

SKINNER'S THEORY OF BEHAVIOR
AN EXAMINATION OF B. F. SKINNER'S CONTINGENCIES
OF REINFORCEMENT: A THEORETICAL ANALYSIS¹

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Psychologists are well aware that Skinner denies that a hypothetical construct lurks somewhere in the corpus of his work. It is appropriate, then, to ascertain why he has presented us with a new book entitled *Contingencies of Reinforcement: A Theoretical Analysis*. Because it is well known that this book is a collection of previously published papers, it is also appropriate to determine what new analyses, if any, are given in this work.

Throughout the book, Skinner returns to the lean and muscular style of writing that was characteristic of his earlier works. The presentation is much more compact than *Science and Human Behavior* (Skinner, 1953). This is especially true of the new material, in the form of a preface and notes to the various chapters, which occupy 105 of the 279 actual pages of the text. It represents a significant elaboration and natural extension of his earlier work to some previously untouched domains, a restatement or reformulation of some of his earlier methodological and conceptual contributions, and an effective rebuttal to numerous critics.

In his preface, Skinner explains that he uses the word "theory" at least three different ways: (1) an explanatory system involving hypothetical constructs; (2) a critique of the methods, data, and concepts of a science of behavior; (3) the interpretation of familiar facts, such as our observation of human behavior in daily life, in the light of scientific analysis.

It is, of course, in the first and most famous sense of the word "theory" that Skinner is usu-

ally understood to be speaking when he says that he does not have one. In this new book, Skinner restates his well-known arguments on this subject, but these play a minor role.

Skinner's claim is that his theorizing is entirely in the second and third senses of the word "theory" mentioned, and that this is best illustrated by his analyses of some important areas of psychology, namely perception, rule-governed behavior, and the relation of genes and the environment to behavior.

PERCEPTION

Kantor (1970), in an otherwise perspicacious analysis, chided experimental analysts of behavior, and *ipso facto* Skinner, for not making efforts toward "the investigation of adjustments including perceiving, remembering, thinking, and feeling behavior among other classes as performed by organisms of all genera and species. . . . Once it is determined that certain kinds of behavior occur as, for example, perceiving in learning situations, what is then required is the component analysis and description of the behavior in question, whatever its type or complexity. . . . While it is true that a few TEAB workers have ventured to describe perceptual behavior naturalistically, I would suggest that a sizable experimental attack would result in an extremely valuable change in the description and interpretation of such behavior." The punishment was not quite warranted.

It is true that there is no section of any work by Skinner entitled "Perception"; Chapter 8 of *Contingencies of Reinforcement* is "Behaviorism at Fifty". However, the theory has been in existence for 36 years. It was originally formulated in response to a friendly challenge issued by Professor A. N. Whitehead (Skinner, 1957), and has undergone various reformula-

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tions and refinements since. It was, for example, "hidden" in the William James Lectures in 1947, in *Science and Human Behavior* (Skinner, 1953), and in *Verbal Behavior* (Skinner, 1957).

Our understanding and appreciation of Skinner's theory will be enhanced by our knowledge of the new reasons he gives for its formulation. He previously reminded us that behaviorism is a philosophy of a science, namely, psychology, and the message of behaviorism is that psychology should be about behavior. This seems to be so old hat, to be such an overworked, outdated issue; but we should be reminded of it. Our commitment to behaviorism is so strong that we tend to forget that one can raise legitimate, hard questions about what have been called "raw feels", or "sense data". Many eminent philosophers and psychologists have puzzled long and hard about such matters, and we seem to ignore the fact that the issue is still very much with us, even though so distinguished a philosopher as Herbert Feigl has devoted a whole book to the subject (Feigl, 1967), and psychology's P. E. Meehl tells us that he is still wrestling with the problem (Meehl, 1966). Furthermore, it might be objected that in principle, psychology might have a dual subject matter: behavior and mind.

Behaviorists argue that if one agrees that what is to be explained is behavior, then mind turns out to be neither an explanandum nor a satisfactory explanans (Hempel and Oppenheim, 1948). That is, the behaviorist argues that if behavior is what is to be accounted for, then one does not have to account for minds. Furthermore, behavior may be adequately explained without reference to mental processes or states, or the like, or even to physiological ones, for that matter.

Skinner, of course, has something further to say about all this. He points out that J. B. Watson was premature in his proposal that psychology should be the science of behavior; Watson did not have much evidence for that position at that time. However, it was not, and still is not, a satisfactory answer to rule "images" out of court as private, and hence not the subject matter of a science, as Watson seemed to have done. Even though Freud showed that mental activity and behavior did not *require* consciousness, his was not the whole answer, either.

