General Article

SKINNER, CREATIVITY, AND THE PROBLEM OF SPONTANEOUS BEHAVIOR

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Behavior is generative, by which I mean that it is probabilistic, continuous in time, and always novel. At first glance, B.F. Skinner's work would seem to make contact with generative aspects of behavior, since he studied the "emitted" behavior of "freely moving organisms," since he analyzed language, music, literature, and other creative activities, and since he himself was an exceptionally creative individual. In fact, Skinner's work focuses on the effects of various interventions *on ongoing behavior; it says little about where that behavior comes from in the first place. Generativity theory suggests that simple behavioral processes of the sort Skinner studied operate simultaneously on the probabilities of a large number of different behaviors. Instantiated in a computer model, the theory has successfully emulated complex, novel performances in both human and animal subjects, and it may some day allow for the real-time simulation of novel performances in individual human subjects.*

In Memoriam

Burrhus Frederic Skinner

1904–1990

PS *dedicates this issue to the memory of a preeminent psychological scientist and charter member and William James Fellow of the American Psychological Society, noting the occasion with a commissioned article on a topic exemplified by Skinner's life and work.*

Spontaneity is only a term for man's ignorance of the gods.

—Samuel Butler

First occurrences have an air of magic about them: your first kiss, your child's first word, your first publication. The behavior of organisms has many firsts, so many, in fact, that it's not clear that there are any seconds. We continually do new things, some profound, some trivial. We "solve problems," which by definition means we're doing new things in situations we've never faced before. We write poems and improvise on the piano and devise scientific theories. We speak new utterances all the time, even, sometimes, in faculty meetings.

When you look closely enough, behavior that appears to have been repeated proves to be novel in some fashion. If you say the word "pigeon" several times, a spectrograph will show clear differences in each occurrence (and, as I can testify from personal experience, passersby are likely to point to you and mutter *behaviorist*). You never brush your teeth exactly the same way twice, and even the rat's lever press varies in subtle ways with each occurrence. Variability is typical of all behavior, from nystagmus in our eyes to the slight tremor in our hands. Even if, somehow, you could repeat some response precisely, it would still be novel in the sense that each occurrence is the product of a changed organism. Curiously, we are relatively insensitive to variability and novelty in our behavior, even to the extent that concepts in the behavioral sciences may have been compromised by our insensitivity as scientists to such variability and novelty (Epstein, 1982).

Behavior is also fluid and continuous. We speak of a lever press as if it were a discrete entity, but it is not. The rat moves from the feeder to the lever, one or both of its front paws move toward the lever as it flicks its tail, a paw depresses the lever and slides off as the rat moves its head from side to side and twitches its whiskers, the rat moves away, and so on. The click of a microswitch suggests, falsely, that a discrete "response" has occurred, but the rat is active continuously, and what occurs is multidimensional and complex. The operation of a feeder—the delivery of a "reinforcer"—does not simply "strengthen a response"; rather, it impacts the flow of behavior in complex ways. Just as we are often insensitive to novelty and variability in behavior, we are also insensitive to continuity. We hear discrete "words" in a spoken sentence, for example, but the acoustic signal is typically continuous.

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Finally, behavior is inherently probabilistic. A large number of factors converge continuously on an always-active nervous system to produce behavior. As thresholds are passed and firing rates increase, circuits controlling the occurrence of many different behaviors are activated. The behavior you actually see is the result of a complicated numbers game. People are quick to agree that it's difficult to predict what someone will do or say next, but that's not what I mean by probabilistic. Probabilistic systems, even chaotic ones, may be highly predictable and easy to describe mathematically. I mean rather that behavior is the result of a very complicated process that is in part stochastic. Focusing on one instance or one dimension does not do justice to the system.

I use the term *generative* to denote these three aspects of behavior: that behavior is novel, continuous, and probabilistic. Various scientists and theoreticians have been concerned with generativity in various ways and in various contexts (e.g., Arieti, 1976; Chomsky, 1965; Sternberg, 1988; Wertheimer, 1945). In this essay, I look at the issue narrowly, first by examining generativity in the context of B.F. Skinner's work, and then by summarizing my own work in this area.

