Behavioral pharmacology: Issues of reductionism and causality

Article · January 1990

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Advances in Behavioral Pharmacology

VOLUME 7

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LAWRENCE ERLBAUM ASSOCIATES, PUBLISHERS
1990 Hillsdale, New Jersey Hove and London
Behavioral pharmacology is now over 3 decades old. In 1955 Peter Dews published his article "Differential sensitivity to pentobarbital of pecking performance in pigeons depending upon the schedule of reward." This epoch-making study, whose implications have yet to be fully fathomed, emerged from a contact between Peter Dews, a pharmacologist at Harvard Medical School and B. F. Skinner's laboratory across the river in Cambridge, then peopled by such talented researchers as William Morse, Charles Ferster, and Richard Herbstein. Morse was soon to join Peter Dews' laboratory, to be joined later by Roger Kelleher, and the trio of Dews, Morse, and Kelleher and their academic offspring were to provide a headwater of principles and research methods that remain in the mainstream of behavioral pharmacological investigation. But these proximal historical details obscure the distal, but deeper influences. The two disciplines whose confluence yielded behavioral pharmacology, namely the experimental analysis of behavior and pharmacology, have a common methodological origin in the classical physiology of Claude Bernard (1957). This has been emphasized by Travis Thompson (1984) in his retrospective review of Bernard's An Introduction to the Study of Experimental Medicine wherein he says:

The experimental analysis of behavior has typically had its academic home in the discipline of psychology. Paradoxically, the experimental analysis of behavior shares much more with the tradition of Claude Bernard than it does with those of Wundt or Freud. Indeed, if an 19th century progenitor of contemporary behavior-analytic theory were to be identified, it would be Bernard and not, as is often claimed, James or Pavlov. James' pragmatism and Pavlov's reflexology were
And, as I have said elsewhere: "The animal chamber and the cumulative recorder, as ingenious as they are, are direct descendents of the dissecting table and the smoked drum" (Marr, 1984, p. 358).

Bernard may also be credited with important developments in basic pharmacological method and theory as exemplified by his famous studies of the actions of curare. His experimental demonstration that curare had a physiological loci of action was to have powerful implications for pharmacology in general. Methodologically, he emphasized the value of techniques we now call the "ABA design" and "systematic replication" (Bernard, 1957; Sidman, 1960).

The level of analysis of Bernard's physiology, that is, the focus on organ and tissue function is also reflective of both classical pharmacology and the modern experimental analysis of behavior. It is easy to make the correspondence between the study of organ and tissue function on the one hand and, on the other hand, the rate and pattern of keypecks and the sequence or distribution of interresponse times.

The experimental analysis of behavior has provided orderly and repeatable data on the actions of individual organisms. The data are subject to the most compelling visual display as well as detailed quantitative scrutiny. So it was with physiological investigation of organ and tissue systems. The rate and pattern of the heartbeat, the response of an isolated smooth muscle to electrical or chemical stimulation, and yes, the secretion of saliva, all represent orderly phenomena subject to the prediction and control inherent in a functional analysis. Pharmacology advanced by the study of the effects of drugs on these kinds of response systems.

Consideration of the axiom that behavior is a biological property of organisms, coupled with the orderly data that can emerge from an appropriate behavioral analysis leads one to consider the significance of the study of the effects of drugs on behavior, or, more precisely, drug-behavior interactions. This reasoning, however, is both glib and formal and requires a more careful consideration of what might be termed the causal structure of the field of investigation. That is, the relationships and their directions holding between variables of one class, say pharmacological, and variables of another class, say behavioral. This causal structure is reflective of the rationale and methods for investigation. It constitutes the texture of functional relations and in a sense is descriptive of the consequences maintaining the behavior of the investigator.

Causal structure also embodies the levels of analysis that can enter into an account, that is, characteristic reductive modes. The term "reduction" itself plays a number of complex roles in the language games of science, no doubt a major source of the vigorous controversies engendered by the term. Ernest Nagel (1979) provided a framework concerning the logic of reductive explanations which is useful for explorin behavior interpretations.

Nagel distinguished two geneous. A formal paradigm general theory of implicative terms. For example, the e deducible from Newtonian velocity, mass, acceleration entering into functional rels what Skinner (1950) descri appeals to events taking pit, described in different sions" (p. 193).

