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# MENTALITY AND THE DEEPER LOGIC OF LAWFULNESS: HAS FOLK PSYCHOLOGY A SCIENTIFIC FUTURE?

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PREFACE

This essay was born as an invited paper on the future of theoretical psychology. I had hoped to point out how psychonomic science's prevailingly slovenly concept management thwarts our prospects for genuine progress in popular areas of molar psychology, notably cognition, which may well prove refractory to scientific systemization no matter how astutely endeavored. But to develop this thesis I needed to make clear certain basic features of the logic of technical science--what with initial reluctance yielding to amused irony I have labeled "SLese" in acronymic shorthand for "Science's Language of Lawfulness"--that are not well understood even by scientists whose professional thinking is well-disciplined in these respects, much less by others who have never seriously played the game. And as I sought to articulate SLese's distinctive character, I discovered that I had finally hit upon a level of formalistic abstraction at which the manifold complexities of conceptual/methodological practices throughout the spectrum of effective sciences can be comprehended in an astonishingly cohesive view of how technical sciences work and why they are able to generate a mastery of nature so much more powerful than managed by ordinary language. I have done my best to share that insight here, with the result that this essay is no longer a critique of what psychology is doing wrong--I still speak to that, but not in the detail it deserves--but has become instead a treatise on the foundations of lawfulness in complex natural systems. Or rather, it sketches the framework of such a treatise. For repeatedly we are introduced here to advanced issues that are impractical to pursue on this occasion. (If you would like a specific example, probably most profound is the problem of molar causality, as brought out forcefully in the problematic causal status of laws derived by Input Abstraction--see p. 86f.)

Unhappily, what has emerged here is tortuous to read, not so much from any intrinsic difficulty, but because you will likely find its abstractions largely alien to your accustomed forms of thought and devoid of manifest content in whatever substantive matters are your personal specialties. So you will need exceptional motivation to study this with enough concentration and persistence for its formalistic novelties to become first meaningful and then richly significant for you. Lacking means to inject you with the excitement I find in this new perspicacity, I can only promise--for you to trust or doubt as prompted by your experience with salesmen, Big-Picture academics, and my own past writings--that these ideas can revolutionize or at least importantly strengthen your own work in almost any branch of empirical research, systems theory, or philosophy and methodology of science if you are serious about foundations. Especially to be urged are two groups of immediate applications:

First, in those sectors of science and metascience where advanced SLese is practiced with real achievement, we can now conceive of multivariate causal structure far less constrictively than explicit in traditional system models, thereby potentiating unpredictable advances in much the way that instrument innovations stimulate new developments. Above all, it is now feasible to undertake comprehensive theories of multivariate analysis and experimental design, whose enormous literature has to date said almost nothing about what we can hope to learn from the statistical parameters allegedly underlying sample data, and whether conventional patterns of data colligation adequately exhaust the forms that are most interpretively significant. (I have long projected a book on these matters that no longer needs be deterred by insufficiency of insight into the generic nature of data structure.)

Secondly, at the other extreme of technical sophistication, large expanses of professed concern for lawfulness in modern psychology and philosophy, notably cognitive psychology, philosophy of mind, and philosophy of science, are functionally illiterate in SLese and cannot even give thought to what they may have been missing without first acquiring a smattering of this. For example, ajudicating whether laws having any hard-science value can be expressed in the unSLesed informationprocessing jargon now pandemic in cognition theory (I think not, but urge that the question be debated) requires some understanding of what a properly SLesed model of cognition would be like. Again, the "computational theory of mind" now center-stage in philosophical psychology is so superficial a parody of mental mechanism that any functionalist account of mentality couched in these terms must remain largely vacuous in accomplishment no matter how laudable its intent. And it denigrates only the scope, not the quality, of extant philosophy on causality/lawfulness to point out how cripplingly empoverished its nomic-dependency formalizations have remained. How, for example, can its most advanced standard schematism $\underline{Pr}(\beta|\alpha) = \underline{r}$ (i.e., "the probability of $\beta$-ness given $\alpha$-hood is $\underline{r}$") do justice to the structure of such simple generalities as "A diploid organism is almost certain to be of the same species as its parents"? For illumination of this and deeper mysteries, read on.

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## PROLOG.

This essay addresses many issues foundational to the scientific study of mentality. But foremostly it is about the language of that inquiry. For it seeks above all to articulate the conceptual apparatus for practical management of complex detail in detection, description, and inductive extrapolation of system behavior that has given technical science the extraordinary epistemic power demonstrated in its more advanced developments. This machinery of scientific thought has been explicated only fragmentarily in the extant literature even where its applications have been most successful, and remains comprehended scarcely at all in many intellectual quarters that aspire to scientific achievement or profess concern for products and methods of scientific inquiry. In this regard I single out cognitive psychology, the philosophy and methodology of causality/lawfulness, and the philosophy of mind for special citation; for it is specifically to advance my work in these particular areas that I have found it essential to get clear on how the causal/compositional structure of a complex natural system can be captured in a language that is instrumentally effective even if perforce formalistic.

You will not find the ensuing document to be a pleasant read. Study of engineering manuals is always a grim scrabble for purchase on initially alien ways of thinking/doing; and what I am asking you to work through here is a treatise on concept engineering in technical science. Let there be no mistake about this: The abstract claims I shall be making about the logical character of law-statements are intended not as sociology-of-knowledge overviews of locutionary styles currently favored in science professions, but as design schematics for crafting the finer details that we--and that includes you as well as myself--must put into any psychonomic theories of mentation or philosophical theses about the nature of laws if our conjectures are to be worth taking seriously. If you have had experience with computers, think of this essay as roughly comparable to the text on FORTRAN or BASIC, at first baffling but eventually enlightening, that guided your first steps at programming. The parallel is imperfect; for I cannot here provide the hands-on practice that how-to-do-it training requires, and moreover what I am trying to lay out is not so much operational specifics on talking good science as it is a metatheory of the forms essential to this, together with some explanation of why they are so important, which you should find instructive even if you are already experienced in these techniques. Nevertheless, the intended payoff of this metatheory is in the applications for which it prepares, notably, coming to grips with core issues in cognitive science and the philosophy of causality/lawfulness that current discussion of these matters scarcely touch.

<u>SLese</u>: <u>The medium of scientific thought</u>.

It is widely recognized that modern sciences often achieve remarkable power in dealing with the world around us. This strength has multiple sources, prominent among which are evolved techniques for systematized observations from which tough-minded intuctive reasoning can extract information about the causal mechanisms that produce everyday events. But the fulcrum on which the successful sciences lever methodology into achievement is a special way of thinking about natural phenomena that cuts across all the varied content areas of natural science and is embodied in certain technical language constructions that everyday English foreshadows only crudely if at all. This special language is centered upon <u>conceptualization of lawfulness</u>, and in the more advanced quantitative sciences draws its force from a formal complexity far beyond the simplistic 'All ... are ...' and

'If anything is ... then it is ...' models of generality popular in philosophers' accounts of science from afar.

Despite its proven prowess, however, the logical character of Science's Language of Lawfulness--for brevity call this "SLese"--has not become clear even to its serious practicioners much less to hangers-on. Expertise in the use of specialized concepts no more insures awareness of how these work than proficiency in one's mother-tongue requires recognition of its rules of communication; and written SLese generally verbalizes its views on lawfulness only by mathematical equations that elliptically conceal almost all the propositional structure of the ideas they express. As a result, these ideas are often poorly understood, especially in softer sciences that are disposed to voice the summary slogans of SLese without saying anything in it for real. Although it is far from certain that all topic-worthy inquiries can be effectively conducted in SLese, this is still the only epistemically profitable game in town. In the behavioral sciences, our far-too-frequent misfit between substantive research and the elementary SLese taught as statistics, research design, and multivariate modelling in graduate methodology courses is manifest reason for seeking a better grasp of SLese's preconditions of application, what it can achieve for a particular content area once in place, and what may be its limitations.

Much of what follows here is an explicit detailing of SLese's most essential conceptual properties, not merely such aspects as are already familiar in the orthodox literature on experimental methodology and abstract systems theory, but more importantly its features whose near-total suppression by conventional ellipses has blocked access to deep insights that show forth in articulate SLese about the nature of causal regularity, the logic of "structure," and how the behavior of complex macro-systems emerges from assemblages of micro-phenomena. The latter are Big Issues for both the philosophy and substantive practice of science, on which enormous quantities of pretentious vacuities have appeared and which are hence apt to be dismissed as idle word-play by researchers whose vision is bounded by current thinking in their local specialities. The trouble with Big Issues, however, is not that they are unimportant--quite the opposite is evident to anyone who can face them without flinching--but that they are so conceptually elusive. That is no longer true of System Structure.

Explicit awareness of SLese's full power can be had only at the cost of some patience and effort to master certain technical formalisms needed to verbalize generalities about SLese's finer propositional grammar. But these formalisms employ only the most elementary symbolic logic, set theory, and standard notation for function-composition. In these terms, one can perceive an astonishingly simple unity throughout the entirety of what successful sciences say, from the methodology of data analysis, through levels of causal and acausal explanation both for particular events and for laws themselves, to accounts of how things are constituted. In short, revealed here is the conceptual apparatus latent in SLese that enables us to <u>see science whole</u>.

# PART I. THE STRUCTURE OF LAWFULNESS IN NATURAL SYSTEMS

## CHAPTER 1. THE CONCEPTUAL CHARACTER OF NATURAL SCIENCE.

Modern psychology is a natural science. Or rather, a good fragment of the broadly diverse activities that count as professional psychology today consists of endeavors to create products that merit this label. To declaim this grandly seems pretentiously trivial; yet it has a point. For after setting out a model of natural science that is more or less the technical ideal in modern practice, I want to consider what it would be for cognitive psychology to take this ideal seriously. My statement of this ideal admittedly slights many important complexities of the reality it schematizes. But boldly simplified guidelines are precisely what a good norm is supposed to provide.

My intent in Chapter 1 is threefold. First, I want to lay down as base that the primary intended content of any natural science comprises certain subject/predicate truths and principles that govern them, very much as reflected by the classic "covering law" model of scientific explanation. But secondly I shall also try to sketch <u>why</u> a science's intended content has this coarse formal character. These initial considerations are largely "philosophical"--an off-putting epithet that belies the practicality of what is at issue--in that their foreground concerns and modest technicalities arise from a style of thinking in which only readers passingly acquainted with modern analytic philosophy are likely to be fluent. But philosophical fluency is not required to catch the gist here. From there, we proceed to unfold how the philosopher's simplistic paradigm of natural principles is transformed through use of certain elementary formalisms of modern algebra into the elegantly powerful <u>functional</u> conception of lawfulness that is the backbone of SLese. More advanced integrations of regularity made possible by SLese regimentation, notably, recursive processes and macro-systems, are subsequently overviewed in Chapter 3 following examination in Chapter 2 of how SLese's applicability to particular substantive areas of research can be had only at a price of painstaking conceptual preparations that in molar psychology we seldom seem willing or able to carry through.

Because Chapter 1's development is meticulously abstract, the few examples given here may not suffice to show you how these formalisms catch hold of the issues most importantly familiar to you in your own substantive, methodological, or philosophical work on lawfulness. If so, you may find it helpful to browse ahead in Chapter 2. I cannot promise that you will quickly appreciate, much less accept, everything I say there; but it will give you a taste of the applications to which these abstractions are directed.

_Ideal science_: _Some preliminary heurisms_.

According to the linguistic sensitivities of most dictionaries, a "science" is _a systematized body of knowledge_. Much in this easy aphorism warrants expansive approbation. First, it acknowledges 'science' to be a count-noun under which we are to distinguish a diversity of particular sciences. Secondly, it takes each of these to be a distinctive cognitive content of some intricacy. Moreover, this content is to be belief-worthy and so must consist of propositions, i.e., what declarative sentences assert. So our first step of idealization is

_Heurism 1_. Ideally, a science is a corpus of declarative sentences.

We do not, however, want to honor a set $\Sigma$ of sentences with the title 'science' unless $\Sigma$ is severely constrained in epistemically important ways. But constrained how? That depends on how boldly we want our model to highlight unattainable perfections, commencing with the extremes envisioned in the classical philosophic analysis of propositional knowledge as _justified true belief_ (see e.g. Chisholm & Swartz, 1973). To draw out the force of this notion, we recognize first of all that justified belief requires _believers_, which is to say that a sentence corpus $\Sigma$ can be a science only relative to some population of sentient individuals (most saliently *us-now*) for whom these sentences have some elite cognitive status. Let us call these individuals an "epistemic community" for the corpus Σ while appreciating that its members are in general a narrowly select subset of a much larger society that includes many epistemic communities--often overlapping but rarely identical--for a broad diversity of specific sciences. (The latter are often colligated into administrative clusters familiar as academic departments such as Psychology, Physics, Biology, etc. in which a given specific science may have greater affinities outside of its cluster than within.) In our idealized model of science, we presume that an epistemic community *EC* for science Σ has a common language of which Σ comprises certain declarative sentences whose status as "scientific" for *EC* derives from the epistemic character of the propositions expressed by these sentences for members of *EC*.

*Note*. In this essay, I shall take "sentences"--by which I henceforth mean only declarative sentences--and other linguistic expressions to be not just overt symbol patterns but signs-plus-meaning in some particular language-in-use. And I shall allow myself a modest disregard for technical niceties in the use of semantic quotation marks, both in using these where Quinian corners more properly belong and in sometimes eliding them altogether when our concern is primarily for the concept expressed by certain words. Although equivocation between use and mention of symbols can wreak disaster upon a philosophic enterprise, this is often a harmless convenience that I promise not to abuse here.

That "science" is fundamentally a relation between a specific corpus of sentences and a particular spatio-temporally localized community of cognizers reflects a practical reality far more profound than just that H-1's mention of "sentences" makes implicit reference to a particular language of which these are sentences in use. For as professional science has developed as a social institution, scientific knowledge is public, consensual knowledge. It is public in that a science's constitutive sentences are registered in archives accessible to all members of its epistemic community EC and in which the recorded tokens of these sentences are tagged with EC's imprimature for approved material of this kind. And it is consensual in that EC establishes its imprimature as license for confident belief in the sentences so demarked. Very roughly, the way this works is that EC so trains its members in specialized proficiencies of observation, inference, and communication that a sentence S is admitted into the EC-archived corpus of Σ only when the particular individuals responsible for its entry have reliably high confidence in S's truth. Thereafter, perception of S's archival record is taken by other members of EC to be strong evidence that S has been correctly appraised as true and is hence beliefworthy *simpliciter*.

In practice, the situation is of course much more complex than this. The archives are real enough, but only a portion of them--books and journals--are widely accessible. Raw-data stores are prevailingly ephemeral collections of cryptic abbreviations in private files. On the other hand, many of the sentences published in EC's archives for science Σ--e.g., criticisms of other work, recommendations for future research, efficacy appraisals of certain methodologies, etc.--are not really part of the Σ-corpus even though they have relevance for it. As a result, the scope of Σ for EC is seldom demarked by any clear consensual criterion for this. Further, the sentences actually published in EC's literature on Σ vary widely in credibility, ranging from decent approximations to ideal certainty in summaries of experimental results, through probability-parameter estimates to which graded confidence ratings are assigned under the egis of one statistical theory or another, to loosely conceived speculations of acknowledged problematicity. Indeed, it is precisely concern for possibilities whose truth-status is provocatively unresolved that makes the practice of science a directed activity. Even so, it is not unfair to say that what defines a particular science for a given EC is some more or less vague conception of an intended content Σ comprising sentences for which EC takes epistemic responsibility, and that for each sentence S in Σ, other things equal, the more justifiably certain EC is about S's truth or falsity, the closer to perfection science Σ is for EC. Thus in simplistic summary we have

> Heurism 2. Ideally, a perfected science Σ for epistemic community EC is a corpus of true sentences enduringly recorded in a public medium accessible to all members of EC and distinguishable there by cues which members of EC rationally believe to be impeccable indicators that sentences so marked are true. A working science for EC is a corpus Σ of sentences (or, alternatively, a criterion for sentence selection that picks out a set Σ) such that (a) for each sentence S in Σ, the negation of S is also in Σ, and (b) the members of EC are trying to bring it about that the set of all true sentences in Σ is a perfected science for EC.

Further refinements of H-2 would set out more detail on EC's diversified engineering and transmission of Σ-sentence credibilities, and would also acknowledge the transience of a particular scientific corpus for a particular EC. But real life complexities in the socio-epistemology of science are not my present concern. Rather, having begun by emphasizing that a particular science is identified foremostly by a specific propositional content, or at least by a rationale for judging which sentences fall within that science's purview for EC, I now want to consider what features of this intended content cash out the notion that sciences are systematized bodies of knowledge. For H-2 sketches only the sociological side of institutionalized science without distinguishing the composition of a scientific corpus from that of lesser public-information compendia such as telephone directories and stockholders' reports. And clearly any delimitation of "science" needs recognize also that the sentences crafted by a successful science have a cohesiveness/confluence/synergy fraught with import far beyond that of any random collection of vouchsafed facts.

Following the lexicographer's lead, we can hint at this extra ingredient by appending to H-2 a clause requiring any sentence corpus $\Sigma$ that counts as a science also to be suitably "systematized." Yet telephone directories and stockholders' reports, too, are systematic in ways appropriate to their usage; so this qualifier does not really tell us much until we explore what sorts of systemacy are foundational to hard-core science. And that will take patience and care; so I had best remind you of its point here. We shall not further refine H-2's discrimination of science from non-science, for partitioning continuua into categories goes beyond concept analysis into arbitration of jurisdictions. Our concern here is only (!) to explicate the conceptual machinery that generates technical science's extraordinary epistemic power. And since that consists throughout of contrivances to make nature's intricate orderliness humanly comprehensible, "scientific systemacy" is an appropriate label under which to inventory these devices. But what matters is their varied particulars, not their common subsumption under this abstract predication.

The details of scientific systemacy inhere in the fundamental endeavor of all natural sciences to explain and predict within their respective content domains. I hope you agree that the disciplines we call "science" have this aim, for it is a brute historical fact about them that I cannot easily document except by appeal to the prevalence of its acknowledgment by introductory science texts. But once this premise is granted, we can argue that the technical features of SLese to be reviewed here are not mere accidents of its etymology but are strongly motivated responses to task demands. Most immediately, any science is task-directed to search for systemacy in the form of "principles"; and probing the logic of that is where articulation of SLese begins.

*Scientific systemacy* 1. *Principles*.

The content of a typical natural science is built upon a core of sentences that the science seeks to *account for* in a broad sense that subsumes both prediction (i.e. forecast/diagnosis/retrodiction) and explanation. To "account for" a sentence *S* is more precisely to account for *S*'s truth value, and is what we purport to do by developing an argument of form

(1*a*) *A* and *L*; therefore, *B* ,

in which *A* and *L* are sentences (usually complex ones) that we take to be true, *B* is either *S* or a sentence that entails denial of *S*, and sentence-triple <*A*,*L*,*B*> is under additional constraints that will emerge as we proceed but whose fine details still remain importantly obscure even at the frontiers of thought on this matter. When our belief in *B* has been acquired independently of our believing *A* and *L* , perhaps even being a source of the latter, argument (1*a*) is an "explanation" of *B* so long as *A*-and-*L* has the right force for *B*; whereas if our belief in *B* derives from our beliefs in *A* and *L*, (1*a*) is a "prediction" of *B* that may or may not explain *B* as well. (The constraints on <*A*,*L*,*B*> intuitively needed for (1*a*) to explain are stronger than required for it to predict.) But either way, if (1*a*) is to be an "accounting for" *B*, part of its antecedent must be a *principle* (law, regularity, rule), *L*, under which antecedent remainder *A* is specifically relevant to *B*. To be sure, we often argue "*A*, therefore *B*" without making the inference's governing principle explicit, especially when *B* is a logical consequence of *A*. (Indeed, the logical form by which an argument channels assent from premises to conclusion is always distinct from any of its premises.) But for an argument to be rational, it must be governed by some principled relevance-bridge that we can describe and defend if the argument's merit is called into question; and we can take it to be an empirical fact that any explanation/prediction of the sort for which natural sciences take responsibility would have no force were not some assertable principle--most desirably, a "law of nature"--an implicit or explicit premise of the argument.

(I hesitate to illustrate explanatory/predictive arguments and their governing principles just yet, because examples tend to be prejudically narrow if kept simple or distractingly complex if made realistic. Even so, I had best remind you how importantly these abstractions figure in your personal cognitive life. When your observation this afternoon, that the sky overhead is roiling black with lightening-streaked clouds, convinces you that a severe rainstorm is about to break, your reasoning is essentially an argument of form (1a) in which A is your perception that the daytime sky here-and-now is roiling black [etc.], B is your anticipation that it will storm here shortly, and L is your generalized conviction that where-and-whenever in this climate the daytime sky is roiling black [etc.], it almost always storms there soon after. In practice, you are seldom conscious of drawing upon such generalities when interpreting your perceptions; but you are usually able to acknowledge them when challenged to clarify the relevance of your observations to your expectations, and if your confidence in such a principle becomes shaken, so does your reasoning falter in regard to the particulars it ties together for you. In the present example, the argument from menacing sky to forthcoming rain is evidently a prediction. Whether it is explanatory as well, i.e., whether it helps us to understand why it will rain, depends on subtleties in how we interpret the generality.)

When our confidence in the premises by which we aspire to account for a sentence B will not sustain a fully indicative argument of form (1a), we often resort instead to a subjunctive argument in hypothetical mood

(1b) If A and L, then B ,

or in mixed indicative/hypothetical mood

(1c) L; therefore, if A, then B .

<*A*,*L*,*B*> must still satisfy special constraints if these arguments are to count as hypothetical explanation/prediction of *B*, but judgment is now withheld about the truth of *A* and *L* in (1*b*), or of *A* in (1*c*). In these latter cases, the argument purports to establish not that *A* and *L* do in fact account for *B*, but that they *may* do so, dependent on their truth.