B.F. SKINNER

A Contradiction

Skinner is well known for two positions that bear on generativity and that appear to contradict each other. On the one hand, central to his work was the distinction he drew between operant and respondent behavior. Operant behavior is "emitted," he said, whereas respondent or reflex behavior is "elicited" or "drawn out" by a specific stimulus (Skinner, 1938). Operant behavior has no obvious eliciting stimulus; it is, by definition, the kind of behavior usually called "spontaneous" (Skinner, 1938, pp. 19–20). To study such behavior, Skinner avoided using "reflex preparations" in which the movements of animals are constrained; instead, he studied the behavior of the whole, freely moving organism. Operant behavior is surely generative, and Skinner certainly observed many generative phenomena.

On the other hand, Skinner didn't believe in spontaneity, and, although he used the word occasionally, he usually put quotation marks around it. He was, indeed, a strict determinist, attributing all behavior to our genetic endowments and environmental histories (Skinner, 1955–56, 1971, 1989), with most of his career devoted to the study of the latter. He believed that he had fully reconciled these two positions—his belief in the active organism and his belief in determinism—through his use of selection as a causal mode (Skinner, 1981a). Behavior that appears to be spontaneous is part of a "class" of responses that has been selected by past reinforcers, said Skinner, just as a new species is part of a class of organisms that has been selected by contingencies of survival in evolution. True, operant behavior has no obvious eliciting stimulus, but it is occurring now because similar behavior (members of the response "class") has been reinforced in the past.

There is a problem here, and it's simple enough to state, although Skinner himself never seemed concerned about it. Selection alone doesn't produce anything new in evolution. Mechanisms of variation are also necessary. Selection merely limits the range of variation that occurs in the next generation. Similarly, reinforcement doesn't produce any of the particular behavior variants from which it may select (except to the extent that it is acting as an eliciting or discriminative stimulus, but these cases are not pertinent to Skinner's position). Before behavior can be selected in ontogeny, it must somehow be *generated* (cf. Segal, 1972; Staddon, 1975). Mechanisms of variability must exist, some relatively trivial, perhaps, and some profound. To rely on so-called "random" variation is by no means enough to account for the dramatic and complex instances of novelty we often observe in behavior; *Beyond Freedom and Dignity* (Skinner, 1971) was not the product of random variations of spoken or written English. To put it another way, Skinner's deterministic dyad always needed another factor: Behavior is determined by genes, environmental history, and certain *mechanisms of variability.*

Shaping

Skinner named and popularized the technique of reinforcing successive approximations, the "shaping" technique. Without mechanisms of variability—indeed, without fairly orderly mechanisms of variability—shaping could not work. The textbook account of shaping oversimplifies the process. Here is a slightly more detailed view:

You'd like a hungry pigeon to turn in circles. You wait for almost any approximation at first, say, turning the head to the right. Then you immediately operate a feeder, and the pigeon eats. You operated the feeder following a slight head turn, but other behavior was undoubtedly reinforced, as well: The pigeon may have been lifting a wing, stepping, and opening its beak just as it turned its head. *A great deal of irrelevant behavior is always captured by reinforcement.* You may also have inadvertently strengthened one or more *sequences* of behavior: The pigeon may have pecked a spot on the wall just before it turned its head, and you may have strengthened pecking-and-turning. The pigeon also continues to engage in *other* behavior: It walks, flaps its wing, and so on. Many behaviors seem to be competing with each other, and your reinforcer seems merely to have altered the distribution in some complicated way.

Among other things, you will probably see the pigeon turn its head again. When you see the pigeon turn its head a little farther than it did before, you operate the feeder. Again, you observe many different behaviors—more stepping, partial turns, wing lifts, and so on—and you continue to "increase your requirement." Old behaviors continue to appear in some fashion, and you also observe various new forms appearing, along with variants of many of the forms you have (deliberately or inadvertently) reinforced. If you continue to operate the feeder at judicious moments, within a few minutes the pigeon will turn in full turns—while continuing to engage in other behaviors, as well.

