CHAPTER 2. INTERLUDE: THE PRACTICING OF PREACHMENTS.

Chapter 1 has set out the basic grammar of SLese, culminating in canonical law-form (8'), or (10') if we want to make explicit the provision for residuals and indeterminacy that is already implicit in (8'). Before turning to SLese's more advanced systemizations, we had best pause to take stock of what has so far been accomplished, what is still needed to make clear SLese's full force, and what import these metatheoretical abstractions have for the practice of psychonomic science.

Formulas (8'/10') and their notational variants have become so commonplace in the literature on which behavioral-science students are trained that without risk of serious dissent I could simply have declared at the outset that search for generalities subsumed by this form is the official aim of psychonomic science. And to have done so would have been to prattle pointlessly in platitudes. A wealth of sophisticated epistemic technology, opening into a glory of frontier problems in the philosophy and practical methodology of science, is packed into schema (10'). But none of that is <u>manifest</u> in (10'); rather, such equations are just pegs on which to hang an understanding of scientific lawfulness that is currently accessible only from apprenticing in specialist enterprises that variously exercise its fragments. A broad spectrum of these specialties exists today, ranging from study of particular regularities in numerous branches of substantive science (whose SLese quality varies from edifying in physics to rather less than that in psychology), through applied metasciences of which systems engineering and the methodology of data analysis & research design are most noteworthy, to more academically abstract grapplings with lawfulness in the large by general systems theory (e.g., Klir, 1972; Mesarovic & Takahara, 1975) and philosophy of science (e.g., Mackey, 1974; Causey, 1977; Skyrms, 1980). But none of these exercises more than a moiety of SLese's full gamut; and each has suffered needless impairment of proficiency at its own game (excepting physics and physical engineering?) through insufficient articulation of its position within the whole.[7] To optimize the efficiency with which we detect and exploit

[7] For whatever it is worth, I should record that the account of SLese sketched in this essay has emerged from my work in four problem areas that have been nearly disjoint in acknowledged overlap: (1) applied multivariate methodology and measurement theory (e.g., Rozeboom, 1966a, 1966b, and recent unpublished monographs on multivariate causal models); (2) substantive behavior theory (e.g., Rozeboom, 1970 pp. 103-157, 1974); (3) the philosophy of causality, conditionality, and semantics of theoretical concepts (e.g., Rozeboom, 1971, 1973), and (4) the psychonomic nature of cognitive structure (e.g., Rozeboom, 1969, 1974 p. 234f.). In each of these areas I have found state-of-the-art awareness of causal complexity to be cripplingly curtailed; and it has been my prolonged effort to break through the conceptual fog on each of these fronts that has enabled me, finally, to perceive SLese's quintessential features and to marvel at its reach.

lawfulness, apprenticing in extant applications is not enough, especially in the behavioral sciences; an explicit metatheory of the conceptual machinery and open issues underlying the surface show of formulas like (10') is also badly needed.

The present essay seeks to communicate three main facets of SLese appreciation, with the immediate applied goal of urging upon psychonomic science the importance of taking SLese seriously. These points are: (1) the deeper logic of lawfulness and how SLese domesticates its feral complexity; (2) what SLese is good for, i.e., the enormous power of explanation/prediction/control that advanced sciences can achieve by integrating ensembles of data and functional laws through SLese's special formalisms; and (3) why the language of any effective empirical science pretty well must embody some version of SLese, and hence why any attempted accounting for some received natural phenomenon has little chance of significant success unless it not merely talks a simulacrum of SLese but genuinely thinks that way. The still-to-come development of point (2), which overviews the advanced theory of causal structure, is perhaps too technical for readers unskilled in mathematical formalisms to follow in detail. So before I lose much of my audience, let me reprise what has so far been accomplished and preach a bit of morality.

According to the account developed here, any explanatory law describes a situation having five distinctive ingredients: (1) an open class of objects whose instantiatings of certain specified properties are in some sense responsible for certain other objects being the way they are in certain specified respects; (2) an output variable (i.e., set of contrastive attributes); (3) a tuple of conjoint input variables (generally including a "residual" component that treats as supplementary input whatever the well-described input leaves unaccounted for in the output); (4) a transducer ϕ that converts each alternative configuration $\underline{X}$ of input attributes, when realized under certain preconditions, into determinately placed realizations of output alternative $\phi(\underline{X})$; and (5) a set of transduction-enabling domain preconditions that includes a <u>locus structure</u>. (This last locution, you will recall, recognizes that when events $\ulcorner \underset{\wedge}{x}_1;\underline{o}_1 \urcorner, \ldots, \ulcorner \underset{\wedge}{x}_m;\underline{o}_m \urcorner$ jointly bring about event $\ulcorner \underset{\wedge}{y};\underline{o}_{m+1} \urcorner$, the bearers $\underline{o}_1, \ldots, \underline{o}_m, \underline{o}_{m+1}$ of these ϕ-coupled attributes generally differ from one another inter alia by displacements in space/time, so that the principle under which this complex of input events determines this output must include among its preconditions some rather strong constraint on how loci $\underline{o}_1, \ldots, \underline{o}_m, \underline{o}_{m+1}$ are related.) Moreover, I have not simply arrogated this five-point thesis but have tried to bring out how technical sciences are largely <u>compelled</u> to view lawfulness this way by the logical character of principled explanation and our practical exigencies of inductive inference.

We have further noted that any variable $\underset{\wedge}{z}$ acknowledged as input or output by a scientific *law may well be abstractively or translocationally derivative from one or more other variables--which is to say that the reality of $\underset{\wedge}{z}(\underline{o}) = \underline{z}$, i.e. of object $\underline{o}$'s having value $\underline{z}$ of variable $\underset{\wedge}{z}$, may lie in $\underline{z}$'s demarking either some molar summary of $\underline{o}$'s properties of certain more basic sorts (i.e., a-derivation) or some attribute of an object distinct from $\underline{o}$ but identified by definite description in relation to $\underline{o}$ (i.e. t-derivation). Why SLese should find a-derivation useful will become plain only later. But we have already seen how t-derivation allows any *law to be written with a manifest same-object locus structure. That is, t-derivation is the formalism by which we gain access to canonical law-schema (8'/10') and from there to the system integrations this makes manageable.

