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A behavior-analytic perspective on development

Uma perspectiva analítico-comportamental do desenvolvimento

Una perspectiva analítico conductual del desarrollo

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ABSTRACT

In this article, I describe how my behavior-analytic perspective on infant and child development evolved and what a behavior-analytic theory of development might look like. I begin by describing how my approach to teaching child development courses changed from focusing mostly on behavior analysis to teaching critical thinking skills that would enable students to evaluate traditional developmental research and theory and then to find a behavior-analytic perspective more appealing. I describe the critical thinking skills I teach students, including nominal fallacy, circular reasoning, reification, and parsimony. Throughout, I contrast a traditional developmental approach to research and theory with a behavior-analytic approach. In particular, I note differences between the between-subjects experimental designs used by developmental researchers and the within-subjects experimental designs used by behavior analysts in terms of internal and external validity and their implications for theory construction. I argue that, because behavior-analytic theory is inductively derived from decades of experimental analysis, it is in a better position to achieve the goals of prediction, control, and understanding and can generate successful technologies of behavior change.

Keywords: behavior analysis, cognitive psychology, developmental psychology, theory, nominal fallacy, parsimony, reification

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RESUMO

Neste artigo, descrevo como minha perspectiva analítico-comportamental da infância e do desenvolvimento infantil evoluiu como uma teoria analítico-comportamental do desenvolvimento. Começo descrevendo como minha abordagem para o ensino do desenvolvimento infantil mudou de um foco na análise do comportamento para o ensino de habilidades de pensamento crítico que possibilitariam aos estudantes avaliar a pesquisa e a teoria tradicional de desenvolvimento, tornando uma perspectiva analítico-comportamental mais atrativa. Descrevo as habilidades de pensamento crítico que ensino aos estudantes, como a falácia nominal, raciocínio circular, a reificação e a parcimônia. Contrasto a abordagem tradicional de pesquisa e teoria do desenvolvimento infantil com a abordagem analítico-comportamental. Em particular, aponto as diferenças entre os delineamentos experimentais entre-sujeitos utilizados por pesquisadores do desenvolvimento e os delineamentos intrassujeito usados pelos analistas do comportamento, em termos de validade interna e externa, e sua implicação para a construção da teoria. Argumento que, por ser indutiva e derivada de décadas de pesquisa experimental, a teoria analítico-comportamental é o melhor caminho para alcançar os objetivos de predição, controle e compreensão, podendo gerar tecnologias mais promissoras de mudança do comportamento.

Palavras-chave: análise do comportamento, psicologia cognitiva, psicologia do desenvolvimento, teoria, falácia nominal, parcimônia, reificação

RESUMEN

En este artículo describo cómo mi perspectiva analítico conductual de la infancia y del desarrollo infantil evolucionó a una teoría analítico conductual del desarrollo. Empiezo por describir cómo cambió mi enfoque de enseñar los cursos de desarrollo infantil, de centrarse en el análisis del comportamiento a enseñar habilidades de pensamiento crítico que permitirían a los estudiantes evaluar la investigación y la teoría del desarrollo tradicional y luego encontrar a la perspectiva analítica del comportamiento más atractiva. Describo las habilidades de pensamiento crítico que enseño a los estudiantes, como la falacia nominal, las razones circulares, la reificación y la parsimonia. Contrasto el enfoque tradicional de investigación y teoría del desarrollo infantil con el enfoque analítico comportamental. En particular, apunto las diferencias entre los delineamientos experimentales entre sujetos utilizados por investigadores del desarrollo y los delineamientos intra-sujeto usados por los analistas del comportamiento, en términos de validez interna y externa, y su implicancia para la construcción de la teoría. Argumento que como la teoría analítico conductual es inductiva y derivada de décadas de investigación experimental, está en una mejor posición de alcanzar los objetivos de predicción, control y comprensión, pudiendo generar tecnologías más prometedoras de cambio del comportamiento.

