The brain does not control behavior: the environment controls the brain

- An interview with John W. Donahoe. conducted by Børge Holden
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A central feature of folk psychology and most of academic psychology is the belief in inner causation. Historically, different body organs have been used in inner explanations of behavior. We are "histrionic", we speak "from the liver", we can be "good-hearted" and so on. Today, the brain is usually conceptualized as the organ that controls behavior, at least in popular speech. What is your comment on the purported "control" position of the brain?

I do not regard the brain as controlling behavior in the sense that it is the *origin* of control. The origin of control is always to be found in the environment. Experience changes the brain or, more particularly, the strengths of connections—or synaptic efficacies—between neurons in the brain. The brain is the physical medium through which the environment affects behavior. It is the locus of change, not its cause. The role of the brain, and the changes produced therein by the action of the individual environment, are analogous to the role of the genes, and the changes produced therein by the ancestral environment. That is, genes are the physical medium that implements the changes produced by the selecting effect of the ancestral environment and synaptic efficacies are the physical medium that implements the changes produced by the selecting effect of the individual environment. But, synaptic efficacies are not the ultimate causes of behavior any more than are genes. The causes of changes in gene frequencies and synaptic efficacies are both traceable to the selecting effect of the environment.

Radical behaviorism rejects the concept of what Skinner called "autonomous man." By that, he meant self-directed man, man that acted independently of the effects of past environments. To ascribe the ultimate causes of behavior to the brain is the modem version of the autonomous-man doctrine in which behavior changes through acts of will, intelligence, wisdom, and the other inhabitants of the mentalistic pantheon. The behavior that we attribute to will, intelligence, and the like is the joint result of selection by the individual and ancestral environments. That is my overall view of the true cause of human behavior.

Is the notion of the brain as a controlling organ also central in neurobiology?

As you have posed it, this is a statistical question: If you poll 100 neuroscientists, how might they respond? I suppose that most neuroscientists believe that the causes of the brain events that they observe do not extend beyond what they study, namely, neural tissue. Nevertheless, when pressed, they would undoubtedly concede that the selecting effect of the environment is the cause of the structures they observe and of their functions. Darwin's principle of natural selection pervades all of biology and informs research at all levels of analysis. Thus, the ultimate source of control for the neuroscientist is the environment as it is for the behaviorist. However, I believe that many *cognitive* neuroscientists, to the extent that they are influenced by mainstream cognitive psychology, view the brain as earlier philosophy regarded the homunculus. That is, such cognitive neuroscientists act as if inner "representations" of the environment were the true causes of behavior.

Let me illustrate. When David Palmer and I had completed several chapters of our book "Learning and complex behavior," we sent them to several publishers who then sent it to reviewers. One of the reviewers, a cognitive psychologist, said that our approach made no allowance for what he called a "real-time executive," which is simply computer jargon for the homunculus. The reviewer was unwilling even to consider the possibility that the brain functioned without a central controlling agent. The possibility that complex behavior emerged from dynamic interactions among large numbers of neurons was unacceptable in principle.

Please understand, the issue is not whether people behave in ways that encourage the use of such terms

as will, intention, purpose and the like. Clearly, we do. The issue is whether these mentalist notions—to the extent that they refer to brain events at all—are usefully considered in our efforts to understand behavior. In the radical behaviorist approach, both behavior and the neural processes that intervene between the environment and behavior are seen as *effects* of selection, not causes. To accord a privileged status to mental processes, or neural processes for that matter, is to confuse cause with effect.

Still, one frequently hears that the effects of a teaching or training program are caused by changes in the brain. Does that make sense?

