in these circumstances, it would appear that the basic armament of human reason includes not only induction of the statistical kind, but a process of *ontological induction* as well. The logical form of the latter, however, still remains to be spelled out. True, its rough outline has already been shadowed in the philosophy-of-science literature. Feigl, for example, has lectured on theoretical concepts as "triangulated in logical space," and Bridgman has suggested (Bridgman, 1927, p. 59) that physical reality is imparted to a construct by the existence of alternative "defining operations." But such statements serve only as suggestions which leave still obscure the actual formal machinery of the inference. It is the purpose of this article to begin identification of this machinery. A sketch will be offered—and sketch it is, since in scientific methodology, as in traditional philosophy, complex issues are so intimately intertwined that one can scarcely discuss one intelligibly without temporarily simplifying or ignoring many others—of how theoretical constructs fall more or less mechanically out of certain logically complex empirical variables, here called "structural variables," whose formal characterization reveals that the data language of a science supports a fairly elaborate type-hierarchy in the Russellian sense. In order to accomplish this, however, it will first be necessary to clarify the logical grammar of scientific "laws" (or better, "natural regularities") and to formalize what is probably the basic methodological concept of science, the scientific "variable." A bit of this preliminary ground has already been covered by Carnap and Menger (*cit. infra*), but I have yet to detect signs that these insights have seen much dissemination.

2. Scientific Variables.

It is impossible to do much reading in mathematics, science, statistics, or logic, nowadays, without dealing extensively with "variables." It does not seem generally to be appreciated, however, that the term 'variable' is seriously ambiguous in its various occurrences. One would think that a non-intuitive technical concept this fundamental would have an extensive literature devoted to it, if only to explain its usage to the novice. Actually, while logicians have made *their* use (or uses) of the notion reasonably clear, discussion of its quite different character in scientific methodology is virtually nonexistent. I am aware of only one modern thinker, the mathematician Karl Menger, who has seriously attempted to disentangle these various meanings and to explicate the formal properties of "variables" in the scientific sense (e.g., Menger, 1954, 1955, Ch. 7). In what follows, I shall differ somewhat from Menger both in terminology and scope,[2] but the present interpretation is basically in agreement with his.

While a number of secondary meaning shades can be recognized in each category, the term 'variable' as used today has two basic, radically distinct, senses: The logical, or *syntactic*, on the one hand, and the substantive, or *scientific*, on the other. (1) In the syntactic sense, a *variable* is a linguistic structuring device which acts, as it were, as a place-marker to index the spot where a descriptive constant would occur under more determinate circumstances and to afford a point of application for logical operators. Examples are 'x' in the propositional function 'x is blue', 'y' in the statement 'There is a number, y, such that $y = 2 + 3$', and 'z' in the definite description 'The z such that $z = 2 + 3$'.[3] A *variable* in the syntactic sense cannot properly be said to *vary*—i.e., to partake of change, alteration, or flux. At best, the expression in which the variable occurs is altered by replacing (*not* varying) the variable with a constant or another variable. On the other hand, subject to restrictions concerning the scope of operators, the variables in a given proposition can freely be replaced with others without changing the proposition's meaning. (2) Very much in contrast, the notion of *variable* as employed in science denotes such abstract entities as Weight, Height, Habit-strength, Eye-color, etc. A scientist's variables are part of his subject-matter, not of his linguistic machin-

[2] Menger, with the typical mathematician's viewpoint, admits only of quantitative variables, defined extensionally.

[3] Many components of mathematical formulas would also qualify as examples, except that traditional mathematical notation is severely elliptical, including suppression of all logical operators, and the precise roles of the variables in such formulas tend to be ambiguous.

ery, and the terms by which he refers to them function syntactically as descriptive constants, not as syntactic variables. Variables in the scientific sense are the sort of thing which *can* properly be said to fluctuate ("John's weight has varied a great deal this year"), and *cannot* be interchanged (or rather, the terms referring to them interchanged) without altering the meaning of what is being said.

In order to distinguish the scientific sense of 'variable' from its syntactic meaning, some writers have used the expression 'variable quantity'. In addition to an unfortunate tendency to preserve the original ambiguity, however, this phrase unwisely restricts the concept to quantitative variables. Within recent years there has been an attempt by some statisticians to adopt the term 'variate' for variables that *vary*, but this usage has as yet made only limited headway—certainly none in the empirical sciences. While it would indeed be advantageous to have separate expressions for the syntactic and scientific meanings of 'variable', this term in its latter sense is so deeply and synonymlessly imbedded in scientific thinking that any discussion of scientific methodology as it is actually practiced has a strong commitment to this usage. Consequently, except where explicitly qualified by the adjective 'syntactic', the word 'variable' will henceforth occur in this article only in its scientific sense.

The concept of (scientific) *variable* has emerged as a powerful technical device for systematic description of the attributes of things and the inter-dependencies found among them. In particular, in empirical sciences which have moved beyond the classificatory stage, it provides a standard form for the recording and relating of data. For this and other reasons, it is advantageous to begin with a brief look at the concept of *data*.

Every empirical science rests upon a foundation of propositions, recorded in detail (albeit usually in abbreviated notation) in laboratory protocols or field reports and summarized in technical journals, whose truth has (presumably) been verified by direct observation. The facts which these propositions describe are the "data" of the science, and the statements which express them are its "datum-sentences." This "direct observation" by which scientific data are determined is not direct experience of "sense data" in the sense of phenomenalism, but is usually mediated by perceptual accessories such as counters, microscopes, photographic plates, and the like. Neither are datum-sentences regarded as so incorrigable that they cannot be brought under suspicion of faulty observation or even outright fraud when they appear irreconcilable with other data. However, datum-sentences express those beliefs within the official corpus of the science which are regarded as most firmly established. They are the criteria against which empirical (i.e., data language) generalizations and theoretical propositions are assessed, and anchor both ends of the science's inferential chains. That is, datum-sentences are, ultimately, that *from* which and *to* which, by the machinery of generalizations and theories, scientific inferences are made.

Now, the sort of thing that a person seems to observe most directly in this world is that a certain object has a certain attribute, or that certain objects stand in a certain relationship. Correlatively, we find that the datum-sentences of a science (always?) parse grammatically in the subject-predicate form, in which a stated n-adic property is attributed to a specified n-tuple of entities. Drawing upon everyday language for examples in order to avoid the technical details of more precise concepts, the following illustrate the type of sentences which express the raw observations of scientific inquiry:

(1.1) Tom weighs 164 lbs.
(1.2) Linda has blue eyes.
(1.3) John loves Marsha.
(1.4) Rat #39 is exposed to a flashing red light.
(1.5) That ugly beast in the far cage is an orangutan.
(1.6) This rock is harder than that one.
(1.7) Peter has three siblings.

Replacing the subject-terms of these sentences with syntactic variables, we obtain the predicates[4]

(2.1) s weighs 164 lbs.
(2.2) s has blue eyes.
(2.3) s_1 loves s_2.
(2.4) s is exposed to a flashing red light.
(2.5) s is an orangutan.
(2.6) s_1 is harder than s_2.
(2.7) s has three siblings.

which are ascribed, in order, to Tom, Linda, the ordered pair ⟨John, Marsha⟩, rat #39, that ugly beast in the far cage, the ordered pair ⟨this rock, that one⟩, and Peter; or, more accurately, to certain implicitly specified time-slices (i.e., temporal stages) of these.[5]

In classifying datum-sentences, illustrated by (1.1)–(1.7), as subject-predicate in form, I merely wish to indicate something about the syntactic behavior of these sentences as they participate in the science's language maneuvers, not to make

[4] The purist who insists on a distinction between predicates and propositional functions may here and subsequently affix abstraction-operators as appropriate.

[5] When speaking of the time-dependent attributes of an object, it is most convenient to regard the subject of the sentence as a temporal cross-section, or stage, of the object. Another, less satisfactory, alternative would be to treat the subject of the sentence as a pair comprised of the object and a moment in time.

claims about an ultimate logical structure. Most sentences can be formalized in a number of ways, and a sentence which is in subject-predicate form under one analysis may not remain so under another. Thus if 'three' and 'sibling' were logically unpacked, (1.7) would bristle with quantifiers and connectives. However, the way a sentence is *used* defines for it a functional grammar, namely, the minimal syntactic structure necessary to formalize its role in the inductive and deductive chains in which it participates. It is in this sense that I speak of the grammatical structure of scientific propositions, and assert, in particular, that datum-sentences are (usually) of subject-predicate form.[6]

Sentences which functionally have a subject-predicate character behave formally as "atomic" sentences, and in contrast to functionally molecular or generalized sentences, express what feel like brute, elemental facts which call for explanation and prediction but do not themselves have explanatory or predictive force.[7] Restrict the facts cited in the previous sentence to "known" facts and one also has a passable description of the epistemic status of data. Hence we may introduce the adjective 'datumform' to describe sentences which are functionally subject-predicate in character, thereby indicating that such sentences are psychologically on a par with statements of raw data in their lack of ability to systematize and explain. This concept will be of service in Section 5, below.

Now, the predicates which first emerge from the datum-sentences of a science by abstraction over their subject-terms may be seen to stand in certain interesting formal relations to one another; namely, they form internally incompatible clusters. Thus compare (2.1)–(2.7) with

(3.1) s weighs 108 lbs.
(3.2) s has grey eyes.
(3.3) s_1 abhors s_2.
(3.4) s is not exposed to a flashing red light.
(3.5) s is an elephant.
(3.6) s_1 is softer than s_2.
(3.7) s is an only child.

Each predicate $(2.i)$ is incompatible with predicate $(3.i)$ in that no entity (more precisely, no ordered n-tuple of time-slices of objects) can satisfy both, even though no such incompatibility exists between $(2.i)$ and $(3.j)$ $(i \neq j)$. Thus no object can

[6] As will shortly be seen, however, codification of data in terms of (scientific) variables has the result that (2.1)–(2.7) do not adequately reflect the subject-predicate analysis which a science would ultimately give to (1.1)–(1.7)

[7] That is, no atomic sentence can be deduced from any set of other atomic sentences. Whether any atomic sentence can be *inductively* inferred from other atomic sentences (e.g., '$P(a_n)$' from '$P(a_1)$', …,'$P(a_{n-1})$') without passing at least implicitly through an inductive generalization is perhaps an open question, though one which I am inclined to answer negatively.

at once weigh 164 lbs. and 108 lbs., though there is no reason why it cannot both weigh 164 lbs. and have grey eyes. Obviously, (2.i) and (3.i) ($i \neq 4$) can be augmented with still other alternatives which share this mutual incompatibility. In fact, by suitably altering certain terms in (2.i) as indicated in (4.i), we can generate a set of predicates which are mutually exclusive and exhaustive over an appropriate domain of ordered n-tuples. That is, each element of the domain satisfies exactly one predicate in the set.

(4.1) s weighs ____ lbs. (Variously insert names of all the positive real numbers.[8])
(4.2) s has ____ eyes. (Variously insert adjectives describing all possible coloration-totalities.[9])
(4.3) s_1 ____ s_2 (Variously insert verbs designating all possible emotional attitude-totalities.[10])
(4.4) s ____ exposed to a flashing red light. (Variously insert 'is' and 'is not'.)
(4.5) s is a(n) ____ . (Variously insert the names of all possible genera of organisms.)
(4.6) s_1 is ____ s_2 (Variously insert 'harder than', 'equally hard as', and 'softer than'.)
(4.7) s has ____ siblings. (Variously insert adjectives referring to all the positive integers, including zero.)

Then each of these sets of predicates (when synonyms are winnowed out) are mutually exclusive and exhaustive over their subject-domains when these domains are, respectively, time-slices of objects in (4.1), time-slices of organisms in (4.4), (4.5) and (4.7), time slices of eye-bearing organisms in (4.2), ordered pairs of time-slices of organisms in (4.3), and ordered pairs of time-slices of objects in (4.6).

To simplify discussion, let us henceforth assume that to every meaningful predicate or propositional function '$P(x)$' there corresponds a property P such that an entity in the domain of 'x' satisfies '$P(x)$' if and only if it has property P. (The ontologically queasy reader may substitute 'class' for 'property' and 'belongs to' for 'has' in the previous sentence if it will help him tolerate this move. Philosophical scruples or no, transition from the formal to the material mode of speech not only avoids problems due to *de facto* linguistic inadequacies (cf. Note 8), complications arising from synonymy and idiom, etc., but is also necessary to reflect

[8]Actually, since the class of all positive real numbers is non-denumerable, not all of these predicates can be thought of as actually existing in the language. Similarly, many of the other predicate-sets involved in the definition of scientific variables exist only in imagination, since not all the necessary definitions and conceptual clarifications have actually been carried out (e.g., (4.3) and (4.5)). The assumption made in the following paragraph will obviate this difficulty, however.

