ANALYSIS AND SUBSUMPTION IN THE BEHAVIORISM OF HULL*

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The background hypothesis of this essay is that psychological phenomena are typically explained, not by subsuming them under psychological laws, but by functional analysis. Causal subsumption is an appropriate strategy for explaining changes of state, but not for explaining capacities, and it is capacities that are the central explananda of psychology. The contrast between functional analysis and causal subsumption is illustrated, and the background hypothesis supported, by a critical reassessment of the motivational psychology of Clark Hull. I argue that Hull's work makes little sense construed along the subsumptivist lines he advocated himself, but emerges as both interersting and methodologically sound when construed as an exercise in the sort of functional analysis featured in contemporary cognitive science.

Introduction. The Received Doctrine about scientific explanation is that it consists of subsumption under law. The classic expression of this doctrine is the Deductive-Nomological (D-N) model of Hempel and Oppenheim (1948), but it has dominated without serious competition at least since Newton. Yet, the Received Doctrine is beset with difficulties. It is well-known that nomic subsumption is not generally sufficient for explanation (e.g., Kim 1973), and there are reasons for supposing it isn't necessary, even in mechanics, the domain most often used to illustrate monic subsumption (Cummins 1978). Indeed, the literature abounds with well-taken critiques of the Received Doctrine. Nevertheless, scientists and philosophers alike continue to force scientific explanation into a subsumptive mold. This appears to be due mainly to the lack of any well-articulated alternative to the Received Doctrine. Those who abandon the subsumptive pattern find themselves in a methodological vacuum: if we think of a methodology as a canon for evaluating applications of an explanatory strategy, we can see that the dominance of the Received Doctrine was bound to have the consequence of limiting methodological studies to uncovering and clarifying the canons of nomic subsumption. The

unfortunate result has been a tendency to equate scientific methodology with the methodology of nomic subsumption.

Nowhere has the Received Doctrine been more influential than in psychology. Yet most psychological explanation makes little sense as subsumption, as I will try to illustrate in section II. A good theory of psychological explanation, therefore, requires an alternative to the Received Doctrine. Part I is an attempt to sketch such an alternative.

I

A major contention of this essay is that psychological phenomena are typically explained, not by subsuming them under psychological laws, but by treating them as manifestations of capacities that are in turn explained by analysis. Thus, a contrast between two explanatory strategies—subsumption and analysis—is central to what follows. In order to see this matter clearly, we need to distinguish between two kinds of theorizing, one of which standardly achieves its goals via causal subsumption, the other via analysis.

Transition Theories. Many scientific theories are designed to explain change. The point of what I call a transition theory is to explain changes of state in a system as effects of previous causes—typically disturbances in the system. The emphasis is on what will happen *when* (i.e., under what conditions). Subsumption under causal law is the natural strategy: one tries to fix on a set of state variables for the system that will allow one to exhibit each change of state in the system as an effect of a previous change of state. A transition law, therefore, requires a systematic way of representing the states of the target system, S, and a systematic way of transforming those representations such that, given a representation R of an event e in S at t, the (or a) transformation of R will represent the effect e' of e in S. We can picture the situation as in figure one.

The wavy line represents the temporal sequence of events in S. R_s is a function that maps events in the system S onto the canonical represen-

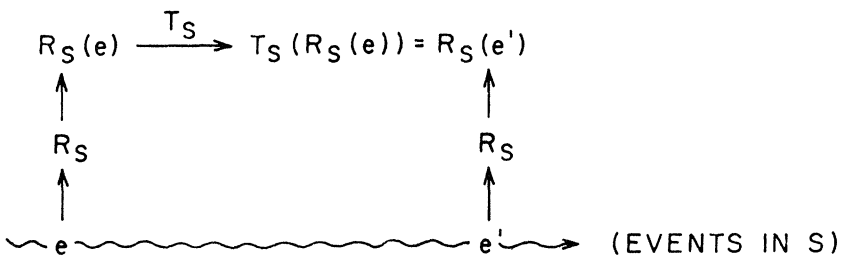

$$R_S(e) \xrightarrow{\ T_S\ } T_S(R_S(e)) = R_S(e')$$

Figure 1.

tations licensed by the theory, and T_s is a function that maps the domain of R_s into itself.