All learning is a reflection of events that occur in the nervous system and other systems of the body, e.g., endocrine systems. To believe otherwise is to accept dualism and reject physicalism. But, changes in synaptic efficacies do not cause behavior; they mediate the causal effects of the environment, as do changes in gene frequency. A behavior analyst studies the functional relations between the selecting environment and behavior. The neuroscientist studies the biological processes that implement those functional relations. Skinner was very clear that these are complementary enterprises, not competing views of the same phenomena. Skinner regarded radical behaviorism as a branch of biology. He believed that if one were interested in what transpired between the stimulating environment and the behavioral changes produced by that environment, one must carry out what he called “brain science.” Skinner was very interested in the possibility of "filling the gap" (his words) between the stimulating environment and the behavior of the organism. He did not believe, however, that this would substitute for an analysis of the relationships between the environment and behavior. Rather, it would make the account more complete. Changes in the nervous system are a part of the causal chain that extends from behavior back to the selecting environment.

A radical behaviorist has said something like "nothing we know about the brain separately tells us anything about behavior." I assume this means that we have to know what the person is doing concurrently to have something to correlate brain activity with. Does this mean that brain research will be informed by different psychological theories, and can the strong position of cognitive psychology be traced within brain research?

Your question raises several general issues. First, there are the differing views of the brain held within cognitive psychology and behavior analysis. I want to reemphasize that many people within cognitive psychology treat the brain as if it were a scientifically respectable surrogate for the homunculus. Many cognitive neuroscientists have set for themselves the task of finding, in the brain, events that are correlated with the mental processes that they have inferred from behavior. Behavior analysts regard this as a fool’s errand. Clearly, every aspect of human behavior has some event in the brain with which it is correlated. How else could it be if we are not to become mystics? Thus, the question is not whether such correlates exist—they must—but what is their function. The difficulty of the cognitivist program of inferring underlying brain processes from behavioral observations is that a given complex behavior may, in general, be the product of many different combinations of intervening biological processes. The relation of complex behavior to underlying neural process is a one-to-many relation. Consequently, the likelihood is vanishingly small that speculations about underlying brain processes based solely on behavioral observations are correct. Sometimes, we make a lucky guess about underlying processes from observations of behavior. Indeed, the beginning stages of investigation are often guided by such guesses. However, to regard these inferences as sufficient to specify the biological process is to make too much a virtue of necessity. Unfortunately, the occasional lucky guess is sufficient to maintain such behavior. The cognitive approach is unwitting testimony to the power of superstitious reinforcement magnified by the greater availability to memory of the few lucky guesses.

Radical behaviorists believe that filling the gap between the environment and behavior requires an experimental analysis of neuroscience. In other words, the radical behaviorists adopt what you might call a bottom-up strategy: First, one identifies the basic processes through experimental analysis—whether at the neural or behavioral scale of measurement—and, then, one examines the implications of those fundamental processes for more complex phenomena. This strategy exemplifies the normal mode of explanation in science, which is reductionist: Higher level phenomena are understood as the concerted

expression of lower level processes. The cognitive approach, in which unobserved (even unobserv*able*) processes are proposed to "explain" complex behavior, seems an especially American enterprise. My knowledge of European work, most notably in Great Britain, indicates that it is more apt to draw upon information from the real central nervous system instead of what Skinner called the "Conceptual Nervous System." I am thinking particularly of the important contributions to understanding complex behavior of neuropsychology—the effects of brain damage on human behavior.

To what extent is neuroscience necessary to understand and explain behavior?

You are asking whether one needs to know about neuroscience in order to provide a satisfactory interpretation of behavior. I would say that in probably 95% of the cases, the answer is "no." For most practical purposes, knowing about the underlying physiology is of no particular benefit. For example, self-injurious behavior such as head-banging in autistic children can often be eliminated without knowledge of the biological processes that implement the dysfunctional behavior or of the environmental history that selected it. However, knowledge of the accompanying biological processes may be useful at the margins as when the behavior does not yield to behavioral treatment rapidly enough for prevent permanent bodily damage. The foregoing is a pragmatic answer to your question. There are intellectual/philosophical considerations as well. These arise from a belief in the continuity of all sciences and this belief motivates the search for the neural processes that implement behavior, the cellular process that implement neural processes, the biochemical processes that implement cellular processes, and so on.

