SOME THOUGHTS ON THE RELATION BETWEEN
BEHAVIOR ANALYSIS AND BEHAVIORAL NEUROSCIENCE

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A comprehensive science of behavior is concerned with two related, but nevertheless distinct, questions. The first question is "How is an organism’s behavior functionally related to its environment?" The second question is "How do the organism’s neural and hormonal systems mediate those functional relations?" Behavior analysis addresses the first question, whereas behavioral neuroscience addresses the second. The neural and hormonal information that behavioral neuroscience provides is important for a comprehensive science of behavior because the information enhances the possibilities for the prediction and control, rather than because it logically validates an explanation of behavior. Although cognitive psychology is ostensibly concerned with internal events, it reflects the influence of social and cultural mentalistic traditions more than the means by which neural and hormonal systems mediate functional relations.

A comprehensive science of behavior is presumably concerned with two interrelated questions:
1. How is an organism’s behavior functionally related to its environment?
2. How do the organism’s neural and hormonal systems mediate those functional relations?

Behavior analysis deals with the first question and the relation between environmental circumstances and organism called behavior. These relations exist at phylogenic, ontogenic, and cultural levels (Catania & Harnad, 1988). Selection by consequences is the significant causal mode at these three levels, and there are parallels across the three levels regarding (a) the unit that is selected and (b) the consequence that does the selecting.

However, the second question differs from the first, and consequently a science that differs from behavior analysis is needed to deal with it (Donahoe, 1996; Reese, 1996). Behavioral neuroscience is just that.

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different science. It is concerned with the operating characteristics of the underlying neural and hormonal systems (a) within a behavioral event, and (b) between one behavioral event and the next. For example, within a behavioral event, we might be interested in the neural and hormonal continuity from the time the organism comes into contact with an antecedent stimulus through the time the response occurs. Alternatively, between behavioral events, we might be interested in the neural and hormonal continuity from one experience to the effects of that experience as measured in the future. In this regard, readers may note that Skinner (1974, p. 221) has suggested that a behavior-analytic account has two “unavoidable gaps,” and that a different science is needed to “make the picture of human action more nearly complete” (see also Catania & Harnad, 1988, p. 470). We shall discuss some implications of Skinner’s statements later in the present article, but for the moment, let us emphasize that events providing the neural and hormonal continuity spoken of above are not stimuli in the behavioral sense. If the events are construed as stimuli, the analysis remains at the behavioral level. Neural and hormonal factors are not stimuli in the behavioral sense, so the analysis that is being proposed here is not to study neural or hormonal factors and call them stimuli, or to say the science studies private stimuli. The subject matter identified by the second question is not behavioral stimulation in any sense, and it requires a categorically different science to address it.

Contributions of Behavioral Neuroscience to Causal Explanations of Behavioral Events

Given the distinction above between the two sciences, we can summarize the information that neuroscience provides in the following way:

1. How neural and hormonal systems provide continuity between stimulus and response within a behavioral event,
2. How neural and hormonal systems are changed by experience,
3. How neural and hormonal systems store those changes from one behavioral event to the next,
4. How a changed organism behaves differently in the future,
5. How the internal biochemical context modulates stimulating action of the environment.

This same set of contributions holds true for allied disciplines, such as behavioral pharmacology and behavioral toxicology, which study the effects of various substances on an organism’s physiological and behavioral systems. The substances examined in such disciplines change the organism, by selectively affecting (a) sensory systems mediating input/responsiveness to environmental stimulation or (b) motor systems mediating output or (c) links between sensory and motor systems. Nevertheless, the information provided in the allied disciplines remains the same.
Genetics, Selection by Consequences, and Behavior

_Innate Behavior, Sensitivity to Contingencies, and Selection By Consequences_

Let us develop this point of view a bit further, by considering the contribution of genetics to a science of behavior. We begin by considering the topic of innate behavior, selected by “contingencies of survival.” Suppose certain behavioral traits and characteristics are distributed across a group of organisms, as a result of the genetic endowment of those organisms. These traits and characteristics are essentially various forms of unconditioned responsiveness to environmental stimulation. Ethologists might label these forms of unconditioned responsiveness as taxes, kineses, fixed action patterns, or reflexes.

Now suppose that certain forms of responsiveness favor survival. Organisms that possess these forms survive for one reason or another. Those that do not will perish. The organisms that scientists study in today’s laboratory are necessarily members of species that have survived. They have had these forms of responsiveness transmitted to them through the DNA they have inherited from preceding generations. The forms of innate responsiveness contribute to a behavioral definition of a species, as a kind of a behavioral phenotype.

