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Animal Cognition as the Praxist Views It

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EPSTEIN, R. *Animal cognition as the praxist views it.* NEUROSCI BIOBEHAV REV 9(4) 623–630, 1985.—The distinction between psychology and praxics provides a clear answer to the question of animal cognition. As Griffin and others have noted, the kinds of behavioral phenomena that lead psychologists to speak of cognition in humans are also observed in nonhuman animals, and therefore those who are convinced of the legitimacy of psychology should not hesitate to speak of and to attempt to study animal cognition. The behavior of organisms is also a legitimate subject matter, and praxics, the study of behavior, has led to significant advances in our understanding of the kinds of behaviors that lead psychologists to speak of cognition. Praxics is a biological science; the attempt by students of behavior to appropriate psychology has been misguided. Generativity theory is an example of a formal theory of behavior that has proved useful both in the engineering of intelligent performances in nonhuman animals and in the prediction of intelligent performances in humans.

Praxics Generativity theory Insight Behaviorism Cognition Problem solving

ANIMAL cognition as the praxist views it is the subject matter of another field. That field is called *psychology*, a term derived from the Greek "*psyche*," which originally meant "breath" and came to mean "spirit" or "mind." Cognition is also the subject matter of a recent, somewhat informal, amalgam of disciplines called "cognitive science," and it has also been of concern to some researchers in ethology, anthropology, sociology, and other disciplines.

Psychology is mind's home, however, and for more reasons than etymology. Yet in no field has the study of mind been more strenuously challenged. How can this be? How could the study of mind have been challenged in the very field that was established to study it?

In this essay I review some of the events that led many psychologists to challenge and, indeed, to attempt to forbid the study of mind. I conclude that these challenges were misguided and that they were destructive in several respects. The fact that psychology conferences are still convened with titles like "The Question of Animal Cognition"—the title of the conference that set the occasion for this paper—shows how destructive these challenges have been. The fact that the word "mind" has been replaced by an awkward one with three syllables—that even mentalists are reluctant to use the language of mentalism—shows the power of these challenges. The fact that Griffin [23,24], Roitblat [39], Premack and Woodruff [38], Gallup [22], and others have had to labor in recent years to convince their colleagues of what, in effect, is obvious—and that they have been attacked for doing so—suggests that challenges to the study of mind may have retarded its advance.

Equally important, controversy in psychology has impeded the growth of another discipline, the study of behavior. Some psychologists, as well as investigators in other fields, have not been concerned with mind but rather with behavior for its own sake. They have been concerned with questions such as: How is the behavior of organisms, human and otherwise, determined by genes, nutrition, drugs, sleep deprivation, operant and classical conditioning, modeling, neural interventions, and so on? Is behavior predictable, and, if so, what principles do we need to predict it?

Advances in the study of behavior have been significant in many domains of study. In particular, advances in what I call generativity theory have shed light on the origins of some of the behaviors that lead psychologists to make inferences about mind.

In spite of real advances in both the study of mind and the study of behavior, investigators in each discipline have shown an almost pathological intolerance of the other discipline. Why?

I have become increasingly concerned with this question because, a few years ago, my research thrust me into the vortex of an intense debate. Though it began 40 years before I was born, the intensity of the criticisms leveled against my work sometimes made me feel as if I were responsible for the entire controversy. It became increasingly important to me to have done with this controversy because, when I wasn't reenacting old debates, I seemed to be making significant discoveries about the origins of intelligent behavior. As the data accumulated and a theory began to take shape, the work seemed more and more to justify *itself*. So why, I asked, was I spending so much time attacking cognitivists? And why, especially, did I have to spend any time at all justifying the study of behavior?

I am happy to report that I have now resolved these questions completely—at least to my own satisfaction. We are all unwitting players, I now believe, in a curious drama that made its debut in 1905 and that has been playing more or less continuously ever since. The stage had been set long ago. As is often the case on Broadway, the stars of the first performances were charismatic; stand-ins and replacements were often mere technicians. The plot was absurd.

ACT ONE: THE PROHIBITION AGAINST MIND

A geneticist makes his way through the august halls of the

Zoology Building at his university. He stops at the office of the chairperson and demands an audience with her. He tells her that genetics has a lot to contribute to zoology and that he wants an office and laboratory space in her department. She is surprised but intrigued by his style, his enthusiasm, and the force of his arguments. He tells her that problems of classification in zoology could be handled in a more sophisticated and objective way by genetics and, indeed, that the proper *subject matter* of zoology is genes. The chairperson is on her guard. Finally, the geneticist tells her that he insists on assuming the chairmanship of the Zoology Department immediately. She throws him out of her office.