Palabras clave: análisis del comportamiento, psicología cognitiva, psicología del desarrollo, teoría, falacia nominal, parsimonia, reificación

After I received my Ph.D. and secured an academic position, I began teaching courses in child development. As a behavior analyst teaching courses in traditional psychology, I was immediately confronted with how to reconcile my scientific training in behavior analysis with a subject matter that was primarily cognitive in nature and that relied on research designs of questionable reliability. I began by lecturing solely about behavior analysis. I was, after all, very enthusiastic (as I still am) about it. Unfortunately, I think I came across as a little too enthusiastic. Some students were turned off because they felt as if I were forcing my beliefs on them and not giving other approaches equal time. In retrospect, they were probably right.

I wish I could say that the solution to the problem of how to teach a traditional subject matter – child development – without compromising my scientific values occurred to me in an epiphany. In actuality, however, the solution evolved over many semesters and years. In my courses on child development using a traditional textbook, I began by teaching students some critical thinking strategies I had learned in my training as a behavior analyst to deal with a traditional subject matter. What resulted was not only the application of those critical thinking skills to the subject matter of child development, but, in fact, an overarching behavior-analytic theory of child development.

After years of teaching child development from this perspective, I acquired an extensive verbal repertoire consisting of long strings of intraverbal behavior. When B. F. Skinner died in 1990 and a call for papers was put out for a special issue of the *American Psychologist* honoring him and his accomplishments, I submitted an article titled, "Theory in Behavior Analysis: An Application to Child Development," which was published (see Schlinger, 1992). In that article, I applied behavior-analytic theory to two topic areas in child development: infant memory and perceptual size constancy. One of the cognitive developmental psychologists whose work I had referenced, Jeffrey Fagen, wrote a commentary not only on my article but on an article by Gewirtz and Peláez-Nogueras (1992) in the same issue of the *American Psychologist*. He argued that the concept of reinforcement – which Gewirtz and Peláez-Nogueras and I had discussed – was insufficient to explain certain findings from his lab and that the concept of learned expectancy was needed (Fagen, 1993). I penned a reply (as did Gewirtz & Peláez-Nogueras, 1993) in which I argued that learned expectancies were not adequate scientific explanations because they could not be defined independently from the behaviors they were said to explain, did not point to real observable events, and were not congruent with findings from other behavioral sciences such as neurophysiology and evolutionary biology (Schlinger, 1993). Although I did not state this explicitly at the time, the upshot of my criticisms was that the concept of expectancy was not a parsimonious explanation of the observed behavioral relations. In all of these writings, I applied a set of critical thinking skills to cognitive concepts and explanations of child behavior.

After the *American Psychologist* publications, I immediately began expanding the arguments I made in those articles into a book in which I applied behavior-analytic theory to other topic areas in infant and child development (see Schlinger, 1995). At the time, I viewed my book as a follow-up to the books on child development by Bijou and Baer (Bijou, 1976; Bijou & Baer, 1978). Since then, I have tackled a few other more specific topic areas in child development, especially Theory of Mind (ToM) (see Schlinger, 2009, 2017a), as well as general concepts in behavior analysis and child development (Schlinger, 2002), all through the lens of a behavior-analytic theory.

In the present essay, I describe some of the critical thinking strategies I employed in my classes on child development as well as in my writings. I point out how in general I have interpreted traditional research and theory in child development based on those strategies in combination with a behavior-analytic theory derived from decades of the experimental analysis of nonhuman and human behavior. Throughout, I contrast a behavior-analytic approach to research and theory with more traditional developmental approaches.

CRITICAL THINKING STRATEGIES

Observation

To begin to deal with child development from a scientific perspective, one must start with the hallmark of the scientific method: observation. In psychology, as in other disciplines, this means one must observe the phenomena of interest. As mental or cognitive processes (e.g., mind, schemas, memories, consciousness) can never be observed, any approach based on these constructs violates this requirement. Of course, psychologists can only observe behavior (or neurological events with much greater difficulty), which means that those whose main interest is cognitive processes must always infer them from observed behavior. That would not be a problem if cognitive psychologists had already built up inductively derived principles based on observed events and then extrapolated those principles to unobserved events assumed to have the same properties (see Schlinger, 1998). But that has not been the case.

