

ON THE REDISCOVERY OF THE PRINCIPLE OF RESURGENCE

SOBRE EL REDESCUBRIMIENTO DEL PRINCIPIO DE RESURGIMIENTO

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Abstract

“When behavior that was recently reinforced is no longer reinforced, other behavior that was reinforced under similar stimulus conditions tends to recur.” This fundamental behavioral principle was first formulated in the early 1900s but subsequently disappeared almost entirely from the scientific literature on behavior. Faced with some puzzling phenomena I had observed in pigeon laboratories in the early 1980s, I formulated this principle as a way of explaining these phenomena. It is notable that the process described by this principle probably played an important role in generating the first occurrence of this principle in my own covert verbal behavior. The principle has considerable explanatory power. It sheds light on ubiquitous phenomena such as shaping and extinction, helps to explain a variety of otherwise mysterious results found in the published literature on behavior, has possible clinical applications, and plays an important role in the emergence of novel behavior.

Keywords: resurgence, generativity theory, extinction, shaping, Columban simulations

Resumen

“Cuando la conducta que se reforzó recientemente ya no se refuerza, otra conducta que fue reforzada bajo condiciones de estímulo similares tiende a recurrir.” Este prin-

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cipio fundamental de la conducta fue formulado por primera vez a principios de los 1900s pero subsecuentemente desapareció casi por completo de la literatura científica de la conducta. Frente a algunos fenómenos confusos que yo había observado en los laboratorios de palomas a principios de los 1800s, formulé este principio como una forma para explicar estos fenómenos. Es notable que el proceso descrito por este principio probablemente jugó un papel importante en la generación de la primera ocurrencia de este principio en mi propia conducta verbal encubierta. El principio tiene un poder explicativo considerable. Ayuda a aclarar fenómenos ubicuos tales como el moldeamiento y la extinción, ayuda a explicar una variedad de resultados publicados en la literatura sobre la conducta que podrían ser misteriosos, tiene posibles aplicaciones clínicas y juega un papel importante en el surgimiento de conducta novedosa.

Palabras clave: resurgimiento, teoría de la generatividad, extinción, moldeamiento, simulaciones Columba

If there be nothing new, but that which is
Hath been before, how are our brains beguil'd,
Which, labouring for invention, bear amiss
The second burden of a former child.

—William Shakespeare

If, in 1905, Albert Einstein had emerged from the dark and dusty corridors of the Swiss patent office, Herculean mental labors complete and manuscript in hand, only to find that the special theory of relativity had already been proposed, he might have tossed his manuscript into the nearest trash bin and returned to his dreary job, never to emerge again. That is what usually happens with reinvention —except sometimes.

I had better luck. When, one day in 1981, I emerged from my pigeon laboratory in Boston, Massachusetts with the principle of resurgence echoing loudly in my head, I had the good fortune to find that even though I wasn't the first person to conceive of the principle, it was completely absent from every textbook on behavior I could find at the time (e.g., Bower & Hilgard, 1981; Catania, 1979; Donahoe & Wessells, 1980; Fantino & Logan, 1979; Ferster, Culbertson, & Boren, 1975; Hintzman, 1978; Karen, 1974; O'Leary & Wilson, 1975; Rachlin, 1976; Reynolds, 1975). The principle had been clearly formulated and even tested to some extent in the early and mid

-1900s by such luminaries as Clark Hull (e.g., Hull, 1934, 1952), but it somehow disappeared. The more I learned about this phenomenon from my own experiments and the historical literature, the more astonished I became by this disappearance. This is because the principle of resurgence is powerful—one of only a handful of fundamental laws of behavior, in my view. It not only helped me explain some puzzling phenomena I had been observing in the laboratory for several years, it also proved helpful in explaining a variety of different findings in published studies, and it provided significant insights regarding ubiquitous phenomena such as extinction and shaping. It also proved to be one of the pillars of *generativity theory*, a formal, predictive, empirical theory of novel behavior I proposed in 1985 (Epstein, 1985a, 1990, 1991, 1996a, 1999, 2014, 2015). Resurgence is one of the key behavioral phenomena underlying creativity because it is one of only two behavioral phenomena which is guaranteed to get multiple repertoires of behavior competing with each other, the other being multiple controlling stimuli.

In this essay, I will try to set the stage for the contemporary research described in the papers in this special issue of the *Mexican Journal of Behavior Analysis* by explaining how and why I came to formulate the principle of resurgence and by describing experiments I conducted to try to understand it. It is notable that the behavioral process described by the principle may have been responsible for the first occurrence of the principle in my own covert verbal behavior. Looking back, I still find that to be extraordinary.

A Puzzling Autoshaping Experiment

In the summer of 1977, while enrolled as a graduate student at the University of Maryland in Baltimore County, I volunteered to work with Professor Skinner at Harvard. Technically retired, he hadn't conducted research for many years, but we soon started an ambitious research program we called the Columban Simulation Project in which we were successful at getting pigeons to engage in complex behavior often said to be indicative of higher mental processes in humans (Epstein 1981, 1984a, 1996a).¹

Although Skinner didn't have a laboratory, we soon created one from scavenged parts, remnants of a lab that had been shut down in the Washington, D.C. area, and devices we built in the basement of his home in Cambridge. We conducted a variety of experiments, including ones on "Symbolic Communication between Two Pigeons,"

¹ To watch a 30-minute film about the Columban Simulation Project, visit <http://www.youtube.com/watch?v=QKSvu3mj-14>.

(Epstein, Lanza, & Skinner, 1980), "Self-Awareness in the Pigeon" (Epstein, Lanza, & Skinner, 1981), and "The Spontaneous Use of Memoranda by Pigeons" (Epstein & Skinner, 1981). We also took on more serious challenges at times, including one of trying to figure out why autoshaping worked. Brown and Jenkins (1968) had shown that when the brief illumination of the key light in a pigeon chamber reliably preceded food delivery, pigeons would, after repeated daily sessions, reliably start pecking the key. Recalling a phenomenon he had observed in his laboratory in 1946, Skinner suggested to me one day that this might be because of evolutionary programming that somehow causes pigeons to drive reliable predictors of food toward a food source.