At first blush it seems fatuous to report that schema (8') is contrivably universal for talk of lawfulness when the behavioral sciences have seldom questioned this in the first place. But the operative conclusion here is that same-object format must be earned, not taken for granted. Every *law has a more or less complex locus structure; and if for good technical reasons we prefer not to make this formally explicit in the manner exhibited by (9'), then we must perforce embed it in the t-derivational character of the *law's variables. What this means in practice is that when we seek to clarify our conception of some initially vague same-object *law L, our task is only well-begun when we have detailed what attributes are the values of L's pre-scaled input/output variables and what transducer we conjecture may connect these. We must further set out what sorts of objects are in L's manifest domain D, i.e. how they are alike but also what distinguishes one from another, and how D-objects are related to whatever individuals are the real loci of events subsumed by L. As shown, e.g., by the obscure temporal boundaries of the person-stages to which we commonsensically ascribe such attributes as bodily measurements, test scores, stimulations (e.g., being-exposed-to-a-flashing-red-light), cognitions (e.g., seeing-that-it-is-raining, planning-tonight's-party), and actions (e.g., turning-left, campaigning-for-mayor), we generally have great latitude in how we specify L's domain objects, after which it still remains to choose some translocational assignment to these of the attributes we suppose to be governed by L. (Thus, what we might mean by 'John's weight today' tolerates many variants of interpretation if John-today is 24 hours thick; and these still remain open, even if less intuitively so, when John-today is restricted to John's instantaneous stage at high noon.) How we exercise these options can make considerable difference for the truth of L. Nor can we afford to exercise them whimsically; rather, a research area's domain definitions must be carefully standardized if it hopes to systematize an ensemble of local laws in explanation, e.g., of its process phenomena. For the domain of a causal system is the intersection of the system's local-law domains, and is hence vacuous if the latter have no objects in common--as obtains, e.g., if the objects in one local domain differ in temporal width from those in another.

To be sure, pressure to precision in our conceptions of domain objects and locus structure arises only when we undertake actually to verbalize some conjectured *law. That brings us to sermon time.

On taking Ideal Science seriously.

Early in this essay I took pains to argue that ideally, a working science is a corpus of published sentences, the core of which contains primary *data whose predicates define the science's primary variables, and primary *principles (*laws) hypothesized to account for them. Does this seem too truistic to need saying? Indeed it should; yet the softer sciences often violate this truism in practice. In particular, large sectors of molar psychology scarcely ever verbalize in print actual instances of their primary predicates, much less contrast sets of these more refined than default-binaries or conjectured *laws that might govern their instantiations. Much of psychology--especially cognitive psychology, on which I shall eventually concentrate--is a corpus without a core, a superstructure that scarcely ever touches ground.

At first shrug this charge seems absurd, considering our annual flood of empirical research. But the variables on which psychonomic studies report hard data are seldom of interest in their own right; rather, they are putative diagnostic indicators or distal sources of what is our primary concern, namely, something that mediates between environment and the organism's overt behavior and, in cognitive psychology, is known to us mainly through commonsense mentalistic folklore. Psychology's focus on the inner organism is our prideful birthright; but by the same token, overt measures are not our primary variables, and data on the former do not translate into probable *data on the latter unless we specifically articulate the inference--which for basic events simply does not get done in cognitive psychology. Moreover, research reports usually mumble their SLese even when speaking of observations. It is hard to cash out SLese formalisms by linguistically well-formed sentences in empirical applications, especially in the fashion on which system integrations can be built. Why this is so can best be appreciated from a simple physics example that is utterly typical in the respects at issue.

Why SLese is easier to fake than to practice.

Imagine the following experiment, which could easily be done as a classroom demonstration: Attach a tall ringstand pole to one edge of a long table, and to the pole clamp a bare unfrosted lightbulb in such fashion that sliding the clamp along the pole alters the bulb's elevation but always positions its filament directly over a fixed point on the table. Next, mount an 8 inch pencil vertically on the table 12 inches from this fixed table point and suppress all appreciable illumination in the room except from the movable bulb, which remains lighted and hence causes the pencil to cast a shadow on the table top. Finally, repeatedly alter the bulb's elevation and, after each adjustment $\underline{i} = 1,\ldots,100$, say, measure and record both the bulb filament's distance in inches from the table top ($\underline{x}_i$) and the length in inches of the pencil's shadow ($\underline{y}_i$). This gives you a trial sequence $\langle\underline{x}_1,\underline{y}_1\rangle,\ldots,\langle\underline{x}_{100},\underline{y}_{100}\rangle$ of measurement pairs in which, if you have been reasonably careful in technique and have included only lightbulb elevations under which the pencil's shadow does not extend beyond the table, each $\underline{y}_i$ differs only trivially from $96/(\underline{x}_i - 8)$.

Were you actually to make these observations, not just imagine doing so, you would have little doubt that a causal law is at work here, one that can prima facie be expressed in form-(10') ellipsis as

$$\underline{L}_{ps}: \qquad \text{In } \underline{D}_{ps},\ \underline{y} = 96(\underline{x} - 8)^{-1} + \underline{e}\ ,$$

wherein $\underline{e}$ is a negligible residual. (Subscript 'ps' is short for 'pencil shadows'.) Your recorded numbers $\langle\underline{x}_i,\underline{y}_i\rangle$ ($\underline{i} = 1,\ldots,100$) scale the values of certain variables $\hat{x}$ and $\hat{y}$ for a sample of individuals from the domain $\underline{D}_{ps}$ of $\underline{L}_{ps}$; <u>but what, in words, are these variables and the observed $\underline{D}_{ps}$-objects</u>? What I am asking you to do is to fill the blanks in

> On the $\underline{i}$th trial of this experiment, I observed that ___ had value $\underline{x}_i$ of variable ___ and value $\underline{y}_i$ of variable ___

in such fashion that, after paraphrasing into ordinary English, the result would be admissible as statement of fact in a court of law. (The paraphrase wanted is conversion of your completion of '___ has value x_i of variable ___', and similarly for y_i, into an idiomatic subject/predicate clause.) And I urge that you *really* try to do this, else you will not appreciate the operational difficulty of this verbalization.

. . .

Now that you are back, I venture that your idiomized sentence completions have probably exploited my own description of this experiment to read something like

> On the ith trial in this experiment, the lightbulb was x_i inches above the table and the pencil shadow was y_i inches in length.

This suggests taking $\underset{\wedge}{x}$ and $\underset{\wedge}{y}$ in L_{ps} to be translocations of $\underset{\wedge}{x}$': *Elevation-in-inches-of-*__*-above-*__ and $\underset{\wedge}{y}$': *Length-in-inches-of-*__, respectively. But even if we accept that descriptors 'the lightbulb', 'the table', and 'the pencil shadow' succeed at unique reference in some contexts, and moreover that 'the ith trial in this experiment' provides such a context, that still identifies neither just one D_{ps}-object to be your manifest locus of the ith trial's instantiation of L_{ps} nor a translocation function that maps this object into the ith trial's lightbulb, table, and pencil shadow, respectively. Shall we simply assume that *something* answers to the description, 'the ith trial in this experiment', and contains the ith trial's lightbulb, table, and pencil shadow as uniquely identified parts? Or shouldn't we prefer to appoint something less ontologically nebulous--say, the region of spacetime within the experiment's room over the smallest continuous time interval that spans both acts of measurement on the ith trial--to be the common manifest locus of the ith trial's events?