Henceforth I shall write '*explanation' and '*prediction' for subjunctive arguments that *would* qualify unconditionally as explanations or predictions were there to be no doubt about their premises. More generally, the asterisk prescript in '*datum', '*principle', etc. will signal withdrawal of truth presumptions from terms that we normally understand to include veridicality as a condition on their proper application.

To say that *L* in (1*a*,*b*,*c*) is a "*principle" under which *A* is relevant to *B* is to imply that *L* has explanatory/predictive generality under which the relevance coupling it establishes on <*A*,*B*> is likewise imparted by *L* in the same conceptual fashion to all sentence-pairs in an open class of which <*A*,*B*> is just one instance. For this to be so, the force of *L* in (1) must be essentially indifferent to the specific meaning of certain words or phrases in <*A*,*B*> so long as these satisfy some *L*-specific criterion that includes whatever linguistic properties are needed to make *A* and *B* grammatically well-formed sentences. Then the class of sentence pairs relevance-coupled by *L* is generated by all the different permissible substitutions for the terms in (1) to which *L* is indifferent. Without evident loss of generality, we may presume that these replaceable terms are names (or "nominals" if you prefer to understand 'name' in its narrow sense that excludes descriptions), i.e., words or phrases whose grammatical role is to refer.[1] And we can also contrive that *L*'s admissibility criterion

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[1] Principles that *guide* arguments do not necessarily generalize just over names. For example, when we infer 'Either it's raining or it's snowing' from 'It's snowing' under the valid inference form

*R*: *p*; therefore, *p*-or-*q* ,

'p' and 'q' in R are placeholders for sentences, not for names. But R is not an asserted premise in this argument; indeed, this schema is neither a grammatically well-formed sentence nor can be made into one by treating its placeholders as bound by universal quantifiers. On the other hand, if we convert schema R into

S: For all sentences P and Q, if P is true then P-or-Q is true ,

we can infer 'The sentence 'Either it's raining or it's snowing' is true' jointly from S and 'The sentence 'It's snowing' is true'. Unlike R, S is a grammatically respectable statement of principle. But observe that R achieves its assertability by generalizing over names, specifically names of sentences. The difference between R and S illustrates why it is doubtful whether principles that do not generalize just over nominals can ever be asserted as premises of an argument.

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for the terms over which it generalizes has been made conceptually explicit in (1) by expanding A to include a suitable predication on these names. To clarify, let us use standard logicians' notation '$\alpha(a_1,\ldots,a_m)$' (similarly '$\beta(b_1,\ldots,b_n)$', '$\gamma(c_1,\ldots,c_r)$', etc.) to denote the sentence formed by inserting any m-tuple $\langle a_1,\ldots,a_m\rangle$ of names ($m \geq 1$) into any m-place predicate $\alpha(\_,\ldots,\_)$. (We shall usually abbreviate $\alpha(\_,\ldots,\_)$ simply as $\alpha$.) For example, if $\alpha$ is the predicate (i.e. sentence schema) '__ is taller than __' while $\langle a,b\rangle$ is the pair of names <'John','Mary'>, $\alpha(a,b)$ is the completed sentence 'John is taller than Mary'. Whenever the polyadicity (i.e., number of distinct name-places) of predicate $\alpha$ is not specifically at issue, we may condense this notation to $\alpha(a)$, etc., with the understanding that a is a name tuple containing just as many terms as there are gaps to receive them in $\alpha(\_,\ldots,\_)$. Then L-principled argument schema (1b) can be written more articulately as

(2) Given $\gamma(a,b)$, if $\alpha(a)$ and L, then $\beta(b)$ ( Replaceable: a,b ) ,

wherein $\langle a,b\rangle$, i.e. $\langle a_1,\ldots,a_m,b_1,\ldots,b_n\rangle$, comprises the names (nominals) to whose specific meaning L is indifferent, and $\gamma(a,b)$ stipulates satisfaction of the preconditions for that indifference.[2]

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[2]For the technical philosophy of explanation, it is also important to appreciate that the 'if/then' in (2) is the dialectic conditional of hypothetical argument (cf. Rozeboom, 1973 p. 61), not the connective in a conditional assertion akin to '$p \supset q$'. This is why (2) is not entirely equivalent to statement-form

(2*) For all things $\langle x,y\rangle$ such that $\gamma(x,y)$, if $\alpha(x)$ and L, then $\beta(y)$ .

For (2) schematizes an inferential <u>argument</u> whose support for its conclusion given its premises needs not be conclusive (more on that below), whereas (2*) aspires to be an assertable <u>sentence</u> exploiting a conditional connective whose nature is not merely unclear but may not exist at all with a force that closely parallels that of the dialectic conditional in (2).

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If you are not fully comfortable with symbolic-logic notation, you can read (2) in ordinary English as

(2') Given that $\underline{a}_1,\ldots,\underline{a}_m,\underline{b}_1,\ldots,\underline{b}_n$ is a collection of things satisfying certain background requirements $\gamma$, if $\alpha$ is a condition or complex of conditions that holds for sub-collection $\underline{a}_1,\ldots,\underline{a}_m$, and $\underline{L}$ (a law/principle/regularity/generality applying to all kind-$\gamma$ collections) is also true, then sub-collection $\underline{b}_1,\ldots,\underline{b}_n$ has feature $\beta$.

However, formula (2) has a delicate precision which is damaged by the turgidity of (2'). My thesis here (for you to discredit by finding counterexamples if you disagree) is that whenever in real life we produce a plausible predictive/explanatory argument that makes overt or covert appeal to some assertable "principle," both the premises and conclusion of this argument contain name-like expressions whose replacement by other names, given certain substitutional constraints which can be verbalized in an auxillary premise, yields a new argument having the same logical form, the same grounding principle, and the same inferential force as the original. I have said nothing as yet about what a statement $\underline{L}$ must be like in order to serve as an argument's principle; the essential point right now is simply that purporting to explain sentence $\underline{B}$ by sentence $\underline{A}$ under some asserted or conjectured principle $\underline{L}$ <u>imposes a subject/predicate parsing on the grammar of A and B</u>. If that seems trivially true to you (which it is not), fine--you needn't worry further about these last few paragraphs. But recognizing one way or another the subject/predicate form of sentences linked by the epistemic relation of explanation/prediction is a crucial first step toward articulating the conceptual character of technical science. Formula (2) deftly captures this parsing while leaving completely open--as the ontic-category terms in (2') do not--what sorts of entities are referred to by nominals $\underline{a}$ and $\underline{b}$, and what is said about them by predicates $\gamma(\_,\_)$, $\alpha(\_)$, and $\beta(\_)$.

When sentences $\underline{A}$ and $\underline{B}$ in explanation/prediction (1$\underline{a}$,$\underline{b}$,$\underline{c}$) are parsed by their relevance-coupler $\underline{L}$ as having respective subject/predicate compositions $\alpha(\underline{a})$ and $\beta(\underline{b})$, it by no means follows that the terms in $\underline{a}$ and $\underline{b}$ are all distinct. Quite the contrary, usually the main restriction on what name replacements are acceptable in (1), the precondition whose satisfaction is provided for by (2)'s auxillary premise $\gamma(\underline{a},\underline{b})$, is for the $\underline{b}$-terms to have common reference with certain terms in $\underline{a}$. In such cases, generic form (2) can be specialized to put these identity constraints into the argument's logical form rather than asserting them in the argument's antecedent. For example, the simplest and most familiar restriction on the replaceable names in (2) is that $\underline{a}$ and $\underline{b}$ must name the same thing. In that case, (2) becomes

(2.1) Given $\underline{a} = \underline{b}$, if $\alpha(\underline{a})$ and $\underline{L}$, then $\beta(\underline{b})$ ( Replaceable: $\underline{a}$,$\underline{b}$ ) ,

which is equivalent simply to

(2.2) If $\alpha(\underline{a})$ and $\underline{L}$, then $\beta(\underline{a})$ ( Replaceable: $\underline{a}$ ) .

(Conversely, the move from (2.2) to (2.1) illustrates how $\underline{L}$-acceptability restrictions on the subject terms in $\alpha(\underline{a})$ and $\beta(\underline{b})$ can be imposed by auxillary premise $\gamma(\underline{a},\underline{b})$.) More generally, however, precondition $\gamma(\underline{a},\underline{b})$ in (2) may require only a subtuple of the names in $\underline{a}$ ($= \langle \underline{a}_1,\ldots,\underline{a}_m \rangle$) to be referentially identical with $\underline{b}$-names.

To say that $\underline{L}$ is indifferent to the specifics of $\langle \underline{a},\underline{b} \rangle$ in (2) implies that no part of $\langle \underline{a},\underline{b} \rangle$ is conceptually related to any part of $\underline{L}$ in a fashion relevant to the argument but not made explicit by $\gamma(\underline{a},\underline{b})$. But that is just ancillary to the main conditions which $\underline{L}$ must satisfy before (2) counts as explanatory or predictive. And what are these requirements? No one really knows, for the concepts needed either to express or to fulfill them are still distressingly immature. The best I can do for them here is to hint at the role we want $\underline{L}$ to play in our epistemic economy, briefly note aspirants to that role cultivated by current practices, and voice pious trust that the latter's present obscurities will diminish as our understanding of natural necessity continues to deepen.

One apparent requirement on L, manifest in (2)'s status as argument, is that belief in $\beta(b)$ be justified by belief in $\gamma(a,b) \& \alpha(a) \& L$ when belief in $\gamma(a,b) \& \alpha(a)$ alone does not suffice for this. Were it not for one major complication, we could put this by saying that $\beta(b)$ is to be logically entailed by $\gamma(a,b) \& \alpha(a) \& L$ but not by $\gamma(a,b) \& \alpha(a)$. In practice, however, candidates for L-status that we actually manage to verbalize seldom warrant total confidence in $\beta(b)$ given $\gamma(a,b) \& \alpha(a) \& L$. So we want L first of all to have a conceptual force under which, for all name tuples $\langle a,b \rangle$ satisfying $\gamma$, $\beta(b)$ is rationally plausible given $\gamma(a,b) \& \alpha(a) \& L$ to a degree that can fall considerably short of certainty.

Secondly, in order for L to sustain *predictions of form (2), L must be of such epistemic character that we can attain arbitrarily high rational confidence in $\gamma(a,b) \& \alpha(a) \& L$ without deriving this in part from the strength of our belief in $\beta(b)$. For this reason, the "accidental" generality

$G_1$: For all things $\langle x,y \rangle$ such that $\gamma(x,y)$, either not-$\alpha(x)$ or $\beta(y)$ ,

or more broadly

$G_r$: $\beta(y)$ holds for $(100 \times r)\%$ of all things $\langle x,y \rangle$ such that $\gamma(x,y) \& \alpha(x)$ ,

wherein $r$ is a number in the closed unit interval and $G_r$ is equivalent to $G_1$ when $r = 1$, cannot play the L-role for prediction of $\beta(b)$. For although $\beta(b)$ is a logical consequence of $\gamma(a,b) \& \alpha(a) \& G_1$, any uncertainty one has about $\beta(b)$ at the start of an inference to $\beta(b)$ given $\gamma(a,b) \& \alpha(a)$ must rationally be mirrored by some corresponding uncertainty about $G_1$. That is, given $\gamma(a,b) \& \alpha(a)$, the plausibility of $G_1$ (and more generally of $G_r$) derives in part from that of $\beta(b)$ and hence does not allow $G_1$ to be rationally established prior to $\beta(b)$.

__Example__. Suppose that in your undercover surveillance work, you have previously noted several instances, and no violations, of the generality

$\underline{G}$*: All persons entering McGavin's Bar today are IRA supporters.

(To see $\underline{G}$* as an instance of $\underline{G}_r$, with $\underline{r} = 1$, take $\gamma(\underline{x},\underline{y})$ to be $\underline{x} = \underline{y}$. In a more refined reading, $\underline{G}$* would interpret $\gamma(\underline{x},\underline{y})$ to impose the condition that $\underline{x}$ and $\underline{y}$ are temporal stages of the same enduring person in some fixed lag relation, say $\underline{y}$ a little later than $\underline{x}$, whose details would be crucial to the truth of $\underline{G}$* if political attitudes are highly volatile.) If you understand $\underline{G}$* to claim nothing more than a local collection of coincidences, in the way you might wager in a crap game that no throw in the next series of dice passes will show boxcars, and you now observe Rabbi Cohen entering McGavin's Bar, you would take your doubts about Cohen's interest in Irish politics as strong evidence against $\underline{G}$*. But if you interpret $\underline{G}$* to assert some lawful connection between entry into McGavin's Bar today and IRA sympathy, due say to conspiracy, your confidence in $\underline{G}$* may have become very high on inductive grounds prior to Cohen's entry; and rather than your prior opinion about Cohen's politics discrediting $\underline{G}$* for you, $\underline{G}$* is now an $\underline{L}$ under which you can rationally infer from Cohen's entry that he too, surprisingly, is an IRA supporter.

These brief remarks do not adequately convey the complexity of inferential possibilities here, but they give the general idea.

The same inferential priority of $\underline{L}$ over $\beta(\underline{b})$ given $\gamma(\underline{a},\underline{b})\,\&\,\alpha(\underline{a})$ needed for *prediction is also required for $\gamma(\underline{a},\underline{b})\,\&\,\alpha(\underline{a})\,\&\,\underline{L}$ to *__explain__ $\beta(\underline{b})$. But in this case we want $\underline{L}$ to imply further--our third condition on $\underline{L}$--that $\beta(\underline{b})$ is __due to__ $\gamma(\underline{a},\underline{b})\,\&\,\alpha(\underline{a})$. (Thus in the example just given, even if we take $\underline{G}$* to be a non-accidental generality which sustains predictions, we would scoff at the suggestion that Cohen's entry into McGavin's Bar today __explains__ his inferred

IRA sympathy. Our background knowledge tells us that bar entries seldom _bring about_ same-day political preferences, albeit in cases of conspiracy the converse might well occur.) Just what it means to hold one state of affairs explanatorily responsible for another is a continuing enigma that needs detain us at this point only long enough to note (_i_) that it subsumes not merely _causal_ productions but also _becausal_ responsibilities under which, e.g., John's being tall is due most immediately to his being 78 inches in height (cf. Rozeboom 1968, p. 145), and (_ii_) that only when our behavior is linked to other events by production relations do we acquire any _control_ over our world. Commonsensically, (_ii_) is truistic; but it emphasizes that whatever explanation adds to mere prediction is not just a preciosity of disengaged intellect but an essential factor in rational action. Moreover, prediction too appears grounded upon explanation in that only when we think that $\alpha(\underline{a})$ and $\beta(\underline{b})$ have causal/becausal sources in common given $\gamma(\underline{a},\underline{b})$ does it make sense to believe any $\underline{L}$ under which $\beta(\underline{b})$ is predictable from $\gamma(\underline{a},\underline{b})\,\&\,\alpha(\underline{a})$. There is a profound intimacy between our presuppositions about the world's explanatory order and what we take to be rational inference that still remains a largely untold story. (However, see Rozeboom 1971.)

For satisfying the first two requirements on $\underline{L}$--imperfect strength and inferential priority over its instances--technical science nowadays favors an appeal to probabilities, namely, by construing $\underline{L}$ to assert or entail

(3) The probability of $\beta(\underline{y})$ conditional on $\gamma(\underline{x},\underline{y})\,\&\,\alpha(\underline{x})$ is $\underline{r}$

for some number $\underline{r}$ in the closed unit interval. Whatever objective probabilities may be, if they exist at all they are something that we can learn inductively with high confidence prior to our acquaintence with many particular events on which these bear, and can hence ground predictions about the latter. But orthodox conditional probabilities tell us nothing about what is due to what. (This is because as probabilities are traditionally axiomatized, $\underline{Pr}(\beta|\alpha\gamma)$ and $Pr(\alpha|\beta\gamma)$ have equal ontological status reflected by equation

$$\underline{Pr}(\alpha|\gamma) \times \underline{Pr}(\beta|\alpha\gamma) = \underline{Pr}(\alpha\beta|\gamma) = \underline{Pr}(\beta|\gamma) \times \underline{Pr}(\alpha|\beta\gamma) .$$

For a strong illustration of why conditional probabilities must be distinguished from production forces, see Humphreys, 1985.) So explanation requires more of $\underline{L}$ than just (3). Although we still have no good theory of what that something more may be, we can simply posit--as modern philosophers often do--that there exist *de re* responsibility couplings that justify our making if/then assertions with subjunctive or counterfactual force, and for which we can provide conceptually by introducing a connective '$\underset{r}{\twoheadrightarrow}$' in contexts primarily of form

(4) For all things $\langle \underline{x},\underline{y}\rangle$, $\gamma(\underline{x},\underline{y})$ & $\alpha(\underline{x}) \underset{r}{\twoheadrightarrow} \beta(\underline{y})$ ,

which is to be read heuristically as something like

(4') For any $\langle \underline{x},\underline{y}\rangle$, $\gamma(\underline{x},\underline{y})$ & $\alpha(\underline{x})$ would insure $\beta(\underline{y})$ with probability $\underline{r}$ .

(The latter is only heuristic for (4) because its use of "probability" conflates de re dependency with an urged strength of cognitive expectation.) For example,

(4.1) For any $\langle \underline{x}_1,\underline{x}_2,\underline{y}\rangle$, if $\underline{x}_1$ and $\underline{x}_2$ are $\underline{y}$'s parents, and $\underline{x}_1$ and $\underline{x}_2$ are both human, then almost certainly $\underline{y}$ is human,

(4.2) For any $\langle \underline{x},\underline{y}\rangle$, if $\underline{x}$ and $\underline{y}$ are temporal stages of the same chicken egg with $\underline{y}$ five seconds later than $\underline{x}$, then $\underline{x}$'s being dropped makes it rather likely that $\underline{y}$ is broken,

are commonsense generalities whose conditionality connections are understood to express defeasible production forces whose respective strengths, though imprecisely identified, are evidently different in the two cases.

Actually, we need not just one concept of de re dependency but a family of these that express different <u>kinds</u> of lawful coupling, notably causal bringings-about at various levels of molar abstraction but ranging from noncausal becausings at one extreme to powerless conditionalities such as expressed by (3) at another. The issue of molar causality will arise later. Meanwhile, I shall speak of all form-(4) generalities as <u>laws</u> (or *<u>laws</u> if truth is not presumed) in a generic sense that subsumes as many species (causal, becausal, etc.) as '$\xrightarrow[r]{}$' has different readings.[3] (However, form (4) is merely preliminary to the far more powerful

[3] Note that this takes a "law" to be a true sentence having a certain conceptual character. I would much prefer to say that laws are aspects of reality independent of language. But while it is easy enough to declare that laws de re are what gerundized law statements refer to, I choose on this occasion to shun responsibility for the ontology of such entities. Even so, it is infelicitous to suppress all talk of laws as something out there. Please forbear.

SLese conception of lawfulness toward which we are working our way.)

For simplicity we shall here accept the standard presumption that if '$\xrightarrow[r]{}$' expresses a suitably strong production coupling, statement (4) can itself serve as the $\underline{L}$ in an explanation of $\beta(\underline{b})$ by $\gamma(\underline{a},\underline{b})\,\&\,\alpha(\underline{a})\,\&\,\underline{L}$. Strictly speaking, however, a strong case can be made that (4) is only a symptomatic consequence of the $\underline{L}$

that most properly grounds the explanation. (See Dretske, 1977; Tooley, 1977.)

The problem of making sense out of (4) is greatly exacerbated by allowing parameter r therein to take values less than 1. Especially obtrusive is how small r can become while still allowing $\gamma(\underline{a},\underline{b})\,\&\,\alpha(\underline{a})\,\&\,\underline{L}$ to "explain" $\beta(\underline{b})$. (Since explanatory arguments need not be deductively conclusive, 'therefore' in (1) or 'if/then' in (2) does not require $\underline{r} = 1$.) Some philosophers of science, notably Salmon (1970, 1975), have argued that explanatory use of (4) places no constraint on r at all--a thesis which not only is hard to resist once $\underline{r} < 1$ is allowed, but also needs not remain intolerably counterintuitive if we concommitantly propose that explanations under form-(4) laws have a graded goodness which is an increasing function of r. (I.e., if explanation is not all-or-none, (4) is explanatory under any r but provides a better explanation when r is large than when it is small.) On the other hand, there are formal contrivances under which it is harmless to pretend that $\underline{r} = 1$ in all explanatory principles (see p. 39 below), and moreover deep reasons to suspect that this may well be how, at bottom, the world really is. So I will henceforth write simply '$\longrightarrow$' in form-(4) *laws with the understanding that if any question arises about their probabilistic strengths, we idealize this as $\underline{r} = 1$.

Basic ingredients of a scientific corpus.