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The formative model for
which is useful for exploring the reductive aspects of the causal structure of drug-behavior interactions.

Nagel distinguished two forms of reductionism: homogeneous and heterogeneous. A formal paradigm for homogeneous reduction is the deduction from a general theory of implications (or theorems) in which there is a communality of terms. For example, the cosmos of Kepler and the kinematics of Galileo are deducible from Newtonian laws. Moreover, there is a common terminology: velocity, mass, acceleration, etc., or, more generally, a common set of variables entering into functional relations. In contrast, heterogeneous reduction embodies what Skinner (1950) described as “any explanation of an observed fact which appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions” (p. 193).

Nagel’s category of homogeneous reductionism explanation is more familiar to behavior analysts. The matching law and its cousins, and various formulations of reinforcing principles are representatives. Ferster and Skinner’s (1957) analysis of schedules of reinforcement is based on conditions prevailing at the moment of reinforcement; and concepts of response strength and probability of reinforcement per response have represented basic formulations of behavior from which the characteristic aspects of behavior are assessed or derived. A common set of variables and terms, e.g., reinforcement, reinforcement frequency, rate, patterns of interresponse times, prevail.

The concern with “units of behavior” or the “molar vs. molecular” issue would appear to entail the heterogeneous form of reductionism (cf. Thompson & Zeiler, 1986). However, in all cases the so-called units or the molecular components are treated as behaviorally significant events. Behavior is reduced to behavior—a homogeneous reduction after all.

One of the most general “covering laws” of behavioral pharmacology is the notion of rate dependency, (or rate constancy, if you prefer), i.e., that the rate of responding is an important determinant of a drug effect. Again, the effects of a drug on behavior is explained by reference only to a behavioral variable—rate. A large variety of experiments and observations has been interpreted in the context of this principle. It has been questioned from several quarters, most recently by Branch (1984); but it could be argued that for better or worse, it is, or has been, the only general integrative principle in behavioral pharmacology. But perhaps things are not all that bleak. Through vigorous and careful experimental analysis, salient variables and functional relations at the behavioral level have emerged that could provide for effective prediction and interpretation of drug-behavior interactions without the necessity to engage in shifts in levels of analysis. An example is the investigation of the stimulus properties of drugs which has evoked principles of stimulus control in the interpretation and classification of drug effects on behavior.

The formative model for causal structure in the physiology-pharmacology
axis has provided analogs in the behavior-pharmacology axis. The formulation of a causal structure for the physiology-pharmacology axis is constructed from the investigation of the question "What does a drug do to the activities of an organ or tissue?" For example, how does a drug affect the rate and pattern of the heartbeat? The causal structure begins to take form from additional questions derived from simply showing the drug has an effect: "What do the drug effects tell us about the mechanisms of action?"; "What do the drug effects tell us about the drug, i.e., how the drug might be classified?"; and, "What do the drug effects tell us about the physiology of the organ system?"

These questions do not, of course, exhaust the possibilities. The form, range, and depth of such questions depend on the theoretical and methodological framework of the related fields, their knowledge bases, and the levels of analysis to which (or from which) explanatory appeals are directed.

Comparable questions arise as to the relations between behavioral and pharmacological variables. Thus: "What does a drug do to behavior?"; "What variables control these effects?"; "What do the effects on behavior tell us about the mechanism of action of the drug?"; and, "What do the drug effects tell us about behavior?" The answers to such questions will depend minimally on what is meant by behavior, entailing an entire philosophical and methodological approach. The foregoing questions might have been very different or, at least very differently stated. Likewise, "mechanism of action" is in itself an ambiguous phrase to which one might respond with behavioral, physiological, biochemical or even molecular relationships.