There are many ways in which 'The D_{ps}-object on trial i of this experiment' can be assigned a specific referent with associated lightbulb, table, and pencil shadow for each $\underline{i} = 1,\ldots,100$. Which one you choose does not much matter, at least not initially, so long as you do choose one and moreover do so in a way that in principle includes other objects as well. For if $\underline{L}_{ps}$ is to have any generality beyond just your experiment's 100 trials, whatever specific meaning you give to predicate '__ is a $\underline{D}_{ps}$' must have an open applicability under which, at other times and places, it makes sense for you to be asked, and for you possibly to affirm, 'Is this an object of kind $\underline{D}_{ps}$?' when 'this' is a contextually meaningful demonstrative or description. You may not be able to answer confidently for a particular this about which relevant information is lacking; but if neither direct perception nor verbal information ever enables you to judge whether this is a $\underline{D}_{ps}$, you cannot assert $\underline{L}_{ps}$ simply because you have no conception of its domain or locus structure.

Suppose, however, that you have indeed given suitable meaning to '__ is a $\underline{D}_{ps}$'. If the perceptual and/or verbalizable meanings the other relevant concepts in this example have for you insure that every $\underline{D}_{ps}$-object contains or is otherwise translocated into a unique lightbulb, table, and pencil shadow, then $\underline{L}_{ps}$ is a meaningful *law for you even though it may not be a true one unless you have included in your $\underline{D}_{ps}$-concept this experiment's relevant constancies of pencil length and placement, table size, lower bound on lightbulb elevation, etc. But how wide a scope does that give $\underline{L}_{ps}$? Clearly $\underline{L}_{ps}$'s domain is so narrow, containing in fact so few objects beyond the 100 in your experiment, as to be virtually worthless except insofar as it can be expanded into a broader-scope law of shadows. Some domain expansion can be achieved just by eliminating irrelevancies such as pencil color, table material, ringstand support, etc. from the definition of $\underline{D}_{ps}$. And other constancies in $\underline{D}_{ps}$ (e.g., length and placement of pencil) are values of variables that conjoin $\hat{x}$ to determine $\hat{y}$ in a domain $\underline{D}'_{ps}$ much broader than $\underline{D}_{ps}$ under a transducer that can also be identified empirically with ease. But we soon get into trouble if we try to expand $\underline{L}_{ps}$ into a law of shadows for a domain whose members do not each contain (or translocate into) a unique lightbulb, table, and pencil shadow. So long as we don't mind replacing variables $\hat{x}$ and $\hat{y}$ by other regular variables of which these are domain restrictions, the pencil can be generalized to opaque objects of many sorts, the table to any flat supporting surface, and the lightbulb to any point-source of strong light. But for situations containing multiple light sources, or several shadow-casting objects, or shadows that are not confined to one planar surface, the commonsense conceptual apparatus that works so well for $\underline{L}_{ps}$ and its simpler domain expansions becomes hopelessly inapplicable. The same molecular principles of lighting that underlie molar law $\underline{L}_{ps}$ also apply to the most general situations of distributed illuminations and opacities; but $\underline{L}_{ps}$ and its easy expansions neither subsume more than a vanishingly small proportion of the latter nor give us much clue to what variables most usefully characterize the input/output alternatives in a more fundamental law of lighting.

Stability: The elusive SLese imperative.

The double point of this example is not only that we can collect, analyze, and report observations with exemplary precision about the values of data variables while remaining unconsionably vague about what variables and objects these are values on and for, respectively, but also that regardless of how precisely we have identified the domain and locus structure of an empirical law, its scope is almost always deficient in a crucial respect that our Shadows illustration has not yet made completely clear. What is objectionable about the narrow domains of empirical *laws, i.e. ones that literally subsume the sample data we cite in their support, is not that they fail to include a large proportion of everything there is--no scientific law does that--but that they are severely unstable in the following important sense: Almost every causal object o, i.e. one that is the real or manifest locus of causal events, is closely succeeded by certain other causal objects {o'} whose properties derive more strongly from those of o than from those of any other object that does not mediate the succession from o to o'. (Typically, o and o' are stages of the same enduring thing with o' a little later than o.) Then a domain D of causal objects, and likewise any *law or variable whose domain is D, is "stable" iff almost all the close successors of objects in D are also in D, and is "unstable otherwise, i.e. if the close successors of D-objects are often not in D. (Evidently, domain stability/instability is technically a matter of degree. But for metatheoretic discussions, the loose dichotomy generally suffices.)

When in real life we classify things according to their "sort" or "natural kind," our intent is first of all for natural kind D to be stable--as needed for our information that o is a D to remain relevant when, seconds, hours, or days later, we are dealing with o's successors--and secondly for D to be the domain of a recursive system of laws--stable laws--which iteratively account for processes in sequences of a D-object's successors. (No domain stability, no discernable process regularity.) Pending later development of this point's details (p. 70ff.), it is fair to claim that laws and regular variables having stable domains are far more central to systematized science than are unstable ones. But empirical laws and (regular) observation variables are almost always in practice unstable, insomuch as their domain preconditions seldom arise except by special contrivance and are ephemeral even then. Consider, e.g., how rarely the preconditions of L_{ps} come together even by human design much less by happenstance, and how quickly they become disrupted. And how many data variables do you personally know of in research practice whose values are not operationally defined relative to some instrument or circumstance whose applications are intermittant and fleeting? Arguably, the empirical regularities that we can find so perceptually striking under careful domain preparations are molar abstractions from underlying complexes of stable molecular laws (see p. 101ff., below). But the salient epistemic conclusion remains that insomuch as the variables in empirical *laws on which we report data in real-world research almost always have unstable domains, variables governed by the stable laws we yearn to identify are perforce theoretical entities that generally do not even distribute over the same domains as do the data variables from which we infer them. So even where we have defined our observational predicates to the highest standards of scientific clarity, we can and prevailingly do still leave obscure not merely the value alternatives for the more stable variables of which we take the former to be manifestations but even the objects that have these inferred properties.