Psychologists whose main interest is unobserved mental or cognitive events and processes might argue that they are no different than other scientists, such as theoretical physicists, who hypothesize about unobserved events such as quarks, or astronomers who hypothesize about extrasolar planets. The difference, as I have argued (Schlinger, 1998), is that theoretical physicists and astronomers base their speculations on a foundation of inductively derived, objectively observed events, whereas cognitive psychologists do not. Thus, for example, theoretical physicists and astronomers deduce the existence of (i.e., theorize about) unobserved events (quarks, dark matter, extrasolar planets) because they have already observed real events under controlled conditions in laboratories and in the natural world. They then test those theories by looking for observations of physical events predicted by the theories and consistent with other observations.

Cognitive psychologists – and by that moniker I mean any psychologist who observes behavior as a way to infer "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, p. 193) – observe behavior not because they are necessarily interested in the behavior per se, but rather because they are interested in what the behavior reflects or represents about some cognitive event or process (e.g., Frensch, 2001; Solso, 2001).

Developmental psychology is a subdiscipline of cognitive psychology in that most developmental psychologists are interested in constructs that cannot be independently observed and, therefore, directly verified. Historically, developmental researchers have been interested in such mental constructs as expectancies, schemas, operations, memories, secure or insecure attachment, morality, and Theory of Mind (ToM), among many others. Developmental psychologists talk about these constructs as if they were more than just the behavior of children occurring under certain circumstances. However, as with other members of the cognitive family, developmental psychologists

must infer these underlying cognitive events and processes only from the observation of the behavior of children. Of course, the alternative, as I will argue later, is to consider the behavior as the variable of interest in its own right. As we will see, considering behavior as a dependent measure in its own right, and not as a function of cognitive constructs, has both theoretical and practical advantages and loses nothing of the importance given to it by inferring unobserved events. The ways in which cognitive psychologists infer mental or cognitive events from observed behavior results in several errors of logic, or, more specifically, of verbal behavior.

Nominal fallacy and circular explanations

One error of verbal behavior is called nominal fallacy, in which simply naming or labeling something is tantamount to explaining it. For example, if you observe a child running around a classroom, behaving impulsively, having difficulty paying attention, and being easily distracted and ask the teacher why he is doing those things, the teacher might say, "That's ADHD." The assumption is that calling it ADHD explains the observed behaviors. Of course, ADHD is the abbreviation for attention deficit hyperactivity disorder, which is simply the name of the behaviors you observed – attention deficit and hyperactivity – not an explanation for those behaviors.

Likewise, other concepts in developmental psychology also fall prey to the same error. So, for example, if a child whines and clings when a parent leaves a room, and we are told that is "insecure attachment" as if we have somehow explained the behavior of whining and clinging, we are committing the error of nominal fallacy.

Verbally, it is not much of a stretch to move from nominal fallacy to circular explanations (or reasoning). Thus, if a teacher says that a child runs around the room and has trouble paying attention because he has ADHD, that qualifies as a circular explanation if the only evidence for the explanation – that he has ADHD – is the behaviors you observed. In English, the word because is derived from the Old French *par cause de* (by reason of) and the Middle English *by cause*. Thus, when we say that a behavior occurs because of something, the something is assumed to be the cause. However, the faulty logic of circular explanations becomes clear when we ask what evidence there is for the explanation. For example, if we are told that a child searches for a hidden object because she has object permanence, we may ask what evidence there is for the object permanence. And if the only evidence is the very behaviors we are trying to explain – the search behaviors – then this is a circular explanation.

In his book *Verbal behavior*, B. F. Skinner (1957) described what he called explanatory fictions – explaining behavior by appealing to mental or cognitive events or processes, the only evidence for the behaviors to be explained. For example, a cognitive developmental psychologist might say that a child predicts that someone else will act in a certain way because she has a ToM. But if we then ask what evidence there is that the child has a ToM and are told that it is that the child can predict how the person will behave, then ToM is an explanatory fiction or a circular explanation. As Skinner (1957) wrote, "It is the function of an explanatory fiction to allay curiosity and to bring inquiry to an end" (p. 6). In other words, once someone offers an explanatory fiction or circular explanation, we tend to be satisfied that the cause of the behavior has been identified and we are likely to stop looking for the actual cause.

Explanatory fictions and circular explanations are pervasive in psychology and among lay people. The problem, as noted above, is that explanatory fictions and circular explanations fool us into believing that we have identified the cause of the behavior.

