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On the orderliness of behavioral variability: Insights from generativity theory

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ABSTRACT

Over time, many natural phenomena that had long appeared to be disorderly have been found to be orderly and predictable under specifiable conditions. First introduced in the early 1980s, generativity theory is a formal, predictive theory of the behavior of organisms that reveals the orderliness, moment to moment in time, in apparently disorderly behavior – even the surprising behavior a community sometimes calls "creative." According to this theory, under two specific conditions – when behavior is ineffective or when stimuli present in the environment are novel, compound, or ambiguous – novel behavior emerges in a predictable way as a result of a dynamic process in which multiple behavioral processes operate simultaneously on the probabilities of multiple behaviors. The process can be represented by a series of equations called transformation functions. Instantiated in a computer model, the equations have proved useful in the moment-to-moment prediction of the emergence of novel behavior in both pigeons and people. A graphical method that generates a "frequency profile" has also helped to reveal the orderliness in the apparently disorderly behavior of individuals. Generativity theory makes no assumptions about the existence or nature of cognitive mechanisms and does not depend on the statistical analysis of aggregated data to show the orderliness in complex behavior. Although its predictive power in the laboratory is perhaps unparalleled, the full potential of generativity theory has yet to be explored.

1. Introduction

Charles Darwin's ability to detect certain patterns in the apparently disorderly physical characteristics of 26 types of birds in the Galápagos Islands helped him to formulate one of the most profoundly important theories in the history of science, the theory of evolution (Desmond & Moore, 1991). He deduced that the orderly variability in phenotypes produced by sexual reproduction in each generation of a given species, in combination with the selective survival requirements exerted by different environments, could, over time, account for the creation of new species. He knew nothing about genes, meiosis, gametes, or syngamy – about the biological mechanisms underlying the phenomena he observed – but he inferred that such mechanisms must exist.

Although the details vary from one scientific domain to another, this is generally what science is all about: finding the orderliness in apparently disorderly phenomena, often at just one level of observation. Ideally, that orderliness is eventually expressed in formal terms so the theory can be used to make specific predictions; this increases the utility of the theory and also helps to establish its validity. Einstein's general theory of relativity is an excellent case in point. First published as a set of field equations (Einstein, 1915), in the years and decades that followed, the mathematical form of the theory proved to be successful in making accurate predictions about gravitational time dilation, the bending of starlight around the sun, and other natural phenomena (Einstein, 1915). Just recently, the theory was shown to predict with remarkable accuracy the redshift of light that occurs in massive galactic clusters (Wojtak, Hansen, & Hjorth, 2011).

The behavior of organisms is one of those natural phenomena that often appears to be disorderly and unpredictable. Although it is true that hundreds of millions of human drivers manage to stay in the correct lanes on roadways every day, when human behavior is not constrained by salient stimuli (curbs, signs, and lines on the road), the consequences of previous actions (traffic tickets and warnings), or rules and laws (descriptions of how one must drive to avoid future tickets), it often appears to be quite disorderly. In new or challenging situations especially, it can be difficult to predict what people will do, think, or say – even for *them* to predict what they will do, think, or say. People also do genuinely new things; virtually every sentence that we speak or write is new in some respects, our dreams are sometimes extraordinary, and occasionally an individual will do something so new and interesting that a community will label the action or its product "creative."

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Although the language of creativity is applied erratically by a community (depending on what it happens to consider interesting at the time), there is also no question that occasionally someone behaves in ways that are profoundly new, producing the kind of output Csikszentmihalyi calls "Big C" creativity (e.g., Csikszentmihalyi, 1996; Csikszentmihalyi & Epstein, 1999).

We face a range of predictability challenges here, which can be said to exist on a continuum from fairly easy (predicting which turns someone will take on the way home from work) to quite difficult (predicting which turns someone will take when he or she is lost in a new city) to probably impossible (predicting the specifics of an amazing new mobile computer application someone devises while being lost in a new city, which will someday help millions of people to avoid getting lost).