In psychology, the classic illustration of our yen to move beyond unstable data variables to stable theoretical ones contrasts observed scores with "true" scores on psychometric tests. An enduring person _s_ has an actual ("observed") score on a given test _A_ only at times when _s_ is actually tested on _A_, at least if we follow standard practice in restricting the observed-score variable's range to regular values. (Including a _Not-tested_ alternative in the variable's range pretty well precludes its participation in functional laws whose domains accept instantiations of this anomaly.) Broadly speaking, testing _s_ on _A_ consists of coupling s with an A-instrument over some time period t of appreciable duration at whose termination changes in the instrument accrued during the coupling are given a numerical rating and taken to be the observed-A score either for the segment of s during t or--custom is very vague about this--for some broader or narrower temporal chunk of s derived by translocation from testing period t. Since no person-stage is in the observed-A variable's domain unless it is so tied to an A-test outcome, this variable is unstable in the extreme. Yet we take our conception of A-testing also to demark a stable underlying "true-A" variable whose domain includes all instantaneous person-stages whether included in an A-tested segment or not, and which is identified for us by the causal role we ascribe to it in a theory primarily of how A-outcomes are produced under A-testing circumstances but also, eventually, of how true-A relates in stable laws to other trait dimensions underlying person behaviors of many kinds in many circumstances. So conceived, true scores are typical instances of the "dispositions" to which commonsense and technical sciences alike so frequently appeal for explanations of an object's transient properties, and whose much-debated ontology/semantics/epistemology is impractical to review here. (However, see Rozeboom, 1973, 1984.) True scores and other paradigmatic dispositions are by no means the only theoretical attributes we infer from suitably patterned data (cf. Rozeboom, 1972), but on the other hand, as illustrated by our Shadows example, neither do all empirical phenomena urge theoretical interpretations upon us. For the most part, unstable empirical laws appear to be of scientific interest precisely to the extent that they give us an inductive handle on stable explanatory variables governed by laws that are likewise stable.

Explicit identification (or at least posited *identification) of stable variables over instantaneous person-stages, first of all by inference of true scores underlying observable outputs on publically well-defined testing instruments and from there to inductive specification of other variables over this same domain by factor analytic techniques, has allowed "trait" theories of human personality & abilities to be one sector of modern psychology in which SLese formalisms are given honest content. (See e.g. Cattell, 1979.) Regardless of reservations one may have about the profundity of psychonomic wisdom achieved by trait research, this is the material in which abstract quantitative models of causal recursion and system dynamics seeking psychological applications find their most comfortable embodiment. Only in the behavior systems of Hull and Tolman do we find comparably explicit verbalization of stable *laws conjectured to govern determinately specified stable variables. Admittedly, mid-Century behavior theory has needed a bit of help from its friends (notably, MacCorquodale & Meehl, 1954; Rozeboom, 1970 p. 103ff.) to polish up its sterling SLese qualities, and various more recent developments that find the label 'mathematical modelling' congenial are often not far behind this in SLese honesty. Even so, I find that when I want a paradigm of what it is for a psychological theory of some intricacy to be well-SLesed, I can distill this out of the Hull/Tolman legacy with far less estrangement from the original texts than I can bring off elsewhere.

Defining variables wholesale.

Before resuming decrial of cognitive science's prevailing SLese poverty, it is only fair to acknowledge a special problem that molar psychology has in this regard. Abstractly, it seems as though an ideal science Σ should be able to itemize its content by first listing its primary variables; next, for each primary variable $\underset{\wedge}{y}$, writing down the primary *laws judged to be worthy conjectures about $\underset{\wedge}{y}$'s production in whatever restrictions of $\underset{\wedge}{y}$'s domain seem cogent; next, applying Step One followed by Step Two to all secondary variables in Σ's primary *laws for which Σ also accepts explanatory responsibility, and so on. Unhappily, none of this is strictly feasible in molar psychology for the simple reason that the number of entries called for on each list is infinite or at least indeterminate. To illustrate, behavior theory's inventory of achievement respondings includes all Approach variables of form $\underset{\wedge}{r}_Q$: *Degree-of-movement-toward-Q*, where 'Q' specifies by position or content some theorist-selected place in the organism's environment. Generally such an $r_{\wedge Q}$ is unstable, having as its domain only those organism-stages s-at-t to which Q is present; but lacking strong constraints on Q that behavior theory has never sought to impose, not merely are these $r_{\wedge Q}$-variables transfinite in totality, but any one s-at-t is in the domain of an infinite subset of them. (A molecular description of behavior as muscle-fiber movement, on the other hand, can presumably get by with only finitely many response variables.) Achievement respondings and almost all other basic variables of molar psychology can be denoted only by naming an open category that comprises them--e.g., the class of Approach variables--without identifying each instance individually.

Open categories of variables, and *laws to govern them, are no insuperable obstacle to hard science so long as we identify these by verbal schemata that yield well-specified variables/*laws of these kinds when suitably completed, and accompany each schema with operational rules for generating its completions together with enough specific instances thereof to convince us that this schema-cum-completion-rules really does give us roughly what we want. (It is best to begin with schemata that generate regular values of the intended variables, and decide afterward how to group the basic predicates so schematized into contrast sets.) 'Degree-of-movement-toward-Q', which envisions collecting into variables the array of Approach properties demarked by predicate schema '___ is moving in-fashion r toward Q', is an intermediate-quality illustration whose schema is far more specific in denotation than is just the label 'Approach behavior', but still needs to clarify both its intended range of 'Q'-instantiations and what particular movement alternatives are to be these variables' values. (Thus, 'its mother', 'the water cup', and 'the red wall' are presumably admissible for 'Q', whereas 'its mother-one-hour-later', 'a water cup', and 'redness' are dubious. And for describing s's movement at t with respect to an axis from s-at-t to location Q, do we put this axis through, say, these objects' centroids rather than their points of nearest proximity; do we look at movement just of s-at-t's centroid or are we to include also the differential Q-ward motions of s's various body parts; and is "degree" of movement just velocity along the direct-approach axis or do we want a richer vectoral description of the motion relative to this axis?) We can never be completely specific about such details, of course, anymore than perfected precision is ever humanly attainable. But for our conception of an open category Y of variables to embody some genuine grasp of what we are professing to talk about in this way, we need to verbalize a few particular values y of some specific Y-instances y, and then carefully think through what predicate '___ has value y of y' or its idiomization means to us: If it is perceptual, how and to what do we apply it with what apparent degree of intra/interpersonal reliability; if it is theoretical, to what other observational/theoretical predicates do we link it by what conjectured generalities. Unless Y-instantiating is already well-practiced, our first conscientious encounters with these particulars will reveal hitherto unrecognized obscurities, both conceptual and factual, whose fathoming is crucial to this topic's progress. Whereas if we cannot or will not confront even a few determinate predications under category Y, then talk of Y-phenomena may be welcome in the Arts as imaginative literature but as Science is pretentious sham.

The pathos of unSLesed psychology: Two examples.

Modern cognitive psychology teems with reference to abstract categories of variables, or of attributes, or of processes and phenomena in which values of basic cognitive variables presumably figure somehow, whose instances are never made explicit either by thoughtful examples or by schemata-*cum*-completion-rules. Most of these are mentalistic categories under which ordinary language provides a disorderly abundance of particulars. But the rare, refreshing examples of everyday usage that occasionally surface in the technical literature no more suffice to clarify cognitive psychology's basic variables than everyday talk about soil and rocks is good enough to ground a science of minerology. I find it impossible to communicate a sense of this void in general terms, much less any feeling for why it matters, but perhaps I can exhibit some of this through an example or two.