Consider the practice of describing the behavioral effects of drugs in such heterogeneous reductive modes as alterations in emotional, motivational or perceptual states, excitatory or inhibitory processes and the like. However, the experimental analysis of behavior, as manifested in behavioral pharmacology, has eschewed these kinds of expressions and has focused upon functional relations wherein changes in behaviors (e.g., keypecks, lever presses, verbal responses) are expressed in terms of such directly manipulatable variables as (1) schedule arrangement, (2) rate of responding, (3) consequences, (4) context, (5) drug history, and (6) behavioral history. Thus, answers to the question "What does a drug do to behavior?" are, first of all, generally constrained to statements about relative probabilities of actions and their distributions in time. This is in conformity with the old pharmacological principle that drugs do not confer new physiological, and by extension, behavioral properties or processes; they can only modify, i.e., enhance, reduce, or alter the frequency or pattern of a given function. As Peter Dews (1964) once expressed it: "Drugs change the temporal patterns of force development by the heart; they do not make the heart secrete urine" (p. 200). This is congruent with a homogeneous reductive perspective. However, physiological or behavioral changes may possibly emerge from the presence of new combinations of molecular structures in which the drug participated directly or indirectly as, for example, by agents that alter genetic mechanisms.

The six variables cited (1) behavior; that is, changes in arrangement, etc., separately drug can be looked upon asion. This is perhaps most properties of drugs. These mental events, drugs may a native stimuli. Such studies ion of drugs and for extent through the experimental an

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The formulation of the behavior and pharmacology axis is constructed from the rate and pattern of the drug effects to the activities of an organ system. The rate and pattern of the drug effects tell us about the mechanisms of action of drugs. The behavior of the drug effects can be looked upon as simply another environment or historical manipulation. This is perhaps most clearly exemplified by the study of the stimulus properties of drugs. These investigations demonstrate that, like other environmental events, drugs may act as reinforcing, punishing, eliciting, or discriminative stimuli. Such studies have both basic and applied value for the classification of drugs and for extensions of the principles of stimulus control gained through the experimental analysis of behavior in the absence of drugs.

The behavior analyst manipulates environmental variables with the ultimate consequence of developing a systematic, functional account of behavior. Because the imposition of a drug may be treated as another class of environmental manipulations, as suggested earlier, it follows that this manipulation might prove useful for the analysis of behavior. This aspect of behavioral pharmacology has not received the attention it deserves, yet for behavior analysts it should provide one of the most compelling rationales for the investigation of drug-behavior interactions. Drug effects can enlighten our understanding of behavior as well as reveal our naivete. As an example of the latter, punishment is usually defined as a consequence of responding that suppresses responding (Arzin & Holz, 1966). Certain drugs diminish the suppressive effects of punishing events, an important effect in the screening of putative anxiolytic agents. Yet as Branch, Nicolson, and Dworkin (1977) have shown, suppressive effects of some events, e.g., timeouts or brief stimuli associated with periods of nonreinforcement, are not reduced by the usual anxiolytics. Just as “all that glisters is not gold,” “all that suppresses is not punishment,” at least from a pharmacological perspective.

The spectre of heterogeneous reductionism gestates in questions of the form: “What do the mechanisms of action of drugs tell us about their effects on behavior?”; or “What do behavioral effects of drugs tell us about the mechanism of action of drugs?”; and, perhaps most fundamentally: “What do the mechanisms of action of drugs tell us about the mechanism of actions of behavior?”

What one means by “mechanisms of action” will, of course, depend on whether one accepts the value of a homogeneous reductive explanation or, conversely, exclusively pursues explanatory schemes at other levels. (One might engage in the diplomatic flight to parallelism, but then the issue of relationships between drug and behavioral mechanisms of action seems to lose its meaning.) We leave the world of homogeneous explanations when we no longer view a drug as an aspect of the environment or as an “establishing operation,” to use Michael’s term. Beyond these roles, a drug must then be viewed as a modifier of physiological or biochemical processes. The “pharmacology” in behavioral pharmacology then asserts itself. As mentioned earlier, classical pharmacology essentially shared its methods and levels of analysis with classical physiology. Following the views of Claude Bernard, neither science could have been said to be reductionistic in the heterogeneous sense. But modern physiology and phar-
macology share in the general fund of biological explanation of which there are at least five aspects: (1) biochemical/biophysical; (2) physiological (i.e., organ and tissue function); (3) morphogenic-developmental; (4) behavioral-environmental; and (5) species adaptation and evolution.

