# PARADIGMATIC BEHAVIORISM'S THEORY OF INTELLIGENCE: A THIRD-GENERATION APPROACH TO COGNITION

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## RESUMEN

*Teoría de la inteligencia del behaviorismo paradigmático: Un enfoque de la tercera generación sobre la cognición.-* El presente artículo señala algunas deficiencias en los análisis conceptual y conductual anteriores, de los complejos fenómenos «cognitivos». Luego se describe el enfoque del behaviorismo paradigmático al estudio de dichos fenómenos complejos. El método representa una estrecha e interactiva mezcla del análisis conceptual en desarrollo y de un programa empírico avanzado. Los principios conductuales son valorados, en primer lugar, con muestras de partes del complejo fenómeno conductual. La investigación progresivamente avanza hacia el manejo de muestras cada vez más complejas e inclusivas. Son diversos los métodos de investigación implicados, desde los reversibles de refuerzo de caso único hasta los diseños de grupo y de utilización de test psicométricos. La metodología central está integrada por una *investigación experimental-longitudinal* que implica el entrenamiento de los sujetos en repertorios funcionales durante largos periodos de tiempo, en donde el aprendizaje se detalla mediante el registro de cada estímulo, cada respuesta y cada refuerzo presente. El análisis conceptual también se ha desarrollado desde lo simple hasta lo complejo, introduciendo conceptos del tipo de *repertorio conductual básico* y *aprendizaje acumulativo-jerárquico*, asumiendo que la compleja conducta humana es aprendida en un proceso de adquisición de repertorios simples que constituyen la base de un aprendizaje más complejo que, a su vez, sirve de base para aprendizajes todavía más complejos en repetidos pasos de adquisición. Esta metodología y análisis conceptual constituyen el fundamento para una teoría de la inteligencia que es behaviorista y experimental, y que especifica qué es inteligencia, cómo se aprende, cómo funciona, cómo se justifica conductualmente su medida y cómo puede ser manipulada mediante procedimientos de intervención. La teoría también sirve de base para tender un puente entre el conocimiento behaviorista y el tradicional, proporcionando de este modo un modelo metodológico para resolver el cisma existente entre el behaviorismo radical y la psicología tradicional.

**Palabras clave:** Inteligencia. Conductismo paradigmático.

## ABSTRACT

The present paper indicates some of the shortcomings in previous conceptual, behavioral analyses of complex «cognitive» phenomena. It then describes the approach of paradigmatic behaviorism in studying such complex phenomena. The method involves a close, interacting mix of developing conceptual analysis and an advancing empirical program. Behavior principles are first tested with samples of parts of the complex behavioral phenomenon. The research moves progressively towards dealing with more and

more complex and inclusive samples. Various research methods are involved ranging from single subject reinforcement reversals to group designs and the use of psychometric tests. A central methodology consists of *experimental-longitudinal research* that involves training subjects in functional repertoires over long periods of time where the learning is stipulated by recording every stimulus, every response, and every reinforcer involved. The conceptual analysis has also been developed from the simple to the complex, introducing concepts such as the *basic behavioral repertoire* and *cumulative-hierarchical learning*, recognizing that complex human behavior is learned in a process of first acquiring simple repertoires that form the basis for more complex learning that form the basis for yet more complex learning in repeated steps of acquisition. This methodology and conceptual analysis provide the basis for a theory of intelligence that is behavioristic and experimental and that stipulates what intelligence is, how it is learned, how it functions, how its mesurement is justified behaviorally, and how it can be manipulated in intervention procedures. The theory provides also a basis for establishing a bridge from behavioristic knowledge to traditional knowledge, and in this way provides a model methodology for resolving the radical behaviorist traditional psychology schism.

**Key words:** Intelligence. Paradigmatic behaviorism.

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We are all well aware of the traditional behavioristic position regarding cognitive phenomena. A central part of WATSON's behavioral revolution was the rejection of the varied concerns studied under the rubric of consciousness. The pejorative term «mentalism» has been applied in contemporary times to every type of cognitive term, like personality, attitudes, reasoning, attribution, images, word meaning, intelligence, and so on. The rejection of mentalism was made on sound methodological grounds, at least in certain respects. For example, it is seen that some children do better in school than others and it is said they are more intelligent. Furthermore, this term is then endowed with explanatory powers; it is said that children's performance in school is due to their internal level of intelligence, generally assuming some internal biological difference, even though there are no observations made of such differences. When I wrote my 1963 book I described and exemplified this methodology in detail.

> For a school psychologist to say that Jimmy is constantly fighting because he is a hostile-aggressive child may sound very impressive, but it really adds no new knowledge, for if we ask how one knows that this is a hostile-aggressive child, the reply is that the child is always fighting. The term «hostile-aggressive», then is only another name... It [is circular and] does not explain the behavior (STAATS, 1963, pp. 16).

As was pointed out in this account, the fact that a verbal test can be constructed that will give a measure that predicts the child's fighting behavior may be useful, but it does not provide substantiation for the method of assuming explanation of the behavior by some internal personality process. It was still necessary to make a major point of the methodology of traditional personality theory and personality tests in 1963, and the manner in which this methodology differed from a behavioral explanation.

Today, of course, all behaviorists are familiar with these points. They are still integral to the underlying methodology of behavioral approaches and are evidenced in various activities of research and applied behaviorists. What is typically not realized, however, is that the methodological critique of areas like personality theory and personality testing does not dispose of the questions that these fields have raised concerning the cause and prediction of important types of human behavior. While our behavioral tradition is quite correct in saying, as did WATSON, that «personality is the sum of activities that can be discovered by actual observation of behavior» (1930, p. 274), that simple statement itself provides no explanation, also, of the various pheno-

mena involved. While WATSON said something to the effect that he could make a child into a butcher, a baker, or a candlestick maker, he never did that or explicated how to do so. Critiques, especially when bolstered by conceptual analyses that show how the human process involved can be composed of learned behaviors, strengthens our confidence in behaviorism and disenchants us with traditional psychology. But such critiques and beginning analyses do not constitute systematic programs of study.