Where did the orderly variants come from, and could we have predicted them? It's not enough to know what we thought we were reinforcing; we had multiple effects on the flow of behavior, and many new forms turned up, not simply "random variants" of specific response forms. Note that *without the new behavior, we could not have proceeded with the shaping process*. Could we have predicted, precisely, what new behaviors would occur, moment to moment? What principles would allow us to make such predictions?[1]

Reinforcement may, in some sense, alter the probability of a response, but *where does the response come from in the first place*?

Probability

Throughout his long career, Skinner spoke of the probability of responding, but, early on, he concluded that the concept of probability per se had limited usefulness in the study of behavior (Skinner, 1953, p. 62). It was not directly measurable, for one thing, and it was a *statistical* idea, always unsavory to Skinner (see Skinner, 1964). In the early years he spoke of "strength" of responding, which he was careful to define in physical terms. The strength of a reflex, for example, could be defined in terms of latency or threshold values (Skinner, 1931, 1932, 1938). Ultimately, he settled on *rate of responding* as the ideal measure of the "strength of an operant" (Skinner, 1938, p. 58). To Skinner, science could not proceed without a repeatable unit, and the occurrence or nonoccurrence of a particular instance of operant behavior, normally defined in terms of a simple switch closure, was just the thing (cf. Skinner, 1935).

Let me recast this argument to suit the present discussion: We'd like to get at probability directly, but we can't, so we will limit our discussion to "response strength." "Frequency" is our best measure of the strength of so-called "spontaneous" or "emitted" (that is, "operant") behavior. Thus we shift from *probability,* a fairly abstract concept, to *strength,* defined in physical terms in various ways, to *frequency,* normally defined in terms of switch closures.

Some chance events led young Skinner to invent a simple device for recording frequency data in real time in a powerful form: the cumulative record (see Skinner, 1956). If he had had camcorders and computers at his disposal, would he have settled for this? Would he have abandoned probability in favor of frequency?

Fluidity

Skinner is often portrayed as a stimulus–response psychologist. He objected strongly to this sort of portrayal (e.g., Skinner, 1974), mainly because it suggested that he was a Pavlovian, which he certainly was not. Skinner even had reservations about the usefulness of the very concepts "stimulus" and "response," although he employed them throughout his career. Skinner (1935) recognized the fluidity that exists on both sides of the equation. He proposed to define a response in terms of its *function*—its effect on the world—rather than in terms of its appearance, in order to approximate more closely "the natural lines of fracture along which behavior and environment actually break" (p. 40). In a published interview in the 1960s, he even rejected the concept of response almost entirely: "As it stands, I'm not sure that response is a very useful concept. Behavior is very fluid; it isn't made up of lots of little responses packed together. I hope I will live to see a formulation which will take this fluidity into account" (quoted in Evans, 1968, pp. 20–21).

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[1] In a substantive paper on the "provenance of operants," Segal (1972), following Skinner, asserts that reinforcement strengthens a "class" of responses which includes all of the variants, including the novel forms (e.g., p. 5). The assertion does not lend itself to falsification, unfortunately, and it doesn't help us to know exactly which responses will be in this new "class." No principles or mechanisms are given that would allow us to specify the new members. The class idea itself is suspect, because it can't easily handle the dynamic and ever-changing nature of behavior. One reinforcer may change behavior in certain ways, the next in new ways, the third in still other ways, and, even without reinforcers, behavior continues to unfold in new and interesting ways. It seems preferable to try to state these ways precisely and to specify the dynamic mechanisms, rather than to say simply that a different hypothetical "class" of behaviors is lighting up with each reinforcer, as if that solves the problem. Segal also notes that novel behavior of evolutionary significance can be produced by other procedures besides shaping: deprivation, certain schedules of reinforcement and punishment, the presentation of releasers, and so on. All such behaviors are grist for the generativity mill, as I argue below.