Unfortunately, as we have only labeled the behavior and then used that label as an explanation, we are back where we started: not knowing the cause(s) of the behavior and, in the case of ADHD and other "disorders," not being able to change the problem behaviors.

Reification

Another verbal error committed by many psychologists, including developmental psychologists, and one that often naturally follows from explanatory fictions, is that of reification, also known as the reification fallacy. Simply stated, the reification fallacy is when an abstract concept is treated as if it were a real event or physical entity. For example, the term "emotion" is an abstract concept, but most people talk about having emotions and even trying to identify different emotions as if we can create a taxonomy of emotions. As Peters (1963) put it many years ago, "Specifically, the error consists in assuming that, because we have a single noun-word, 'emotion,' something in nature must correspond to it, something as independent, as unique and unchanging, and as readily capable of entering subject-predicate relations with other things" (pp. 437-438). Kroger and Wood (1993) referred to reification as "the readiness to transform a metaphor into an entity with thing-like character" (p. 1297). Although several psychologists have identified and warned against the error of reification (e.g., Eacker, 1972; Kroger & Wood, 1993; Macken & Jones, 2003; Notterman, 2000), most psychologists seem not to have noticed.

Just as there are numerous concepts in psychology that have been reified (e.g., mind, phonological loops, personality traits, mental disorders), many concepts in developmental psychology have fallen prey to this error. For example, the Piagetian concept of object permanence began as a series of observations by Piaget and others in which children behaved in some manner appropriate to absent or hidden objects. Usually, the behavior involves searching for an object that has been hidden or that is out of sight. Calling it search behavior is thus more parsimonious than calling it object permanence (Harris, 1987). Whereas object permanence began as a description of the search behavior, it morphed grammatically into something children possess, which then determines the search behavior.

Consider another Piagetian concept, that of schemas. According to Piagetian psychologists, schemas are ways of acting on the world; in other words, they are actions. But then, in a grammatical twist, a schema becomes the "basic structure" or "essence" of behavior (Ginsburg & Opper, 1988). So, what does the essence of behavior mean? Ginsburg and Opper point out that no two actions by an infant are precisely the same. For example, with respect to thumb-sucking, "There is no one act of thumb-sucking, but many; in fact there are as many as the number of times the child brings the thumb to the mouth" (p. 21). Ginsburg and Opper go on to say that there is a structure to thumb-sucking, namely, that "the infant has acquired a regular way of getting the thumb into the mouth," and this regularity is what Piaget meant by schema. As I have noted (Schlinger, 1995, pp. 124-125), this description is very similar to what behavior analysts would call an operant; that is, many topographically different responses that all produce the same outcome of thumb-sucking. Thus, Piaget's (and Ginsburg & Opper's) verbal behavior was controlled by the same observable facts as that of behavior analysts (see Schlinger, 2013, and Skinner, 1945, for an analysis of how the verbal behavior of psychologists and behavior analysts is controlled by observed behavioral relations). The difference is that Piaget's term – schema – has become reified into a cognitive structure.

In general, developmental psychology has been dominated by a structural approach in which behavior is classified according to its form or structure as opposed to its function. Elsewhere (e.g., Schlinger, 1995, pp. 16-26), I have described the characteristics of a structural approach to behavior and some of its attendant problems. For present purposes, the point about a structural approach is that it readily leads to the error of reification. For example, with respect to so-called normative stages of development, Lipsitt (1981) has written that "we come to regard those stages as real conditions of the organism rather than as artifacts of our observational procedures and methodologies" (p. 31). I think he was referring to the verbal behavior of stage theorists.

There are several other problems with structural approaches beyond that of reification. For one, once an adjectival term becomes a noun – or in the case of schema, begins as a noun – it becomes easy to place the noun – as a thing – inside the individual. Thus, young children have object permanence or schemas or ToM. And individuals have emotions, memories, consciousness, personality, etc. If we follow the grammatical trail from descriptors to entities possessed by individuals, the next logical step is to assume that those entities cause the very behavior from which they were inferred in the first place. You should recognize this as the error of circular reasoning or as an explanatory fiction. As if that were not enough, another grammatical problem arises from reifying behavior: "Locating the determinants of behavior inside children makes it easier to describe children as the originators of their actions" (Schlinger, 1995, p. 25). Thus, children "perceive," "decide," "judge," and so on. Contrast this with the behavior-analytic approach in which the environment evokes and selects behavior.