It would, of course, be unfair to suggest that the classic behaviorisms never made analyses of complex cognitive or personality topics in straightforward behavioral terms, many of them very valuable. But this work has typically had certain characteristics that has limited them as the bases for programs of empirical work. For example, MOWRER (1954) proposed a theory of word meaning in language that was based upon classical conditioning principles, certainly a behavioristic account. He described communication in terms of this analysis, but the analysis was not stated in terms that led to the development of a general empirical analysis of language communication. Other behaviorists made similar analyses in a variety of topics like reasoning (MALTZMAN, 1955), personality and psychotherapy (DOLLARD and MILLER, 1950), problem solving (JUDSON, COFER, and GELFAND, 1956), concepts (HULL, 1920), abstraction and images (SKINNER, 1953), and so on. None of these established continuing lines of empirical or theoretical work. A recent statement by McPHERSON, BONEM, GREEN, and OSBORNE, concerning SKINNER's analysis of language (1957) makes the point very well.

For whatever reasons, *Verbal Behavior* has not provided a conception that has led to the empirical examination and explanation of verbal behavior. If the past is a predictor of the future there is no reason to suspect that it will eventually do so. However, for the future viability and completeness of behavior analysis as a science of all behavior, verbal behavior research

and the eventual explanation of verbal behavior are necessary, with or without *Verbal Behavior* (1984, p. 165).

These radical behaviorists leave the reason largely unanalyzed. Let me say that I believe it is important in constructing the next generation of behaviorism to recognize the great contributions of what has gone on before. But we also have to consider the reasons why many central areas of concern —to which the previous behaviorists, at least in part, have addressed themselves— have not been productively dealt with to this day in a progressive, building, empirical manner. This is a large topic for consideration —which has been part of the task in developing what has aimed to be a third-generation behaviorism (STAATS, 1957, 1963, 1968a, 1968b, 1970a, 1970b, 1971a, 1972b, 1972, 1974, 1975, 1981, 1983), called paradigmatic (or social) behaviorism. This task involves major changes in the basic learning theory employed, in the elaboration of this theory to deal with human learning, since new principles are involved, in the methodology of making analyses of human behavior problems, in the use of non-behavioral knowledge in this process, and in the empirical methodology that is developed for such analyses.

Let me add that paradigmatic behaviorism had its birth in the context of formulating a behavioristic approach to the comprehensive and detailed study of human behavior. Its contextual focus was not that of formulating a learning theory to fit the multifarious findings of the animal laboratory. The basic learning theories of the second generation had already focused on that mission. Thus, even on the basic learning-theory level paradigmatic behaviorism saw the task in a different light, that of formulating the basic learning theory not only in the context of the animal learning findings but in the context of human level study as well. This perspective yielded a new basic theory. In addition, the systematic empirical efforts of paradigmatic behaviorism focused on analyzing complex human behavior rather than on the animal laboratory. My first formal experimental work,

my dissertation, was entitled «A behavioristic study of human problem solving» (1956). In the process of conducting that analysis I came to recognize that applying conditioning principles directly to such complex phenomena as human problem solving and reasoning or any complex behavior, in the framework of a single experiment, forced one to use a simple conception of that human process. The experimental definition of the human process was generally characterized in a way that it would fit one of the elementary principles of conditioning, whereas in actual life very complex events are involved. But most behavioristic analyses of human phenomena consisted of experimental analogues that, while instructive, were simplified to the point where there was little resemblance to the real human performances involved. This was one reason such analyses generated little continuing empirical or theoretical development. It tended to be thought that the simple analysis in terms of learning principles answered the problem raised by traditional mentalistic approaches. I think we can see that same methodological stance today, for example, in some of the attempts to consider complex processes that are treated by cognitive psychologists with such simple principles as rule-governed behavior. The principles are too simple to be satisfactory in dealing with the complex processes involved. Nevertheless, analyzing some cognitive process with some reasonable behavioral principle seems to satisfy curiosity concerning the process, to remove it as something to be concerned about.

I suggest that these are some of the reasons that the book Verbal Behavior did not lead to systematic empirical study. Another reason was the book was based solely on limited naturalistic observation. The only mentioned attempt at experimentation involved an instrument called the verbal summator, whose value was never shown (SKINNER, 1957). Thus, no empirical implementation was indicated. In addition, while productive word classes were defined in terms of reinforcement principles there was little analysis of the conditions of learning invol-

ved in the acquisition of the classes. It is interesting to note that in this respect the first-generation behavioristic analysis of language did include incipient developments of this kind. That is, WATSON describes some attempts to affect the speech development of an infant through reinforcing it for saying «da». The procedure was to take away the child's bottle and then to say the syllable outloud to the child. If he repeated the sound the bottle was returned to the child. WATSON notes an increase in the frequency of the vocal response. He states that verbal habits are formed through such learning and names two classes of verbal response that I call labels and reading STAATS (1968) and SKINNER (1957) calls tacts and texts. «It is quite obvious in the child of three that the word 'mama' is called out ... by the sight of the mother, ... [and] by the sight of the printed English word 'mother'» (WATSON, 1930, p. 231). This actually provides an interesting example of generational theory advancement. WATSON made a first analysis of language. The second-generation behavioristic analysis of the classes of verbal operants in Verbal Behavior was a great advancement. I will suggest that the same type of advancement lies in the third-generation works of paradigmatic behaviorism. For neither WATSON's original analysis, nor SKINNER's Verbal Behavior established a foundation for continuing research and theoretical elaboration.

One of the new characteristics of paradigmatic behaviorism, that dates from its beginnings in the early 1950s, has been its insistence on advancing its theoretical analyses in such a way that empirical operations are indicated. Central in this has been its concern with considering complex human processes as learned —analyzing their acquisition stages— rather than being concerned only with the manipulation of behavior. Thus, paradigmatic behaviorism has focused on providing the theoretical analysis, the general methodology, an extensive set of empirical works, and specific directions for empirical development of the analysis of complex human behavior, including that which is traditionally considered under

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cognitive and language terms. It was these elements that I began to call behavioral analysis by 1963 —in perhaps the first use of that term, at least as a formally-named methodology (STAATS, 1963, pp. 459-460). As the following statement indicates I was concerned about the usual behavioristic analyses of complex processes based as they were on naturalistic observations.