Skinner continues his argument that it is particularly important that a science of behavior face the problem of privacy and solve it without abandoning the basic position of behaviorism. It will not do to revert to mentalism no matter how well disguised by operationalist platitudes. He again reminds us that "an adequate science of behavior must consider events taking place within the skin of the organism, not as physiological mediators of behavior, [*or as other kinds of events uniquely accessible to the owner, and lacking physical dimensions*] but as part of behavior itself." [Italics mine]. That is, conscious events, which have been the traditional subject matter of that branch of our science known as the psychology of perception, are real, physical, objective behaviors caused by real movements. The behavior may be overt, covert, verbal or nonverbal; it is not restricted to any one of those categories.

Skinner recognizes an important point that is all too often lost sight of in discussions of perceptual questions; what is at issue here is not a thing but a process. The language of perception is one of entities—images, contents, colors, shapes, percepts, ideas, feelings, thoughts, and so on. William James was well aware of all of this, and vehemently opposed to it. Getting rid of the "things perceived" is half the battle—Skinner explains why. If we recognize that perception is a process, then it is easier to recognize it as a kind of behavior. It is true, perception is very different from the other kinds of behavior that we know about; it does not seem very much like moving one's foot as in walking. If Skinner is right, it would be more correct to say that we green, yellow, square, up, loud, bright, and so on, where these are conditioned operants—and all this seems very strange.

Is there a parallel to this elsewhere; if there is, might it not help relieve some of the traditional puzzlement arising from these odd assertions? Perhaps we should look for processes that are phenomenologically very different from their actuality. Let us consider dissolution. When we dissolve a piece of salt in water, all we see is that the salt vanishes. However, a chemist will tell us what is going on is that intermolecular forces vary in certain ways, and consequently created ions move about between water dipoles with various local fluctuations in several different kinds of fields and convection

currents, and the rate and amount of "wobble" of the things go up slightly, and so forth.

All this is familiar to us from introductory college chemistry. Its point is that a process can be very different from what it appears to be in terms of our immediate awareness. Why, then do some people refuse to accept seeing as a kind of behavior? Perhaps because in addition to being unlike other kinds of behavior with which we are familiar, the end product does not resemble the components. When we see a building, we also see the components of the building—the bricks, the windowpanes, the roof tiles, the drain pipes, and so forth. But when we watch salt dissolve we do not see the component process; all we see is the resultant. The distinction can be likened to the difference between J. S. Mill's mental chemistry and his father's mental mechanics. Objectors to Skinner's account of what is called conscious experience are, in fact, taking a position similar to that of the elder Mill: if you cannot see the proposed components, then they are not the components, for the components remain intact and visible in the resultant. Unlike the Mills, Skinner argues that what is going on is not occurring in the mind, or solely in the nervous system, but in behavior. Furthermore, the only way in which we can observe this behavior is by seeing that we are seeing it, which is what is traditionally called being aware of an experience.

Skinner, then, is arguing that we are behaving in certain ways when we see a green square that has a certain extent, brightness, texture, and depth. Part of this behavior is conditioned operant behavior, probably all of it is. It may be objected that surely this cannot be true, that the changes in behavior that this account requires occur much too rapidly compared to those that we know about, and the complexity is much too great! Why is it somehow less mysterious or more plausible to claim that complex processes occur rapidly in the nervous system or the mind? It is true that amazingly complex and rapid activity must occur when a person or a pigeon scans his visual field. We are conditioned to see hues, edges and other aspects of contour, the cues for "apparent depth of field" as well as depth itself, motion, direction, size, and most importantly, ordinary objects as ordinary objects. It is now quite clear that no one sees sense data (colored patches) and constructs tables and chairs from

them. Skinner makes a forceful point that sense data are not "way stations" that are not normally recognized; they simply are not there. We see objects and their positions and motions directly, because we have been conditioned to do so.

Another possible objection to Skinner's account is that things appear more vivid when the seen thing is there, as opposed to when the seen object is absent. It may be further argued that Skinner's account treats seeing absent and present objects in the same fashion, and is thereby defective. These arguments may be rebutted by pointing out that when we see things that are not there we have fewer controlling antecedents contributing to the probability of that behavior. In another note, Skinner explains why the "copy" theory of perception is inadequate. That account is a result of a fundamental assumption that in perception, a copy of the world is made within the organism. It is devoted to determining the locus of the copy and the method of its formation. It asks whether in imagining a green triangle I reinstate the color on my retina, in a ganglion cell, in some intermediate area in the brain, or in an occipital area. However, these are the wrong questions. What I reinstate, says Skinner, are some of the discriminative stimuli or other antecedents to which I respond in seeing green. By doing so, I reinstate my seeing—the response green.

