The Behavior Analyst 1996, 19, 61–70 No. 1 (Spring)

How Is Physiology Relevant to Behavior Analysis?

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Physiology is an important biological science; but behavior analysis is not a biological science, and behavior analysts can safely ignore biological processes. However, ignoring *products* of biological processes might be a serious mistake. The important products include behavior, instinctive drift, behavior potentials, hunger, and many developmental milestones and events. Physiology deals with the sources of such products; behavior analysis can deal with how the products affect behavior, which can be understood without understanding their sources.

Key words: behavior analysis, developmental milestones, developmental events, physiological processes, physiological products, physiology

Physiology is a biological science, and as Skinner (1974, p. 214) and John B. Watson (1919, pp. 19–21) have said, psychology is not a biological science. The argument in this paper is that behavior analysis is a branch of psychology, not a branch of physiology, and therefore behavior analysts can safely ignore physiological processes.

The argument is not that behavior analysts *must* ignore physiological processes or *should* ignore them, but that behavior analysts do not *need* to consider physiological processes. Also, the argument refers to behavior analysis, not to behavioral pharmacology, psychopharmacology, psychobiology, psychophysiology, physiological psychology, or any other kind of experimental analysis of physiological processes. Researchers in the latter areas could no more ignore physiological processes than behavior analysts could ignore behavior. Further, the argument is not that behavior analysis is irrelevant to physiological research; in fact, several recent reviews have indicated that much neurophysiological research deals with effects of behavior on neural functioning rather than effects of neural functioning on behavior (Maunsell, 1995; Singer, 1995; Ungerleider, 1995). Finally, the argument is not that psychological processes are independent of physiological processes, because that would require the existence of disembodied behavior analogous to a nonsubstantial mind and would require a behavior-body parallelism analogous to mind-body parallelism. The argument, rather, is that the experimental analyses of behavior will not suffer if behavior analysts ignore physiological processes.

Nevertheless, even though behavior analysts can safely ignore physiological processes, ignoring all *outcomes* or *products* of physiological processes would probably be a serious mistake. The products of physiological processes include such behavioral phenomena as *instinctive drift* (Breland & Breland, 1961), *observer drift* (Sulzer-Azaroff & Mayer, 1991, p. 84), hunger, and behavior itself, including not only behavior that is characterized as instinctive, unlearned, or unconditioned, which is also called "species-specific behavior" (Catania, 1992, pp. 97, 396) and "behavior potential" (Kuo, 1976, p. 125), but also all other kinds of behavior. The products also include some developmental milestones—phenomena that mark changes in stages of development—and some life events, which are happenings during the life course that have important consequences (Reese &

A shorter version of this paper was presented in P. Andronis (Chair), *A debate on behavior analysis, biology and evolution,* symposium conducted at the meeting of the Association for Behavior Analysis, Washington, D.C., May 1995.

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Smyer, 1983). Relevant developmental milestones include birth, puberty, and the menopause, which mark the beginning of infancy, adolescence, and advanced age, respectively. Relevant life events include teething, the emergence of mature secondary sex characteristics, and facial wrinkling in old age.

Some of these products seem to function as discriminative stimuli, and some others seem to function as *setting events, setting factors, establishing operations,* or *developmental cusps* (for definition of the respective terms, see Bijou & Baer, 1978, pp. 26–28; Kantor, 1970, p. 106; Michael, 1982; Rosales-Ruiz & Baer, in press). For example, secondary sex characteristics and visible signs of aging may function as discriminative stimuli for approach and nonapproach types of social behavior, respectively, and developmental milestones and teething fit the definition of setting events, setting factors, and so on.

Physiology deals with the sources of such products; behavior analysis can and sometimes does deal with the effects of some of the products, and the effects of the products can be understood without understanding the sources of the products. The contrary view—the view that such effects cannot be understood without understanding their sources—is a form of reductionism, which is discussed in the next section.

REDUCTIONISM

The belief that psychological and behavioral phenomena should be explained in terms of physiological processes is entailed, or at least strongly implied, by the view that psychology and behavior analysis are branches of physiology. This belief also appears in other views, but in all cases it implies acceptance of reductionism. The value of this kind of reductionism is discussed in the present section, following brief discussion of the meaning of *explanation* in behavior analysis.