In most cases, we need not go beyond acknowledging the truism that biological processes underlie all behavior. Because almost any behavior of interest involves the concerted action of tens of thousands of neurons, we will probably never be able to subject that activity to experimental analysis at the time that the behavior is occurring. (By experimental analysis is meant that essentially all of the antecedents and consequences that enter into orderly functional relations are identified and measured under conditions in which the phenomenon occurs.) The basic neural, cellular, and biochemical processes of which the complex behavior is a product may themselves be experimentally analyzed, but the complex behavior that is their cumulative product will likely forever remain the subject of *interpretation.* In interpretation, principles based on experimental analysis are used to understand behavior that occurs under conditions in which experimental analysis is precluded.

On the fringes, there will always be questions about complex behavior that cannot be adequately answered at the behavioral scale of measurement alone. This situation is not peculiar to behavioral science. It is a characteristic of all sciences. One eventually confronts phenomena that cannot be understood at any given scale of the measurement. What science does in such cases is to drop down to a lower scale of measurement to determine whether an adequate account can be provided when these supplementary processes are considered. The central point is that behaviorists are quite open to considering events that are occur in the nervous system whether to deal with practical problems that are refractory to behavioral treatment or to deal with complex phenomena that are incompletely interpreted by behavioral principles alone.

Are cognitive metaphors like "information processing", "retrieval", "memory traces", not to mention "storing", taken literally within neuroscience. If so, to what effects?

For much of cognitive psychology, and for that portion of cognitive neuroscience that is influenced by cognitive psychology, these concepts have great currency. For the behavior analyst, these concepts are fundamentally misleading. They are misleading for two reasons: First, they subtly endorse the homunculus approach to human behavior. That is, they encourage the notion that some "central executive" is directing the "processing" "storing," and "retrieving." Second, and more fundamentally, they imply that experience produces a static entity that can be stored or retrieved. This view of the effects of experience is contrary to experimental analyses of both behavior and neuroscience. On the behavioral scale of measurement, reinforcers change the environmental guidance of behavior; i.e., an environment-behavior relation, not behavior alone. Thus, if anything is "stored," it cannot be a response. Furthermore, because the contingencies of reinforcement typically permit a range of stimuli to be present when any of a

range of responses is reinforced, the environment-behavior relations that are selected are variable in their constituents. Skinner referred to this as the "generic nature" of stimulus and response. Experimental analyses at the neural scale of measurement are consistent with the statistical conception of the products of selection that arises from behavioral research. The neuromodulators (e.g., dopamine) that implement reinforcement directly or indirectly modify the synaptic efficacies of many neurons throughout large regions of the brain. Thus, even a relatively simple environment-behavior relation is implemented by the concerted action of a variable subset of a large population of affectors, neurons, and effectors. The variable behavioral and neural outcome of selection by reinforcement is not congenial to concepts such as storage, retrieval, and the like—which imply a static (even Platonic) conception of the effects of experience.

Perhaps the clearest indication of the difficulties engendered by the static conception occurs in the interpretation of verbal behavior (i.e., language). Consider one type of verbal operant, a *tact*. A tact is a verbal response that is guided by a nonverbal stimulus. Nontechnically, naming an object is a common example of a tact. If I say, "this is my hand," "this is my nose," and so on, these exemplify tacts. For normative cognitive psychology, words such as "hand" and "nose" are thought of as stored in a lexicon from which they may be retrieved. But, when brain damage occurs, the effects of damage are not consistent with this view: A person who cannot emit the word "hand" when sensing the visual stimuli occasioned by a hand may nevertheless say "hand" when touching one or may repeat the word when it is uttered by another. For those committed to the storage metaphor, such findings are problematic. Was the word hand" stored or not? And, if it was stored, why was it retrieved under some circumstances and not others? For the behaviorist, these results are precisely what one would anticipate. The verbal response "hand" is a constituent of many verbal operants. For some operants, the environment-behavior relation includes visual stimuli, for others tactile stimuli, and for still others auditory stimuli. David Palmer has described a number of such examples in "Learning and complex behavior." Evidence from aphasia and from brain damage is much more readily dealt with from a behavioral perspective than a cognitive perspective.