The major behavioral objection to the account that Skinner presents lies in the claim that the behavior that he wants to carry the burden is too fast and precise, and hence too unlike other behavior. But, is it really so unlike other behavior that we know about? We seem to be fixated in these objections on behaviors such as finger flexions or walking. Speaking, on the other hand, is unlike any of these. It is very fast and extremely precise, and it looks very different. Perceptual behavior is an unusual kind of behavior, to be sure, and Skinner is one of the first to point out that we know very little about it. He admits that his account is not complete. However, there is considerable evidence, which he mentions, that indicates that what goes on in perception is much like what goes on in other forms of conditioned behavior. The similarity with many processes involved in verbal behavior is great. For example, Skinner argues that awareness is an operant behavior that is conditioned by

the reinforcing community. The community teaches an individual to report what he is doing and the cause of it. Those who know *Verbal Behavior* will recognize this as a kind of autoclitic behavior. Awareness, then, would not appear without a reinforcing community; an individual born outside one would be unconscious—shades of William James!

Seeing things that are not there is really a minor problem for Skinner, and secondary to his major arguments. However, it does illuminate and support the notion that seeing is a kind of operant behavior. By accepting Skinner's thesis, we can easily explain seeing things that are not there by the same process of supplementary strengthening that in the verbal case produces metonymy and naming things that are not there. Seeing absent objects is not an unusual occurrence. Everyday life and even the history of science has many examples of this. D. J. de Solla Price, among others, tells the story of how French physicists in a situation of high activity and intense personal and national rivalry in the years immediately after 1900 were led to publish nearly 100 papers on N-rays. These mysterious rays were discovered by a reputable physicist at the University of Nancy, and apparently produced changes in the brightness of a dimly illuminated piece of paper or a phosphor. Oddly enough, although the effect was repeatedly obtained in French laboratories, it could not be reproduced in England, Germany, or America. In his explanation of the phenomenon, R. W. Wood "showed it reasonable to attribute all the subjective effects to wishful thinking and to the overpowering difficulty of estimating by eye the brightness of faint objects (de Solla Price, 1961)." There were a great many supplementary variables that strengthened just this behavior.

RULE-GOVERNED BEHAVIOR

In Chapter 6 of *Contingencies of Reinforcement*, Skinner discusses problem solving and shows that it is that behavior which is primarily effective, and hence reinforced, by producing the controlling circumstances, principally S^Ps, for other, *rule-governed* behavior. One of Skinner's examples is traversing the Hampton Court Maze on the basis of footprints in the snow. One just goes down the alleys that have one set of footprints; the others are culs-de-sac.

This example is essentially one of making and using maps, and it seems to contain all the important questions and answers required for an adequate analysis of problem solving and "rule-governed behavior".

For Skinner, a rule is a set of discriminative stimuli. As such it is quite physical, but not all real discriminative stimuli are rules. Only stimuli that are "constructed" are, e.g., trails, maps, signs, directions—whether written or spoken. More formally, a rule is an especially created collection of discriminative stimuli that describes a contingency of reinforcement. The description may be elliptical, but nonetheless is effective due to the conventions of a verbal community. Rules, then, do not exist outside a verbal community.

A philosophically knowledgeable person may object, at this point, that this sounds all too like Bishop Berkeley. Surely rules exist irrespective of anyone's knowledge or statement of them and whether or not they are followed. The rules of Babylonian grammar exist even though no one today speaks Babylonian; the rules of chess would exist even if there were no chess players. If one did not follow these rules the game would not be chess; likewise, if one did not speak according to the rules of Babylonian grammar, the output would not be Babylonian. Thus, somehow, rules, like propositions and meanings, have a life of their own, an existence somewhat apart from their expression.

Skinner invalidates these arguments by carefully distinguishing between a rule and the contingency described by the rule. For example, we may consider the following "natural rule": in order to prevent becoming wet, persons weighing over 75 kg should not walk on ice less than 10 cm thick. This describes a contingency of reinforcement. The contingency exists irrespective of any rule which describes it. The rules may be relatively durable, but in this case are generally less so than the contingency. They may be made more permanent if written on a piece of paper, recorded on magnetic tape, or engraved on tablets of stone. Physical contingencies are usually more durable than rules, but many social contingencies are rather evanescent—Macy's still offers specified kinds of merchandise on sale "today for one day only". If one distinguishes between a rule as a discriminative stimulus or set thereof, and a rule as the class of these, then

the problem of varying expressions of the rule is solved. We may handle this problem in the same way psychologists have treated instances and classes of responses, and we need not appeal to any Platonic meanings or propositions existing in another world, somewhere else.