[9]By "coloration-totalities" I mean color specified in such a way that no object can be two different colors at once. For a discussion of this point with respect to the hoary philosophical problem of color-incompatibility, see Rozeboom (1958a)

[10]See Note 9.

the assumptions actually made in science; for the extensive and apparently indispensable quantification in scientific discourse over predicate terms admits a host of abstract entities. How the language of science may be reworked to minimize these reifications, and what the consequences may be of such an assumption when it can neither be paraphrased away nor justified ontologically is a problem which must be reserved for another occasion.) Let a set K of properties be called a *partition* of a domain D of ordered n-tuples when for each element d of D, there is one and only one property in K which holds for d. Then what (4.1)–(4.7) illustrate is how certain observed properties can be seen to belong to partitions of their subject-domains. But obviously this is completely general: Any set K' of properties which are mutually exclusive over a domain D, including the case where K' has only one member, can be expanded into a partition of D by adding the property of not possessing any of the properties in K' (cf. (4.4)). Hence *any* n-adic property, observed or theoretical, may be analyzed as belonging to a partition of the ordered n-tuples over which the distribution of this property is of scientific interest.

Now, the proper exploitation of a science's data require a record not only of what properties its objects of study have been observed to have, but also what properties they have been observed *not* to have (since one determines the answer to a question as much as does the other). Clearly, therefore, the sorting of properties into partitions affords a tremendous saving of descriptive labor, especially when the partition contains a large number of properties. For then it suffices to state which property in the partition *does* hold for a given entity, since this concomitantly serves notice that the remainder do not. (Imagine the sorry plight of a physicist who had to state separately whether or not an object has a weight of five gms., and six gms., and seven gms., etc.) Additional, equally vital methodological benefits emerge from partitions when the predictive implications of a property, or combination of properties, are considered (see Section 4, below). Only as a scientific discipline does, in fact, work out clearly conceived partitions of its subject-domains is it in position to begin serious processing of its data, and it is for precisely this reason that the concept of (scientific) *variable* is so basic to scientific methodology. For apart from some secondary modifications to attain further technical convenience, a science's variables are simply the partitions of which it avails itself. That is, *as a first approximation, a scientific variable over a domain D is a set, K, of properties such that each element of D has one and only one property in K.* Correlatively, to the same degree of approximation, the *value* of a (scientific) variable K for an element, d, of D is that one property, P, in K which d exemplifies. Then to say that P is the value of the K variable for d is to assert that $P(d) \cdot P \in K$.

The preliminary definition of "variable" just given, however, is not the most satisfactory for giving an account of how scientists actually talk about their variables. For although by this definition two variables with the same range of values would necessarily be identical, we would normally want to say, e.g., that Number-of-siblings and Number-of-traffic-violations, which are obviously different variables, nonetheless have the same range of possible values, namely, the positive integers. More generally, statistical analysis of the distributions of quantitative variables proceeds (e.g., in computation of Means, SDs, etc.) as though the values of the variables are in all cases real numbers. Again, geneticists would insist that the Color-of-father's-eyes and Color-of-mother's-eyes variables are quite distinct, even though they have the same range of values, namely, colors. These and many other instances (including convenience in the formulation of scientific "laws") show that it is more satisfactory to define "variable" in such fashion that a variable is not necessarily the same as the set of its possible values, so that many different variables may have overlapping or coincident value-ranges. There are several ways, differing in notational details and reflecting somewhat different ontological commitments, in which the desired revision can be carried through. Examination of these alternatives and weighing of their respective merits is a chapter on scientific methodology which badly needs to be written, if only to allay terminological discord. (For a rousing debate, ask a group of scientists or philosophers whether *two feet* and *twenty-four inches* are the same value of a single Height variable—and if so, are *two feet* and *twenty-four inches* then the *same* "denominate number"?—or whether Height-in-inches and Height-in-feet are two different variables with the same range of values, namely, positive real numbers.) Such an analysis is unnecessary for the present undertaking, however, so I shall choose the formulation which seems to me to be the most convenient.

It will be noticed that with two exceptions, the predicate-schema (4.i) can much more simply be rewritten as a single relational predicate; i.e., vspace-.5em

(5.1) s weighs n lbs. ('n' ranges over numbers[11])
(5.2) s has c colored eyes. ('c' ranges over coloration-totalities)
(5.3) s_1 feels e for s_2. ('e' ranges over emotional attitude-totalities)
(5.5) s is a member of the x genus, ('x' ranges over genera of organisms)
(5.7) s has n siblings, ('n' ranges over numbers)

where the domain of 's' in (5.i) is the same as in (4.i). At the cost of some awkwardness, schemata (4.4) and (4.6) may also be rewritten in this way; e.g.,

(5.4) s is in the flashing-red-light stimulus state of q.
(5.6) s_1 stands in the qualitative-hardness-relation of h to s_2.

where "qualitative-hardness-relation" is defined to be the set of relations ⟨being softer than, being equally hard as, being harder than⟩, and "flashing-red-light

[11] Of course the range of *possible* values in this case is only positive reals, but there is no harm done, and quite possibly technical advantages to be gained, by granting 'n' a more inclusive range.

stimulus state" is defined to be the pair of conditions ⟨being exposed to a flashing red light, not being exposed to a flashing red light⟩. Each of the propositional functions (5.i) is of the form $\Phi(\alpha, \beta)$ in which the domain, D, of α is a class of ordered n-tuples and the range, R, of β is a set of abstract entities; hence (5.i) determines a certain dyadic relation between elements of D and members of R. Moreover, since for a given element d of D, there is one and only one member of R to which d stands in this relation, the relation is a *function*[12] from D to R. Consequently, we may revise the preliminary definition of scientific variable to read: *A scientific variable over a domain D is a function from D into a set, R, of abstract entities.* An entity of the domain D will then be said to be an *argument* of the variable, and the member of R into which the function maps a particular argument will be called the *value* of the variable for that argument. Thus if the function defined by (5.1) is called the "Weight-in-lbs." variable, the arguments of the Weight-in-lbs. variable are temporal stages of objects, its possible values are positive real numbers, and if Tom weighs 164 lbs. today, the value of the Weight-in-lbs. variable for Tom, today, is the number 164. Similarly, if Tom (today) has blue eyes, the value of the Eye-color variable for Tom (today) is the coloration-totality Blue.

The definition of "variable" just given is somewhat vague about what functions from the domain D are to count as (scientific) variables over D. Actual scientific practice suggests that some restriction should be made. For example, a geneticist studying the inheritance of eye-color would list Father's-eye-color and Mother's-eye-color among his variables, but would not consider Father-of and Mother-of (which map elements of D into other elements of D) to be such, even though he makes important use of the latter functions. Description, above, of a variable's values as "abstract entities" is meant to suggest some such restriction, but is not very helpful for actually spelling it out. Just how the restriction should be drawn, however, is not a matter about which I feel entirely clear.[13] Fortunately, this is not an issue which needs to be resolved here. A restriction which must *not* be made, however, is one which is implicit—or explicit—in practically all previous writings on this subject, namely, that a scientific variable must take *numbers* as its values. To do so would be to make 'variable' synonymous with 'quantitative variable', in flagrant disregard of the obvious fact that qualitative variables (e.g., (5.2)–(5.6)) have a currently indispensible place in a number of scientific disciplines,

[12]We may here define a function from a domain D into a range R to be a 2-place property P such that for a given element d of D, there is one and only one member r of R such that *P(d,r)*. We shall not here be concerned with any possible distinction between a function on the one hand, and the function-defining relation which obtains between an argument and the function's value for this argument on the other.

[13]Perhaps a definition of the subject-matter of the science would specify certain classes of attributes as the focus of its concern (e.g., (4.1)–(4.7)), and the functions which are formally isomorphic with these (e.g., (5.1)–(5.7)) then constitute its variables.

and in some instances have been found to participate in impressive law-systems (Mendelian genetics, the laws of color mixture, and others). It is not sufficient to point out (e.g., Carnap, 1937, p. 54f.) that numerals can be assigned to designate the values of a qualitative variable. Such a procedure is known as a "scaling" of the variable, and when properly done, is a valuable methodological tactic. But using *numerals* to stand for non-numerical entities is to *change* the meaning of a symbol which in its normal meaning designates a number, *not* to convert the variable into one which has *numbers* as its values. Whether or not every non-quantitative variable is isomorphic with a quantitative one is an important question which still awaits study. In any case, simply to assume without further analysis that all variables can be converted into quantitative ones is to undercut the whole theory of scaling, a subject which has already received more attention than most methodological issues in science and is still far from adequately understood.

It was pointed out earlier that any property can be construed as belonging to a partition of the domain under consideration. But obviously, every partition K is isomorphic with a scientific variable over this domain—if a more convenient variable cannot be found, there is always the variable $\mathbf{K}$, defined

$$\mathbf{K}(s, P) =_{\text{def}} P(s) \cdot P \in K,$$

as illustrated by (5.4) and (5.6). Consequently, every sentence in subject-predicate form can be rewritten in the form, 'The value of variable $\mathbf{V}$ for s is x'. This, or some paraphrase thereof, is in principle now standard terminology for datumform sentences in most sciences. Execution of this methodological strategy in practice, on the other hand, is blunted by the fact that many of the variables actually studied—at least in the behavioral sciences—are so poorly conceptualized that even the partitions involved are highly uncertain, much less how they are to be transformed into variables. One of the most important ways in which methodological analysis can be of use to an empirical science is to help clean up its variables. In fact, such observations as that there are a great many alternative ways to partition a domain with respect to what intuitively feels like the same kind of property (e.g., in accord with the variable defined by 's is h inches tall', where 'h' ranges over all positive real numbers, in contrast to the variable defined by 's is between h – .5 and h + .5 inches tall', where 'h' ranges over positive integers only); that a set of kindred properties may have to be augmented with a xenomorphic alternative in order to partition the domain desired (e.g., hair-color, which has to include a value corresponding to baldness if it is to constitute a complete partition of time-slices of persons); that by no means all well-defined variables are empirically fruitful; and other complications which arise when scientific variables are studied in technical detail, all point to the need for development of a comprehensive theory of variables which, among other objectives, attempts to discover the properties which determine a variable's scientific fertility and to devise methods for selecting variables of maximal potential out of the alternative constructions available.

For the ensuing discussion, it will be convenient to adopt the following notational conventions: Specified or arbitrary scientific variables will be designated by boldface capital letters, e.g., $\mathbf{V}$. The 2-place[14] propositional function (e.g., (5.1)–(5.7)) which defines the variable $\mathbf{V}$ will be abbreviated '$\mathbf{V}(s,\ x)$' (or with other syntactic variables in place of 's' and 'x'). Continuing the present policy of not distinguishing between propositional functions and predicates (cf. Note 4), '$\mathbf{V}(s,\ x)$' is also to be construed as a name of $\mathbf{V}$. The first gap in the variable-name (marked in '$\mathbf{V}(s,\ x)$' by the syntactic variable 's') is instantiated by expressions which refer to arguments of the variable and will be called its "argument-place." Similarly, the second gap (marked in '$\mathbf{V}(s,\ x)$' by 'x') is its "value-place." Thus the propositional function 's weighs x lbs.', which defines a weight variable, may be abbreviated simply '$\mathbf{W}(s,\ x)$', in which 's' and 'x' mark the argument-place and the value-place, respectively. Then $\mathbf{W}(s,\ x) = \mathbf{W} =$ Weight-in-lbs.

It is also convenient to introduce the concept of "functor" at this time; namely, a *functor* is a descriptional function (i.e., an "unsaturated" expression analogous to a propositional function except that instantiations of a functor are definite descriptions rather than propositions) which, when instantiated by the name or description of an element d in the domain of *f*, designates the value of f for d (see Reichenbach, 1947, p. 311ff.). Thus when the name of any particular person is substituted for 's' in the descriptional function 'The weight in lbs. of s' the resulting expression designates that number which is this person's weight in lbs. It is customary in mathematics to use the name of a function as a functor-radical which, when prefixed to an argument-name, designates the value of the function for that argument. Thus if 'f' refers to a function *f*, the expression '*fd*' (or '*f(d)*') designates the value of f for the argument d. Similarly, when 's' is a syntactic variable, '*fs*' (or '*f(s)*') is a descriptional function which yields a description of the value of f for any argument whose name is substituted for 's'. At some slight risk of confusion when the function is a scientific variable, we shall continue this practice here. Thus '$\mathbf{V}d$' will be taken to designate the value of variable $\mathbf{V}$ for an argument d. More generally,

$$\mathbf{V}s =_{\text{def}} (\iota x)\mathbf{V}(s,x) \qquad (6)$$
$$= \text{The value of } \mathbf{V} \text{ for } s.$$

Hence,

$$(s)(x)[\mathbf{V}s = x \equiv \mathbf{V}(s,x)] \qquad (7)$$

[14] *Reminder*: We are for convenience treating a variable formed from a set of mutually exclusive and exhaustive n-place relations as a function of *one* argument from a domain of ordered n-tuples, rather than as a function of n arguments. Hence an argument of a scientific variable has n components, where $n \geq 1$.

Functor-notation in which the argument-place is entered as a subscript to the function-name is standard in the statistical literature. For example, the familiar statistical formula, '$\sum_{i=1}^{n} X_i$' translates as 'The value of variable $\mathbf{X}$ for entity #1, plus the value of $\mathbf{X}$ for entity #2, plus ... plus the value of $\mathbf{X}$ for entity #n'.