The Meaning of Explanation in Behavior Analysis

In behavior analysis, explanation is the same as description (e.g., Bijou, 1979; Day, 1969, 1976/1992; Delprato, 1986; Wood, 1978), or more precisely, explaining a phenomenon means describing a functional analysis of the phenomenon (Day, 1969, 1976/1992; Skinner, 1950). A further stipulation is that the concepts in an explanation must be at the same level as the phenomenon to be explained and must be in the same domain as this phenomenon (e.g., Morris, Higgins, & Bickel, 1982a; Skinner, 1938, p. 441, 1969, pp. 237–238). This stipulation reflects Skinner's (1950) argument that an explanation is not useful if it "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), and C. Ferster and Skinner's (1957) comment that their theoretical analysis of schedules of reinforcement was "not theoretical in the sense of speculating about corresponding events in some other universe of discourse" (p. 2). Skinner (1950) gave physiological explanations of behavior as an example.

Two issues about this stipulation are its practicability and its force, or basis. Its practicability hinges on determining the level and domain of concepts and observations. The determination that concepts are at the same level is often very difficult (Nesselroade & McArdle, in press), but as Morris et al. (1982a) pointed out, observations of behavior are clearly not at the same level as intervening variables (as defined by MacCorquodale & Meehl, 1948) and therefore explanations of behavior that include intervening variables are unlikely to be useful. The determination that concepts refer to different domains, such as the behavioral and mental domains or the behavioral and physiological domains, should be easier than the determination of levels; in any case, the stipulation implies that explanations of behavior that include "mental fictions" (Skinner, 1974, p. 18) or physiological processes (Skinner, 1950) are unlikely to be useful.

The force of the stipulation hinges on the usefulness of explanations that are consistent with it. The stipulation is not a categorical principle or presupposition underlying behavior analysis; it is a pragmatic rule adopted because it works, that is, it furthers the goal of behavior analysis. The goal of behavior analysis is to explain behavior (Skinner, 1974, p. 9; he said understand and explain, but these terms seem to be synonyms), and the criteria for demonstrating that an explanation is adequate are successful prediction and control (Skinner, 1974, chap. 1). In other words, truth as defined in behavior analysis is demonstrated not by agreement but by successful working (e.g., Day, 1976, 1977, 1983; Hayes, Hayes, & Reese, 1988; Lamal, 1983; Mapel, 1977; Skinner, 1945, 1974, pp. 16, 31, 235; Zuriff, 1980).

Therefore, the stipulation can be violated if violating it is useful, that is, if it improves the prediction and control of behavior. One violation that might work is referring to physiological processes in explanations, which constitutes a kind of reductionism because it refers to processes in a different domain from the observations. The current value of this kind of reduction, and even further reduction, is discussed in the next section.

Reduction to Physiology and Beyond

Several views of reductionism are described in the present section. The views are not behavior analytic because, as indicated in the section on explanation, behavior analysts typically reject reduction to a different explanatory domain. Nevertheless, the views illustrate issues that behavior analysts might encounter if they accept reductionism.

Reduction to physiology. Teyler (1975), a psychobiologist, defined a reductionist as "a person who seeks to explain a phenomenon by reducing it to the parts of which it is constituted" (p. 139). Bugelski, who was an old-line stimulus–response learning theorist, fit Teyler's definition of a reductionist, and he believed that the parts are physiological. His view was that psychology is "inadequate, incomplete, [and] a technology or art instead of a science" unless the physiological processes that underlie stimulus–response relations are identified (Bugelski, 1973, p. 53). He believed that if underlying physiological processes are identified at least hypothetically, psychological laws can permit prediction and control of behavior, but the understanding of behavior requires knowing the actual underlying physiological processes. Consistent with the first point, Bugelski (1982) attempted to explain learning and imagery phenomena on the basis of "neural action currents," but he said that the particular neural units involved did not need to be specified to make the explanation useful.

Reduction beyond physiology. Teyler (1975) wanted reductions that are more extreme than the kind Bugelski (1973) wanted. Teyler wanted reduction not only of a behavioral whole to physiological processes, but also of physiological processes to chemical processes, and of chemical processes to submolecular structures (pp. 1–2). Jacques Loeb (1912/1964) also said that psychological phenomena are reducible in principle to chemical and physical processes (pp. 61–63); but if one is willing to buy into reductionism at all, his and Teyler's position seems to stop too soon. Why should a reductionist stop at any level above quantum mechanics (if that is the irreducible level)? The answer may be that even if such a reduction is possible in principle, it has not occurred in practice. Stop at physiology because it has given psychology some useful information and no attempt at further reduction has been useful. However, this answer can be challenged because physiology seems not yet to have provided any useful information about the physiological *pro-*

cesses that underlie psychological phenomena.