Creativity

Skinner never studied creativity per se, but he was fascinated by it, and he himself was a study in creativity. Before graduate school he had planned to become a creative writer, and he even received a warm letter of praise from Robert Frost for early compositions (Skinner, 1976, pp. 248–249). As a psychologist, he roamed the creative field: new laboratory equipment and methods (e.g., Skinner, 1956), now widely used; an enclosed crib for babies (Skinner, 1945); a secret, pigeon-guided missile nose cone for the military (Skinner, 1960); a utopian novel (Skinner, 1948); analyses of great works of art, literature, and music (Epstein, 1980; Skinner, 1939, 1941, 1957); new teaching devices and methods (Skinner, 1968). At home Skinner was always tinkering, modifying, inventing, always improving the space around him to make it easier to work and relax. As I write this essay nearly a year after his death, his basement study is still enmeshed by wires and strings attached to oddly shaped gizmos: a counter-weighted magnifying glass (to help him read), a crude tray to hold the television remote control (so he wouldn't lose it), a mechanical finger (to push the pause button on his tape recorder when the phone rang).

But his few explicit commentaries on the creative process (Skinner, 1956, 1957, 1966, 1970, 1972, 1981b) shed little light on that process. In his autobiographical "Case History" paper, we learn about the role that fortunate chance events had in the discovery process, but creative leaps just seemed to happen. Describing the events leading to the invention of the cumulative recorder and, it would seem, to his passion for frequency data, Skinner writes, "One day it occurred to me that if I wound string around the spindle and allowed it to unwind as the magazine was emptied, I would get a different kind of record" (Skinner, 1956, p. 225). Was the creative process so mysterious, even to Skinner, that nothing more could be said? In *Verbal Behavior,* Skinner (1957) speculates that new world blends can come about when "multiple variables" strengthen several "word fragments" simultaneously; the result is "usually nonsense" (p. 303). In general, Skinner (1957) says little about novelty in either speech production or comprehension, one of the complaints leveled against *Verbal Behavior* by Chomsky (1959). In "Creating the Creative Artist," Skinner (1970) argues that society can and should encourage artistic endeavors by providing appropriate reinforcers. He attributes creativity itself to random "mutations," and he is skeptical about being able to discover the details:

> Many of these [mutations] are accidental in the sense that they arise from conditions which we cannot now identify in the genetic and environmental histories of the artist and from unpredictable details of his working methods and conditions. We may not like to credit any aspect of a successful painting to chance, but if we are willing to admit that chance does make a contribution, we can take steps to improve the chances (pp. 69–70).

In "A Lecture On 'Having a Poem,'" Skinner (1972) compared the act of creating a poem to the act of having a child, arguing that in each case the creator is just a "locus" through which environmental variables act; the creator adds nothing to the creation.[2] How, specifically, a particular poem comes about is not stated. In "How to Discover What You Have to Say," Skinner (1981b) gives excellent tips on how to stimulate and preserve one's new ideas, but what new ideas are likely to turn up, and why?

Of special note are Skinner's (1966) comments in a paper on problem solving: "Solving a problem is a behavioral event. The various kinds of activities which further the appearance of a solution are all forms of behavior. *The course followed in moving toward a solution does not, however, necessarily reflect an important behavioral process*" (p. 240, italics added).

Skinner recognized generative aspects of behavior but did not see generativity per se as a problem worthy of study or analysis. He knew that behavior was fluid, probabilistic, and at least sometimes novel, but he did not know how to advance an analysis of behavior without positing a recurring unit; hence the need to divide up behavior into "lots of little [recurring] responses." In a sense, Skinner took generativity for granted, relying on broad-brush explanations of creativity ("chance," "mutations") or on no explanations at all ("One day it occurred to me")—even suggesting that the creative process was not "important." This fit his two-factor form of determinism. Nontrivial mechanisms of variation might have made the organism seem a little too autonomous for Skinner's liking. Ironically, virtually all of operant psychology revolves around spontaneous behavior; without it, we would never have anything new to reinforce.