So we do not "store" memories, because they are not things... But what happens when we remember, according to a radical behaviorist account?

When experience changes us, it changes the strengths of connections between neurons in the brain and the environment-behavior relations that are implemented by those neurons. Typically, a behavior of any complexity involves tens of thousands, even millions of neurons as already noted. Those neurons are not dedicated to a particular environment-behavior relation. Some of them participate in many, many different relations. This means that you cannot talk about a memory as a thing or as having a particular location. Instead, what happens when a "memory" occurs is that the environment *of the moment* activates a pattern of neural activity that implements an environment-behavior relation that includes a behavior that is now scheduled for reinforcement. What is nontechnically called memory is a dynamic, time-varying, transient pattern of neural activity that was selected in a previous environment but that is reinitiated by the present environment.

One of the primary insights of radical behaviorism is that one must explain behavior based on conditions that exist at that moment. From this point of view, when you are not remembering, the memory does not exist. A memory is neither stored nor retrieved; it is generated by the action of the present environment on the products of prior selections. What endure are altered strengths of connections between neurons of the brain. If particular patterns of neural activity are initiated by present stimuli, then certain behaviors will occur. There is nothing that corresponds to the unitary thing "memory" that is stored anywhere.

Does it therefore make little sense to talk about different types of behavior, types of memories and so on?

The critical distinction that my colleague David Palmer has proposed is between those situations in which the current environment contains stimuli that were present when the environment-behavior relation

was originally selected versus those in which such stimuli are absent. In the first situation, recall requires only the behavioral processes involved in all cases of stimulus control, and is referred to as *reminding*. As an example, suppose I were to ask you "What is your mother's name?" That question has almost certainly been asked in the past and you have answered it and presumably been reinforced for doing so. This may be interpreted as straightforward instance of discriminative control because the stimuli that are present at that moment (those provided by the question) are largely stimuli that were present in the past when the response was reinforced.

The more interesting examples of memory occur when (a) some behavior is called for (i.e. scheduled for reinforcement), (b) the behavior is one that can be emitted by the learner in some environment (i.e., the response is in the learner's behavioral repertoire), but (c) the present environment does not contain stimuli sufficient for the emission of the response (i.e., effective discriminative stimuli are missing). In this situation, which we call *remembering*, recall requires additional behavior,. To remember, the person must behave so as to produce stimuli that are discriminative stimuli for the response (the "memory") that is scheduled for reinforcement. We refer to behavior whose function is to produce such discriminative stimuli as *mnemonic_behavior*. The stimuli that mnemonic behavior produces may be either overt or covert. Suppose one were trying to recall the name of a person whom one had just met at a party. As an example of overt mnemonic behavior, one might glance about the room to see the face of the person and, thereby, reinstate more of the stimuli that were present when the name was originally heard. Alternatively, as an example of covert mnemonic behavior, one might silently go through the letters of the alphabet in an effort to find a letter that would be discriminative for the name of the person; e.g., the first letter of the name. Remembering is reminding oneself. Notice that in both reminding and remembering, the behavior of the moment is always guided by stimuli of the moment. Memory is current behavior and is an acquired skill. Memory is not an innate capacity; we must learn to remember.

If the brain does not control behavior, what does it, then? What is a brief description of the function of the brain?

I think Skinner had it about right. The events that occur in the brain fill the gap between the environment and the behavior. A complete biobehavioral science would include not only the functional relations discovered through behavioral work, but also the neural processes that intervene between the environment and behavior. For many purposes, one could provide a quite adequate account of behavior based on environment-behavior relations alone. But, speaking for myself, I find it intellectually more satisfying to try to complete the causal chain, beginning with the environment, continuing with events inside the organism, and concluding with behavior. Consideration of the intervening neural events is seldom necessary for a satisfactory scientific interpretation of even complex behavior, but I find a comprehensive account more compelling.