In the early 1980s, inspired by observations I made over the course of conducting a series of somewhat irreverent experiments with pigeons, I formulated a theory of how behavior is generated moment to moment in time, possibly in a wide range of higher organisms (Epstein, 1985b, 1991, 1996a, 1999). The theory – generativity theory – can be expressed as a series of equations called "transformation functions." Instantiated in a computer model, the equations have proved useful in predicting fairly complex behavior moment to moment in time in the laboratory. Over the years, I have become increasingly confident that generativity theory, or at least something like it, can help us understand how behavior is generated across that entire range of difficulty – from the reappearance of an old, well-established behavior to the occurrence of profoundly new behavior. Along the way, I also developed a type of graph called a "frequency profile" which reveals the orderliness in certain types of performances that appear through direct observation to be disorderly in nature.

Before I present the basics of generativity theory, I will attempt to put the theory into a broader context of some contemporary thinking about the orderliness of behavior.

2. Some models, ideas, and approaches

2.1. How random is behavior?

In recent years, a growing number of experts have come to view some aspects of behavior to be truly random in nature – unpredictable, by definition. In a review of the relevant literature as of 2005, Paul Glimcher identified a number of researchers who not only have concluded that "indeterminacy" is a basic feature of both human animal behavior but even that evolutionary forces may have favored organisms who can behave unpredictably under certain conditions. An animal is less likely to killed, certainly, if a pursuing predator cannot easily anticipate its next move. Entire classes of individual behavior have been studied which, according to Glimcher, are "as fully random as can be measured" (Glimcher, 2005, p. 28). Accordingly, neuroscientists, he says, are uncovering evidence for the existence of "apparently indeterminate processes within the architecture of the mammalian brain" (p. 28). In a broad overview of literature that overlaps fairly little with the studies cited by Glimcher, Allen Neuringer (2002) draws similar conclusions: that behavioral variability is a "stochastic process" (p. 697) and that "organisms have evolved to behave unpredictably" (p. 701).

But there is a problem here. These conclusions, as well as those of many other investigations of behavioral variability (e.g., Emonet & Cluzel, 2008; Hopkinson & Neuringer, 2003; Johansen, Killeen, & Sagvolden, 2007; Machado, 1997; McIntosh, Kovacevic, & Itier, 2008; Reichert, 1978; Shimp, Froehlich, & Herbranson, 2007; Tatham, Wanchisen, & Hineline, 1993), are based on aggregated data – aggregated over trials with individual subjects or, more commonly, across organisms. Do aggregated data tell you anything meaningful about the orderliness of the behavior of organisms?

If, over time, you keep track of whether I drink water or Diet Pepsi with my meals (which, in truth, are virtually the only liquids I ever drink from a glass or bottle, even with my breakfast), you will likely conclude that there is a 50/50 chance that I will drink one or the other with my next meal. As you continue to keep count over time, you will become increasingly confident about this prediction; you might even conjecture that a neural random number generator – the proverbial "mental coin toss" – governs my choice. But if, *on a single occasion*, you observe that I enter the kitchen, then prepare a peanut-butter-and-jelly sandwich – which you know from previous observations that I consume only with water – then open the refrigerator door where a water bottle and a Diet Pepsi bottle stand side by side, will you have any trouble predicting my next move?

Take this a step further. Say you begin to keep careful records of the specific foods I consume with water versus the specific foods I consume with Diet Pepsi, along with specific stimulus conditions and behaviors that reliably precede the consumption of each type of drink. Over time, wouldn't you be able to predict my choice of drinks *on any single occasion* with increasing accuracy, perhaps with nearly 100% accuracy?

Although I am a heavy user of statistics in most of my current research projects (because I'm working with data sets obtained from thousands of people – see, for example, Epstein, McKinney, Fox, & Garcia, 2012), when it comes to understanding the moment-to-moment behavior of a single organism, the statistical analysis of aggregated data can be misleading (cf. Barlow & Nock, 2009; Sidman, 1960; Skinner, 1966, 1976). Is it even meaningful to say, based on aggregated data, that there is a 50/50 chance that I will select one drink or the other when, with the right data in hand, it would be easy to make an accurate prediction about my behavior on any single occasion? Considered in this context, the statistical approach to understanding ongoing behavior in the natural environment may be of questionable value.

