

IN THE ANALYSIS OF BEHAVIOR, WHAT DOES "DEVELOP" MEAN?

**EN EL ANÁLISIS DEL COMPORTAMIENTO,
¿QUÉ SIGNIFICA "DESARROLLO"?**

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ABSTRACT

The basic fact generating developmental psychologies is that across the life span, much behavior change seems orderly and predictable. If that is true, it allows the inference that this change is systematic. The question then becomes, what is the system? Most developmental psychologies posit a teleological endogenous system, in the form of a global goal or outcome toward which this behavior change is supposedly directed. The value of these constructs is that they explain the changes, not that they might be true. Behavior analysis is at least a compilation of very effective behavior-change mechanisms, almost entirely in the form of environmental contingencies, most of which are experimentally manageable. That class of facts allows the emergence of a null hypothesis, in the form of a question: How many of the orderly, predictable behavior changes labelled "developmental" are factually explainable as orderly, predictable applications of the well-known behavior-analytic mechanisms at those points in the life span? To the extent that this question is answered Yes, to that extent the problem of explaining "development" becomes a problem in sociology: Why does society arrange those mechanisms that are predictable at those times?

Key words: development, behavior analysis, environmental contingencies, mechanisms

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RESUMEN

El hecho básico que fundamenta la psicología del desarrollo es que, a través de la vida, muchos de los cambios conductuales parecen ordenados y predecibles. Si esto es verdad, es posible inferir que el cambio es sistemático. Así, la pregunta se convierte en ¿cuál es el sistema? La mayoría de las corrientes psicológicas del desarrollo proponen mecanismos teleológicos endógenos, ya sea en la forma de un objetivo global o de acuerdo con un resultado hacia el cual los cambios conductuales están supuestamente dirigidos. El valor de estos supuestos consiste en que explican los cambios, no en que sean verdaderos. El análisis conductual es una compilación de mecanismos muy efectivos para el cambio conductual, casi enteramente expresados en términos de contingencias de reforzamiento, la mayoría de las cuales se pueden manipular experimentalmente. Esta clase de hechos permiten una hipótesis nula, planteada en la siguiente pregunta: ¿cuántos de los cambios conductuales ordenados y predecibles, llamados "cambios del desarrollo," en efecto se explican mediante la aplicación ordenada, durante períodos específicos, de mecanismos conductuales suficientemente conocidos? Si la respuesta a la pregunta anterior es afirmativa, esto convierte el problema de explicar el "desarrollo" en un problema sociológico: ¿por qué la sociedad arregla que tales mecanismos ocurran en momentos específicos?

Palabras clave: desarrollo, análisis conductual, contingencias del entorno, mecanismos

The concept of development is an old one in behavioral science. Indeed, Developmental Psychology was the seventh division of the American Psychological Association, which "developed" into 45 more divisions. To be seventh in a sequence of 52 implies the concept is basic (or that children are important), and the current size and complexity of the division imply the concept is durable (or that children are important).

The development concept is also extraordinarily complex (Reese, 1991). But the basic phenomena that evoke it are simple enough: Over the life span of most of the organisms we have studied, behavior, structure, physiology, and biochemistry seem to change in orderly, predictable, and therefore, we suppose, systematic ways (Bijou & Baer, 1961). Describing that breadth of change in detail creates a very large taxonomy. Relating the details of that change to age creates a very large epidemiological science; and indeed, those details do relate very well to age. Stating what happens as a function of age is also stating it as a matter of sequences; and indeed, very many of the details of development can be stated as sequences (and since sequences take time, they have some correlation with age). So, if variance-accounted-for were the end-goal of science, stating all forms of organismic development as a function

of age or as a matter of sequences would be a scientific triumph, not just an exercise in epidemiology.