The expansion of the explanatory horizon for pharmacology had its origins in the dawn of the field. Paracelsus had asserted boldly: "The body is made of chemicals, and its ills should be treated by chemicals." But true enlightenment as regards mechanism had to wait some 300 years for Bernard (with some irony) and Erlich and Langley to propose in various ways the concept of receptor. The receptor is one of those rare hypothetical constructs that made good. Only the concepts of atom and gene have had greater success in the scientific venture. The hypothetical nature of the receptor has largely vanished to be replaced by the quantum mechanics of molecular structure and now all of basic pharmacology must pay tribute to it. The receptor is the integrative principle of pharmacology and the effect on behavioral pharmacology has been profound. Behavior has now become tissue—a kind of clam heart or frog rectus muscle—for the assessment of drug-receptor interaction. A review of this role in the armamentarium of the pharmacologist demonstrates first of all behavior's place as another isolatable biological feature of the organism. Second, the use of behavior as tissue provides results which are, generally, on equal standing with organ, tissue or other in vivo or in vitro methods—both in terms of ease and of difficulty of interpretation. For example, behavioral techniques have played a major role in the analysis and classification of opiate receptor activation and antagonism. The volume edited by Seiden and Balster, Behavior Pharmacology: The Current Status (1985) contains several papers illustrating the empirical and conceptual elegance of this work.

Woods and his colleagues (1985) have applied drug discrimination procedures to assess the effects of binding to opiate receptors of certain opiate antagonists such as buprenorphine, beta-funaltrexamine, and beta-chlornaltrexamine. The goal of this work was to compare behavioral procedures and in vitro methods in terms of the usual criteria for irreversible binding to receptors. The outcome is that no one method, in vitro or in vivo (e.g., behavioral) provides definitive evidence and the results of different methods may in conflict. However, consistent results are available in certain cases, yielding convergent evidence for irreversible binding.

Both in vitro and in vivo procedures are also required to distinguish the various categories of opiate receptors—mu, kappa, and sigma—as illustrated by the work of Holzman (1985) and Leander (1985). Each receptor is identified by a constellation of effects—physiological, pharmacological, and behavioral. While no one of these stands out as the reductive marker, a behavioral distinction would seem essential, because opiates (and many other drugs) are, after all, of interest primarily because they affect behavior.

Drugs are often classified in terms of what appear to be a dominant therapeutic or toxic effect. Such an effect may only be that which is considered useful on the one hand or dangerous on the other. While such effects may be considered generic, for example, there are a few drugs (e.g., tranquilizers, anxiolytics, or antispasmodics) which are primarily useful and also have a generic effect or action. Whether you are a Jivaro Indian using a blowgun, or a surgeon performing a surgical procedure. The notion of a heterogeneous reductive theory was to be taken all too seriously. The effect of a drug, meaning as even if we focus on a particular effect what is meant by that effect, description, or to the develop behaviorally of interest may be variables. Dykstra (1985) re.

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One might well ask, however, whether this community is a loose confedera-
tion or a centralized federation. The latter is founded on a "unity of science"
concept, with a hierarchy of levels, each reduced to the one "below" it in the
heterogeneous reductive sense. Perhaps the evolution of the human species, and
my behavior involved in writing this article about reductionism, could be
described, predicted, and ultimately controlled, in principle, by the adroit
application of the laws of quantum field theory. (I do not wish to explore here the
interesting implications of the view of some Idealists and others that physics is
really a branch of psychology!) There are those who could argue persuasively
that behavior is not only reducible to physics by various causal links, but is
identical with certain physical states. In behavioral pharmacology, we might
the one hand or dangerous on the other—stimulant, depressant, antidepressant,
tranquilizer, anxiolytic, convulsant, poison, euphoriant, analgesic—represent
just a small set of "generic" descriptions. To view drugs as having only a single
effect is, of course, naive. The "effect" of a carare-like agent will depend on
whether you are a Jivaro Indian in the Amazon headwaters foraging for food with
a blowgun, or a surgeon concerned with muscular spasms during an abdominal
procedure. The notion of a single "generic" effect is, in part, an outcome of
heterogeneous reductive thinking—as if Erlich's concept of "the magic bullet"
were to be taken all too seriously. One could then speak of the mechanism of
action of a drug, meaning a specific physiological/biochemical action. However,
even if we focus on a particular effect, assuming we can adequately characterize
what is meant by that effect—not a trivial problem when it comes to clinical
description, or to the development of laboratory models—we find that the drug
effect of interest may be controlled or modulated by numerous behavioral
variables. Dykstra (1985) reviewed the role of behavioral variables in the anal-
gesic action of opiates. Stimulus intensity, type of stimulus, response rate, and
schedule were found to be significant determinants of variation in analgesic
effects. These same variables, along with others, would play significant roles in
all the generic categories and implied effects noted earlier. But, one can go much
further. Drugs alter the physiological/biochemical environment. As already
mentioned, such alterations do not imply new functions, but rather modulation of
already established functions. For example, the notion of endogenous opiate or
anxiolytic receptors embodies this principle. There are other sources besides
drugs for such modulations, both endogenous and exogenous. Endogenously, for
example, there are numerous complex rhythms of hormones, physiological func-
tions, and other body constituents. Exogenously, one could consider the food
and other substances we take in or, more in a research or clinical context, to the
numerous procedures for physiological alteration from hormone administration
to brain lesions—the effects of all of these are subject to the categories of
behavioral variables known to modulate drug effects. Behavior-environment
variables and interactions thus take a significant and unique place in a commu-
nity of biological explanations.