Let us next consider conditioned respondent behavior. Respondent conditioning may be construed as a behavioral process that contributes to an organism’s survival during its lifetime by allowing the organism to adapt to its environment. Respondent conditioning occurs when stimulus A, which initially did not elicit a response within a particular response system, comes to do so because of a close and consistent relation between stimulus A and stimulus B, which initially did elicit a response within the response system in question. The exact nature of the “close and consistent relation” is, of course, a matter of ongoing experimental analysis.

Thus, suppose we have a wide variety of organisms. The genetic endowments of some of these organisms have yielded nervous systems that, given certain experiences during their lifetimes, will change in the way that results in the behavioral process called respondent conditioning. Of course, some of the taxes, kineses, fixed action patterns, and reflexes in a given species might not be susceptible to respondent conditioning, which is to say that the nervous systems mediating these processes will not change in the way that constitutes conditioning. The field of biological constraints is concerned with many of these relations.

More concretely, let us consider the case of Pavlov’s dogs. Dogs whose salivary systems respond to the presentation of food itself or to another stimulus that is correlated in a reasonably regular and reliable way with the presentation of food may have a survival advantage over those that do not: The response of the salivary system may mean the dog can swallow or metabolize the food more easily. Alternatively, the capacity for neural systems to change in the way called respondent conditioning could have come about as an incidental by-product of other features that have arisen through selection pressures, much as the spandrels of San Marco arose as
an incidental by-product of architectural practices, rather than deliberately as a space for artwork (Gould & Lewontin, 1979). In either case, the genetically mediated capacity for this behavioral change is an essential aspect of the behavioral makeup of the organism.

Let us conclude with a third case of behavior: operant behavior. Operant conditioning may be construed as another behavioral process that develops and contributes to an organism's survival during its lifetime by allowing the organism to adapt to its environment. Certainly an organism is more likely to survive if it can adapt on the basis of the prior consequences of its behavior. Operant conditioning occurs when the probability of a response in the presence of an antecedent stimulus increases because of the prior consequences of the response in the presence of the stimulus in question. The exact nature of the relations that must prevail among antecedents, responses, and consequences is, of course, a matter of ongoing experimental analysis.

Again, suppose we have a wide variety of organisms. In some organisms, the environmental consequences of certain actions bring about certain changes in their neural systems, and the behavior becomes more probable because those consequences produce those changes. In other organisms, the consequences produce no such changes in their neural systems. Organisms whose nervous systems do change in this way flourish because of differential access to resources. For example, the behavior of seeking food or avoiding predators might be strengthened, affording obvious survival advantages. Organisms whose nervous systems do not change will perish. The behavioral entity that results from this process, or that stands at the end of the lineage at the time in question, is called an operant: Its members function equivalently, and its characteristics can be traced to a common ancestor. The consequence does not have to be directly related to a life-maintaining event, as in drive-reduction, only to a process that proves valuable in some other sense and is passed on to future generations. In short, either (a) a designated response or (b) a response with a new topography in an effector system comes to be made to a given stimulus principally by virtue of the correlation between the stimulus and the response's having had that consequence in the past.

To be sure, responses are not operants the first time they occur; they must occur for some other "reason" before they become operants. Thus, the responses that come to be controlled by their consequences could originally be, say, random uncommitted behavior. In addition, the possibility exists in a given species that a form of behavior, including some of the aforementioned uncommitted behavior, might not even be susceptible to operant conditioning. That is, the nervous systems mediating these forms of behavior will not change in the way that yields operant conditioning, with respect to any of the relations between or among the antecedent stimulus, the response, and the consequence. The organism might not be susceptible to some form of stimulation from the environment. It may not be able to engage in the topography in
question, or even if it could, the response might not come under operant control. The organism might not be susceptible to reinforcement by the consequence. As with respondent conditioning, the field of biological constraints is concerned with many of these relations.

As is the case with conditioned respondent behavior, the specific physiological means of replication, retention, and transmission with operant behavior could be said to be "neural changes." This term is admittedly vague. Whether it should imply synaptic mechanisms, long-term potentiation, or some other mode of cellular plasticity is not currently known. Recent theorists emphasize the importance of adaptive neural networks, which work through synaptic changes. These biobehaviorally informed interpretations of the events that take place within the skin would account for the variation, selection, and retention of behavioral characteristics across time, in a way that is analogous to the way that DNA accounts for the variation, selection, and retention of morphological characteristics across time (for example, see Donahoe, Palmer, & Burgos, 1997). In any event, behavior analysis awaits something for "changes in behavior" that is the equivalent of DNA for "changes in morphology." More will be said about this point in a later section of the present article.