Pseudoreduction. Bugelski's (1973, 1982) position on reduction to hypothetical physiology has a long history. For example, in the 18th and 19th centuries, some French and German materialists interpreted consciousness and thought as secretions of the brain, thus relating psychology to hypothetical (or imaginary) physiologies (Kantor, 1969, pp. 203–204, 258–259). More recent examples are Hull's (1943) use of terms such as *afferent neural interaction,* which sound physiological but are not; Hebb's (1949) invention of a *conceptual nervous system,* which has no referents in the actual nervous system; *adaptive-network* models, which are simulations of physiological processes with units that are "loosely analogous to neurons" (Palmer & Donahoe, 1992, p. 1355); and other versions of modern connectionism, which are no more physiological than Thorndike's "connectionism," as he labeled his version of stimulus–response associationism (Hilgard, 1987, p. 190). Another example is that one of the criteria proposed by cognitive theorists to evaluate the adequacy of any information-processing model is consistency of the model with what is known about neural physiology (Klahr & Wallace, 1976, p. 5; Palmer & Donahoe, 1992; Simon, 1972), but even with this consistency, the model is not physiological.

Because the theories and models mentioned in the preceding paragraph are not physiological, they cannot provide physiological explanations of behavior, although of course they can be scientifically valuable for other purposes that are not considered in this paper. Another kind of approach, which some cognitivists use but which seems to have little if any scientific value, is to substitute "brain" for "mind." Skinner (1974) criticized this substitution because it is an attempt to avoid mind-body dualism rather than an attempt to give a physiological explanation (pp. 77, 117, 213). Some cognitivists also use another metaphor—"in the head"—as a vaguely euphemistic way to say "in the mind," but although Ryle (1949) argued that this phrase is less misleading than and therefore preferable to "in the mind" (p. 40), the cognitivists seem to use it not to avoid mentalism but to make mentalism more palatable (e.g., Brown, 1975; Jenkins, 1971). The phrase has also, however, been used merely for stylistic variation (e.g., Craik, 1943, p. 51; Dewey, 1933, p. 111).

A pragmatic view of reduction. Craik (1943), who was a British mechanist, used the following analogy to describe reductionism:

> [The] plea for physical explanation does not mean that it is useless or incorrect to give apparently non-physical clinical explanations of psychological phenomena—for instance, to say that an unpleasant experience or shock may *cause* amnesia or suppression. It is correct to say that the pressure of one's finger on the self-starter causes the engine to go, but more fundamental to say that the pressure of one's finger causes current to flow in the windings of the starting motor and still more fundamental to give an account of the flow of current and torque exerted by the motor in terms of electronic and electromagnetic theory. (p. 49)

That is, explanations can be given at different levels, and some explanations are "more fundamental" than others, but the explanation at each level may be correct as far as it goes. A further point, not mentioned by Craik, is that even though the number of levels is presumably finite, the success of prediction and control will probably reach an acceptable point at a level far from any conceivable ultimate level. No science is compelled to go beyond the level at which success is acceptable, given that the meaning of success and the criteria of acceptability depend on the goals of that science.

Is reduction to physiology useful? Kantor (1947) devoted an entire chapter (chap. 8) to criticizing "the dogma of the nervous system"—the dogma that "the nervous system is the seat of mentality or at least furnishes explanatory principles for psychology" (p. 92)—and he was especially critical of hypothetical neurology, which he characterized as "imaginary," animistic, and mentalistic (1922, 1923, 1977, p. 148; see also Delprato, 1979). But what about real neurology? Weizenbaum (1976) said,

> Our ignorance of brain function is currently so very nearly total that we could not even begin to frame appropriate research strategies. We would stand before the open brain, fancy instruments in hand, roughly as an unschooled laborer might stand before the exposed wiring of a computer: awed perhaps, but surely helpless. (p. 136)

Weizenbaum's comment is still accurate, as indicated by the following comment by Catania (1992) about the physiology of learning:

> Even if we could watch a brain do something, how would we know that what it was doing was learning? . . . It would be fascinating to know what physiological changes accompany learning. Yet we might have trouble figuring out what to look for in the nervous system if we cannot even say what learning is. In fact, we cannot have an adequate physiology of learning without an adequate understanding of the behavioral properties of learning. (p. 3)

Skinner was somewhat inconsistent about the relation between psychology and physiology (Parrott, 1983, p. 182), but his usual position was the same as Weizenbaum's and Catania's. Skinner said, "No physiological fact has told us anything about behavior that we did not already know, though we have been told a great deal about the relations between the two fields" (1978, p. 123; p. 199 in the 1980 reprint), and "we are still a long way from knowing what is happening in the brain as behavior is shaped and maintained by contingencies of reinforcement" (1984, p. 949). Thus, his usual position was that reductionism is not useful (e.g., 1974, pp. 240–241), a position congenial to most behavior analysts (e.g., Bijou, 1979; Day, 1969, 1976/1992; Morris, Higgins, & Bickel, 1982b).