1. ISSUES OF REDUCTIONISM AND CAUSALITY
inquire, whether the pattern of behaviors we classify as "analgesia" is identical with the activation of opiate receptors. Clearly not, although opiate receptor activation may be a component of an opiate-related analgesia as opposed, say, to aspirin-related analgesia. Many other variables contribute to the effect. Perhaps the collection of those variables could ultimately be described by the activation of many different kinds of receptors. Indeed, some experimental efforts have been made to characterize particular behaviors by hormonal or neural transmitter states and relate these to differential drug effects.

Although Bernard emphasized the value of focusing upon the organ-tissue aspect of biological function, his vigorous assertions of antivitalism gave support to the thesis that all life processes were physiochemical in nature, subject to deterministic laws. Skinner was to do likewise. He attempted to drive the spirits of animism and mentalism—homunculi, ghosts in the machine, mental states, etc., out of the temple of behavior, and at the same time to emphasize that behavior is subject to deterministic functional relations between the variables of history, contingency, context, and behavior itself. Skinner has, however, maintained a complex view of heterogeneous reductionism, asserting that behavior deserved its own place in the scientific sun (what Bernard had also said about physiology). Skinner (1974) is especially contemptuous of neurologizing—the view that behavioral events must be stated in conceptual neural terms, hypothetical constructs and the like. As to the role of real physiology/biochemical science and its relation to behavioral analysis, his concerns have been more pragmatic:

The theory of knowledge called Physicalism holds that when we introspect or have feelings we are looking at states or activities of our brains. But the major difficulties are practical: we cannot anticipate what a person will do by looking directly at his feelings or his nervous system, nor can we change his behavior by changing his mind or his brain. (p. 11)

As previously emphasized, explanation in the experimental analysis of behavior operates in a homogeneous reductive mode in that causal structure is embodied in a specification of the controlling variables of behavior which, in turn, enter into an organized set of functional relations. Functional relations can have the property that the direction of causality is merely a reflection of emphasis, a characteristic considered by many to be a principal weakness of a purely functional account. Thus one might focus on how differences in reinforcement frequency affect how a drug changes rate; or conversely, how differences in rate affect how a drug influences reinforcement frequency. The dynamic relationships that can operate between frequency of reinforcement and rate may be said to be modulated by the imposition of the drug. These are steady-state relations, specifying the reversibility exemplified in the ABA design so endeared by the behavior analyst. Historical variables embodied in drug or behavioral history may or may not be reversible. In the latter case, time is provided an arrow; or, in another idiom, temptation for trans-level (theoretical analyst might describe it) or the repertoire (i.e., potential) is something missing in a presentational analysis.

The physiologist of the fun happening inside the behavior over a behavioral analysis, b says, it is confined to function done today which affects the clearly that fact can be esti-physiologist to supply it. He exposed to contingencies of behavior in a different way, p invalidate the laws of a scientific action more nearly complete.

This is a view of the physiologist the environmental contingency at a temporal distance. Environmental features fit naturally into the physiologist's model which need not imply that all things are reducible to physiological/biochemical/biophysics, biochemistry or behavior, or, in turn, drug effects.