BEHAVIOR AND GENETICS

It is well known that the Darwinian influence upon studies of behavior has been profound. With the rise of Mendelian theory, the synthetic theory of evolution, and quantitative genetics, interest in genetic causes of behavior has increased in many quarters. Furthermore, the development of ethology in Europe has added to this increasing interest in causal variables lying in the genome. Again, these developments are well known, as is the increasing sophistication with which behavioral geneticists pursue their research. Initially, the naive, to our eyes, research was devoted to demonstrating whether a given behavior was hereditary or caused by environmental circumstances. With the development of genetics as a science, the objective became determining which causal locus has the greater influence. Finally, with the development of quantitative genetics, the major thrust of investigations began to be to estimate what proportion of the variance observed in behavior may be accounted for; that is, what are the calculable heritabilities (h^2)? In a new note in *Contingencies of Reinforcement*, Skinner informs us that all these efforts have been in part misdirected. We are told that the proper task of psychology is not any of the above, but the identification of *all* the causal variables producing any kind of behavior and the complete elucidation of the interaction between them producing the behavior. The correct task for behavioral scientists is, according to Skinner, the study of the interacting relations among ontogenetic and phylogenetic variables. Behavioral geneticists tend to "ignore" the environment; so do ethologists in that some environments are classified by them as "unnatural".

This perhaps allows one to neglect less-difficult questions, but if one agrees that all the behavior of an organism must be explained, that all the causal variables must be listed, and their relationships with each other and behavior stated, then behavior in a laboratory is just as natural as behavior in the duck

pond. An analysis of the sort required will prove to be extremely difficult. The contingencies, ontogenetic and phylogenetic, interact; phylogenetic ones may be required for the effectiveness of ontogenetic contingencies. For example, the relatively undifferentiated behaviors out of which operants are created, and the propensity to be reinforced by food substances when deprived of them, are both products of phylogenetic contingencies, and both must exist before the operation of ontogenetic contingencies in order that the latter may have their effect. However, Skinner gives an example of how an instinct or species-specific behavior may be the product of prior ontogenetic contingencies, rather than the reverse. This thought experiment assumes that dogs possess no instinctive tendency to turn around when they lie down, but that lying down in this fashion is reinforced by the production of a more comfortable bed. If this has no special effects leading to increased probabilities of reproduction, presumably the readiness with which the response is learned will not be changed. However, phylogenetic advantages can be imagined: "such a bed may be freer of vermin, offer improved visibility with respect to predators or prey, permit quick movement in an emergency, and so on. Dogs in which the response was most readily conditioned must have been most likely to survive and breed. . . . Turning around when lying down may have become so readily available as an operant that it eventually appeared without reinforcement. It was then 'instinctive'. Ontogenetic contingencies were responsible for the topography of an inherited response."

Emphasis on the similarity of the form or structure of an animal or of its behavior with that of another animal is an example of what Skinner now calls the Formalistic Fallacy, *i.e.*, mistaking a description, whether empirical or purely formal, for an explanation. This constitutes another detriment to the sort of analysis required in this extremely tangled area. Such instances of the Formalistic Fallacy are disadvantageous because they divert attention from the relevant contingencies applying across phyletic categories. What is important is not the phylogenetic similarities between men and monkeys, but the similarity of the phylogenetic and ontogenetic contingencies to which they have been subjected.

A good illustration of the sorts of difficulties and challenges in this area may be given by Skinner's treatment of aggression. It has long been recognized that aggression has many definitions that employ incomplete or incompatible mentalistic or intentionalistic terms. Another approach to the definition of aggression has been by describing the behavior involved. But, this represents another instance of Formalistic Fallacy—controlling variables that *must* be specified are not given due attention in this sort of endeavor. For Skinner, "behavior is aggressive, if it harms others (or threatens to do so)." There are obvious phyletic components to such behavior that lead to increased chances of survival. Animals that aggress when in pain, or restrained, or when previously characteristic reinforcement is no longer forthcoming, may increase their chances of surviving to reproduce. If these behavioral dispositions are the results of genetic programming, then we say that such aggression is the result of phylogenetic contingencies. Such behaviors may be triggered by releasing stimuli and may be accompanied by autonomic responses, which may contribute to survival to the extent that they support vigorous activities. Skinner suggests that these responses are a major part of what is felt in aggression; the distinction among jealousy, anger, rage, hatred, and so on, may be the product of specific phylogenetic contingencies.

