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Some Notes on Theoretical Constructs: Types and Validation from a Contextual Behavioral Perspective

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Abstract

Contemporary contextual behavioral analyses take a somewhat different view of theorizing than is commonly held in most of psychology. In formulating a natural science of behavior, theorists such as J. R. Kantor and B. F. Skinner rejected certain varieties of theoretical constructs. This paper divides theoretical constructs into abstractive and hypothetical formulations. It further subdivides hypothetical constructs into three subtypes, including constructs that are (1) in-principle observable, but at some other level of analysis, (2) in-principle unobservable, and (3) in-principle observable, but unobservable for some technical or practical reason. A distinction is made between the ontological and operational validity of theoretical constructs and methods for determining the operational validity of these constructs are discussed. Finally, the selective effects of experimentation and observation on theory development are discussed.
Key words: theoretical constructs, contextual analysis.

Resumen

Los análisis conductuales contextuales contemporáneos tienen una forma de teorizar algo distinta a la común en la mayoría de la psicología. En su formulación de una ciencia natural de la conducta, teóricos tales como J.R. Kantor y B.F. Skinner rechazaron el empleo de ciertas variedades de contructos teóricos. En este artículo se dividen los constructos teóricos en formulaciones "abstractivas" e hipotéticas. Posteriormente, los constructos hipotéticos se subdivididen en tres subtipos que incluyen los constructos que son (1) en principio observables, pero en algún otro nivel de análisis, (2) en principio inobservables, y (3) en principio observables, pero inobservables por razones técnicas o prácticas. Se establece una distinción entre validez ontológica y operacional de los constructos teóricos y se discuten los métodos para determinar la validez operacional de estos constructos. Finalmente, se discuten los efectos de la experimentación y la observación sobre el desarrollo de las teorías.
Palabras clave: constructos teóricos, análisis contextual

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Science: Evidence and Formulation

It is generally held that science has two minimum overriding requirements: "fidelity to empirical evidence and simplicity of logical formulation, fidelity to the evidence taking precedence in cases of conflict" (Caws, 1967, p.339). Further, Kantor (1967; 1981) suggests that there is a critical distinction between theoretical constructs (i.e., the verbal products of scientists interacting with events of interest) and the crude events themselves. That is to say, talk about the world ought not be confused with the world that is talked about. An analysis of scientific theorizing in contemporary psychology reveals a frequent violation of both the principle that constructs ought to be based upon close contact with events within a proposed field of investigation, as well as frequent conflation of constructs with events. This perspective on theory building is a minority position in psychology; however, the sensibilities that direct it have existed throughout the last century and have produced some remarkably successful basic and applied results. We will begin by making some distinctions among different types of theoretical constructs and discuss construct validation. We will also relate some implications of this analysis to certain problematic areas in contemporary psychological theorizing.

Theoretical Constructs

Scientific constructs may usefully be divided into abstractive and hypothetical types (Duhem, 1914/1954; see also MacCorquodale and Meehl, 1948), as shows in table 1.

Abstractive Constructs

Abstractive constructs refer to events, or properties of events, selected from a total event field. These constructs are produced in the analytic effort, for use in that analysis. They are not held to possess any ontological status as entities independent of other aspects of the field. Mach recognized this process of abstraction, as demonstrated by the following passage: "In speaking of cause and effect we arbitrarily give relief to those elements to whose connection we have to attend in the reproduction of a fact in the respect in which it is important to us. *There is no cause nor effect in nature; nature has but an individual existence; nature simply is.*" (Mach, 1883/1953, p. 447, emphasis added)

Accordingly, we may divide up and emphasize aspects of an observational field in any number of ways depending upon the intent of our analytic effort.

By example, in the investigation of reflexive responding of organisms, aspects of the event field may be abstracted and classed as stimuli (S). Other aspects may be abstracted and classed as responses (R). In addition, constructs may be arranged as abstracted relational formulations such as R=f(S). The purpose of such constructs is purely descriptive and summarative, and as such they facilitate the interactions of behavioral scientists investigating relevant parameters of these phenomenon.