Perhaps the most important point to make about applying the thesis of selection by consequences to behavior and its relation to genetic structure is that it does away with the self as an initiator or creator. That is, evolution does away with teleology and "grand designs" in nature with respect to morphology. One need not invoke a prior agent or vital force as a causal process to understand the development of a species. A comparable viewpoint regarding a causal mode has been a long time coming in the analysis of behavior. The traditional view has the "free" individual initiating action according to mental or psychic processes. Not only is there an ontological question of how something mental can cause something physical like behavior, but also there is the question of whether behavior is at heart unrelated to any determining factors. In the traditional view, it is not related to determining factors.

**Contribution of Genetics to Causal Explanations of Behavioral Events**

We can now summarize the information that genetics provides in the following way:

1. Sensitivity of the nervous system to environmental stimulation,
2. Sensitivity of the nervous system to contingencies: CS-US contingencies for conditioned respondent behavior, and S\textsuperscript{D} : R \Rightarrow S \textsuperscript{R}\textsuperscript{+} contingencies for operant behavior,
3. Context for the form/topography of species-specific, elicited, and emitted behavior,
4. Supply of uncommitted/random behavior, from which environmental circumstances select operant behavior, especially including verbal behavior in humans.

Thus, we see first that a science of behavior is concerned with behavior directly selected according to phylogenetic contingencies, via
inherited genetic mechanisms. Second, a science of behavior is concerned with behavior selected by the material environment during the lifetime of the individual organism. Behavior analysis contributes the study of these relations to a science of behavior. Third, a science of behavior is concerned with behavior selected by the social/cultural environment during the lifetime of the individual organism. Various forms of behavior help the society and culture survive, and these forms are then transmitted to new members of the society and culture for their adoption. Cultural anthropology contributes the study of these relations to a science of behavior. In the second and third cases, the genetic structure has to be such that the organism’s nervous system can change during the lifetime of the organism to yield the kind of behavioral adaptation in question, and understanding these neural changes will help us to understand the development of important classes of behavior.

*Genetics, Behavior, and “Intermediate Steps” vs “Terminal Relations”*

As noted earlier in the present paper, there is a parallel between the current state of behavior analysis and genetic theory in biology prior to DNA (Skinner in Catania & Harnad, 1988, p. 111). Let us formalize this parallel in the following way:

Darwin => Mendel => DNA and “Grand synthesis” // Watson => Skinner => ? That is, Darwin had some very important ideas in the 19th century about variation and selection in morphology. However, he was not able to specify how a given change in morphology was mediated across generations. Mendel actually performed some experiments and formulated quite accurate quantitative relations between parents and offspring of pea plants with respect to yellow vs. green coloring, smooth vs. wrinkled skin, and so on. Although he developed quantitative laws, he too did not identify the physiological mechanism according to which replication, retention, and transmission take place. The grand synthesis awaited the rediscovery of Mendel plus the discovery of DNA, which provided the mechanism.

In psychology suppose we say that Watson had some very important ideas, and Skinner actually performed some experiments and recorded some orderly data some years later. Skinner and behavior analysis dealt with variation and selection and developed some quantitative laws. However, Skinner and behavior analysis were not able to specify how a given change in behavior was mediated across time.

The argument here is that prior to DNA, the science of genetics was concerned with terminal relations, rather than intermediate steps. The discovery of DNA and the grand synthesis established the intermediate steps. Nevertheless, the discovery of DNA did not disprove Darwin or Mendel. Mendel’s laws remain as accurate as always.

Behavior analysis is also concerned in one sense with terminal relations. It can make quantitative statements about the terminal relations between certain environmental variables and resulting behavior, but not any neural or hormonal mechanisms that are the intermediate steps
according to which those terminal relations are replicated, retained, and transmitted. Behavior analysis is concerned with the fact that a behavioral event that takes place on Monday may exert an effect on Tuesday, but not with the neural or hormonal changes that take place and are stored from Monday to Tuesday, such that the organism is a different organism on Tuesday than it was on Monday. In that sense, behavior analysis awaits a contribution from neuroscience about the intermediate steps. That contribution is comparable to that which genetics awaited from biochemistry and DNA. In any event, that knowledge will not disprove the quantitative relations developed in the experimental analysis of behavior. Those quantitative relations will remain as accurate as always.

Let us make certain parallels between the selection of morphology and the selection of certain forms of behavior by the environment more concrete. We can say that the population in question at some initial time T1 contains a mixture of characteristics. The lengths of necks on giraffes are varied; the reflexive responsiveness to stimuli is varied. We can say that the population at some later time T2 has changed in some way, because of the interaction with the environment. The average length of necks on giraffes has increased, say because the environment changed and favored longer necks by affording differential access to food; the average reflexive responsiveness to stimuli has increased, say because organisms with that responsiveness are better able to survive. The difference between the way populations are represented at T1 and T2 implies that some of the members of the ultimate T2 population are in fact present at T1, but just not very many of them. They become more frequent in the T2 population because of some interaction with the environment over time.