GENERATIVITY

Combinations

Creativity has been said by many to be the result of a combinatorial process (Arieti, 1976; Bingham, 1929; Chomsky, 1965; Gardner, 1982; Hull, 1935; Koestler, 1964; Maier & Schneirla, 1935; Sternberg, 1988; Wert-

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[2] Publically, Skinner took a strong empiricist stand on creativity, but in private his views seemed more balanced. We had an amusing exchange one day about his claim that he was creative and inquisitive because, as a boy, he had found "something interesting under every rock." I asked whether other boys accompanied him on his walks through the woods, and he said yes. I asked whether any of the other boys had made interesting discoveries under rocks, and he started to smile and said yes. "Well," I said, "where are those boys now?" He grinned broadly and replied, "*Probably driving trucks.*"

heimer, 1945). For example, Rothenberg, a psychiatrist, describes creativity as a "Janusian" process, after Janus, the god of two faces. New ideas result from "the capacity to conceive and utilize two or more opposite or contradictory ideas, concepts, or images simultaneously" (Rothenberg, 1971, p. 195). Henri Poincaré, the eminent mathematician, made an important discovery one evening after having drunk too much coffee. "Ideas rose in crowds," he wrote. "I felt them collide until pairs interlocked, so to speak, making a stable combination" (Poincaré, 1946, p. 387). Stephen Jay Gould attributes his creativity to his ability to "make connections" (cited in Shekerjian, 1990); Einstein spoke of "combinatory play" in explaining his own creative ability; and the great English poet and playwright John Dryden spoke of a "confus'd mass of Thoughts, tumbling over one another in the Dark" as essential to his own creative efforts (quoted in Ghiselin, 1952).

Simulations

My own interest in combinations began, oddly enough, during research with pigeons—the so-called "Columban Simulation Project," which Skinner and I began in 1979 (Baxley, 1982; Epstein, 1981). We had found yet another way to further Skinner's longstanding campaign against cognitive psychology. With pigeons as subjects, we "simulated" human and chimpanzee performances that had been attributed to cognitive processes and offered alternative accounts of such performances in terms of contingencies of reinforcement (Epstein & Skinner, 1981; Epstein, Lanza, & Skinner, 1980, 1981).

The logic of simulations is actually fairly complicated (see Epstein, 1986), and some of our studies may have had more political value than scientific value. But the outcome of the Simulation Project was in general quite positive, mainly because it got us to look at our avian subjects in new ways and under new conditions. To Skinner's credit, he never once suggested that we use "rate of responding" to measure the extraordinary behaviors we observed as we began to consider self-awareness, symbolic communication, imitation, problem solving, cooperation and competition, morality, the use of memoranda, and other topics from a behavioral perspective. (Some of his devotées have proved to be far less flexible.) Rather, we borrowed or invented measurement techniques as we went along. In one study (Epstein & Skinner, 1981), Skinner whispered a running account of the performances into a tape recorder during critical tests; our data was the transcript of his narration.

More important, we never used closed experimental chambers; here was Skinner without the box, so to speak. The pigeons roamed free in large wire-mesh or Plexiglas chambers where complexity could be seen in all its frustrating splendor, and eventually I began filming or videotaping each performance and analyzing the recorded images, sometimes frame by frame.

The simulation research had a pattern to it. Pigeons were trained to do things that chimpanzees or people could do and then placed in new situations where, very often, new, interesting behavior would turn up that seemed typical of chimps or humans. For example, Epstein, Kirshnit, Lanza, and Rubin (1984) reported that pigeons with the right training history could solve the classic box-and-banana problem in an "insightful" fashion (Koffka, 1924; Köhler, 1925). A small facsimile of a banana was suspended out of reach of the pigeon, and a small box was placed elsewhere in the chamber. The pigeons had received food for pecking the mock banana when it was within reach. Would they use the box to reach the banana?