> In some cases, the behavior of interest may be very poorly specified by prior naturalistic observations. This might occur because the behavior is particularly subtle or complex. Before research can begin to establish the relationship between the determining events and the behavior, systematic observations may be required (STAATS, 1963, pp. 459-460).

The first description of behavior analysis in 1963 was brief but the methodology was elaborated in additional works (see STAATS, 1964). Behavioral analysis in my account was not considered as a specific type of design, but as a continuing, developing strategy of research on particular areas of complex human behavior. The analysis began with systematic naturalistic observations and specific analysis in terms of conditioning principles. It then involved prototypical experiments to test the principles in simplified, laboratory studies of the behavior. The work then progressively moved to more complex samples of the complex behavioral area, involving a progressive advancement along various lines (STAATS, 1964, 1968a, 1977; STAATS and BURNS, 1981; STAATS, BREWER, and GROSS, 1970; STAATS, MINKE, FINLEY, WOLF, and BROOKS, 1964). One of the areas in which this methodology was developed was in the behavioral analysis of reading as a part of language. I first analyzed reading acquisition in the context of reinforcement principles (STAATS and STAATS, 1962). The first empirical studies involved validating reinforcement principles in the context of preschool children learning a small word-reading repertoire (STAATS, STAATS, SCHUTZ, and WOLF, 1962). Another, better controlled experiment in-

vestigated token-reinforcement procedures applied to learning to discriminate and name the letters (STAATS, FINLEY, MINKE, and WOLF, 1964). Problems of remedial learning were studied, beginning in 1958 in work that first introduced the token-reinforcer system (see O'LEARY and DRABMAN, 1971; STAATS and BUTTERFIELD, 1965). Movement toward dealing with the complexity of reading acquisition was commenced in long term, experimental-naturalistic study of individual children learning to read. I worked with my own children, first in infancy, and continuing through childhood (see STAATS, 1968). The methods were then applied formally to other children where behavioral data were gathered on each subject with respect to every stimulus presented and every response made. But the data was also grouped over subjects, and conventional psychological tests of achievement were also employed (STAATS, 1968; STAATS, MINKE, and BUTTS, 1970; STAATS, BREWER, and GROSS, 1970). One of the objectives of the work was to bridge the different methods of obtaining data on human behavior —which was one of the things that the radical behaviorists of the time objected to about paradigmatic behaviorism, although now such usages are accepted by many behaviorists some of whom consider themselves radical.

Well, this is a brief introduction to a body of theory, methodology, research, and application that spans a wide number of areas of study. This framework, in its developing stages, was one of the instigators of modern behavioral psychology. However, in developing characteristics different than those of operantly-oriented radical behaviorism, paradigmatic behaviorism has become rather completely separate. This schism is in the process of being resolved. For example, the Association of Behavior Analysis has invited us to participate in their meeting and we have begun to respond (BURNS, 1985; HEIBY, 1985; LEDUC, 1985; MINKE, 1985; STAATS, 1984, 1985a, 1985b, 1985c; TESSIER, 1985). There is much in common between the two behaviorisms —operant behaviorism and paradigmatic

behaviorism (or whatever therminology the reader wishes to use here)— and elements that could profitably be exchanged. Publication of this paper in this *Journal* will represent another step in establishing avenues of interaction. I cannot in this paper sample the various areas that paradigmatic behaviorism addresses. Instead one area has been selected, to indicate how this approach has dealt with a traditional mentalistic or cognitive area or concern, that of intelligence, an area important in psychology that has not been dealt with very much in radical behaviorism. This has been done to indicate both the substantive work involved as well as the theory that has been formulated. These are important in offering a broad foundation for the conduct of additional theoretical and measurement (or assessment) works, as well as for the conduct of basic and applied research and interventions. These developments are also important in terms of showing traditional psychology that behaviorism can deal productively with complex cognitive phenomena. In these several senses what I am describing represents a general methodology that is characteristic of paradigmatic behaviorism.

## THE BEGINNING CONCEPTION OF INTELLIGENCE

To begin describing the developing nature of the theory that is being presented, let me quote from my 1963 analysis of both intelligence and intelligence tests.

More broadly, it may be suggested that the intelligence behavior in general is learned behavior, largely of a verbal nature, which includes the following: [labels], reasoning sequences, and communication behaviors, reading, arithmetic, and mathematical repertoires; attentional, observing, and discrimination behaviors; and various behaviors under verbal control. Many of these behaviors would constitute a basic repertoire for the preschool child on which further acquisition of beha-

viors important in school would rest. Without the appropriate repertoire, the child would indeed be «retarded» (STAATS, 1963, p. 411).

In this account the basis for the conception of intelligence was the analysis of test items. This analysis considered the different behaviors that the child had to have if he was to successfully respond to each item dealt with. For example, if the child was correctly to answer an item asking for the number of objects displayed, the child had to be able to count the items, or to discriminate and name the number directly —through a training program that was described. Various items were analyzed in these examples. This account gives the nucleus of my general theory of intelligence, but the theory was actually based on more than the analysis of intelligence test items. By 1963 the behavior analysis of intelligence included a decade of research on the types of behaviors that compose what is called intelligence. A word should be said about this study, although it can only be the briefest summary, because there was extensive work involved.

This work began with a simple experimental analogue, that of a cat learning to respond to verbal stimuli such as commands to come when called, to sit, to shake hands, and the like. Animal work on language far surpasses this today, but in 1954 it was significant to see that simple elements of important classes of language behavior could be learned according to straightforward conditioning principles, even when the subject was an animal. This was followed by other manipulations in the naturalistic situation to affect changes in the verbal behavior of humans (see STAATS, 1975, pp. 294-295). In the mid-1950s I began a series of studies of how words could come to elicit emotional responses through classical conditioning (see STAATS, STAATS, and CRAWFORD, 1962) and how emotional words could transfer this emotional property to new stimuli, also through classical conditioning. Because classical conditioning has not been considered within operant theory to be important in the determination of human behavior,

the central significance of this type of learning has not been recognized by many behaviorists even today. The issue is one for separate development, however. It is relevant here to indicate that this was the first systematic experimental study of one of the central language repertoires. Also it may be considered the beginning of experimental (manipulatory) research on functional language repertoires in a behavioristic framework.