Skinner and Kantor converge on the recognition that a dependency statement, such as R=f(S), is not a property of the world, but is instead a product of scientists. According

to Skinner we may "determine and state a correlation between the characteristics common to all of our observed responses and the characteristics common to all our observed stimuli, and we may name that construct the flexion reflex" (1931/1972, p. 452). Skinner calls gratuitous the assumption "that there *is* a flexion reflex which exists independently of our observations and which our observations approximate" (1931/1972, p. 452). The line of demarcation between constructs and events is likewise explicitly relayed in the work of Kantor: "These [dependency] relations are only operationally justified. Such assumptions are not valid except in specific investigational circumstances and do not imply that the events are structured on such a basis" (1967, p. 98).

Hypothetical Constructs

A second sort of construct may be termed hypothetical. Constructs of this sort aim not to describe events observed, but to explain them in terms of some proposed unobserved variable. This type of variable breaks reasonably well into three subtypes. For the purposes of the current discussion we will call these Type I, Type II and Type III hypothetical constructs. They are as follows: those existing in some other field of observation as yet uninvestigated (Type I Hypothetical; HCI); those variables which are in-principle unobservable (Type II Hypothetical; HCII); and, finally, variables which are presumed to exist within the current field of investigation, but which are not currently observable for one reason or another, but which are at least in-principle observable (Type III Hypothetical; HCIII).

Table 1. Types and characteristics of theoretical constructs

Construct Type	**Observability Status**	**Examples**
Abstractive	Directly observable in current field of observation	Stimuli, responses, reinforcement
Hypothetical: Type I	Directly observable, but in some field not currently being observed	Memory traces as neural trace in cognitive psychology
Hypothetical: Type II	In-principle unobservable	Id, ego, Piaget's schema
Hypothetical: Type III	In-principle observable within current field of observation, but not observable for some technical reason	Black holes, sometimes reinforcement histories are appealed to, which have not in fact been directly observed, but are in-principle observable

Memory traces in contemporary cognitive psychology (see Watkins, 1990) provide an example of a Type I hypothetical construct, where the memory trace is presumed to be a neural trace. Freud's id or Piaget's schema are examples of Type II hypothetical explanatory variables which are of the in-principle unobservable variety. In the case of Type III hypothetical constructs, consider the example of the astronomer who, noting the deviation in a planet's orbit, postulates the existence of another as yet unobserved planet as causing that deviation. What is occurring in this example is that, given what is known about orbital patterns and the influence of massive objects one upon the other, and given the observed deviation in the planet's path from what would be expected given known planets, another planet is postulated.

Invalid Constructs

Kantor has suggested "no scientific enterprise will be successful unless the worker derives his constructs from contacts with events" (1967, p. 40). Success though, in the sense of improved orientation to the subject matter and resultant interactive efficacy, is a continuous variable. A scientific enterprise may be successful when its formulations contain constructs not derived from events within a given field of investigation. However, these formulations may be stripped of these constructs with no loss of explanatory force or resultant prediction and control. From Kantor's perspective, hypothetical constructs are considered to be invalid, since by definition they are not derived from interaction within the current observational field. Skinner likewise eschews "any explanation of an observed fact which appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions" (Skinner, 1950/1972, p. 69). Type III hypothetical constructs, the subvariety that are in-principle observable and presumed to exist within the current field of investigation, would be exceptions to this rejection of hypothetical constructs.

Two sorts of events may serve as sources for invalid constructs (i.e., invalid in the sense of being superfluous). One source of invalid constructs may be events from another field of investigation. The second are constructs arising from interaction with purely verbal events.

Type I Hypothetical Constructs: Reductive Sources

Some invalid constructs emerge when constructs in one event field are borrowed for explanatory purposes from a different event field. Thus, physiological events might serve as a source of constructs in a psychological explanation. Pavlov (1927/1960) provides an example of this sort, with hypothesized activities of the cerebral cortex—so called psychic secretions—serving an explanatory function in theorizing about the interrelations of whole organisms in and with environing events[1]. While Pavlov was able to become quite well oriented to the interrelations of organisms and environing events, his neural constructs did not contribute to an improvement in that orientation.

This does not to invalidate the direct study of the relations between behavior and neural events or environmental influences on neural events. What it suggests is that

speculation about these relations ought to take place in the context of direct experimental interactions with the events about which one is theorizing. That is, theorizing about neurology ought to involve direct interaction with neural events.