You have said that the acceptance of radical behaviorism might be enhanced if the neurobiological bases of behavioral principles, e.g. reinforcement, are accounted for. What do you mean by that?

My reasons for thinking that an integration of radical behaviorism with neuroscience would be useful are twofold: As already noted, there are occasional situations in which it is difficult to interpret a phenomenon without appealing to brain processes. The second, and more general, reason that I favor an integration of behavior analysis and neurosciences is that I suspect that there may be a historical parallel between behavior analysis and evolutionary biology. When Darwin proposed the principle of natural selection, it was purely a functional principle. He had no knowledge of the biological mechanisms that implemented the principle. Moreover, until the mechanisms became known, even most biologists did not accept natural selection as the major insight into evolution. Two developments were critical in the acceptance of Darwin's principle of natural selection by the scientific community. One was the identification of a biological substrate that could implement Darwin's principle. The required mechanism became available in the early 1900s with the rediscovery of Mendel's work on heredity. The second development was the quantitative technique of population genetics, which allowed the cumulative effects of natural selection on gene frequency to be traced in a rigorous fashion.

Behavioral research has provided us with a deep functional understanding of how the environment selects behavior through the three-term contingency of environment, behavior, and reinforcer. However, in order for reinforcement to be seen as the fundamental principle of ontogeny, it may well be necessary to identify the biological mechanisms that implement reinforcement and quantitative techniques to trace its cumulative effects. Neuroscience can provide us with knowledge of the biological mechanisms. Artificial neural networks may be able to provide us with techniques to trace the effects of reinforcement, and this is an area of research that we are actively pursuing in addition to our experimental work. Some initial efforts are described in a 1997 volume, "Neural-network models of cognition: Biobehavioral foundations," that I co-edited with a colleague, Vivian Dorsel.

Has radical behaviorism had any influence on the understanding of the brain? What may radical behaviorism contribute to the study of the brain?

Neuropsychology is a burgeoning field of scientific investigation and application. Behavioral psychologists, because they appreciate that experience changes environment-behavior relations, have been able to devise techniques that are very useful for helping people that have suffered a brain damage. I believe that the success of these procedures will encourage neuroscientists to consider the contributions that behavior analysis can make to understanding neural function. Therefore, I am guardedly optimistic. The phenomenon of blindsight is a case in point. As you probably know, in blindsight a particular kind of brain damage prevents the person from talking about the visual environment. However, other behavior—nonverbal behavior—may still be guided by the visual environment. As an example, a person with this kind of brain damage can walk across an unfamiliar room without bumping into the furniture but cannot describe the room. His verbal behavior is that of a blind person

This phenomenon is problematic from a cognitive perspective, but it is exactly what one could expect from a behavioral perspective: Because reinforcers select environment-behavior relations, walking can be guided by visual stimuli but those same stimuli may not guide speaking. In an intact brain, the stimuli that guide verbal behavior also usually guide other behavior. Indeed, the correspondence between verbal behavior and other behavior is often misinterpreted as causation in mentalistic psychology. In this mistaken view, the environment evokes verbal behavior and stimuli produced by the verbal behavior evoke other behavior. Speaking nontechnically, the environment evokes an idea and the idea prompts action. The behavior-analytic view is that verbal behavior and other behavior are constituents of different discriminated operants, both of which are under the control of the environment. The interdependence—if any—between these different operants varies from one situation to the next.

As we close this interview, I wish to make a final comment: Throughout the discussion, I have emphasized that all human behavior can ultimately be traced to the environment—the ancestral environment as understood chiefly through natural selection and the individual environment as understood chiefly through selection by reinforcement. Because of the primary role assigned the environment, one might conclude that behavior analysis regards man as a passive being who is at the mercy of his environment. This would be a mistake, or at least a great oversimplification. If the ultimate causes of human behavior are indeed located in the environment, then—paradoxically—only by accepting this proposition can we effectively design environments to select behavior that permits us to realize our goals as individuals and as a species.