In 1958 I began another line of work in the behavioral analysis of language-cognition, the systematic study of reading, commencing with the learning of word and letter responses under the control of the appropriate printed verbal stimuli. This study was to continue on to include an analysis of the various repertoires of skills that are involved in being an expert reader (STAATS, 1968a, 1975). By 1960 I had also begun to systematically study the manner in which the first language learning of the child could be produced by the systematic use of training procedures, using my own infant children as subjects. I developed procedures for dealing with various of the repertoires of which language is composed, as I have indicated in some of my analyses of language development (see STAATS, 1963, 1968a, 1971a, 1971b, 1975). I systematically presented training procedures to each of my children, for example, to produce a repertoire of motor responses under the control of the appropriate classes of words. One of the first elements involved the verbal stimulus «Come, come to Daddy», in working with my six-months old daughter. At that time she could creep forward, intentionally and I used a set of keys I habitually carry both as the stimulus to first control her approach behavior, and as the reinforcing stimulus when she had accomplished the act. After a number of trials of this type she would respond when I said «Come». I also trained her in her first labeling verbal responses. One early one was very straightforward. She already had the vocal response «E-e-e-h» in her repertoire. I would say «Eat», and also «E-e-e-h» to her during the times that I would feed her spoonfuls of

baby food, sometimes pausing before the next mouthful was delivered it with alacrity. My daughter had a 13 word labeling repertoire by the time she was 9 months old —consisting of such things as «Wahwah», for water, «Dogh», for a toy dog, «Mooh», for moon, «Dada», and «Mama», and so on. She also had a beginning verbal imitation repertoire, and her training was continued on in the various language repertoires, as was the case with my son.

When she was 18 months old I began to train her in number concepts. I held one raisin in one hand and two raisins in the other. Showing the first hand I said «Do you want one raisin?» and, then, displaying the other hand, I said «Or two raisins?». After a few errors she learned to say «Two» to receive the two reinforcers. By the time she was two, as a result of a stipulated training program, she could count unarranged objects up to ten —a performance that PIAGET has said develops in the child at the age of six or seven.

At two I began the training procedures that would lead to a complete reading repertoire in the child some years later. By this time she had a fully functional language repertoire. I constructed the token-reinforcer apparatus for work with young children, whose adaptations I and my students would use in various studies with children, as would many behavior modifiers. It included an aperture where cards could be presented on which there were the appropriate stimuli. Mixing a letter presentation with picture presentations she could name, she was told the name of the letter and then was reinforced with a penny for each response. The pennies were used as tokens for a variety of everyday reinforcers, such as having dessert in the evening. By the time she was two years and three months she had a reading repertoire of 15 letters and a number of words. I have excerpts from her training sessions on tape (and I have my son on film). Her tape demonstrates learning to read words, sentences, stories, as well as syllable and single letter pronunciations —all before she was three years and six months old. Done in 1962, 1963, and 1964 the pro-

cedures and findings are still current in the knowledge they provide of the processes of child development through learning and in the procedures and methods they provide for conducting additional research and applied work (see STAATS, 1968a, 1971a, for accounts of these child training procedures).

Before Jennifer was four years old I introduced protypical training procedures for producing a writing imitation repertoire as well as a free writing repertoire. She could write the letters of the alphabet at the age of four and she had good writing skills before she entered kindergarten. Let me also add that this systematic study of child development, within the context of the developing behavioral analysis, went into various other areas. I had a program for training the children to walk and to swim, and another program to produce toilet training of both types —nocturnal and diurnal. My nocturnal training program is still better than current procedures, in my opinion, although I have never published it. I believe the procedures I described in my 1963 book were the first behavioral account in the literature of diurnal toilet training.

A first step [is] to observe the usual times of bowel-movement of the child. When this has been ascertained, the child [can] be placed upon the «pottie-chair» at a likely time. Without some source of reinforcement, however, the child may attempt to escape or start to cry. This means that not only should the sitting period be short at first, but that some reinforcers —games with the parent, play with toys, and so on— [can] profitably be introduced.

Through this procedure, a necessary adjunct to successful total toilet behavior, sitting in the appropriate place, may be brought under the control of stimuli of the toilet situation through the use of positive reinforcement. The time of sitting on the toilet may also be gradually increased through reinforcement procedures and the child can then be trained to spend periods upon the toilet without being attended by the parent (STAATS, 1963, p. 378).

I also described the classical conditioning of the smooth musculature that occurred in such a training program. I present this account, now over 20 years old, not for historical reasons but to illustrate the point of the methodology used, which involved various types and methods of study. The type of study that I have described here involves experimental-naturalistic research methods. I have not submitted such studies to journals —the orthodoxies of research would not allow that. But they provided essential empirical-theoretical contributions, and they are the basis for many of the things I know, for they constitute the foundation for much of my formal research, for a good many of my theoretical analyses in books and elsewhere, as well as for others who knew of the work or the analyses. In my opinion this type of work can be necessary in turning abstract principles, formulated in laboratory simplicity, into analyses that can be employed in dealing with real behavior. In the present case, let me indicate that the theory of intelligence that I am concerned with in this paper owes much of its foundation to this experimental-naturalistic research. Let me add that the conduct of experimental-naturalistic research is one of the characteristics of paradigmatic behaviorism, a part of its methodology.

## THE BASIC BEHAVIORAL REPERTOIRES

In my first general analysis of complex human behavior, including the first approximation to a theory of intelligence, I had referred not just to responses, or even to behavior, but to repertoires. This is certainly a common term today in radical behaviorism. I believe the work of paradigmatic behaviorism helped bring this about. Moreover, this work provides the term repertoire with meaning that is not given elsewhere. In the definition of intelligence already quoted I said that many of the beha-

viors of which intelligence is composed «would constitute a basic repertoire for the... further acquisition of behaviors important in school» (STAATS, 1963, p. 8). Let me describe research that elaborates this conception, as it is important in intellectual (cognitive) development.