If I am correct in my preceding claim--scarcely an original one--that natural-science corpora arise from an epistemic community's efforts to predict and explain, the grammar of *principles imposes a certain coarse structure on how the corpus of any science Σ is constituted. Essentially, science Σ begins with some selected set $\{\underline{B}_k\}$ of sentences for which its EC wishes to account. But to do so requires that each $\underline{B}_k$ therein be parsed as a subject/predicate sentence $\beta_k(\underline{b}_k)$ in which $\underline{b}_k$ is a name (or name-tuple) over which some *principle of Σ generalizes in accounting for $\underline{B}_k$. Meanwhile, any accounting for the truth-value of $\beta_k(\underline{b}_k)$ is also an accounting for that of $\beta'_k(\underline{b}_k)$ for one or more logical alternatives $\beta'_k$ to $\beta_k$. (For example, any explanation or prediction of the truth/falsity of 'John will become enraged when you tell him what happened' implicitly if not explicitly accounts also for the truth/falsity of 'John will remain placid when you tell him what happened'.) So Σ's initial interest in $\beta_k(\underline{b}_k)$ immediately spreads to many parallel sentences as well, notably others that instantiate this same predicate $\beta_k$ but also ones whose predicates are logically disjoint with $\beta_k$. (Cf. Garfinkle, 1981, on the contrastive facet of explanation.) Let us call the set of all these predicates disclosed by parsing Σ's initial targets $\{\underline{B}_k\}$ of explanation/prediction for subsumption under *principles, together with the ones that Σ takes to be their contrasts, the _primary predicates_ of science Σ. We can then say that if Σ is an idealized working science, its _primary *data_ comprise just the sentences $\{\beta_k(\underline{b}_j)\}$ in which $\beta_k$ is a primary predicate of Σ and $\underline{b}_j$ is any name-tuple (perhaps a 1-tuple) in Σ's language for which $\beta_k(\underline{b}_j)$ is neither meaningless nor absurd. (Absurdity is illustrated by taking 'honesty', 'butter', 'Pi', etc. for $\underline{b}_j$ when $\beta_k$ is '__ is fond of girls'.) And any Σ-*datum $\beta_k(\underline{b}_j)$ that Σ's epistemic community takes to be true is simply a _datum_ of Σ (for _EC_).

From there, the content of working science Σ builds recursively: Let $\gamma(\underline{a},\underline{b})$, $\alpha(\underline{a})$, and $\underline{L}$ be any sentences which account for some primary *datum $\beta(\underline{b})$ of Σ under argument-form (2). That does not suffice for admission of *principle $\underline{L}$ into the corpus of Σ, for $\underline{L}$ may seem too absurdly false or fancifully unconfirmable to hold even heuristic interest, or may be too trivially a consequence of more fundamental *principles under consideration in Σ, or may express an idea that has not yet occurred to anyone. But if Σ's epistemic community does take $\underline{L}$ seriously, then: (_a_) $\underline{L}$ is a _primary *principle_ of corpus Σ; (_b_) any sentence $\underline{S}$ that is a conjunctive component of $\gamma(\underline{a},\underline{b})\,\&\,\alpha(\underline{a})$ but is neither itself a conjunction of other sentences nor a primary *datum of Σ is a _secondary *datum_ of Σ; and (_c_) with certain technical exclusions that we are not yet positioned to state, the predicate derived from secondary *datum $\underline{S}$ by substituting place-holders for all names therein over

over which *principle L generalizes is a secondary predicate of Σ. (The exclusions have to do with formalities of translocation and abstraction aired later, as well as predicates such as '__ = __' that are purely logical.) We will say that Σ's basic predicates are those that are either primary or secondary for Σ, and that Σ's basic sentences consist of its primary and secondary *data. There is little epistemic significance in this primary/secondary distinction; it merely records the nearly trivial point that a science's scope of predicative concerns can seldom be confined just to its initial interests. But some ability to verbalize its basic predicates/sentences--not to itemize them all, but at least to provide honest paradigmatic examples--is requisite for an aspirant science to be well-mounted.

Finally, any primary *principle or secondary *datum of Σ, or molar abstractions from ensembles of these, that Σ's epistemic community also seeks to explain or predict add to the Σ-corpus an array of higher-level *principles and *data to account for these, and so on for a recursive hierarchy of Σ-concerns whose detailed structure has no further relevance here.

Scientific systemacy 2. Variables.

I have stipulated that for any primary predicate $\beta$ in the corpus of science Σ, certain logical alternatives to $\beta$ are also primary predicates of Σ. This point needs expansion, for it unfolds into what is perhaps the singlemost important technical concept of modern science. This is the notion of (scientific) variable, which is quite unlike this term's usage in logic and mathematics.

Any move to account for the truth of a sentence $\beta_k(\underline{b})$ is directed by one or more contrastive sentences $\beta'_k(\underline{b})$ whose predicate $\beta'_k$ is an alternative to $\beta_k$ that might well have been true of b instead of $\beta_k$. One such alternative to $\beta_k$ is always not-$\beta_k$; but when $\beta_k$ is a primary or more broadly a basic predicate of any science Σ that aims at precision and detail, $\beta_k$ will be a member of at least one contrast set $\underset{\sim}{\beta}_k = \{\beta_{kj}\}$ of Σ's basic predicates whose cardinality will almost always exceed two and may well be infinite. By saying that $\underset{\sim}{\beta}_k$ is contrastive, I mean that the predicates in $\underset{\sim}{\beta}_k$ are logically disjoint, i.e., that for any name tuple $\underline{b}$ and any two different predicates $\beta_{ki}$ and $\beta_{kj}$ in $\underset{\sim}{\beta}_k$, it is not logically possible for sentences $\beta_{ki}(\underline{b})$ and $\beta_{kj}(\underline{b})$ both to be true. Let us call any contrast set of Σ's basic predicates an "ur-variable" of Σ, recognizing thereby that these are almost but not quite what the term 'variable' denotes in modern science. (We shall abandon this notion as soon as we attain the real thing.) And for any ur-variable $\underset{\sim}{\beta}_k$ of Σ, say that the <u>domain</u> of $\underset{\sim}{\beta}_k$ comprises just the individual objects (usually subjects-at-times) of which at least one and hence exactly one predicate in set $\underset{\sim}{\beta}_k$ is true. The $\underset{\sim}{\beta}_k$-predicate that is true of any given object $\underline{o}$ in $\underset{\sim}{\beta}_k$'s domain is the <u>value</u> of ur-variable $\underset{\sim}{\beta}_k$ for $\underline{o}$.

Under this definition of "ur-variable," any subset $\underset{\sim}{\beta}'_k$ of ur-variable $\underset{\sim}{\beta}_k$ is also an ur-variable whose domain is part of the domain of $\underset{\sim}{\beta}_k$. And conversely, for any ur-variable $\underset{\sim}{\beta}_k$ over a restricted domain $\underline{D}_k$, we can always contrive for $\underset{\sim}{\beta}_k$ to be a proper subset of another ur-variable $\underset{\sim}{\beta}^*_k$ whose domain is an extension of $\underline{D}_k$. Consider, for example, the finite array $\underset{\sim}{\omega}_w$ of predicates

$$\underset{\sim}{\omega}_w = \{\_\_\_ \text{ weighs } \underline{w} \text{ lbs.}\} \qquad (\, \underline{w} = 1,2,3,\ldots,99,100 \,) ,$$

each of which is understood to be true of a given object $\underline{o} = \underline{s}$-at-$\underline{t}$ just in case $\underline{s}$'s weight at time $\underline{t}$ exceeds $\underline{w} - .5$ lbs. but is not greater than $\underline{w} + .5$ lbs. Then $\underset{\sim}{\omega}_w$ is an ur-variable whose domain is restricted to objects weighing between .5 and 100.5 lbs. But clearly, any set $\underset{\sim}{w}^*$ of numbers that includes the integers from 1 to 100 together with any nonnegative reals less than .5 or greater than 100.5 similarly defines a superset $\underset{\sim}{\omega}_{w^*}$ of $\underset{\sim}{\omega}_w$ that is also an ur-variable so long as the weight-spans of the new predicates so introduced are taken to be suitably narrow. Of all the different ways to so extend $\underset{\sim}{\omega}_w$, the one that seems obviously best (nevermind why) defines the scale <u>Weight-to-nearest-whole-lb</u>. by taking $\underset{\sim}{w}^*$ to comprise all nonnegative integers. (Note that adoption of this scale by a science Σ in no way precludes admission among Σ's ur-variables other weight arrays related to $\underset{\sim}{\omega}_{w^*}$ by converting lbs. therein to some other unit of measurement such as grams, tenths of a pound, etc. That is, we can simultaneously employ various scales of weight with rounding to different precisions.) With $\underset{\sim}{w}^*$ so chosen, the domain of $\underset{\sim}{\omega}_{w^*}$ does not include everything--colors, shapes, numbers, etc. have no weight at all--but there would scarcely ever be reason for a science concerned with weight to seek an extension of $\underset{\sim}{\omega}_{w^*}$ to an even-larger domain.

We do, however, often encounter domain problems of a sort illustrated by ur-variables

$$\underset{\sim}{\gamma} = \{\_\_\text{ has } \underline{c}\text{-colored hair}\} \quad (\text{'}\underline{c}\text{' ranges over disjoint color descriptions}),$$
$$\underset{\sim}{\delta} = \{\_\_\text{'s Stanford-Binet IQ is } \underline{x}\} \quad (\text{'}\underline{x}\text{' ranges over certain numerals}).$$

A science that hopes to study pigmentation or intelligence in humans finds it awkward to extend $\underset{\sim}{\gamma}$ or $\underset{\sim}{\delta}$ to a domain that includes people who are hairless or who have never been tested on the Stanford-Binet. It is simple enough to expand $\underset{\sim}{\gamma}$ (and similarly $\underset{\sim}{\delta}$) into the universal ur-variable $\underset{\sim}{\gamma}^*$ comprising all the predicates in $\underset{\sim}{\gamma}$ together with 'None of the $\underset{\sim}{\gamma}$-alternatives are true of ___'. But this makeshift wastebasket for leftovers does not contrast with the $\underset{\sim}{\gamma}$-predicates in the same natural fashion that the latter contrast among themselves, and cannot be expected to play a causal role parallel to that of the others. I shall provisionally denote as <u>anomalous</u> those predicates that so extend the domain of a naturally disjoint predicate set by saying in effect "none of the above."

We now adopt a familiar ontological heurism that is far less innocent than it seems, but does no immediate harm and can be avoided only with the most tortuous philosophic effort. This is to presume that each basic predicate $\beta_k$ of science $\Sigma$ signifies a <u>property</u> that is had by just the objects of which predicate $\beta_k$ is true and is what we are talking <u>about</u> when we use $\beta_k$ in the basic sentences of $\Sigma$.[4] Thus

[4]It is essential that "properties" be understood in the Platonic realist sense whereby it is possible for distinct properties to have exactly the same exemplars. Properties exemplified by $\underline{n}$-tuples are "relations" if $\underline{n} \geq 2$. And when we refer to "functions," as will soon become important, it is sometimes desirable to construe these nonextensionally as the special case of relations that differ from standard set-theoretic extensional functions in the same way that properties differ from set-theoretic classes. However, these are esoterically technical issues that I shall do my best to keep from becoming explicit.

we presume that '___ has brown hair' and '___ weighs 163 lbs.' signify the properties of Brown-hairedness and Weighing-163-lbs., respectively, and that the possession/lack of these by John today is what most directly determines the semantic truth/falsity of the sentences formed by putting a name for John-today in these predicates' name-receptacles. Then for any ur-variable $\underset{\sim}{\beta}_k$ of science Σ, set theory tells us that there exists a $\underline{\text{function}}$ $\underset{\wedge}{x}_k$ from the domain $\underline{D}_k$ of $\underset{\sim}{\beta}_k$ onto the range of properties signified by predicates in $\underset{\sim}{\beta}_k$ such that for each object $\underline{o}$ in $\underline{D}_k$, the value of $\underset{\wedge}{x}_k$ for $\underline{o}$ is the property signified by the one predicate in $\underset{\sim}{\beta}_k$ that is true of $\underline{o}$. The functions so defined from Σ's ur-variables are $\underline{\Sigma}$'s $\underline{\text{basic}}$ $\underline{\text{variables}}$. (More precisely, any $\underset{\wedge}{x}_k$ so defined from an ur-variable of Σ is a variable that is "basic" for Σ just in case it is not logically derived, in one of the fashions next to be clarified, from other basic variables of Σ.)

Henceforth, I shall use standard functional notation '$\underset{\wedge}{x}_k(\underline{o}) = \underline{x}$' to assert that the value of scientific variable $\underset{\wedge}{x}_k$ for object $\underline{o}$ is condition $\underline{x}$, the typeface contrast therein distinguishing the variable $\underset{\wedge}{x}_k$ itself (e.g., $\underline{\text{Weight}}$ and $\underline{\text{Hair-color}}$) from the assorted properties $\{\underline{x}\}$ (e.g., {weighing-$\underline{x}$-lbs.} and {having-$\underline{x}$-colored-hair}) into which $\underset{\wedge}{x}_k$ maps the various objects in its domain.[5] Thus when $\underset{\wedge}{x}_k$ is a basic

[5] The notation schema '$\underline{f}(\underline{a})$' that so thoroughly pervades modern mathematics, logic, and philosophy is unhappily ambiguous between applications in which '$\underline{f}$' abbreviates a predicate and those wherein it stands for a function. When '$\underline{f}$' is a predicate, '$\underline{f}(\underline{a})$' schematizes a $\underline{\text{sentence}}$, namely, one asserting that object $\underline{a}$ has property $\underline{f}$. In contrast, when '$\underline{f}$' names a function, '$\underline{f}(\underline{a})$' is to be read as the definite description 'the one thing related $\underline{f}$-wise to object $\underline{a}$'. Although logicians sometimes treat predicates, too, as names of functions (namely, by taking predicate '$\underline{f}(\_)$' to signify a function that maps each object $\underline{a}$ into the truth-value of sentence '$\underline{f}(\underline{a})$'), readers of texts in which both uses of schema '$\underline{f}(\underline{a})$' occur must take care to interpret particular instances of this formalism correctly. In our immediate application, typeface $\underset{\wedge}{x}$, $\underset{\wedge}{Y}$, etc. will unambiguously identify those special functions that are scientific variables. However, we shall also soon require notation for other functions that are identified as such only by context.

variable of science Σ, '$\underset{\wedge}{x}_k(\underline{o}) = \underline{x}$' is logically equivalent to the assertion 'Object $\underline{o}$ has property $\underline{x}$'. Note that in contrast to the "variables" of logic and mathematics, which are special linguistic devices, the variables of science Σ are de re aspects of its subject matter.

In practice, science $\Sigma$ generally converts its conceptions of basic variables into more complexely mathematized formalisms. For one, when $\langle \underset{\wedge}{x}_1,\ldots,\underset{\wedge}{x}_m \rangle$ is an $\underline{m}$-tuple of basic variables, we may well find it useful to treat this as a <u>compound</u> <u>variable</u> $[\underset{\wedge}{x}_1,\ldots,\underset{\wedge}{x}_m]$ or more briefly $\underset{\wedge}{X}$, the domain $\underline{D}_X$ of which is the intersection of the domains of its constituent variables $\underset{\wedge}{x}_1,\ldots,\underset{\wedge}{x}_m$ and whose value $\underset{\wedge}{X}(\underline{o})$ for any object $\underline{o}$ in $\underline{D}_X$ is the $\underline{m}$-tuple $\langle \underset{\wedge}{x}_1(\underline{o}),\ldots,\underset{\wedge}{x}_m(\underline{o}) \rangle$ of $\underline{o}$'s values on the simple variables compounded in $\underset{\wedge}{X}$. For example, if $\underset{\wedge}{x}_1,\underset{\wedge}{x}_2,\underset{\wedge}{x}_3$ are the sociological variables <u>Sex-of-first-child</u>, <u>Sex-of-2nd-child</u>, and <u>Sex-of-3rd-child</u> whose domains comprise families having at least one, at least two, or at least three children, respectively, their compound $\underset{\wedge}{X} = [\underset{\wedge}{x}_1,\underset{\wedge}{x}_2,\underset{\wedge}{x}_3]$ is the <u>Sex-of-first-three-children</u> variable over families having at least three children. (Note that this compound variable's domain is <u>not</u> the same as that of each constituent variable.) So if Mrs. Jones has five children of which the first is female and the next two male, $\underset{\wedge}{X}$(Jones'-family) = ⟨<u>female</u>,<u>male</u>,<u>male</u>⟩. Conceptually, concatenation of simple variables into compound ones seems utterly trivial; yet without this compaction device, description of even modestly complex systems would be hopelessly unmanageable.

When $\underset{\wedge}{X}$ is a compound variable whose components are $\underset{\wedge}{x}_1,\ldots,\underset{\wedge}{x}_m$, each simple variable $\underset{\wedge}{x}_i$ ($\underline{i} = 1,\ldots,\underline{m}$) therein is often said to be a <u>dimension</u> <u>of</u> $\underset{\wedge}{X}$<u>-space</u>; while "$\underset{\wedge}{X}$-space" itself is the set of all property $\underline{m}$-tuples $\langle \underline{x}_1,\ldots,\underline{x}_m \rangle$ in which, for each index $\underline{i} = 1,\ldots,\underline{m}$, $\underline{x}_i$ is a value of variable $\underset{\wedge}{x}_i$. Each such property-configuration $\langle \underline{x}_1,\ldots,\underline{x}_m \rangle$, i.e. position in $\underset{\wedge}{X}$-space, that is possible of joint realization by some object in this compound variable's domain $\underline{D}_X$ is a <u>state</u> <u>of</u> $\underset{\wedge}{X}$, while the particular value of $\underset{\wedge}{X}$ that holds for an object $\underline{o}$ in $\underline{D}_X$ is of course $\underline{o}$'s $\underset{\wedge}{X}$-state.

For simplicity, I have introduced compound variables as comprising a <u>tuple</u> of constituent variables. But more generally, the components of compound variable $\underset{\wedge}{X}$ can be organized as $\underset{\wedge}{X} =_{def} [\underset{\wedge}{x}_k : \underline{k} \in \underset{\sim}{k}]$ by any index set $\underset{\sim}{k}$ to each element $\underline{k}$ of which some variable $\underset{\wedge}{x}_k$ has been assigned. For example, $\underset{\wedge}{X} = [\underset{\wedge}{x}_{ij} : \underline{i} = 1,\ldots,\underline{m};\ \underline{j} = 1,\ldots,\underline{n}]$ is a compound variable whose indexing is two-dimensional. (This double-index case is extremely common in practice, and will soon be illustrated.)

For uniformity, we allow the concept of "compound variable" to include limiting case $\underset{\wedge}{X} = [\underset{\wedge}{x}_1]$ in which the compound is a singleton, i.e. contains only one component. And we also recursively allow the components of a compound scientific variable $\underset{\wedge}{X}$ to be compounds or any other nonbasic scientific variables defined prior to their compounding in $\underset{\wedge}{X}$. We shall consider a value $\underline{X} = \langle \underline{x}_1, \ldots, \underline{x}_m \rangle$ of compound variable $\underset{\wedge}{X} = [\underset{\wedge}{x}_1, \ldots, \underset{\wedge}{x}_m]$ to be <u>anomalous</u>--otherwise, <u>regular</u>--just in case one of the predicates signifying the properties in array $\langle \underline{x}_1, \ldots, \underline{x}_m \rangle$ is anomalous. And variable $\underset{\wedge}{X}$ as a whole is anomalous (otherwise regular) iff one of its values is anomalous.

Beyond compounding, any science $\Sigma$ finds it of great technical importance to exploit conceptions of <u>derivative</u> variables. These are of three recursively combinable types that can be simultaneously defined as follows:

<u>Definition 1</u>. Let $\underset{\wedge}{X}$ be any compound scientific variable (possibly a singleton); let $\underline{f}$ be any function from some object domain $\underline{D}_{\underline{f}}$ into the domain $\underline{D}_{\underset{\wedge}{X}}$ of $\underset{\wedge}{X}$; and let $g$ be any function whose domain includes the range of $\underset{\wedge}{X}$. Then the function $[g\underset{\wedge}{X}\underline{f}]$ (or simply $g\underset{\wedge}{X}\underline{f}$ when the brackets seem superfluous) from $\underline{D}_{\underline{f}}$ into the range of $g$ defined by the double composition

$$[g\underset{\wedge}{X}\underline{f}](\_\_) =_{def} g(\underset{\wedge}{X}(\underline{f}(\_\_)))$$

of $\underline{f}$ into $\underset{\wedge}{X}$ into $g$ is a <u>derivative</u> (<u>scientific</u>) <u>variable</u> whose domain is $\underline{D}_{\underline{f}}$, and in which composant functions $\underline{f}$ and $g$ are a <u>translocator</u> and <u>abstractor</u>, respectively. Special cases are $[g\underset{\wedge}{X}]$ and $[\underset{\wedge}{X}\underline{f}]$, which is what $[g\underset{\wedge}{X}\underline{f}]$ becomes when $\underline{f}$ or $g$, respectively, is an Identity function. Unless $\underline{f}$ is an Identity function, $[g\underset{\wedge}{X}\underline{f}]$ is <u>t(ranslocationally)-derivative</u> from $\underset{\wedge}{X}$ (also from $[g\underset{\wedge}{X}]$). If $g$'s domain-restriction to the range of $\underset{\wedge}{X}$ has an inverse, $[g\underset{\wedge}{X}\underline{f}]$ is a <u>scaling</u> of $[\underset{\wedge}{X}\underline{f}]$ (or is a <u>rescaling</u> thereof if $\underset{\wedge}{X}$ is already a scaling/rescaling of some other variable) and $[g\underset{\wedge}{X}]$ is a scaling/rescaling of $\underset{\wedge}{X}$; otherwise, if $g$ is <u>not</u> one-one over the range of $\underset{\wedge}{X}$, $[g\underset{\wedge}{X}\underline{f}]$ is <u>a(bstractively)-derivative</u> from $\underset{\wedge}{X}$ (also from $[\underset{\wedge}{X}\underline{f}]$).

In applications of Def. 1, we allow some or all components of compound variable $\underset{\wedge}{X}$ to be themselves derivative variables defined previously. So Def. 1 is to be understood as a recursion based on possibly-singleton compounds of a science's basic variables. But Def. 1 is not relative to any particular science Σ.