Yet this approach is quite common. Evolutionary biologist John Maynard Smith's (1982) "hawk-dove model" is a case in point. Applying concepts from game theory, Maynard Smith shows that under certain conditions – specifically when the value of a territory is high (implying an animal should protect it aggressively) and the cost of an injury is also high (implying that an animal should retreat to avoid being hurt), the only sensible strategy for that animal is to be aggressive on some occasions (a hawk) and passive on others (a dove), assuming one role or the other unpredictably from one occasion to the next. Survival is enhanced by this strategy; the math is clear.

But, again, by observing a particular animal for a long period of time, wouldn't it be fairly easy to predict which role it will assume on a single occasion? Its behavior on a particular occasion will be the net result of its recent environmental history and the particular stimulus conditions it faces. As the attacker grows near, the appearance, odor, movements, and sounds of that attacker will, from one moment to the next, make all the difference in your prediction. Imagine, in fact, that you could speed up your perception so that the scene will appear to unfold in slow motion, giving you time to analyze all aspects of what is occurring. Fifteen minutes before the attack, could you make a reasonably good prediction? One minute before the attack, could you make a better one? One second before the attack, is there any doubt that you could make an accurate prediction about which role the animal will assume? What's more, by altering the variables of which you know the behavior is a function, couldn't you perhaps guarantee that an animal will *always* behave as a hawk or *always* behave as a dove?

Let us take this idea even further. If, in a particular setting, from one occasion to the next you are highly adept at predicting whether an animal will behave as either a hawk or a dove, wouldn't it be relatively easy for you to compute how many times the animal will assume each role over the course of a year? And if, one conspecific at a time, you studied the animal's entire cohort in the same manner that you studied that animal, couldn't you predict with confidence what average performances would look like for that cohort? In other words, *doesn't the ability to make accurate moment-to-moment predictions about the behavior of individual organisms make the statistical approach to studying behavior redundant*? By this logic, it could be said that assertions about the stochastic nature of behavior are based on a lack of information.

2.2. *An embarrassing case in point*

Late one evening in 1980, when I was a graduate student at Harvard, something extraordinary happened in the pigeon lab. B.F. Skinner ("Fred" to his friends) and I had been conducting an experiment to test a particular idea we had about why a phenomenon called "autoshaping" worked. The particulars of the theory are not important here, especially because it was wrong. What is important is that for a number of sessions, the movement of a small spot of light on a wall had been followed repeatedly by the operation of a dispenser of pigeon feed. Sure enough, after a few pairings of spot and food, both of the pigeons in the experiment began pecking at the spot whenever it appeared (that is "autoshaping"). To make sure the *pairings* were indeed responsible for the pecking, our next step was to present both the food and the spot at random intervals. As predicted, over the course of many sessions, the pigeons stopped pecking the spot almost entirely. The experiment was now officially over.

But that night, because both birds still pecked the spot occasionally, I decided to disable the feeders. My thinking was that this would get rid of those last few pecks. After 15 min had passed, however, something remarkable happened. One of the birds started to peck the spot at a high rate – more than 150 responses per minute – as frequently as it had ever pecked during the first phase of the experiment. I was pretty sure that this was due to an equipment failure of some sort. But a few minutes later, the *second* bird started pecking at a high rate. Each bird pecked the spot nearly a thousand times before the one-hour session was over.

I called Fred at home long before that hour ended. We had stumbled onto an enormous effect – the largest behavioral effect I had ever seen or heard of – and neither of us had the slightest idea how to explain it, even after we replicated the effect in two additional experiments. Using his privilege as a member of the National Academy of Sciences, Fred submitted our write-up of the three experiments to the Academy's *Proceedings* (PNAS), and it was published shortly after (Epstein & Skinner, 1980). That was the only time during Skinner's entire career that he used that privilege. (He had published papers in PNAS in the early 1930s, but those had been submitted by one of his Harvard supervisors, William Crozier (Skinner, 1979, p. 60).)