Despite the foregoing parallelism, the selection of morphology by the environment does differ from the selection of certain forms of behavior. Most notably, although changes in morphology and innate behavior proceed according to Darwinian natural selection, conditioned respondent and operant behavior might be said to reflect Lamarckian principles. The characteristics that are transmitted to the next generation are acquired during the lifetime of the organism in question. The neural changes that are acquired through experience are then retained, such that they contribute to future behavior of the new type. In the case of behavior, the future means responses will occur more frequently in the same organism in the future, rather than the same response will occur more frequently in future organisms. The difference between the way the populations are represented at T1 and T2 does not imply that some of the neural changes ultimately present at T2 are in fact present at T1, and that they in fact are more frequent at T2 because of some interaction with the environment over time. The neural changes are not present at T1 in a given set of neurons, but they are present at T2 in the same neurons because of environmental interaction. (Certainly cultural changes that are acquired during the “lifetime” of the culture are transmitted in a Lamarckian fashion to the next generation; verbal behavior plays a key role in the transmission.)
Given that Lamarckian selection is generally discredited as a process, at issue is whether conceiving of behavioral selection as Lamarckian means theorists need to reconsider the nature of the behavioral unit that is selected. Notwithstanding his own bias toward the gene as the unit of Darwinian natural selection, Dawkins (1988) comments that “if habits are analogous to anything in the Darwinian scheme, it is to genes, not to individual organisms. But they are clearly not very close analogues of genes, and this makes the whole application of the Darwinian analogy at this level difficult” (p. 33).

Behavior Analysis, Neuroscience, Private Behavioral Events, and Cognitive Psychology

Some Speculative Neurophysiology
Ordinarily, bodily states or activities of the intact organism are the objects of introspection. The interoceptive and proprioceptive nerves that detect these bodily states and activities are the medium of contact. What we call the stimulus is the bodily condition, not necessarily the activity in the sensory nerves produced by the bodily condition. More complex cases such as “phantom limb” pain or “referred” pain suggest the physiological mechanism is not restricted to responding to stimulation from the sensed area, but rather will respond given a wide variety of peripheral stimulation to appropriate nerve fibers.

Of course, even with a plausible account of how introspective reports develop, terms describing private events tend to be inexact for two reasons. The first reason is functional. The verbal community that teaches us to apply terms to our private events works under the handicap of privacy: The verbal community does not have precise access to the described conditions, and there may be some inconsistencies across the actual antecedent conditions under which reinforcement is administered. Thus, there is considerable variation as the usage develops.

The second is structural and more in keeping with the neural or hormonal question. At issue is whether we have nerves going to the “right places” (Skinner, 1974, pp. 221-223). For example, the structuralists claimed that with proper training, one might introspect as many as 42,415 different sensations (e.g., Lundin, 1991, p. 88). The sensations were held to be different activities or states of the central nervous system (CNS). However, we simply do not have sensory nerves going to places that make it possible for us to discern that many sensations, even if our interactions with the verbal community were favorable. We reveal a lack of sensitivity in a two-point limen test in the lower back because of the low representation of sensory information, and we presumably have a corresponding lack of sensitivity with respect to the activities of the CNS or other activities going on inside our skins because of low representation of interoceptive and proprioceptive information. People who make various sophisticated claims about their “sensations,” “feelings,” or “states of mind” are simply making fanciful statements that presumably have little
basis for being valid, because they do not have interoceptive or proprioceptive contact with anything that could be identified as the source of such sensations.

In regard to the question of sensitivity to internal events, Natsoulas (e.g., 1983, 1985) has raised a number of incisive concerns about the behavioristic approach. For example, Natsoulas (1983) suggested that

the behaviorist account holds that all awareness of anything requires that whatever it is stimulate one or more of our sense receptors. In addition to a form of responding’s being necessary for awareness of anything, the activation of sense receptors is also necessary. Otherwise, we cannot do what is supposed to be necessary for awareness of anything, namely, that which is called by radical behaviorism “respond to it.” (p. 21)

Natsoulas’s point is characteristically well-taken: For differential responses to develop to private states, there must presumably be some sensory contact with them. So far as we know, this sensory contact is provided by the interoceptive and proprioceptive nervous systems (e.g., Skinner, 1974, pp. 20-21). Whether this contact is central or peripheral is an empirical question, and it may need to be examined on a case-by-case basis.