I offer an exegesis: The geneticist, our protagonist, was a pathetic figure. He was sincere in his mission, but he was not in good touch with reality. He may have been right, but right is never enough. He made some progress, if only briefly, in his attempt to depose a foreign monarch, but he was destined to fail. The monarch was merely a symbol of the larger institution, and institutions resist radical change; at best, they just grow old. The drama was a tragedy, because the protagonist was, in effect, killed. Zoology survived, free to stumble forward in its own way, with or without genetics. Reality triumphed.

Psychology was not so fortunate. The plot started out the same—and it would surely have had the same ending—but then, like in *The Purple Rose of Cairo* when one of the characters jumped off the movie screen, the story ground to a halt halfway through Act Two.

Psychology

Since its earliest history, long before formal departments existed, psychology had always been, as the *Oxford English Dictionary* recorded, "The science of the nature, functions, and phenomena of the human soul or mind," or, as Boring [2] stated, the study of "the generalized, human, normal, adult mind" (p. x). Wolff and Hartley had defined it so in the 1700s, following a tradition of inquiry that can be traced to the ancient Greeks. The first formal investigations were all concerned with mind: those of Fechner, Wundt, James, Ebbinghaus, Mueller, and so on. Even Morgan's Canon, which was later misinterpreted by the behaviorists as a condemnation of the study of mind, was a prescription for simplicity in the study of mind [8].

But, in 1905, in his *Primer of Physiological Psychology* [29], the English psychologist, William McDougall, challenged the traditional definition of psychology. As he later wrote [45]:

> Up to the end of the last century and beyond it, psychologists did in the main concentrate their attention upon the introspectively observable facts, unduly neglecting the facts of human action or behaviour, and ignoring the need for some adequate theory of behaviour and of character. . . . This neglect is implied in the definition of psychology commonly accepted at that time, namely, the "science of consciousness" . . . (p. 54).

The possibility of a new science was in their air around the turn of the century, as McDougall [45] noted. In the 1800s, the philosopher John Stuart Mill [35], had proposed the establishment of a "science of the formation of character," which he labeled "ethology" (modern ethology is not related). Somewhat later, the London physician, Charles A. Mercier [33], proposed that a science of behavior be established, called "praxiology." The French philosopher Espinas [19,20] made similar proposals. In biology, two zoologists, Parker and Haswell [37], laid some of the foundations of modern behavioral biology in a textbook they published in 1897; there, they redefined "ethology" as "the relation of the organism to its environment."

But McDougall [45], the first of our two protagonists, took a different approach:

> . . . it seemed to me that both Mill and Mercier were in error; that what was needed was not a new science of behaviour under a new Greek name, but rather a *reform of psychology* [italics added], consisting of greater attention to the facts of behaviour or conduct. . . . I gave expression to this view in my first book, by proposing to define psychology as the positive science of conduct. I further defended this reform in my *Introduction to Social Psychology* (1908) [30]. And in 1912 I published my little book entitled *Psychology, the Study of Behavior* [31]. (pp. 57–58)

Enter, stage right, an even more luminous star: John Broadus Watson. In 1913, Watson turned the quest for a science of behavior into a *movement* in psychology. Like all good movements, its name even had an *-ism* attached to it: *behaviorism*. Note that behaviorism was not a name for the study of behavior; nor was it the name for a branch of philosophy. *It was the name of a movement to change psychology.* Watson's seminal works on the subject—for example, "Psychology as the Behaviorist Views It" [43] and *Psychology from the Standpoint of a Behaviorist* [44]—were prescriptions for such change.

Watson [43] stated the case even more extremely than McDougall had. Not only was psychology to adopt a new subject matter, but its old subject matter was now *forbidden:*

> The time seems to have come when psychology must discard all reference to consciousness; when it need no longer delude itself into thinking that it is making mental states the object of observation. We have become so enmeshed in speculative questions concerning the elements of mind [and] the nature of conscious content . . . that I, as an experimental student, feel that something is wrong with our premises. . . . I believe we can write a psychology, define it as [the science of behavior], and never go back upon our definition: never use the terms consciousness, mental states, mind, content, introspectively verifiable, imagery, and the like. . . . (pp. 163–166)

Following the scenario we developed earlier, Watson should now have been thrown out of someone's office. But, instead, in 1915, just two years after his seminal paper on behaviorism, he was elected President of the American Psychological Association. (A few years later, he was indeed thrown out of Johns Hopkins University and, in effect, out of academe, but on a matter of moral turpitude.)