Two years later, having now observed dozens of extraordinary pigeon performances in clear acrylic chambers in my own lab, I finally figured out what had happened – *and* why Fred and I had been baffled. Like most experiments conducted in the Skinnerian tradition, our autoshaping experiments were conducted in opaque, sound-attenuated chambers. In other words, we couldn't *see* the birds; that was the problem.

In papers published in 1983 and 1985, I explained the 1980 phenomenon and offered a principle and new data to support my explanation (Epstein, 1983, 1985a). According to the *principle of resurgence*, in a given setting, when behavior that was recently effective is no longer effective, behaviors that had previously been effective in that setting will recur; this phenomenon had been observed by researchers since the early 1900s (e.g., Hull, 1934; see Epstein, 1985a for additional references). In the first phase of the autoshaping experiment, pecking the spot was reliably followed by the delivery of food; from the pigeon's perspective, pecking *produced* food. In the second phase of the experiment, when the appearance of the spot and the presentation of food came at random intervals, *all sorts of behaviors* were followed by the presentation of food: turning, head bobbing, lifting a wing, and so on. Some of those behaviors may even have persisted over time, occurring "superstitiously" (Skinner, 1948). We had no way of seeing the birds, of course, so it never occurred to us that this was happening.

In the all-important third phase of the experiment, no food was delivered, so whatever the birds were doing to "produce" food – wing flapping, turning, etc. – was now ineffective. It took varying amounts of time for those superstitious behaviors to disappear, but when they finally did, the previously reinforced behavior kicked in. Both birds began pecking the spot at a high rate. The resurgence process ultimately became one of the key components of generativity theory. I even found it useful in analyzing my own behavior: in footnote 5 of the 1985 paper, I explained how the *process* of resurgence could account for my formulation of the *principle* of resurgence (Epstein, 1985a; also see Epstein (1996a, p. 145)).

The lesson here was clear: Skinner and I could not explain the effect we had found because we lacked important information about the moment-to-moment behavior of the pigeons.

2.3. *Models of behavior, cognition, and creativity*

Mainstream models of behavior or cognition these days are usually based on the supposition that organisms are information processors. This idea began to take hold in the 1960s when computers started to become mainstream. As they have proliferated over the years, information-processing models of cognition and behavior have become ever more popular. Computers are behaving in increasingly intelligent ways, so intelligent organisms must operate as computers do, the logic goes. Here is the faulty syllogism:

1. *All computers behave intelligently*.
2. *All computers are information processors*.
3. *Therefore, all intelligent entities are information processors* (cf. Epstein, 1984a, 1996a).

The information processing model is just one variation on the kind of mentalistic models that have been common in psychology since its inception as an academic field in the late 1800s. Because models of this sort cannot easily be tested against physical correlates, they have sometimes inspired long-running debates that perhaps will never be resolved until an effective form of neuroscience finally emerges. Eric Kandel, a Nobel-prize-winning neuroscientist, estimates, unfortunately, that this will take another hundred years (Epstein, 2012, p. 50). In the meantime, mentalistic models have few constraints and are sometimes not falsifiable, which has meant over the years that the debates that have emerged among cognitive scientists have proved to be every bit as rancorous as those that took place decades ago between behavioral and cognitive psychologists. The 30-year debate between Stephen Kosslyn and Zenon Pylyshyn about the "algorithms" underlying mental imagery is a case in point (e.g., Kosslyn, 2005; Pylyshyn, 2003; cf. Anderson, 1978). A recent and highly influential mentalistic model of creativity proposed by Mark Turner of Stanford University is not even tied to empirical research; Turner even admits that he is "skeptical" that experiments could *ever* be performed to support or refute the model (Turner, 2014, p. 266).

Some information processing models of advanced human abilities such as creativity or humor are not even tied conceptually to neural or behavioral processes. Schmidhuber's (2012) "Formal Theory of Creativity," for example, was developed as part of his effort to build "artificial robotic scientists and artists equipped with curiosity and creativity" (p. 323); to my knowledge, his model has never been used in the prediction or analysis of actual human behavior.