At the University of Wisconsin in 1965 I set up a laboratory-classroom situation for culturally deprived preschoolers. The nursery-school classroom engaged the four-year olds in typical activities, except that behavior problems were treated with behavioral techniques such as time-out, which I had employed several years before with my own children. In addition, however, each of the four-year-old children would go to a «work room» three scheduled times in the morning for training in reading, number concepts, and writing —using, again the procedures I had previously worked out with my children, employing the token-reinforcer system. The results were clear. Even children who were at risk for educational development could learn cognitive skills in advance of that which would be demanded of them. They learned to read the letters of the alphabet. They learned to discriminate objects by number, they learned to count objects, they learned to read and write the numbers, and some of the children were learning to add the numbers. They also learned to write the letters of the alphabet (see STAATS, 1968a; STAATS, BREWER, and GROSS, 1970). All of this was very clear because every stimulus presented to each child was recorded, as was each response. This was all done while the children were engaged in an activity that they elected, for all participation was voluntary, and the children said they enjoyed their learning periods.

Let me also add that this work involved a new methodology I call experimental-longitudinal research. It is designed to study complex behavioral repertoires that take a long time in acquisition, that involve many different elements, and that do not lend themselves to study by orthodox experimental designs (STAATS, 1968a, 1977; STAATS, BREWER, and GROSS, 1970). The

method involves detailed observation of the training given the child and of the behavioral acquisitions that develop. It does not necessarily involve control subjects, in the manner of group designs, or control conditions as in reinforcement/no-reinforcement reversals that manipulate the display of a response. These orthodox methods have their place in the research methodology I was formulating, but the experimental-longitudinal methods produce data not obtainable otherwise. For example, the hundreds of samples of the responses the children produced in writing letters provided data that I do not think is still available anywhere. For example, in the field of education dyslexia is considered to be the result of a genetic impairment, and one of the sources of proof is that dyslexic children do not «perceive» the letters correctly. That is, they will read or write a $b$ as a $d$ or a $p$ as a $g$. They will write letters backwards, upside down, and canted in various directions. What has not been realized in such accounts is that this type of performance is involved in each and every child's learning sequence. A child exhibits such errors until attaining a higher level of skill through additional learning. This central fact —which cannot be seen until there is a record of just what the learning process is— provides and entirely different conception of the child's development, or lack of development, of complex cognitive skills. Copying and writing the letters involves a complex repertoire of skills that only is gained if the child has the many learning opportunities demanded. Anything that disturbs that process will leave the child at a level of development that includes deficits and distortions. Our methods of study show that when dyslexic children are given additional training their so-called perceptual problems, which are diagnostic of what is called dyslexia, disappear along with their inability to read (COLLETE-HARRIS and MINKE, 1978).

## THE BASIC BEHAVIORAL REPERTOIRE AS A CAUSE

While this work involved new develop-

ments that were relevant in the developing stage of human behavioral psychology, there is continuity of behavioral analysis involved. The traditional goal of behavioristic analyses has been to show how particular human phenomena can be explained by conditioning principles. That goal is one of the things that leaves non-behavioral psychologists dissatisfied, because they are interested in examining human characteristics as *causes*, as the determinants of the way the individual behaves, not as effects, explained by learning. A central concern of the present analysis is to indicate how paradigmatic behaviorism approached this schismatic issue in a new way that was aimed at unification, and how this unificatory perspective was reflected in and grew from the empirical research that is one of the foundations of paradigmatic behaviorism. We can develop this theme by using the present research program as an exemplar. In doing this we must see that the research described above involved more than just the behavioral data. Several lines of research were opened that concerned the manner in which the training the children had received changed the intellectual «capacity» of the children. What I mean by «capacity» is not a mentalistic term. It has a behavioral definition, but it also provides the type of conception and empirical development that can meet the needs of the traditional psychologist.

To begin this account, let me indicate that in addition to the experimental-longitudinal behavioral data the children were also given psychological tests of a traditional sort. Traditional tests have been part of the research methods of paradigmatic behaviorism from the beginning because, unlike radical behaviorism, its behavior analysis of certain psychological tests makes then behaviorally justified (STAATS, 1963). Thus, in the remedial reading studies not only were detailed behavioral data kept of reading responses, subjects were given an achievement test (STAATS and BUTTERFIELD, 1965), and intelligence test (STAATS, MINKE, and BUTTS, 1970), and later a test of perceptual abilities (COLLETTE-HARRIS and MINKE,

1978). In the present study with the four-year-old culturally deprived children the behavioral analysis of intelligence that was first outlined in my 1963 account projected certain expectations concerning intelligence test performance. If the children had learned basic skills in their reading, number concept, and writing training then it would be expected that psychological test that measured such basic skills would be sensitive to the training.

And that is what the results showed. The children were given a Metropolitan Readiness (1948) test before and after the training —and on this test they advanced from a mean at the second percentile to the 24th percentile. On the Stanford Binet (TERMAN and MERRILL, 1937) they advanced from a mean of 101 to a mean of 113. This increase occurred over approximately a seven month period, based on training that occupied a mean of five hours and 28 minutes for the number concept training and 15 hours and 42 minutes for the writing training (STAATS, 1968a) and four hours and 28 minutes for the reading training (STAATS, BREWER, and GROSS, 1970). While not yet definitive, these results contributed to the developing behavioral theory.

For one thing, the concept of the basic behavioral repertoire was given support and elaboration within the context of these studies, and another principle was given clarity in the same way, that is, the principle of cumulative-hierarchical learning. Let me explain by indicating that both of these conceptual developments arose in the context of experimental-longitudinal research. When a short term behavioral study is conducted, or when the same repeated response is the object of observation in reversal designs, or when long-term studies are conducted on complex behaviors but the observations do not detail and record the training stimulus presentations and the responses made to them, or when the dependent variable is an unanalyzed test performance as a function of training, the study does not indicate the manner in which complex learning develops, or the manner in which skills learned at one point of the process then be-

come causative elements in the way the later learning proceeds. The experimental-longitudinal research on complex behavioral repertoires that has been outlined here, however, does reveal such complex, developmental elements.