Watson was an extremely visible and influential man. His striking personal appearance, his flamboyant personal style, and his forceful writing made him succeed where others would have failed. The *Zeitgeist* helped: Watson promised that the new psychology would lead to many practical applications, and, as I have already mentioned, the possibility of a science of behavior was in the air.

Other psychologists took up Watson's cry, or at least some variation on it. Skinner, Kuo, Hull, Meyer, Tolman, Guthrie, Hunter, Lashley, Schneirla, Lehrman, and others fought the behaviorist battle. Like the Japanese holdouts found on some South Pacific islands years after the end of World War II, some are still fighting.

But the war was lost long ago. Less than three percent of the membership of the APA identifies itself openly with the behavioristic tradition, and that percentage is declining. The *APA Monitor* runs nearly 100 advertisements a year for academic positions in the study of cognition and only one or two a year for positions in the study of behavior. Behavioral laboratories tend to be small-scale affairs these days, and some have shut down for lack of funding. Post-doctoral positions are rare. As things stand today, a behavioristic flag-waver would hardly be elected president of the APA.

Though the behavioristic movement was influential for about three decades, the study of mind held its own, and, in the 1950s, with the advent of computers and the alliances that were formed between psychologists, linguists, computer scientists, and philosophers, the study of mind began to flourish as it never had before.

And so, it seems to me, it should be. Behaviorism—as a movement in psychology—was an aberration.

ACT TWO: THE STUDY OF BEHAVIOR

It is unfortunate that the scientific study of behavior was launched with an ism. An effort to establish new programs and departments would surely have been more successful in the long run than the movement to redirect existing departments. But behaviorism, as a movement, was more than a useless exercise. Its primary mission, the appropriation of psychology, was not achieved, but it made at least three major contributions.

First, it convinced many people that the study of behavior *qua* behavior was a legitimate enterprise, and, indeed, scores of behavioral laboratories were established right under the skeptical eyes of traditional psychologists.

Second, it created an almost fanatical concern with objectivity in psychology proper. Külpe [27] had criticized the cognitive research of his day in part because it was difficult to replicate, but modern experiments on cognition are usually easy to replicate and elegantly designed. Methodologically, at least, every psychologist became a behaviorist [1].

And third, the movement itself evolved into a school of philosophy, which is today the proper referent of the word "behaviorism." One of the foremost philosophers in the world, W. V. Quine, considers himself a member of this school, as do Skinner, Day, and other prominent psychologists.

That behavior is a legitimate subject matter is undeniable. Even cognitive scientists do not deny it (they *do* object to attacks on their field, as indeed they should). But Watson put the study of behavior on a steep and thorny road. The study of behavior, which some now call *praxics* [7]—a blend of "physics" and "*praxis*," the Greek word for "behavior"—has not yet seen its day for a rather mundane reason: It was established in the wrong academic department.

Act Two, the formation of a comprehensive, naturalistic science of the behavior of organisms, has been in limbo for a long time. Rather than continue the advance, we act out old battles. The old actors replace themselves with trusted graduate students, who bring new vigor to the roles. "The study of mind is objectionable because . . ." "The study of behavior is the true subject matter of psyche-ology because . . ." "We deserve space and resources in your field because . . ." The students merely complete the sentences.

Because praxists have tried to function in psychology departments, they have had relatively little contact with scientists in kindred fields: behavioral genetics, ecology, ethology, evolutionary biology, and those areas of physiology and anatomy that are concerned with behavior. Praxics programs, if indeed such programs finally evolve, should include scientists from these fields and others. The behavior of organisms is probably the most complicated subject matter science has ever considered. A comprehensive science of behavior must consider all of the variables of which behavior is a function: genes, conditioning, drugs, neurophysiology and anatomy, sleep deprivation, physical trauma, modeling, and so on.