Parsimony

One of the main problems resulting from errors of nominal fallacy, circular explanations, and reification is that explanations of observed behavior are less than parsimonious. The term parsimony in science is usually used to refer to explanations. Thus, a parsimonious explanation is one that makes the fewest assumptions. So, if we say that a child screams and cries and throws things on the floor because she is possessed by evil spirits, that is not a parsimonious explanation of the behaviors because it makes a lot of assumptions, namely, that evil spirits exist, that they can inhabit someone's body, and that they can make a person behave in a certain way. Notice that we cannot disprove the explanation; we can only state that it is not parsimonious. Similarly, if we are told that a child behaves in those ways because her Id is overpowering her Superego and the Ego is too weak to control the Id, this explanation is likewise not parsimonious because it makes a lot of assumptions: that ids, egos, and superegos exist and that they can make someone behave in a certain way. Notice that we cannot disprove the Freudian explanation either. If, however, we are told that the child screams and cries and throws things because when she does so her parent gives her candy (to quiet her down), that is a parsimonious explanation because it only makes one assumption – that the parent gives the child candy – and that assumption can be tested because it points to an observable, physical event – giving candy. Of course, just because an explanation is parsimonious it does not mean that it is correct. But scientists are urged to exhaust parsimonious explanations before moving on to other, less parsimonious, ones.

Parsimony, from the Latin *parcere*, meaning to spare, has been a staple of scientific thinking since at least Sir Isaac Newton. Newton's first rule in his four "Rules of Reasoning in Philosophy" from the third book of his *Principia Mathematica* (1846), originally

published in 1687, is: "We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances" (p. 384). The principle of parsimony has undergone several iterations throughout history. For example, even before Newton's first rule of reasoning in philosophy, the 14th-century logician and Franciscan friar William of Ockham, is said to have stated that "entities should not be multiplied unnecessarily," now known more simply as Occam's razor. The law of parsimony got its name from the Irish mathematician and astronomer Sir William Rowan Hamilton (who also gave Occam's razor its name). Hamilton wrote that:

> The law of parsimony … prohibits, without a proven necessity, the multiplication of entities, powers, principles, or causes; above all, the postulation of an unknown force where a known impotence can account for the phenomenon. We are, therefore, entitled to apply "Occam's razor" to this theory of causality… (1856, p. 580).

The principle of parsimony was again stated by the comparative psychologist Lloyd Morgan who wrote, "in no case may we interpret an action as the outcome of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale" (Morgan, 1894, p. 53).

Karin-D'Arcy (2005) put all of this into perspective concerning psychology and developmental psychology when she wrote:

> Simply put, Occam's razor and Hamilton's law of parsimony counsel that when explaining a metaphysical or natural phenomenon, one should take care to not postulate a theoretical entity that need not exist. These principles, which are the philosophical parent and grandparent of Morgan's canon, are useful in every field of science and philosophy. Morgan's adaptation of these principles is specifically addressed to comparative psychologists, and is applicable across cognitive and developmental psychology research pursuits. (p. 180)

Despite the history of parsimony both outside and within psychology, in recent years several philosophers and scientists have debated its utility or have argued that it may be overrated, especially as it relates to extending human characteristics to nonhuman animals (e.g., Dacey, 2016; Epstein, 1984; Karin-D'Arcy, 2005; Montminy, 2005; Sober, 2012; Zentall, 2018).

Notwithstanding the historical and ongoing debates about parsimony and Morgan's canon, if we accept the job of explaining behavior of human or nonhuman animals, that is, identifying its causes, then we must posit causes that are physical and, as much as possible, observable. To that end, behavior analysis is in an excellent position to offer more parsimonious explanations than cognitive psychology because behavior-analytic explanations point to observable or potentially observable environmental events. When events are observable, they are directly testable. The next question, then, is how to test potential explanations, and, once again, we witness significant differences between behavior-analytic and traditional developmental approaches.