Since the point of a transition theory is to explain changes as the effects of causes, its subsuming laws must be causal laws. Non-causal correlation, however law-like, won't explain changes, it will only predict them. For example, it is a law that thermal and electrical conductivity co-vary, but a change in one does not explain a change in the other. On the other hand, any law-like pair $\langle R_s, T_s \rangle$ that is empirically adequate defines a disposition or capacity of S, and it is these, not individual events, that are generally the target explananda of scientific theories. Laws such as the one correlating thermal and electrical conductivity, as well as genuine causal laws (e.g., "a change of u to v in the length of an ideal pendulum causes a change of $2pi(\sqrt{v/g} - \sqrt{u/g})$ in its period,"), are not explanatory theories; they are the explananda of such theories.[1]

This fact is more or less obvious in the case of the conductivity relation, but it is obscured in the case of causal laws by two facts: (i) the fact that causal laws do explain individual state transitions as effects, and (ii) the fact that a transition theory for a system S can often be derived from a transition theory for a more general type of system S^* of which S is a special case. Point (ii) is the really seductive item. It is important to realize, therefore, that a transition theory for S^* simply defines or specifies a dispositional property of S^*. Dispositions want explaining for reasons made famous if not clear by Moliere. Asked why opium puts people to sleep, Moliere's doctor replies that opium has a *virtus dormitiva*. The prospect of having to take seriously at the most fundamental theoretical level what we regard as a joke elsewhere is sufficiently unwelcome to lead us to ask whether there is some other explanatory strategy to exploit besides more and more general subsumption of one transition theory to another.

Property Theories. Many scientific theories are not designed to explain changes but are rather designed to explain properties. The point of what I call a property theory is to explain the properties of a system, not in the sense in which this means, "Why did S acquire P?", or, "What caused S to acquire P?", but rather in the sense in which this means, "How does S instantiate or realize P?", i.e., "In virtue of what antecedently understood facts about S does S have P?" Just as we can ask, "Why did the gas get hotter?", we can ask, "What is it for a gas to

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[1] This helps to explain why little premium is put on the distinction between causal laws and law-like correlations in actual practice: the problem is to specify dispositional explananda, and both do this equally well.

have a temperature?'' Understood as an answer to this latter question, the kinetic theory of heat is not a transition theory but a property theory: it explains what temperature is—how temperature is instantiated—in a gas, but does not, by itself, explain changes in temperature.

Many of the most pressing and puzzling scientific questions are questions about properties, not about changes. We know a lot about what causes pain, but there is no very good theory about what pain is. Good property theories are wonderfully satisfying: we know how temperature is instantiated, how inheritance is instantiated, how electricity is instantiated, how solubility is instantiated. I think we are close to knowing how life is instantiated and how intelligence is instantiated, though we are a long way from knowing how intentionality or consciousness are instantiated.

The natural strategy for explaining properties is to construct an analysis of S that explains S's possession of P by appeal to the properties of S's components and their mode of organization. Successful analysis yields an explanatory payoff when we come to see that something having the kinds of components specified, organized in the way specified, is bound to have the target property. Although non-dispositional properties can be explained by the analytic strategy—e.g., the syntactic and some semantic properties of an inscription or utterance are typically explained by analysis—the foregoing discussion should make it clear that the usual targets are bound to be dispositions. This is particularly true in psychology, since it is neither individual actions nor categorical properties of an organism that want psychological explanation, but its capacities. Property theories thus underlie and explain transition theories. A good transition theory is not an explanatory theory, for, though it does explain individual events, this is not its primary scientific role. Its primary role is rather to *specify precisely* the explananda of explanatory theories, viz., the dispositional properties of systems. It is then the business of a property theory to explain these via analysis.

When the explanandum is a sophisticated dispositional property, as it typically is in psychology, the process of explaining P by analyzing S generally has, as a preliminary step, an analysis of P itself into simpler capacities of S or S's components. This preliminary step I call *functional analysis*. Since this is what will concern us in the discussion of Hull below, and since a full-dress discussion of explanatory analysis and property theories would be a long paper by itself, I shall restrict my attention here to functional analysis.