<u>Example</u>. Consider the variable $\underset{\wedge}{\bar{h}}_0$: <u>Mean-parental-height-in-inches</u>. This is built upon the single basic variable $\underset{\wedge}{h}$: <u>Height</u> over a domain of enduring-thing stages that includes bisexual organisms. Our first move is to scale Height as $[g_1\underset{\wedge}{h}]$: <u>Height-in-inches</u> where $g_1$ is the function that maps each basic height property, Being-<u>z</u>-inches-tall, into the number <u>z</u>. (Modern sciences almost always replace their basic variables first-thing by numerical scalings thereof, even when there is nothing at all quantitative in the variable's initial conception.) Next, take $\underline{D}_0$ to be the set of all temporal stages of enduring things that issue from bisexual gamete fusion, and let $\underline{f}_{♂}$ ($\underline{f}_{♀}$) be the function that maps each object <u>o</u> (= thing-<u>s</u>-at-time-<u>t</u>) in $\underline{D}_0$ into <u>o</u>'s male (female) parent at the time of <u>o</u>'s (i.e. <u>s</u>'s) conception. Finally, let $g_\mu$ be the function on tuples of numbers whose value for any argument is the arithmetic mean of its argument's components. Then Mean-parental-height-in-inches has domain $\underline{D}_0$ and a/t-derivational composition

$$\underset{\wedge}{\bar{h}}_0 =_{def} g_\mu[g_1\underset{\wedge}{h}\underline{f}_{♂}, g_1\underset{\wedge}{h}\underline{f}_{♀}] \quad .$$

From there, $\underset{\wedge}{\bar{h}}_1$ : <u>Mean-paternal-grandparental-height-in-inches</u> is $\underset{\wedge}{\bar{h}}_1 =_{def} [\underset{\wedge}{\bar{h}}_0\underline{f}_{♂}]$, i.e.,

$$\underset{\wedge}{\bar{h}}_1 = g_\mu[g_1\underset{\wedge}{h}\underline{f}_{♂}, g_1\underset{\wedge}{h}\underline{f}_{♀}]\underline{f}_{♂} = g_\mu[g_1\underset{\wedge}{h}\underline{f}_{♂}\underline{f}_{♂}, g_1\underset{\wedge}{h}\underline{f}_{♀}\underline{f}_{♂}] \ ,$$

and similarly for many other ancestral height averages.

In this example, $\underline{f}_{♂}$ and $\underline{f}_{♀}$ are translocation functions through which we can formalize the heights (or any other properties) of <u>o</u>'s parents as a compound property of <u>o</u>; $g_1$ is a scaling function that serves to represent heights by numbers; and $g_\mu$ is an abstractor that imposes a one-dimensional metric of equivalence classes on the range of $[g_1\underset{\wedge}{}\underline{hf}_{\text{♂}}, g_1\underset{\wedge}{}\underline{hf}_{\text{♀}}]$ by ignoring between-parent height differences. It should be evident in this case how any composite of the properties of any selected array of a bisexual $\underline{o}$'s ancestors can be fashioned into a property of $\underline{o}$ by judicious application of abstraction and translocation. Attempting similar assignments to $\underline{o}$ of properties of $\underline{o}$'s descendents, however, is greatly complicated by the non-functionality of progenation. That is, only by careful constraints on some subset $\underline{D}_{\alpha}$ of $\underline{D}_0$ can we insure that a description of form 'the type-$\alpha$ descendent of $\underline{o}$' has a unique referent for each $\underline{o}$ in $\underline{D}_{\alpha}$. Translocation is a powerful formalism that can easily produce nonsense if applied with insufficient thought.

Note also in this example that the objects in $\underline{D}_0$ assigned values of Mean-parental-height-in-inches are momentary stages of organisms, not these enduring things in their temporally protracted entireties even though we could just as easily have defined translocators $\underline{f}_{\text{♂}}/\underline{f}_{\text{♀}}$ to do it the other way. That is, John-today and John-yesterday are two different objects; and John-today's-having-$\underset{\wedge}{\bar{h}}_0$-value-68.7 is prima facie a different event from John-yesterday's-having-$\underset{\wedge}{\bar{h}}_0$-value-68.7 even though both derive from the very same birth situation. For the illustrative purpose at hand, choosing $\underline{D}_0$ to comprise momentary stages rather than temporal entireties of organisms was arbitrary; but it would emphatically not be so were Mean-parental-height designed to be part of a compound variable that also includes process variables whose variation from time to time in the same enduring organism is our target of study. This observation foreshadows Chapter 3's expansions upon translocation as the formalism by which technical sciences manage to collect causally interrelated events into compact, conceptually tractable packets even when these are widely dispersed in space and time.

For any simple or compound scientific variable $\underset{\wedge}{X}$ and object $\underline{o}$ in $\underset{\wedge}{X}$'s domain, we shall call $\underline{o}$'s-having-value-$\underset{\wedge}{X}(\underline{o})$-of-$\underset{\wedge}{X}$ an <u>event</u>, and will abbreviate this as $\ulcorner\underset{\wedge}{X};\underline{o}\urcorner$. Thus if $\underset{\wedge}{X}$ is Weight-in-lbs. while $\underline{o}$ is John-today, and John weighs 163 lbs. today, $\ulcorner\underset{\wedge}{X};\underline{o}\urcorner$

is the event designated by the gerundive noun-phrase 'John's having a weight-in-lbs. today of 163'.[5a] When later we speak of $\underline{o}_i$'s value of variable $\underset{\wedge}{X}$ being a cause of

[5a]Whether commonsense "events" should be so explicated as the referents of (some) gerundized sentences has been a recent philosophic controversy that we could dodge here simply by calling state-of-affairs $\ulcorner \underset{\wedge}{X};\underline{o} \urcorner$ something else, say a "singular fact." However, I put it to you that not merely does the "event"-talk adopted here prevail in scientific quarters, neither do we find in technical studies of natural lawfulness any motivation for a concept of "event" other than this one. Scientific practice does not close out the Kim/Davidson debate, but its preference for one side over the other is overwhelming.

$\underline{o}_j$'s value of variable $\underset{\wedge}{y}$ (where $\underline{o}_j$ may or may not be the same object as $\underline{o}_i$), this is elliptic for saying that the <u>event</u>, $\ulcorner \underset{\wedge}{X};\underline{o}_i \urcorner$, of $\underline{o}_i$'s having whatever $\underset{\wedge}{X}$-value it does have is a cause of event $\ulcorner \underset{\wedge}{y};\underline{o}_j \urcorner$. And we may henceforth regard any "accounting for" a datum-sentence '$\underset{\wedge}{X}(\underline{o}) = \underline{X}$' as equivalent to accounting for the event $\ulcorner \underset{\wedge}{X};\underline{o} \urcorner$. When $\underset{\wedge}{X}$ is a variable of science $\Sigma$, $\ulcorner \underset{\wedge}{X};\underline{o} \urcorner$ is a basic event, or a compound event, or an a-derivative or t-derivative event, etc., of $\Sigma$ according to whether variable $\underset{\wedge}{X}$ is recognized in $\Sigma$ as basic, or compound, or etc. And without trying to define the notion precisely, we shall also say that complex $\Sigma$-events are <u>molar</u> for $\Sigma$ when they are not simply conjunctions of $\Sigma$'s basic events. Most paradigmatically, $\ulcorner \underset{\wedge}{y};\underline{o} \urcorner$ is a molar event if $\underset{\wedge}{y}$ is holistically a-derivative from a compound variable $\underset{\wedge}{X}$. (The a-derivation, $\underset{\wedge}{y} =_{def} [g\underset{\wedge}{X}]$, of $\underset{\wedge}{y}$ from $\underset{\wedge}{X} = [\underset{\wedge}{x}_1,\ldots,\underset{\wedge}{x}_m]$ is "holistic" iff its abstractor does not decompose as $g[\underset{\wedge}{x}_1,\ldots,\underset{\wedge}{x}_m] = [g_1\underset{\wedge}{x}_1,\ldots,g_m\underset{\wedge}{x}_m]$.) Thus, John's-having-a-mean-parental-height-in-inches-of-68.7 is a molar event holistically a-derivative from the height-state of John's parents at the time of his birth. Moreover, we shall generally speak as though this classification of events has been freed of dependence on how these are viewed by some particular science, albeit in all likelihood such distinctions among event complexities will always remain relative to some conceptual framework warranting description as a "level of analysis."

The importance of a/t-derivative variables will emerge gradually as we proceed. But one prominent genus of these worth immediate recognition consists of <u>sample statistics</u>, defined by various abstractor functions on the distributions of scores on compound variables in groups of objects. Any array $\underset{\sim}{S} = \{\underline{S}_k : \underline{k} \in \underset{\sim}{k}\}$ of data sentences ($\underset{\sim}{k}$ any index set) can be formalized as a single compound datum statement '$\underset{\wedge}{Z}(\underline{s}) = \underline{Z}$' by taking $\underline{s}$ to be the totality of things whose properties or relations are set out by $\underset{\sim}{S}$, while $\underset{\wedge}{Z} =_{def} [\underset{\wedge}{z}_k\underline{f}_k : \underline{k} \in \underset{\sim}{k}]$ is the $\underset{\sim}{k}$-indexed array of t-derivative variables in which translocator $\underline{f}_k$ selects out of $\underline{s}$ the object or objects to which the $\underline{k}$th sentence in $\underset{\sim}{S}$ ascribes some property/relation formalized as a value of variable $\underset{\wedge}{z}_k$. For example, the most classic form of a data array is $\underset{\wedge}{Z}(\underline{s}) = \underline{Z}$ with $\underset{\wedge}{Z} =_{def} [\underset{\wedge}{X}\underline{f}_j : \underline{j} = 1,\ldots,\underline{n}] = [\underset{\wedge}{x}_i\underline{f}_j : \underline{i} = 1,\ldots,m;\ \underline{j} = 1,\ldots,\underline{n}]$, wherein $\underset{\wedge}{x}_i$ is the $\underline{i}$th component of a compound variable $\underset{\wedge}{X} = [\underset{\wedge}{x}_1,\ldots,\underset{\wedge}{x}_m]$ on which each sample member has been observed, and $\underline{f}_j(\underline{s})$ is the $\underline{j}$th member of sample population $\underline{s}$. The complex event $\ulcorner \underset{\wedge}{Z};\underline{s} \urcorner$ is sometimes called an "experiment" with set-up $\underset{\wedge}{Z}$. Then any abstractor function $\underline{g}$ on the range of $\underset{\wedge}{Z}$ is a "sample statistic" on set-up $\underset{\wedge}{Z}$ whose value in a particular experiment $\ulcorner \underset{\wedge}{Z};\underline{s} \urcorner$ is $\underline{g}\underset{\wedge}{Z}(\underline{s})$. In particular, all traditional data-summary measures--means, variances, correlations, regression coefficients, etc.--are sample statistics of this sort.

Example. SLese formalizations of sample data and their statistical abstracta are exemplars of system-complexity management far too important to leave unillustrated. In empirical research records--mainly in private data files, but sometimes in the appendices of circulated reports as well--it is extremely common to find numerical tables of the sort

| | | Object | | | | |
|---|---|---|---|---|---|---|
| | | #1 | #2 | #3 | #4 | #5 |
| Variable | $\underset{\wedge}{y}_1$ | 71 | 76 | 65 | 68 | 69 |
| | $\underset{\wedge}{y}_2$ | 167 | 225 | 120 | 136 | 94 |
| | $\underset{\wedge}{y}_3$ | 19 | 34 | 28 | 14 | 18 |
| | $\underset{\wedge}{y}_4$ | 1 | 1 | 0 | 0 | 0 |

$\underset{\wedge}{y}_1$: Height, in inches

$\underset{\wedge}{y}_2$: Weight, in lbs.

$\underset{\wedge}{y}_3$: Age, in years

$\underset{\wedge}{y}_4$: Sex, freudian coding (1 for male, 0 for female) ,

where the file may elsewhere identify these indexed individuals more fully (recording, e.g., that #1 is Richmore J. Prudy, Social Security No. 786-24-4415, on

Jan. 18, 1986) and may also cross-reference other data tables that organize additional information about these objects. (Cf. mention below of bio-social relations among individuals #1,...,#5 rather more intricate than normally found in real object samples.) The entry in row $\underline{i}$ (= 1,2,3,4) and column $\underline{j}$ (= 1,2,3,4,5) of this table is the value of the $\underline{j}$th member in this group of objects on the $\underline{i}$th dimension in the indicated scaling of vital-statistics compound [Height, Weight, Age, Sex]. Observe first of all that this format provides a far more compact and orderly record of these 20 events than do discursive sentences like

> When observed on 1/18/86, R. J. Prudy was a 167 lb., 19-year-old lad 71 inches tall. At R.J.P.'s birth, his 34-year-old-father stood six feet four inches and weighed 16 stone, while his mother, then 28 years of age, weighed 55 kilos at a height of 165 cm. On her 14th birthday, R.J.P.'s oldest sister was 5 ft. 8. in. tall and weighed 136 lbs.; four years later, she was an inch taller but 42 lbs. lighter.

Such economy of expression is in itself sufficient reason to replace the original commonsensically conceived data variables whose values are <u>attributes</u> (e.g., weighing-167-lbs.) with standardized numerical scalings thereof. (If one's intended data analysis can use the family-tie and time-lag relations among these Prudy-stages, these too can be systematically tabled albeit not in an array indexed this simply.) But more importantly, we can now think of this 4x5 number array as the value of compound variable $\underset{\wedge}{Z} =_{def} [\underset{\wedge}{y}_i \underline{f}_j : \underline{i} = 1,2,3,4; \underline{j} = 1,2,3,4,5]$ for a single complex object $\underline{s}$ whose parts $\underline{f}_1(\underline{s}),\ldots,\underline{f}_5(\underline{s})$ are individuals #1,...,#5, respectively. (It would be easier here to write simply $\underline{s}_j$ for the $\underline{j}$th member of sample $\underline{s}$, but I want to make explicit that selecting parts of sample $\underline{s}$ by use of indices as descriptors is a version of translocation.) Thinking this way is not just a bookkeeping convenience; it is conceptually essential for exploiting the mathematical machinery of modern data analysis. For example, the <u>covariance</u> matrix $\underline{C}_{YY}$ (sometimes called "dispersion") of the joint distribution of any number-valued variables $\langle \underset{\wedge}{y}_1,\ldots,\underset{\wedge}{y}_m \rangle$ in any $\underline{n}$-member sample $\underline{s} = \langle \#1,\ldots,\#\underline{n} \rangle$ of objects drawn from the domain of compound variable $\underset{\wedge}{Y} = [\underset{\wedge}{y}_1,\ldots,\underset{\wedge}{y}_m]$ is defined algebraically as the matrix product

$$\underline{C}_{YY} =_{def} \underline{n}^{-1}\underline{Z}J_n\underline{Z}^T \qquad ( J_n =_{def} I_n - \underline{n}^{-1}1_n 1_n^T ) ,$$

wherein super-T denotes matrix transpose, $I_n$ and $1_n$ are respectively the order-$\underline{n}$ Identity matrix and Unity column vector, and $\underline{Z}$ is the $\underline{m} \times \underline{n}$ score matrix $\underline{Z} = [\underline{z}_{ij}: \underline{i} = 1,\ldots,\underline{m}; \underline{j} = 1,\ldots,\underline{n}]$ whose $\underline{ij}$th element $\underline{z}_{ij}$ is the score $y_i\underline{f}_j(\underline{s})$ on the $\underline{i}$th component variable $\hat{y}_i$ in $\hat{Y}$ for the $\underline{j}$th member $\underline{f}_j(\underline{s})$ of object-sample $\underline{s}$. As you may know, $\underline{C}_{YY}$ tells for each pair of variables in $\hat{Y}$ how strongly deviations from the mean on one are accompanied in this sample by a corresponding deviancy on the other. But if you aren't familiar with matrix algebra or the covariance statistic, no matter: The essential point here is simply that mathematical formula

$$\underline{Cov}(\hat{Q}) =_{def} \underline{n}^{-1}\hat{Q}J_n\hat{Q}^T \qquad ( \underline{n} = \text{number of } \hat{Q}\text{'s columns} )$$

defines a certain abstractive (i.e. many-one) mapping of the domain $\{\underline{Q}\}$ of number matrices into itself; and the composition into $\underline{Cov}$ of the matrix-valued variable t-derived by crossing a compound numerical variable $\hat{Y}$ with the individuals in $\underline{n}$-membered samples $\{\underline{s}_n\}$ of objects in $\hat{Y}$'s domain defines a compound a/t-derivative variable $\hat{C}_{YY} =_{def} \underline{Cov}([\hat{Y}\underline{f}_j: \underline{j} = 1,\ldots,\underline{n}])$ over the set of all such samples that has proved enormously valuable for digesting sample data on the component variables in $\hat{Y}$. Indeed, within-sample covariance is the heart of modern multivariate data analysis, mainly because (roughly speaking) no other interpretively significant abstractions from raw event collections have so many powerful and elegant algebraic properties. But to understand and exploit this algebra, your thinking must be able to represent arbitrarily large score matrices, and the coefficients in functions thereof, by simple graphic codes such as the letter '$\underline{Z}$' as used above that you perceive as units and learn to manipulate by certain formalistic rules of symbol transformation even while you also remain able, when need arises, to unpack '$\underline{Z}$' and its ilk into the structured complexes for which they go proxy.

In applied research, "analysis" of data from an experiment $\ulcorner \underset{\wedge}{Z};\underline{s} \urcorner$ consists of computing the values of certain sample statistics $\{g_h\}$ for this experiment, whereafter "interpreting" the data comprises efforts to account for these a/t-derivative events $\{\ulcorner g_h Z;\underline{s} \urcorner\}$. Admittedly, the formalities of technical data analysis are rather more elaborate than captured by this one-sentence synopsis. But the salient point here is not so much that the methodology of applied data analysis can be insightfully developed as an algorithmic exercising of a/t-derivations (though that is true enough) but that a modern science's main targets of explanation are often holistic properties of sample populations rather than one-at-a-time basic events. Thus, theory on a given topic may be motivated to explain the curious configuration of intercorrelations observed among certain variables within some local selection of individuals. Ultimately, we explain sample statistics only by implicitly accounting for some or all of the basic events from which these are abstracta. (See p. 88ff. below for details.) But our epistemic access to explanations for such parts of a data array is through our detection and interpretation of statistically abstracted patterning within the whole (cf. Rozeboom, 1961, 1972).

Not all variables derivative from the basic variables of a science $\Sigma$ are explicitly variables of $\Sigma$, for vastly more exist de re than can ever be conceptualized by $\Sigma$'s epistemic community. But for any compound $\underset{\wedge}{X} = [\underset{\wedge}{x}_1,\ldots,\underset{\wedge}{x}_m]$ of $\Sigma$'s variables, and any functions $\underline{f}$ and $g$ known to $\underline{EC}$ such that the composition of $\underline{f}$ into $\underset{\wedge}{X}$ into $g$ is well-defined, $[g\underset{\wedge}{X}\underline{f}]$ is implicitly a derivative variable of $\Sigma$ that becomes explicitly so if $\underline{EC}$ finds it convenient to formalize some of what $\Sigma$ says about variables $\underset{\wedge}{X}$ in terms of statements about $[g\underset{\wedge}{X}\underline{f}]$. Beyond that, it is entirely possible for a variable $\underset{\wedge}{y}$ that is basic in one science to be implicitly or explicitly a-derivative, t-derivative, or both in another. In particular, this is not precluded by the first science's conception of $\underset{\wedge}{y}$ not making linguistically manifest that $\underset{\wedge}{y}$ is a/t-derivative.

The significance of this point--i.e., that $\underset{\wedge}{y}$ can be a basic variable of science $\Sigma$ even though in fact $\underset{\wedge}{y} = [g\underset{\wedge}{X}\underline{f}]$ for variables $\underset{\wedge}{X}$ not known to $\Sigma$--is profound; for all issues of molar/molecular contrasts in levels of science and the prospect of reducing one to another (a matter that Chapter 3 will consider in some detail) rest upon it. Indeed, we may stipulate that variable $\underset{\wedge}{y}$ "reduces" to variables $\langle \underset{\wedge}{x}_1, \ldots, \underset{\wedge}{x}_m \rangle$ just in case $\underset{\wedge}{y}$ in fact <u>is</u> (i.e. is identical to) $g[\underset{\wedge}{x}_1, \ldots, \underset{\wedge}{x}_m]\underline{f}$ for some abstractor $g$ and translocator $\underline{f}$ that are not both Identity functions.

<u>Scientific</u> <u>systemacy</u> <u>2</u> (<u>continued</u>). <u>Functional</u> <u>*laws</u>.

When a basic *law of science $\Sigma$ is written in the language of its (scientific) variables, form (4) becomes

(5) For all things $\underline{o}_i$ and $\underline{o}_j$ such that $\gamma(\underline{o}_i, \underline{o}_j)$, $\left(\underset{\wedge}{X}(\underline{o}_i) = \underline{X}\right) \rightarrow \left(\underset{\wedge}{z}(\underline{o}_j) = \underline{z}\right)$ ,

wherein $\underset{\wedge}{z}$ is a basic variable of $\Sigma$ or more likely some scaling thereof, $\underset{\wedge}{X}$ is a possibly-singleton compound basic or derivative variable of $\Sigma$, $\underline{X}$ and $\underline{z}$ are particular values of $\underset{\wedge}{X}$ and $\underset{\wedge}{z}$ respectively corresponding to the properties signified by $\alpha$ and $\beta$ in (4), and $\gamma(\underline{o}_i, \underline{o}_j)$ includes inter alia stipulation that $\underline{o}_i$ and $\underline{o}_j$ are respectively in the domains of $\underset{\wedge}{X}$ and $\underset{\wedge}{z}$.