Analysis of the inner structure of scientific variables reveals many varieties and degrees of formal complexity. While such details are here basically irrelevant, two brief observations concerning this internal structure will be helpful. The first is that just as the domain of a variable may be comprised of ordered n-tuples where $n > 1$, so may the values of the variable have multiple components. Thus a spatial-position variable over a domain of time-slices of particles would take as its values triplets of numbers corresponding to coordinates in a three-dimensional reference frame. When the value-place in the variable-name has more than one component, the variable may be called a *vectoral* variable, or more generally, when higher degrees of order are involved, a *tensoral* variable. When two or more variables over the same domain are being explored for their joint implications, it may be convenient to concatenate them into a single vectoral variable. Thus, instead of inquiring about the values of the two variables $\mathbf{V}_1$ and $\mathbf{V}_2$ for an entity d, we may instead attend to the value for d of the single vectoral variable $\mathbf{V}_{12}$, where

$$\mathbf{V}_{12}(s,x) =_{\text{def}} \mathbf{V}_1(s,x_1) \cdot \mathbf{V}_2(s,x_2) \quad (x = \langle x_1, x_2 \rangle).$$

The other observation is that the argument-place, 's', in '$\mathbf{V}(s,\ x)$' may be part of a subordinate functor enclosed in the variable-name, so that an argument d of $\mathbf{V}$ is not the actual grammatical subject of the proposition '$\mathbf{V}(d,\ r)$'. Thus in the propositional function,'s's father has x colored eyes', which defines the genetics variable, Father's-eye-color, the argument-place is imbedded in a descriptional function, 's's father'. Therefore, if John's father has brown eyes, the value of this variable for John is Brown; yet when 'John' and 'Brown' instantiate the argument-place and value-place, respectively, of the variable-name, the subject of the resulting proposition, 'John's father has Brown colored eyes', is John's father, not John. Any variable $\mathbf{V}(s,\ x)$ over a domain D can be converted into another variable, $\mathbf{V}^*(s^*, x)$, over a domain D^* (where D^* may or may not coincide with D) through use of a function f which maps D^* into D; namely,

$$\mathbf{V}^*(s^*, x) =_{\text{def}} \mathbf{V}(fs^*, x).$$

This suggests the drawing of a distinction between the *formal argument* and the *factual argument* of a variable; namely, that the formal arguments are those entities whose names instantiate the argument-place of the variable-name, while the corresponding factual arguments are those entities which comprise the actual grammatical subject of the propositions so formed. (There are some technical complications in carrying through this distinction precisely, but these need not concern us here.)

3. The Logical Typology of Scientific Variables.

The examples used in the preceding section lend themselves to the implicit supposition that the arguments of scientific variables are always *particulars*, or ordered n-tuples thereof. The definition of 'variable' which has been adopted makes no such assumption, however. Actually, science makes important use of variables whose various domains consist of abstract entities (or ordered n-tuples thereof) at all levels of abstraction. In particular, among the arguments of scientific variables can be found the whole Russellian type-hierarchy of particulars, classes of particulars, classes of classes of particulars, etc.

Probably the most famous variable with classes as its arguments is the Cardinal-number variable, defined by the propositional function, 'c has n members'. This variable appears in various guises throughout all branches of science (e.g., 'Species c has n living representatives'), especially in the definition of statistical concepts. The latter, in turn, form a group of variables over classes which have become exceedingly important in contemporary science. Crudely, what a statistical assertion says is something about the behavior of a given variable, or set of variables, within a certain class of arguments, and statistical concepts such as "Mean," "Standard deviation," "Correlation," etc., generate scientific variables whose arguments are these classes and whose values correspond to alternatively possible statistical "behaviors" of the specified sort. For example, 'The mean Height-in-inches in population c is h', or '$\mathbf{M_H}(c, h)$' for short, designates a Mean-height-in-inches variable whose domain can be any set of populations whose members also belong to the domain of the Height-in-inches variable (e.g., species of organism, contemporary U.S. Sunday school classes, etc.), and whose value for an argument c (e.g., Miss Smith's Sunday school class last week) is the sum of the values of the Height-in-inches variable for the members of c, divided by the value of the Cardinal-number variable for c (e.g., Tom's height-in-inches plus Dick's height-in-inches plus Harry's height-in-inches, divided by three.)

Variables whose arguments can be classified according to the Russellian type-system may be given a corresponding type designation; namely, the type-level of a variable $\mathbf{V}$ is one greater than the highest type of any element in an ordered n-tuple which comprises an argument of $\mathbf{V}$. Thus the Height-in-inches variable, whose arguments are particulars and hence of zero type-level, is a first-level variable. The Mean-height-in-inches variable, whose arguments are classes of particulars and hence first-level in type, is a second-level variable. A third-level variable is one whose arguments are classes of classes, many examples of which are to be found in sampling statistics. (Thus, the Standard Error of the Mean for a variable $\mathbf{V}$ in samples of size n from population P is the standard deviation of the means of $\mathbf{V}$ among all samples of size n drawn from P. So described, the SE of the mean of $\mathbf{V}$ has population P as its formal argument, but its factual argument is a class of classes.) In general, the different possibilities for statistical behavior of a certain kind by an nth-level variable within a class of its arguments define an $(n+1)$th-level statistical variable.

Since the formal and factual arguments of a variable need not always agree in Russellian type, a corresponding distinction may be drawn between the variables's *formal type-level* and its *factual type-level.* In this paper, unqualified reference to type-level will always be to factual type-level.

The present definition of a variable's type-level ignores the logical type of the variable's values, and is defined for variables whose arguments are abstract entities other than classes only to the extent that the logical types of the latter are well defined. While this is adequate for the present purpose, which is to provide a convenient classification of variables according to the logical complexity of their arguments, future investigations may well suggest modifications of this definition.

Variables whose values are ascribed to arguments by datum-sentences are sometimes known as "observation variables." While the easiest sentences to verify are usually those whose subjects are particulars, it by no means follows that only first-level variables can be observation variables. In particular, if the values of a variable $\mathbf{V}$ can be observed for a sufficiently large number of members of a class c, then the statistical properties of $\mathbf{V}$ in c may also be said to be "observable"; for even if c is indefinitely large, repeated observations of the values of $\mathbf{V}$ for members of c, buttressed by modern sampling theory, in principle permit determination of the statistical behavior of $\mathbf{V}$ in c to any desired degree of accuracy. Hence variables which reflect such sampling-determinable statistical properties may also be regarded as "observation variables," if only in an extended sense.

4. Natural Regularities.

While no science can exist without a substantial foundation of data, if sciences were merely compendia of datum-sentences, they could scarcely command the pragmatic respect and intellectual interest they in fact do. But of course, the recording of data is only a means to detecting and formulating the interdependencies which exist among the attributes of things. That is, it is both an official goal and an active pursuit of science to discover laws of nature.

Now, the pragmatic force which underlies the concept of "law"—or better, to avoid this term's obscure but supercharged philosophical tensions, "lawful relation" or "regularity"—is that under certain conditions, knowledge about one event or events provides information about (i.e., is helpful for predicting) another. In the more sharply defined concepts of modern science, this is put by saying that a regularity is what holds under background or "boundary" conditions C when the value of a variable $\mathbf{V}$ for an entity s in circumstances C is dependent upon

s's value of another variable $\mathbf{U}$. ("Natural" regularities are then those which in some sense are extra-logical, in contrast to "analytic" covariations such as that between Height-in-feet and Height-in-inches. However, drawing this distinction sharply turns out to be extraordinarily difficult.) The precise statistical definition of this sort of dependency is not difficult to state: Variables $\mathbf{U}$ and $\mathbf{V}$ are not independent under circumstances C when and only when the joint probability distribution of $\mathbf{U}$ and $\mathbf{V}$, given C, fails to be the product of the marginal probability distributions of $\mathbf{U}$ and $\mathbf{V}$ in C. Explanation of this technical definition by showing its derivation from the intuitive notion of "providing information," and discussion of other problems which it introduces (notably, the difference between *probability* and *frequency*) would be a major undertaking which would lead us far afield. However, an intuitively simpler formulation which is almost equivalent to the technical one is that $\mathbf{V}$ is dependent upon $\mathbf{U}$ under C when the "best estimate" (defined in terms of the means of contingent distributions) of $\mathbf{V}$ for an entity s known to be in circumstances C, given also the value of $\mathbf{U}$ for s (and when no other information about s is utilized), is a non-constant function of the latter. (As an additional aid to intuitive appreciation, this condition, in turn, holds when the value of $\mathbf{V}$ for an entity in circumstances C can be predicted better when the value of $\mathbf{U}$ is known for that entity than when it is not known.) Therefore, subject to a slight, here irrelevant qualification,[15] *variable* $\mathbf{V}$ *is lawfully related to variable* $\mathbf{U}$ *in circumstances* C *if and only if the best estimate of the value of* $\mathbf{V}$ *for an entity* s *in circumstances* C (*disregarding other information which may be available about* s) *is a non-constant function of its value of* $\mathbf{U}$. The function which relates the best estimate of an entity's value of $\mathbf{V}$ to its value of $\mathbf{U}$ in circumstances C is known as the "regression" of $\mathbf{V}$ on $\mathbf{U}$ in C.

By saying only that it is the "best estimate" of one variable which is a function of another in a lawful relationship, we recognize the element of uncertainty found in even the tightest of empirical regularities. However, the logical details of statistical regression and the theory of prediction are neither simple nor particularly germane to the purpose at hand. Therefore, formal discussion of lawful relationships will here be confined to the ideally simple case in which the value of $\mathbf{V}$ is perfectly predictable from the value of $\mathbf{U}$ in circumstances C. In this case, a lawlike relation obtains between two variables under background conditions C when the value of the one variable for an entity which conforms to C is a determinate non-constant function of the value of the other variable for that entity. That is, the logical form

[15] Namely, it is in principle possible for the contingent distributions of $\mathbf{V}$ to be a non-constant function of $\mathbf{U}$ even when the means of the contingent distributions do not vary with $\mathbf{U}$.

of a (perfect) law-statement[16] is

$$(8) \qquad (s)(x)[C(s) \supset [\mathbf{U}(s,x) \supset \mathbf{V}(s, fx)]]$$

in which f is a (non-constant) function which maps values of $\mathbf{U}$ into values of $\mathbf{V}$. (8) may also be written as a functor-equation; namely,

$$(8') \qquad (s)[C(s) \supset [\mathbf{V}s = f(\mathbf{U}s)]],$$

which has greater brevity than (8), but does not make the logical form of the law so explicit. Conventional formulas asserting lawful relations (see examples below) are usually analyzable most directly as ellipses for form (8′).

Though instructive, it would be prohibitively space-consuming to support (8) by analyzing a number of "laws" as they are actually known to science and commonsense. (The analyses would also be intellectually taxing and perhaps disconcerting: Tightening up the variables involved would uncover a morass of unsuspected logical complexity and vagueness over detail, while the philosophically puzzling entities which appear as the arguments of many of these variables make only too clear that analytic philosophy is still a long way from spelling out all the prima facie ontological presuppositions of science and everyday life.) For the moment, therefore, I shall give only three abbreviated examples, one which is non-empirical. Others will appear in the course of subsequent discussion.

(*a*) *The area of a circle.* As every schoolboy knows, the formula for finding the area of a circle is '$A = \pi r^2$'. What the schoolboy probably does *not* know, however, is the proposition which this equation is intended to convey. (Obviously, the symbol-sequence '$A = \pi r^2$', which has no literal meaning, must be an abbreviation for something else.) In ordinary English, the relation between a circle's radius and area in terms of a given unit of measurement, say centimeters, may be expressed, 'Any circle with a radius of x cm. is πx^2 sq. cm. in area'—in symbols,

$$(s)(x)[\mathrm{Circ}(s) \supset [\mathbf{R}(s,x) \supset \mathbf{A}(s, \pi x^2)]].$$

An alternative, more compact formulation is that the area of a circle in sq. cm. is Pi times the square of its radius in cm.; i.e.,

$$(s)[\mathrm{Circ}(s) \supset [\mathbf{A}s = \pi \cdot (\mathbf{R}s)^2]]$$

[16] See likewise Carnap (1958, p. 169). There is, however, one obscure but important respect in which (8) is lacking; namely, it is written purely extensionally. Actually, to be classed as a genuine law-statement, (8) has to authorize subjunctive and counterfactual conditionals (e.g., if the value of $\mathbf{U}$ for s *were* x, the value of $\mathbf{V}$ for s *would be* fx), which raises some rather nasty questions about, e.g., the value of $\mathbf{V}$ predicted from a value of $\mathbf{U}$ which does not in fact obtain for any entity in circumstances C. But this complication (which is worked into the more general statistical definition of a law by the difference between *probability* and *frequency*) has to be ruled out of bounds in the present discussion.

the relation of which to the conventional ellipsis is readily apparent.

Because of their clarity in concept and simplicity in relationship, mathematical laws are much easier to analyze than are natural regularities. Other mathematical examples could be proliferated endlessly, especially when more than one independent variable is admitted. (For reduction of the case of n independent variables to form (8), see below.) However, it is empirical relationships which are of primary concern here, and so the following two examples illustrate the much messier laws found in the natural sciences.