Consider the term 'information', possibly the singlemost prevalent noun in cognitive psychology. Where, I ask you, have you ever seen in this literature any sentence, or schema thereof, that predicates of particular objects some condition, or change of condition, that is paradigmatically informational? In contrast, ordinary-language information talk is free with specifics such as

	Mary	informed		that his house was on fire.
i)	The phone call	told	John	about the fire.
	Smoke over his roof	alerted		of his problem.
ii)	The letter contains the information			that Mary won 1st-class honors.
		possesses the information		that Mary won honors.
iii)	John	is informed		about Mary's success.
		has learned		about Mary.

in which the square brackets indicate approximate synonymies. This array illustrates that ordinary language speaks of "information" in a diversity of grammatical contexts, some of which however are evidently based on others. Although careful analysis of these variations would be digressive here, I invite your assent to the following conclusions, some of which are more debatable than I let on:

1) As a verb, "informing" is a process wherein a consequent condition of a rather special sort is brought about by some not-so-special antecedent. Indeed, the leftmost bracketing in sub-array (i) indicates that ordinary language puts little constraint on what can be a manifest informer so long as it goes proxy for an event having the right sort of cognitive consequence. (Thus, 'Mary informed John that *p*' is short for something like 'Mary's announcement brought about John's awareness that *p*'.) If so, something's being in an informational state is ontologically more basic than the process of informing even though the concept's root lies in the verb.

2) Ordinary language distinguishes a vast plurality of particular informational states that can be predicated of things; however, these do not form contrast sets (unless we insist--dubiously--that information is always veridical) but partake instead of a loose partial order in which some derive analytically from others. Thus, John's being informed that Mary won honors entails his being informed about Mary's success which in turn entails his being informed about Mary. Each basic commonsense informational state, i.e. one that is not evidently an analytic abstraction from another, has an *intentional content* characterized by a declarative proposition in the same still-obscure way that classical mental acts of believing, hoping, remembering, etc. have propositional contents. But precisely *what* is characterized propositionally in an informational state, and *how*, commonsense leaves largely enigmatic.

3) Ordinary language admits two fundamentally different kinds of informational states, those of *cognizers* on the one hand and of noncognizing *carriers* on the other. This contrast shows forth in (*ii*) vs. (*iii*): An enduring document (book, drawing, magnetized tape, etc.) or transient stimulus (acoustic waveform, light display, etc.) can contain (convey, store, etc.) the information that-*p* which John possesses, but John does not commonsensically contain that-*p* anymore than the document possesses it. (That is to some extent an overstatement, since conceivably John's-possessing-the-information-that-*p* might carry the information that-*p* to another cognizer. But even here the commonsense distinction persists between carrying information and cognizing it.) And although a document can inform John that *p*, John cannot so inform the document even though he can record that-*p* in, or convey it by, the document. Arguably (cf. Dretske, 1981, to the contrary) an object *o*-at-*t* *carries* the information that-*p* only relative to one or more cognizers *s*-at-*t*' by virtue of *o*'s having some configured property *P* at *t* such that under a suitable coupling of *o*-at-*t* to *s*-at-*t*', *o*'s-having-*P*-at-*t* would cause cognizer *s*-at-*t*' to learn-that-*p*, believe-that-*p*, or perhaps ø-that-*p* for some other mental-act mode ø which ordinary language is

willing to negotiate. (Commonsensically, becoming informed-that-p requires an onset of thinking-that-p paradigmatically but perhaps not essentially as a result of perceiving a carrier in circumstances that make that-p prima facie beliefworthy; but whether this thinking-that-p must further be an uninhibited believing-that-p is not intuitively clear.) But not all causal antecedents of a learning-that-p intuitively carry the information-that-p, albeit commonsense remains exceedingly unclear about which ones do and which ones do not. Thus when the smoking of John's roof informs him of the fire, commonsense is not insistent even that the roof's smoking contains this information, at least not fully, much less that more remote causes of the roof's smoking and hence of John's being informed of the fire (e.g., the overheating of John's chimney) are such carriers.

The salient point to take from this everyday usage is that "information" predications are flagrantly non-basic even while ordinary language gives us little clue to what *are* the more basic relational and nonrelational events from which informational conditions abstract. To say that *o*-at-*t* carries information about *Z* is presumably to assert that there is some proposition that-*p* that is both carried by *o*-at-*t* and makes reference to *Z*. This existential quantification is conceptually honest enough even if it does confront us with the persisting mystery of what it is for a proposition that-*p* to be about *Z* rather than about something else or nothing at all. But when *o*-at-*t*'s carrying the information that-*p* begins to analyze in turn as *o*-at-*t*'s having some property *P* under certain circumstances wherein this *P*-event is disposed to bring about certain ~~ø~~ings-that-*p* in cognizers appropriately related to *o*-at-*t*, ordinary language is altogether lacking in examples of the requisite circumstances/relations/etc. Some specific instances leading to some provisional generalities in this regard is the least we can expect from a technical science that professes to study information processing.

So what do we find in the technical cognition literature? Here are three snippets from recent issues of *Psychological Review*:

> ... retinal image information is analyzed by parallel functional subunits ... [1982 p. 408].
>
> ... the most generally accepted conception of the [memory] trace is that of a collection of features or a bundle of information ... [1978 p. 62].
>
> ... visual perception involves the processing of information about objects and events at many distinct levels of the visual system ... [1980 p. 113].

The first and perhaps second of these are tolerable abstractions under the carrier sense of informational states. But what specific instances of carrier objects o-at-t and carrier attributes P do these envision? (Even the first case, which presumably intends its carrier to be the distribution of light intensities over a retinal surface at t, leaves open whether t is an instant or a duration.) And what cognizers s-at-t' is this o's-having-P-at-t apt to inform of what proposition that-p by virtue of what attendant circumstances? The same could be asked of the third quote, except that if multiple activities in s's visual system at t' are partly constitutive of s's being visually informed that-p at t', as seems rather likely, it makes no sense to think of these as ordinary-language information carriers. Needless to say, neither the articles here tasted nor the wider literature to which they belong give any specific examples of events that cognitive theorists propose are informational nor hint at principles, commonsensical or technical, under which anything warrants that classification. This is science???

One can argue that cognitive psychology's slovenly treatment of "information" does not much matter, precisely because this notion is not cognitively basic and could be striken from the corpus with little real loss of content. Experimentation with such deletions shows information-talk to be in fact more insidiously beguiling than you might expect. But as I will now also show, need to articulate basic variables arises even more urgently at the respectable end of cognitive theory.