<u>Examples</u>. (4.1) readily if nonidiomatically translates into SLese as

(5.1) For all bisexual organisms $\underline{o}_i, \underline{o}_j, \underline{o}_k$ such that $\underline{o}_i$ and $\underline{o}_j$ are the parents of $\underline{o}_k$, $\left([\text{Species}_1, \text{Species}_2](\underline{o}_i, \underline{o}_j) = \langle \underline{H}.\ \underline{\text{sapiens}}, \underline{H}.\ \underline{\text{sapiens}} \rangle\right) \rightarrow \left(\text{Species}(\underline{o}_k) = \underline{H}.\ \underline{\text{sapiens}}\right)$ ,

in which '$\text{Species}_k(\underline{o}_1, \ldots)$' is short for 'Species of the <u>k</u>th individual in tuple $\langle \underline{o}_1, \ldots \rangle$'. (The clumsiness of (5.1)'s parents'-species clause derives from its faithfulness to (5)'s compound-input formalism; otherwise, this could be written more simply as '$\left(\text{Species}(\underline{o}_i) = \underline{H}.\ \underline{\text{sapiens}}\right)$ & $\left(\text{Species}(\underline{o}_j) = \underline{H}.\ \underline{\text{sapiens}}\right)$'.) Example (4.2) is more difficult to scientize, because the only ur-variables provided by ordinary language as contrast sets for (4.2)'s input/output predicates are the default binaries {'is dropped', 'is not dropped'} and {'is broken', 'is not broken'}. There is nothing inherently wrong with binary variables; however, for reasons noted shortly, technical sciences seek much more finely graded contrasts whenever possible. In case (4.2), droppage is essentially replacement of rest by gravity propulsion, while as a first refinement of egg breakage we can take Shell-integrity to be the trichotomous variable whose values are ⟨intact, cracked, shattered⟩. Then (4.2) can be rewritten in form (5) as

(5.2) For any $o_i, o_j$ such that $o_i$ is a temporal stage of some chicken egg of which $o_j$ is also a stage five seconds after $o_i$, (Propulsion($o_i$) = earth-surface gravity acceleration from rest) → (Shell-integrity($o_j$) = shattered).

In practice, we often contrive for $\Upsilon(o_i, o_j)$ in (5) to hold for at most one $o_j$ given any $o_i$. In that case, there is a translocation function f on a domain D such that $\Upsilon$ is true of any $\langle o_i, o_j \rangle$ just in case $o_i$ is in D and $o_j = f(o_i)$. (Construction: Take D to comprise all $o_i$ such that some $o_j$ satisfies $\Upsilon(o_i, \_)$, and define f to be the function such that, for each $o_i$ in D, $f(o_i)$ is the one $o_j$ such that $\Upsilon(o_i, o_j)$.) Then (5) can be rewritten as

(6) For any object o in D, $(\hat{X}(o) = X) \rightarrow (\hat{y}(o) = y)$ ,

wherein $\hat{y} =_{def} [\hat{z}f]$ is the t-derivative variable over D that assigns to o the value of $\hat{z}$ that really holds for o's $\Upsilon$-correlate f(o). Most commonly in such conversions of (5) to (6), the tuples that satisfy $\Upsilon$ are pairs of temporal stages {s-at-t} of enduring subjects {s} while f is a forward-lag displacement f(s-at-t) $=_{def}$ s-at-t+Δ within the same subject for some fixed time-increment Δ and D comprises nonterminal subject-stages of some causally relevant kind. Then (6) has the more specific form

(6') For all $\underline{s}$-at-$\underline{t}$ in $\underline{D}$, $\left(\underset{\wedge}{X}(\underline{s}\text{-at-}\underline{t}) = \underline{X}\right) \longrightarrow \left(\underset{\wedge}{z}(\underline{s}\text{-at-}\underline{t}+\Delta) = \underline{z}\right)$ .

Thus, (5.2) is evidently equivalent to

(6.2) For any chicken egg $\underline{s}$ at any time $\underline{t}$ when $\underline{s}$ is still intact, (Propulsion($\underline{s}$-at-$\underline{t}$) = <u>earth-surface gravity acceleration from rest</u>) $\longrightarrow$ (Shell-integrity($\underline{s}$-at-five-seconds-after-$\underline{t}$) = <u>shattered</u>) .

Moreover, a *law of form (5) can always be recast by tricks of translocation into form (6), and in practice virtually always is, even when $\gamma(\underline{o}_i,\_)$ has multiple satisfiers for fixed $\underline{o}_i$. If nothing more parsimonious can be found, we can take $\underline{D}$ to comprise all tuples $\langle \underline{o}_i,\underline{o}_j \rangle$ satisfying $\gamma$ and replace variables $\underset{\wedge}{X}$ and $\underset{\wedge}{z}$ in (5) by the t-derivative variables $\underset{\wedge}{X}\underline{f}_1$ and $\underset{\wedge}{z}\underline{f}_2$ whose values for each $\langle \underline{o}_i,\underline{o}_j \rangle$ in $\underline{D}$ are respectively the value of $\underset{\wedge}{X}$ for $\underline{o}_i$ and $\underset{\wedge}{z}$ for $\underline{o}_j$. That is, we can take $\underline{f}_1(\underline{o}_i,\underline{o}_j) =_{def} \underline{o}_i$ and $\underline{f}_2(\underline{o}_i,\underline{o}_j) =_{def} \underline{o}_j$ and thus have $[\underset{\wedge}{X}\underline{f}_1](\underline{o}_i,\underline{o}_j) = \underset{\wedge}{X}(\underline{o}_i)$ and $[\underset{\wedge}{z}\underline{f}_2](\underline{o}_i,\underline{o}_j) = \underset{\wedge}{z}(\underline{o}_j)$. For example, recalling our stipulation that $\text{Species}_k(\_\_) =_{def}$ Species(the $\underline{k}$th member of tuple __), we can rewrite (5.1) as

(6.1) For any triple $\langle \underline{o}_i,\underline{o}_j,\underline{o}_k \rangle$ of bisexual organisms in which $\underline{o}_i$ and $\underline{o}_j$ are the parents of $\underline{o}_k$, $\left([\text{Species}_1,\text{Species}_2](\underline{o}_i,\underline{o}_j,\underline{o}_k) = \textit{H. sapiens}, \textit{H. sapiens}\right) \longrightarrow \left(\text{Species}_3(\underline{o}_i,\underline{o}_j,\underline{o}_k) = \textit{H. sapiens}\right)$ .

Were the essence of scientific lawfulness adequately captured by generality-form (4) (as philosophers often seem to think), transformation of this into (5) and from there into (6) would be a pointless descent into obfuscation. But in fact, we require this regimentation for access to SLese's conception of causality as functional production under which one <u>variable</u> makes a difference for another. Specifically, it is a standard expectation of technical science that if the basic variables from which $\underset{\wedge}{X}$ and $\underset{\wedge}{y}$ are derivative "carve nature at the joints," and (6) is true for one particular selection of their values, then by the same production principle it should be true for others as well. That is, there should exist some not-necessarily-proper subset

$\underset{\sim}{X}'$ of $\underset{\wedge}{X}$'s range $\underset{\sim}{X}$ such that each $\underset{\wedge}{X}$-state $\underline{X}'$ in $\underset{\sim}{X}'$ is coordinated with some $\underset{\wedge}{y}$-value $\underline{y}'$ for which (6) remains true under replacement of $\langle\underline{X},\underline{y}\rangle$ therein by $\langle\underline{X}',\underline{y}'\rangle$. If so, this cluster of parallel laws can be expressed simultaneously for all $\underset{\wedge}{X}$-states in $\underset{\sim}{X}'$ by a generality of form

(7) For all $\underline{o}$ in $\underline{D}$ and all $\underline{X}$ in $\underset{\sim}{X}'$, $\left(\underset{\wedge}{X}(\underline{o}) = \underline{X}\right) \longrightarrow \left(\underset{\wedge}{y}(\underline{o}) = \phi(\underline{X})\right)$ ,

wherein $\phi$ is a function--call it a <u>transducer</u>--into the range of $\underset{\wedge}{y}$ from an argument-domain that includes $\underset{\sim}{X}'$. Almost certainly, $\underset{\sim}{X}'$ will not contain any anomalous $\underset{\wedge}{X}$-values--which is why the contrast between a variable's regular and anomalous values is so important, and why we stipulate that variables are presumed regular unless explicitly declared otherwise. But conversely, we expect $\underset{\sim}{X}'$ to include most if not all regular $\underset{\wedge}{X}$-states that are compatable with membership in $\underline{D}$.

Indeed, seldom do practicing scientists infer a form-(7) *law by piecemeal collection of form-(6) *laws ascertained separately for each different $\underset{\wedge}{X}$-state. Rather, under the presumption that (7) is <u>explanatorily prior</u> to its form-(6) instantiations for particular values $\underline{X}$ of $\underset{\wedge}{X}$, one begins by postulating the existence of a functional law relating $\underset{\wedge}{y}$ to $\underset{\wedge}{X}$ in $\underline{D}$ under a transducer $\phi$ sufficiently well-behaved to be identifiable from a finite number of its points, and then proceeds by some admixture of observation and theory to estimate $\phi$ by analyzing sample distributions from $\underline{D}$ on $\langle\underset{\wedge}{X},\underset{\wedge}{y}\rangle$ or (since $\underset{\wedge}{X}$ and $\underset{\wedge}{y}$ are seldom observable entirely without error) on certain other variables diagnostic of $\underset{\wedge}{X}$ and $\underset{\wedge}{y}$ under some plausible measurement model. (See p. 90ff. below.) This practice, which builds confidence in (7) for a particular $\phi$ even when $\underset{\sim}{X}'$ includes $\underset{\wedge}{X}$-states not exemplified in $\underline{D}$, amounts operationally to search for patterning in $\underline{D}$-sample distributions on $\langle\underset{\wedge}{X},\underset{\wedge}{y}\rangle$ that shows us how to infer $\underset{\wedge}{y}(\underline{o})$ from $\underset{\wedge}{X}(\underline{o})$ for new objects $\underline{o}$ in $\underline{D}$ regardless of whether $\underline{o}$'s particular $\underset{\wedge}{X}$-state has been encountered previously. That search has no assurance of success; and even when $\phi$ has ideal finite identifiability, we always find in practice that our sample information leaves us with much less confidence in $\phi(\underline{X})$ (i.e., the $\underset{\wedge}{y}$-value into which imperfectly identified transducer $\phi$ maps particular $\underset{\wedge}{X}$-state $\underline{X}$) for some input states $\underline{X}$ than for others. Even so, technical science's most fundamental and momentous discovery has been that when variables are carefully defined to be precise on fine distinctions among contrastive attributes that stand in known comparison relations, and especially when these comparisons are quantifiable differences in degree, <u>such variables do in fact prevailingly appear to participate in form-(7) laws with inductively accessible transducers</u>. Arguably, only a world in which transducers generally correspond to real causal unities behind consiliances of form-(4) conditionals could make it possible for such inductions to succeed. But whatever the underlying ontology, the prowess of scientific inference is grounded on <u>thinking</u> of lawfulness in accord with (7) rather than (4) or even (6).

(More specifically, since (6) is the limiting case of (7) wherein $\underset{\sim}{X}'$ comprises a single $\underset{\wedge}{X}$-state $\underline{X}$, scientific accounting for an initially given variable $\underset{\wedge}{z}$ seeks to identify an object-domain $\underline{D}$ and a sufficiently articulate compound variable $\underset{\wedge}{X}$ such that for some broad-domain $\underset{\wedge}{y}$ either the same as $\underset{\wedge}{z}$ or from which $\underset{\wedge}{z}$ is a-derivative, ($\underline{a}$) $\underline{D}$, $\underset{\wedge}{X}$, and $\underset{\wedge}{y}$ satisfy (7) for some inductively ascertainable $\phi$, and ($\underline{b}$) $\underline{D}$ contains as much of the domain of $\underset{\wedge}{y}$ as can be managed. The greater the input diversity acknowledged in our articulation of $\underset{\wedge}{X}$, the better our potential success at ($\underline{b}$) but also the greater our practical inductive difficulties under ($\underline{a}$). Success in scientific research requires some rough optimization of this trade-off.)

<u>Illustration</u>. Law (6.1) of human speciation is evidently derivative from some far more general principle

(7.1) For all bisexual-organism triples $\langle \underline{o}_i, \underline{o}_j, \underline{o}_k \rangle$ such that $\underline{o}_i$ and $\underline{o}_j$ are the parents of $\underline{o}_k$, and any pair $\langle \underline{S}_a, \underline{S}_b \rangle$ of species satisfying constraints $\underline{Q}$, $\big([\text{Species}_1, \text{Species}_2](\underline{o}_i, \underline{o}_j, \underline{o}_k) = \langle \underline{S}_a, \underline{S}_b \rangle\big) \longrightarrow \big(\text{Species}_3(\underline{o}_i, \underline{o}_j, \underline{o}_k) = \phi(\underline{S}_a, \underline{S}_b)\big)$ .

This says simply that the species of any bisexual organism $\underline{o}_k$ is determined by the species of $\underline{o}_k$'s parents given certain constraints $\underline{Q}$ on the latter--except that (7.1) does not actually identify either $\underline{Q}$ or this law's transducer $\phi$. From what we know about speciation, part of $\phi$ is $\phi(\underline{S},\underline{S}) = \underline{S}$ for any single species $\underline{S}$; and that would suffice to complete (7.1) were $\underline{Q}(\underline{S}_a,\underline{S}_b)$ taken to be the strong constraint $\underline{S}_a = \underline{S}_b$ that precludes application of (7.1) to hybreds. However, were biologists to introduce an expanded phenotypic taxonomy of species that includes classification for all hybred possibilities, and research on hybredization were able to produce offspring from a decent diversity of sample crosses, it might be possible to let $\underline{Q}$ exclude only pairings that are in some sense biologically impossible and find enough regularities among the observed hybredizations to infer what species should result from crosses not yet examined, i.e. to identify or at least usefully estimate transducer $\phi$ over the entirety of its biologically possible input states. (Note, moreover, that what we in fact now know about $\phi$ in (7.1) illustrates how our confidence in what output is produced by an imperfectly identified transducer $\phi$ from one of its input states may differ considerably from our confidence in its yield from another.)

Similarly, once precisifying (4.2) into (5.2) or (6.2) alerts us to the manifold of egg-release alternatives, it is natural to ask how these variations differentially affect egg breakage. But that leads immediately to realization that the effect of an egg's Propulsion on its ensuing Shell-integrity strongly interacts with, inter alia, the distance, direction, and hardness of other objects nearby. So if we want a functional expansion of (6.2) whose $\rightarrowtail$-strength is high, the input must be a compound conjoining Propulsion with other variables still to be specified. This second example, unlike (7.1), sustains little scientific interest either theoretical or applied. Even so, it would be straightforward albeit far from trivial to fill in details of, say, schema

(7.2) For any intact chicken-egg stage s-at-t in an ordinary unchanging environment, $\Big([\text{Propulsion},\text{Hardness-surround}](s\text{-at-}t) = \langle P,H\rangle\Big)$ $\longrightarrow \Big(\text{Shell-integrity}(s\text{-at-}t+5'') = \phi(P,H)\Big)$ .

In this improvement of (6.2) we take Propulsion to be a compound variable whose components specify direction as well as vigor of thrust, while Hardness-surround is a tuple $H = [h_k : k \in k]$ of variables, indexed by a finite set $k$ of selected directions in physical space, such that the value of each H-component $h_k$ for s-at-t measures the distance from s-at-t to the nearest hard object in direction k. (The still-far-from-perfect ⟶-dependency in (7.2) could be further strengthened by including yet more input variables such as hardness/jaggedness of surrounding objects and viscosity of ambient gasses/liquids, as well as by careful standardization of "ordinary environment.") The important point to be taken here is that although the number of alternatively possible input states on [Propulsion,Hardness-surround], even with the number of directions dimensionalizing H chosen to be rather small and their values coarsely rounded, is enormously larger than would ever permit more than a tiny proportion of [P,H]-states to have their respective Shell-integrity implications ascertained independently of one another, the physics of this situation allows us to identify and communicate transducer function $\phi$ in (7.2) by a practical description that trained members of our epistemic community can effectively use to compute $\phi(P,H)$ for <u>any</u> suitably verbalized input state $\langle P,H\rangle$. In this particular case we would work out $\phi$ by inference from more basic physical principles already well known, notably, laws of ballistics. But were (7.2) an example of some serious law we are attempting to learn empirically, we would conjecture $\phi$ to be (roughly speaking) the most orderly function from [P,H]-space into the range of S (Shell-integrity) compatible with the small number of [P,H,S] data-points we have so far recorded for objects in this domain, and would thereby acquire a recipe for inferring (with, to be sure, appreciable uncertainty) what S-values will result even from [P,H]-states we have not yet encountered.

The canonical form, and its concealments, of well-SLesed regularities.

Although sentence schema (7) sets out the essence of a functional *law, its expression there is more complicated than needs be. For if we include in the definition of domain $D$ a condition that holds only for objects whose $X$-states are in $X'$, and take seriously our heurism that the conditionality arrow in (7) is errorless determination, (7) can be written

(8) For all $o$ in $D$, $y(o) = \phi(X(o))$ by $\twoheadrightarrow$-dependency of $y$ upon $X$ in $D$ .

(More precisely, (8) claims $y(o)$ to be $\twoheadrightarrow$-determined jointly by $X(o)$ and, presumably, certain unspecified additional properties common to members of $D$ but not likely identical with $D$-ness as such.) A prevalent, notationally powerful ellipsis for (8) is simply

(8') In $D$, $y = \phi(X)$ ,

use of which requires context to indicate the particular kind of conditionality envisioned and to make clear that $\langle X,y \rangle$ comprises the scores on $\langle X,y \rangle$ for an arbitrary object in $D$. The properties that characterize domain $D$ are what the physical sciences have traditionally called "boundary conditions." In the behavioral sciences, $D$ is usually conceived as some "population" that has been more or less representatively sampled by data from which the *law in question has been inferred, and is seldom identified more precisely than by a description something like "individuals similar to the ones observed in this study."

Although ellipsis (8') for (8) is the canonical form in which scientific practice almost always expresses its functional *laws, it is nevertheless important also to appreciate that this contrives to suppress explicit mention of the locus difference between the dependency-coupled events to which it applies. Thus, the most direct functional counterpart of pre-functional *law-form (5) is

(9) For all $\langle \underline{o}_i, \underline{o}_j \rangle$ such that $\Upsilon(\underline{o}_i, \underline{o}_j)$, $\underset{\wedge}{y}(\underline{o}_j) = \phi(\underset{\wedge}{X}(\underline{o}_i))$ by $\twoheadrightarrow$-dependency of $\underset{\wedge}{y}$ upon $\underset{\wedge}{X}$ under preconditions $\Upsilon$,

where $\Upsilon(\underline{o}_i, \underline{o}_j)$ generally imposes nonrelational constraints on objects $\underline{o}_i$ and $\underline{o}_j$ (if nothing else their respective membership in the domains of $\underset{\wedge}{X}$ and $\underset{\wedge}{y}$) together with some proper relation between them such as location displacement. Moreover, as illustrated by (5.1), the input formalism in (5) may suppress additional locus structure which, when made explicit in (5)'s functional expansion, further explicates (9) as

(9') For all $\langle \underline{o}_1, \ldots, \underline{o}_m, \underline{o}_{m+1} \rangle$ such that $\Upsilon(\underline{o}_1, \ldots, \underline{o}_m, \underline{o}_{m+1})$, $\underset{\wedge}{y}(\underline{o}_{m+1}) = \phi(\underset{\wedge}{X}_1(\underline{o}_1), \ldots, \underset{\wedge}{X}_m(\underline{o}_m))$ by $\twoheadrightarrow$-dependency of $\underset{\wedge}{y}$ upon $\langle \underset{\wedge}{X}_1, \ldots, \underset{\wedge}{X}_m \rangle$ under preconditions $\Upsilon$.

Formally, (8) is a natural specialization of (9'), whereas conversely, subsumption of (9') under (8) may require resort to translocations that seem awkwardly contrived (cf. example (7.1)). Even so, form (8) is technically much superior to (9'), not just because the condensation of (8) to (8') does not work well even for (9) much less (9'), but more profoundly because colligation of *laws that we can effectively integrate in considerable complexity under law-form (8') become impenetrable conceptual snarls under form (9/9'). We shall examine the major variants of these integrations in Chapter 3; and if you can discern there a manageable way to think about the behavior of such law-systems that is <u>not</u> grounded throughout on formalism (8'), I urge you to publish your alternative.