To test this idea, we scavenged detritus from his basement—including the motor from an old record player, an empty spool of thread, and an adding machine cover—to build a panel on which a pigeon would see a spot of light move from right to left toward a feeder and on which we could record, at least roughly, the direction of the pecks as a pigeon's beak struck the clear piece of plastic through which it was seeing the spot. If the pigeon were, in a sense, *driving* the spot toward the feeder, we would detect that by the directionality of the pecks.

In the experiment we subsequently conducted with two pigeons, both began pecking at the moving spot during the very first session in which the spot and food were paired, but we didn't see much directionality in the pecks, even after dozens of daily sessions. The hypothesis appeared to be wrong, in other words. After 52 daily 1-hour sessions, by which time response rates for each pigeon were greater than 150 responses per minute, we decided, somewhat arbitrarily, to implement a standard control procedure to ensure that the pairing of the light and spot was responsible for the pecking. Specifically, we now presented the food and spot independently of each other but at the same overall rate they had been presented previously. After about a dozen sessions of this condition, responding to the spot was all but eliminated, but one of the pigeons continued to peck the spot at about 15 responses per min and the other at about 5 responses per min.

This bothered me. So when I went to the lab the next night, I disconnected the feeder. Presenting the spot alone would, I reasoned, get rid of those last few pecks, which might have been maintained, I thought, by the occasional coincidental pairing of pecking with food delivery.

I placed the pigeons in their respective chambers, which were enclosed in large opaque sound-attenuating enclosures, and started the session. Over the next few minutes I heard a few clicks from the relay racks, indicating that the pigeons were continuing to peck the spot at a low rate. But about 10 min into the session, one pigeon started pecking the spot continuously and at a fairly high rate (Figure 1). A few minutes later, the second pigeon started doing the same thing. I thought there had been an

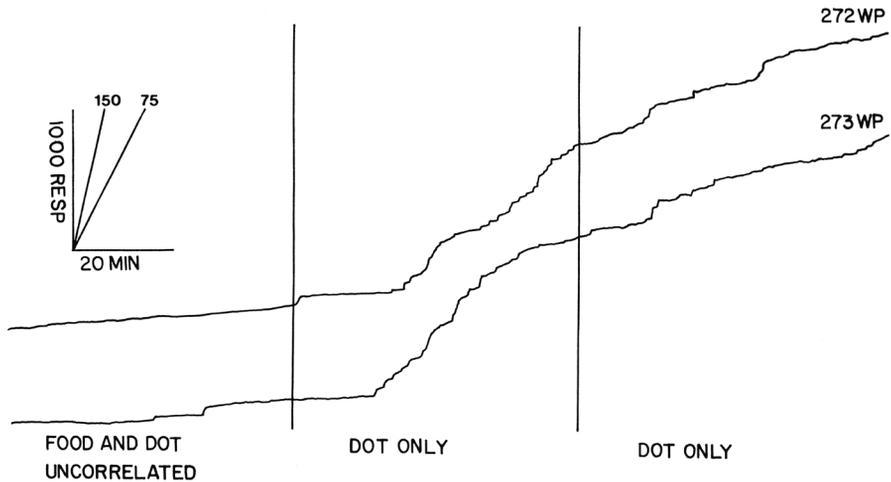


Figure 1. Cumulative record segments for the two subjects in Experiment 1 of the autoshaping experiment (Epstein & Skinner, [1980]). The segments in the left panel are from the last 1-hour session in which in which the food and dot presentations were uncorrelated and show very few pecks to the illuminated spot. Segments in the center panel are from the next session, in which only dot presentations occurred. The subjects paused for between 10 and 15 minutes and then began to peck the spot at relatively high rates. Segments in the right panel are from the next session, in which pecks to the spot are gradually decreasing in frequency. From *Proceedings of the National Academy of Sciences of the United States of America*, 77, 6251-6253.

equipment failure, but everything checked out fine, and I verified through the peep holes in the enclosures that the pigeons were really pecking the spot rapidly. They continued to do so until the session ended. Long before that I was on the phone to Skinner, even though I was pretty sure I would be waking him (he was an early riser). He was as shocked as I was. What on earth was this? Some sort of frustration phenomenon, perhaps? Pecking remained fairly strong during the next day's session but was largely gone by the third or fourth session.

We quickly conducted two follow-up experiments, each of which replicated the original finding and ruled out some possible ways to account for it. In Experiment 2, for example, we shifted from a condition in which the spot appeared intermittently, to one in which the feeder operated intermittently, to one in which the spot again appeared intermittently. If the burst of responding we had observed in Experiment 1 had been a frustration effect, we should have seen pecking in the third condition, but we didn't. In the fourth condition of Experiment 2, we paired the spot and food presentations as we had in Experiment 1, then presented these events independently of each

other until most responding disappeared, then presented the spot alone. Several minutes into the session, two of the three pigeons in this experiment began pecking at fairly high rates. We replicated the effect again in a third experiment, this time with a different breed of pigeon (to make sure this wasn't a breed phenomenon).

Skinner was so impressed by the magnitude and mysteriousness of this effect that, for the first and only time in his career, he used his privilege as a member of the National Academy of Sciences to submit our report for immediate publication in the *Proceedings of the National Academy of Sciences USA* (Epstein & Skinner, 1980).

Our paper concluded, among other things, that the effect "is not easily accounted for by current knowledge about either operant or respondent behavior" (Epstein & Skinner, 1980, p. 6253; Epstein, 1996a, p. 128). In other words, we had no idea why it occurred. We might have figured it out if we had been using open chambers or if we had videotaped each performance and then analyzed the tapes, but perhaps not.

The Columban Simulations and an Interconnection

Over the next few years I continued to conduct various kinds of simulations of complex human behavior with pigeons (e.g., Epstein, 1984b, 1985b, 1987; Epstein, Kirshnit, Lanza, & Rubin, 1984; Epstein & Medalie, 1983), and because I was using clear plastic chambers that were open to view, I could see a lot of things that were normally not seen in many of the pigeon laboratories of the day. My students and I were also doing a lot of shaping, typically training one response, then another, and sometimes even a third or fourth, and then placing the pigeon into a chamber in which there was some sort of problem to solve. At that point, we videotaped, watched, and waited.