Each pigeon looked confused at first. It stretched repeatedly toward the mock banana, motioned toward the box, stretched again toward the banana, and so on. After a minute or so, each pigeon began, suddenly, to push the box directly toward the toy banana—sighting the banana as it pushed—stopped pushing in just the right spot, climbed, and pecked the banana.

Successful birds had had three types of training. They had learned to push the box toward small targets at ground level; they had learned to climb onto a fixed box and peck the banana overhead; and we had withheld food for jumping and flying toward the banana when it was suspended out of reach. Variations in training produced interesting, systematic variations in performance during the test situation (Epstein et al., 1984); for example, birds that had not learned to push never did so during the test and therefore could not solve the problem. Epstein and Medalie (1983) and Epstein (1985a) used similar procedures to get pigeons to solve more complicated problems, and Epstein (1987a) showed that a pigeon with five different types of training could solve a complicated problem using four separate repertoires of behavior in under 4 min. The pigeon solved a variation on the box-and-banana problem in which it first had to retrieve the box from behind a Plexiglas door.

These and other studies showed dramatically that previously established behaviors manifest themselves in new situations in new, interesting, and orderly ways. They also showed that differences in training affect new performances systematically (also see Birch, 1945; Köhler, 1925; Schiller, 1952; Shurcliff, Brown, & Stollnitz, 1971). But is that enough?

A Japanese researcher visiting my laboratory in the early 1980s seemed impressed with the performances I was generating, but he left me, albeit politely, with a disturbing question: "Where does all the new behavior come from?" I recast the question as follows: Can a rigorous, moment-to-moment account of the emergence of a novel performance be formulated?

Initial Efforts

I first attempted such accounts by using simple, empirical principles to account for changes in a novel performance as it unfolded over time, as evidenced by a videotape record. For example, the period of "confusion" evident when a pigeon is first confronted with the box-and-banana problem seems to be a simple competition of two repertoires occasioned by features of the test situation. The test situation has features common to two training situations and hence should occasion behavior with respect to the box and behavior with respect to the banana simultaneously (Epstein et al., 1984). *Multiple controlling stimuli* make repertoires compete, and the relative strength of the repertoires is determined by properties of the stimuli (see Epstein, 1990).

Other simple principles help account for other aspects of the performance. The bird stops pushing in the right place, for example, because its pushes have produced increasingly closer approximations to a stimulus the bird has seen during training—box-under-banana, the stimulus in the presence of which climbing and pecking has been reinforced with food. This is an example of a process called *automatic chaining,* or simply *autochaining.* Behavior often changes the environment in a way that changes the probability of subsequent behavior. Even a turn of the head sometimes has this effect, because it radically changes the visual field (Epstein, 1985a).

The principle of *resurgence* also proved useful in constructing this type of account. When recently reinforced behavior is no longer effective, previously reinforced behavior recurs (Enkema, Slavin, Spaeth, & Neuringer, 1972; Epstein, 1983, 1985b; Estes, 1955; Hull, 1934; Lindblom & Jenkins, 1981; Mowrer, 1940; O'Kelly, 1940; Sears, 1941; cf. Epstein & Skinner, 1980). During periods of extinction in problem-solving situations, other behaviors that were effective in the past in that setting tend to recur and compete (Epstein & Medalie, 1983), a process that is often essential to the emergence of a solution.

Strings and Functions

I have offered moment-to-moment accounts of a number of such performances using simple principles of this sort, but a more productive approach to understanding generativity began to evolve when I extended the research to human subjects, at first studying variants of Maier's (1931) pendulum problem and, more recently, a variety of mechanical problems involving building blocks, brooms, keys, and so on. As I watched many performances with people and pigeons unfold, I became increasingly aware of inadequacies in my running accounts of novel performances. First, I was dividing up the performances into arbitrary segments to fit my principles. What basis I did have for asserting that multiple controlling stimuli were operating only during the first 10 sec of a performance, resurgence only during the next 60 sec, and so on, or that only three repertoires were competing during the first few minutes and only two during the next few minutes? I had been asserting the obvious—that behavior and the environment are fluid and continuous—but I was violating my own precept. No audience or reviewer ever took me to task on this point, which made me especially wary. Second, I could make reasonably good predictions about the emergence of a novel performance, but my predictions were imprecise, as informal, verbal predictions tend to be.