EXPERIMENTS IN DEVELOPMENTAL PSYCHOLOGY VS. BEHAVIOR ANALYSIS

In psychology experiments, different values of an independent variable (IV) are manipulated, and the effects are observed on one or more dependent variables (DV). Typically, the independent variables are some type of environmental manipulation, and the dependent variable is the behavior of individuals, even if the stated variable of interest is some

cognitive process. In psychology in general, and in developmental psychology in particular, it is standard practice to use between-subjects designs, which are more often called (although not technically accurate) group designs. Very generally, in such a design, participants are assigned randomly to conditions representing different values of the IV.

Consider, for example, an experiment in which there are five different values of the IV. In such a case, the minimum number of participants would be five, one for each value of the IV. If one were to run the experiment with the minimum number of participants, however, no firm conclusions could be drawn about the effects of the values of the IV on the DV because each participant is different, and those differences could have contributed to the results. The solution, as every undergraduate psychology major knows, is to add participants in order to minimize the effects of individual differences on the results.

The reason such designs are referred to as group designs is because now each value of the IV is associated with a group of participants. One problem with such a design is that the performances of the participants in each group must now be averaged, and a statistical test must be used to determine the degree to which the different values of the IV may have been responsible for the results. This exercise, however, raises another problem, namely the design does not and cannot really allow the researchers to conclude that the values of the IV were directly responsible for any changes in the DV. All that can really be said is that it is very likely (e.g., $p < .05$ or .01) that the results were not produced by chance. This is a serious problem for the internal validity of the experiment because nothing can be said about the effects of the IVs on the behavior of any one individual.

These problems can be solved quite easily by incorporating a within-subjects design, sometimes also called a single-subject design (although that moniker is misleading in that it is sometimes interpreted as one subject) or a single-case design (which incorrectly implies that it is a case study). Although the minimum number of participants in a within-subjects design is one, the maximum number is infinite. One important feature of within-subjects designs is repeated measures in which each participant is exposed to each value of the IV. So, in our hypothetical experiment with five values of the IV, each participant would be exposed to all five values, usually in a different order. According to Normand (2016), "repeated measures of an individual's performance should constitute the relevant 'population' – a population of representative individual performance measures. For internal validity, having representative samples of performances is more important than having a representative sample of a population" (p. 1).

Perhaps the most important feature of a within-subjects design, however, is what can be said about the effects of the IV on the DV. Recall that, in a between-subjects design, no strong conclusions about cause and effect can be made. In other words, the internal validity is low. In a within-subjects experiment, however, the internal validity is much higher because researchers can see the direct effects of the values of the IV on the DV, and those effects are or can be replicated within the same experiment.

For example, in an experiment with two values of the IV, say either on or off, a simple design would be a withdrawal or ABAB design in which there is a baseline (A) condition in which the IV is not present, followed by a condition (B) in which the IV is present, followed by a return to baseline (A) and again by the condition with the IV (B). Notice that the participant's performance has been repeatedly measured in four conditions. Another advantage of such a design is that replication – the hallmark of experimentation – is built into the experiment. In

particular, the participant's performance in both A and B is replicated. Moreover, we could continue to expose the participant to each condition as many times as we wished, and if the performance in each condition matches that in the previous similar conditions, our confidence of experimental control and internal validity is enhanced. We could then, of course, perform the same experiment with as many participants as we like. At the very least, we would have several individual experiments with high internal validity; and if the performances between participants are similar, we would also have achieved some degree of external validity.

The standard experimental design employed by developmental researchers is technically a between-subjects design because many participants are initially assigned to each IV value. The scores or quantitative measures of the participants' performances are averaged, and one of many possible statistical tests are employed to assess the significance of the results. The results are often displayed not in graphic form but in tabular form with means and standard deviations, which reflects the statistical analysis but does not allow for visual analysis of the data. The biggest problem with such an approach, however, other than the fact previously mentioned – that the internal validity is low – is that there is no accounting for between-subject variability: the greater the range of scores, the greater the inter-subject variability. The design cannot explain why individual participants performed the way they did, or why some did not perform at all. Moreover, when between-subject experiments in developmental psychology are replicated using within-subjects designs, the results are often quite different (e.g., Nighbor, Kohn, Normand, & Schlinger, 2017).