Watson (1913, p. 174) was of course biased against including any centrally initiated processes. He originally thought the only possibility was for peripheral processes to influence behavior. Skinner (1957) declined to “make guesses about the muscular or neural substratum of [covert] verbal events . . . we can talk about both [covert and overt] forms of response . . . without identifying physiological mediators” (p. 435). Nevertheless, Skinner (1957) repeatedly appealed to “subaudible,” “incipient,” and “inchoate” forms of verbal behavior (e.g., pp. 143, 400). Although Skinner (1957) noted “difficulties in assuming that covert behavior is always executed by the muscular apparatus responsible for the overt form” (p. 435), he later stated that “So far as we know, the [covert] responses are executed with the same organs as observable responses but on a smaller scale” (Skinner, 1969, p. 242). Indeed, in a still later writing he stated

I agree that the kind of thinking which seems to be merely covert behavior (“truncated, unemitted, reduced, impotent behavioral acts”) may be so reduced that there is no muscular involvement to be sensed proprioceptively. Must we appeal to some minute behavior which never reaches a muscle? If so, it is a problem for the physiologist. There is a possibility that the effect is sensory. I believe that my analysis of seeing makes this a possible alternative . . . . I see no reason why we should not also call the action of efferent nerves behavior if no muscular response is needed for reinforcement. That may occur in the thinking that retreats beyond the point at which muscular action can be detected. (Skinner in Catania & Harnad, 1988, p. 331, 485)
Perhaps the point of contact between sensory and motor systems, or between different aspects of sensory systems, will prove to be fairly central after all.

Natsoulas (1985, p. 93) has also questioned Skinner’s statement that we do not have nerves going to the right places. For example, Natsoulas (1985, p. 89) has asked “Why cannot brain processes be objects of introspection?”, but the matter has not been resolved. Standard texts in neurophysiology such as Shepherd (1979, p. 365) point out that perhaps as many as one third of the motor neurons in the cortex are located outside the traditional motor cortex of the frontal lobe and are actually in classical somatosensory areas of the parietal lobe. Perhaps the overlapping pathways with the collaterals and various other projections provide a relatively central point of contact between “motor” functions and “sensory” functions, given the traditional dichotomy. Thus, there may well be relatively central points of contact that plausibly account for the development of some kinds of introspective reports (cf. Skinner, 1974, p. 223). However, even if talk of this kind of sensory contact is valid, the sensory contact is presumably insufficient to support the many extravagant claims made by contemporary traditional psychologists about the relation between brain processes and behavior as it involves introspective reports and private events.

**Private Perceptual Responses**

One of the more intriguing features of behavior analysis is that perception may be treated as a behavioral process, in the sense that it is controlled by antecedent and consequent relations, as are other behavioral processes. For example, Skinner (1953, pp. 273-274) talks of unconditioned seeing, (classically) conditioned seeing, and operant seeing. Unconditioned seeing occurs when what is seen is controlled by the features present in seen object itself. One sees the object as it is when presented under optimum conditions, there are no conflicting circumstances, and so on. Conditioned seeing occurs when what is seen is controlled by antecedent features associated in a regular and reliable way with the object, rather than the object itself. One might “see” a completed ring when a ring that actually had a small segment missing was presented for a very brief time. Operant seeing occurs when there is some consequence for seeing. The distinctive feature of operant seeing is that there is a consequence for seeing something: dog lovers may see a dog when presented with an ambiguous figure because dogs are important, chess players may see patterns of movements with pieces because winning is important, and visual imagery may be used in problem solving because it mediates a solution and solving the problem is important. The process depends on certain events in the prior experience of the individual, however.

Operant seeing can occur at all because of the stimulation derived from the act of perceiving, rather from the object perceived (Skinner, 1953, pp. 263, 273). As before, there are presumably points of contact
between the sensory system and other response systems that do not involve light energy in the case of vision or auditory energy in the case of audition. The points of contact, as Natsoulas (1983) has suggested, allow the act of perceiving to contribute to discriminative control over subsequent behavior, rather than merely the content of what is perceived. One can see in the absence of the thing seen, but the response of seeing may be controlled partly by collateral internal stimulation, rather than by any “mental image” where the term “mental” implies another dimension beyond the behavioral.

Confirmation of Interpretations About Private Events qua Behavioral

Much of the behavior-analytic verbal material on the neural or hormonal underpinnings of private events is admittedly interpretive, and the promissory note is offered that neurophysiologists will eventually confirm or at least correct the technical details of what behaviorists have been saying (cf. Schnittler, 1984, 1986). Moore (1998, pp. 207-208) has defined interpretation as the use of scientific terms and principles in talking about facts when too little is known to make prediction and control possible, or when precise manipulation is not feasible. An example of interpretation is the theory of plate tectonics. This theory is an interpretation of a vast number of facts about the nature of the earth’s crust. It uses terms and principles taken from much more accessible material and from experimental analyses and their technological applications. The basic principles governing the behavior of material under high pressure and high temperature can be studied in the laboratory under controlled conditions, but no one literally manipulates continental landmasses and seismic events in geoscience. Rather, the role of plate tectonics in explanations of the formation of surface features of the earth is interpretation (example taken from Skinner in Catania & Harnad, 1988, pp. 207-208). The point is that once the interpretive verbal behavior is emitted, it can be confirmed by generating additional discriminative stimuli that increase the probability of the verbal response being emitted as a tact and thereby having the ability to occasion effective action (Skinner, 1957, p. 425). The issue of confirmation is critical because statements on private events qua behavioral can be confirmed by greater knowledge of the underlying physiology.