Type II Hypothetical Constructs: Purely Verbal Sources

Another sort of event that scientists may interact with in the formulation of constructs are purely verbal events. Explanations of the increasing speed of falling bodies provides a remote historical example of a construct of this type. Prior to the time of Galileo it was thought by some that the increase in velocity of objects as they fell was a result of their increasing exuberance as they came nearer and nearer the ground. This construct (i.e., the happiness of the falling body) has its history in primitive animism and anthropomorphism, and is thoroughly devoid of contact with actual observations of falling bodies.

Expectancies in learning theory serve are a more recent incarnation (see Tolman, 1932). Expectancy interpretations of reinforcement suggest that an expectancy involves the "the formulation of a prediction (and the possibility of a response)… Any response that is required is based on the prediction, but the prediction usually is more elaborate than may be inferred from the observable response" (Atkinson and Wickens, 1971, pp. 86-87). It is not in dispute that human subjects can and do formulate predictions. For example, I predict that I will type another sentence after this sentence. I can observe this prediction directly in my own behavior. However, what direct evidence do we have that a nonverbal organism engages in the "formulation of a prediction?" If we have no access to the prediction apart from our access to the history that generated it and the response that followed it, and, if that history and the response that follows are well correlated, what explanatory force is added by our speculation as to the prediction? If the events are not well correlated, how can we appeal to the absence of expectancy/prediction when history and response are our only means of accessing the prediction?

Examining again my prediction that I will write another sentence, sometimes I predict, but more often, I merely write. Consider the evolutionary inefficiency if a prediction were required prior to every response! From this perspective, predictions to which we have direct access, by self-observation or by asking the subject, are directly observable (at least by an n of 1). Since they are directly observable, they are properly abstractive variables in these instances. When we begin to speculate about animal "predictions" we have moved into the realm of Type II hypothetical variables. Again, as was the case with reductive constructs, having such a formulation may not prevented effective orientation with respect to the behavior of interest, but just as the happiness of falling bodies is superfluous to the empirical relations emerging from the direct observation of falling bodies, so to are speculated predictions superfluous to the prediction of animal behavior.

Construct Validation

Within this system of theorizing, constructs may be validated or invalidated only via

contact with the events with which the creators of constructs interact in the formulation of those constructs. Thus, valid constructs (those derived from interaction with events within the field of investigation) may be invalidated by ongoing interactions (e.g., experimentation and observation) with the events within that particular field of investigation.

Constructs based on reductive portage from another event field, for example, Pavlov's physiological reductionism, hold some advantage over purely verbal constructs in that there are at least *some* observable events which could serve as a source of disconfirmation. When physiologists began to study the actual neural events of the cerebral cortex, psychic secretions were nowhere to be found.

Even if, however, one possessed valid constructs of physiological events occurring in a Pavlovian conditioning paradigm, those events could in no way confirm or disconfirm constructs articulated in the psychological field. For instance, the temporal gap between presentation of a neutral stimulus and an unconditioned stimulus is related to the stimulus functions that the neutral stimulus comes to have. A precise empirically derived statement of this relationship could not be invalidated by any amount of information about the physiology of the organism in question (see also, Skinner, 1938, chapter 12).

Likewise, neural constructs derived as result of interaction with neural events are not susceptible to psychological validation or invalidation; their validity can be assessed only in the field from whence they were derived. Thus, the invalidation of Pavlov's cerebral constructs had to wait for the interaction of scientists with events in the physiological field before they could be disconfirmed.

Similarly, only verbal events can validate or invalidate constructs emerging from purely verbal sources. An animistic explanation, such as the happiness of the falling body in the above example, cannot be rightfully rejected as result of any interaction with the events in any field of natural science (excepting a natural science of language and logic). This sort of explanation may be rejected only on the basis of interaction with verbal events such as the principle of parsimony -which is a relational formulation resulting from interactions with the events involved in the practice of scientific explanation. This points to the role of a naturalistic philosophy of science in assessing the qualities of those explanations that, in the past, have facilitated increasingly effective orientation and those which have not.

Operational Versus Ontological Validity

When constructs are considered as operationally justified modes of speaking, they may be evaluated within the operational context in which they are to be utilized. Ideally, constructs ought to orient the investigator to a given field of investigation such that the events within the field in a way that allows for an increasing ability to predict an influence those events.

Aspects of events within a field may be arbitrarily abstracted (e.g., rates of occurrence, temporal order, etc.) from the total event field just as the field itself is abstracted from the totality of events. Dependency relations among abstracted properties might be constructed such that an investigator can manipulate aspects of the event field and

observe subsequent interactions among other abstracted aspects of the field. These manipulations may serve to better orient the investigator to interrelations among events in the field; however, it would be a mistake to then attribute the manipulative prowess achieved by way of this better orientation to the ontological validity of the constructs.