Hockley & Corballis (1982, p. 190f.) nicely summarize a popular recognition-memory research design as follows:

> In [Sternberg's] paradigm, the subject memorizes a set of items and is then presented with one or more probe items, and the task is to decide whether

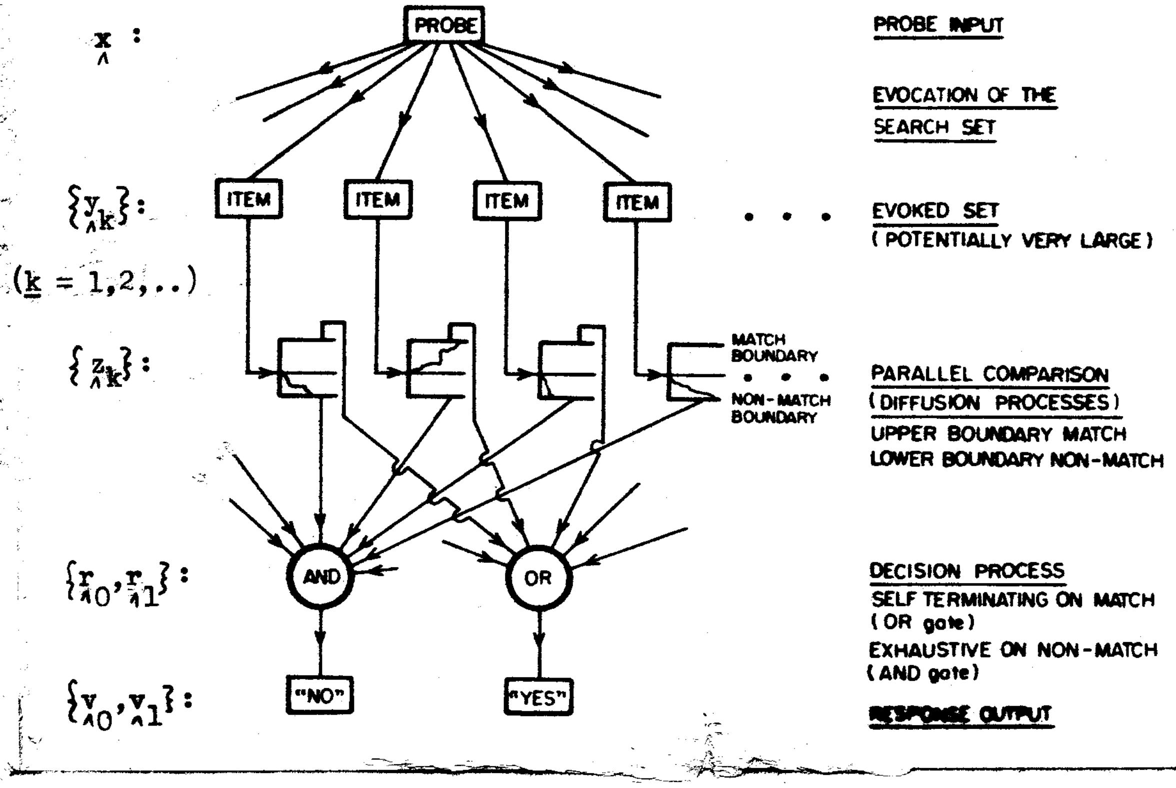

Figure 1. An overview of the item recognition model. [Ratcliff, 1978, p. 61]

> each probe is or is not a member of the memorized set. ... [Reaction-time data] suggest that the subject scans the set serially in order to determine whether it contains the probe. ... [Recently, Ratcliff has] proposed that the probe is compared in parallel with each item in memory [and the probe/item similarity] drives a diffusion process toward either a match or a non-match boundary.

At least three groups of variables are implicated here; (1) storage variables having relatively persistent values instated by item memorizing, (2) probe variables whose values are reset on each trial, and (3) probe/store-interaction variables whose moment-to-moment flux throughout each trial constitutes the recognition process. But how are these variables identified in this paradigm's literature? They aren't. The closest anyone seems to have come is in Ratcliff's Figure 1, reproduced here with an added column of cell labels on the left, wherein each cell can be taken to stand for a variable even though Ratcliff himself makes that interpretation clear only for row $\{z_k\}$.

Critique of this model requires a prefatory word about "items." Focal stimuli in memory research are spatio-temporal configurations of localized visual or auditory patterns (including a null or blank pattern as one alternative that may or may not deserve classification as "anomalous") of which alphanumeric "words" are most typical. These localized feature clusters, whose detailed problems of definition we ignore here, are <u>stimulus</u> <u>items</u>; and any internal conditions conceived to correspond with stimulus items in some fashion allowing us to think of them as "codings" or "representations" of their respective external counterparts are also "items" in an extended sense. Coding correspondence is only roughly one-one, first of all because any given *s*-at-*t* is unable to discriminate between some items, and secondly because we allow *s*-at-*t* to have available a multiplicity of representations for the same stimulus item, especially alternatives at different levels of degradation or "fade" as may be needed by theories of perceptual inattention and memory loss. How cognitive theory might individuate these internal codings when simple reference to their external counterparts does not suffice is evidently a question easier to evade than to answer.

In the cells-as-variables reading of Fig. 1, "probe" $\hat{x}$ is an external-input variable whose range is the set of logically possible stimulus items and whose value on a particular trial is a stimulus item presented by the experimenter (e.g., *s*-at-*t* is shown <u>CEP</u> vs. <u>DOG</u> vs. <u>372</u>, etc.) Alternatively, $\hat{x}$ can be replaced by a perceptual variable $\dot{\hat{x}}$, driven by the external probe, whose value at any moment (e.g., *s*-at-*t*'s perceiving <u>CEP</u>ly vs. <u>DOG</u>ly vs. <u>372</u>ly, etc.) is a coding of the external-probe item. (More generally, close study of input processing finds evidence for a succession of pre-perceptual and perceptual variables; but these need not always be made explicit, here in particular.) Each "evoked" variable $\hat{y}_k$ likewise ranges over item codings and, in immediate-memory applications of Fig. 1 (the version for long-term memory suggested by Ratcliff's reference to "evocation" is more complicated), has its value determined when the system is in learning mode by (say) the *k*th-from-last item in the sequence of $\hat{x}$-values just received. (Take care to appreciate that in this model, a temporal sequence of different values on a <u>single</u> input variable $\hat{x}$ or $\dot{\hat{x}}$ during learning is transformed into a synchronic distribution of values over an **array** of internal "item" variables $\{y_k\}$. When the system is switched to recognition mode by a context cue identifying the latest x-value as a "probe," values of $\{y_k\}$ remain constant except for retention degradation. System mode and its sources are additional variables not shown in Fig. 1. The dynamics of storage during learning mode is a tricky business which Fig. 1 does not touch.) In contrast to item-valued variables x (or $\dot{x}$) and $\{y_k\}$, each of Fig. 1's diffusion-process variables z_k is numerically scaled and, in recognition mode, changes autoregressively (cf. equation (11), below) as a joint function of x, y_k, z_k, and a random disturbance e_k until its value reaches either an upper or lower limit where it sticks until further notice. Finally, "decision" variables r_0 and r_1 are binary functions of $\{z_k\}$ that respectively register whether all of $\{z_k\}$ have reached their lower limits or one its upper limit; and v_0 (v_1) is a binary response variable that outputs vocalization "no" ("yes") or nothing according to whether r_0 (r_1) signals go or no-go.