Both during the many shaping sessions and during the test sessions, we frequently observed the recurrence of previously reinforced behavior. So had everyone, of course, who had ever used the shaping procedure over the previous 40 years or so. While shaping a response, one pays attention mainly to new forms —the ones that more closely approximate the target behavior; recurrences are largely ignored. In test sessions, however, because we were simply *watching* and not *intervening*, recurrences were strikingly obvious. In fact, it became increasingly clear that novel behavioral sequences that were sometimes adequate to solve problems were formed as previously established responses recurred and became interconnected in various ways. Skinner and I had been talking openly (but uncomfortably) about what we called "the spontaneous interconnection of repertoires" as early as 1979 or 1980. (We thought the word "spontaneous" was problematic because it implied free will. See Epstein [1991] for a discussion of this issue.)

One day in 1981, I had an insight—in the same way, perhaps, that Köhler's (1925) chimpanzees had their insights. Specifically, it appeared that the process that is described by the resurgence principle, followed by an interconnection of covert verbal responses, yielded the principle of resurgence in my own thoughts. Here is how I described this unfolding of events a few years later:

The origin of the resurgence principle in my own behavior would seem in part to exemplify the principle. I was watching a student train a bird for the "tool use" experiment (Epstein & Medalie, 1983). To my knowledge at that time, I had never heard or read of a principle such as resurgence (several months passed before someone pointed out the connection to regression), and I was perplexed by the reappearance of box pushing that had occurred in a previous test. My account was inadequate, or, in other words, I was behaving ineffectively with respect to what I was observing. I repeated the word "reappearance" more than once, and then less common synonyms occurred to me in quick succession: "reoccurrence," "recurrence," "*resurgence*." Skinner and I had used the latter term to describe the autoshaping phenomenon heretofore described in the text (Epstein & Skinner, 1980). Though a year had passed since the autoshaping paper had been published, I was still puzzled by the effect we had reported. I now found myself thinking about *both* experiments. Over the next day or so, common elements occurred to me and then a simple rule that seemed to account for both results. (Epstein, 1985c, p. 151; Epstein, 1996a, p. 143)

And what, you may ask, were those common elements? Perhaps they are obvious to you, but they were not to me. Bear in mind that the results of our autoshaping experiment had baffled every graduate student and faculty member associated with the Harvard pigeon laboratory, including both Richard Herrnstein and Skinner. Moreover, in a study similar to that reported by Epstein and Skinner (1980), Lindblom and Jenkins (1981) also offered no clear explanation for the effect. All I knew was that two different procedures that I had never thought of at the same time had three features in common:

- (1) a period of time during which a pigeon was engaging in a specific response and food was being delivered almost immediately following that response, followed by
- (2) a period of time during which that specific response was no longer being reinforced but during which reinforcers continued to be delivered (in the case of the simulation research, a new response was being taught), followed by
- (3) a period of time during which reinforcers were no longer delivered.

I had a problem with that second feature (#2 above). In the simulation procedures, we would train Response A, then Response B (and, possibly, Responses C and D), and then withhold reinforcement in the test situation. In the autoshaping experiment one could reasonably say that pecking the spot was reinforced once it started occurring (although no contingency was in place), but during the next phase of the experiment, when food delivery and the appearance of the spot were occurring *independently* of each other and pecking the spot nearly ceased, *what response was being reinforced?*

Aha. Shades of Skinner (1948)—another interconnection. It didn't *matter* that no *specific* response contingency existed. Every reinforcer was reinforcing *something*. It didn't even matter whether any particular response was "hooked" and repeated "superstitiously"—the phenomenon Skinner (1948) had reported. All that mattered during the response-independent food delivery phase of the autoshaping experiment was that alternative responses—that is, responses other than pecks to the spot—were being reinforced. So when reinforcement stopped—the third phase of the experiment—it took time for those alternative responses (which shall forever remain unknown) to weaken sufficiently to induce the recurrence of behavior that been reinforced previously. In fact, it is possible in this situation that *dozens* of previously reinforced responses had to weaken sufficiently before pecks to the spot would recur, because many different responses were undoubtedly reinforced adventitiously over the 10-to-12 1-hr sessions in phase two. Even response forms that had been "hooked" would undoubtedly have "drifted" into different forms over time, a phenomenon Skinner (1948) also had observed. Once I had that insight, the principle of resurgence was obvious.^{2,3}

Soon after this I published a study in which I stated the principle and replicated the Epstein and Skinner (1980) finding with a simpler, clearer procedure (Epstein, 1983). With each of six pigeons, pecks on one of two keys in a two-key chamber, counterbalanced across the six, were first reinforced and then extinguished for between 1 and 12 sessions; then an alternative response (described in Table 1 of Epstein [1983]), which was incompatible with key pecking, was reinforced 20 times. After

² Lindblom and Jenkins (1981) considered the possibility that the resurgence of pecking they had observed in their experiments was related to the extinction of behavior that had been reinforced adventitiously when food and spot presentations occurred independently, but they rejected this possibility as unlikely, and their experiments did not test for this possibility directly.

³ I have long believed that human thinking is limited in ways that make it difficult for us to study our own behavior objectively—especially our covert behavior (Epstein, 1982, 1996). We invent constructs (mind, soul, memory) and use metaphors (the brain is an information processor) rather than face what we are: organisms. No words or rules or even memories are "encoded" in our brains. Neuroscientists can't find them because they are not there to be found. I have never seen any evidence that thinking works any differently than overt behavior does (see Epstein [2008] for a defense of this view). The steps leading to my reformulation of the resurgence principle seem consistent with laws similar to those that govern overt behavior. Being organisms hinders us in many ways, but in this instance it may have helped.

delays ranging from 21 to 195 s after the last alternative response had been reinforced, once the frequency of that response had declined substantially, all of the pigeons resumed pecking the key upon which pecks had previously been reinforced, and the rate of responding was particularly high for three of the pigeons. Virtually no pecks occurred on the second key, which served as a control.

This experiment was more informative than the ones reported by Epstein and Skinner (1980) in part because it included a condition in which the first response was clearly extinguished; it thus showed that previously *extinguished* responses could be made to recur by the extinction of a newly reinforced response. This made a response-prevention interpretation of resurgence less viable. The new experiment also included a control key; the lack of responding on that key demonstrated the specificity of the resurging repertoire.