Three matters must be resolved in order to achieve such a reductional relations that provided bridging principles. This, in turn, reducing processes in reference to the constituents of the environment of the beha-physiological constituents. It mody dynamics to kinetic theories of the no inherent properties of the environment is identical with the "emergent properties" then at the relevant levels which Nothing in principle prevents behavioral principle is derived from biochemical laws. The value earlier derived not from kn
The physiologist of the future will tell us all that can be known about what is happening inside the behaving organism. His account will be an important advance over a behavioral analysis, because the latter is necessarily “historical”—that is to say, it is confined to functional relations showing temporal gaps. Something is done today which affects the behavior of an organism tomorrow. No matter how clearly that fact can be established, a step is missing, and we must wait for the physiologist to supply it. He will be able to show how an organism is changed when exposed to contingencies of reinforcement and only the changed organism then behaves in a different way, possibly at a much later date. What he discovers cannot invalidate the laws of a science of behavior, but it will make the picture of human action more nearly complete. (p. 215)

This is a view of the physiological/biochemical events as mediating between the environmental contingencies and behavior—a kind of aether mediates action at a temporal distance. The view discussed earlier of drugs serving as environmental features fits nicely here as the behavioral effects of drugs could be mediated by the physiological/biochemical aether. This view, in itself, does not necessarily imply that all drug effects or behavioral principles in general are reducible to physiological/biochemical events, in the sense that the laws of biophysics, biochemistry or physiology, by themselves yield the principles of behavior or, in turn, drug effects on behavior.

Three matters must be resolved to achieve a heterogeneous reduction. First, in order to achieve such a reduction, laws would have to contain terms or derived relations that provided bridges to, or a commonality with, statements of behavioral principles. This, in turn, is dependent upon some theory of the role of the reducing processes in reference to behavior, not to some putative inherent properties of the constituents of the reducing system. What one is actually talking about here is the verbal behavior of scientist, and not the “revealed nature” of physiological constituents. In the most successful of reductions—classical thermodynamics to kinetic theory of gases, statements about temperature were reduced to statements about the average kinetic energy of gas molecules. There are no inherent properties of molecules that would allow the observation that temperature is identical with the average kinetic energy of the molecules. So-called “emergent properties” then depend on theoretical systems adequately developed at the relevant levels which include laws to bridge one level with another. Nothing in principle prevents this possibility. Presently, however, no significant behavioral principle is derivable strictly from physiological, not to mention biochemical laws. The value of drug effects in the analysis of behavior discussed earlier derived not from known mechanisms of actions of drugs, say, at the
receptor level, but rather proceeded from already established behavioral effects. Drugs can modify behaviors in ways not possible with other behavioral procedures. For example, they may modify rates of responding without changing reinforcement frequency or other aspects of contingencies. Why or how this occurs in a biochemical context is not considered as part of a behavioral analysis or theory.

Second, as Skinner pointed out long ago (1938), the principles of behavior will provide the framework for a physiological analysis. It was classical thermodynamics that provided the impetus for kinetic theory and statistical mechanics. Reductive explanation has always proceeded from already known principles at one level to conceptual and empirical analysis of the sub-adjacent level. We are taught to look behind a phenomenon, or perhaps beneath it. However, as Mercer (1981) has emphasized in an analysis of hierarchical systems in biology, each level of the hierarchy provides the boundary conditions for interpreting the laws at the sub-adjacent level. Laws are essentially useless without boundary conditions. But the boundary conditions are not part of the laws, nor can they ever be. To quote from Mercer:

"Complexity . . . results from placing restraints on components to introduce organizing relationships and these are boundary conditions in the physical sense. Furthermore, in complex systems organized in a hierarchical fashion, the components on a physiochemical level may be under the control of organized elements on a higher level of the hierarchy, which in this case determine the behavior at the physiochemical level. (p. 18)"

Part of the boundary conditions for biological systems find their source in evolutionary processes which are essentially indeterminate in character in that the direction of evolution is not predictable from laws in the sublevels of the biological explanatory hierarchy. An important part of the boundary conditions of interest here are represented by history, context, and contingency. One might look at a drug as an additional constraint to the functioning of the physiochemical system, just as substrates impose organization upon enzyme systems. If the drug were to be considered as part of a physiochemical system, say a drug-receptor complex, then its action would still be subject to boundary conditions specified by behavioral variables.