Developmental psychologists, however, tend to go beyond description, and try to explain why that development relates so thoroughly to age; or why it so often comes in those apparently invariant sequences; or why those sequences sometimes are matters of not just one behavior changing, but large classes of behavior changing together. Then something quite different from a triumph emerges. Put pessimistically, no explanation has succeeded very well; put optimistically, many quite different explanations have succeeded equally well. Among the many alternative explanations of development, stage theories are seen as the "lesser evil" (Siegler, 1992). To explain why behavior changes at particular ages, stage theories postulate developing organizations of the mind that enable behavior change at those ages. Each stage of development represents an internal change in the child. How this internal change produces the change in behavior, and what caused the internal change itself, are often unclear. When they are unclear, the problem of explaining development has produced not an explanation, but the invention of a different level at which explanation is needed, and at which its absence may not be noticed.

Do We Need Teleological Explanations?

Explanations sometimes tend to postulate an end point to development as if that explained it--as if teleology were a respectable form of explanation (cf. Overton & Reese, 1973; Spiker, 1966). Teleologically, development is whatever it takes to reach that end point. But the end point has prerequisites, and the prerequisites have prerequisites, which in turn have prerequisites, etc. That is why we see dependable sequences, which are so lengthy that age becomes a reasonable unit of time. The problem then is to specify an end point such that all the sequential, age-related details of development can be seen as how to get to just that end point. This is obviously difficult. The end points offered, in trying to explain so much, tend to be global and vague (Rosales-Ruiz & Baer, 1996). Candidates such as "complexity" are nominated, but such concepts are quite likely to be embarrassed by one or another part of the dependably observable details of development that happen not to fit that concept. Too much of that embarrassment engenders a more contextual solution: to specify numerous end points, each designed to explain only some respectably interesting domain within the totality of development (e.g., Siegler, 1981; cf. also reviews by Flavell, 1982, 1992; Siegel, 1991; Siegler, 1989). That strategy tends to weaken the concept of development a little more with each subdivision found to require its own special explanation. "Development" devolves into "developments." The ultimate case, in which every detail is

explained by a different mechanism, approaches the ultimate destruction of the development concept as such--depending on how different is "different."

Whatever their focus or breadth, these theories still explain behavior change in terms of other, hypothetical changes within the organism. For them, what develops is not behavior, but an innate capacity of the organism. Behavior is seen as the product of that changed capacity. Thus, these theories claim that what children know, regardless of their environment, is sufficient to predict when behavior change should occur. Yet the only available evidence is that behavior (including knowledge), structure, physiology, and biochemistry seem to change in an orderly, predictable, and systematic way across the life span of most organisms. When explanations of those changes must be made empirical, and the empirically demonstrable explanations tend to look different in different cases, rather than systematically alike across them all, we have a superb irony: An apparently cohesive process needs explanation; abundant cohesive theories arise, but the provable explanations of various parts of that process do not seem cohesive. That leaves developmental psychology with a great deal of empirical knowledge that behavior changes, apparently explanatory mechanisms that cannot be empirically tested (e.g., equilibrium, Piaget, 1975), and an explanatory logic that assumes one given change leads to another until a final point is reached.

Unlike those approaches, behavior analysis looks at the relation between the environment and the behavior of the organism to account for behavior change in any sequence. Suppose behavior really does change systematically across the life span. Behavior analysis knows a good deal about how to change behavior. The mechanisms it knows are the environmental contingencies: the relations among a behavior, its consequences, and its antecedents; and the past history of those relations and relations physically like them (stimulus and response generalization) or made like them (functional classes and equivalence classes) (cf. Baum, 1994). This knowledge does not establish that there are no other ways to change behavior, it only proves that these ways are dramatically powerful and general. As best we know, they are equally powerful and general at almost all points in the life span. Thus, if we rely on these ways to explain orderly, predictable, and systematic behavior change across the life span, we must suppose that not these ways, but the application of these ways, changes in a correspondingly orderly, predictable, and systematic way across the life span.