The 1983 paper also included examples of the principle's early appearance in psychology, as well as of its current sad state:

A simple principle of potentially wide application may be stated as follows: When, in a given situation, recently reinforced behavior is no longer reinforced, behaviors that were previously reinforced under similar circumstances tend to recur. The principle has been stated in various ways for more than fifty years (Barker, Dembo, & Lewin, 1941; Estes, 1955; Freud, 1920; Hull, 1952; Maltzman, 1955; Masserman, 1943; Mowrer, 1940; Notterman, 1970; Sears, 1943; Yates, 1970), though no systematic investigations have been made. Recent statements of the principle often cite no references (e.g., Notterman, 1970; Yates, 1970; cf. Staddon and Simmelhag, 1971). (Epstein, 1983, p. 391; Epstein, 1996a, p. 129)

In a follow-up paper in 1985, I presented some additional data (see Figure 2) and also speculated about possible applications of the resurgence principle. I noted, first of all, that even when the principle had been stated in one form or another in recent literature, its mention was "incidental to other observations and other concerns" (Epstein, 1985c, p. 145; Epstein, 1996a, p. 136). Staddon and Simmelhag (1971), for example, stated that extinction "should involve the reappearance of old (in the sense of previous extinguished) behavior patterns" (p. 25); they cited no references but instead included a footnote indicating that they could find "only one published report of this effect" (p. 25) – namely, a study about dolphin training by Pryor, Haag, & O'Reilly (1969).

I also wrote about the possible relation between resurgence and Freud's concept of regression and then described what I called "the clearest cases" of resurgence, which had long ago been reported by Hull (1934, 1952):

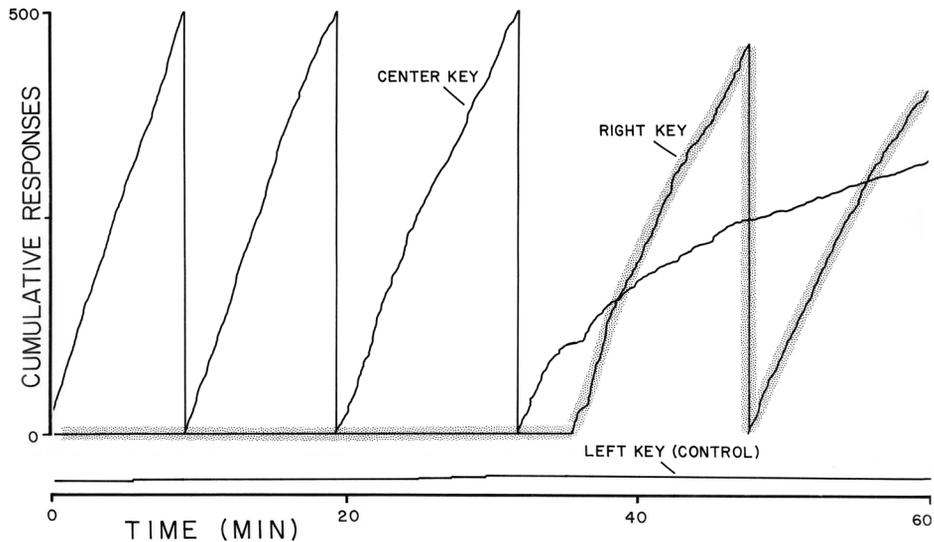


Figure 2. Overlapping cumulative record segments from the first session in which reinforcement was withheld on all keys for bird 13YP in the experiment described by Epstein (1985). Each of the three segments shows responding on one key. Responding on the center key had been recently reinforced according to a variable-interval 1-minute schedule. During the first half of the session, a high steady rate of responding was maintained on this key. It became less stable after the first 1,000 responses. A smooth deceleration is apparent during the last 20 minutes. (Note that the pen resets downward after 500 responses.) The lower line corresponds to the left key, upon which pecking had never been reinforced. The pigeon pecked this key only 7 times during the hour. The shaded line corresponds to the right key where there was a history of VI 1-minute reinforcement. No responses occurred on this key while responding on the center key was strong, but the pigeon began pecking it at a high rate at roughly 40 minutes into the session. It pecked the key nearly 900 times during the last 20 minutes. From Epstein, 1985. Copyright © 1985 by the Association for Behavior Analysis International. Reprinted with permission.

In one experiment he trained rats to run down a straight 40-foot alley for food. In early sessions, he observed a “speed gradient”: the rats ran more and more rapidly as they approached the food. The gradient disappeared after a few days but reappeared when food was withheld. In a second experiment, rats were first trained to run down a 20-foot alley and then down a 40-foot alley. When food was withheld on the longer alley, the rats tended to stop at the 20-foot mark. (Epstein, 1985, p. 155; Epstein, 1996a, p. 136).

I suggested that the principle seemed to be essential in understanding the emergence of novel behavioral sequences I had observed in some of my recent simulations. More important, I pointed to what appeared to be unexplained anomalies in recent experiments which could perhaps be understood if the resurgence principle were

brought to bear. One case concerned the puzzling results of a recent study of VI and VT schedules of reinforcement (Catania & Keller, 1981) that could easily be explained using the resurgence principle. A second case concerned a report by Enkema, Slavin, Spaeth, and Neuringer (1972):

They reinforced pigeons' key pecks with food and then eliminated key pecking by no longer reinforcing it and by making a container of free food available in the rear of the experimental chamber. When the free food was removed from the cup, key pecking returned at a high rate. Presumably the free food quickly established approach and feeding behaviors toward the cup. When the food was removed, these behaviors were presumably extinguished over some period of time, and a previously successful response, key pecking, reappeared. Again, a pause, perhaps on the order of several minutes long, should have occurred before the resumption of key pecking. (Epstein, 1985c, p. 148; Epstein, 1996a, p. 140)

I suggested that resurgence "probably occurs in any domain in which behavior is sometimes ineffective (along with other extinction effects)," including both single and concurrent schedules of intermittent reinforcement, which suggests that "[the] characteristic rates and patterns of responding which are produced by particular schedules may be generated in part by the continual resurgence of previously reinforced sequences when responses are not reinforced" (Epstein, 1985c, p. 149; Epstein, 1996a, p. 141).