Finally, one could argue that a physiological reduction of behavior or behavior-drug interactions would provide simplicity, in a Machian sense, by encompassing a wide range of facts into a small set of principles. As Skinner said more than 50 years ago "... I know of no simplification of behavior that can be claimed for a neurological fact" (1938, p. 425). Fifty years have not led us to revise that statement. This is not to say it will remain true. However, a perusal of Shepard's fine text, *The Synaptic Organization of the Brain* (1979), and the now classic Cooper, Bloom, and Roth (1986), suggests that the world of neurophysiology and neurochemistry is of an order of complexity far greater than it is usually given credit. A few transmitters were known, and largely to the peripheral nervous system. Storing and releasing perhaps one transmitter is irreversible. Different phyla carry out similar functions. Tissues with as few as 10^5 neurons may be to talk about pharmacologists have generated systems most than cholinergic and indole amines. This house key down the street who feasible strategy—but some of Mach's (1960) comment on Mach suffices him for a maximum of Mutual ignorance, however is attempts at synthesis and reduction. Behavioral pharmacology does behavior analysis with a disciplines as neurophysiology arrangement is ripe for stimuli significant behavioral variables really interpretable as reduce analysis will continue to play a interact with biological system.

established behavioral effects. These are sometimes corresponding without changing contingencies. Why or how this part of a behavioral analysis is usually given credit. A few short years ago, less than a half-dozen neural transmitters were known, and what one knew of their chemistry was confined largely to the peripheral nervous system. Now we see the brain as synthesizing, storing and releasing perhaps thousands of transmitters, hormones, modulators, etc. Synaptic action depends not simply on the special properties of these substances, but on exact geometries of fine cellular structures. The neural interactions themselves are highly nonlinear, time-dependent and, in significant cases, irreversible. Different phyla appear to have different neural organizations to carry out similar functions. Truly remarkable behaviors are exhibited by creatures with as few as $10^5$ neurones, but it takes perhaps 6 orders of magnitude more cells to be able to talk about those creatures. Those calling themselves psychopharmacologists have generally focused, with good reason, on those neurochemical systems most thoroughly studied by others—acetylcholine, the catechol, and indole amines. This may be a bit like the drunk looking for his lost house key down the street where there is the most light. Perhaps this is the only feasible strategy—but some researchers in this field often remind me of Ernst Mach’s (1960) comment on Descartes: “... a minimum of experience always suffices him for a maximum of inference” (p. 363).

Despite the dangers and limitations of heterogeneous reductionism, it is an inevitable process of the scientific enterprise. It proceeds most effectively from a careful homogeneous reduction which, in turn, provides the appropriate interpretations and explanatory scope and goals of a heterogeneous reduction. Of course, fields like behavior analysis and neurochemistry can proceed in parallel, each concerned largely, if not exclusively with its own domain. Clearly, the more knowledge gained within these separate domains, the more can be contributed by each to the construction of analytic and synthetic bridges between them. Mutual ignorance, however is only amplified by premature and grandiose attempts at synthesis and reduction.

Behavioral pharmacology seems to be in a fortunate position, bridging as it does behavior analysis with areas of pharmacology, including such supporting disciplines as neurophysiology, neurochemistry and neuroanatomy. While this arrangement is ripe for stimulation of heterogeneous reductions, as long as most significant behavioral variables are specified at the behavioral level and not really interpretable as reduced events in a physiochemical domain, behavior analysis will continue to play an essential role in our understanding of how drugs interact with biological systems.

REFERENCES


The initial suggestion that the alterations in stimulus control pharmacology by Dews (1958) drugs could depend on four ch helpless (3) what the environment is like and discriminative stimuli affect the past. Dews suggested further determined by environmental gested that chlorpromazine in control of behavior.

Evidence for this conclusion respond under a multiple fixed rule, responses were reinforced presence of blue lights and occurred. Once performances were maintained in TROL, indicating that the stimuli a single dose of chlorpromazine the early portions of the fixed-r