However, not all aggression, as defined by Skinner, is the product of phylogenetic contingencies. Indeed, it appears that a large part of aggressive behavior is the product of complex and subtle contingencies of operant reinforcement. As is the case with much social behavior, conditioned reinforcement is the primary effective variable. It appears that most aggressive behavior is shaped and maintained by conditioned reinforcement, much the way most appetitive behavior is, for in both cases, unconditioned reinforcers tend to be few and far between. The principal conditioned reinforcers appear to be various signs of damage. Their reinforcing functions are generated and maintained in three ways: (1) they function as way-stations to other unconditioned reinforcers, such as food, sex, rest, freedom from predators, a dry place, and so forth; (2) they are means to or signs of damaged other organisms; (3) they are the product of an evolutionary process. Skinner suggests that we have also to

consider the possibility that a capacity to be reinforced by signs of damage may have evolved under the phylogenetic contingencies that lead to phylogenetic aggression. Individuals may have been selected not only so that they would behave in such a way as to drive off predators or sexual competitors, but also behave in such a way as to produce any stimuli commonly preceding these effects, such as the signs of damage associated with successful combat. It does not seem probable, Skinner then argues, that phylogenetic contingencies have made *physical damage* reinforcing to an organism, much as they have made food, rest, or sexual activities reinforcing. Stimuli that are conditioned reinforcers and related to aggression may be shown to have all the properties of conditioned reinforcers related to other events, *e.g.*, they may reinforce behavior that is not itself damaging, such as observing wrestling, boxing, professional football, and, in other countries, bull-fights or cock-fights.

"A given instance of aggression can generally be traced to both phylogenetic and ontogenetic contingencies, since both kinds of variables are generally operative upon a given occasion. The fact the phylogenetic contingencies have contributed to the capacity to be reinforced by ontogenetic evidences of damage makes the interrelation particularly confusing." This warning is especially apt when we consider some of the practical problems that psychologists are called upon to solve. It has been argued that man is genetically defective in that he has no pre-programmed inhibitory mechanism that will constrain his aggressive tendencies. Skinner argues here, as he has in other situations, that the concept of inhibition is not needed. "We do not say that a carnivore refrains from eating vegetables because of an inhibition; it's ingestive behavior which is evoked only by certain kinds of stimuli. Even if it were true that tigers kill all animals except tigers we should not need to hypothesize that tiger-killing is inhibited by a special mechanism. Contingencies of survival will explain a discrimination among kinds of prey."

This, indeed, is true, but there could be genuine ontogenetically or phylogenetically generated inhibition of aggressive impulses. Such inhibition, if it exists, may be demonstrated in the form of an experiment such as that done by Brown and Jenkins (1967) with respect to inhibition in discrimination learning. It may

well be that the escalating intra-specific aggression characteristic of humans is the product of defective ontogenetic contingencies; it seems probable that it is, but only an experimental analysis will enable us to tell. Skinner, of course, argues that just that sort of analysis is needed for effective and practical attack upon current problems of national and international scope in this realm.

CONCLUSION

In a recent review, Rychlak (1969) complained that the new book by Evans (1968) told the reader very little about what Skinner did. This complaint reflects a more traditional literary view, related to the Formalistic Fallacy. In novels and plays, Skinner informs us, the writer must report only the behavior of the characters and let the reader or spectator have the fun of making inferences as to its causes. To misquote Quine (1969), the latter has all the verisimilitude of an experimental analysis with none of the fuss.

What would we gain by being told that Skinner drinks beer and watches professional football games on television on Sunday afternoons? Certainly, he argues, we would be given no more information by this. Rather, our uncertainty is increased, as is our opportunity to invent fictional inner causes of behavior. We are not told whether the beer drinking reinforces the seeing, or *vice versa*, or whether both behaviors are under the control of other currently active variables. The contingencies of reinforcement that have shaped and maintained these behaviors are the major items of interest for psychologists, rather than the behavior itself.

In a vein similar to that of the preceding paragraphs, Skinner presents arguments addressed to several other psychological issues, and thereby extends and develops his system. His latest work, like his earlier *Verbal Behavior*, again provides evidence that his systematic analysis may form the basis for a second behavioristic revolution. For although Skinner and Watson proclaimed similar programs, only Skinner, as *Contingencies* and his other works show, has been able to carry his out in a consistent fashion.

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