(b) *The law of falling bodies.* The simplest case of this physical principle is usually expressed by the formula '$s = \frac{1}{2}gt^2$', in which 'g' stands for the empirically determined numerical constant, 32.16 (or thereabouts). This highly elliptical law-statement may be unpacked into a functor-equation,

$$(s)[C(s) \supset [\mathbf{S}s = \tfrac{1}{2}g \cdot (\mathbf{T}s)^2]],$$

in which an argument s is a time-slice of an object o at time t, and which says that if an s ($= o,t$) conforms to a rather complicated set of background conditions C—i.e., o is in a period of free-fall at t which began with zero velocity at a position near the earth's surface, etc.—then the distance in feet ($\mathbf{S}s$) between o's position at t and at the time it began free-fall is a certain function, namely $16.08 \times (\ )^2$, of the length of time in seconds ($\mathbf{T}s$) that o has been in free-fall at t. A more general statement of the laws of motion would describe the position of o at time $t + \Delta t$ as a stated function of the position, velocity, and force acting upon o at time t, an assertion which also analyzes readily in form (8) or (8′).

If the empirical "constant" g in the (simple) law of falling bodies is determined to one or two further decimals, it is found to vary a bit with different geographical locations. Hence '$s = \frac{1}{2}gt^2$' may be interpreted as a family of laws, one for each different region in which free-fall takes place. Thereby hangs a tale, the telling of which will shortly receive protracted attention.

(c) *The law of color-afterimages.* A person who stares fixedly at a color c and then transfers his gaze to a grey surface will soon see an afterimage whose color is the complement of c. Putting this is somewhat more precise terms (and in doing so we are forced to be explicit, if somewhat arbitrary, about certain features which are so easily left comfortably vague in ordinary speech), we could say, 'For any person o at time t, if o has maintained steady visual fixation on a colored patch for at least $30''$ prior to t, and at t transfers his fixation for at least $2''$ to a neutral grey surface, then, if o fixated a c colored patch just prior to t, o experiences at time $t + 2''$ a color which is the complement of c.' Read 's' for the pair, 'o,t', and form (8) is unmistakable. This example, incidently, illustrates that functional relationships are not restricted to quantitative variables. Both variables here take colors as their values, and the function, *complement-of*, which maps colors into colors, is defined by the laws of color mixing with no reference to quantities.

Sentences whose functional grammar classifies them as being of form (8) or (8′) are used to infer the value of $\mathbf{V}$ for an entity s from the value of $\mathbf{U}$ for s and the information that $C(s)$. Consequently, such a sentence has a genuine predictive and, to at least some extent, explanatory value, even if it is merely a fairly restricted empirical generalization occupying a relatively low rung in the hierarchy of scientific laws. (I ignore degenerate non-empirical statements such as one describing the relationship between $\mathbf{V}$ and $\mathbf{V}$. It might fairly be questioned whether such sentences have a functional grammar at all.) I shall therefore call a statement which is functionally of form (8) or (8′) (or more generally, a statistical assertion of the sort that (8) idealizes) a *lawform* sentence, irrespective of whether it qualifies fully as a "law" in the most profound philosophical sense (whatever this might be), to contrast its psychological status as a force for systematization, prediction and explanation with that of datumform sentences. The type-level of a lawform sentence will here be identified with that of the highest variable to which it makes reference.

Before turning at last to exploration of ontological induction, I cannot forbear two brief general comments on the nature of scientific "laws" as described here. First of all, it might be wondered if form (8) or (8′) has sufficient formal complexity to do justice even to ideally perfect laws. After all, may not a dependent variable be a function of several independent variables; and must a law always relate the values of independent and dependent variables for the same argument? But since the values of n variables for the same argument are equivalent to the value of a single n-dimensional vectoral variable for that argument, (8) encompasses laws with more than one independent variable. Further, while dependent and independent variables must have the same *formal* argument in an application of (8), the *factual* arguments may differ. Thus the law of color-afterimages, as described above, lawfully connects color experiences which are seconds apart; while as a further example, a genetical principle relating a person's blood-type to that of his parents would, in application, link three different factual arguments—i.e., person s, s's father, and s's mother—even though the dependent variable and the two independent variables, namely, Blood-type (of s), Father's-blood-type, and Mother's-blood-type, all have the same formal argument. In particular, any perfect relationship between variables $\mathbf{U}$ and $\mathbf{V}$ based on a more general (not necessarily one-one) coordination of their arguments (see Menger, 1958) can be expressed as a relation of form (8) between variables $\mathbf{U}^*$ and $\mathbf{V}^*$ obtained by substituting suitable descriptional functions in the argument-places of '$\mathbf{U}(s, x)$' and '$\mathbf{V}(s, x)$'. Hence while a lawform sentence may indeed display a more complex form than that made explicit in (8), this further complexity may be subsumed under the internal structure of the constituent variables.

Of course, to claim that all scientific statements are either functionally datumform or functionally lawform in the sense defined here would be decidedly premature. Just what other functional propositional forms play a significant role in scientific procedures is a question for further research.

My other comment has to do with the notion of "law" as it has tended to occur in the philosophical literature. Perhaps the most frequent conception has been to portray law-statements as simple universally quantified conditionals, i.e.,

$$(s)[P(s) \supset Q(s)],$$

sometimes accompanied with an admonition that the implication is in some nomological sense "necessary," or with a clause '$(\exists s)P(s)$' affixed to prevent the conditional from holding vacuously. My quarrel with this formulation is not so much that it is wrong, but that it fails to bring out a feature which is essential in both the scientific and the intuitive concept of "law." To say that possession of a property Q is lawfully influenced by possession of property P is to imply that occurrence of P *makes a difference* for occurrences of Q. Therefore, full expression of a law must indicate the tendency to Q not only among entities which have P, but also among those which do not. Granted, when $(s)[P(s) \supset Q(x)]$, that P would lack relevance for Q only if $(s)Q(s)$, the assertion '$(s)[P(s) \supset Q(x)]$' still fails to make the relevance explicit. Moreover, need to manifest the dependency of Q on P becomes particularly acute when we leave idealized cases and get into probabilistic relationships. If $P(s)$ implies $Q(s)$ with $p\%$ likelihood, we know nothing about the connection between P and Q unless we also know something about the probability of Q among entities which are *not* P.

A much more serious mistake in the philosophical conception of "law," on the other hand, has been the occasional assumption that a scientific law is what entails the time-sequence of an entity's properties of a certain kind —i.e., that any function which maps the time-coordinate of any time-slice s of a given entity E into the value of a certain variable $\mathbf{V}$ for s is a law of the sort dealt with in science. Russell sets up this concept of "law" (Russell, 1948, p. 312) and then points out the paradox (arguing from it that some restriction must therefore be placed on functions which count as laws) that every sequence of events is then necessarily lawful, since some function, no matter how irregular, will yield the value of $\mathbf{V}$ for temporal stages of E as a function of time. But while this relationship can indeed be put into form (8), notice how queerly it emerges. Let '$E(s)$ and '$\mathbf{Tc}(s,t)$' state, respectively, that s is a time-slice of entity E and that t is the time-coordinate of s. The "law" relating $\mathbf{V}$ to $\mathbf{Tc}$ for time-slices of E is then

$$(s)(t)[E(s) \supset [\mathbf{Tc}(s,t) \supset \mathbf{V}(s,ft)]],$$

in which f is the necessary function. But logical form is not the whole story of natural regularities, and the present case differs from laws which get studied by the empirical sciences in several important methodological respects, the most critical being that by definition, no two ss conforming to boundary conditions E can have the same value of the independent variable. (From this the existence of a function which perfectly relates $\mathbf{Tc}$ and $\mathbf{V}$ within time-slices of E follows analytically because the joint distribution of $\mathbf{Tc}$ and $\mathbf{V}$ is then necessarily such that each contingent distribution of $\mathbf{V}$ contains at most one member.) What this "law" actually does is merely to map certain arguments of $\mathbf{V}$ into their values of $\mathbf{V}$, and is hence logically of a kind with the single variable $\mathbf{V}$ itself, *not* with relationships between two logically distinct variables. Confessedly, the additional conditions to which a regularity must conform in order to count as a *natural* law were suggested only casually above; and indeed, inventory of lawform propositions discloses the need for a whole taxonomic spectrum between "analytic" and "synthetic" in the classic sense. Nonetheless, it should be clear that the distinctions which must be made would exclude the Russellian time-sequence *per se* as an instance of the sort of laws which are at stake in scientific research or philosophic inquiry about, e.g., the limits of determinism. Of course, it may well be that the iterated application of a set of natural laws generates a predictable time-sequence, but the progression of an entity's values of a variable, no matter how simple or complex the function which describes it, is not itself such a law.

5. Structural Variables.

As just seen, a natural regularity may be asserted by a universally quantified statement which says that any entity conforming to certain background conditions C has its value on one variable related by a stated function to its value on another variable. However, this same factual content may alternatively be expressed by a sentence whose grammatical structure is quite different, namely, as an assertion about the *class* of entities which conform to conditions C. For consider the following paraphrase of (8): 'It is a property of the class of entities in condition C that each member thereof has its value of $\mathbf{V}$ related to its value of $\mathbf{U}$ by the function f.' That is, let

$$R_{\mathbf{V}f\mathbf{U}}(c) =_{\text{def}} (s)[s \in c \supset \mathbf{V}s = f(\mathbf{U}s)]. \tag{9}$$

Then '$R_{\mathbf{V}f\mathbf{U}}(c)$' is a predicate over classes which states of a class to which it is ascribed that $\mathbf{V}$ is a function f of $\mathbf{U}$ therein. Adopting the notational convention that '$\hat{C}$' designates the class of entities satisfying conditions C, i.e.,

$$\hat{C} =_{\text{def}} (\imath c)[(s)[s \in c \equiv C(s)]], \tag{10}$$

it follows that (8) and (8′) are logically equivalent to

$$R_{\mathbf{V}f\mathbf{U}}(\hat{C}) \tag{11}$$

But although (8) and (11) express the same factual material, they nonetheless differ in important respects, both formally and psychologically. Formalized as (8), which is *lawform*, this material is in syntactical position to authorize inferences about the value of variable **V** in certain instances, and to be treated as one of the explanatory principles of the science. Conversely, (11) is *datumform*, and so formalized, has the syntactical status of an atomic, not generalized, sentence. (Of course, the suppressed structure of (11) can be reclaimed by adduction of (9), but that is another matter.) By itself, this syntactical difference would be merely a trick; but to the extent that (8) and (11) reflect different functional grammars which may be given to their common content, the distinction is fundamental: If the *use* which is made of this belief is adequately formalized by (11), then the holding of relation f between **U** and **V** in C is pragmatically a simple *datum*, a brute fact about class $\hat{C}$ which, rather than having explanatory value, is on a par with other data. Moreover, even if this content is for some purposes best rendered in form (8), it may still be most methodologically illuminating to employ form (11) for the analysis of other cognitive contexts in which it occurs. Thus if $\hat{C}$ is a thing-kind, say a species of organism (see example below), (8) might be used for certain practical problems of estimation, yet in a scientific investigation wherein thing-kind $\hat{C}$ and its peers are themselves the objects of study, the status of information about the relation of **V** to **U** in C would most properly be formalized as a datumform assertion like (11), while a lawform reconstruction such as (8) would be misleading. That is, to turn the argument around, when our primary concern is to make sense out of the similarities and differences among various classes of a certain sort, we frequently find that one of the important ways in which these classes differ is in the pattern of covariation certain first-level variables display within them. That one of these classes sustains the particular internal covariational pattern it in fact does is then simply a to-be-accounted-for datum, no different in epistemic status from any other datum except that the subject in this case happens to be a class rather than a particular.

Since the "structure" of a thing is the way in which its parts are interrelated, I shall call a predicate such as (9), which describes a possible way in which certain properties may happen to hang together within a class, *a structural predicate*, and the corresponding property *a structural property.*

By considering the various possible ways in which two variables can be related within a class (the detailed spelling out of which involves some complications which may be ignored here), structural properties may be sorted into partitions which then determine structural variables. Let the dyadic predicate '$\mathbf{R}_{\mathbf{VU}}(c, \phi)$' be defined for any two variables **U** and **V** as

(12) $$\mathbf{R}_{\mathbf{VU}}(c, \phi) =_{\text{def}} (s)(x)[s \in c \supset [\mathbf{U}(s, x) \supset \mathbf{V}(s, \phi x)]],$$

in which 'c' ranges over classes whose members are arguments of both **U** and **V**, and 'ϕ' ranges over various possible functions by which $\mathbf{V}$ can vary with $\mathbf{U}$.[17] Then $\mathbf{R_{VU}}$ is a *structural variable* whose arguments and values are classes and functions, respectively; and instead of (11), we may equivalently write

$$\text{(13)} \qquad \mathbf{R_{VU}}(\hat{C}, f),$$

or

$$\text{(13}'\text{)} \qquad \mathbf{R_{VU}}\hat{C} = f.$$

Of course, the formal arguments and values of a structural variable do not *have* to be classes and functions, respectively. A function which maps, say, particulars into classes (e.g., an object into a class of its parts) could be used to define a structural variable whose factual arguments are classes but whose formal arguments are particulars. Likewise, if the functions which are the values of a structural variable can be put into a one-one correspondence with abstract entities of another sort, the latter can be arranged to be the values of the variable (see example below).