My dilemma virtually demanded that I take two small steps:[3] First, I supposed that the various processes I was invoking were operating continuously in time and concurrently, and, as a corollary, that every process was operating simultaneously on the probabilities of every behavior that might occur in the situation.[4] I did not have high hopes for this conjecture, but it seemed unavoidable. Second, I cast four simple principles—extinction, reinforcement, automatic chaining, and resurgence—into linear equations in a simple state model and entered parameters describing Maier's two-string problem. A computer simulation produced surprising results. It yielded smooth, overlapping probability curves in what I have come to call a "probability profile" (Fig. 1); it yielded a reasonable, humanlike solution to the problem; and it predicted some of the dynamics of frequency data obtained with human subjects (Epstein, 1985c, 1990).

Producing Multiple Repertoires

Multiple repertoires of behavior would indeed seem to be the stuff of creativity, and generativity theory may be helpful in specifying how repertoires compete and interact over time. New sequences and new topographies result from such interactions, with the resultants immediately available as new components in the generative process. Presumably any repertoires of behavior, established or induced by any means, can feed this process.

Circumstances that produce multiple repertoires of behavior would seem to be of special value in driving the

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[3] Generative principles have been helpful in accounting for advances in my own thinking, but there is inadequate space here to attempt such a discussion. Viewed as covert perceptual and verbal behavior, thinking is wholly amenable to the kind of analysis offered here (Epstein, 1991).

[4] This system seemed simpler than the arbitrary one I had been employing, and it also seemed to have a far better fit to the nervous system.

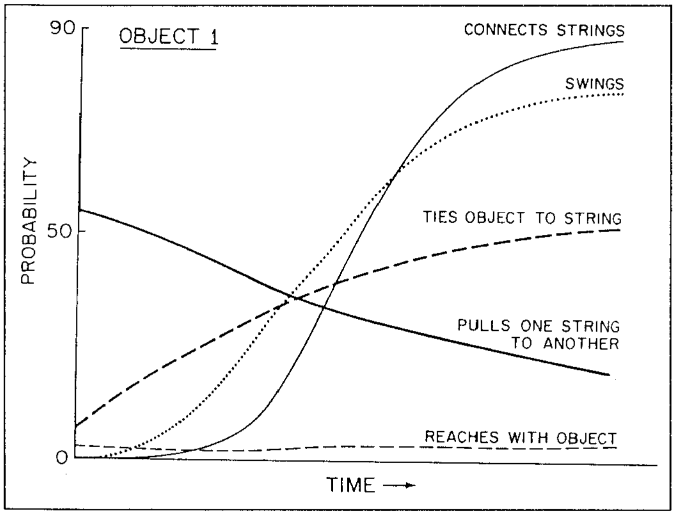

Fig. 1. A probability profile, shown for five behaviors relevant to a variant of Maier's (1931) two-string problem. The subject's task is to connect two long strings that are hanging from a ceiling, but both strings cannot be reached at the same time. A small hooklike object is present that the subject can use if he or she wishes. The solution is to tie the object to either string, set it in motion, extend the other string toward the swinging string, catch the swinging string, and tie. This profile was generated using the transformation functions mentioned in the test with parameters for an object that produced rapid solutions to the problem and no irrelevant reaching (Epstein, 1990). 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.

process, and two phenomena are especially notable. In the natural environment, multiple controlling stimuli abound, and failure is not uncommon. The multiple stimuli produce multiple repertoires of behavior directly and the failures indirectly, through the resurgence of previously established behaviors. Thus the real world is a rich source of generativity. Other mechanisms may also spur the process: states of deprivation, complex instructions, releasers, intermittent schedules of reinforcement, modeling, dietary factors, and so on (cf. Segal, 1972).