In the clinical realm, I said, symptom substitution might sometimes occur because of resurgence —a possibility that suggested practical interventions. Resurgence was just one process exemplifying the idea that responses interact; a fuller understanding of the dynamics of behavioral interaction, I suggested, might enhance the effectiveness of existing therapies. I also said that the resurgence principle might be applicable to the foraging theories then being developed by behavioral ecologists (e.g., Lea, 1979), as well as to an analysis of problem solving. We are effective in solving problems in large part because we bring so much of our behavioral history into play in new situations. "Artificial intelligence programs may fail as models of human intelligence," I wrote, "because they neglect this aspect of the behavior of organisms" (Epstein, 1985c, p. 150; Epstein, 1996a, p. 142). Finally, I suggested that resurgence may be only one of a small number of phenomena that can make multiple repertoires of behavior available to compete with one another in real time in a single organism; if so, it is probably critical to the creative process, which results from interconnections among competing behavioral repertoires, both overt and covert.

Looking back, I am surprised to find that I neglected one important topic in these papers: shaping, the differential reinforcement of successive approximations to a tar-

get response. Shaping could not occur successfully unless (a) resurgence occurred and (b) resurgence was both orderly and predictable. When one is using shaping to teach a pigeon to turn in a circle or raise its wings, one takes advantage of the fact that every time one operates the feeder, the particular form of behavior one has now strengthened will invariably recur at some point in an orderly distribution of responses that includes (a) responses one has previously reinforced, (b) new variants of the responses one has reinforced, and (c) interconnections among some or all of them. If this sounds complicated, that's because it is! If one reinforces too many *interim* responses too many times – that is, too many responses that fall short of the target – one may never get there. Worse still, after the target has been achieved, once *the target itself* is placed on an intermittent schedule of reinforcement, *interim responses will inevitably recur*; in other words, the target behavior will be unstable. If, on the other hand, one reinforces too *few* interim responses, or, worse yet, the *wrong* interim responses, one also might never reach the target.⁴

Generativity Theory

As I explain in detail in a recent essay (Epstein, 2014), in 1982 and 1983 I became increasingly adept at providing running verbal accounts of my pigeons' novel performances in various test situations —accounts that invoked various well-established behavioral principles in piecemeal fashion. I also became increasingly dissatisfied with the awkwardness and imprecision of such accounts. Different behavioral processes don't turn on and off as a pigeon proceeds over time to stretch toward a toy banana, push a box toward the banana, climb onto the box, and peck the banana (Epstein et al., 1984). Behavior is continuous in time, and behavioral processes, I conjectured, probably operate simultaneously, not sequentially. I began experimenting with ways to model generative phenomena based on the idea that *multiple behavioral processes operate simultaneously on the probabilities of multiple behavioral repertoires*. I expressed the new theory, which I called generativity theory, as a series of linear equations representing four simple behavioral processes. The equations are in a form suitable for iterative modeling —that is, the kind of modeling in which the data (probabilities of occurrence) are processed repeatedly through the equations, each iteration generating new probabilities for small successive time intervals.

The first two equations represent phenomena that affect individual responses, and the second two represent the two simplest ways in which two different responses might

⁴ Although I have not attempted it, I believe that shaping could be successfully modeled using the transformation functions of generativity theory.

interact with each other. The four equations, which I called transformation functions, represent the following processes:

(1) *Extinction*. The probability of Response y decreases over time. The variable ϵ indicates the rate of change:

$$Y_{n+1} = Y_n - Y_n * \epsilon$$

(2) *Reinforcement*. The probability of Response y increases over time. The variable α indicates the rate of change:

$$Y_{n+1} = Y_n + (1 - Y_n) * \alpha$$

(3) *Resurgence*: A decrease in the probability of Response y' causes an increase in the probability of Response y . The variable $\lambda_{yy'}$ indicates the strength of the relation between Responses y and y' . That strength likely depends on multiple factors, the most important of which is the extent of overlap in the stimuli controlling the two responses. A negative value indicates a resurgence relation:

$$\text{for } \lambda_{yy'} < 0 \text{ and } y'_n - y'_{n-1} < 0 , \\ Y_{n+1} = Y_n + (1 - Y_n) * (-\lambda_{yy'}) * y'_n$$

(4) *Automatic Chaining*: An increase in the probability of behavior y' causes an increase in the probability of Response y . A positive λ indicates an automatic chaining relation:

$$\text{for } \lambda_{yy'} > 0 \text{ and } y'_n - y'_{n-1} > 0 , \\ Y_{n+1} = Y_n + (1 - Y_n) * \lambda_{yy'} * y'_n$$

Although each concept, along with its corresponding equation, is simple, generativity theory asserts that all of these processes (and perhaps others, of course) are operating simultaneously and continuously on the probabilities of all possible responses that can occur in a given situation—hundreds, perhaps, even thousands in the case of verbal responses. This is where a computer becomes indispensable.

To my surprise, the very first time I ran the computer model that incorporated these equations, my program produced a graph showing smooth overlapping probability curves that reasonably described an actual novel performance I had observed with my pigeons. I now had a formal, predictive, empirically-based theory of ongoing behavior—including novel behavior—which I would soon apply to a variety of novel performances with both pigeons and people. I presented generativity theory for the first time at the 1984 meeting of the International Society of Comparative Psychology