At a retirement dinner a few years ago, Skinner proposed a unique solution to the mind-body problem (caught as they are in the middle of Act Two, psychologists are, sad to say, still concerned with this problem). On one side of a long strip of cloth he had attached various symbols for spirit or mind: the Greek letter psi, the Star of David, the Cross, and the Crescent. On the other side he had attached symbols for body: the brain, the heart, and, he said, "courtesy of the Alka Seltzer Company," the stomach. His solution was to twist one end of the strip 180 degrees and then to attach the two ends. He now had a one-sided figure, a Möbius Strip. At last, mind and body were one.

I propose a more realistic, yet equally simple, solution to the mind-body problem: *two departments*.

I have presented elsewhere a variety of arguments for drawing a distinction between behaviorism and the study of behavior and for separating praxics from psychology [7]. To summarize, behaviorism should be distinguished from the study of behavior because: (a) No laboratory science should be constrained by a formal school of philosophy. The range of variables and topics that behavioral psychologists have investigated has been limited unnecessarily by the ism. (b) Praxics laboratories should be open to nonbehaviorists. One can discover interesting things about the determinants of behavior no matter what one's opinion are about feelings, mind, or free will. (c) Confusion between the science and the ism in the public eye has cost the science credibility and funding. The ism is, understandably, unattractive to Americans, but the science makes none of the unattractive assertions of the ism. Moreover, millions of people have benefitted substantially from contributions the science has made to pharmacology, medicine, therapy, education, business, and industry. Appreciation for the science may finally come when it is distinguished from the ism.

Praxics must go free of psychology because: (a) Psychology has a terrible public image, which is largely deserved. (b) "Psychology" is an inappropriate name for the study of behavior. (c) The concept of mind and the interest people have in it are in no danger of disappearing. (d) The takeover attempt was inappropriate to begin with, and the debate has accomplished nothing. (e) A split will likely mean new resources for both parties. (f) The establishment of an independent science of behavior will allow behavioral psychologists to realign themselves with the biological sciences. (g) The new science will also mean the fulfillment of a dream many biologists have shared: the creation of a comprehensive behavioral biology. (h) The new science would provide enormous benefits to society.

GENERATIVITY THEORY

My own research has led in recent years to what I call generativity theory—a formal theory of the determinants of ongoing behavior. Traditional theories of behavior—often called "learning theories"—have dealt with either the acquisition or the maintenance of behavior. Theories of acquisition have dealt with the manner in which new behavior is taught, for example, by classical conditioning, operant conditioning, or modeling. Theories of maintenance have dealt with how previously established behavior is maintained over time, for example, by various schedules of reinforcement.

Theories of this kind were criticized by Köhler, Chomsky, and others on the grounds that a great deal of behavior is "generative" or "productive." Virtually every sentence we write or speak is new. When we solve problems, draw, dance, or cook a new dish, we behave as we never have before. Indeed, when you look at behavior closely enough, you find that organisms *never* do the same thing twice. The most salient characteristic of behavior outside of the laboratory is that it is ever changing and ever novel. Theories of acquisition or maintenance cannot deal adequately with ongoing behavior in the natural environment.

The very concept of the operant belies both the fluidity and novelty of ongoing behavior, as Skinner himself has occasionally noted (e.g., Skinner, quoted in Evans [21], pp. 20–21; Skinner [40,41]). After all, in order for you to reinforce some response, it must first appear. *But where does the first response come from?* The concept of the operant sheds no light.

Skinner [41] acknowledged that ongoing behavior was fluid and probabilistic, but he believed that the probability of ongoing behavior could not be assessed directly. He offered the construct "response strength" as a substitute for probability and then made what has long been regarded as his greatest contribution to the study of behavior: He suggested that the *rate* at which an organism repeats some response could be used as a measure of the strength of that response. Science, he said, must always have some "recurring unit" with which to work. In this case the unit was the response; its rate was a measure of its strength, and strength, in turn, was at least related to probability.

Generativity theory, in contrast, allows us to estimate probability directly and, indeed, to predict any number of behaviors continuously in time in novel environments. The theory may be summarized briefly as follows: Previously established behaviors manifest themselves in new situations to produce new behaviors; I call this assertion the principle of novelty. The process by which old behaviors become new ones is called *transformation*, and the equations that predict such changes are called *transformation functions*.

At present, the theory utilizes four such functions, each of which represents some known behavioral phenomenon that has been studied empirically: reinforcement, extinction, resurgence, and automatic chaining. In other words, unlike many theories of mind, generativity theory does not refer to any constructs and does not make use of any metaphors.