The second issue on which more information is needed is the neurophysiological structures that detect the occurrence of a private response. Whether it is meaningful to say that our interoceptive nervous system senses CNS activity, or whether as yet unknown mechanisms of contact with CNS functions will be discovered, is not altogether clear, as Natsoulas (1983) and Schnittler (1984) have incisively pointed out (see also Creel, 1980). Neurophysiological activity of some sort participates in every behavioral event. Is consciousness a brain process (Place, 1956)? The self-descriptive verbal responses we call “consciousness” presumably entail brain processes in the same sense that a rat’s lever pressing entails brain processes. Presumably, one can not claim any
event of behavioral significance has transpired without there being neurophysiological activity that can be known about, in the brain or elsewhere. Nevertheless, concepts derived from neurophysiological activity do not explain instances of behavior, at least as behavior analysts ordinarily use the term explain (Moore, 2000a). Consider the rat's lever pressing. Neurophysiology is involved in the motor activity of pressing the lever, and no doubt the brain is actively processing neural impulses from many locations in the rat's nervous system. However, a knowledge of how physiology mediates discriminative control does not allow one to express the facts of which feature of the environment actually exerts that control.

Cognitive Psychology

Moore (1996) has suggested that cognitive psychology is concerned with specifying in an abstract fashion the functional properties of the underlying "Amental" acts, states, mechanisms, processes, structures, and entities that afford competence. Importantly, the internal phenomena are explicitly not behavioral. How does one make sense of the writings about cognitive psychology, and how does it relate to the intradermal issues being discussed here? As Kantor (1947) noted, physiological factors constitute participating components in larger interactional systems, but is it the case that we should regard the postulation of physiological substrates, especially in the nervous system, as the valid way to invoke mental processes as causes of behavior?

For behavior analysis, questions about cognitive psychology and mental processes are to be engaged as questions about verbal processes. Because there is no literal mental dimension, there can be no mental acts, states, and so forth, to occasion the talk, so there must be discriminative stimuli and reinforcers from the behavioral dimension that control the verbal behavior we call cognitive.

Skinner has commented extensively on the nature and origin of mentalistic language in cognitive psychology (e.g., Skinner, 1945, pp. 292-293; Skinner, 1974, pp. 165-166, 169; Skinner, 1978, p. 77, 81; Skinner in Catania & Harnad, 1988, p. 447, 472). In short, Skinner's argument is that

[T]he reasons for the popularity of cognitive psychology . . . have nothing to do with scientific advances but rather with the release of the floodgates of mentalistic terms fed by the tributaries of philosophy, theology, history, letters, media, and worst of all, the English language. (Skinner, 1987, p. 111)

Many of these points were anticipated by Woodworth (1921) in a perceptive statement made years before:

Instead of "memory", we should say "remembering"; instead of "thought" we should say "thinking"; instead of "sensation" we should say "seeing, hearing", etc. But, like other branches, psychology is prone to transform its verbs into nouns. Then what
happens. We forget that our nouns are merely substitutes for verbs, and go hunting for the things denoted by the nouns; but there are no such things, there are only the activities that we started with, seeing, remembering, and so on.

Intelligence, consciousness, the unconscious, are by rights not nouns, nor even adjectives or verbs; they are adverbs. The real facts are that the individual acts intelligently—more or less so—acts consciously or unconsciously, as he may also act skillfully, persistently, excitedly. It is a safe rule, then, on encountering any menacing psychological noun, to strip off its linguistic mask, and see what manner of activity lies behind. (pp. 5-6)

Moore (1981, 2000b) has suggested that verbal behavior of cognitive psychologists is presumably under highly complex multiple control, just as verbal behavior of other scientists is under highly complex multiple control. Part of the control is legitimate, to be found in scientific operations and contact with data. However, part is spurious, to be found in social-cultural verbal traditions. Thus, behavior analysis views cognitive talk as being occasioned partly by private behavioral events or functionally organized neural or hormonal systems, and partly by social-cultural traditions. At issue is the balance between the two sources. On a behavior-analytic view, cognitive talk invoking causal mental processes is controlled to a great extent by social-cultural traditions, and cognitive psychology would be better off to maximize control by scientific operations and contact with data, and to minimize control by social-cultural traditions. If an explanation does appeal to causal phenomena from the mental dimension, then one needs to analyze that explanation so as to determine whether it is occasioned by one or more of the following factors, and make the accompanying decisions regarding its nature and suitability:

1. Processes or relations that are actually in the behavioral dimension, whether publicly observable or private. In this case, the purported mental phenomenon is appropriate for analysis as a private behavioral phenomenon. On this view, any explanation involving a private behavioral phenomenon needs to be further connected, at least in principle, with the public processes or relations that are responsible for the private phenomenon exerting an effect in the current instance; or
2. Neural or hormonal processes, in which case the mental term is appropriate for physiology but only in a limited way for psychology, as described above; or
3. Complex social-cultural epistemological preconceptions, in which case the term is a fanciful explanatory fiction (e.g., from “folk psychology”), and is of interest only in regard to the social and cultural conditions that promote its use, rather than as a genuinely explanatory term. No such mental dimension exists, and no such causal mental phenomena exist in this dimension. Talk of such causal phenomena in a mental dimension is occasioned by other factors, rather than those that cause behavior.
Thus, the argument here is that appeals to causal mental states and cognitive processes are occasioned too much by social-cultural factors, and not enough by operations and contacts with data. By virtue of this imbalance, the explanatory talk may be dismissed. Although we might question whether we are dismissing something that we should not, on balance the dismissal seems justified (see further discussion in Moore, 2000b, p. 151). On this view, is cognitive psychology a legitimate theoretical neuroscience? For behavior analysis, the answer is clearly not: “cognitive constructs give physiologists a misleading account of what they will find inside” (Skinner, 1978, p. 111).

Implications and Conclusions: How to Specify the Relations Among Behavior Analysis, Behavioral Neuroscience, and Contemporary Cognitive Psychology

Complementary?

Moore (1997, p. 242) suggested behavior analysis and neuroscience were complementary. Use of the term complementary implies that the more that is known of one factor, the less needs to be known of the other factor to predict or control behavior adequately. Thus, the term implies a sense of cooperative rather than competitive, mutually supportive rather than mutually exclusive, and reciprocal rather than restricting (see also Donahoe, 1996).

Given this sense of complementary, how then might we portray the various relations among the sciences? Consider the pairwise comparisons:
1. Behavior analysis and behavioral neuroscience: complementary,
2. Behavior analysis and cognitive psychology: not complementary because of the nature of cognitive psychology,
3. Behavioral neuroscience and cognitive psychology: not complementary because of the nature of cognitive psychology.

Comparisons 2 and 3 above are regards as not complementary for the same reason that the phlogiston theory of combustion is not complementary with the oxygen theory. Phlogiston is a fanciful approach, in that it can be shown through an analysis of verbal processes to be largely a function of spurious factors. It may have been popular at one time in the history of science, but it was superseded by an explanation that incorporated naturalistic factors that exist in space and time. Cognitive theories in psychology are examples of fanciful theories, like phlogiston in chemistry. A critical examination of the factors that are responsible for them suggests they can be attributed to spurious factors that are cherished for irrelevant and extraneous reasons (Moore, 1996).

“Interaction” Between Nature and Nurture?

Sometimes we speak of an interaction between nature and nurture. Let us critically examine this sort of language. Intuitively, the term “interaction” implies a relation between two or more instances of same kind of cause. However, neuroscience and behavior analysis deal with different kinds of causes. Recall the two sciences address two different questions.
Consider Aristotle's treatment of causation, and how the categories can be interpreted in an abstract sense to apply behavior analysis. Although Winston (1987) has argued persuasively against the interpretation, perhaps the comparison will nevertheless be useful for the purpose of illustration:

<table>
<thead>
<tr>
<th>Aristotelian 's 4 Causes and Corresponding Behavior-Analytic Interpretation</th>
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<tbody>
<tr>
<td><img src="image" alt="Table 1" /></td>
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</table>

This table seeks to interpret the sense of efficient, formal, final, and material causes in the language of behavior analysis (see also Moore, 1984). Accordingly, efficient causes may be interpreted in an abstract sense as the contingency, formal cause as discriminative control, final cause as reinforcement, and material cause as the physiology of the sentient and behaving organism. The table, then seeks to reflect the varied sense of the term cause, when we answer questions dealing with why an object or event is what it is.

On this view, it is confusing to say that there is an "interaction" between nature and nurture. Interaction implies same kind of cause, as in interaction between two or more efficient causes, formal causes, etc. Nature is a material cause. Nurture could be any of efficient, formal, or final causes. In any event, we are talking about several different senses of the term cause. Refraining from saying "interaction between nature and nurture" means we reduce the danger of taking a factor as being one kind of cause when it is actually another, for example, of equating a material cause with a formal cause. Material and formal causes may co-vary, as when the material out of which a statue is carved must be amenable to the actions of the sculptor so that it can be shaped according to a design, but material causes are not identical with formal causes.