Orderly, Predictable, and Systematic Contingencies

How could the application of the environmental contingencies differ in an orderly, predictable, and systematic way across the life span? Perhaps the

power of some consequences to reinforce or punish varies in an orderly, predictable, and systematic way across the life span. Perhaps the discriminability of some antecedents does. And, perhaps most powerful of all, culture changes those contingencies in an orderly, predictable, and systematic way across the life span. For example, perhaps culture teaches parents, teachers, and peers that certain responses of children (and adults) should be taught only at certain ages and in certain sequences; perhaps culture teaches them to use different reinforcers, punishers, and antecedents to do that, depending on the child's age or the behavior's place in some sequence; perhaps culture tells them when to rely on old antecedents and consequences and when to condition new ones; perhaps culture tells them when to rely most on consequences and when to rely most on antecedents--when and what to reward, punish, or extinguish, and when and what to explain.

To whatever extent this explains what is orderly, predictable, and systematic in behavioral development, then the proper study of behavioral development is not so much the behavior of the person being taught, but the behavior of the people doing the teaching. That is, if development is behavior change, and if it is to be explained by the principles of behavior management--which are so dramatically effective at behavior change--and if the behavior called developmental is orderly, predictable, and systematic, we might well look for a corresponding order, predictability, and system in how the behavior-management techniques are managed by whoever and whatever manages them.

Affirming the Consequent: A Serious Error

If our only tool is a hammer, then all problems are nails. If development is orderly behavior change, and if the only way we explain behavior change is by behavior management, then the responsible behavior management must be as orderly as the behavior change. Therefore, we should study primarily the development not of behavior but of behavior management across the life span. We should also acknowledge that in doing so we are affirming the consequent, a serious error in logic. (Behavior management changes behavior; therefore, if behavior changes, it must be behavior management doing it. Logical? No. True? Sometimes, sometimes not.) Still, the logical error of affirming the consequent is often made quite deliberately in science, because while it certainly is an error in logic, it need not always be one in fact.

The question is, do the facts support affirming the consequent, even if it is illogical? If we observe the development of behavior management in the natural environment, and find any powerful relations between age and sequence and the behavior-management techniques usually applied, we would have a

correlational endorsement of the thesis that behavior develops in an orderly way across the life span because it is taught in an orderly way across the life span.

Unfortunately, being correlational, it would also be just as good an endorsement of the opposite thesis that: behavior-management techniques change in that orderly way because behavior and behavior-changeability change in just that orderly way. Perhaps it is, after all, still the development of behavior that we must study, because perhaps it is the development of behavior that drives what management techniques will be applied to it.

Then why not make the study experimental, to see if we can avoid one of those two distressingly opposite interpretations (both of which are true sometimes)? Ethics, especially for the human case, prohibit much of that kind of experimental analysis. When we take experimental control of the behavior-management techniques to be applied to someone's case (especially a child's case), it is to remediate a serious problem, not to clarify some theories of development. Experimental control of how real-world people will manage trivial or inconsequential behaviors is always acceptable, of course. But control of trivial behaviors is correspondingly trivial proof that one view of development is better than another. We will not test experimentally and in real-world people the possibility that important, serious, and valuable behaviors develop in an orderly, predictable, and systematic way across the life span only because culture moves us to teach them in a correspondingly orderly, predictable, and systematic way across that life span. We cannot take experimental control of what amounts to a lifetime of a lifestyle of how to teach what to whom. (Lest this be interpreted as a statement of regret, we affirm here that we would not want to live in or allow a society in which that possibility could be tested experimentally.)

Indeed, this problem of proof pervades the developmental issue. The essential fact necessitating the concept is that whole, coherent classes of behavior change in an orderly, predictable, and therefore presumably systematic way across the life span; we wish to know the system at work. But, apart from theories that say it must be, what is our proof that behavior change is indeed orderly and predictable across the life span? The simplest assertion is the pervasive correlation of behavior change with age. Behavior analysis, however, has too often changed behaviors at ages much too early, according to the simplistic timetables that once made up the introductory texts of the field. That allows a presumption: The fact that so much behavior change is correlated with age may testify to little more than an absence of systematic attempts to undo that correlation with the most powerful behavior-analytic tools available for that job.