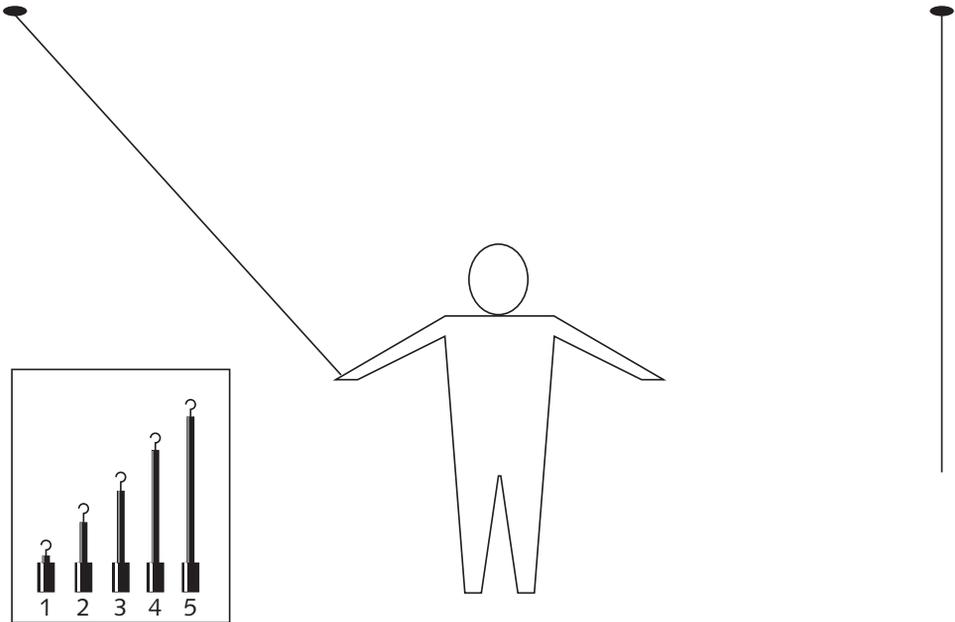


Figure 3. Maier's (1931) two-string problem. Subjects are instructed to tie the two ends of the strings together, but they quickly learn that they can't reach both strings at once. They learn this by pulling one string toward the other and reaching. Many people then try pulling the second string toward the first, which makes little sense. When provided with a long heavy object (#5 in inset), a subject is highly likely to use it to extend his or her reach, but the object that is provided is not long enough to reach the other string. When provided with a short heavy object (#1), a subject is much more likely to solve the problem, which requires tying the object, short or tall, to one string and swinging it, then pulling the other string toward the swinging string and catching it when it comes near. Appropriately, the problem is sometimes called "the pendulum problem." Provided with a long object (#5), the probability of using that object to try to extend one's reach is so high many people fail to solve the problem. Objects of intermediate lengths produce predictable outcomes according to those lengths. All of these cases can be modeled using the transformation functions of generativity theory. From Epstein, 2014. Copyright © 2014 by Elsevier. Reprinted with permission.

and published the theory the next year (Epstein, 1985a). In the mid 1990s, I also developed a way of graphing the unique novel performances of individual subjects as a set of overlapping curves that resemble probability curves—the so-called "frequency profile" (e.g., Epstein, Thomas, Crawford, Mejia, & Robertson, 2015). The generativity model produces "probability profiles" that are similar in appearance to the frequency profiles of real performances by individual subjects (Epstein, 1991, 1996a, 1999, 2014) (see Figures 3 and 4). Over time, the theory proved helpful in developing an empirical, competencies-based approach to boosting creativity (Epstein, 1996b, 2000; Epstein, Schmidt, & Warfel, 2008; Epstein & Phan, 2012; Epstein, Kaminaka, Phan, & Uda, 2013).

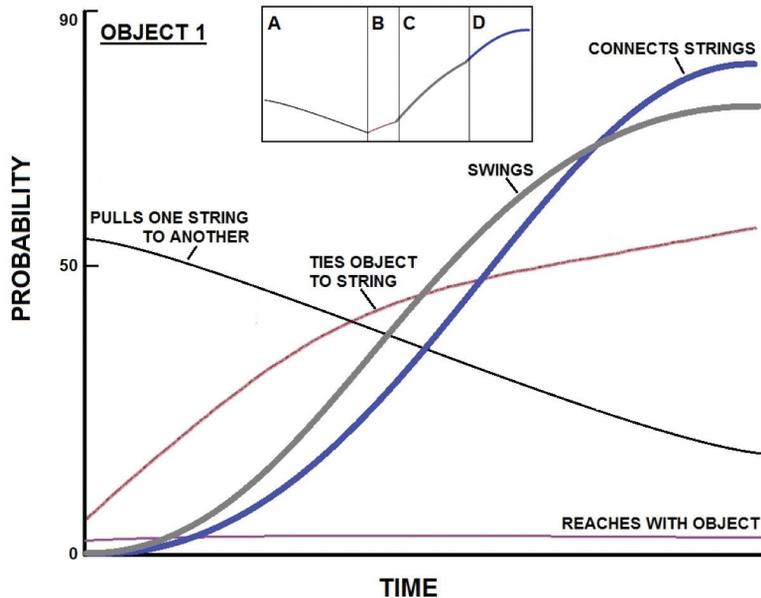


Figure 4. Probability profile for Maier's (1931) two-string problem. A probability profile generated by the transformation functions of generativity theory (see text), generated for five responses relevant to the solution to Maier's (1931) two-string problem. The abscissa is labeled "time," which corresponds to iterations of the computer algorithm, each a scalable moment of unspecified duration. The profile was generated with parameters for a short object (#1 in Figure 3), which generally produced rapid solutions to the problem and little or no irrelevant reaching. The probability of reaching with the object in hand (to extend one's reach) stays low throughout the session. The decrease in the probability of pulling one string toward the other (which is always ineffective) leads to an increase in the probability of tying the object to a string (resurgence). Once the object has been tied to the string, that increases the probability of swinging the string (automatic chaining), which increases the probability of catching the swinging object and tying the two strings together (automatic chaining). The inset (center top) shows just the top edges of the four latter probability curves. This oddly shaped line suggests the dominant responses we will see over the course of a test sessions, namely (A) pulling, (b) tying, (c) swinging, and (d) connecting the strings. The computer model that generates the curve uses discrete state methodology, running a set of initial probabilities through all four equations to generate a new set of probabilities, then running those through the equations again, and so on.