<u>Causal transduction vs. acausal regression.</u>

From the premise that a law of form (8) governs variables $\langle \underset{\wedge}{X}, \underset{\wedge}{y} \rangle$ in domain $\underline{D}$, it generally follows that

(8") For all $\underline{o}$ in $\underline{D}$, $\underset{\wedge}{y}(\underline{o}) = \phi_k(\underset{\wedge}{X}(\underline{o}))$

also holds for many different functions $\{\phi_k\}$ on the range of $\underset{\wedge}{X}$. (More technically, we envision $\{\phi_k\}$ as restricted to functions whose domain is just the not-necessarily-proper subset of $\underset{\wedge}{X}$-states that are logically compatible with membership in $\underline{D}$.) For as a rule, by distributional happenstance or nomic constraint, many logically possible $\underset{\wedge}{X}$-states remain unrealized in $\underline{D}$ (i.e. no $\underline{D}$-objects happen to have just those particular combinations of scores on $\underset{\wedge}{X}$'s components) with the result that given (8), (8") also holds for every function $\phi_k$ that agrees with $\phi$ over the $\underset{\wedge}{X}$-states occurrent in $\underline{D}$. For example, suppose that $[\underset{\wedge}{x}_1, \underset{\wedge}{x}_2]$ determines $\underset{\wedge}{y}$ in $\underline{D}$ under linear function

$$\underline{y} = 2\underline{x}_1 + 3\underline{x}_2$$

while $\underset{\wedge}{x}_1$ and $\underset{\wedge}{x}_2$ are each in turn determined in $\underline{D}$ by a common source $\underset{\wedge}{z}$ according to

$$\underline{x}_1 = \underline{z}\,, \qquad \underline{x}_2 = 2\underline{z}\;.$$

Then for all $\underline{o}$ in $\underline{D}$,

$$\underset{\wedge}{y}(\underline{o}) = \underline{a}_1\underset{\wedge}{x}_1(\underline{o}) + \underline{a}_2\underset{\wedge}{x}_2(\underline{o})$$

for every choice of real numbers $\langle \underline{a}_1, \underline{a}_2 \rangle$ such that $\underline{a}_1 + 2\underline{a}_2 = 8$. (This is because for each $\underline{o}$ in $\underline{D}$ and any $\langle \underline{a}_1, \underline{a}_2 \rangle$, $\underset{\wedge}{y}(\underline{o}) = 2(\underset{\wedge}{z}(\underline{o})) + 3(2\underset{\wedge}{z}(\underline{o})) = 8\underset{\wedge}{z}(\underline{o})$ while $\underline{a}_1\underset{\wedge}{x}_1(\underline{o}) + \underline{a}_2\underset{\wedge}{x}_2(\underline{o}) = \underline{a}_1(\underset{\wedge}{z}(\underline{o})) + \underline{a}_2(2\underset{\wedge}{z}(\underline{o})) = (\underline{a}_1 + 2\underline{a}_2)\underset{\wedge}{z}(\underline{o})$.) We may take it to be an essential demand on our still-evolving conceptions of de re conditionality that if the values of $\underset{\wedge}{y}$ for objects in $\underline{D}$ are <u>due to</u> their $\underset{\wedge}{X}$-states, then $\underset{\wedge}{X}$ $\twoheadrightarrow$-determines $\underset{\wedge}{y}$ in $\underline{D}$ under at most one particular function $\phi$ among those satisfying (8"), namely, the one for which it would be proper to say for each $\underline{D}$-compatible $\underset{\wedge}{X}$-state $\underline{X}$, actually realized in $\underline{D}$ or not, that any $\underline{o}$ satisfying the defining conditions of $\underline{D}$ and having value $\underline{X}$ of $\underset{\wedge}{X}$ <u>would</u> also have y-value $\phi(\underline{X})$.[6] We shall say that this one function $\phi$, which

[6] Actually, this is not entirely correct unless certain homogeneity constraints are placed on members of $\underline{D}$. Otherwise, it is conceivable even if implausible that different functions in (8")'s satisfaction set $\{\phi_k\}$ are variously causal in different homogeneous subsets of domain $\underline{D}$. We shall presume that $\underline{D}$ has the requisite homogeneity whenever this point is relevant.

sustains a full-range counterfactual, is the <u>causal</u> (or becausal) <u>transducer</u> of $\underset{\wedge}{X} \longrightarrow \underset{\wedge}{y}$ in $\underline{D}$, whereas any function $\phi_k$ other than $\phi$ for which (8") also holds is an (__acausal__) __regressor__ of $\underset{\wedge}{y}$ upon $\underset{\wedge}{X}$ in $\underline{D}$. Even when $\phi_k$ in (8") is only a regressor, however, we still want to say that (8") is a "law" so long as it is a logical consequence of other generalities that are laws having causal or becausal force. For not only is any such (8") then true of nomic necessity, not just by happenstance, the boundary between functional generalities whose transducers/regressors are or are not causal is still far too blurred (see p. 81ff. below) for this to be a useful defining condition on lawfulness.

__Nomic indeterminacy__.

For many methodological purposes, including present aims, it suffices to idealize the laws of a perfected science as a set of generalities having form (8) or, more articulately, (9'). But in practice we must settle for rather less than (8). For although it is easy enough to __conjecture__ *laws having fully deterministic form (8), our evidence for the dependency of a known variable $\underset{\wedge}{y}$ upon an identified variable $\underset{\wedge}{X}$ in any population $\underline{D}$ sampled by archived data never warrants conclusion that $\underset{\wedge}{y}$ is an errorless function of $\underset{\wedge}{X}$ in $\underline{D}$ unless we can persuade ourselves that the untidiness always found in empirical multivariate distributions of any decent sample size is in this case due entirely to "observation error."

There are two ways in which uncertainty can be built into our explicit conceptions of functional *laws. The high-tech version encouraged by mathematical statistics' awesome advance is to replace $\phi$ in (8') with a function that maps each $\underset{\wedge}{X}$-state $\underline{X}$ realizable in $\underline{D}$ into a probability distribution over $\underset{\wedge}{y}$-values conditional on the joint properties of belonging to $\underline{D}$ and having value $\underline{X}$ of $\underset{\wedge}{X}$. But we need also to acknowledge the only-too-likely prospect that the basic variables compounded in $\underset{\wedge}{X}$ interact with unidentified additional variables $\underset{\wedge}{E} = [\underset{\wedge}{e}_1, \ldots]$ in conjoint production of $\underset{\wedge}{y}$; and standard SLese formalisms for doing this allow us to avoid the horrendous conceptual complications that arise from taking the outputs of *laws to be expressly conditional probabilities. Specifically, it is virtually always cogent to view (8) as idealizing a more realistic

(10) For all $\underline{o}$ in $\underline{D}$, $\underset{\wedge}{y}(\underline{o}) = \psi(\underset{\wedge}{X}(\underline{o}),\underset{\wedge}{E}(\underline{o}))$ by $\longrightarrow$-dependency of $\underset{\wedge}{y}$ upon $[\underset{\wedge}{X},\underset{\wedge}{E}]$ ,

or more briefly

(10') $$\text{In } \underline{D},\quad \underline{y} = \psi(\underline{X},\underline{E}) \quad ,$$

wherein '$\underset{\wedge}{E}$' is a placeholder for unknown variables that supplement $\underset{\wedge}{X}$ in production of $\underset{\wedge}{y}$ in $\underline{D}$. (That is, the law-statements schematized by (10/10') implicitly begin with existential quantification 'There exists a compound variable $\underset{\wedge}{E}$ such that ...'.) For example, given sample data from domain $\underline{D}$ on numerically scaled dependent variable $\underset{\wedge}{y}$ and a compound $\underset{\wedge}{X} = [\underset{\wedge}{x}_1,\ldots,\underset{\wedge}{x}_m]$ of variables thought to be joint sources of $\underset{\wedge}{y}$ under conditions $\underline{D}$, it is established scientific practice to hypothesize that $\underset{\wedge}{y}$ is partially determined by $\underset{\wedge}{X}$ in $\underline{D}$ according to a principle of form

(10") $$\text{In } \underline{D},\quad \underline{y} = \phi(\underline{X}) + \underline{e}$$

whose supplementary input component $\underset{\wedge}{e}$ is conceived as a "residual" variable wherein is composited whatever influences on $\underset{\wedge}{y}$ are unmediated in $\underline{D}$ by the variables in $\underset{\wedge}{X}$. The value of $\underset{\wedge}{e}$ for each $\underline{D}$-object $\underline{o}$ is computable as $\underset{\wedge}{e}(\underline{o}) = \underset{\wedge}{y}(\underline{o}) - \phi(\underset{\wedge}{X}(\underline{o}))$; and the conditional distribution on $\underset{\wedge}{y}$ in $\underline{D}$ given any particular $\underset{\wedge}{X}$-state $\underline{X}$ thereby estimable from sample data on $\langle\underset{\wedge}{y},\underset{\wedge}{X}\rangle$ in $\underline{D}$ is tantamount to an estimated conditional probability distribution in $\underline{D}$ over $\underset{\wedge}{y} - \phi(\underline{X})$ given input $\underline{X}$. Residuation form (10") is the special case of (10') in which $\psi$ has additive decomposition

$$\psi(\underline{X},\underline{E}) \;=\; \phi(\underline{X}) + \psi'(\underline{E})$$

for an unknown supplementary compound $\underset{\wedge}{E}$ and an unknown transducer component $\psi'$ from which computable residual $\underset{\wedge}{e}$ abstracts according to a-derivation $\underset{\wedge}{e} =_{def} [\psi'(\underset{\wedge}{E})]$. Unhappily, both this classical additivity presumption and our equally ingrained custom of estimating $\phi$ in (10") to be some function that more or less minimizes the overall discrepancy between $\underset{\wedge}{y}$ and $\phi(\underset{\wedge}{X})$ in our observed sample of $\underline{D}$-objects are justified far more by mathematical expediency than by thoughtful argument. Even so, despite distressing suboptimalities in extant SLese theory and practice involving residuation form (10"), we have every reason to anticipate that whatever treatment of indeterminacy in nomic regularities proves most tenable, its SLese formulation will continue to be some variant of (10') augmented by some theory of estimating *X*-conditional distributions on $\psi$(*X*,E) in *D*.

Notes:

1) An important complication with generic residuation model (10) is that if a particular <*D*,X,y> has one E-supplement for which (10) is true, there will generally exist many admissible aspirants to this role, at different mediation distances from y, that vary greatly in how the transducer $\psi$ corresponding to a particular choice of E apportions differential responsibility for y between E and the variables in X. Moreover, some of these alternatives for E and $\psi$ essentially trivialize the model, notably, when E is taken so close to y as to mediate all of X's effect on y. Even so, it has become possible in modern multivariate theory not only to detrivialize (10) by cogent constraints on hypothesized supplementary y-sources E but also, under favorable research circumstances, to learn more about the latter by including y in a compound Y of data variables all thought to be affected by X in *D* and then studying the distributional patterning in sample data on Y from which X-scores have been partialled out.

2) Despite the prevalence of probabilistic thinking in modern science, formulating the output of nomic generalities as classical conditional probabilities is a workable alternative to residuation model (10) only for *laws in which the locus structure provided for by $\gamma$ in (9/9') is concealed by translocation within a probabilistic version of manifest form (8'). This is because (*a*) objective probabilities are classically relations on properties ("event-types"), *not* on localized events, and (*b*) the only way for this classic conception of probabilistic conditionality to acknowledge cause/effect location shifts is

by making these internal to the properties it couples in the manner illustrated by conversion of (5.1,2) to (6.1,2). For a simple pre-functional example, consider attempting to rephrase (4.2) as an explicit probability statement, say one in which 'rather likely' is sharpened to a precise probability value such as .85. The most straightforward probabilification of (4.2), namely,

(4.2') For all stages x and y of the same continuant egg with y five seconds after x, if x is dropped then the probability of y's being broken is .85 ,

will not do at all; for the conclusions it yields from appropriate antecedents are unconditional probabilities predicated of individual shell-integrity events. To see why this is intolerable even were we to make sense out of single-case probabilities, observe that we want our probabilification of (4.2) to be compatible with a plurality of egg-breakage laws having different outcome reliabilities as we variously enrich the antecedent of (4.2) with additional input details.[6aa]

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[6aa] We pass quickly here over what is perhaps the most urgent problem in the advanced theory of statistical probability, namely, working out some conception of chanciness that can be meaningfully ascribed to single cases (cf. Salmon, 1979). A recently favored gambit is to replace classical probabilities by "propensities," these being viewed as properties of individual objects that manifest themselves in relative frequencies of certain outcome alternatives under suitable release circumstances. Thus, rewriting (4.2') as

($4.2^+$) For any ‹x,y› in D [D the domain of (4.2')], if x is dropped then y has an 85%-strength propensity to break,

avoids the ill-formedness of (4.2'). But ($4.2^+$) claims an exceptionless universality that is incompatible with, say,

($4.2^{++}$) For any ‹x,y› in D, if x is dropped over a blanket then y has a 15%-strength propensity to break,

and with many similar variations on the input conditions in (4.2') that we need to tolerate in arrays of compatible egg-breakage *laws with overlapping domains but different $\underset{r}{\rightarrow}$-strengths. Regardless whether single-case propensities exist (and I see no reason why not, though I would also argue that they do not really advance the chance-theoretic ends for which their partisans invoke them), the uncompromising exceptionlessness of *laws like ($4.2^+$) and ($4.2^{++}$) make evident that law-claims in practice need to admit uncertainties--objective, epistemic, or both--that are not wholly detachable in instance-conclusions.

Yet neither can we acceptably cash out the classic $\underline{Pr}(\beta|\alpha) = \underline{r}$ schema as

(4.2") The probability of Broken-egg-stage-ness given Dropped-egg-stage-hood is .85 ;

for dropping an egg produces breakage not in the release stage but only, after some lag, in its successors. To capture the standard format of conditional objective probability here we need some t-derivational construction of the sort

(4.2"') The probability of Being-a-broken-egg-stage given Being-an-egg-stage-with-a-dropped-precursor-stage-five-seconds-earlier is .85 .

There is nothing amiss in such translocations: Having already praised them for providing access to the power of law-form (8'), I can scarcely cavil at their use in probabilistic softenings of (8'). But it is important to appreciate how rigidly orthodox conditional-probability formulations of nomic generality are committed to suppression of locus structure. To be sure, there is no evident reason why the classical objective-probability calculus cannot be expanded to recognize locus displacements in conditionals--indeed, schema (3) on p. 16, above, has already pointed out how that expansion might commence. But at present, philosophers and practicioners of science who favor conditional probabilities over form-(10) residuation for coping with nomic indeterminacy are well-advised on one hand to develop an appreciative understanding of SLese translocational formalisms, and on the other to ask themselves whether views on causality they find attractive when expressed as conditional probabilities on properties whose t-derivational compositions are notationally concealed still seem plausible when, as in $(4.2''')$, that locus structure is made manifest.

As you can see, SLese management of nomic uncertainty is still deeply problematic, far more uncomfortably so than is widely recognized. But here we can safely forego further concern for this important matter except for observing that any ultimate indeterminacy in production of $\underset{\wedge}{y}$-states by this variable's real causal sources can always be formalized as though this stochastic residual is a contribution of supplementary $\underset{\wedge}{y}$-source $\underset{\wedge}{E}$ in (10). Given a particular $\psi$ in (10) and state $\underline{X}$ just on $\underset{\wedge}{X}$ for a given object $\underline{o}$ in $\underline{D}$, our uncertainty about $\underline{o}$'s standing on $\underset{\wedge}{E}$ maps under $\psi(\underline{X},\_\_)$ into a corresponding distribution of uncertainty about $\underset{\wedge}{y}(\underline{o})$. Whether or not this uncertainty is based in part on inclusion in $\underset{\wedge}{E}$ of what is in fact some ultimate causal indeterminacy of $\underset{\wedge}{y}$-events matters only for our metatheory about the circumstances under which we can in principle come to be sure of $\underset{\wedge}{E}(\underline{o})$. So unless we are interested in probabilities for their own sake (which to be sure is true of probability theorists and ontologists), model (10) provides us with as much imperfection of lawfulness as we ever need while still allowing the theory of mediated causality to derive product-laws (see p. 67, below) by the mathematics of function composition.

## The Constitution of Causality

Treating nomic uncertainty as unidentified supplementary input can be disparaged on philosophical grounds as perseveration in a naively extremist view of causal determination which modern physics has shown untenable. The small grain of truth in this protest does not seriously impugn the practical utility of residuation form (10'). But it does lead nicely to meditation on some of causality's deepest ontological mysteries. SLese formalisms are largely indifferent to how these puzzles should be resolved, and I have taken care to preserve that openness here. Yet some applications of SLese work out far more effectively than do others, and there is every reason to suspect that these differential successes can be properly explained only by telling much of the ontological story. So although any present detailing of the latter could be little more than speculation, it would be remiss here not to exhibit its outline.

What does it mean to affirm, or deny, that the world is causally deterministic? Classically, Determinism is the thesis that every event is caused. But that slogan tolerates many readings within a multifaceted manifold roughed in, say, by

$$\left\{\begin{matrix}\text{Strong}\\ \text{Weak}\end{matrix}\right\} \left\{\begin{matrix}\text{global}\\ \text{local}\end{matrix}\right\} \left\{\begin{matrix}\text{lawful}\\ \text{anomalous}\end{matrix}\right\} \text{ determinism .}$$

The first facet of this array, "strong" vs. "weak," distinguishes determinism theses erected on the predicate '___ has a complete ("sufficient") cause' from ones that settle for '___ has a partial cause'. (It is perhaps a strain on common understanding to treat weak determinisms as varieties of *determinism*; but disenfranchizing them is unproductive.) The "lawful"/"anomalous" contrast acknowledges that one event might possibly be a cause of another even though that coupling is not an instantiation of any nomic generality. (I do not consider any version of anomalous determinism to be seriously defensible, but the prospect needs examination.) And I offer the "global"/"local" contrast as proxy for a rather protean spread of alternatives for the scope of causal connection. I have a specific interpretation in mind for these particular labels as variants of strong determinism; but before those can be mounted some larger causality boundaries must be drawn.

Verbalizing precise hypotheses in the theory of causation is an undertaking of enormous technical difficulty. Consider even the least demanding model suggested above, Weak Global Determinism: Presuming the existence of a category of entities called 'events' and a binary Cause-of relation thereon, this can be stated as the thesis that for every event $e_i$ there is an event $e_j$ that stands in the Cause-of relation to $e_i$. (To convert this minimalist thesis into Strong Global Determinism, substitute 'Sufficient-cause-of' for 'Cause-of' and 'a set $\{e_k\}$ of events' for 'an event $e_j$'.) But even holding in abeyance our problem of specifying what counts as causal connection--this is, after all, a theoretical notion which we can only hope to pin down by the role we give it in hypotheses about how the world works--this generality remains pompously empty until we clarify its scope. There should be little disagreement that whatever "events" may be, they constitute the domain in which causality operates. But unless we have grounds on which to judge whether something is or is not an event independently of our opinion about its causal connections, determinism theses about events as a class can be little more than idle word-play.

In contrast to anomalous determinisms, which offer no clues whatever to the nature of causal relata, any variant of lawful determinism implies that causally linked events are entities specifiable by linguistic expressions that instantiate the antecedents and consequents of properly verbalized laws. So if you accept the account I have given of SLese, and agree that the events at issue in causality debates are the sort of thing about which some version of lawful determinism logically could be true, you must also accept that the latter can be referred to by gerundized subject/predicate sentences. That is only a first step toward delimiting causality's domain, but it is an essential one.

Accordingly, let us for now adopt the ontological premise that for every true sentence paraphrasable as '*s* has *P*' (or '*s* is *P*-ish', or more formalistically '*P*(*s*)') with '_s_' a possibly-singleton tuple of nominals, there exist (_a_) an object (or object tuple) designated by '_s_', (_b_) a property (possibly relational) signified by '_P_', and (_c_) a factive entity designated by the gerundive nominal phrase '_s_'s having _P_'. (I don't really believe this wholesale existence postulate anymore than you do; but we pretty well have to start with some such platonic ideal until we can figure out what defensible restrictions of it are practical.) I shall here call the _s_'s-having-_P_ so picked out by any true sentence '_s_ has _P_' a _putative_ _event_ whose (putative) _locus_ and _character_ are respectively object _s_ and property _P_. (Don't fret yet that this makes a putative event's decomposition into locus and character relative to a particular subject/predicate parsing of its sentential description.) This enables us to commence inquiry whether there are not many distinct _kinds_ of putative events differing significantly in how, if at all, these partake in causal relations. For few putative-event descriptions derived from ordinary-language sentences inspire much confidence that these name full-blooded participants in the world's causal order.

Consider, for example,

$e_1$: _Seven_'s being a prime number,

$e_2$: _Maroon_'s being a rather dingy color,

$e_3$: _John's-passing-out-at-last-night's-banquet_'s being caused by his having drunk two liters of port,

$e_4$: _London-(now)_'s being famous for its theater,

$e_5$: _John-(now)_'s being older than Mary-(now),

$e_6$: _Mary-(now)_'s being younger than John-(now),

$e_7$: _John-(now)_'s having had at birth a mean parental weight of 145 lbs.,

$e_8$: _John's-parents-(at-the-time-of-his-birth)_'s having a mean weight of 145 lbs.,

$e_9$: _John-(now)_'s stooping to tie his shoelace,

with each putative event's locus marked by italics. (Pretend that 'John' and 'Mary' here designate real people having the properties indicated.) These can all be argued on various grounds to lie outside of causality's pale, albeit the readiest objections are ones that a sophisticated theory of causality should scorn as spurious. Most foolish is the popular notion that none of $e_1$-$e_8$ is a real event because, unlike $e_9$, these are not happenings but mere presences of state. In technical science, however, becomings are supervenient upon sequences of momentary beings and are far more awkward to subsume under laws than are the latter. Likewise to be dismissed is exclusion of $e_9$ from the realm of causality on grounds that this event is a purposive action and can hence be explained not by natural causes but only by its agent's reasons. This posture has long enjoyed the status of received doctrine in philosophy of mind, but it is backed by little more than uncomprehending prejudice against the very idea of mental mechanism and has now largely fallen into disrepute.

On the other hand, scarcely anyone would disagree that $e_1$, $e_2$, and $e_3$ are "events" only by gerundival charity. Undoubtedly explanations of sorts can be found for seven's being a prime number, for maroon's being a dingy color, or for one event's being caused by another, perhaps even lawful explanations; but even more surely these would not be causal explanations. Abstract objects such as numbers and colors, and especially causal events themselves as distinct from their loci, do not seem to be even remotely of a kind with entities whose properties can be produced or brought about in them by causally antecedent events. In light of such cases we may say programmatically, preparatory to deeper analysis, that any object $s$ is a causal locus iff $s$'s ontological nature does not preclude its being the locus of an event which has a cause or an effect. Or better, to stay clear of modality obscurities, let us stipulate more strongly that $s$ is a causal locus iff $s$ has some property such that $s$'s having it has a cause or effect. Note, however, that this definition gives no clue to which English nominals designate causal loci.