Each of the equations is linear, of the form of Bush and Mosteller's [4] linear operator model of learning. The four may be considered simple principles of covariation (cf. [26,42]) in which the first two describe changes in the probability of some behavior as a function of consequential events in the environment. The others describe changes in the probability of some behavior as a function of the occurrence of other behaviors emitted by the organism.

That is to say, at any point in time the probability of some behavior may increase or decrease as a function of an event in the environment. For example, you press the "orange soda" button on the soda machine, and nothing happens. The probability of pressing it again is decreased. The probability of some behavior may also increase or decrease as a function of an event in an organism's own behavior. For example, as the probability of pressing the orange soda button decreases, other behaviors that have been effective under similar conditions in the past become more likely: pressing another button, calling for help, kicking the machine, and so on. According to the theory, all four of these phenomena occur simultaneously and continuously with all possible behaviors that can occur in a given environment.

I will illustrate the theory in two ways: first, by giving a verbal account of the emergence of a complex, novel performance in a pigeon, and second, by showing how the equations have been used to predict ongoing, novel behavior in human subjects.

"Insight" in the Pigeon

Epstein, Kirshnit, Lanza, and Rubin [15] showed that pigeons with appropriate training histories could solve one of Köhler's classic box-and-banana problems in an insightful manner the first time they were confronted with it. In the test situation, a pigeon was faced with a small facsimile of a banana, pecks to which had been previously reinforced with food. The banana was out of reach of the bird, and jumping and flying toward it had previously been extinguished. Also in the chamber was a small box. At first each subject looked confused: It stretched repeatedly toward the banana, motioned toward the box, turned in circles beneath the banana, and so on. Then, quite suddenly, it began to push the box toward the banana. Each subject stopped pushing when the box was beneath the banana, quickly climbed on the box, and pecked the banana.

To analyze a performance of this sort one must do two things: First, one must assess the contribution that previously established behaviors made to the emergence of the new sequence. With pigeons, one can do this by varying the training histories of different birds and then placing them in the test situation. By doing so, we showed that the novel performances varied in orderly ways with respect to the training histories. Deficient training histories produced deficient, yet orderly performances [15].

The more difficult task is to determine the transformation principles: How, moment-to-moment in time, are the old behaviors transformed into the new performance?

I shall offer a verbal account of the performance as it unfolds, but the account is unsatisfactory. Generativity theory holds that the kinds of processes I will describe operate continuously and simultaneously. A verbal account cannot do justice to such complexity.

Multiple controlling stimuli. The pigeon's apparent confusion would seem to be the result of multiple controlling stimuli. That is, in training, the stimulus *box-alone* (with a small green target available at ground level) had come to control pushing movements toward the box, because, with the box present and the banana absent, pushing the box had produced food. The stimulus *box-under-banana* had come to control stretching toward and pecking the banana, because, in the absence of the green target, climbing onto the box and pecking the banana had produced food. In the test, neither stimulus was present, but a compound or intermediate stimulus was: The green spot was absent, the banana and box were present, and the banana was not over the box. The situation therefore resembled *two* situations to which the bird had been exposed during training. It is as if the green spot (of the first training scenario) had been moved just out of the bird's view and the banana (of the second training scenario) had been shifted away from the box.

When an organism is confronted simultaneously with two stimuli (say, both red and green are illuminated on a stop-light) that control separate responses (pressing the brake pedal and pressing the accelerator pedal, respectively), or with a stimulus intermediate between the two stimuli, both responses tend to occur, in rough alternation [5,34]. Hence, the pigeon stretched toward the banana, oriented toward the box, looked back toward the banana, and so on.

Changing dynamics. This alternation was unstable, since neither behavior was successful. Moreover, in training, the bird learned not to jump or fly toward the banana when the banana was alone in the chamber and out of reach. Thus, stretching toward the banana should have weakened more rapidly than behavior with respect to the box. Within seconds, the bird should have come to face the box more and more directly, which indeed it did. It was now faced with *box-alone*, almost precisely the stimulus that controlled pushing, and the bird began to push.

"Functional generalization." Why the bird pushed toward the banana is a more complicated matter, dealt with in detail elsewhere [9, 11, 15]. Other experiments suggest that the subject pushed toward the banana not because the banana resembled the training target but because pecking the banana had been reinforced; that is, because the banana was "important." The spread of effect from the training target to the banana is reminiscent of what some call functional generalization [3].