This is not a paper about generativity theory, however. I mention it only to suggest how important resurgence is in the emergence of novel behavior. Resurgence is only one of two phenomena that quickly and reliably cause multiple responses to compete with each other, the other being multiple controlling stimuli. As responses compete, they produce both new response sequences and new blends. In fact, response competition is probably responsible for the emergence of most if not all significantly novel behavior, including the valuable sort of novel behavior a culture sometimes labels "creative." If extinction did nothing but weaken the particular response we had recently

been reinforcing, it would be useless for the creative process. (Think about the thousands of experiments on extinction which have looked *only* at the decrement it produces in the nonreinforced response. It's almost tragic). Because extinction in fact has multiple effects, however, including resurgence, it plays a key role in the emergence of novel behavior, which underscores the value of failure. Failure is not something we should fear, I have argued; it is something we should celebrate, if only because it often stimulates creativity (along with unpleasant emotions—but that is another story). If you want to bring out the creativity in people, challenge them (Epstein, 2000).

An Unforgettable Exercise

I often begin a lecture or seminar on generativity theory or creativity with a simple shaping exercise I call "Abuse the Professor". I do this to get the group used to working together to shape a response that would normally have a low probability of occurring in the present setting: getting me to touch my toes, for example, or to stand on top of a desk. First we do some practicing, and the group quickly learns to work together to shout "Yes!" whenever I move in ways that approximate a target behavior. I also give some general advice such as, "Reinforce generously at first—don't be picky—but then become increasingly selective about which movements to reinforce. "No's" and verbal tips are not allowed (see "The Yes! Game" in Epstein [2001] for details).

Next I ask for a volunteer to serve as coordinator for the exercise. I explain that I will leave the room, at which point the coordinator's job is to work quickly with the group to decide what it will try to get me to do upon my return to the room. "Keep it simple," I say. "No sequences. Oh, and nothing obscene." (I learned the hard way that that last instruction is quite important.) I depart, and when the target has been agreed upon, the coordinator brings me back to the room, and a very loud and fun shaping process begins. Typically the group can guide me to the target behavior in under a minute, which usually gets people cheering.

"Okay," I say, "you've had your fun. Now it's time for *Part Two* of the exercise, which I call *"Abuse the Audience Member."* (For details, see "The Not-for-the-Fainthearted Game" in Epstein [2000].) I begin by finding a "brave" volunteer, and he (or she) leaves the room. Then I ask for another volunteer to come forward to assist me. After I give some guidelines, my assistant leads the group in a quick discussion about what the new target should be: It needs to be unusual and something the subject can repeat at least three times. Shaping a two-or three-step sequence is allowed, as long as the last step can be repeated. A typical choice: walk to the other side of the room and move the window shade up and down three times.

I then explain that it will be up to the assistant, not me, to determine when the target has been met. I also explain that she (or he) will stand near the front corner of

the room during the exercise with a chair behind her and that when, *in her judgment*, the target has been met, she will *quietly sit down*. That is the signal to the group that the extinction period has begun —no more *Yes-es*. So the group needs to keep one eye on the assistant while the shaping is in progress. I now place a chair near the front corner of the room (and usually remain standing beside it), and the assistant brings the subject back into room, explaining merely that “the group is going to try to get you to do something”.

The shaping begins. With lots of *Yes-es* and sometimes some cheering and laughing, the target is typically reached within a few minutes, at which point the assistant sits and the *Yes-es* stop (although a few stragglers in the group who hadn’t noticed that the assistant sat down or who didn’t understand my instructions usually shout a few extra *Yes-es* before going silent).

Now, with no more reinforcers coming, we all wait and watch carefully, typically for 10 to 15 minutes —sometimes even longer— just as my students and I used to do in front of the pigeon chambers. The behavior that follows is orderly, predictable, and consistent with the tenets of generativity theory.

Here is what happens during the extinction period:

- (1) The subject continues to repeat that last step several times (say, raising and lowering the window shades), sometimes exaggerating or varying the response (raising the shade faster or slower or to different heights).
- (2) The rate of repetition slows.
- (3) The subject turns to the group, looking confused.
- (4) In roughly reverse order, he (or she) repeats almost all of the interim responses that had been reinforced (moving her hands in the air near the window, turning toward the window, walking toward the window, etc. – the details depend on what those interim responses were).
- (5) As he continues to fail, he shows more and more signs of frustration and starts talking aloud, saying things like, “I don’t get it” or “I’m running out of ideas.” If the subject asks me a question directly (typically, “Can this even be done?”), I reply “You can do it” but never anything else.
- (6) Blends, new sequences, and genuinely new responses (like opening and closing the window) become more frequent, with those reinforced interim responses occasionally still occurring.

I stop the exercise when the subject becomes inactive for at least a minute (this rarely happens) or when the time I have allotted for the exercise has elapsed. At this point, I ask the subject to give us his interpretation of what just happened; he tells us, roughly, about the covert verbal behavior he was experiencing —his thoughts and

“hypotheses” about what we were trying to get him to do— all more behavior which could, in theory, be analyzed in the framework of generativity theory. I then inform the subject about our actual procedure and then ask the group to describe what they saw. Without fail, people report having seen the frequent recurrence of previously reinforced responses —resurgence. Those observations serve as a bridge to my lecture on generativity theory and the orderliness of behavior —even novel behavior.

This exercise often has quite an impact on people. I have had former students remind me about it more than 10 years after they participated in it. There is an orderly relation between what we have experienced and how we behave in new situations, and much of what we do in those situations is predictable. The exercise makes both points dramatically. It also demonstrates that resurgence is a robust phenomenon that plays an important role in the emergence of novel behavior.

Causation

Is resurgence a causal mechanism? Can the principle of resurgence be used to explain the emergence of old behavior? If you use the principle prudently, the answer to both questions is yes, as noted by Cleland, Guerin, Foster, and Temple (2001). Equation 3 above is as much a law of behavior as $e=mc^2$ is a law of physics. The principle of resurgence allows one to make specific predictions about behavior given specific conditions. Moreover, if a previously established response recurs when a new response is undergoing extinction, one can reasonably speculate that resurgence was the “cause”—in other words, that these events are predictable according to the principle. In the latter case, one can also conduct experiments to test one’s hypothesis.

Is the term “resurgence” sometimes used in other ways? Is it sometimes used descriptively? Is it a mistake to use a description as an explanation (as in “this fluid is flowing slowly because it is *viscous*”)? Yes to all three questions. The term “resurgence” is borrowed from the vernacular, as are many terms used in the hard sciences (e.g., adaptation, focus, momentum, reflection, spin). It is bound to be used inconsistently and, at times, improperly. That in no way invalidates the principle of resurgence, however.