In the remainder of this paper, I shall try to show how appreciation of structural variables illuminates the otherwise mystifying emergence of theoretical concepts from routine data-processing. Before plunging into such potentially controversial matters, however, let me first illustrate the concept of "structural variable" with a relatively innocuous example. Since organisms of the same species are approximately the same shape, albeit of different sizes due to variation in age and other growth factors, the height of an organism belonging to a given species will be (approximately) proportional to the cube-root of its weight. Thus for a certain species of chimpanzee, we would have, say, the species-specific empirical regularity that for any member, s, of *P. troglodytes*, if s weighs x lbs., then s is $.9\sqrt[3]{x}$ feet tall. That is, letting '$\mathbf{H}$' and '$\mathbf{W}$' designate the Height-in-feet and Weight-in-lbs. variables, respectively,

$$(s)(x)[s \in P.\,troglodytes \supset [\mathbf{W}(s, x) \supset \mathbf{H}(s,\ .9\sqrt[3]{x})]].$$

Similarly, we would find for hippopotami, let us say, that

$$(s)(x)[s \in H.\,amphibious \supset [\mathbf{W}(s, x) \supset \mathbf{H}(s,\ .2\sqrt[3]{x})]].$$

[17] To make this a genuine partition, the various ways in which $\mathbf{V}$ can vary with $\mathbf{U}$ must include less-than-perfect statistical covariations, and indeed, some of the examples discussed subsequently would bear little resemblance to reality if only perfect relationships were admitted. Strictly speaking, therefore, '$\mathbf{R_{VU}}(c, \phi)$' needs to be defined by a statistical formulation more complex than (12). However, the present article only attempts to sketch the outlines of what are actually quite involved methodological concepts, and (12) will here suffice as a simplified formal description of structural variables in the same way that (8) suffices as a simplified description of laws.

More generally, for each species S there is a numerical constant[18] k such that

$$(s)(x)[s \in S \supset [\mathbf{W}(s,x) \supset \mathbf{H}(s,\ k \ \cdot \sqrt[3]{x})]].$$

The magnitude of this constant varies characteristically from one species to another, assuming smaller values as the mass of the species-shape tends to be spread out horizontally rather than vertically.

Now while these height-weight covariations can be expressed in a way which gives them the form of laws, and circumstances can be imagined (e.g., episodes in the life of a zoo-keeper) in which they might be useful for predicting the height of a particular organism, it still seems a bit odd to think of these relations as a family of scientific principles, one for each species, which help to explain why a given organism has the particular height which it does. Rather, in the search for biological principles, the relation between height and weight within a particular species would be regarded simply as a distinguishing characteristic of the species as a whole, which in any study of, say, speciation and other genetic processes, constitutes part of the brute data which are to be systematized and explained. For most scientific purposes, therefore, intraspecies height-weight relationships would be expressed by sentences which are functionally datumform and should be formalized as such, e.g.,

$$\mathbf{R}_{\mathbf{HW}}(P.\,troglodytes,\ .9^*),$$

$$\mathbf{R}_{\mathbf{HW}}(H.\,amphibious,\ .2^*),$$

and more generally for species S,

$$\mathbf{R}_{\mathbf{HW}}(S,\ k^*),$$

where k^* is the function, k-times-the-cube-root-of. In the present example, since the values assumed by $\mathbf{R}_{\mathbf{HW}}$ over the domain of species are distinguished only by a numerical parameter, this number may itself be taken as the value of the variable; i.e.,

$$\mathbf{R}^*{}_{\mathbf{HW}}(S,\ k) =_{\text{def}} \mathbf{R}_{\mathbf{HW}}(S,\ k^*).$$

Then the relation between Height-in-feet and Weight-in-lbs. within a species may alternatively be described by the datumform assertion that the value of the structural variable $\mathbf{R}^*{}_{\mathbf{HW}}$ for species S is a certain number, k. Either way, laws may

[18]Even in real life, where no statement of this form would literally apply because of the statistical imperfections which appear, we can still arrange for the relation between height and weight within a species to be described by a function in the family, k-times-the-cube-root-of, by simply *stipulating* the **H-W** relation in S to be the best-fitting curve of this form. However arbitrary this might seem, it does serve to associate with each species a characteristic shape-describing constant and illustrates something which, in fact, is repeatedly done in the datum-level struggles of a science to define manageable observation variables.

now be sought which connect $\mathbf{R_{HW}}$ or $\mathbf{R^*_{HW}}$ with other variables—e.g., principles which account for changes in the value of $\mathbf{R_{HW}}$ during the evolutionary development of a species, correlations between $\mathbf{R_{HW}}$ and ecological adaptations, etc.

More generally, whenever the particulars studied by a science fall naturally into distinctive classes which differ significantly among themselves in the patterns of covariation which the first-level observation variables show therein, the structural variables which are defined by these covariations add a second layer of observation variables which participate in lawlike relationships of the second level. The latter, in turn, if sufficiently variable from one set of background conditions to another, similarly generate still another layer of third-level structural variables which form the ingredients of third-level regularities, and so on up the type-hierarchy until relationships are found which no longer depend upon restrictive background conditions.

Empirical sciences abound with concepts based on structural variables, and examples are not hard to come by, even though the intimate connection between empirical structural properties and theoretical concepts (see next section) frequently makes a given instance susceptible to alternative interpretations. Thus in physics, the specific gravities of minerals may be defined as the values of a structural variable concerning weight-volume relationships, while the force-potential (e.g., g) acting at different positions in a force field may be construed in terms of the Time-from-release – Distance-and-direction-traveled relations that would be found among standard test-particles placed at those points. Even more obvious examples are the Machian conception of "mass," defined as a force-movement relationship, and the "half-life" of radioactive substances. In fact, the possibility of establishing more satisfactory functional relations among lower-level variables by distinguishing among entities which would otherwise appear to be more or less of a kind is not infrequently used to help *identify* thing-kinds—e.g., the Hertzsprung-Russell diagram in astrophysics, on which different bands of plotted points are taken to discriminate Population I from Population II stars, and normal stars from white dwarfs (see Luyten, 1960) or the dark-adaptation curve of physiological optics, whose two limbs so strikingly reveal the difference between photopic and scotopic vision.

Similarly, structural variables help to explain why variables defined as ratios or other combinatory functions of two or more observation variables over the same domain—i.e., the sort of variable which Campbell (1920) labeled "derived magnitudes" and which have been called "intervening variables" in the recent psychological literature—are in some instances quite useful, even though they might seem only to lose some of the information contained in the defining variables. For if the various relations between variables **U** and **V** within different scientifically significant divisions $\{C_i\}$ of the domain of $\mathbf{U}$ and $\mathbf{V}$ form a set of functions distinguished only by a certain parameter, and the value of this parameter for one of these categories C_i—i.e., the value of the structural variable $\mathbf{R}^*{}_{\mathbf{VU}}$ for C_i—can be computed by a certain function f from the values of $\mathbf{U}$ and $\mathbf{V}$ for any member of C_i, then the "intervening" variable $\mathbf{I}_{\mathrm{VU}}(s, x)$,[19] where

$$\mathbf{I}_{\mathrm{VU}}s =_{\mathrm{def}} f(\mathbf{U}s, \mathbf{V}s),$$

determines the same partition as $\{C_i\}$ except for discriminating between the members of different categories with the same value of $\mathbf{R}_{\mathbf{VU}}$, and computation of the value of $\mathbf{I}_{\mathrm{VU}}$ for an entity s to a large extent determines the category C_i to which s belongs. That is, when a structural variable helps to differentiate among certain important groups of entities, it may be possible to extract out of the lower-level variables which comprise the structural variable another lower-level variable whose values have roughly the same significance as these groupings. (For further discussion, see next section.) Thus in the biological example just examined, dividing the height-in-feet of an unidentified organism by the cube-root of its weight-in-lbs. would provide a clue as to its species, while if it belongs to a heretofore unknown species, the value of this ratio, assessed in terms of known empirical regularities in which the structural variable $\mathbf{R}^*{}_{\mathbf{HW}}$ has been found to participate, would lead to expectations about other attributes of this organism and its species. Another example closer to actual usage may well be the physical concept of "density," the value of which for a homogeneous object corresponds to the specific gravity of the substance of which it is composed. The suggestion obtains, then, that what distinguishes those "intervening variables" or "derived magnitudes" which are scientifically fruitful from the vastly greater number which, if introduced, would be utterly worthless, is that the former descend from higher-level structural variables.

6. Ontological Induction I: Dispositional Variables.

In the previous section, I have tried to set forth the methodological properties of a highly important but heretofore philosophically unacknowledged type of concept in the empirical sciences, the "structural" variable. Especially salient features of structural variables are that (a) their values correspond to lawlike relations among lower-level variables where the appearance of such a regularity is regarded primarily as a distinguishing feature of a class in which it appears; (b) their factual arguments are second-level or higher in logical type, hence involving the higher

[19]The subscripts to '$\mathbf{I}$' are entered in roman type, rather than boldface, to indicate that $\mathbf{I}_{\mathrm{VU}}$ is on the same type-level as $\mathbf{U}$ and $\mathbf{V}$. A subscript affixed to the symbol for one variable in reference to another will here be printed in boldface if and only if the subsidiary variable is of lower type than the main variable.

reaches of formal complexity; and (c) to the extent that their values reflect statistically confirmable relations among observation variables, structural variables are themselves observation variables, and as such belong to the empirical foundations of the science, in contradistinction to its theoretical superstructure. Given an appreciation of this formal background, we are now in position to examine more substantive aspects of the role which structural variables play in scientific thinking; in particular, their origins as datum-concepts and the virtually irresistible pressures to theoretical inference which they exert.

As was briefly indicated in Section 5, the circumstances under which an empirical regularity observed between variables $\mathbf{U}$ and $\mathbf{V}$ under conditions C is likely to be regarded simply as a datum about the class $\hat{C}$ rather than as an explanatory principle of the science, are those in which the background conditions C are only one out of a number of comparable alternatives upon which the particular relation between $\mathbf{U}$ and $\mathbf{V}$ is very much dependent. For clearly under these circumstances the variable $\mathbf{U}$ does not in itself suffice to determine $\mathbf{V}$ in the manner observed. Some other factor must be involved, of which a particular $\mathbf{U}$-$\mathbf{V}$ relationship is then an indication. Moreover, if these alternative background conditions under which the various relations of $\mathbf{U}$ and $\mathbf{V}$ are observed are such in nature that they cannot themselves be admitted as this additional determinant (see examples below), and no other agent to which direct responsibility for these $\mathbf{U}$-$\mathbf{V}$ relations is attributable can be teased out of the principles already known to the science, then the $\mathbf{R_{VU}}$ variable becomes the observable counterpart of the inferred—and hence theoretical—variable which is presumed to underlie it, and the higher-level empirical regularities in which $\mathbf{R_{VU}}$ is found to participate indicate an isomorphic network of theoretical hypotheses.

Suppose, for example, that one of the adding machines in a certain office is observed to be doing its sums in a rather peculiar manner one day; specifically, that it always adds one too many. Now this observation is actually an empirical generalization, and might be expressed somewhat as follows: 'If the add-bar of Machine #3 is depressed at any time t on Feb. 30, 1961, then, if the number (actually *numeral*, but it will be harmless to waive the distinction here) showing in the dial of Machine #3 at t is x_1 and the number simultaneously pressed into its keyboard is x_2, the number which shows in the dial of Machine #3 a few moments after time t is $x_1 + x_2 + 1$.' There should be no difficulty in recognizing this as assertion of a functional relationship between the Numbers-in-keyboard-and-dial and Number-in-dial-a-moment-later variables for entities which conform to the background conditions of being a time-slice of Machine #3 on Feb. 30, 1961, when its add-bar is pressed. (At the price of a more complicated functional relationship, it would be easy to add the position of the add-bar as a third component of the independent variable and broaden the background conditions to include all temporal stages of Machine#3 on this day.) Yet this relationship would never be taken as an explanatory principle of cybernetics. Rather, it is simply a brute fact about Machine #3 on this day, epistemically on a par with any other datum which might be available about this machine, but requiring the formal machinery of a structural predicate for its expression. The relationship between the Numbers-in-keyboard-and-dial and Number-in-dial-a-moment-later variables is very much a function of the particular background conditions prevailing (i.e., those defining the set of machine stages scrutinized), and is discovered by observant secretaries and laboratory assistants to depend upon (i.e., to participate in second-level empirical regularities with) such factors as the type of machine involved, length of time since last repair, position of power plug, etc. Moreover, when an exasperated office-worker says of Machine #3, 'This machine isn't working properly; *there's something wrong with it*', this "something wrong" is something attributed to it at every moment of the period during which the malfunction takes place, including those times when, through inactivity, the machine is not actually yielding incorrect sums, and is hence an inferred characteristic to which responsibility for the observed input-output relation is attributed.