We see, to begin, that it is important not only to study how single behaviors can be manipulated through reinforcement. When we study complex behavior repertoires we see that there is a cumulative-hierarchical learning process involved. My work with these various children, beginning with long-term study of my own infants, showed clearly that there are basic behavioral repertoires that are linked in a causative manner. In simple illustration the child has to learn a complex language repertoire consisting of a number of subrepertoires before the child can proceed with learning other cognitive repertoires. A language repertoire is a basic behavioral repertoire that is necessary for learning early number concepts, for learning to copy and write the letters of the alphabet, for learning to read the letters, and so on. These skills then in turn constitute basic behavioral repertoires important to the acquisition of yet more advanced repertoires. As has been indicated, for example, the reading repertoire is a complex of subrepertoires, some of which are learned in a sequential or hierarchical order. But the reading repertoire is then a basic behavioral repertoire necessary for the acquisition of a vast number of sensory-motor, language-cognitive, and emotional-motivational repertoires in the usual adult population.

These concepts and principles do not create a disturbance for the behaviorist. It can readily be agreed that there is no conflict here between paradigmatic behaviorism and standard behaviorism methods. But the systematic theoretical development of the concept of the basic behavioral repertoires and the principles of cumulative-hierarchical learning represent a new development in opening new avenues and directions of empirical and theoretical development. And, quite centrally, the development of the basic behavioral repertoire/cu-

mulative-hierarchical learning structure of theory provides a bridging framework by which to unify traditional bodies of knowledge with a behavioral body of knowledge —in a manner that meets all of the objective requirements of a strict behaviorism. We must realize that in good part traditional psychology rejects behaviorism because the latter will not accept personality as a cause —for treating personality as behavior is to treat personality as an effect. Paradigmatic behaviorism, however, says that the basic behavioral repertoires are aspects of personality in the traditional sense of being causes. The basic behavioral repertoires —personal characteristics— do determine how the individual will learn and perform. Realizing this provides a basis for unification developments.

## INTELLIGENCE AS BASIC BEHAVIORAL REPERTOIRES

As I indicated in beginning this paper, behaviorists have traditionally criticized mentalistic conceptions that infer intelligence to be a cause of the child's performance, when intelligence is employed as a circularly defined therm, specified solely by the child's behavior, either on an intelligence test or in classroom performance.

But this type of criticism does not tell us what are the bases of the individual differences in learning ability and performance that are so readily seen and that constitute the basis for an interest in defining and measuring intelligence. Saying that individual differences in learning and performance are due to differences in learning (reinforcement) history does not constitute an adequate answer either —unless that learning history is specified. Standard behaviorism has not given that specification. Paradigmatic behaviorism requires specification, even when the conception is behavioristic. *The fact is that some children learn easily and some do not.*

The differences are very important and we must ask why. The theory and research on the basic behavioral repertoires provides the answer specifically, within a true behavioristic framework that is explanatory.

To most people intelligence is considered a personal quality that determines individual differences in learning ability and performance with the subsidiary conception that intelligence is inherited. Paradigmatic behaviorism says, rather, that the individual differences in learning ability depend not on biologically based personality qualities but rather upon the stage or place in the cumulative-hierarchical learning process that the child's life conditions have brought the child. The child who is advanced in the cumulative-hierarchical learning process demonstrates rapid learning, the child who is retarded in the cumulative learning process demonstrates slow learning. This theory has different implications for treating problems of intelligence than does the traditional conception. Moreover, this theory suggests new things to look for, to study, and to measure.

This conception derives from my experimental-longitudinal research. That is, I first noted in working with my own children on reading and writing that the learning became progressively easy. It was laborious for the children to learn the upper case alphabet, for example, but the lower case alphabet learned afterward came easily and quickly. Such experimental-naturalistic findings were supported by the more formal data of experimental-longitudinal research. The evidence appeared in whatever type of learning was involved. In learning to read the alphabet, for example, the child typically would take four times as many trials, as many reinforcers, and as much time, to learn the first four letters of the alphabet as he or she would for the 13th, 14th, 15th, and 16th letters. The same thing was true with writing, and number concepts. Four-year-old children become better learners —more intelligent by usual definition— as a function of learning the basic behavioral repertoires. Not only does it take fewer

learning trials for the same child to learn new material, but an additional benefit is that as the child has exposure to reinforced learning participation the child learns to respond more rapidly in such learning situations. For example, in learning to read, the four-year-old children made almost twice as many responses per minute after 60 learning sessions (involving some 300 minutes of training) as occurred when they began the training, with a progressive increase occurring over this period (STAATS, BREWER, and GROSS, 1970). Is this an important finding? It certainly is, along with the other findings, given that the traditional developmental psychology approach cautions us not to begin to train the child too early —because they may not be biologically ready. On the contrary, the present analysis states it is the training that makes the child a good learner, a rapid worker, that is, intelligent, and «maturationally ready».

But this by no means answers all the questions that psychology has concerning the topic of intelligence. Moreover, this does not exhaust the implications of the paradigmatic behaviorism theory of intelligence. Let us take the traditional environmentalist demonstrations that children exposed to enriched environments will show increases in intelligence (WELLMAN and PEGRAM, 1944). Such results have not impressed the field much because there is nothing explicit in the studies. Just what was done to effect the change in the children's test performance is not shown, nor is the manner in which the change occurs, and what the change consists of. Perhaps the enriched environment just taught the children how to respond specifically to the intelligence test items. Moreover, without explicit statement of the training, and analysis of what the effects are, there is little basis for generalizing the findings, for replicating them, and so on. Such results only provide the basis for a general philosophy, in the same way that the genetic orientation provides a basis for a philosophy that intelligence is inherited, but offers no direct proof or principles to use in dealing with intelligence and its problems. Traditional psycho-

logy has expended great resources on measuring intelligence without being able to say what it si or how it functions or how it can be manipulated. An approach that could provide such specifications would indeed be important to traditional psychologists, especially if it could show profound things about intelligence that are not exposed through traditional methods of study.