In the context of theoretical explanation, to ascribe a function to something is to ascribe a capacity to it that is singled out by its role in an analysis of some capacity of a containing system. When a capacity of a

containing system is appropriately explained via analysis, the analyzing capacities emerge as functions.[2]

By a functional analysis, I mean an analysis of a capacity of a system into sub-capacities of that system such that exercise of the analyzed capacity is reduced to programmed exercise of the analyzing sub-capacities. By "programmed" I simply mean organized in a way that could be specified in a program (or flow-diagram). Assembly line production provides a familiar example. Production is broken down into a number of distinct and relatively simple tasks. The system has the productive capacities it does in virtue of the fact that the units on the line have the capacity to perform one or more of these simple tasks, and in virtue of the fact that, when these tasks are executed in an organized way, the finished product is produced. We come to understand the capacities of the line when we see the "program", for the functional analysis it effects allows us to explain those capacities by exhibiting manifestations of them as organized exercises of the analyzing capacities.

The explanatory force of a functional analysis is proportional to (i) the extent to which the analyzing capacities are less complex than the analyzed capacity, and (ii) the extent to which the analyzing capacities differ in kind from the analyzed capacities. Evidently, the greater the gap in complexity and kind between analyzing capacities and analyzed capacity, the more sophisticated the program must be—i.e., the more powerful the analysis must be—to close the gap. As the program takes up more and more of the explanatory burden, the physical facts underlying the analyzing capacities become less and less special to the analyzed system. This is why it is plausible to suppose that the capacity of an electronic calculator and a mechanical calculator to compute a given function might have substantially the same explanation: they might execute the same program, albeit in virtue of very different physical facts. The relative independence of the details of physical realization is also what makes it possible to formulate a functional analysis of a capacity of S while remaining neutral and/or ignorant of the physics or physiology of S. An electronic and a mechanical calculator might share a functional analysis even though they share no other theoretically interesting description that they don't also share with rocks and alarm clocks. There is therefore a level of theory about calculators, and plausibly about organisms as well, that is independent of whatever theory is relevant to describing the details of realization.

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[2]This is the view I defended in (1975), and I still think it is substantially correct. However, it is the analytical style of explanation, especially as applied to complex capacities, that interests me, not the proper explication of the concept of *function*. Thus, 'functional analysis' is here no more than a technical term for a theory designed to analytically explain a capacity or disposition.

The special features and explanatory role of functional analysis deserve a much fuller exposition (see Cummins forthcoming), but the sketch just provided should suffice for my present purpose, which is to illustrate how the distinction between transition theories and property theories, and the correlative distinction between subsumption and analysis, can help to illuminate historical cases of psychological theorizing. My stalking horse will be Clark Hull.

II

> The term *theory* in the behavioral or "social" sciences has a variety of current meanings. As understood in the present work, a theory is a systematic deductive derivation of the secondary principles of observable phenomena from a relatively small number of primary principles or postulates. . . . In science, an observed event is said to be explained when the proposition expressing it has been logically derived from a set of definitions and postulates coupled with certain observed conditions antecedent to the event (Hull 1943, pp. 2–3).

> A natural event is explained when it can be derived as a theorem by a process of reasoning from (1) a knowledge of the relevant natural conditions antedating it, and (2) one or more relevant principles called postulates (Hull 1943, p. 14).

The above quotations from Clark Hull's *Principles of Behavior* make it clear that Hull accepted a deductive-nomological model of explanation of the sort worked out by Hempel. I want to emphasize two features of Hull's view, for they will be crucial to what follows. First, Hull assumes that the explananda are *observable events*. Second, the observable events that are the explananda of a theory are also the data that test the theory. Together, these points dictate the form of Hull's theorizing: he is after a transition theory, for this is the only kind of theory his philosophy of science recognizes. He is well aware of this, for he writes:

> Scientific theories are mainly concerned with dynamic situations, i.e., with the consequent events or conditions which, with the passage of time, will follow from a given set of antecedent events or conditions. The concrete activity of theorizing consists in the manipulation of a limited set of symbols according to the rules expressed in the postulates . . . in such a way as to span the gap separating the antecedent conditions or states from the subsequent ones (1943, p. 382).

This is a description of the picture of a transition theory that appears in figure one.

Hull's Behaviorism. A Watsonian behaviorist thinks of an organism at a given instant as a bundle of S-R connections. These are altered over time by two factors: maturation, debilitation, injury and the like, and classical conditioning (stimulus substitution). Stimulus and response are, in principle if not in practice, to be described in psychologically neutral physical terms, preferably the terms of physics, chemistry, and anatomy. Sophisticated behavior is to be explained by analyzing it into a sequence of simple S-R connections, execution of the n*th* response producing conditioned proprioceptive stimuli for the n + 1*st* response in the chain.