Automatic chaining. It is often the case that our own behavior generates stimuli that control other behaviors; in other words, that one behavior changes the probability that other behaviors will occur. Simply turning one's head may bring into view a reminder note posted on a wall, an approaching tiger, or a candy bar, and relevant behavior may then follow. The bird stopped pushing the box in the right place because, as it pushed, it arranged for itself closer and closer approximations to *box-under-banana*, precisely the stimulus that controlled climbing and pecking. Indeed, some subjects climbed and stretched toward the banana prematurely; they immediately dismounted, pushed further, and climbed again.

Resurgence

Other, more complicated performances require other principles. An especially useful one is the *principle of resurgence: When, under given conditions, a response that has recently been effective is no longer effective, other responses that were effective in the past under similar conditions tend to recur* [6, 12, 25, 28, 36]. Thus, when a doorknob that has always turned easily will not turn, many other behaviors that have gotten one through doors tend to recur: One turns harder, pulls up or pushes down on the knob, kicks the door, shouts for help, and so on. Many of the apparently "spontaneous" performances we have studied can be accounted for by this simple principle, and it would appear to have many other possible applications in the interpretation of clinical phenomena, foraging behavior, apparent experimental anomalies, problem solving, and behavior under certain schedules of reinforcement [12, 16, 17].

Resurgence is, in effect, the converse of automatic chaining: A *decrease* in the probability of one behavior produces an increase in the probability of one or more other behaviors.

Interconnection

The novel performance generated in the "insight" experiment was the result of the spontaneous interconnection of two repertoires of behavior which had been established separately. Interconnection is one of only four behavioral phenomena that may ultimately account for all instances of novel behavior [14]. Epstein [11] characterizes the interconnection process as follows:

> Interconnection is likely when multiple behaviors are made available, either through resurgence of previous reinforced behaviors during extinction . . . or by multiple controlling stimuli. . . . Multiple behaviors may combine to produce new sequences [18], behaviors that have new functions [18], or behaviors that have new topographies. Interconnections come about moment-to-moment in time through a variety of processes, any and all of which may be operating simultaneously. One important process is automatic chaining: One behavior changes the environment or the orientation of the organism and hence produces stimuli that make other behaviors more or less likely. When topographies are compatible, blends may appear, as one sees in verbal behavior or painting. The dynamics can be extremely complicated as behaviors are simultaneously waxing or waning in strength, resurging, producing new stimuli, and so on. (p. 132)

As formal theory, generativity theory is not, strictly speaking, a theory of interconnection. Rather, it is a theory about how the probabilities of multiple behaviors change over time. The notion that distinct and separate behaviors become interconnected does not do justice to the continuous and probabilistic nature of behavior. Generativity theory predicts continuous and probabilistic changes in behavior, which one might then label *post hoc* as interconnections, but the label would only trivialize the dynamics. The second example will make this clearer.

The Two-String Problem

Epstein [10] reported a replication of Maier's [32] classic two-string problem, also known as the pendulum problem. Thirty undergraduate students served as subjects. They were shown two long strings hanging from a high ceiling 4.8 m apart and instructed to tie the ends of the strings together. One of two small objects was also pointed out to them, which, they were told, they could use to assist them in connecting the strings. Half the subjects were shown a small, heavy black cylinder about the size of pack of cigarettes; it had a small metal hook at one end (Object 1). The other subjects saw the same black cylinder with a long (39 cm) black rod extending from it, to which was attached a large hook (Object 5).

The two strings were far enough apart so that both could not be reached at the same time. Extending one string as far as possible toward the other and then extending one's reach with either of the objects would also not succeed; even Object 5 was not long enough to reach the other string. The problem could be solved by attaching either object to one string, swinging it forcefully, retrieving the other string and pulling it as far as possible toward the swinging string, catching the swinging string, and then, finally, attaching the two strings.