Paradigmatic behaviorism's theory has those contributions to make, even in its present state, and many potentialities for additional development. Let me give one additional example. Let us go back to the original analysis that the items on intelligence tests measure aspects of the basic behavioral repertoires. I elaborated this conceptual thesis in 1971 in a book entitled *Child learning, intelligence and personality*. In this book I selected various items on intelligence tests and showed how the tests actually sampled the various basic behavioral repertoires. Many items sample the subrepertoires that compose language —for example, the labeling repertoire, the verbal-motor repertoire (where words elicit motoric response elements), the verbal-emotional repertoire (where words elicit emotional responses), the verbal-imitation repertoire (where words can elicit imitation verbal responses), the word-association repertoire, the number-concept repertoire, and so on. The previously described studies gave empirical support to the concepts of the basic behavioral repertoires and the cumulative-hierarchical learning principles. But it is important to make a deeper analysis of the knowledge of intelligence provided by the field of psychometrics. Moreover, it is important to establish the theoretical bridging that would make it possible to unify the separate realms of psychometrics and behavioral study.

We have now completed three studies that provide this bridging theory and that (with the theory) open the way for making a further analysis of intelligence, its measurement, and the manipulation of intelligence —that is, provide an avenue by which to construct a complete explanatory account of intelligence. The general strategy was to analyze particular intelligence tests in terms of the basic behavioral repertoires involved. Then training was given to children involving the relevant basic behavioral repertoires, but not training on intelligence test items. Mostly, the children learned repertoires that would not be recognized as being related to the intelligence tests. Yet the analysis of the basic behavioral repertoires and the intelligence tests was so explicit that it was possible to predict just what the changes in intelligence would be, in important instances predictions that were quite different than would be expected by the intelligence test theory involved. The results, as will be indicated in part, then provided a basis for elaborating the paradigmatic behaviorism theory of intelligence (STAATS and BURNS, 1981).

The first study (STAATS and BURNS, 1981) dealt with various concerns, using the WPPSI intelligence test (WECHSLER, 1967). The theory underlying this test is that there are different independent factors of intelligence that can be measured by using the different subtests of the total test, which are sets of different types of items. The child's general intelligence is thus thought to result from the combination of the several independent intelligence factors. The child can be high in one factor and low on another, and various combinations are possible, some of which are considered to have diagnostic implications for psychopathology. The particular factors of intelligence selected for study were the Geometric Design and the Mazes subtests of the WPPSI. The former is generally considered to measure the child's abstract-conceptual-mathematical intelligence, the second would generally be thought to measure the problem solving intelligence of the child. The analysis in terms of basis behavioral repertoires indicated, in contrast, that rather than these two types of measured intelligence being independent, they are really very similar. For both subtests, the paradigmatic behavioral analysis reveals the child has to have a repertoire of sensory-motor skills of an attentional sort that involve inspection of line drawings, comparison of line drawings, and

the skill of holding a pencil and making line drawings in intentional directions, guided by the visual stimuli of other line drawings. In reproducing geometric figures the skills have to include copying imitational skills, where one compares what is being drawn with the standard stimulus. In the Mazes test the pencil has to be moved within confining lines in tracing a path. This task, especially, is presented by means of complicated verbal instructions. Thus, the child must also have a full repertoire of language, especially the verbal-motor repertoire which makes possible the elicitation of motor responses through verbal stimuli (STAATS, 1968, 1971a, 1975).

When analyzed in terms of the basic behavioral repertoires involved both the commonalities and the differences between these two intelligence test tasks can be seen. Moreover, we can see why it is that these are good intelligence tests. The reason is that the basic behavioral repertoires that are measured by the tests are the same basic behavioral repertoires that are valuable when the child goes to school and is faced with the educational tasks that are encountered. That is, the ability to follow verbal instructions, the attentional and discrimination skills, the ability to hold a pencil and to make intentional and guided lines, the imitational ability to reproduce line drawings, and so on, are central in learning to write letters, numbers, words, to read, and so on, primary tasks in the early school years.

The analysis also tells us why some children are considered to be intelligent when measured by certain intelligence test, or when evaluated by a teacher in school. The children who have learned the necessary basic behavioral repertoires will do well in both situations. They will learn the tasks that are presented. They will do well if they have previously been exposed to the training that will give them the basic behavioral repertoires. What experiences would do that, we might ask. The research that has been recounted already studied those skills in experimental-longitudinal detail. The skills are included in those the child acqui-

res in learning to read and write the letters of the alphabet. A child who has had the advantage, at least in part, of being trained to copy and write and read letters of the alphabet will have at least part of the sensory-motor and language skills necessary to do well on the Geometric Design and Mazes subtests of the WPPSI. The child will be considered intelligent and, in fact, will be intelligent in this respect.

And now we can see that an analysis that has ben made with this degree of specificity, breadth, and detail, including procedures for training, can serve as the foundation for research. The analysis said that the explicit procedures for training children to copy and write and read the letters of the alphabet would have the effect of making them more intelligent on the WPPSI, on tests such as the Geometric Design and Mazes tests, but not on others that would require different basic behavioral repertoires. The analysis was supported in a study in which 11 preschool children received the training and 11 control children did not. The training involved a total of six hours spread over and average of 60 sessions during a school period extending from November to May. The children received a mean of about $2.50 worth of reinforcers. They learned to write a mean of 15 letters of the alphabet and on a reading test they showed they had learned to read a mean of 17 letters, whereas the control children could only read eight. A psychometrist without knowledge of the research gave the intelligence tests. The children receiving the writing training tested to be significantly more intelligent than the control children on both the Geometric Design and Mazes subtests, but not on a subtest unrelated to the basic behavioral repertoire that was learned. Extrapolating the improvement shown on the two subtests to the whole intelligence test indicated the children each would have gained 14 IQ points as a result of the training. It may be concluded that training that is this effective for such a brief period at such a low cost has important implications not only for theory but for elaboration in practical intervention work. This study was also

important in showing the generality of the training in the basic behavioral repertoires, since the children showed elevated intelligence on two different WPPSI subtests. That generality is one proof that the children had not just learned specific skills of little significance. Moreover, the findings challenge the traditional concept of separate factors of intelligence, since the one type of training affected both kinds of intelligence. It may be concluded that the paradigmatic behaviorism theory and findings provide a more profound conception of intelligence than is provided by traditional psychological theories and, it might be added, more profound than the conception the originator of the particular test employed.