Another instance: Suppose that a certain rat seems to have developed a discriminatory response in the Skinner-box, as shown by the fact that he presses more frequently when the light is off than when it is on. This discovery was made, let us say, by keeping track of the rat's lever presses during a 30 min. run during which periods of illumination were alternated with periods of darkness. Let '*0*' designate the temporal segment of this rat during this run. Then the observation made (say) is that for any time-slice s from *0*, s's lever-pressing rate is 3.2 responses per minute when the light is on and 47.6 responses per minute when the light is off—obviously an empirical regularity within this set of rat stages. Now the relationship between light-state and lever-pressing rate is by no means the same for all time-segments of objects in Skinner-boxes, even when the objects are living organisms or when the time segments are from the same animal. It is, in fact, quite variable, and behavioral psychologists have worked hard to discover the factors which influence stimulus-response covariations of this sort. The relation between stimulus-conditions and lever-behavior observed among *0*'s temporal stages is simply a distinguishing feature of *0*, a to-be-accounted-for datum which might be genuinely puzzling (e.g., if the rat had just been trapped in the wilds) or is to be expected in view of known behavioral principles and the animal's recorded conditioning history. In fact, most behaviorists would agree without hesitation (given certain additional data which are of no concern here) that this animal had demonstrated a lever-pressing-to-dark habit. Yet this "habit" is something attributed to the rat at every moment of his Skinner-box stay (and beyond), whereas the light-lever correlation which was actually observed is a property of the *class* of time-slices from *0* and is *not* a property common to every member of this class. With scarcely a thought, the *empirical* structural property is converted into a lower-level *theoretical* attribute.

Again: When we see that John is able to do long division, what we have *observed* is a correlation between certain types of arithmetic problems presented to John under normally motivating conditions and the numeral-producing behavior of John shortly thereafter; yet the *ability* to do long division is attributed to John at each moment in the period of his life wherein this relation obtains, whether he is actually solving a long division problem at that moment or not. This example is not drawn so explicitly as the previous two, because the ability-concepts that we employ so glibly in everyday life are horribly vague. Not only are the criterion-responses loosely conceived, but description of an ability as what an individual *can do* ignores the antecedent stimulus-conditions. Nonetheless, a little reflection reveals that with a few possible exceptions, the criterion for an ability is not the mere emission of a certain act, but performance of that act in response to (i.e., in covariation with) the proper environmental circumstances. In similar fashion, other psychological traits (e.g., honesty, irascibility, generosity, shyness, etc.) are attributed to an individual on the basis of observed regularities between the type of situation in which he finds himself and the behavior in which he subsequently engages.

Obviously there is no limit to the examples available of this sort, not only from psychology but also from physics and any other discipline which makes use of predicates ascribed according to how an object reacts to certain test-conditions (e.g., tensile strength as assessed by maintenance of integrity under traction, temperature as determined by thermometric effect, flexibility as described in terms of shape-distortion under strain, electrical resistance as computed through amperage-voltage covariation, etc.) In general, any concept about the *response characteristics*, *reaction tendencies*, *powers*, or *effects* of a thing, to the extent it is a part of the data language, is about a structural property and must be expressed by a structural predicate—except that usually, as now to be discussed, it is allowed to slip down a level in the type-hierarchy and become a theoretical concept. I venture that the vast majority of technical concepts in empirical science, when scrutinized closely, may be seen to be of this sort, and a great many from ordinary language as well.

More generally, if an empirical regularity,

$$(s)[C(s) \supset [\mathbf{V}s = f(\mathbf{U}s)]],$$

has been observed to hold under conditions C, when this particular relation between $\mathbf{U}$ and $\mathbf{V}$ does *not* generally hold under circumstances other than C and has no ready explanation in terms of other known principles, the following chain of inference is likely to occur in scientific—and everyday—thinking:

(1) Since $\mathbf{V}\text{s} = f(\mathbf{U}\text{s})$ is not generally the case, that this is so within the class

$\hat{C}$ is a distinguishing feature of $\hat{C}$ (which way of looking at the matter, of course, is functionally datumform).

(2) What it shows, moreover, since $\mathbf{U}$ and $\mathbf{V}$ are not by themselves able to sustain this relation, is that members of $\hat{C}$ partake of some additional condition which, along with $\mathbf{U}$, is able to determine $\mathbf{V}$ in the manner observed—i.e., that there is an attribute $D_{\mathrm{V}f\mathrm{U}}$ such that

$$(s)(x)[D_{\mathrm{V}f\mathrm{U}}(s) \cdot \mathbf{U}(s,x) \supset \mathbf{V}(s,fx)], \tag{14a}$$

$$(s)[C(s) \supset D_{\mathrm{V}f\mathrm{U}}(s)]. \tag{14b}$$

Now so far this reasoning is impeccable, for C itself satisfies the conditions imposed on $D_{\mathrm{V}f\mathrm{U}}$ by (14*a, b*). However, further circumstances of the $D_{\mathrm{V}f\mathrm{U}}$-concept's birth contribute additional restrictions which in most cases eliminate C or other data-language constructs satisfying (14*a, b*) as an acceptable replacement for $D_{\mathrm{V}f\mathrm{U}}$. As a preliminary, note that a restricted regularity $\mathbf{R_{VU}}(\hat{C}, f)$ appears most datum-like when the background conditions C are defined in reference to some *particular*, such as a temporal segment of some object (cf. preceding examples). Since it is a prejudice of science that the concepts which participate in genuine explanatory principles must be truly *general*, and since the hypothesized property $D_{\mathrm{V}f\mathrm{U}}$ is conceived as being in some vague but important sense *responsible* for the relation between $\mathbf{U}$ and $\mathbf{V}$ in $\hat{C}$, clearly $D_{\mathrm{V}f\mathrm{U}}$ cannot be equated with C in this case. Even when $\hat{C}$ is a thing-kind, one can usually argue that its defining properties are not the sort that can properly be said to account for the structural attributes of $\hat{C}$. Hence a stipulation which should accompany (14*a, b*) is that '$D_{\mathrm{V}f\mathrm{U}}$' must not be defined in such a way that '$D_{\mathrm{V}f\mathrm{U}} = C$' is logically true, although the possibility of an *empirical* identity of $D_{\mathrm{V}f\mathrm{U}}$ and C need not be excluded.

A more fundamental reason why the inferred property $D_{\mathrm{V}f\mathrm{U}}$ cannot simply be identified with the background conditions C, however, is that it is the structural property $\mathbf{R_{V\mathit{f}U}}$ which is the clue—and at this initial stage, the only clue—to the presence of $D_{\mathrm{V}f\mathrm{U}}$. Hence by parity of reasoning, if $D_{\mathrm{V}f\mathrm{U}}$ is attributed to the members of C_i on the grounds that $R_{\mathbf{VU}}(\hat{C}_i)$, it must likewise be attributed to the members of any other class C_j such that $R_{\mathbf{V\mathit{f}U}}(\hat{C}_j)$, so long as $\hat{C}_j$, like $\hat{C}_i$, is a class whose structural properties inspire inference to attributes which underlie them. More generally, the chain of reasoning continues as follows:

(3) The relation, *f*, between $\mathbf{U}$ and $\mathbf{V}$ in $\hat{C}$ is only one of the possible relations which might have been found between these variables; and had another value, *g*, of $\mathbf{R_{VU}}$ been observed for $\hat{C}$, another underlying condition $D_{\mathrm{V}g\mathrm{U}}$, contrasting with $D_{\mathrm{V}f\mathrm{U}}$, would have been inferred to hold for the members of $\hat{C}$. These alternatives hence constitute an inferred variable $\mathbf{D}_{\mathrm{VU}}$,[20] the values of which correspond to

[20] See Note 19.

and may hence be identified with those of $\mathbf{R_{VU}}$,[21] such that

$$(s)\,(\phi)\,(x)\,[\mathbf{D}_{\mathrm{VU}}(s,\phi)\cdot\mathbf{U}(s,x)\supset\mathbf{V}(s,\phi x)]. \tag{15a}$$

Then, if $\hat{C}_i$ is a class within which the observed relation between **U** and **V** is interpreted symptomatically, the assumptions built into the $\mathbf{D}_{\mathrm{VU}}$-concept yield the hypothesis that

$$(\phi)\,[\mathbf{R_{VU}}(\hat{C}_i,\phi)\supset(s)\,[C_i(s)\supset\mathbf{D}_{\mathrm{VU}}(s,\phi)]]. \tag{15b}$$

It must be stressed that when an inferred variable $\mathbf{D}_{\mathrm{VU}}$ emerges in this way, it does *not* do so as a *deduction* from $\mathbf{R_{VU}}$. In particular, '$\mathbf{D}_{\mathrm{VU}}$' is not defined in such fashion that (15*b*) is logically true for all possible arguments $\hat{C}_i$ of $\mathbf{R_{VU}}$, for this would quickly bring disaster (see below). What (15*a*, *b*) describe is the conclusion of an inferential leap, the premises of which are a number of observations of form '$\mathbf{R_{VU}}(\hat{C}_i, f)$'. It is in short, an *ontological induction*, for it postulates the existence of a new variable intimately related to but distinct from—in fact, different in logical type from—the structural observation variable $\mathbf{R_{VU}}$. The conclusion so reached is certainly not unconditionally assertable; rather, it is a hypothesis which may or may not be borne out by subsequent evidence, and whose initial credibility is influenced by secondary factors such as the nature of $\hat{C}_i$, past successes of similar inferences, etc., just as confidence in a statistical induction is swayed by considerations of this sort. In particular, for (15*a*, *b*) to be acceptable it is necessary that $\hat{C}_i$ be what in some intuitive sense is a "natural" class. For if classes of otherwise heterogeneous particulars are assembled by picking and choosing according to their values of **U** and **V**, a structural property $R_{\mathbf{V}f\mathbf{U}}$ can be made to hold by sheer definition. For example, suppose that $\hat{O}$ is the set of temporal stages of a certain organism during a particular period of its life, and that within $\hat{O}$, the relation between the occurrence of a certain stimulus S and the emission of a certain response R is null—i.e., the probability of an emission of R, given the occurrence of S, is the same as the probability of an emission of R, given the non-occurrence of S. $\hat{O}$ can be conceptually divided into two subclasses, $\hat{O}_1$ and $\hat{O}_2$, where $\hat{O}_1$ contains those time-slices in $\hat{O}$ for which either S is present and R is emitted or S is absent and R is not emitted, and $\hat{O}_2$ contains the remainder of $\hat{O}$. Perfect, though opposed, non-null stimulus-response relationships then hold in both $\hat{O}_1$ and $\hat{O}_2$, in contrast to the null-relationship in $\hat{O}$, and an s which belongs, say, to $\hat{O}_1$ would be ascribed incompatible values of the habit-variable $\mathbf{D}_{\mathrm{RS}}$ by application of (15*b*) to both $\hat{O}$ and $\hat{O}_1$. However, $\hat{O}_1$ and $\hat{O}_2$ are obviously artificial in a

[21] Since at this stage the values of $\mathbf{D}_{\mathrm{VU}}$ are distinguished by the **U-V** relations which are their criteria, it is most convenient to conceive $\mathbf{D}_{\mathrm{VU}}$ in such fashion that the abstract entities which it takes as values are the same as the corresponding values of $\mathbf{R_{VU}}$. (How to replace a variable whose values are in one-one correspondence with those of a second variable with another variable whose values are the same as the second should be obvious from the discussion in Section 5.)

way that precludes any incentive to account for the S-R relationships therein by appeal to an underlying habit. It is the S-R covariation within the "natural" class $\hat{O}$ which determines the S-R habit-strength attributed to the members of $\hat{O}$. Just what differentiates "artificial" classes from those which are psychologically able to sustain an ontological induction is a question which still awaits exploration.[22]

Since the connection between $\mathbf{D}_{\mathrm{VU}}$ and $\mathbf{R}_{\mathbf{VU}}$ is conceived as a manifestation of *natural* regularities, not as a logically necessary consequence of a formal construction, recognition of the error-variance always found in lawful relationships as determined empirically and the perpetual revisions undergone by our beliefs about the world under the impact of increasing knowledge immediately qualifies the assumptions which emerge from an ontological induction with the reservation that they are undoubtedly simplified approximations to a more complex reality. Consequently, once a theoretical variable $\mathbf{D}_{\mathrm{VU}}$ has been generically introduced as in (15a, b), it is permissable to feel relatively free about secondary modifications in its postulated relations to other variables, if reasons so warrant. One likely revision is to relax the assumption (cf. (15b)) that all members of a class in which $\mathbf{R}_{\mathbf{VU}}$ is symptomatic of $\mathbf{D}_{\mathrm{VU}}$ have precisely the same value of $\mathbf{D}_{\mathrm{VU}}$. In experimental behavioristics, for example, extinction of an S-R habit as a function of unreinforced stimulus presentations is frequently determined by observing the S-R covariation within successive blocks of trials. If the value of $\mathbf{R}_{\mathbf{RS}}$ for the class of the organism's temporal stages during a given trial-block were taken to reveal the *exact* value of $\mathbf{D}_{\mathrm{RS}}$ at each moment therein, extinction of the habit (i.e., the change in the value of $\mathbf{D}_{\mathrm{RS}}$ as a function of unreinforced trials) would have to be construed as a nomologically puzzling step-function whose details, moreover, would depend on an arbitrary grouping of the trials into blocks. It is much more satisfactory to assume a continuous decrement in habit-strength with extinction trials and to regard the value of $\mathbf{R}_{\mathbf{RS}}$ within a block of trials as an estimate of the *average* value of $\mathbf{D}_{\mathrm{RS}}$ therein. Another modification which may become desirable, once '$\mathbf{D}_{\mathrm{VU}}$' has been admitted in explanation of $\mathbf{R}_{\mathbf{VU}}$, is to weaken the initially assumed one-one correspondence between the values of $\mathbf{D}_{\mathrm{VU}}$ and $\mathbf{R}_{\mathbf{VU}}$ by allowing for the possibility that more than one value of $\mathbf{D}_{\mathrm{VU}}$ might give rise to the same relationship between $\mathbf{U}$ and $\mathbf{V}$. What is conceptually fundamental about ontolog-