Are Stages Necessary to Explain Development?

A more sophisticated set of facts is the frequent appearance of uniform sequences of behavior change, indeed, sequences of change of unified classes of behavior, called stages, only loosely tied to age. The argument is that the uniformity of these stage sequences, and their size, may testify to their necessity; that necessity is the key fact of development, even if not yet the explanation of it. In effect, necessary sequence means that each behavior, or each stage, in that sequence is somehow a prerequisite for the next. But how can we prove that a sequence is a necessary one? Again, the uniformity of the sequence, obvious to naturalistic observation, may testify to little more than a lack of systematic attempts to change the sequence with the most powerful behavior-analytic tools. To *prove* the sequence is necessary, we must show that the second behavior or class never emerges until the first has appeared. That is, we must have tried *every* conceivable way to make the second behavior or class appear in the absence of the first, and have failed. In the days when we knew little about shaping and diverse task analyses, that might have seemed a feasible criterion. Today, there are simply too many conceivable ways of making the second behavior or class appear in the absence of the first, to ever say that we have tried them all, and that they all have failed.

Thus, the assertion that any sequence is a necessary sequence is not provable. But, to prove that a sequence is not necessary, we need only show once that we can make the second behavior or class appear in the absence of the first. That transforms the question from, "Is this a necessary sequence?" to, "Under what conditions does the emergence or shaping of the first behavior or class make the emergence or shaping of the second more probable, and under what conditions does it not?" We may discover that the conditions under which the presence of the first behavior or class maximizes the probability of the prompt emergence of the second are more common than the conditions under which the presence of the first behavior does not maximize the probability of the prompt emergence of the second; that will explain why we thought the sequence was a necessary sequence. But it will also continue to affirm that the sequence is not necessary.

Similarly, we may well ask how unified are the behaviors that together make up a stage? The classing of behaviors so that they covary as a class when change operations are applied to only some of them is a well known problem in behavior analysis; and the removal of behaviors from such a class is also a well known problem (cf. Baer, 1982). As a result, we should think about classes, and therefore about the unity of the covarying behaviors that define stages, as provisional: We know that we can make and unmake some

classes at will; that leaves the possibility that we can make and unmake any of them, if it were ethical and if we had the time and environmental control it would require. Thus stage, like sequence, is a fragile, vulnerable concept, because it is based on a fragile, vulnerable set of behavioral covariations.

CONCLUSION

In our opinion, the essence of behavior analysis is its derivation from and steady if not perfect dependence on proof through experimental analysis. To a remarkable extent, as behavioral sciences go, behavior analysis has been a proof-driven enterprise. To a considerable extent, as just argued, the questions of behavioral development are not suitable for experimental analysis. Too much of development will remain a problem for only conceptual analysis, not experimental analysis. (See Skinner's 1957 book, *Verbal Behavior*, to consider language acquisition as a case in point). Thus it can be concluded that the analysis of behavioral development is not a perfect behavior-analytic problem. Two recent texts (Novak, 1996; Schlinger, 1995) show admirably that behavior-analytic principles can be applied as well as cognitive, dynamic, or genetic ones. They cannot show proof, however, that the behavior-analytic principles are the best of them, should any student-reader value proof.