The conflation of constructs and events has led philosophy and science down some extremely self-absorbed and unfruitful paths of investigation. The extent to which constructs rightly orient the investigator to the field is the criterion against which the validity of the construct is evaluated within this system of theorizing. This validity is, however, not ontological validity, rather it is operational validity. The majority of psychological science considers increased ability to make remote predictions an to influence events a evidence of the ontological status of the theory. Loudan calls such an assumption convergent epistemological realism (Loudan, 1981). That is, to the extent that a theory produces many remote predictions, allows for the control of some events, generates a new lines of research, then to that extent we are satisfied that the key theoretical terms in the theory refer to "real" events. The problem with this position is that when one looks at the historical record, we can find instances of theories, such as the XIX century theory of the ethers that were productive in every modern sense of the word, yet we would now deny that the key terms of theories of the ethers referred to real events. By contrast, atomic theories during that same period were extraordinarily unproductive, yet we might argue that the key terms of atomic theory did refer to real events. (See Laudan, 1981, for a critique of convergent epistemological realism, a position which suggests that increasing successes is equivalent to increasing ontological validity.) In fact, the term atom dates back at least to Democritus (approximately 440 b.C.). The generativity of atomic theory shows extraordinary growth at a point in time when methods for direct examination of the properties of atomic components became available in the latter part of the XIX century (e.g., the many experiments using cathode ray tubes). According to the position described in this paper, what was needed for atomic theory to progress, was the selective effects of experimentation on theorizing.

Theoretical constructs orient a scientist to a field of investigation. Limits to the goodness of orientation provided by a given set of constructs are encountered. Intimately tied to the amplification of knowledge is the amplification of problems. Theories are ways of talking that help in the solving of problems, but there are always new problems. Old ways of talking may persist to the extent that problems at a certain level are still adequately handled by them. Newtonian physics is still capable of rendering soluble certain problems, and is still taught. When Newtonian physics is taught though, we do not have a difficult time keeping our estimation of their validity at the operational level. Quantum theory has facilitated interaction with physical events that could not have occurred in the Newtonian paradigm. We would be well advised though, not to take the ontological validity of the new physics any more seriously than the old. It too will introduce problems even as it solves others. If the history of scientific knowledge amplification has taught us anything at all, it is that we will eventually be exposed to the corrigibility of our most prized theories.

A Contemporary Contextualistic Behavioral Rejection of Invalid Constructs

In-principle Unobservable Constructs

The "why" of rejecting hypothetical constructs that exist in some in-principle unobservable realm is well explicated in Skinner's rejection of mentalism (1974). To attribute interactive participation, or worse causal efficacy, to events existing in a nonspatiotemporal realm calls up a host of problems that have plagued philosophy for 2,000 years. Construing observed events as caused by unobservables is classic dualism and leads to all of the puzzles inherent in a dualistic position. Among the problems emerging from a dualistic stance are: Do spatiotemporal and nonspatiotemporal things interact? If so, of what sort is that interaction? We can describe the interactions of spatiotemporal events, but how would we describe the interaction of one event that was spatiotemporal and one event that was not? Where indeed would we look for such an interaction? All of our talk about the qualities of events are spatial and temporal qualities. What language would we use to describe events of some other sort?

The potency of contextual behavioral analysis is due in part to its being founded upon rejection of these sorts of explanations that, according to Skinner, "kept attention away from the external antecedent events which might have explained behavior, by seeming to supply an alternative explanation" (1974, p.18). A comprehensive, experimentally verifiable and naturalistic description of behavior necessarily involves appeal only to independent variables that are at least in-principle observable and in-principle manipulable and dependent variables that are at least in-principle observable. The reason for Skinner's rejection is not because these events are unobservable or unmanipulable, but because of the problematic practical consequences of appeal to such variables when one's goals are prediction and control.

Reductive Constructs

The rejection of reductive constructs in a contextual behavioral analysis is likewise based upon issues of practicality. Behaviorism, as formulated by Skinner (1974), is concerned with the interactions of whole organisms in and with environing events. This is not to deny the importance of, say, physiological events. Physiological events are also events of interest in their own right; however, they need to be analyzed and investigated by very different means and using very different analytic constructs than are used in psychology. In addition, there is no reason that direct analyses at the intersection of these domains cannot be done simultaneously.