The advantage of experiments in which the control of independent variables has been maximized and in which variability has been minimized as much as possible is that orderliness in behavior can be revealed. When such orderliness can be replicated, it is possible to discover not only individual functional (i.e., cause-and-effect) relations (between values of the IVs and DVs), but also laws or principles. This is how inductive science proceeds. And once laws have been discovered inductively, a theory emerges in which it is possible to go beyond the individual instances comprising the functional relations and extrapolate those laws to novel instances. This is what has happened in the science of behavior analysis.

WHAT IS A BEHAVIOR-ANALYTIC THEORY OF DEVELOPMENT?

Elsewhere (e.g., Schlinger, 1992, 1995), I have written that theory in science and in behavior analysis is an inductive process in which the discovery of scientific facts (i.e., orderly, repeatable functional relations) forms the foundation of scientific laws and theories. Each level (facts, laws, and theories) is an empirical generalization derived from and based on the previous level.

Consider the concept of reinforcement. It began as an observation of a repeatable functional relation with a few animals from a single species and was quickly expanded experimentally across a range of species – including humans – and settings. And many of its parameters were identified. The law of reinforcement is simply a summary of those observations, which suggests that the law is universal. Behavior-analytic theory incorporates the principle of reinforcement with other principles that have been derived inductively in a similar manner (e.g., punishment, extinction, discrimination, and generalization). Behavior-analytic theory is based on the discovery of a functional unit of analysis – the four-term contingency. This functional unit of analysis is unique in psychology and distinguishes behavior analysis from cognitive (and developmental) psychology (Schlinger, 1995; Sidman, 1986; Zeiler, 1986).

Such a unit even permits a better understanding of the structure of behavior. As Branch (1977) put it:

> The crux of the issue is not whether units can have structure; they do. The important question is, to what is the structure due? A structural account points to structural aspects of behavior … whereas a functional account will emphasize the role of manipulable variables in the formation of units. (p. 172)

It is this analytic unit and behavioral laws that permit behavior analysts to apply their theory to a wide range of behavioral phenomena.

Applying Behavior-Analytic Theory

Behavior-analytic theory has been applied in at least two ways. First, applied behavior analysis (ABA) has been used to ameliorate a wide range of behavioral problems in children and adults, as well as in nonhuman animals. The success of ABA is based on the experimental, or functional, analysis of the behaviors to be changed (see Schlinger, 2017b). As discussed above, this experimental analysis distinguishes behavior analysis from other branches of psychology, including developmental psychology, because an experimental behavior analysis can reveal direct cause-and-effect relations that can be replicated within subjects. Moreover, one measure of the superiority of one experimental and theoretical approach over another is the extent to which either can engender technologies. In psychology, we can judge the value of a theory, in part, based on whether it can generate a successful technology of behavior change.

An area in which a behavior-analytic theory has proven to be superior to other approaches is the education and treatment of children diagnosed with autism and related disorders. A number of outcome studies, including meta analyses, have documented not only the effectiveness of early, intensive behavior-analytic interventions (e.g., Eikeseth, Smith, Jahr, & Eldevik, 2002; Eldevik et al., 2009; Sallows & Graupner, 2005), but of their superiority over other approaches (e.g., Howard, Sparkman, Cohen, Green, & Stanislaw, 2005; Howard, Stanislaw, Green, Sparkman, & Cohen, 2014). The multi-year outcome studies by Howard and her colleagues comparing intensive behavior-analytic interventions to more eclectic intensive approaches showed that the behavior-analytic interventions produced huge gains on a variety of cognitive and intellectual assessments. As Howard, Stanislaw, Green, Sparkman e Cohen (2014) wrote:

> At their final assessment, children who received IBT were more than twice as likely to score in the normal range on measures of cognitive, language, and adaptive functioning than were children who received either form of eclectic intervention. Significantly more children in the IBT group than in the other two groups had IQ, language, and adaptive behavior test scores that increased by at least one standard deviation from intake to final assessment. (p. 3326)

The fact that intensive behavior-analytic interventions produce significant gains in language is consistent with other research showing the effectiveness of the application of a behavior-analytic theory to teaching language – based largely on Skinner's (1957) analysis of verbal behavior – to children with language deficits (see Carr & Miguel, 2013; LaFrance & Miguel, 2014).