That we need to prepare a similar distinction between causal properties and ones whose instantiatings are incapable of having causes or effects; or at least never in fact do so, is illustrated by $e_4$-$e_8$. (This point is already plain in the characters of $e_1$-$e_3$, but $e_4$-$e_8$ extend it to properties even of prima facie causal loci.) Start with $e_4$: Could London be caused to have its theatric reputation? Although one might question whether London-(now) is a causal locus at all, that is no problem if we construe cities to be merely special regions of space/time. But is $e_4$'s character capable of causal production? It analyzes into a vague but complex existential generalization over beliefs to which the character of

$e_4'$: London-(now)'s being believed by someone to have great theater

is a pale approximation. If $e_4'$ cannot be caused, then surely neither can $e_4$. Yet even if we accept that fully determinate believings such as

$e_4''$: John-(now)'s believing that London has great theater

have causes, and moreover agree that $e_4''$ provides an explanation for $e_4'$ through our inference from the sentence describing $e_4''$ first to

John (now) believes of London that it has great theater

and from there to

London is believed by someone to have great theater,

we are not thereby committed to viewing $e_4''$ and its sources as causes of $e_4'$. Arguably, event characters defined by existential generalizations over causal properties supervene upon their abstraction bases without themselves being woven into the world's causal fabric.

Similar doubts about the causal status of supervenient properties arise from cases like $e_5/e_6$. To begin, it seems passing strange to distinguish $e_6$ from $e_5$ at all; for they appear to differ from each other and from

$e_{56}$: <John-(now),Mary-(now)>'s standing in the Older-than relation

only by inconsequential paraphrasings of their descriptions. Yet if we persist in the platonistic ontology that gives us Being-older-than-Mary and Being-younger-than-John as distinct real existents, we want to distinguish any instantiating of the one from all instantiatings of the other--whence $e_5$ and $e_6$ call for treatment as nonidentical events also differing from $e_{56}$. (Whether our apparent need to distinguish among $e_5, e_6, e_{56}$ is genuine remains for the deeper theory of explanatory structure to clarify.) If we do make this move, we may then wish to claim that $e_5$ and $e_6$ supervene noncausally upon $e_{56}$ so that Being-older-than-Mary and Being-younger-than-John would not be causal properties even were the Older-than relation from which they derive to be undisputably causal. Moreover, the latter too is suspect. For $e_{56}$ in turn derives analytically, not causally, from the conjunction of, say,

$e'_5$: John-(now)'s being 34 years old

and

$e'_6$: Mary-(now)'s being 27 years old ;

and one may question whether any relation over causal loci should be counted as causal when it abstracts from those objects' nonrelational properties.

Finally, what are we to make of $e_7$ and $e_8$? Well, $e_7$ is noncausally supervenient upon $e_8$ which in turn abstracts from, say, the pair of events

$e'_7$: John's-mother-(at-time-of-John's-birth)'s weighing 123 lbs.

and

$e'_8$: John's-father-(at-time-of-John's-birth)'s weighing 167 lbs.

So the character of $e_7$ is arguably noncausal because it derives by translocation from the character of $e_8$, and the latter in turn is arguably noncausal because it derives by abstraction from the character of $\langle e'_7, e'_8 \rangle$.

However, overly docile acceptance of this supervenience argument that $e_4$-$e_8$ have noncausal characters amounts to dismissal of causality from our practical affairs. Our larger point at issue, you will recall, is that the domain over which tenable determinism theses aspire to generalize can only be a proper subset of putative events, insomuch as many of the latter are seemingly excluded from the very possibility of causal functioning by the nature either of their loci or their characters.

If we continue to denote as "causal" those putative event loci/characters whose natures do not preclude their participation in causes or effects, and say that a putative event is a "proper" event iff its locus and character are both causal, we can conclude that the domain of causality is at most proper events and may well be rather less than that. Yet if we then hold also that events which are noncausally supervenient upon others--for simplicity albeit with some overextension call these molar events--are not proper events, we must conclude that few if any putative events in our commonsense ken have causes or effects. Indeed, you will be hard-pressed to verbalize even one gerundized ordinary-language sentence, precisified by technical science as much as you like, whose putative referent is not analytically derivative by abstraction and/or translocation from more basic events which account for it noncausally. (If you work at it you may be able to come up with an instance or two whose supervenience is not flagrant; but your difficulty in doing so suffices to make the point.) So on pain of incoherence, we must either (a) expunge causal thinking from management of our practical affairs, or (b) develop servicable concepts of causality that do allow molar events to play causal roles. Since (a) is humanly impossible--don't kid yourself, there is no way we can successfully engage the world bereft of views on what brings about what else--we are left with (b) as the most important challenge now confronting the advanced theory of causality.

At first thought, the problem just raised seems easily obviated: Even if $e_5$ and $e_6$ above are not strictly caused by $e'_5$ & $e'_6$, nor $e_7$ and $e_8$ by $e'_7$ & $e'_8$, we should have little hesitancy in viewing $e_5$-$e_8$ as consequences of the events on which they supervene and hence also as outcomes of any more remote events that are genuine causes of the latter. So why not simply understand "cause" and "effect" in a sense sufficiently liberal to include supervenience dependencies? (After all, while it does not seem quite right to say that John's-being-older-than-Mary has been caused by John's-age-being-34-years-and-Mary's-being-27, only small twinges of impropriety accrue to admitting the former as an effect of the latter.) But that gives molar events causal status only as epiphenomena lacking causal consequences of their own. How can we justify our practical intuitions that some molar events *produce* others? Consider, for example, our explaining the upward movement of an untethered hot-air balloon by saying that this object's rising is caused in part by its density's being less than that of the air which surrounds it. The density of this balloon as a whole (its shell and enclosed volume) is merely an average of the densities of its parts, just as the character of $e_8$ is a-derivative from those of $e'_7$ and $e'_8$. Yet must we take that to imply that the balloon's rise is not really brought about by its holistic density but only by the ensemble of its parts' densities? If so, the supervenience of each part's density upon that of its sub-parts, and so on down to infinitesimals, would seem to deny that any finite density feature of the balloon can be a genuine causal determinant of its flight.

The challenge of molar causality, then, is this: Suppose, as a counterfactual heurism, that we have well-developed concepts of "*f*(oundational)-events" and "*f*(oundational)-causality" such that we fully understand what it is for one *f*-event to be an *f*-cause or *f*-effect of another, while moreover *f*-causality complies with whatever conditions $\alpha_f$ we consider ideal for causal connection. Write '$\underline{E}_f$' for the set of all *f*-events and '$\underset{f}{\rightarrow}$' for the *Is*-*an*-*f*-*cause*-*of* relation on $\underline{E}_f$. Then a *molar*-*causality structure of quality* $\alpha_g$ is any 2-tuple $\langle \underline{E}_g, \underset{g}{\rightarrow} \rangle$ in which $\underline{E}_g$ is a set of molar events supervenient upon the events in $\underline{E}_f$ while $\underset{g}{\rightarrow}$ is a relation on $\underline{E}_g$, defined from $\underset{f}{\rightarrow}$ and the derivations of $\underline{E}_g$-events from their grounds in $\underline{E}_f$, that satisfies certain conditions $\alpha_g$ of adequacy on molar counterparts of $\underset{f}{\rightarrow}$. Optimally, $\alpha_g$ should include the same intrinsic constraints $\alpha_f$ that we ideally suppose of $\underset{f}{\rightarrow}$; however, we may find that these can be attained for a particular molar domain $\underline{E}_g$ only to some degree of imperfect approximation. And $\alpha_g$ may also impose constraints on $\underset{g}{\rightarrow}$ in relation to $\underset{f}{\rightarrow}$. (E.g., it should forbid $e_2 \underset{g}{\rightarrow} e_1$ whenever there exist $\underline{E}_f$-events $e'_1$ and $e'_2$ upon which $e_1$ and $e_2$ are respectively supervenient while $e'_1$ is an *f*-cause of $e'_2$.) Once we have worked out the theory of ideally grounded molar-causality structures, and allow molar causality to include $\underset{f}{\rightarrow}$ as a limiting case, it should be routine to relax this into a theory that allows the ground, $\langle \underline{E}_f, \xrightarrow[f]{} \rangle$, of $\{\langle \underline{E}_g, \xrightarrow[g]{} \rangle\}$ to be itself a molar-causality structure (at some quality $\alpha'_f$ entailing $\alpha_f$) which may supervene in turn upon some unspecified more basic system of causally connected events. We will then possess the intellectual resources, or at least a good part of them, needed to pick out particular strata/sectors/levels/networks/ensembles/kinds $\underline{E}_g$ of molar events in humanly practical terms that do not expressly identify the derivational constitutions of events in $\underline{E}_g$, and to conceive (with luck, correctly) of each selected $\underline{E}_g$ as ordered by a molar-causality relation $\xrightarrow[g]{}$ of some servicable quality $\alpha_g$ that justifies our thinking of $\xrightarrow[g]{}$-connection as bona fide causal production even while we remain receptive to future discovery that molar system $\langle \underline{E}_g, \xrightarrow[g]{} \rangle$ supervenes upon some molecular underlay $\langle \underline{E}_f, \xrightarrow[f]{} \rangle$.

A successful theory of molar causality will not come easily. An articulated understanding of SLese formalisms is prerequisite to it, and refinements of the Causal Metaprinciples described on p. 81ff. below are also foundational. But these constructions only give us a recursively elaborated plenitude of lawful generalities whose assorted causal qualities (i.e., those of the event dependencies they variously subsume) remain problematic. The real work begins when we try to sort the latter into associated molar-causality structures of respectable quality--which is to say that still another prerequisite for serious study of causal structure is setting out the conditions a relation on events must satisfy if it is to pass even as a weak simulacrum of causal connection, much less to count as perhaps the real thing. So as a final prefatory word on molar causality here, let me suggest three major requirements for any event-relation $\xrightarrow[g]{}$ to merit acceptance as causality at any molarity level:

1) $\xrightarrow[g]{}$-connection must be governed by SLese-describable laws. In particular, this requires that whenever $\underline{e}_1 \xrightarrow[g]{} \underline{e}_2$ for any events $\underline{e}_1$ and $\underline{e}_2$, there must exist loci $\underline{o}_1$ and $\underline{o}_2$, event characters $\underline{P}_1$ and $\underline{P}_2$, scientific variables $\underset{\wedge}{P}_1$ and $\underset{\wedge}{P}_2$ (i.e. contrast sets of event-characters) of which $\underline{P}_1$ and $\underline{P}_2$ are respectively values, and a complex of partly-relational conditions $\gamma(\_\_,\_\_)$ satisfied by $\langle \underline{o}_1, \underline{o}_2 \rangle$,

such that ($\underline{a}$) $\underline{e}_k$ ($\underline{k}$ = 1,2) is $\underline{o}_k$'s-having-$\underline{P}_k$, and ($\underline{b}$) whenever $\gamma(\underline{o},\underline{o}')$ for any event loci $\underline{o}$ and $\underline{o}'$, events $\ulcorner \underset{\wedge}{P}_1;\underline{o}\urcorner$ and $\ulcorner \underset{\wedge}{P}_2;\underline{o}'\urcorner$ not merely exist (i.e., $\underline{o}$ and $\underline{o}'$ have values of $\underset{\wedge}{P}_1$ and $\underset{\wedge}{P}_2$, respectively) but moreover $\ulcorner \underset{\wedge}{P}_1;\underline{o}\urcorner \underset{g}{\rightarrow} \ulcorner \underset{\wedge}{P}_2;\underline{o}'\urcorner$. (Precondition $\gamma$ on $\langle\underline{o},\underline{o}'\rangle$ here may include the existence of additional events that conjoin $\ulcorner \underset{\wedge}{P}_1;\underline{o}\urcorner$ in production of $\ulcorner \underset{\wedge}{P}_2;\underline{o}'\urcorner$.)

2) The laws governing $\underset{g}{\rightarrow}$-connection envisioned in (1) must specify a <u>manner</u> (at minimum, a function) by which each $\ulcorner \underset{\wedge}{P}_1;\underline{o}\urcorner \underset{g}{\rightarrow}$-produces a value of $\underset{\wedge}{P}_2$ at loci satisfying $\gamma(\underline{o},\_\_)$; and when more than one such manner is compatible with all extant $\gamma$-pairings of $\underset{\wedge}{P}_1/\underset{\wedge}{P}_2$-events, these have a priority ordering wherein just one has the primacy that warrants subjunctive inferences telling which $\underset{\wedge}{P}_2$-state <u>would</u> be $\underset{g}{\rightarrow}$-produced at loci satisfying $\gamma(\underline{o},\_\_)$ from any value of $\underset{\wedge}{P}_1$ conjectured for $\underline{o}$.

3) $\underset{g}{\rightarrow}$-connection must be a <u>strict</u> <u>partial</u> <u>order</u>; i.e., transitive, antisymmetric and irreflexive, or, equivalently, transitive and asymmetric. That is, $\underline{e}_1 \underset{g}{\rightarrow} \underline{e}_3$ whenever $\underline{e}_1 \underset{g}{\rightarrow} \underline{e}_2 \underset{g}{\rightarrow} \underline{e}_3$, and $\underline{e}_1 \underset{g}{\rightarrow} \underline{e}_2$ only if not $\underline{e}_2 \underset{g}{\rightarrow} \underline{e}_1$. (<u>Note</u>: antisymmetry allows $\underline{e}_1 \underset{g}{\rightarrow} \underline{e}_2 \underset{g}{\rightarrow} \underline{e}_1$ only if $\underline{e}_2 = \underline{e}_1$ and hence $\underline{e}_1 \underset{g}{\rightarrow} \underline{e}_1$, while irreflexivity forbids the latter. A relation that is both antisymmetric and irreflexive is said to be "asymmetric"--see Suppes, 1972, p. 69f.)

Conditions (1) and (2) are programmatic as stated, the latter horrendously so; but it is relatively straightforward to cash them out in axiomatic models of $\underset{g}{\rightarrow}$-structure built upon (3). Admittedly, (1) forecloses anomalous determinism; but that is a privation easily endured. And although one who supposes that a useful distinction can be drawn between immediate and mediate causation may wish to protest (3)'s presumption of transitivity, balking at that amounts to denial that $\underset{g}{\rightarrow}$-connection might be densely mediated, i.e., that when $\underline{e}_i \underset{g}{\rightarrow} \underline{e}_j$ holds, this generally derives from the existence of some intervening event $\underline{e}_m$ (or some intervening array $\{\underline{e}_m\}$) such that $\underline{e}_i \underset{g}{\rightarrow} \underline{e}_m \underset{g}{\rightarrow} \underline{e}_j$.

According to my own intuitions in this matter, partial-ordering of causal relata is the one requirement on causal connection that is rigidly axiomatic at any molarity level. Indeed, this may well be more than just an ordering of events. For arguably, (3) should be strengthened by

> 3') For any two loci *o* and *o*', and any molar-causality relation $\underset{g}{\rightarrow}$ on events, say that *o* $\underset{g}{\rightarrow}$-*precedes* *o*' iff *o* is the locus of a $\underset{g}{\rightarrow}$-cause of an event whose locus is *o*'. Then $\underset{g}{\rightarrow}$-precedence is a partial order: transitive and anti-symmetric, though perhaps not irreflexive. (*Note*: The order properties of $\underset{g}{\rightarrow}$ stipulated by (3) entail that $\underset{g}{\rightarrow}$-precedence is transitive, but do *not* suffice for its anti-symmetry postulated by (3').)

Be clear that $\underset{g}{\rightarrow}$-precedence is a relation on *loci*, not just on their having various selected attributes as is $\underset{g}{\rightarrow}$. Since (3') does not claim $\underset{g}{\rightarrow}$-precedence to be a *strict* (irreflexive) partial order, it allows an effect to have the same locus as its cause. But its antisymmetry does forbid any production sequence $e_1 \underset{g}{\rightarrow} e_2 \underset{g}{\rightarrow} e_3$ to give $e_3$ but not $e_2$ the same locus as $e_1$. That is, under (3'), a causal process never loops back upon any site from which it has exited. (My hunch is that causal productions at levels sufficiently molecular always incur some locus displacement which, however, may not be discernable in all molar-causality structures supervenient thereon.) And (3') conjecturably expands into a far more comprehensive locus ordering. For if we say that one locus "supports" another iff the first is either part of or $\underset{g}{\rightarrow}$-precedes the second for some $\underset{g}{\rightarrow}$, locus-support in the large may well be a partial order if our standards for molar causality are not overly lax. Be that as it may, (3') urges that the causal sequencing of events is channeled by an ontologically prior order on their loci--which requires causal loci to have certain "essences" independent of their features imposed by causality. We shall develop this important prospect shortly.

Insomuch as any respectable Determinism thesis must indicate what sorts of causality it takes to be at issue, contentions in this matter should by rights be put on hold until we have worked out some inventory of molar-causality structures.

But we can still get on with the broader issues by indulging in a programmatic concept of "generic" causality. Specifically, suppose that for a certain quality standard $\alpha_{min}$, we agree that any event-relation $\xrightarrow[g]{}$ should be viewed as <u>generically causal</u> just in case, for some selection $\underline{E}_g$ of events, $\langle \underline{E}_g, \xrightarrow[g]{} \rangle$ is a molar-causality structure of at least quality $\alpha_{min}$. Then the disjunction of all event-relations that are generically causal is also a relation on events that we may call "gen-causality." That is, any putative event $\underline{e}$ is, by definition, a gen-cause of another, $\underline{e}'$, just in case $\underline{e} \xrightarrow[g]{} \underline{e}'$ for some generically causal relation $\xrightarrow[g]{}$ of passable quality. We should not presume gen-causality to be itself a causal relation, for it may well not satisfy any reasonable choice of $\alpha_{min}$. But it does usefully block out the scope of causality in that any event $\underline{e}$ has a cause or an effect at some level of molar causality iff $\underline{e}$ has a gen-cause or gen-effect, respectively. So long as we have not singled out any particular $\xrightarrow[g]{}$ for special attention, we can shorten 'gen-cause' and its cognates to 'cause' simpliciter.

<u>Infra-causal events and the essence of causal loci.</u>

In light of these considerations, we can refine the boundaries of causal connection by saying that an object is a causal locus iff it is the locus of any event having a gen-cause or gen-effect, that a property is causal iff it is the character of any event having a gen-cause or gen-effect, and that a putative event is causally proper--otherwise improper--iff its locus and character are both causal. Within these domain limits, we can glibly define the Determinism varieties listed earlier as

<u>Weak Global Determinism</u>: Every proper event has some (partial) gen-cause.

<u>Strong Global Determinism</u>: Every proper event has a jointly sufficient set of gen-causes.

<u>Strong Local Determinism</u>: Any proper event has a jointly sufficient set of gen-causes <u>if</u> it has any (partial) gen-cause at all.

(Under any defensible choice of $\alpha_{min}$ these are all implicitly versions of lawful determinism. The corresponding definition of Weak Local Determinism is vacuous.) But these remain mere word-games until we make some progress on our scarcely-begun task of delimiting which putative event loci/characters are generically causal; and now, we don't even have supervenience as a partial criterion for this discrimination. Intuition continues to insist (with some justification that will surface shortly) that neither the loci nor characters of $\underline{e}_1$-$\underline{e}_3$ are causal even in the generic sense. But although we no longer have evident reason to dismiss $\underline{e}_4$ or $\underline{e}_7/\underline{e}_8$ as causally improper, important problems remain with $\underline{e}_5/\underline{e}_6/\underline{e}_{56}$ and their underlay $\underline{e}_5'$ & $\underline{e}_6'$. For John-(now)'s-being-34-years-old and its ilk call for recognition that not all putative events which contribute to causal productions are themselves brought about causally even in an ideally deterministic universe.

Briefly, the point is that when an event $\ulcorner y;\underline{o}_{m+1}\urcorner$ is caused by some array $\ulcorner x_1;\underline{o}\urcorner,\ldots,\ulcorner x_m;\underline{o}_m\urcorner$ of antecedent occurrences under a law of form (9')--and locus structure such as made explicit in (9') virtually always underlies SLese-ideal format (8)--the domain precondition $\mathcal{T}(\underline{o}_1,\ldots,\underline{o}_m,\underline{o}_{m+1})$ which submits the state of $\underline{o}_{m+1}$ to control by the state of $\langle\underline{o}_1,\ldots,\underline{o}_m\rangle$ is not itself the output of some other causal law. To be sure, $\mathcal{T}(\underline{o}_1,\ldots,\underline{o}_m,\underline{o}_{m+1})$ is generally a conjunction of relational/nonrelational events whose loci are subtuples/elements of $\langle\underline{o}_1,\ldots,\underline{o}_m,\underline{o}_{m+1}\rangle$; and __some__ of these conditions may well have been caused. (See the metaprinciple of Domain Constriction, p. 82f. below.) But $\mathcal{T}(\underline{o}_1,\ldots,\underline{o}_m,\underline{o}_{m+1})$ generally contains an irreducible core that is explanatorily prior to all operations of causality, the locally relevant fragment of a realm of Being required to establish the very possibility of causal propagation. Rather than arguing this abstractly, I call upon your intuitions for standard cases wherein the momentary multidimensional state of some enduring thing $\underline{s}$, say John's home computer, or the smallest bacillus in John's colon, or John himself, or etc., at any given time $\underline{t}$ is a major source of this same thing's state shortly after $\underline{t}$. (See e.g. egg-breakage illustration (7.2), above.) Although $\underline{s}$-at-$\underline{t}$+$\Delta$'s having $\underline{s}$-at-$\underline{t}$

as its same-continuant precursor at lag $\Delta$ is a precondition for $\ulcorner \underset{\wedge}{x};\underline{s}\text{-at-}\underline{t}\urcorner$ to affect $\ulcorner \underset{\wedge}{y};\underline{s}\text{-at-}\underline{t}+\Delta\urcorner$ in the particular way it does, as distinct from how it affects other $\underset{\wedge}{y}$-events $\ulcorner \underset{\wedge}{y};\underline{s}'\text{-at-}\underline{t}+\Delta'\urcorner$ at any lag $\Delta'$ different from $\Delta$ even in same-thing case $\underline{s}' = \underline{s}$ much less in things $\underline{s}'$ other than $\underline{s}$, it seems absurd to suggest that anything __causes__ $\underline{s}\text{-at-}\underline{t}+\Delta$ to be so displaced from $\underline{s}\text{-at-}\underline{t}$ Rather, this locus relation is a brute infra-causal __given__ that may or may not supervene upon nonrelational infra-causal events such as $\underline{s}\text{-at-}\underline{t}$'s and $\underline{s}\text{-at-}\underline{t}+\Delta$'s having particular locations in absolute time, but either way is to be explained, if at all, in some manner other than a causal story.