The basic picture Hull presents is not greatly different on the face of it. Classical conditioning is supplemented by operant conditioning (the law of effect), and since this presupposes that certain behaviors are simply emitted, and hence are not responses to identifiable stimuli, the organism is not conceived solely as a set of S-R connections, but as a set of S-R connections plus a set of relatively weighted tendencies to emit certain behaviors. Define an organism's behavioral repertoire at a time as the set of behaviors it can emit at that time together with its associated probability distribution: an organism, then, is characterized at a moment by a set of S-R connections and a behavioral repertoire.

What is distinctive at first sight about Hull's psychology is the use of "intervening variables" to characterize the internal psychological condition of the organism. No one has ever thought that perception of food causes salivation in dogs, or that pecking is emitted in pigeons without the mediation of internal processes. If we ask why perception of food causes salivation,or why pecking is emitted, the answer must be that the organism is internally structured in a way that accounts for these facts. With this, of course, other behaviorists agree. What is controversial is Hull's claim that internal states and processes can and should be characterized not physiologically or introspectively, but in terms of their psychological functions. A glance at the summary diagram on p. 383 of *The Principles of Behavior* (Fig. 2) clearly reveals that, though Hull assumes his constructs characterize physiological mechanisms, the characterization is not in terms of physiological properties but in terms of what philosophers of mind call their functional role: roughly, their contribution in context to the causation of behavior.[3]

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[3]Here is glossary of the symbols in the diagram of figure one.
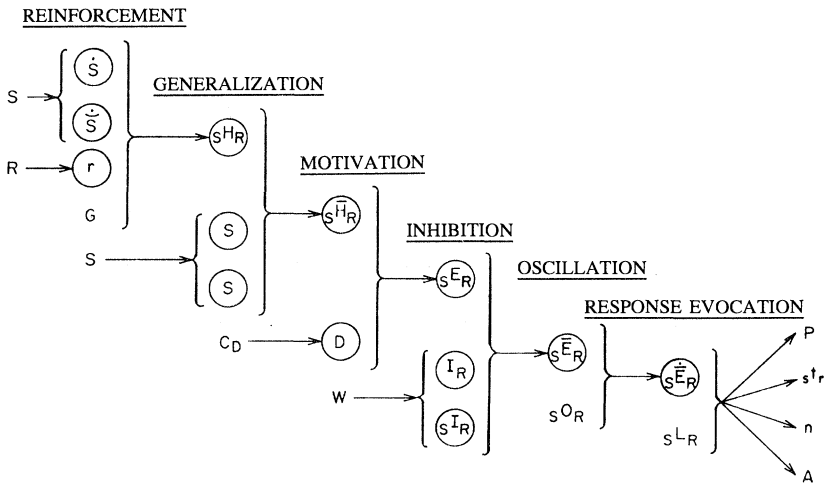|	
$\dot{S}$	physical stimulus energy in learning
R	organism's reaction
$\dot{s}$	neural result of $\dot{S}$
$\check{s}$	neural interaction from 2 or more stimulus components
r	efferent impulse leading to reaction
G	reinforcing event
$_sH_R$	habit strength
S	evocation stimulus (same continuum as $\dot{S}$)

Figure 2.   Summary diagram of Hull's theory, from Hull (1943, p. 383).

Because of this, Hull is sensitive to the charge that he is introducing constructs that are in principle beyond observational verification. "Reaction potential", for instance, could not be observed even if we could "look into a person's head", unless we already knew what physiological process instantiated the function characteristic of reaction potential. And if we knew *that,* we wouldn't be worried about observational verification of the psychological reality of reaction potential. To avoid the charge of introducing unverifiable entities, Hull insists that intervening variables must be "anchored at both ends".

It is worth pausing to make this precise. To introduce an intervening variable sigma, we must (i) specify a function *f* that allows us to calculate a value for sigma given values for one or more independent variables

$_s\bar{H}_R$	generalized habit strength
C_D	drive determining event (deprivation)
D	drive strength
$_sE_R$	reaction potential
W	work in evoked reaction
I_R	reactive inhibition
$_sI_R$	conditioned inhibition
$_s\bar{E}_R$	effective reaction potential
$_s\dot{O}_R$	oscillation
$_s\dot{\bar{E}}_R$	momentary effective reaction potential
$_sL_R$	reaction threshold
P	probability of reaction
$_st_r$	latency
n	number of reactions to extinction
A	amplitude

representing stimuli or other observable antecedent conditions, and (ii) we must specify a function g that allows us to calculate a value for a response given a value for sigma.