A similar point could be made on the basis of a comment by Moore (1981): In behavior analysis a reinforcer is defined as a stimulus that has a certain effect on behavior, and the question of why it has that effect is in the province not of psychology but of physiology or genetics. Conditioned reinforcers are not exceptions. A conditioned reinforcer is a stimulus that prior to a certain history was not a reinforcer and that after this history was a reinforcer, but although it is called a *conditioned* reinforcer because of its history, it is called a *reinforcer* not because of its history but because of its effect on behavior. So far, neither physiology nor genetics has provided a general answer to the question of why stimuli have the reinforcing function.

Conclusion about reductionism. Watson (1919) said,

> It has been claimed by some that behavior psychology is really physiology. That this is not the case appears from even a casual examination of the respective scopes of the two provinces. . . . Physiology has nothing to tell us of the character and personality of different individuals nor of their emotional stability or lack of emotional control, nor as to what extent their present place in life is dependent upon their upbringing. Physiology tells us nothing of man's capacity to form and retain habits, nor of the complexity of man's habit organization. (pp. 19, 20–21)

Skinner (1974) made the same point:

> We know some of the *processes* which affect large blocks of behavior—sensory, motor, motivational, and emotional—but we are still far short of knowing precisely what is happening when, say, a child learns to drink from a cup, to call an object by its name, or to find the right piece of a jigsaw puzzle, as we are still far short of making changes in the nervous system as a result of which a child will do these things. (p. 213, italics added)

The context of Skinner's comment does not clearly indicate whether the processes referred to are behavioral or physiological, but in either case the last clause makes the same point as Watson's comment.

Why cite statements made so long ago? Because they are still correct, despite extraordinary advances in instrumentation such as magnetic resonance imaging (MRI) and positron emission tomography (PET). In fact, the events monitored in MRI and PET are not neuronal activities but products or correlates of these activities, and the relation between the monitored events and the underlying neuronal activity is not well understood (Ungerleider, 1995). Thus, MRI and PET indicate where neuronal activity occurs, but do not indicate what the neural mechanisms are. In fact, the way stimuli (sensory inputs) are "encoded" at the neuronal level has not been definitely established (D. Ferster & Spruston, 1995). An analogy is that research has shown that damage to the language and speech areas of the brain results in verbal disability, but no research has shown that excellence in verbal ability has a basis in excellence in these brain areas (Das, Kirby, & Jarman, 1975).

Advances in Neuropsychology and Neurophysiology

The foregoing argument, that reduction of psychology to physiology is not useful, can be challenged on the basis of advances being made in the fields of neuropsychology and neurophysiology regarding brain–behavior relations. Examples are (a) research showing that neuroelectric brain patterns predict accuracy of responses in a reaction-time task (Gevins et al., 1987), but with much variance still unaccounted for and, as the investigators said, without identifying the origins of the neuroelectric patterns; (b) Luria's extensive research on brain activity (e.g., 1980); (c) research relating general cognitive processes to evoked brain potentials—examples are use of the P300 latency as an index of stimulus processing in the Stroop task (Duncan-Johnson & Kopell, 1981), use of the amplitude of a late potential to study attitudes (Cacioppo, Crites, Berntson, & Coles, 1993), and use of the P300 amplitude to study the effects of expectancies on perception (Begleiter, Porjesz, Yerre, & Kissin, 1973); (d) research relating specific cognitive processes and specific brain loci (Kosslyn, 1988; Posner, 1993; Posner, Petersen, Fox, & Raichle, 1988); (e) research indicating differences in brain structure and functioning of musicians with perfect pitch versus other musicians and nonmusicians (Schlaug, Jäncke, Huang, & Steinmetz, 1995a; but see also comments by Sacks, 1995; Schlaug, Jäncke, Huang, & Steinmetz, 1995b); and (f) research indicating different brain loci of the processes (or results) of deliberate memory formation and automatic habit formation (Petri & Mishkin, 1994). The last example is very important because it provides a neurological basis for synthesizing cognitive and behavioral theories without eclectic mixing of active- and reactive-organism models.