The second study dealt with the area of number problems that appear widely on various children's intelligence tests. The assumption underlying this usage is that there is a factor of intelligence that is responsible for the child being successful on such problems —a personal, inherited factor. BEN-BOW and STANLEY's recent findings that there are sex differences in mathematics ability have led many psychologists to believe that boys are inherently better than girls in this intelligence area (see KOLATA, 1983). Paradigmatic behaviorism takes its same position in this as in other areas of intelligence and ability —that there are basic behavioral repertoires involved that depende upon cumulative-hierarchical learning. Again, two groups of four-year-old children were formed, one being given training, successively, on the basic behavioral repertoires of discriminating objects by number, counting up to 16 objects, making a stipulated number of marks on paper, a form of counting, counting a subset of a larger set of objects, subtracting from a set of objects a stipulated subset, and the like. The results showed that just learning to discriminate small numbers of objects and to count objects had the effect of making the experimental subjects significantly more intelligent on the intelligence test items that were given to the subjects before and after the training period. The additional training added additional increases in intelligence for the chil-

dren receibing the training. It should be emphasized that the experimental children did not receive training on responding to any problem they subsequently faced on the intelligence test items, and that these four-year-old culturally-deprived children all learned number concept skill such as counting objects that PIAGET has said do not develop in children until the age of six or seven. It would be expected that as a consequence these children would be able to learn better in school situations involving number concept learning and be considered as more intelligent in performance as a consequence. Again, the cost in time and money, about the same as in the first study, would suggest that the type of training in number concepts given the children was functional and valuable, as well as theoretically, empirically, and heuristically significant.

The third experiment, designed and conducted by Leonard BURNS (STAATS and BURNS, 1981), concerned another widely used type of intelligence test item that is considered to measure conceptual ability. For example, the child is asked «In what way are a cat and mouse alike?» (WECHSLER, 1974, p. 74). The child receives points towards his or her IQ score if answering «They both are animals». Young children of three to six or seven will usually give answers such as «They chase each other», and do not give appropriate class names, and this is usually taken to mean that biological maturation is necessary for the development of the conceptual ability necessary to respond correctly. The paradigmatic behaviorism theory of intelligence (STAATS, 1971a) has considered this type of intelligence test ability of depend upon the basic behavioral repertoires of language elements the child has learned, particularly on learned classes of labels.

This aspect of the theory of intelligence was studied in the third experiment by giving one group of children training in the labeling language repertoires for various classes of objects as well as training in describing the objects in terms of the name of the common element of the class, which is the

class name. The training for the experimental subjects involved 15 classes of objects, so the children learned a large number of labels and descriptions of the objects in terms of their common qualities. The results were very important in showing the effect of training in basic behavioral repertoires on the *general* performance of the child. That is, following the training period the children were given various types of conceptual tests, tests that are traditionally thought to measure different conceptual abilities. They were given *similarities test* items of the type exemplified above, a *picture-sorting test* (where the subject has to group in concept-classes a number of stimuli), a *reason-for-sorts test* (which asks the children why they have sorted categories as they did), and a *category-identification test* (which gives the subject categories of pintures and requires the subject to indicate what the pictures have in common). The important finding was that the language training given to the children —in a reinforcement-based procedure that provided formally what the child usually learns informally in everyday language experience— produced significant changes on the *various* intelligence tests. It should be noted that such tests are employed for the diagnosis of brain damage and retardation that is considered to be biologically based. What the tests measure, however, is not a mentalistic conceptual ability, but rather an advanced language repertoire that is the basis for solving various types of conceptual problems.

Let me conclude this summary of these three experiments by saying that the three groups of experimental children each received training in only one type of basic behavioral repertoire, and thus had its intelligence increased in only the one area. It would, of course, be possible to give children multiple types of training and in this manner produce large scale increases in intelligence. And here we have the basis for an intervention study that could be the basis for solving problems of intelligence.

## THE HEURISTIC VALUE OF THE THEORY

I began this paper by quoting from a radical behaviorist account that stated that the book *Verbal Behavior* had not produced any research on the central topic designated by its name —and I suggested why. While we must recognize the great importance of the analysis in *Verbal Behavior*, we must also recognize that a third-generation development has been necessary to advance the behavioristic conceptual framework to provide a foundation for research and applied programs on language development and function, and in the cognitive processes generally. Paradigmatic behaviorism has been constructed to provide that framework. It has exemplificed the ability to do so in various areas, and it provides a model for additional elaborations and innovations in areas that it has not covered. In this context it is relevant to refer to the work of Aimée LEDUC (1985) who has extensively applied the theory and methods of paradigmatic behaviorism's analysis of intelligence in treating the lack of intellectual development in a child whose abusive and deficit environment had left her functioning on the level of a feral child.

Let me suggest that what we have here is a framework theory, general methodology, and specific procedures and analyses that can be used broadly by behaviorists to reveal new directions of research and application that in addition to their value within behaviorism itself will also provide the bridge for linking behavioristic study with legitimate interests and knowledge in non-behavioral psychology. This pertains to various areas of interest, as it does to the field of intelligence, its measurement, and to the treatment of retardates, learning disability cases, and other children with cognitive deficits. The prototypical research that has been described when filled in with the large number of studies needed, can provide a complete theory of intelligence; what it is, how it comes about, how to insure its presence in children in good quantity, how to treat problems of intelligence, how intelli-

gence functions in the individual's performance, what IQ tests are and what they yield, how to construct better intelligence tests that will enable us to say what it is the child has and what it is that the child lacks and that, as a result, can be closely tied in with the needs of clinical and developmental psychology for insuring the desirable development of the child. This type of knowledge can provide a *tour de force* for this field that will demonstrate to non-behavioral psychology how a behavioristic approach can be valuable in a way that links to and utilizes important knowledge in non-behavioral psychology. I would suggest that this is a direction behaviorism should generally take in its third-generation step forward, and that this represents one of the new characteristics of paradigmatic behaviorism that has a number of directions for development by psychologists of a behavioristic orientation.

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