From the point of view of proof, it is viable and apparently fruitful to question the extent to which behavior does in fact change in an orderly, predictable, and thus presumably systematic manner across the life span. Exceptions have been found, and surely more will, one after another. The question is how far that will go--how much order and prediction will be left after well informed, energetic, programmatic attempts at undoing that orderly predictability have been pursued at length. It is possible to prove that there is less order and predictability than seems true at present. We may not prove that, of course, but it is possible. By contrast, it is not possible to prove that behavior changes in an orderly, predictable way across the life span; that is only a disprovable, not a provable, proposition. To defend the concept of development, we can only, at any moment, point to the behavior changes that have not yet been taken under enough experimental control to change their apparent order and predictability across the life span. That is a terribly vulnerable definition of development.

An astonishingly arrogant question can be imagined: If we knew enough about managing behavior, would we still think that in addition to being managed, it also did something called "develop?" We will know more and more about behavior management, so a less arrogant question arises as well: Will

we ever think the first question was not so arrogant after all? The fruitful course is to proceed as if to find out.

REFERENCES

- Baer, D. M. (1982). The imposition of structure on behavior, and the demolition of behavioral structures. In D. W. Bernstein (Ed.), *Response structure and organization: Nebraska symposium on motivation* (pp. 217-254). Lincoln, NE: University of Nebraska Press.
- Baum, W. (1994). *Understanding behaviorism: Science, behavior, and culture*. New York: Harper Collins.
- Bijou, S. W., & Baer, D. M. (1961). *Child Development I: A systematic and empirical theory*. New York: Appleton-Century-Crofts.
- Flavell, J. H. (1982). Structures, stages, and sequences in cognitive development. In W. A. Collins (Ed.), *The concept of development: The Minnesota symposium on child psychology* (pp. 1-28). Hillsdale, NJ: Erlbaum.
- Flavell, J. H. (1992). Cognitive development: Past, present, and future. *Developmental Psychology*, 28, 998-1005.
- Novak, G. (1996). *Developmental psychology: Dynamical systems and behavior analysis*. Reno, NV: Context Press.
- Overton, W., & Reese, H. W. (1973). Models of development: Methodological implications. In J. R. Nesselroade & H. W. Reese (Eds.), *Life-span developmental psychology: Methodological issues* (pp. 65-86). New York: Academic Press.
- Piaget, J. (1975). *The development of thought: Equilibration of cognitive structures*. Oxford: Blackwell.
- Reese, H. W. (1991). Contextualism and developmental psychology. In H. W. Reese (Ed.), *Advances in child development and behavior* (Vol. 23, pp. 187-230). New York: Academic Press.
- Rosales-Ruiz, J., & Baer, D. M. (1996). A behavior-analytic view of development. In E. Ribes & S. W. Bijou (Eds.), *Recent approaches to behavioral development*. Reno, NV: Context Press.
- Schlinger, H. (1995). *A behavior analytic view of child development*. New York: Plenum Press.
- Siegel, L. (1991). On the maturation of developmental psychology. In F. S. Kessel, M. H. Bornstein, & A. J. Sameroff (Eds.), *Contemporary constructions of the child: Essays in honor of William Kessen* (pp. 251-264). Hillsdale, NJ: Erlbaum.
- Siegler, R. (1981). Developmental sequences within and between concepts. *Monographs of the Society for Research in Child Development*, 46, 1-13.
- Siegler, R. (1989). Mechanisms of cognitive development. *Annual Review of Psychology*, 40, 353-379.
- Siegler, R. (1992). What do developmental psychologists really want? In M. R. Gunnar & M. Maratsos (Eds.), *Modularity and constraints in language and cognition*:

The Minnesota symposia on child psychology (Vol. 25, pp. 221-232). Hillsdale, NJ: Erlbaum.

Skinner, B. F. (1957). *Verbal behavior*. Englewood Cliffs, NJ: Prentice-Hall.

Spiker, C. C. (1966). The concept of development: Relevant and irrelevant issues. In H. W. Stevenson (Ed.), *Concept of development: A report of a conference commemorating the fortieth anniversary of the Institute of Child Development, University of Minnesota* (pp. 40-54). *Monographs of the Society for Research in Child Development*, 31 (5, Serial No. 107).