Now, given f and g, it is evidently possible to eliminate reference to sigma in the theory by simple composition of functions. For suppose we have $f(s) = $ sigma and $g(\text{sigma}) = r$. Then we may replace these two equations with '$g(f(s)) = r$' which refers only to s and r. Thus, Hull's requirement that intervening variables be anchored at both ends amounts to the requirement that they be eliminable: intervening variables are allowable only if they aren't necessary!

So why have them? Neal Miller (1959) gives the following explanation (see Fig. 3). If there are more than two independent variables and more than two dependent variables, then a theoretical economy is achieved by introducing an intervening variable. Here is his argument. Suppose we have two stimulus variables, s_1 and s_2, and two response variables, r_1 and r_2. If we do not introduce intervening variables, we have to state four functional relations as indicated in Fig. 3(1). If we introduce an intervening variable, sigma, we still have four functional relations to state, as in Fig, 3(2). Now compare the situation involving more observables (Figs. 3(3) and 3(4)). Without sigma, we have nine functional relations; with sigma, only six. Introducing sigma introduces economy, says Miller.

This argument is unsound. The number of functions—i.e., state equations—we need to specify to represent the relations between independent and dependent variables is always equal to the number of dependent vari-
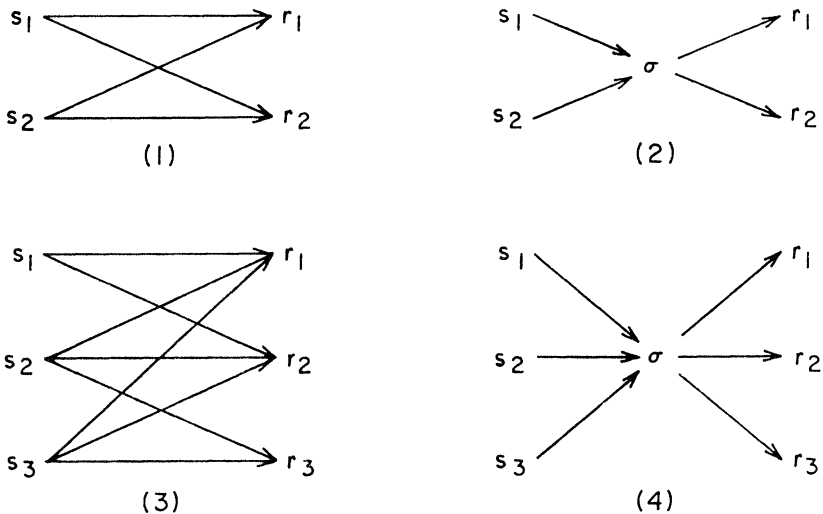

Figure 3.

ables. For Fig. 3(3), we need three equations:

$$f_1(s_1,s_2,s_3) = r_1$$

$$f_2(s_1,s_2,s_3) = r_2$$

$$f_3(s_1,s_2,s_3) = r_3.$$

For Fig. 3(4) we have:

$$f(s_1,s_2,s_3) = \text{sigma}$$

$$g_1(\text{sigma}) = r_1$$

$$g_2(\text{sigma}) = r_2$$

$$g_3(\text{sigma}) = r_3$$

which, by composition of functions, gives us:

$$g_1(f(s_1,s_2,s_3)) = r_1$$
$$g_2(f(s_1,s_2,s_3)) = r_2$$
$$g_3(f(s_1,s_2,s_3)) = r_3.$$

Miller seems to have confused the number of lines of causal influence (the arrows) with the number of equations needed to represent them, no doubt because the phrase "functional relation" is used in both senses by psychologists.[4]

Whatever intervening variables do, they don't achieve theoretical economy. What *do* they achieve? Since the causation of behavior is mediated by internal states and processes, the obvious answer is that intervening variables are introduced to describe these states and processes in a way that affords an explanation of behavior.