[22] This business of "artificial" classes, incidently, undermines the possibility of defining '$\mathbf{D}_{\mathrm{VU}}(s,\phi)$' as '$(\exists c)[s \in c \cdot \mathbf{R}_{\mathbf{VU}}(c,\phi)]$'. It is instructive to note that a very similar situation holds in the case of statistical induction, namely, that the formal pattern of the induction, when applied to certain oddly constructed antecedents, yields (inductive) conclusions which are not only intuitively unacceptable, but also clash with the conclusions which inductively follow from more "natural" antecedents (cf. Goodman, 1955, pp. 30f, 74f). This difficulty is currently perhaps the most enigmatic aspect of statistical induction, and the fact that ontological induction, as set forth in (15a, b), shares this same peculiarity is perhaps a little additional evidence that ontological induction and statistical generalization are on a par as primitive inferential devices by which human belief is extended beyond the narrow range of the given.

ical induction as described by (15a, b), however, is that *this is the basic form in which a theoretical concept first precipitates out of a set of empirical observations.* Once the new theoretical entry achieves recognition as an autonomous element in the conceptual scheme of the science, it is then in position to begin a course of revision and development which may soon obscure its humble origins.

Just because the credibility of an ontological induction varies with the nature of the class whose structural properties are the grounds for the inference and the inference itself can be reconstructed as a series of explicit steps as in (1)–(3) above, it by no means follows that the ontological inductions which actually occur in scientific and everyday thinking usually or even frequently follow such a course of deliberate reason. Quite the contrary, what makes ontological induction an "animal inference" of a kind with statistical induction is that it usually comes off with no conscious intent at all—in fact, for those restricted covariation-manifesting classes to which attention is drawn spontaneously, a deliberate effort is required to *abstain* from the inference. For the primordial incentive behind ontological induction is simply an urge to attribute to the individual members of a class a structural property which holds only for the class as a whole. Since literally this is logical absurdity, it has the effect (shades of the Axiom of Reducibility!) of introducing a lower-level theoretical surrogate for a data-language concept of higher logical type. This process, which might be called "typological sedimentation," is facilitated by the fact that the members of the class showing the structural property are in many cases readily conceivable as the parts of a more comprehensive entity of the same logical type as these parts, to which the structural property can then be attributed indirectly in that it is a property of this whole that the class of its parts has the structural property in question. Thus it is a property, say, of time-segment 0 of a certain organism that the class $\hat{O}$ of time-slices from 0 manifest a certain stimulus-response covariation. But we are not accustomed to distinguishing clearly between the attributes of a whole and those of its parts, especially when the whole is a temporal interval of some object, and so the structural property of the whole becomes naively conceived as a property shared by its components. Rather than demanding methodological sophistication, ontological induction is abetted by the logical laxity of language in use. No wonder, then, that even the radical empiricist, who imagines himself to be concerned only with what he can observe, actually conducts his private thinking and public utterances with a vocabulary profusely studded with theoretical terms.

Moreover, since ontological induction transpires so readily without conscious intent, it would be surprising if the empirical regularities which define the structural variable on which an ontological induction is based needed to be articulately expressed and well documented. It has been amply demonstrated, for rats and pigeons as well as people, that behaviors which correspond, in more mentalistic terms, to belief in such regularities can frequently be conditioned by one or two experiences of the proper sort. Hence theoretical suppositions would be expected to emerge long before their observable correlates become accepted knowledge. In fact, the recent history of theoretical behavioristics shows that the mere *plausibility* of certain empirical covariations lends respectability (and at times, unfortunately, also dogmatic credulity) to the theoretical constructs which they support.

In view of the infrarational nature of typological sedimentation, it might be suspected that structural predicates would not be the only concepts of higher type-level to undergo this downward drift *cum* theoretical transfiguration. To some extent this suspicion appears to be borne out: There is a tendency, for example, for the frequency of an attribute P in a class to be converted into a "propensity" to P possessed by each member of the class. For reasons about which I am not entirely clear, however, only structural properties seem to urge inference to theoretical attributes with the same relentless, unreasoned compulsion that characterizes other primitive forms of inference[23]—which is why the conception of "ontological induction" set forth here has been limited to this case. The manner and extent to which other observable properties of classes invite introduction of lower-level theoretical concepts is another of the many problems in scientific methodology still awaiting investigation.

As the reader is doubtlessly aware, attributes conceptualized primarily in terms of how an entity characteristically responds to or is affected by a certain kind of treatment are known in the philosophical literature as "dispositions." Despite the earnest philosophical scrutiny which dispositional concepts have received within recent decades, their analysis is still obscure. Efforts to locate them wholly within the data language have repeatedly proved unsuccessful, and the feeling appears to be spreading that dispositions are probably best classified as a form of low-grade theoretical entity. To this conclusion the present analysis lends substantial support. For while our purpose has been to trace the gestation of theoretical notions within the data collations of empirical science, not to clarify the meanings of dispositional terms already in use, concepts which are newly born of an ontological induction as portrayed in ($15a$, b) certainly pertain to the sort of thing—habits, abilities, response characteristics, and the like—which have traditionally been called "dispositions." Whether all dispositional concepts are of this kind, or whether some should be analyzed as more involved constructions related to but not identical with theoretical terms introduced by ontological induction is an issue

[23] One important factor may be that structural properties, unlike other class-attributes, provide simple, readily grasped postulates about the observable effects of their theoretical surrogates (i.e., ($15a$)). Conversely, it is very difficult to comprehend, e.g., just how a "propensity" to have a property P lawfully results in a certain frequency of P in a class of entities sharing that propensity.

for another occasion.[24]

That dispositional concepts which originate like '$\mathbf{D}_{\mathrm{VU}}$ in (15a, b) must on the whole be part of the theoretical framework of the science, rather than data-language constructs, has already been pointed out. Any data-language criteria for the values of the $\mathbf{D}_{\mathrm{VU}}$ variable, if applied ruthlessly, will in general inconsistently assign more than one value of $\mathbf{D}_{\mathrm{VU}}$ to a given argument, and are hence unacceptable as *rigorous* criteria for these values. One data-language construction which has a certain interest for special cases, however, is the propositional function '$\mathbf{V}s = \phi(\mathbf{U}s)$' (i.e., 'The values of **U** and **V** for s fall under the function ϕ'). This is unsatisfactory as a generic definition of '$\mathbf{D}_{\mathrm{VU}}(s, \phi)$' because a single pair of values $\langle \mathbf{U}s, \mathbf{V}s \rangle$ will simultaneously fall under a great many of the possible functions mapping values of **U** into values of **V**. (The situation is even worse when the more realistic case of imperfect covariation is considered.) However, it may be that the values of $\mathbf{R}_{\mathbf{VU}}$ which actually obtain within the domain of classes under consideration (e.g., weight-volume relations in minerals, height-weight relations in species, etc.) belong to a restricted set of functions identified by a parameter which can be determined from the values of **U** and **V** for any member of an argument of $\mathbf{R}_{\mathbf{VU}}$. Then, as discussed in Section 5, it is possible to define an "intervening" variable $\mathbf{I}_{\mathrm{VU}}$ which is essentially equivalent to '$\mathbf{V}s = \phi(\mathbf{U}s)$' when '$\phi$' takes values only from this restricted set of functions and whose value for an entity s identifies the value of $\mathbf{R}_{\mathbf{VU}}$ in the "natural" class to which s belongs. In such a case, the observation variable $\mathbf{I}_{\mathrm{VU}}$ is isomorphic with the dispositional variable $\mathbf{D}_{\mathrm{VU}}$ and it becomes very uncertain whether $\mathbf{I}_{\mathrm{VU}}$ should itself be held responsible for the observed **U-V** relations, or whether it is merely a good observational indicator of a more fundamental theoretical variable.[25]

7. Ontological Induction II: Structural Covariation.

According to the views developed above, dispositional concepts are nascent theoretical terms, born of structural predicates by ontological induction. Now clearly, dispositionals (at least those which have been traditionally so classified) do not alone suffice to provide the richness of theoretical texture found in the conceptual systems of most sciences today. It would appear, however, that many of the more abstruse elements and linkages of the theoretical network can also be shown to derive more or less immediately from observable regularities of higher

[24]What I have in mind is a possible necessity for distinguishing between dispositional predicates which *refer* to the theoretical property responsible for the **U-V** relation, and dispositional predicates which assert that an entity *has* a property which is responsible for the **U-V** relation.

[25]Thus Campbell (1920, p. 276): "It is rather difficult to say whether, in the present stage of development of physics, we actually mean by density this ratio [of mass to volume], or whether we merely employ that ratio as an indication of some other property, which is what we really mean by that term."

type-complexity. Discussion of these further developments with even the simplified precision employed so far is rather unwieldly, not only because of the logical intricacies encountered but also as a result of the distracting conceptual gaps and obscurities which quickly appear in the analysis of any example from actual scientific practice. Consequently, I shall here give only a brief, informal outline of how the empirical regularities found among structural variables, and the still higher-level structural properties which these in turn define, deposit additional strata of theory and afford the "triangulation in logical space" that gives convincing substance to an otherwise tenuous theoretical notion.

It was pointed out earlier that once a restricted regularity is treated simply as a datum concerning the value of a structural variable $\mathbf{R}_{\mathbf{VU}}$ for a certain class, an obvious move is to look for further regularities in which $\mathbf{R}_{\mathbf{VU}}$ is itself a participating variable. Thus, once it is observed that the sets of time-slices comprising various temporal segments of an organism differ in the relations they sustain between presentation of a certain stimulus S and emission of a certain response R, it becomes of concern to discover laws which govern this S-R covariation. Once discovered, such laws can be formulated—cumbersomely—as *empirical* regularities of higher logical type, or alternatively, with greater formal simplicity, as *theoretical* principles holding for the dispositions inferred from the structural variables. Thus the behavioral law, that the strength of a conditioned reflex is a function of the number of learning trials, can be expressed either as an *observed* dependence (under suitable background conditions) of the relation between stimulus S_c and response R within the time-slices of an organism between times t and $t + \Delta t$ upon the number of joint presentations of S_c and the unconditioned stimulus S_u received by this organism prior to t, or as an *inferred* dependence of an organism's S_c-R reflex strength at time t on this conditioning-history variable.

Now, *some* empirical regularities in which the dependent variable is structural can be rewritten to avoid the structural variable altogether by distributing its first-level components between the independent and dependent variables of a first-level regularity. While the formal details of this situation are too complex to be developed here in any generality, an illustration is the aforementioned dependence of conditioned reflex strength on the number of previous conditioning trials. The empirical relation between number of conditioning trials and the S_c-R structural variable can be rephrased without reference to the latter by stating that under the stipulated background conditions, emission of response R is a function jointly of S_c-presentation and the number of past conditioning trials. Moreover, since the relation between emission of R, presentation of S_c, and conditioning history can thus be expressed wholly in a first-level statement without recourse to an S_c-R reflex inferred from the S_c-R structural variable, this reformulation does nothing to confirm the existence of the S_c-R reflex—if anything, it weakens the latter's credibility by suggesting that this theoretical entity may be superfluous. However, such paraphrasing away of theoretical implications is *not* possible for all lawful relations involving structural variables. Many important regularities, as actually determined empirically, can be given lawform expression *only* on the higher type-levels, and these then contribute additional materials to the theoretical structure of the science. The prime example, here, is the case of *structural covariation.*

As the name implies, structural covariations are simply those natural regularities in which both the dependent and independent variables are structural. If **U**, **V**, **X**, and **Y** are first-level variables, it may turn out that the relation between **U** and **V** within a class which meets certain specifications K is itself a function of the relation between **X** and **Y** therein—i.e., that

$$(c)(\phi)[K(c) \supset [\mathbf{R}_{\mathbf{YX}}(c, \phi) \supset \mathbf{R}_{\mathbf{VU}}(c, F\phi)]], \tag{16}$$

where F is a function which maps values of $\mathbf{R}_{\mathbf{YX}}$ into values of $\mathbf{R}_{\mathbf{VU}}$. For example, within the sets of time-slices comprising various temporal segments of a certain organism, it might be observed that the strength of the relationship between presentation of stimulus S_1 and emission of response R waxes and wanes from one temporal segment to another in accordance with the strength of the relation therein of R to a second stimulus S_2. As a matter of fact, this tendency of the strength of one S-R covariation to influence that of another with the same response component is a highly important empirical phenomenon known in behavioral psychology as "stimulus generalization."