As cognition models go, this one is more articulate than most. Yet it is still only that--a model sketch which says little that can really be believed about recognition, if only because it has not begun to suggest how its schematic fragment of memory theory can be realized within a view of the fully functioning organism. Though metatheoretically instructive, it is substantively trivial to demur that the "yes" and "no" boxes in Fig. 1 are better read as alternative responses on a single speech dimension (as Ratcliff likely intended) rather than as separate binary output channels, and that $\langle r_0, r_1 \rangle$ can just as well be fused into one trichotous variable. The model's most important estrangement from reality lies in its orthodoxly farcical treatment of stimulation. Any organism ***s*** receives far more input at any time ***t*** than just one stimulus item. Even in recognition research, for example, ***s*** can be shown two or more training and/or probe words at a time, and those scarcely begin to exhaust all the environmental features that impinge upon ***s***-at-***t***. How to handle this input abundance in SLese is a profoundly challenging problem. Most abstractly, we need an indexed array $\{x_i : i \in \mathbf{i}\}$ of stimulus variables such that all relevant input to ***s***-at-***t*** is characterized by ***s***'s configuration of values on $\{\underset{\wedge}{x}_i\}$ at t. If we want each input dimension $\underset{\wedge}{x}_i$ to be item-valued, we have no choice but to parse environment by an indexed set $\{f_i : i \in \underset{\sim}{i}\}$ of translocators over organism-stages that pick out regions of s-at-t's surround so differentiated that each f_i(s-at-t) is the site of just one stimulus item from the range of item alternatives. Then if $\underset{\wedge}{x}$ is the local-content variable that identifies stimulus items in sites thereof, we define each input dimension $\underset{\wedge}{x}_i$ to be $\underset{\wedge}{x}_i = [x f_i]$ ($i \in \underset{\sim}{i}$), i.e., s-at-t's input on channel $\underset{\wedge}{x}_i$ is the item present in region f_i(s-at-t) of its surround. From there, for the first stage of stimulation-as-received we envision an array $\{\dot{\underset{\wedge}{x}}_i : i \in \underset{\sim}{i}\}$ of item-valued perceptual or pre-perceptual variables such that the value of each $\dot{\underset{\wedge}{x}}_i$ for s-at-t is a coding of $\underset{\wedge}{x}_i$(s-at-t) more or less degraded by s's momentary attention coefficient at t for channel $\underset{\wedge}{x}_i$. As an alternative to $\{\dot{x}_i\}$, or as consequent upon it, we can try to describe post-attentive input by a much smaller array $\{\ddot{\underset{\wedge}{x}}_j : j \in \underset{\sim}{j}\}$ of item-valued perceptual variables whose cardinality reflects our intuitions about attention-span limits. An honest theory of $\{\ddot{\underset{\wedge}{x}}_j\}$, however, needs to include a mechanism for selecting which items in array $\{\underset{\wedge}{x}_i$(s-at-t)$\}$ or $\{\dot{\underset{\wedge}{x}}_i$(s-at-t)$\}$ are copied in what arrangement into $\{\ddot{\underset{\wedge}{x}}_j$(s-at-t)$\}$.

Acknowledging the fullness of input in Fig. 1 replaces $\underset{\wedge}{x}$ in this model by $\{\underset{\wedge}{x}_i\}$, $\{\dot{\underset{\wedge}{x}}_i\}$, or $\{\ddot{\underset{\wedge}{x}}_j\}$, which calls in turn for expanding $\{y_k\}$ into a doubly indexed array of item-valued memory-store variables $\{\underset{\wedge}{y}_{jk} : j \in \underset{\sim}{j},\ k \in \underset{\sim}{k}\}$ such that the value of $\underset{\wedge}{y}_{jk}$ for s-at-t is (or is a memory-loss degradation of) the value of perceptual variable $\ddot{\underset{\wedge}{x}}_j$ for s at some prior time related to index k. (Index set $\underset{\sim}{k}$ can be small for immediate memory, but for long-term memory must be very large.) And Ratcliff's recognition process then needs a diffusion variable $\underset{\wedge}{z}_{j'jk}$ for each combination of a perceived-cue variable $\underset{\wedge}{x}_{j'}$ with a storage variable $\underset{\wedge}{y}_{jk}$, augmented by an array of selection or modulation factors that manage by some to-be-devised mechanism to keep the match between non-probe input components on recognition trials (e.g. features of the presentation apparatus) and stored items additional to the designated target set (e.g. those same apparatus cues during learning) from making a shambles of the model's intended implications. Finally, when this expansion has been worked out for the system's behavior in recognition mode, it remains to consider what all these variables do when the subject is not performing on experimenter-controlled recognition tasks. By rights, *s* should continue to receive on $\{x_i\}$, perceive by $\{\dot{x}_i\}$ or $\{\ddot{x}_j\}$, and store in $\{y_{jk}\}$ at all waking times; but what happens to the $\{z_{j'jk}\}$-processes, their modulators, and their *r*-effects when *s* is eating breakfast, arguing with his roommate, or watching TV? Perhaps the domain-stability to which this model aspires is no greater than that of our Law of Shadows (p. 45, above), with most of its variables presumed to take regular values only under the ephemeral circumstances of laboratory memory experiments. But if so, what insight can any such model provide into the nature of domain-stable mechanisms of retention, recognition, and recall in ordinary life?