However, the contrast between causal and infra-causal event characters that stands out so starkly in time lags is in practice convolutedly blurred. For the properties signified by many of our most familiar molar predicates appear to abstract jointly from causal and infra-causal underlays. For example, the __34-years-oldness__ featured in $\underline{o}_5^!$ seems nearly as infra-causal as we can get; and were this nothing more than the width of a temporal interval, that appraisal would be fair enough. But '$\underline{o}$ is 34 years old' analyzes something like 'The time span between $\underline{o}$ and the birth-stage of an enduring thing whose stages include $\underline{o}$ is 34 years'; and it is hard to imagine how explication of 'birth-stage' if not 'enduring thing' could avoid reference to causal properties. The weight of this point doesn't rest mainly on Age, however. Far more forceful examples abound if we agree that space is conjugate to time in the ontology of infra-causality.

When causal-event compound $\langle \ulcorner \underset{\wedge}{x}_1;\underline{o}_1\urcorner,\ldots,\ulcorner \underset{\wedge}{x}_m;\underline{o}_m\urcorner\rangle$ picks out satisfiers of $\mathcal{T}(\underline{o}_1,\ldots,\underline{o}_m,\_\_)$ as the sites of its $\underset{\wedge}{y}$-effects in accord with some particular transduction principle $\underset{\wedge}{y}(\_) = \phi(\underset{\wedge}{x}_1(\_),\ldots,\underset{\wedge}{x}_m(\_))$, the features distinguishing loci that qualify from the overwhelming majority that do not must include more than just temporal relations to $\langle \underline{o}_1,\ldots,\underline{o}_m\rangle$. Other than causal properties, whose presence in propagation directive $\mathcal{T}$ is arguably artifactual in the way clarified on p. 82ff. below, two main candidates for this role are put forth by our entrenched locutionary styles for identifying mundane subjects of predication. Most prevalent in ordinary language are proper names, demonstratives, or definite descriptions purporting to designate spatially extended _things_ that endure through time with an ontic _one_ness that makes it meaningful and sometimes correct to say that _this-now_ is the same thing (in the continuant-identity sense of "same") as _this-then_. Implied by this usage is that ontological particulars (i.e., impredicable bearers of attributes) are temporal stages of such continuants, that time-shifts within the same thing are propagation displacements _par excellence_, and that where in space a given thing is at any moment is one of its "accidental" (contra "essential") features. But an alternative also tolerated by ordinary language and strongly favored by at least some branches of advanced physics is to speak of _places_--locations in space--at particular times as what it is that undergo impositions of one accidental feature rather than another. In this latter view, space joins time to comprise the essence of ontological particularity: The loci of causal events just _are_ spacetime points or regions (collections) thereof, and the basic locus displacements of effects from their causes are excursions in time and place specifiable as such.

We have no need here to take sides on the ontology of space and time; my abstract formulation of causal events as constituted by locus-cum-character has carefully evaded any commitment to the nature of loci. But I do submit that even disregarding relativistic contentions that spatial intervals cannot be sharply distinguished from temporal ones, it is quite plausible even if still unsettled that an outcome event's spatial relations to its causes are as much infra-causal preconditions of this production as are its temporal lags. If so, many macro-properties that we traditionally take to be accidental features of thing-stages, notably shapes, sizes (distances between shape-salient boundary points), and above all spatial positions are really supervenient upon the infra-causal properties of spacetime regions. But not on those alone. For if causal loci are fundamentally collections of spacetime points, a commonsense continuant thing is surely some spacetime region within which the structured distribution of causal micro-properties has a thingy integrity lacking in other regions that intersect/enclose this one. (Just what the latter amounts to is a seminal obscurity toward whose clarification significant progress will be made in chapters to come.) Then to say that John's home computer, or the smallest bacillus in John's colon, or John himself now has the location fully specified by a certain set of spatial coordinates (from which abstract this thing's present shape, size, overall position, and orientation) is not merely to describe a certain spacetime place as being where it is, but also to impute a rather special causal state to that place. The claim, in short, is that 'John is now here' most properly analyzes as 'Here-now is Johnish'.

How, then, should we classify the characters of such putative events as John's being 34 years old, his home computer's standing 15 inches high, and his smallest intestinal bacillus's being rod-shaped? Are these causal, infra-causal, or what? Rather more is in this than just some arbitrary decisions about our use of causality labels. At the very least we want to recognize distinctions that matter while ignoring ones that do not; and more importantly, there is reason to suspect that this particular issue is salient for selecting variables and boundary conditions in applied scientific research. To establish a baseline, consider the putative event

$\underline{e}^*$: Location $\underline{o}_i$'s being 24 inches northwest of and 13 seconds before location $\underline{o}_j$.

According to the definitions just struck, is this event causally proper? Well, its locus (which we may take to be the pair $\langle \underline{o}_i, \underline{o}_j \rangle$ rather than just the gerundive's grammatical subject $\underline{o}_i$) is clearly causal, so $\underline{e}^*$ is proper iff Being-24-inches-northwest-of-and-13-seconds-earlier-than is causal. Now surely no event with this character is **produced** on any level of molar causality—nothing can bring it about that one spacetime region is separated from another by this interval. But might one have some causal effect? Certainly there may well be some causal law in which this relation is one of the $\gamma$-preconditions establishing $\underline{o}_j$ as the site of some event $\ulcorner \hat{y};\underline{o}_j \urcorner$ brought about in part by an event $\ulcorner \hat{x};\underline{o}_i \urcorner$. So $\underline{e}^*$ is in this case a contributor to

$\underline{o}_i$'s $\underset{\wedge}{x}$-state being a cause of $\ulcorner \underset{\wedge}{y};\underline{o}_j \urcorner$. But must we therefore consider $\underline{e}^*$, too, to be a cause of $\ulcorner \underset{\wedge}{y};\underline{o}_j \urcorner$? Not necessarily: There is no reason why the events conjunctively described in the antecedent of an instantiated causal law may not partition among disjoint kinds of source events that are not all causally proper. It only remains for us to work out the salient categories and take care to heed them where relevant, e.g., in ajudication of Determinism hypotheses. Provisionally, we can say that an event-character $\underline{P}$ is infra-causal, not causal, iff $\underline{P}$ is in the preconditions $\gamma(\_\_,\ldots,\_\_)$ of some causal law even though no event having character $\underline{P}$ has a gen-cause. But this may not be quite what we want here, insomuch as it cramps inquiry into what events, if any, might be uncaused causes. Better, perhaps, is to add that infra-causal properties are moreover "essential" in the strong sense that our conceptions of them provide construction of descriptors that we can feel sure uniquely identify specific causal loci. Thus, we feel confident that a suitably precise nominal of form 'The location having spatiotemporal coordinates ...' picks out a unique referent; whereas for any definite description that includes appeal to some causal accident of its purported referent, it remains problematic whether 'The object such that (nonessentially) ...' has one and only one satisfier. But how to convert this epistemic notion into a hard ontological criterion is unclear.

Even more puzzling is where to put properties such as shape, size, and position in the world's causal order. It will not do to argue that their supervenience upon spatial relations precludes their characterizing outputs of causal laws. For insomuch as most molar states of macro-objects over which we hope to achieve some operational control abstract to one degree or another from the infra-causal arrangements of those objects' parts, any theory of causality suitable for human affairs must contrive to admit such properties as producable. That does not seem unattainable in principle, so long as the abstraction bases of these supervenient properties include micro-states that are indisputably causal. Indeed, the extraordinary success of Newtonian mechanics, whose primary output variable is the spatial position of thing-stages, illustrates how nicely event-characters that are largely infra-causal can in some cases, under well-chosen boundary conditions, be governed by laws that certainly seem causal. Perhaps the best way to develop understanding of the causal/infra-causal distinction and its supervenience fusions will be initially to relativize this division to each particular molar-causality structure $\langle \underline{E}_g, \underset{g}{\rightarrow} \rangle$, with the infra-causal properties therein being those by which we identify particular loci of $\underline{E}_g$-events independently of their characters governed by $\underset{g}{\rightarrow}$-laws. The ensemble of these relativized contrasts may or may not then point to some limiting absolute distinction between causal accidents and infra-causal essences. It should be of great ontological interest whether we can find support in this for positing infra-causal event characters, such as might rehabilitate Aristotelian "substances," meatier than bare spatiotemporal positioning.

I take from these musings on the ontology of location a loose directive and a vague surmise. The directive is that the theory of molar causality, which seeks explanatory orderings of events not merely in production priorities within any one $\underset{g}{\rightarrow}$-level but also in compositional dependencies across levels, must extensively weave abstractions from infra-causal essences into its multi-layered fabric of causal progressions. In this, one molar-causality structure $\langle \underline{E}_g, \underset{g}{\rightarrow} \rangle$ will contrast with another in part by how infra-causal features are abstractively/translocationally blended with causal ones in, on one hand, the descriptors by which we identify loci of $\underline{E}_g$-events, and on the other in the compositions of their characters. And my surmise is that different ways of doing this may well matter considerably for the scope and inductive accessibility of laws governing $\underline{E}_g$-events. (The systemic importance of scope--i.e., breadth of domain--will become evident in Chapter 3.) Thus, a law written to yield conclusions about where John, Mary, and their thing-peers are located in space at various stages of their lives is irrevocably restricted in application to circumstances wherein micro-features are distributed with the special lumpiness definitive of continuants like John and Mary; whereas laws for propagation from one dated place to another of feature patterns that are in varying degrees

Johnish or Maryish could apply everywhere everywhen. If so, sorting out what manners of abstraction from what mixes of what causal/infra-causal underlays give what molar predicates and locus descriptors greater SLese potency than others in what restricted contexts should be an enterprise wherein philosophers of ontology and methodologists of scientific practice can collaborate with mutual profit.

Philosophical problems of lawfulness: A summary sampler.

You may find it anticlimactic that we shall not terminate this extended introduction to Determinism with any appraisal of its major variants' differential merits. But answers are premature where questions are still inchoate. Instead, Determinism has been our foil for blocking out the framework of ontological issues that must be worked through in some detail before we can claim any real understanding of causal/becausal explanation. The huge problem emphasized here is our need to develop some ontic classificational scheme wherein putative events are ordered in hierarchies of abstractive/translocational supervenience, cross-cut at various molarity levels $\{\underline{E}_g\}$ by corresponding systems of laws within each of which a forceful subset (contrasted with the powerless consequences thereof) defines on $\underline{E}_g$ a relation $\xrightarrow[g]{}$ that satisfies our requirements to count as causal production at this level. And we have observed that just what those requirements should be also remains a high-priority obscurity, as does some disentangling of essence from accident in the states of causal loci. But within this broad frame are many specific questions. Without any suggestion of completeness, here are a few that seem especially provocative for philosophical speculation.

Do causal events have occult sources? Were it not for some tension with the provisional definition of 'infra-causal' suggested with reservations above, we could stipulate that an occult source of any event $\ulcorner \hat{y};\underline{o} \urcorner$ is another event that is a gen-cause of $\ulcorner \hat{y};\underline{o} \urcorner$ but has no cause whatever of its own. To avoid tedious refinements, I will settle for this approximation anyway, with the addendum that this notion brokers the alliance of residuation model (10) (p. 36 above) with Strong Local Determinism. For these presume (*a*) that when $\ulcorner\underset{\wedge}{X};\underline{o}\urcorner$ determines $\ulcorner\underset{\wedge}{y};\underline{o}\urcorner$ only partially, there is always some array $\ulcorner\underset{\wedge}{E};\underline{o}\urcorner = \langle\ulcorner\underset{\wedge}{e}_1;\underline{o}\urcorner,\ulcorner\underset{\wedge}{e}_2;\underline{o}\urcorner,\ldots\rangle$ of supplementary events such that $\ulcorner\underset{\wedge}{X};\underline{o}\urcorner$ and $\ulcorner\underset{\wedge}{E};\underline{o}\urcorner$ are a jointly sufficient (errorless) cause of $\ulcorner\underset{\wedge}{y};\underline{o}\urcorner$ under a causal law of form (10) *even though some components of* $\ulcorner\underset{\wedge}{E};\underline{o}\urcorner$ *may totally lack any causes of their own and are thus* (roughly speaking) *entirely unpredictable*. An alternative prospect, however, is (*b*) that there exist causal events $\ulcorner\underset{\wedge}{y};\underline{o}\urcorner$ on at least one molarity level whose totality of causes and infra-causal sources partially determine $y(\underline{o})$ without doing so under the transducer of any errorless causal law. I have contended that the formalistic needs of SLese systemizations pretty well require us to proceed as though (*a*) is true, even had we reasons more practical than metaphysical conjecture to believe (*b*). But acceptance of (*a*) confronts us with many delightful perplexities about occult sources, such as where are their loci in relation to those of the events they affect, and how if at all do their characters differ in kind from infra-causal essences on one hand and producable event-characters on the other. Whereas if we opt for (*b*), we have an even greater challenge in trying to make the notion of irreducibly partial transduction intelligible.

*The structure of explanation*. For any declarative sentence *S*, let '*G*[*S*]' stand for the gerundive nominalization of *S* or its idiomatic equivalent, and consider the English sentence frames

*G*[___] was caused by *G*[___] ,

*G*[___] caused it that ___ ,

*G*[___] was an effect of *G*[___] ,

*G*[___] brought about *G*[___] ,

*G*[___] gave rise to *G*[___] ,

*G*[___] resulted from *G*[___] ,

That ___ resulted in *G*[___] ,

*G*[___] had the result that ___ ,

*G*[___] was responsible for *G*[___] ,

___, owing to G[___] ,

G[___] is why ___ ,

G[___] was due to G[___] ,

G[___] accounts for G[___] ,

___ because ___ .

What do these, together with their tense variants, have in common? Answer: All require presumedly true sentences to fill their blanks (albeit some also accept agent names in place of one 'G[___]'); each is usually best understood to carry an implicit "in-part" qualifier (as in 'G[___] was (partially) responsible for G[___]'); and each is one way to claim that the factive entity described by one of its arguments is wholly or partially explanatory for the other. At minor risk of begging the question whether the connections predicated by such verbs of explanation are in some cases relations on what true sentences mean rather than on what they signify, let us agree that the sentential arguments these take, explicitly nominalized or not, all designate states-of-affairs or more briefly objective facts, of which causal events are a special case. Given this understanding, linguistic intuition tells me and, I trust, you that the verb-phrases just listed divide into two synonymy groups, the first eight and last six respectively (with some ambiguity in 'result'), while the first group entails the second but not conversely. Thus if John's slipping on the ice caused him to break his arm,[6a] it follows that John broke his arm because he slipped on the ice; whereas conversely, when John is older than Mary because their ages are respectively 34 and 28 years, no causality at any molarity level seems implied. If for standardization we take 'G[___] is (was, will be) due, wholly or in part, to G[___]', or equivalently '___ (at least to some extent) because ___', as our paradigm of explanatory relevance

[6a] Locution-form '___ caused o to D' can safely be viewed as idiomatic for '___ caused o's D-ing', even though someone beguiled by the myth of Agency as a force distinct from event-causality might wish to read this as alleging an explanation for an agent's acting.

in the large, we are given powerful entry into Explanation's most profound philosophical obscurities by the deceptively simple question, What are the order properties of Because? Specifically, what true sentences, if any, signify facts that are not due even in part to any other facts; are the easy arguments proving conversely that every fact has other facts due to it indeed sound;[6b] and most importantly, is the Is-due-to relation a

[6b]For any (meaningful declarative) sentences 'p' and 'q', if it is true that-p, then G[p-or-q] is due to G[p]; and if it is true both that-p and that-q, G[p-and-q] is due partially to G[p] and partially to G[q]. Or so it seems.

strict partial order on its domain? Commonsense is adamant both that nothing is due to itself (irreflexivity) and that anything which is due to something due in turn to something else is also due to the latter (transitivity), which together entail asymmetry as well. However, when one contemplates even the full array of causal events interlaced by supervenience as well as gen-causality, much less the broader factive domain that includes events like $e_1$-$e_3$ above and beyond that whatever underlies the truth of scientific laws, mathematics, semantics, ethics, alethic/deontic logics, and still other systems of abstractly recondite conjecture, it becomes problematic in the extreme whether our world's putative totality of concrete/abstract/particular/universal/simple/complex facts can indeed be partially ordered by becausal dependency. Yet if our order intuitions about the structure of explanation prove untenable, can we plausibly retain their presumption on any level of causality? And would not loss of partial-ordering even by becausal connection in the large, much less by molar-causalities in the small, degrade our explanatory-dependency concepts to near-vacuity if not incoherence?

The enigma of causal transduction. It is no secret that the subjunctive reading of 'If ... then ...', which envisions that its antecedent somehow necessitates its consequent, is still darkly obscure. But this mystery's deeper reaches become visible only in efforts to fathom the forcefulness rider on SLese formalism (9'): The statement that $\underset{\wedge}{y}(o_{m+1}) = \phi(\underset{\wedge}{X}_1(o_1),\ldots,\underset{\wedge}{X}_m(o_m))$ for all object tuples $\langle o_1,\ldots,o_m,o_{m+1}\rangle$ satisfying precondition $\mathcal{T}$ claims no more than extensional coincidence (Humean "constant conjunction") until we add that function $\phi$ manifests some forceful binding of $\hat{y}$ to $\langle \hat{X}_1,\ldots,\hat{X}_m \rangle$ under locus structure $\Upsilon$. This must be an integrative coupling of universals, not just a repetitous linking of particular events; and enormous perplexities reside therein, foremost of which is simply how to verbalize speculations on what such a bond might consist in that are even intelligible, nevermind true. But two better-focused questions are also salient. The first expands upon the point noted earlier (p. 31a) that inductive accessibility of SLese-format laws seemingly requires causal transductions to issue from couplings of universals at the level of regular variables--i.e., sets of contrastive causal characters that are ontologically parallel, not derived some from others by negation--rather than from aggregation of disparate input/output connections among particular values of these variables. Leading off how that can be is the more palpable question of what achieves the mutual exclusivity in a variable's array of regular values: Why is an object that has one specific height, or surface color, or number of legs, or electrical resistance, or mass, or etc. incapable of simultaneously having some other height, surface color, etc. as well? Arguably, we sometimes--always?--contrive disjointness in such property arrays by how we superveniently construct them from others that are not similarly contrastive. (Cf. Rozeboom, 1958, on color incompatibilities.) But where that is so, we are thereby urged to spell out the nature of these underlays and their sub-SLese regularities that manage somehow to coalesce into functional laws closer to human comprehension; and where it is not, the problem remains of making ontological sense out of parallel incompatibilities in event characters.

Secondly--and finally for this survey of causal ontology--acknowledgment that causal production is generally control of an output character by a multiplicity of inputs working together in creation of their effect, despite their being distributed over diverse loci that may even be transfinite in number, should make us curious how causal transduction can ever manage to harness influences from many scattered sites and extract from them a collaborative resultant. One answer--indeed, the only mechanism I can conceive that is not supervenient upon lower-level compilations of this sort, albeit that may only attest my poverty of imagination--is for production of $\ulcorner \underset{\wedge}{y};\underline{o}_{m+1}\urcorner$ by $\langle\ulcorner \underset{\wedge}{X}_1;\underline{o}_1\urcorner,\ldots,\ulcorner \underset{\wedge}{X}_m;\underline{o}_m\urcorner\rangle$ under transduction $\underset{\wedge}{y}(\underline{o}_{m+1}) = \phi(\underset{\wedge}{X}_1(\underline{o}_1),\ldots,\underset{\wedge}{X}_m(\underline{o}_m))$ not to involve any genuine interaction among antecedent events $\{\ulcorner \underset{\wedge}{X}_i;\underline{o}_i\urcorner\}$ at all; but rather, for each $\ulcorner \underset{\wedge}{X}_i;\underline{o}_i\urcorner$ to impart a certain $\underset{\wedge}{y}$-tendency $\hat{\underline{y}}_i = \hat{\phi}(\underset{\wedge}{X}_i(\underline{o}_i))$ to $\underline{o}_{m+1}$, independently of all other events $\ulcorner \underset{\wedge}{X}_j;\underline{o}_j\urcorner$ ($\underline{j} \neq \underline{i}$) not mediating the effect of $\ulcorner \underset{\wedge}{X}_i;\underline{o}_i\urcorner$ upon $\ulcorner \underset{\wedge}{y};\underline{o}_{m+1}\urcorner$, under a tendency transducer $\phi_i$ selected by some precondition $\gamma_i$ relating $\underline{o}_{m+1}$ just to $\underline{o}_i$, while $\underset{\wedge}{y}(\underline{o}_{m+1})$ is then simply the sum (under some concatenation operator that is not necessarily arithmetic addition) of all these parallel tendencies $\{\hat{\underline{y}}_i\}$ accumulated at $\underline{o}_{m+1}$. I have studied the mathematics of concatenative laws at some length in Rozeboom, 1978, while arguing there that certain prevalent styles of substantive theory construction largely compel presumption that the laws they seek have indeed such a concatenative composition. (Specifically, this holds when the output variable is hypothesized to be affected in the same fashion by all variables in a class of inputs having unspecified cardinality.) Be that as it may, the suggestion to be taken here is simply that deeper analysis of conjoint causality may well place rational constraints on the transducers we can expect in laws that govern phenomena of certain common formal kinds. Whether these constraints are inconsequential metatheoretic curiosities, or instead offer serious guidance to substantive science, is for pursuance of the inquiry to reveal.