Variables and Definitions

Toward the end of my 1983 paper on resurgence, I stated the obvious – that when one examines any behavioral phenomenon closely, things get complicated:

Resurgence is undoubtedly affected by many variables—the schedules according to which both the old and new behaviors are reinforced, the type and

magnitude of the reinforcers (will the old response resurge if the old and new responses are established with different reinforcers?), the period of time over which the responses are reinforced, the time that elapses after the old response is last reinforced, the overlap in the stimulus conditions under which the responses are reinforced, and so on. Matters are complicated when many responses are available to resurge, as is probably the rule in the natural environment: What variables determine the order of resurgence and the distribution of responses? The degree of extinction of the previously reinforced response is undoubtedly an important factor. It is likely that the more complete the extinction, the less the resurgence (Rawson et al., 1977). The data for birds 17Y and 39Y show, however, that resurgence can occur even when extinction has been extensive and is virtually complete. (Epstein, 1983, p. 395; Epstein, 1996a, pp. 132-133)

Over the years, researchers have discovered how these and other variables affect the resurgence process (e.g., Cleland, Foster, & Temple, 2000; da Silva, Maxwell, & Lattal, 2008; Lattal & St. Peter Pipkin, 2009; Lieving & Lattal, 2003; Reed & Morgan, 2006; Sánchez, & Nieto, 2005; Wacker et al., 2013). Researchers have also shown that resurgence occurs with people, not just with pigeons, and that it may play a role in the reappearance of maladaptive behavior (e.g., Lieving, Hagopian, Long, & O'Connor, 2004; Volkert, Lerman, Call, & Trosclair-Lasserre, 2009; Wilson & Hayes, 1996).

I will leave it to others to weigh in on the significance of such studies and will comment only on two somewhat general issues.

First, labels are often imprecise or misleading, which is why I tried to move away from verbal accounts of continuous performances when I developed generativity theory. This issue is applicable to one of the variables that affects resurgence —namely, how thoroughly the old response has been extinguished. For the sake of argument, let's hypothesize that the more thoroughly the old response has been extinguished, the smaller the resurgence effect will be. Let's also acknowledge that the degree to which a response has been extinguished can vary continuously from "not at all" to "almost completely." It makes little sense to say that just because we are at or near the "not-at-all" end of continuum, the reappearance of the old response is no longer an example of "resurgence" but rather of "response prevention." Isn't the latter just a special case of resurgence in which one particular variable (degree of extinction) is at or near an extreme value? Invoking another causal mechanism in this case is like saying that we should no longer call a fly ball a "hit" if the bat is the maximum size allowed in baseball. How about saying simply that "the more thoroughly the old response has been extinguished, the smaller the resurgence effect will be" (if indeed that proves to be true)?

Singling out either end of the extinction continuum is especially problematic given that it is doubtful that either extreme is possible to achieve with actual organisms. Extinction is never really complete, and some degree of extinction is almost certain to occur even in the response-prevention scenario. Consider the simple experiment in Epstein (1985; see caption, Figure 2). When the schedule of reinforcement shifts from the right key to the center key in this procedure, because the schedules are fairly lean and because all three keys are always illuminated, the pigeons peck the right key hundreds of times before shifting exclusively to the center key. Even without any official “extinction” sessions, pecks to the right key have been extinguished to some extent.

Second, there seems to be some disagreement about how the principle of resurgence should be stated. In some recent papers, my original statement of the principle has been modified to say that it is previously “extinguished” behavior that recurs (e.g., Shahan & McSweeney, 2011). I think that narrowing the definition in this way is a mistake, especially if we are limiting ourselves to behavior that has literally been subjected to an extinction procedure in a laboratory. The broader statement of the principle, which is also reflected in Equation 3 of generativity theory, says simply that old behavior recurs when current behavior is no longer effective. How those old responses were established or why the probability of their occurrence was low before the current response became ineffective is important only parametrically, in my opinion. If previously established behavior recurs when current responding ceases to be effective, I am inclined to call that resurgence.

To clarify, let’s say someone is trying to exit a room by turning a doorknob that has always turned easily. Now it is stuck. The resurgence principle predicts that *all other behavior that has ever gotten that person through a door* will begin to increase in probability. In a probability profile, we would see the curve representing “tries to turn the doorknob” slope downward, indicating a rapid decrease in probability (compare the curve representing “pulling one string to another” in Figure 4). Overlapping that curve, we would see multiple curves representing other behavioral repertoires—jiggling the doorknob, banging on the door, kicking the door, shouting for help, etc.—sloping upward from a probability near zero and eventually sloping downward again if they are ineffective (compare the curve labeled “ties object to string” in Figure 4). How and when and why those old responses were acquired or lost is irrelevant. They all become relevant to the situation when turning the knob fails. While it is interesting and even surprising that previously *extinguished* responses will sometimes recur, it is no less important that other long-absent responses may also recur—the more, the better. The larger the number and the more diverse those responses, the more likely it is that someone will be able to open the door—either because one of those old responses works or because a novel sequence or blend of those responses works.

In this situation, as one response after another fails to get the door open, if one is stranded in that room long enough—days, perhaps—what very old response will *eventually* recur? The answer, I believe, is *calling out for one's mother*. When I suggested this many years ago at a lecture I gave on resurgence at the University of Massachusetts, a well known psychologist promptly shared a relevant story: She had recently gotten stuck in a bathroom stall in a public bathroom in Mexico while on a visit there with her husband. Try as she might, she couldn't open the door, and no one was around to assist her. After a while, she started crying and called out aloud for her mother, who, she said, was then in a nursing home thousands of miles away.

Is crying out for one's mother "extinguished" as we grow older? I doubt it. But it is a very weak response, especially when one's mother is far away. The resurgence principle, at least the way I conceive of it, suggests that *all* behavior that may have helped you to be effective under circumstances that are similar to the current circumstances is likely to recur when your current behavior is no longer effective—behavior that was extinguished, punished, forgotten, or programmed by genes. That kind of process, it seems to me, has obvious survival value, and hence it is the kind of process that evolution might have selected for over time.

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