Behavior-analytic theory has also been used to explain a variety of phenomena that have either not been experimentally analyzed or for some reason are not at present amenable to experimental analysis, including many that go by cognitive names, such as memory (Palmer, 1991), cognition

(Palmer, 2003), auditory imagining (Schlinger, 2009), consciousness (Schlinger, 2008, 2009c), and, of course, infant and child development (e.g., Bijou, 1976; Bijou & Baer, 1978; Gewirtz & Peláez-Nogueras, 1992; Schlinger, 1992, 1995). In all of these instances, the theoretical approach still represents a functional analysis (see Schlinger & Normand, 2015), but not necessarily an experimental one. Previously, I have provided much more detail about how the critical thinking strategies described above, and a behavior-analytic theory, may be applied to the conceptual understanding of infant and child behavior development (Schlinger, 1992, 1993, 1995, 2002). In short, I have suggested that, to better understand development in childhood, one may employ the functional behavior-analytic unit to explain the behavior observed by developmental psychologists. I have argued that such an approach is parsimonious in that it makes fewer assumptions than traditional developmental theoretical approaches because the explanations are described in terms of physical (environmental) events that can be directly and experimentally tested.

Finally, in addition to applying behavior-analytic theory to changing behavior and to understanding novel or untested behavior, behavior-analytic theory may also be helpful in analyzing traditional terms and concepts in the developmental literature (e.g., maturation, developmental stages, etc.). On the one hand, they may be analyzed at face value as scientific concepts. When this is done, however, these and similar concepts reveal their significant shortcomings, as has been noted (e.g., Bijou & Baer, 1978; Lipsitt, 1981). I have tried to show in the current paper that these and similar concepts are subject to many logical errors, such as nominal fallacy, circular reasoning, and reification and, hence, are less than parsimonious.

On the other hand, these concepts may be interpreted in terms of the behaviors said to evoke them as instances of verbal behavior (Schlinger, 2002). This approach to understanding traditional psychological terms and concepts as verbal behavior was suggested by Skinner (1945), and seconded by me (Schlinger, 2013); in other words, the terms can be viewed as verbal responses that can be analyzed in terms of the observable behavioral variables that evoke them. Such an approach has an unintended benefit: by determining the variables responsible for evoking developmental terms as verbal responses by developmental psychologists, behavior analysts can establish that the same variables may evoke different ways of talking about them. For example, previously I mentioned how the Piagetian concept of schema is similar to the concept of the operant, at least based on how Piagetian psychologists talk about schemas. To the extent that the verbal behavior of behavior analysts and developmental psychologists is controlled by similar observations, both groups of scientists may be able to interact more easily with each other toward the common goal of understanding the behavior of children.

A FINAL WORD

I have described the benefits of teaching certain critical thinking repertoires to students of psychology and behavior analysis. But there is a possibly larger issue here, namely that, once students learn these critical thinking strategies, they can apply them beyond the realm of psychology and behavior to other and, potentially, bigger issues. For example, there is a lot of information available in books, magazine articles, and on the Internet on how children develop and on strategies for effective parenting and teaching. But a lot of this information suffers from the same lack of critical thinking as the traditional developmental literature; that is, frequent

mention is made of unobservable constructs (e.g., self-esteem, ToM), explanations are usually circular, and, when research is cited, it is often non-experimental. Moreover, strategies for changing behavior, as in suggestions for parenting or teaching, are not based on sound science. Applying the critical thinking strategies discussed in this article can better prepare students of psychology and behavior analysis to be more discerning consumers of such information.

Another example of how learning the critical thinking strategies described in this article can be important is that there are currently a number of areas where anti-science attitudes are becoming popular, for example, childhood vaccinations and climate change. There is even a resurgence in the number of people who believe that the earth is flat! The scientific consensus on these issues is clear and unequivocal. Nonetheless, anti-science views regarding these topics are becoming more prevalent and to the extent that such views influence public policy, they have the potential to cause great harm. Once critical thinking strategies are taught to students of psychology and behavior analysis, such repertoires can hopefully generalize to broader issues. And if we can teach these strategies to elementary school children along with what science is and how scientists come to their conclusions, then perhaps we can stem the tide of anti-science thinking and move to a world where science and reason prevail. Moreover, children can be taught at an earlier age how to think scientifically not just about the physical world, but about behavior as well. Finally, a functional behavior analysis may be able to help us understand the causes of such illogical thinking in the first place.

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