I hope it is clear that no derision of Ratcliff's model is intended here. Fragmentary as it is, his conjectured comparison mechanism is a useful SLese-style counterbalance to the serial-processing biases that have afflicted modern cognitive psychology.[7a] But it also nicely exhibits how distant even the good stuff

[7a]Ratcliff's 1978 paper was harbinger of a recently burgeoning trend in technical theories of perception and memory to relinquish serial-processing flow diagrams in favor of refurbished SLese conceptions of multivariate central processes. (See, e.g., McClelland & Rumelhart, 1981; Murdock, 1982; Anderson, 1983). So for hard-core psychonomic science, my jibes here and in Chapter 4 at doing psychology by computer metaphor may happily be at risk of losing their point.

in cognition theory remains from actually verbalizing specific variables and *laws to govern them, and moreover how even cursory inspection of this gap raises disturbing questions about whether it can be closed. You will surely have noticed that my description of input channels $\{xf_i\}$ has remained grandiosely schematic in both the stimulus items chosen for local-content variable x's contrastive values and the translocators $\{\underline{f}_i\}$ that pick out the sites of these items' external occurrences. If you try to flesh out this schema, you will find that you scarcely know how to begin--for good reason, since the notion of item-valued input variables may well be unrealizable for those stimulus items that are paradigmatic in molar psychology. It is easy enough to define translocators $\underline{f}_i$ that map each s-at-t into some nearby location of events generally having sensory impact upon s at t, even though domain-stability for these site-selectors is tortuous to contrive. (E.g., $\underline{f}_i$(__) might be cashed out as "The circular patch of opaque surface closest to ___ that is 4 inches in diameter and whose center is at solid angle [such-and-such] to the coordinate axes of ___'s head planes," which is domain-stable, or as "The video screen closely in front of ___," which is not.) But site $\underline{f}_i$(s-at-t)'s condition alternatives will have no domain-stable partition matching the "item" alternatives envisioned by memory research. Thus, if $\underline{f}_i$(s-at-t) includes a visual surface of appreciable size it can simultaneously display several different verbal-learning words, not to mention innumerably many other pigment configurations that are beyond the pale of orthodox memory research. And if molar input variables cannot be defined as item-valued without sacrificing all semblance of domain-stability, then neither have we reason for confidence that item-valued internal variables are a theoretical desideratum.

Here is a fundamental disanalogy between organisms and computers: Whereas the physical construction of computers makes it highly appropriate to view their assorted input/central-processing/output registers as variables whose content alternatives are physical codings for whatever "items" we appoint them to represent for us, the only thing comparable to computer registers in organisms are individual neurons or groups thereof. But there are no environmental registers whose content alternatives are the stimulus items of molar psychology, and we have only the computer metaphor to motivate thinking of an organic system's reception and retention of information as transference of coded/recoded molar stimulus items from one mental register to another.

Of course, if item-valued variables were molar psychology's only input option, we would simply have to confront their definitional difficulties as best we can. But in fact an alternative has long been standard in behavior theory. This is to treat each molar stimulus $\underline{S}_i$ not as one value of an input channel that ranges over different stimuli but as the variable itself or, rather, as an index thereof. (For behavior theory & conditioning research, a "stimulus" is essentially what an "item" is for memory theory.) In its simplest and most common version, stimulus-indexing of input associates each stimulus $\underline{S}_i$ with a binary input variable x_{S_i} whose value for any <u>s</u>-at-<u>t</u> is 1 or 0 according to whether $\underline{S}_i$ is or is not present to <u>s</u> at <u>t</u>. (Precise definition of "presence" is seldom attempted.) Or when $\underline{S}_i$ is an abstraction whose more determinate versions vary in intensity or degree, we can let x_{S_i} be a graded variable whose value for <u>s</u>-at-<u>t</u> scales the degree to which $\underline{S}_i$ is present to <u>s</u> at <u>t</u>. ¶ Unhappily, traditional dimensionalizations of input by stimulus indexing have failed to acknowledge that a stimulus $\underline{S}_i$ isn't just present/absent to <u>s</u>-at-<u>t</u> <u>simpliciter</u>, but is present in some configuration of <u>manners</u> or <u>ways</u>. Thus if $\underline{S}_i$ = <u>green</u>-<u>circle</u>, <u>s</u>'s surround at <u>t</u> may contain any number of green circles distinctive in size, pigmentation detail, distance and direction from <u>s</u>-at-<u>t</u>, justapositions with other stimuli, etc. etc. Since such variation in <u>how</u> $\underline{S}_i$ is presented can make considerable difference for its behavioral effects, stimulus-indexed input variables need to be conceived most basically as *status*-valued, where the "status" of S_i for s-at-t is whatever we describe by detailing the behaviorally relevant ways in which S_i bears upon s at t. Clearly, S_i-status is in general richly multifaceted--which is to say that this is best SLesed as a compound variable $\underset{\wedge}{X}_{S_i} = [\underset{\wedge}{x}_{S_i j} : j \in \underset{\sim}{j}_{S_i}]$ whose indexing may have a complex structure largely specific to S_i. (E.g., when S_i = *green-circle*, $\underset{\sim}{j}_{S_i}$ may contain subsets $\{\underset{\sim}{j}_{S_i k} : k = 1,2,\ldots\}$ for which $\{\underset{\wedge}{x}_{S_i j}(s\text{-at-}t) : j \in \underset{\sim}{j}_{S_i k}\}$ characterizes the radius, distance, direction, brightness, etc. of the green circle *k*th closest to s at t.) Also evident is that present schematics for dimensionalizing input by status-valued stimulus-indexed variables are still horrendously programmatic. Even so, this program is one to which traditional (i.e., pre-AI) molar psychology has long implicitly subscribed, and is far easier to realize for well-defined stimulus items than is the program of item-valued input channels.

Taking coded "items" to be *indices* rather than *values* of internal variables likewise provides an effective SLese alternative to the mental-registers model of cognitive state. Let us loosely categorize as "ideas" all the representations, item codings, percepts/concepts/thoughts, etc. to which molar psychology appeals in describing the varied momentary cognitive or sub-cognitive conditions of its subjects. Then each idea A will have some distinctive status for each s-at-t, where A-status is a multifaceted manner in which A is or is not involved in s's psychonomic economy at t. Some of these facets presumably characterize A's condition in s-at-t's memory store, others how A is or is not perceptually active in s at t, still others how A is participating at t in s's activated remembrances, reasoning, reverie, planning, decisioning, etc. But mainly, it still remains for cognitive theory to *work out* what the salient dimensions of A-status are for particular ideas A. This can only be achieved by writing down a decent sample of well-formed predicates that (*a*) encorporate the A-idea idea, and (*b*) are judged provisionally suitable for inclusion in the working corpus of serious psychology. Only when we make linguistically articulate what specific A-wise things we hope to affirm or deny on various occasions of cognition can we sort these into SLese-effective contrast sets.

I have not aired this opposition between item-valued and item-indexed variables merely to illustrate concealments in our familiarly facile grand-category talk that pursuit of SLese honesty brings to light. How to dimensionalize the panorama of external stimulation that confronts sentient organisms, and thereafter the mentation which supposedly waxes and wanes in the course of internal reactions thereto, is probably the most crucial foundation problem that now divides the adjective from the noun in cognitive science. We must dimensionalize input and ideation, for these are hopelessly beyond our comprehension as holistic totalities; yet whether we can do so to SLese satisfaction is still darkly uncertain. I have still not said enough about this matter to exhibit it as more than a metatheoretical curiosity. But its substantive importance and difficulty will deepen in Part II.