Here is an obscure but recent example of two very different approaches to trying to understand the same highly specific behavioral phenomenon: the particular way in which a rat turns away from another rat that is trying to steal its food – the so-called "dodger/robber" scenario. Bringing an information processing perspective to the problem, Whishaw and Gorny (1994) manipulated variables that affect a dodger's behavior – food size, food hardness, and certain characteristics of the behavior of the robbers – and, averaging data across rats, concluded that the dodgers' behavior was not a fixed action pattern but rather was guided by cognitive processes and, in particular, that "rats estimated the time required to complete eating and adjusted the size of their evasive movements to gain this time" (p. 35). Recently, Bell and Pellis (2011) took a more conservative look at this scenario, wondering whether rats were capable of complex computations:

> …if the rats calculate the angular displacement appropriate for a given dodge, they would have to calculate the time it takes to eat a piece of food, then enter that information into an algorithm along with other variables, such as robber identity and movement. Moreover, the algorithm would require updating to incorporate adjustments needed due to the distance and approach speed of the robber. Such calculations posit the existence of cognitively demanding mechanisms that would seem inefficient. (p. 659)

After replicating the effect and manipulating appropriate variables, they concluded that a simple cybernetic model is all that is required to account for the dodgers' behavior, indicating that this model "accounts for all of the behavior present, including phenomena that might previously have been attributed to foresight, planning and computation" (p. 7). At first glance, it would appear that Occam's Razor would settle the matter; the simpler explanation should be preferred. But Bell and Pellis' (2011) conclusions, like those of Whishaw and Gorny (1994), are based on a statistical analysis of aggregated data, and they also conjecture that the rats are following "simple rules" (part of the title of their paper). But where, exactly, are those rules? Are they in the rats? If not, how are the rats following them?

One obvious problem here is the reliance on groups and statistics, especially if it would be possible to observe individual organisms continuously in time. But the larger problem is the high likelihood that neither goals nor rules exist anywhere inside animals. In fact, even though *humans* can "recite rules" and "state or envision goals" and "recall facts," *reciting*, *stating*, *envisioning*, and *recalling* are all just different kinds of behavior that need to be understood, modeled, and predicted. It is highly unlikely that rules, goals, or even memories will ever be found inside *humans*, never mind rats.

Space constraints prevent me from exploring this issue in detail, but it can reasonably be argued that the information processing approach to understanding the behavior of organisms was without merit from the start. As I have watched computational neuroscience grow as a discipline over the years, I have been become increasingly convinced that it has little or no relevance to the actual mechanisms that explain how organisms behave. No one has found or will ever find the proverbial "memory trace," and no one has found or will ever find a "representation" of Beethoven's 5th Symphony in a person's brain. When we *recall* hearing that symphony, we do not "retrieve" a "memory"; rather we *do* very roughly what we *did* when we actually heard the symphony, and actual neural activity reflects this (Gelbard-Sagiv, Mukamel, Harel, Malach, & Fried, 2008). Hearing, after all, is behavior; it is activity in the auditory system and brain. When we recall hearing a symphony, we engage in similar activity. *We can do so without a representation of that symphony existing in the brain in any possible sense of the word "representation"*. When we clap our hands repeatedly, we are not recalling the clap; we are simply behaving in roughly the same way over and over. Recall is the same kind of phenomenon, except that the stimulus that originally occasioned the behavior is now absent, which is why recall is so imprecise.

When we *formulate a new rule* or *have a new idea*, we do not change the parameters of an internal algorithm or do something mysterious in a mental world; rather, generative processes in the nervous system, stimulated by new challenges in our environment, allow verbal and perceptual elements we have already acquired to merge in new and sometimes useful ways (cf. Rosenbaum, 2014).