Generativity and Shaping

A rigorous analysis of shaping would seem to be within reach. Critical to the shaping procedure is the repeated and systematic withholding of reinforcement. The animal keeps succeeding briefly and then failing for a while. Failure is inducing a resurgence of previously established behaviors, including earlier forms that have been captured during the shaping process itself, a common observation during shaping (Pryor, Haag, & O'Reilly, 1969; Staddon & Simmelhag, 1971). New forms evident during the shaping process are not merely random variants but are resultants of competing repertoires, some of which blend to form increasingly exaggerated forms. This approach lends itself to formal analysis, and, if successful, such an analysis will account not only for changes in the target behavior but also for the dynamics of the many behaviors that appear during the shaping procedure.[5]

Practical Implications

Generative phenomena are undoubtedly affected by individual differences—in speed of acquisition and transformations, the number of repertoires that can be supported simultaneously, emotional factors, and so on. But the bottom line may be that these processes are operating all the time in everyone, meaning that in a very real sense we are all creative. People labeled "creative" by society may simply be producing more valued products (Csikszentmihályi, 1990; Glover, 1980), or they may have certain skills that enhance generative processes or better utilize the output of such processes (Guilford, 1962; Shekerjian, 1990; Simonton, 1984; Skinner, 1981b; Torrance, 1971).

Several practices follow directly from generativity theory as a means to enhance creativity. The most important is to capture some of the new that is being generated all the time. Artists carry sketchpads and writers carry notebooks for this purpose. Finding conditions under which one can take the time to pay attention to competing repertoires is also important, and one can enhance the competition by acquiring new skills and knowledge (thus increasing the number of repertoires available to compete), by exposing oneself to diverse and changing situations (roughly, multiple controlling stimuli), and by exposing oneself to new challenges (and the possibility of extinction-induced resurgence).

Real-Time Simulation

I am currently working with several students to develop software that may allow for the real-time simulation of the behavior of individual human subjects in a simple situation. At one terminal the subject performs a simple task: pushing buttons to move a dot across the screen. At terminals linked to the subject's terminal, we will see a probability profile showing overlapping probability curves for each of the buttons, the predicted path of the dot, and statistics comparing our predictions to chance predictions in real time. Every press of a button will alter the profile and our predictions. It may be pos-

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[5] The approach also seems consistent with the observation that shaping occurs more rapidly in adult organisms than in young organisms (Segal, 1972). A far greater number of repertoires are available to resurge and complete in the adult.

sible in this situation to stay ahead of the subject by several seconds or more, even when the functions of the buttons are so complex that casual observers—and even the subjects themselves—cannot make accurate predictions. A simulation of this sort, if successful, will further validate the approach to understanding ongoing behavior that has been outlined in this essay. It may also lead to applications of generativity theory in artificial intelligence.

Interventions

Reinforcement, punishment, extinction, time out, instructions, modeling, prompting, manual guidance, and so on—the kinds of procedures studied and developed by Skinner and his students—are not generative mechanisms per se. Rather, they are *interventions* that interrupt and redirect the flow of behavior by altering the probabilities of many different behaviors (cf. Dunham & Grantmyre, 1982; Thompson & Lubinski, 1986). Even simple interventions necessarily have multiple and complex effects, although our procedures may, unfortunately, lead us to overlook complexity in many situations. In a way, interventions are the exception and generativity is the rule, for without interventions organisms continue to behave in new and interesting ways indefinitely. The organism is truly active, even if the activity of organisms proves to be wholly orderly and predictable.

Am I suggesting that determinism, or at least Skinner's brand of determinism, is dead? I'd prefer to sidestep the question with an assertion, one I have been making for several years (Epstein, 1984, 1987b). *Isms* are common in the early stages of a science, but they are damaging in the long run. *Determinism, behaviorism, environmentalism, nativism*—all are distractions, really. It will take the joint efforts of many scientists in several fields to advance an effective understanding of human behavior, by far the most complex subject matter in all the sciences. The time has come to proceed in this worthy endeavor without ideology or *ism,* as colleagues with a common purpose.

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