In order to clarify the role of intervening variables, it is useful to consider how we might explain the behavior of a machine or factory the insides of which, let us assume, we cannot directly observe—e.g., the Ford factory at River Rouge. This plant takes iron ore, wages, power, etc., as input and gives Fords as output. Now we could, if we wished, attempt to express precisely various measurable features of the output as functions of measurable features of the input. The resulting set of equations would yield predictions of the plant's "behavior". Since, however, the contribution of the factory to the character of output is large compared to the contribution of input, the features of output we could expect to

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[4]The points made here are touched on in Fodor (1965) and the ensuing discussion: Berlyne (1966), Osgood (1966) and Fodor (1966).

predict (or control) in this way would be severely limited: what output will be made of will be largely unpredictable, leave alone (leave WAY alone) features of the output such as acceleration capacity and mileage. I suspect tonage is about all we'll be able to handle, plus latency (we could radio-actively tag in-going ore and thereby determine production time), probability of output (Fords vs. M-16's), and number of units producible after wages stop. Pretty limited fare.

Hull buffs will recognize that these outputs are analogues of Hull's dependent variables. For 'tonage' read 'amplitude'; for 'probability of output' read 'probability of reaction evocation'; for 'latency' read 'latency'; for 'number of units producible after wages stop' read 'number of unreinforced reactions to produce extinction'. It seems clear that if Hull introduced intervening variables in order to theorize about the contribution of the organism, the gambit failed to yield more than the rather boring response variables achievable without the gambit. This should come as no surprise: the requirement that intervening variables be eliminable *guarantees* that they will add *nothing* to output predictability. The point of theorizing about the contribution of the organism or factory would *seem* to be to expand and enrich the predictable and explicable features of output, but Hull's methodological requirement effectively undermines this motivation.

Let's pause to consolidate. Why intervening variables? Not for theoretical economy. Perhaps to enrich and expand the explanatory and predictive scope of the theory then? But the eliminability requirement blocks that possibility. And finally, we must wonder about the explananda: a theory that purports to be a general theory of behavior should endeavor to explain more than Amplitude, Probability of Reaction Evocation, Number of Reactions to Experimental Extinction, and Latency. All of these except *n* (number of reactions to extinction) characterize particular responses, and science is almost never in the business of explaining particular events. "Why did that white stuff disappear in your coffee?" is not a scientific question; it calls for no theory, but for a particular cause (it dissolved) and, perhaps, some back-up justification of the causal claim (the white stuff is sugar, and sugar is soluble in water, which is mostly what your coffee is). "Why does sugar (or anything) dissolve in water (or anything)?" does call for theory precisely because it is not an individual event but a capacity that is the explanandum. A general theory of behavior should at least address questions about striking behavioral capacities, questions such as these:

—Why are organisms subject to the Law of Effect?
—Why does an after-image perceived as a spot on a wall shrink when the wall is approached?

—Why do people regularly commit the gambler's fallacy?

—Why does alcohol affect memory, and how?

—Are recall and recognition reasonable tests of what is remembered, or are they just two of many special tasks for which memorized information is selectively made available?

—Why does the complexity of an English description of a color predict the memorability of colors for non-English speakers with no color vocabulary?

—Why is it easier to learn concepts of the form $(A \ \& \ B)$ than of the equivalent form $-(-A \lor -B)$?

Derivation of values for A, t, p, and n couldn't possibly answer these questions.

The problem is Hull's philosophy of science—his adherence to the doctrine that explanation is nomological subsumption of data. It is not. That doctrine confuses scientific explanation with scientific testing. One consequence is that the explananda of a theory are misidentified with the data that support it, i.e., with the data it subsumes. Hempel pointed out years ago that (narrow) inductivism fails to distinguish theory construction from theory testing. Since testing is inductive in character, narrow inductivism left no room in science for theories that are not generalizations of the data. We now require a comparable distinction between theory testing and theoretical explanation. To assimilate the logic of explanation to the logic of testing leaves us no conceptual space to delineate the difference between the data that support a theory and its intended explananda.

Let us, therefore, abandon the idea that the explananda of Hull's theory are the dependent variables of the theory: it is obvious that intervening variables are supposed to have an explanatory function in Hull's theory, and it is equally obvious that they add nothing to the nomic subsumption of the dependent variables, nor do they serve to enrich or expand the scope of those variables. So long as we suppose that the explananda of the theory are the dependent variables, the explanatory role of intervening variables will remain mysterious.