However, this research has demonstrated correlations between certain psychological processes and activity of certain brain structures, sometimes highly localized ones, and even activity of single neurons, but it has not yet provided explanations of psychological processes. Thompson (1986), a neurophysiologist, said in an article promoting research on the neurophysiology of learning and memory, "The success of this . . . approach has been the source of great optimism and *will probably* lead to fundamental insights into the physical basis of memory over the next few years" (p. 941, emphasis added). Thompson characterized as "putative" many of the brain mechanisms he discussed, and many of them are still putative. Thus, the evidence does not undermine a conclusion made by Loeb (1912/1964) that the study of brain anatomy and the localization of a function in a neuron or a group of neurons "may give data concerning the path of nerves in the central nervous system but . . . it teaches little about the dynamics of brain processes" (pp. 35–36).

Another consideration is that Skinner's (1978) comment cited earlier—that this kind of research has not generated any new psychological principles—is still correct. For example, in a brief summary of brain-scan research, Posner (1993) cited evidence that the locus of brain activity shifts with practice on a task from the cortex to lower brain centers, which is consistent with the psychological finding that well-practiced behavior requires little or no conscious effort (e.g., Hasher & Zacks, 1979; LaBerge & Samuels, 1974). He also cited evidence that perception and visual imagery occur in the same brain areas, which is consistent with the psychological finding that perception and visual imagery involve the same mental processes (e.g., Brooks, 1967). Granted, this kind of convergent validation is nice, but it adds no new information about behavior.

When physiological-sounding terms such as *neural action current, afferent neural interaction,* and *conceptual nervous system* do not have physiological referents, they are intervening variables or hypothetical constructs (MacCorquodale & Meehl, 1948), not physiological terms (Reese, 1982, 1993). Also, approaches such as connectionism that do not refer to actual physiological processes are not physiological. These concepts and approaches seem unlikely to be useful for behavior analysis.

PHYSIOLOGICAL PRODUCTS

Despite the preceding argument, many phenomena have been identified as products of physiological processes, and these phenomena can be used in behavior analysis whether or not they are actually products of physiological processes. They can be used because, as already mentioned, they seem to function as discriminative or reinforcing stimuli (e.g., secondary sex characteristics and visible signs of aging) or as setting factors (e.g., puberty and the menopause), and they may have other functions. These functions can be studied without any consideration at all as to the sources of the phenomena themselves, even though a complete explanation, going beyond the stipulation of usefulness, might deal with the sources of functions, that is, explain why the phenomena have the functions they in fact have.

For example, puberty is associated with changes in many secondary sex characteristics, and puberty seems often to serve as a setting factor that influences the discriminative and reinforcing stimulus functions of some of these characteristics. This setting-factor function may reflect "raging hormones," but it may also reflect the history of reinforcement—in some social groups secondary sex characteristics before puberty have the same discriminative and reinforcing stimulus functions as secondary sex characteristics after puberty (e.g., McCandless & Evans, 1973, pp. 244–247).

Most behavior analysts would probably start with the learning-history hypothesis: Puberty as a setting factor could be studied by using the variant of the multiple baseline design in which different persons are substituted for different kinds of behavior; the persons could be selected to represent prior, early, and late stages of puberty. The discriminative and reinforcing stimulus functions of secondary sex characteristics could be studied by using a reversal design in which the conditions are associated with live, videotaped, or pictured models in different stages of maturity, perhaps with nearness or clearness to the research participant varied in accordance with a conjugate reinforcement schedule. If this kind of research does not support the learning-history hypothesis, perhaps the problem should be turned over to a researcher with expertise in physiology, physiological psychology, psychophysiology, psychopharmacology, or the like. Such a researcher might also be a behavior analyst, or might collaborate with a behavior analyst, or might work without a behavior analyst.

CONCLUSION

Stemmer (1987) argued that mentalistic explanations of behavior, including mental behavior, are likely to be more complex but to cover less evidence than neurophysiological explanations. However, his argument required positing some hypothetical neurophysiological phenomena, and unless the hypothesized existence and hypothesized functions of these phenomena are confirmed empirically, behavior analysts might consider his neurophysiological explanations to be no more useful than the mentalistic ones, and therefore might prefer purely behavioral explanations that are consistent with the stipulation about different levels and different domains.

Real physiology has so far not helped behavior analysts to explain the behavioral phenomena they study. Therefore, ignoring physiological processes seems unlikely to be an obstacle to progress in behavior analysis. However, research in other branches of psychology, especially developmental psychology, suggests that some products of physiological processes affect behavior in the same way as discriminative stimuli, setting factors, and other kinds of causal or controlling events that behavior analysts study. Many products of physiological processes cannot be directly manipulated in human research participants, and although they may be indirectly manipulable (as in the hypothetical experiment outlined in the preceding section), the manipulation may often be difficult both conceptually and empirically. However, ignoring physiological products because of the difficulty of determining whether they have behavioral functions would be like losing a wallet in a field and then searching for it under a streetlamp because the light is better there.

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