Now, while a structural covariation such as (16) can be written in a form which refers only to first-level variables, the result is a virtually incomprehensible logical monstrosity, bearing little or no resemblance to a lawform statement. To give these findings data-language expression in a way that reveals their significance, the scientist has no recourse but to the formal machinery of structural concepts. This has three immediate consequences: First of all, by focusing unavoidable attention on **U-V** and **X-Y** relations conceived as data, it virtually guarantees the introduction of theoretical variables $\mathbf{D}_{\mathrm{YX}}$ and $\mathbf{D}_{\mathrm{VU}}$ by ontological induction from the corresponding structural observation variables. Secondly, the desirability of these inferences is enhanced by the logical complexity of a second-level lawform assertion such as (16), especially when it is put to use in deriving predictions about particulars. It is much more convenient to replace (16) with the corresponding first-level hypothesis about the covariation between $\mathbf{D}_{\mathrm{YX}}$ and $\mathbf{D}_{\mathrm{VU}}$, namely,

$$(s)(\phi)[K'(s) \supset [\mathbf{D}_{\mathrm{YX}}(s, \phi) \supset \mathbf{D}_{\mathrm{VU}}(s, F\phi)]], \tag{17}$$

where K' is some characteristic which distinguishes members of classes which fill requirements K. Finally, through finding a natural regularity (i.e., (17)) which involves $\mathbf{D}_{\mathrm{YX}}$ and $\mathbf{D}_{\mathrm{VU}}$ in a way that goes beyond their initial *ad hoc* introduction via ontological induction, there is reassurance that these hypothetical entities are not misguided illusions, but do, in fact, exist. In such fashion, observed structural covariations support the objective reality of theoretical variables and lay down the "triangulating" nomological connections necessary to make the theory a cohesive whole.

Moreover, the conditions K under which a structural covariation such as (16) is found to hold may again be only one of many alternatives which differ widely among themselves in the particular relation they sustain between $\mathbf{R_{YX}}$ and $\mathbf{R_{VU}}$. Then it is a distinguishing feature of the class of classes $\hat{K}$ that the structural variable $\mathbf{R_{VU}}$ is related therein to the structural variable $\mathbf{R_{YX}}$ by the function F, and we are on our way to another datum-sentence of still higher type, namely,

$$\mathbf{R}_{\mathbf{R_{VU}}\mathbf{R_{YX}}}(\hat{K}, F), \tag{18}$$

which attributes the value F of a third-level structural variable, $\mathbf{R}_{\mathbf{R_{VU}}\mathbf{R_{YX}}}$ to $\hat{K}$. Then, by essentially the same inductive process as described in (15a, b), $\mathbf{R}_{\mathbf{R_{VU}}\mathbf{R_{YX}}}$ authorizes introduction of still another theoretical variable which eventually sinks two levels to become responsible for the relationship between the more peripheral theoretical variables $\mathbf{D}_{\mathrm{VU}}$ and $\mathbf{D}_{\mathrm{YX}}$ in the same way that the latter are attributed responsibility for the empirical **U-V** and **X-Y** relations. Or rather, this additional theoretical variable *could* be inspired by this third-level structural variable. Actually, since the theoretical assertion (17) would have already substituted for the more unwieldly (16), the structural theory-language variable $\mathbf{R}_{\mathbf{D}_{\mathrm{VU}}\mathbf{D}_{\mathrm{YX}}}$ rather than $\mathbf{R}_{\mathbf{R_{VU}}\mathbf{R_{YX}}}$ would be the antecedent for the ontological induction to $\mathbf{D}_{\mathrm{D_{VU}D_{YX}}}$. Further, remaining strictly in the data language, the restricted structural covariations in which $\mathbf{R}_{\mathbf{R_{VU}}\mathbf{R_{YX}}}$ and other third-level variables participate will define fourth-level structural variables which then deposit still another layer of theoretical variables, and so on.

If the apparatus of typological stratification and sedimentation pictured here appears horribly involved, it is no more (and in fact considerably less) than what actually occurs in scientific research and theory. I do not mean to imply that all or even many of these higher-level variables actually receive explicit recognition in the language of a science, for its theoretical development precludes the necessity of this. What I do want to stress is that a great many theoretical concepts and the postulates which connect them are so intimately tied to higher-level observation variables and their relations that if the theoretical structure were to be abandoned in favor of a radical empiricism, then it *would* be necessary to make use of this forbiddingly elaborate typological machinery in order to express what has been discovered empirically. This conclusion is a highly significant one on at least two counts. For one, it appreciably clarifies the way in which certain theoretical assumptions, or components thereof, seem *warranted* to tough-minded specialists in that area, while others may be shrugged aside as gratuitous and irresponsible. What the present analysis brings out is the apparent necessity for an irreducible minimum of theoretical structure in order to formulate the more complex empirical findings of a science in manageable propositions, while proposals which go beyond this central core are speculative in a way that the latter is not. (That ontological induction as described here is the *only* inferential process which contributes to this hard core of theory, by the way, is definitely *not* one of the present contentions. Neither is it here concluded that more tenuous conjectures have no place in science. Imaginative extrapolations and even wild guesses, no matter how seemingly arbitrary, may lead to valuable discoveries so long as they are entertained as stimuli to novel explorations and not confused with those elements of theory which the documented empirical findings of the science *support.*) Moreover, it is now possible to see how what appears to be a highly recondite dispute over theoretical components deep in the entrails of the theory may actually be susceptible to a direct experimental resolution in terms of whether or not certain higher-level empirical regularities do, in fact, obtain. This helps to explain the somewhat enigmatic observation that although according to the Hypothetico-deductive view a theory is supported or disconfirmed in its entirety, practicing scientists frequently show surprising concordance in their application of certain empirical findings to a circumscribed region in the theoretical network. It also exposes the methodological naivete of those radical empiricists who, in arguing that theories are parasitical superfluities which accomplish nothing that cannot be handled better without leaving the data language, actually cut themselves off from appreciation of empirical regularities which are too complex to be handled strictly in observation-language sentences.

Perhaps no better illustration of these points can be found than in the behavioral phenomenon of stimulus generalization, described above, and the unobserved processes controversially hypothesized to mediate between the stimulus input and response output of a conditioned habit. (Indeed, it was search for an experimental resolution to the long standing question of "What is learned?" that first led me to awareness of structural variables and ontological induction.) The manner in which one habit, S_1-R, depends upon another, S_2-R, itself varies from one organism to another, and from time to time even in the same organism. Hence the degree of generalization from S_2-R to S_1-R, if described strictly in terms of what is actually observed, is a third-level structural variable. Moreover, while hard data are meager, there is good reason to suspect (see Rozeboom, 1958b) that the tendency of S_2-R to generalize to S_1-R is a function of the extent that occurrence of S_2 has been contingent upon the occurrence of S_1 in the organism's past experience. Finally, it is also likely that the *extent* to which the past contingency of S_2 on S_1 influences the degree of generalization from S_2-R to S_1-R is itself a function of currently unknown but experimentally identifiable factors—thus introducing a fourth-level variable and inviting search for fourth-level laws. Obviously, this towering hierarchy of observation variables and the strictly empirical relations in which they participate is conceptually unworkable; only the corresponding network of theoretical assumptions can mold these data (more accurately, these prospective data) into a usable form. But the theory which so emerges is a theory of the internal processes mediating between the external stimulus and overt response of a conditioned habit, a subject which has been a high-pressure source of unresolved controversy for well over a generation, and one which some experimental behaviorists have derided as being empty of empirical significance. What has happened is that behavior theory has bogged down in a wrangle over the admissibility of idea-like "expectancies" between conditioned stimuli and learned responses, not realizing that the manifest bone of contention between "S-R" and "Cognitive" theorists, namely, whether behavioral principles can be conceived wholly in terms of peripheral stimuli and responses or whether hypotheses about central brain (or mental) processes are also necessary, actually has little relevance for a theory of *behavior*. What has really been at stake is involvement in the conditioned habit of a mediating element—which can be conceived *either* in S-R or Cognitive terms—which maintains a linkage with the original unconditioned stimulus. For it is such a theoretical mediation variable, with no commitment as to its physiological nature, that follows with inductive immediacy from the fourth-level structural variable abstracted from empirical generalization phenomena in the way described above; and discovery of the observable regularities in which this latter variable participates will provide immediate, controlled access to an unobservable central process which has seemed to be little more than a dogma of transempirical faith.

8. Recapitulation and Overview.

The focal contention of this article has been that a *scientific* theory (i.e., a set of beliefs about the unobserved which can legitimately be construed as a claim to *knowledge*), far from being either a flight of empirically useless fancy or a bold intuition about the unknown, constrained by known data only to the extent of avoiding unsightly clashes with the latter, is actually built around a virtually indispensable framework of theoretical suppositions forced upon the science by the data themselves. In support, it has been shown that empirical regularities, when interpreted simply as data, define "structural" observation variables on higher levels of the Russellian type-hierarchy, out of which lower-level theoretical variables are distilled by an inferential process so immediate, insistent and unreasoned (but *not* irrational) that it deserves to be recognized as a second kind of primary induction, *ontological* in contrast with *statistical*. Moreover, the higher-level empirical relationships in which these structural variables participate confirm the reality of and enrich our knowledge about these inferred entities by weaving them into a corresponding theoretical network. To be sure, ontological induction, at least to the extent it has been sketched here, is not the only determinant of this hard core of theory. Structural properties may not be the only observables to deposit theoretical constructs, and in any case, once born, a theoretical notion is immediately subject to transfiguration under the stress of felicity of expression, continuity, analogy, and other forces which may bear upon an evolving concept. But what should now be clear is that there need not be any methodological *mystery* about the genesis and maturation of scientific theories. While the psychology of imagination may still have something to contribute to our understanding of these matters, especially in regard to perception of patterns in unruly raw data and subcognitive inspirations to unorthodox research tactics eventuating in important new discoveries, it becomes more and more plausible to me that a formal rubric can be spelled out for the derivation of *justified* theory from a body of empirical data. That the logical form of this derivation would not be *deduction* does not, as Popper (1959, p. 30 ff.) has contended, make such a rubric part of a somehow philosophically illicit "psychology of knowledge" as opposed to the (ah, virtue restored!) "logic of knowledge." The logical relations in which propositions can stand to one another are in no ways limited to entailment and contradiction, while the actual transmission of belief along an inferential sequence is as much a psychological process when the inference is deductive as when it is inductive. (Some remarks about the "justifying" of inferences would be germane here if space permitted, but it must suffice to point out that to the extent "justification" involves *more* than observing the logical relations among components of the inference, it also goes beyond a mere "logic of knowledge," and can take this additional step for induction as well as for deduction.) Whatever the inadequacies of the present arguments (and I am only too aware of how much they leave undone), I submit that the ideas marshalled therein are sufficiently suggestive to warrant a more detailed unfolding and scrutiny.

Behind the present thesis about theory-origins, however, lies a more encompassing and urgent intent, namely, the detection, clarification, and refinement of basic formal methodology at the working edge of science. If contemporary research scientists tend, as many unfortunately do, to regard philosophical dicta with indifference and scorn, this is due at least in part to the failure of philosophy to give the experimentalist any appreciable technical assistance with the conceptual tools and tactics that he actually uses. Philosophy of science has consisted primarily in philosophizing *about* science, not *for* science, remaining by and large content to explain scientific methods (schematically portrayed) to outsiders and to tidy up an occasional pile of philosophical debris. Yet despite the vaunted precision of scientific language (and to be sure, it is a marked improvement over everyday discourse), it is still tormentingly vague; and the festerings of gratuitous assumptions and masked ignorance, sheltered from the antisepsis of critical judgment within the folds of innumerable ellipses, contractions, ambiguities, grammatical inconsistencies, and the like, exude conceptual poisons which stultify effective thinking on many important scientific issues. There are massive quantities of heavy-duty analytical labor to be done in scientific methodology, work that cannot be accomplished by appending exegetical footnotes to the scientific achievements of past eras, but only by grubbing down into the litter and confusion of problems now in progress to show by precedent and by principle how they can be dealt with more resourcefully. The methodological concepts outlined here—the nature of scientific variables, the logical form of laws, structural variables and their vital role in theory-building—have been forged under the impact of demands of this sort, and have afforded at least one behavioral scientist a certain amount of insight into some of the more baffling aspects of his trade. How much of the living methodology of science yet remains even to be surveyed, much less explored in depth, is barely hinted at by the loose ends dangling from nearly every paragraph of the present work. And if these views help provoke more serious attention to the wilderness between orthodox science and orthodox philosophy, then even if they prove ultimately to be misguided in substance they will have served their purpose.

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