So what does Hull's theory explain? Once we take off the blinders of the D-N model Hull pushed so hard, I think the answer is obvious: the theory is not a general theory of behavior, but an analytical explanation of the capacity to be operantly conditioned. (Hereafter, I shall refer to this capacity as COC.) The summary diagram of the system we looked at earlier is a flow-chart analysis of COC (see Fig. 4). It would be a tedious but conceptually trivial task to use this flow-chart in conjunction with the glossary at the back of The Principles of Behavior to construct a computer program, and hence to realize COC as analyzed by Hull, on
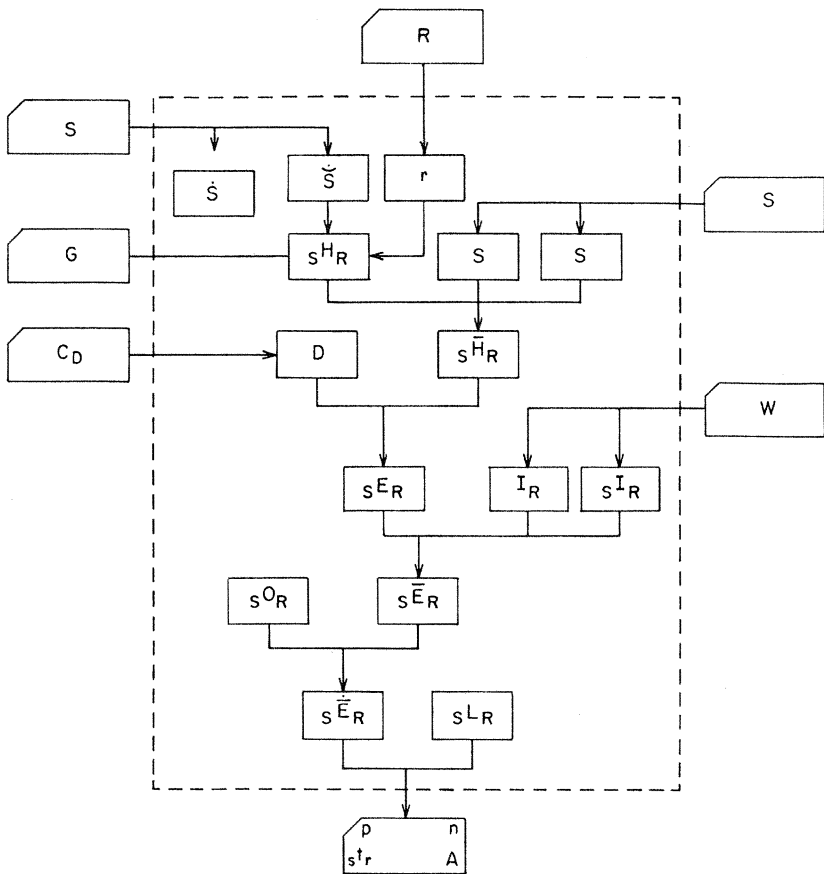
Figure 4.

a computer. We could then test the analysis by (i) specifying values of the theoretical parameters, and (ii) comparing output with experimental findings.

This, of course, is just what Hullians did, although the simulation was not automated, but done with pencil and paper. But thinking of it as simulation helps us focus on the explanatory role of the theory; it brings out several points of importance.

First point. Thinking of the theory in this way has the advantage of putting the dependent variables in their place: they are data, not explananda. The explanandum is *COC*. So the theory has a serious point, and so does its distinctive feature—the use of intervening variables—for these now appear as analyzing capacities. The Eliminability Requirement even makes a kind of sense: it becomes the requirement that analyzing func-

tions be functionally specified, i.e., specified in terms of their connections to other functions.

Second point. COC is a capacity, and a capacity is defined by an input-output function. This is what the Hullian state equations that Miller was confused about do: they specify the capacity to be operantly conditioned. Intervening variables are utterly irrelevant to this specifying role of S-R functions. What *is* relevant is whether the equations fit the data, for that is what tells us whether the capacity we have *specified* is a capacity the organism *has*.

Third point. From this point of view, it is clear that A, $_st_r$, p, and n are measures of output—i.e., that a set of values for these variables is supposed to identify an exercise of COC. There is a theoretical commitment here: the idea is that the empirically possible values for the quadruple $\langle A, _st_r, p, n \rangle$ for a given organism identify and distinguish the empirically possible exercises of COC in that organism. A, $_st_r$, p and n no longer figure as general response measures, but simply as measures of exercises of COC.

Fourth point. A corresponding point can be made about the independent variables, $\dot{S}$, R, G, S, C_D, and W, with this difference: whereas we can motivate the inclusion of some of these, e.g., R and S, on the grounds that a capacity without something comparable to these as inputs wouldn't be COC at all, others are motivated solely on the grounds that they are needed to predict output. Inclusion of W, for example, is motivated on these grounds, and hence rests on the thesis that specifying $\langle A, t, p, n \rangle$ is the right way to specify an exercise of COC.