Modern cognitive psychology may be used to illustrate some problems with reductive analyses. According to some cognitive psychologists "mental processes are brain processes" (Ellis and Hunt, 1983, p.11). While there may be instances where observation is not possible for technical reasons, why would one speculate about neurological events when one could examine those events directly?

There are many highly sophisticated means currently available for observing neural

events (e.g., computer axial tomography, positron-emission tomography, etc.). Advances in our understanding of the activities of the nervous system will surely be more likely to emerge from these direct methods of observation, rather than from the unrestrained theorizing of psychologists whose work is unnecessarily remote from the subject matter purportedly of interest.

Even within the cognitivist camp, some have come to criticize this sort of neural mediationism. Watkins (1990) argues forcefully against mediationism in memory research. Behavior analysts, though they may not agree with all of Watkins' conclusions, should find his explication the problems inherent in neurally mediated explanation clear and concise:

"The memory trace is but one of a countless number of hypothetical constructs within the mediationalist's arsenal, but it is the heart of mediationism and is the essential reason why today's theories are inherently more complex than the phenomena they purport to illuminate... The trace plays host to all manner of unconscious activities, and cutting it out of our thinking would, directly or indirectly, put an end to an entire parasitic netherworld... It would be hard to deny that remembering implies an enduring change in the rememberer or that the quest to learn more about that substrate constitutes a legitimate -even exciting- science. But such a science is not the same as the science of memory. Students of the substrate of memory, unlike students of memory, have or seek techniques to study the retention stage independently of other stages. Students of memory do not have such techniques and never will have (Watkins, 1990, pp.330-331).

Skinner (1961/1972) argues that the physiological inner man is no less problematic than the mental inner man. The validity of a behavioral science need not rest upon, nor reduce to, the "hard facts" of physiology. As Skinner puts it: "The effects of deprivation and satiation on behavior are not the same as the events seen through a gastric fistula... Both sets of facts, and their appropriate concepts, are important -but they are *equally* important, not dependent one upon the other. Under the influence of a contrary philosophy of explanation, which insists upon the reductive priority of the inner event, many brilliant men who began with an interest in behavior, and might have advanced our knowledge of that field in many ways, have turned instead to the study of physiology. We cannot dispute the importance of their contributions, we can only imagine with regret what they might have done instead" (Skinner, 1961/1972, pp.325-326).

Summary

In this paper, I have argued that adherence to empirical evidence and sharp distinction between constructs and events is a key aspect of the philosophy of science undergirding contemporary behavioral psychology. I have also argued for certain criteria for construct formulation and validation, and that an increase in the potency of this already powerful analysis will be best advanced by adherence to these fundamental principles. The following enumerates what we take to be some of the key points and underlying assumptions of our case:

1. Formulated constructs ought to be continuous with the events within the field of purported interest.

2. The ultimate validity of constructs is reducible to the extent of improvement in orientation to the field of interest they provide (i.e., enhanced prediction *and* influence).

3. Constructs ought not be confused with the crude events with which the scientist interacts.

4. Constructs are never attributed ontological validity as result of any operational successes, rather they are maintained as operationally valid. The extent of this validity may be assessed according to the metric described in proposition two.

5. Divergence from the above will at best be superfluous and at worst will draw the investigator's efforts in directions unfruitful to the advancement of a given field.

Although the position presented in this paper would be far too instrumentalist for Popper, his comments on the composition of theories is relevant: "We choose the theory which best holds its own in competition with other theories... This will be the one which not only has hitherto stood up to the severest tests, but the one which is also testable in the most rigorous way. A theory is a tool which we test by applying it, and which we judge as to its fitness by the results of its application" (1934/1959, p.108).

What I propose is an approach to theorizing that is consistent with the expected effects of selection by consequences. I have argued against theories that appeal to events outside observations and operations, because of their inherent isolation from the selective effects of experimentation. I propose that science in general, and psychology in particular, will be best advanced by a rich interplay of theorizing and experimentation. The sort of theory suggested is expected to maximize these selective effects

Notes

1. The term «environing» is used instead of environment to highlight our view that the pertinent environment, from a psychological perspective, is not the static, objective environment, but instead the psychologically active, functioning environment.

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