Before conducting the experiment, we asked 148 students who were not subsequently involved in the experiment to tell

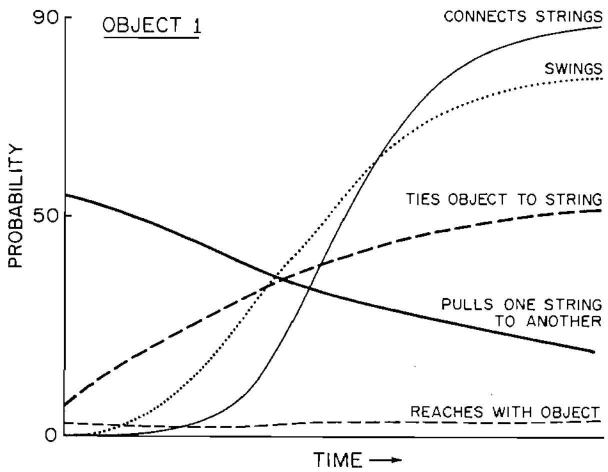

FIG. 1. A probability profile generated by the transformation functions described in the text, shown for five behaviors relevant to Maier's [32] two-string problem. The profile was generated with parameters for the short object (Object 1), which produced rapid solutions to the problem and no irrelevant reaching. Note that pulling one string to the other decreases steadily in probability and that other behaviors increase in probability in an orderly sequence. Tying the object to the string makes swinging more likely, which, in turn, makes connecting the strings more likely.

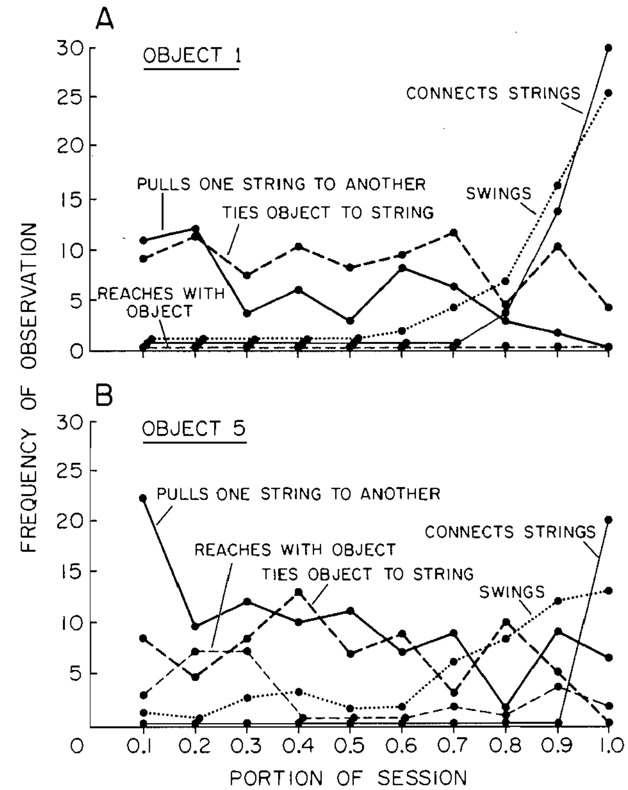

FIG. 2. The frequency with which two observers recorded the appearance of five of the behaviors that were monitored in the experiment described in the text. Plotted are the sums of their observations during each tenth of a session. Panel A shows the results with 15 subjects who were given access to the short object (Object 1), and Panel B shows the results with 15 subjects who were given access to the long object (Object 5). Note the similarity of these curves to those generated by the transformation functions (Fig. 1). In general, pulling one string toward the other decreases steadily in frequency. Tying the object to the string appears at a low frequency throughout the session. Swinging increases in frequency toward the end of the session, which, in turn, allows the subjects to connect the strings. Also note the appearance of reaching early in the sessions recorded in Panel B.

us how they might use the two objects. Eighty percent of the 71 students who were shown Object 1 said they would use it as a weight ("paperweight," "pendulum weight," and so on), and none suggested using it to extend his or her reach. Similarly, more than 80 percent of the 77 students who were shown Object 5 said they would use it to extend their reach ("back scratcher," "pull curtains," and so on), and none suggested using it as a weight. We therefore predicted that Object 1 would lead to more rapid solutions than would Object 5, because the latter would presumably produce attempts to solve the problem by reaching.

Two observers monitored nine behaviors continuously. The behaviors were selected and defined to make them as discriminable as possible. The observers listened through earphones to a common tape that allowed them to synchronize their observations in 15 sec intervals.

Results. As predicted, solutions occurred much more smoothly and rapidly with the short object. The average solution time with Object 1 was 2.75 min, and all of the subjects solved the problem within the 15 min allotted. Only 11 of the 15 subjects in the other group solved the problem within the allotted time. Allowing those who failed solution times of 15 min, the average solution time with Object 5 was 7.25 min.