Fifth point. For the theory to be a general theory of COC, not just a theory of COC in rats, or some particular rat, it must contain parameters that we can "tune" for particular organisms or species. It is obvious enough that these *are* present in Hull's theory. I have two comments about them.

(i) Consider $_sE_R$ (excitatory potential). This is calculated as follows in *The Principles of Behavior:* $_sE_R = {}_{s_1+s_D}\bar{H}_R ((\dot{D} + D)/(\dot{D} + M_D))$.

Now obviously, changing the slash (division) to an "×" (multiplication) would be changing the analysis, not tuning it. On the other hand, $_s\bar{H}_R$ is a function of an empirical constant, $-j'$. The force of saying it is an empirical constant is precisely that it has to be experimentally determined, and hence might differ from organism to organism, or from species to species.

(ii) $-j'$, of course, is an intervening variable in the sense of not being a measure of an observable. On the other hand, we cannot calculate $-j'$ from independent variables, and we need it to calculate a value for $\langle A,$

$_st_r$, p, $n\rangle$. Hence, $-j'$ cannot be eliminated as can genuine intervening variables; mathematically, it is an independent variable. This is just what identifies it as a "tuning parameter"; it is empirically determined but not a measure of an observable.

How do you empirically determine the value of something like this? You pick the value that gives you the best fit between the state-equations and the data. If the state-equations prove difficult or impossible to tune in this way, then they are just wrong. Conceptually, this is clear enough. Practically it is sometimes messy because of the use of statistical techniques in the analysis of data. It is difficult to tell whether a borderline fit means bad tuning or a wrong model. The fact that the fit can be improved by tuning may not mean much unless God or another experimenter tells us how well one can do with a different model altogether.

In practice, the problem of finding a set of state-equations (including tuning parameters) that fits the data is so difficult that it is easy to lose sight of the fact that the ultimate explanatory point of the enterprise is not to predict the data, but to explain COC. This seems to me to be precisely what has happened in learning theory generally, not just in Hull. But it is easier to diagnose in Hull because the idea that the dependent variables are the explananda of the theory is so evidently inconsistent with (i) the descriptive scope of the theory, and (ii) the use of intervening variables together with the Eliminability Requirement. The same thing is happening in certain circles in cognitive psychology: the problem of specifying cognitive capacities—i.e., of constructing a set of state-equations that matches input-ouput data—is so difficult that people have lost sight of the point. The state-equations don't explain anything: the input-output data they subsume are not the explananda; the state-equations are a specification of the explanandum, viz., the capacity they specify. Capacities are not explained by specifying them, be it oh-so-carefully-and-mathematically. They are explained by analyzing them and, ultimately, by exhibiting their instantiations in the systems that have them. Specification of a capacity can be a fascinating and challenging scientific problem, but it is not explanation. To the extent that psychology limits itself to this sort of problem—the specification problem—it forfeits its claim to be an explanatory science. To his credit, Hull did insist on his intervening variables, and hence did have a shot at explaining something worth explaining—viz., COC—though his adherence to the D-N model of explanation kept him from seeing this clearly. (The explanatory attempt failed, of course, because COC is not to be analyzed into motivational capacities such as drives, but into inferential capacities: a capacity to be operantly conditioned is a capacity to make a certain kind of inductive inference. But that is another story altogether.)

What emerges is that the kind of psychology Hull was trying to do can be pictured as a kind of three-phase project. Phase one is the specification problem, phase two is functional analysis, and phase three is instantiation. This is the standard form of a property theory that has a capacity as explanandum. The three part division should not be taken to mean that the phases are independent of one another. In practice, one should play both ends against the middle, functional analysis being a sort of middle-man, adjusting the requirements of phase three and phase one to each other in a way that makes significant explanation possible. We should never forget that science is something we do in order to increase our understanding, and that it is explanations that do the job. True theories are not always helpful in this regard. It is therefore perfectly in order to reject well-confirmed theories in favor of competitors on the grounds that the competitor allows the construction of an explanatory picture. Looked at in this way, it is clear that neurophysiology (instantiation) constrains psychology no more than psychology constrains neurophysiology. The trick is to get one of each that can be glued together into a coherent explanatory whole by employing the strategy of analysis.

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