**Explanation and Physiological Reductionism?**

Earlier in the present article, we noted that Skinner talked of two "unavoidable gaps" in a behavioral analysis. The first is within a behavioral event, and the second is between behavioral events. Let us critically examine this and other of Skinner's language, because it is potentially very troublesome.
If behavior is a subject matter in its own right (Skinner, 1938, p. 440), then presumably explanations of that behavior do not have to engage a subject matter at a different level to be valid. However, Skinner has also said

Eventually, we may assume, the facts and principles of psychology will be reducible not only to physiology but through biochemistry to physics and subatomic physics. (Skinner, 1972, p. 303)

and

I agree that “only an account of the machinery within the skin can explain behavior.” (Skinner in Catania & Harnad, 1988, p. 334)

The use of such terms as “reducible” and “only,” as well as the entire “gaps” argument (e.g., Skinner in Catania & Harnad, 1988, p. 470) is troubling because such language implies that a behavioral account is necessarily limited in principle, and that it can never identify all the relevant factors that are necessary to secure an adequate explanation. Ironically, Skinner seems to be guilty of reductionism here, by saying that explanations in behavior analysis are really limited in principle, because they don’t identify underlying physiological mechanisms, and that behavior analysis is just something to do until a sufficiently sophisticated neuroscience comes along and provides the “ultimate” and “complete” explanations for behavior in terms of those mechanisms (see also Reese, 1996).

One way to resolve the problem of reductionism is to recognize that neurophysiology is concerned with the mediation of functional relations between the organism and environment (Skinner, 1969, p. 283). As such, if the current neural or hormonal states are known, they may be used as a basis for prediction, manipulation, and control, instead of a possibly inadequate specification of history in terms of environmental interactions. On this view, behavior analysis and neuroscience provide mutual and reciprocal support for each other; neuroscience does not provide the logical grounds for validating behavior-analytic explanations. As suggested earlier, behavior analysis and a theoretical behavioral neuroscience may therefore be regarded as complementary sciences, in the sense of mutually supporting. In a more practical vein, physiological information, such as how an organism has been changed by interactions with its environment during its lifetime, can compensate for a possibly inadequate behavioral specification of those interactions as a basis for making predictions or taking direct action. Overall, behavior analysis gives neuroscience one of its directions, just as Mendel’s studies of the numerical relations among the traits of successive generations of pea plants gave the study of the gene one of its directions (e.g., Catania & Harnad, 1988, p. 470).

Pragmatic Basis

More formally, then, we can state that if a primary goal of behavioral
science is to manipulate, predict, or control behavior, or to provide a sufficient data base that occasions such actions, then the following conclusions are appropriate:

1. In principle, knowledge from either behavior analysis or neuroscience can serve as adequate basis for attempts to manipulate, predict, and control behavior.

2. If knowledge from one domain is limited, attempts to manipulate, predict, and control behavior on the basis of that knowledge might be made more effective if they are informed by the other science.

3. Neuroscience is not necessary to provide the logical or empirical grounds for validating behavior-analytic theories, explanations, or technological advances.

The link between behavior analysis and either genetics or neuroscience is therefore pragmatic. Each science can inform the other in its characteristic way, but genetics and neuroscience are not necessary to validate behavior-analytic explanations, any more than behavior analysis is necessary to validate explanations in genetics or neuroscience (see also Baer, 1996).

*Summing Up: The Complementarity of Behavior Analysis and Behavioral Neuroscience*

In retrospect, then, behavior analysis and neuroscience support each other by virtue of their respective contributions to direct action, as opposed to logical/theoretical/explanatory contributions. Direct, effective action with respect to behavior (manipulation, prediction, control) can be predicated on knowledge of (a) environmental history, (b) neural or hormonal states produced by noncontrived interactions with environment, or (c) the way that contrived neural or hormonal interventions/manipulations have modified the organism so that it responds differently than otherwise to the stimulating action of its environment.

Nevertheless, an independent science of behavior as a function of environmental circumstances is necessary for at least three reasons: (a) We may not know how to control behavior through neural or hormonal interventions/manipulations, but we can control behavior through manipulations of environmental circumstances; (b) even if we do know how to control behavior through neural or hormonal interventions/manipulations, their implementation may not always be practical; and (c) a behavioral technology is propadeutic to discovering the functional relevance of information about the current neural or hormonal state of the behaving organism, where that state has been produced through interactions with the environment or through neural or hormonal interventions/manipulations that have modified the organism, such that it responds differently than otherwise to the stimulating action of its environment. The possibilities of such an effective interchange between the life sciences is just beginning to be realized. However, the interchange can only be fully realized when the mentalism that supports looking for inner, self-sufficient causes in another dimension, whether those causes are conceived as cognitive/mental or physiological, is superseded.
References