The data also suggested that the latter group performed poorly because, as predicted, Object 5 produced irrelevant reaches. Reaching was observed 15 times with the long object and not once with Object 1. The probability of reaching with Object 5 within 30 sec of having picked it up or having tied it to a string was 0.21. In most other respects, transitional probabilities were nearly equal for the two groups.

Film segments and transitional probability data suggested many examples of resurgence, automatic chaining, and so on. For example, many subjects appeared to set the object in motion "accidentally" just before they swung it forcefully, which suggests automatic chaining. Indeed, Maier [32] reported that subjects who had difficulty with the problem often solved it quickly after he casually brushed up against a string (and thus set it in motion). We observed several instances in which subjects tied the object to the string and then, after several minutes without a solution, carefully put the object down, apparently because they had "given up." By releasing the object, they invariably created a slow-moving pendulum, after which they solved the problem rapidly.

Predictions. A more powerful analysis of the performance is possible. As mentioned earlier, generativity theory asserts that behavior is novel and probabilistic because of the continuous and simultaneous operations of a variety of transformation phenomena. Below appear four simple transformation functions, described earlier, that have proved useful in predicting ongoing, novel performances. The functions correspond roughly to phenomena that have been studied empirically under the labels "extinction," "reinforcement," "resurgence," and "automatic chaining," and they are therefore so labeled. However, four basic functions of this sort might have been selected merely on theoretical grounds, since they describe four basic relationships between events in behavior and the environment.

In the equations below, y_n is the probability of behavior y at cycle n of the algorithm, y'_n is the probability of behavior y' at cycle n of the algorithm, ϵ is a constant for extinction (it determines the rate at which the probability of behavior y decreases over cycles of the algorithm), α is a constant for reinforcement (it determines the rate at which the probability of behavior y increases over cycles of the algorithm as a result of certain environmental events), and $\lambda_{yy'}$ is the constant of interaction between behaviors y and y'.

(1) Extinction:	$y_{n+1} = y_n - y_n * \epsilon$
(2) Reinforcement:	$y_{n+1} = y_n + (1 - y_n) * \alpha$
(3) Resurgence:	for $\lambda_{yy'} < 0$ and $y'_n - y'_{n-1} < 0$, $y_{n+1} = y_n + (1 - y_n) * (-\lambda_{yy'}) * y'_n$
(4) Automatic Chaining:	for $\lambda_{yy'} > 0$ and $y'_n - y'_{n-1} > 0$, $y_{n+1} = y_n + (1 - y_n) * \lambda_{yy'} * y'_n$

Figure 1 shows a portion of the probability profile generated by these equations for the two-string problem. Since reinforcers were not presented (which is typical of situations we label "problems"), the value of α was 0, and therefore equation 2 did not contribute to the outcome. Initial probabilities were determined by data, ϵ was estimated (the same ϵ was used for all of the behaviors), and each of the λ's was estimated.

Remarkably, the model generated a solution to the two-string problem (Fig. 1). Moreover, the pattern of overlapping probabilities generated by the equations resembles the frequency data obtained with actual subjects (Fig. 2).

Advances

Generativity theory has led to several advances: First, it has allowed us to engineer complicated, intelligent performances in simple organisms. For example, Epstein and Medalie [16] reported a solution to a stick-type problem with a pigeon, and Epstein [11] reported a solution to the box-and-banana problem by the interconnection of three repertoires. Moreover, Epstein [13] reported the solution to an even more complicated problem (involving the opening of a door) by the interconnection of four repertoires of behavior in a pigeon.

Second, it has led to a formal, empirically-based model of ongoing behavior. And third, the model has proved reasonably successful in predicting ongoing, intelligent performances in human subjects.

ACT THREE: TWO DEPARTMENTS

In 1983, with Paul T. Andronis and T. V. Layng of The University of Chicago, I helped found a society whose purpose is to establish a new, interdisciplinary science devoted to the study of behavior. The Praxics Society is growing, and, at this writing, faculty members at three universities have expressed interest in establishing programs in praxics at their universities. Among other projects, the Society is working to establish a *Science*-like journal called *Praxics*, which will serve the needs of the many scientists—in evolutionary biology, behavior analysis, behavioral genetics, ethology, ecology, neurophysiology, comparative psychology, and so on—who are concerned with the determinants of behavior.

With luck, we may soon reach Act Three of the peculiar and rather belabored drama that McDougall and Watson initiated more than seventy years ago. A science of behavior will finally come to life, and psychology will be free of its intruders.

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