3. Generativity theory

In 1978, inspired by the tendency of researchers who worked with non-human primates to attribute the human-like behaviors of those primates to higher mental processes, Skinner and I set in motion what we called the "Columban Simulation Project," after *Columba livia*, the taxonomic name for pigeon (Epstein, 1981). The idea was to teach the primate researchers a lesson by showing that we could, through operant conditioning techniques, get pigeons to do whatever the chimps were doing. If an identifiable history of training was all one needed to account for a complex, human-like performance, why would one speculate about cognitive processes?

Our first publication along these lines appeared in *Science* in 1980 and was entitled "Symbolic Communication Between Two Pigeons (*Columba livia domestica*)" (Epstein, Lanza, & Skinner, 1980). It made news around the world, even in the *Fiji Times*, the latest issue of which I just happen to have on my desk at the moment (the University of the South Pacific, where I am a faculty member, is located in the beautiful Fiji Islands). Our report was notable if only because of its unusual style of writing. It is, to my knowledge, the only satire *Science* has ever published; even the title was tongue-in-cheek.

Mimicking a recent report published by researchers at Emory University on "symbolic communication" between two chimpanzees (Savage-Rumbaugh, Rumbaugh, & Boysen, 1978), we showed that one pigeon ("Jill") could "transmit information" to another pigeon ("Jack") entirely through the use of "symbols" which Jack had learned to "decode." Jack and Jill could see each other through a clear partition in a two-sided chamber. Jack "initiated each conversation" by pecking a sign that read "What Color?" Jill responding to this "question" by looking behind a curtain where one of three colored lights was illuminated at random – red, green, or yellow – then, with Jack looking on intently, pecked a corresponding alphabet letter – R, G, or Y – which was immediately illuminated. Grateful for receiving the message, Jack now pecked a large button reading "Thank You," which immediately caused food to be delivered to Jill. Then Jack pecked a colored button corresponding to the "information" he had received (both pigeons were male). If his choice matched the color hidden behind the curtain, his feeder would now operate automatically, and the sequence would begin again. The birds were able to repeat this sequence repeatedly with more than 90 percent accuracy. When we covered the letters, accuracy dropped to 30 percent.

In the last paragraph of the paper, borrowing the opening phrase of the famous last sentence of Watson & Crick's famous article on the structure of DNA ("It has not escaped our notice that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material" (Watson & Crick, 1953, p. 758)), we admitted that "It has not escaped our notice" that the entire performance could be accounted for by the pigeons' recent training histories, without any recourse to cognitive, purposive, or mentalistic concepts (cf. Taylor et al., 2010). We concluded that "A similar account may be given of the Rumbaughs' procedure, as well as of comparable human language" (Epstein et al., 1980, p. 545). We followed this somewhat outrageous paper with an equally irreverent one entitled, "Self-Awareness in the Pigeon," which was also published in *Science* (Epstein, Lanza, & Skinner, 1981).

The most remarkable thing about these papers was not their findings. After all, we had not discovered anything; we were simply suggesting to our colleagues in the incipient field of cognitive science that they should be cautious about how they theorize. I acknowledged this in the spring of 1981 when the time came to defend my dissertation, which was about simulating complex human behavior with pigeons. Because of all the media hoopla surrounding the work I was doing with Skinner, instead of just the usual three committee members, 10 Harvard faculty members attended my defense, most of whom were skeptical about pigeon research. At a dissertation defense at Harvard in those days, faculty members could ask you about *anything*, not just about your dissertation topic, so the pressure was enormous. To make matters worse, the two Harvard faculty members with the strongest behavioral backgrounds – Fred Skinner and Richard Herrnstein, arguably Fred's most prominent student – did not attend. Fred did not come because he was technically retired, and Dick did not come because he and I had spent the last few years arguing with each other, mainly about the significance of Skinner's work.

My examiner was William K. Estes, a mathematical psychologist, widely known among the graduate students as one of the most brilliant and unapproachable professors around; I had never even spoken to him before. He opened the proceedings by asking me a key question in a halting, somewhat labored fashion: "Do you think… well… that these, uh, *simulations* you're doing with… *pigeons*… simulations of, well, *distinctive* human behaviors that are indicative of… *higher mental processes*… shed any light on human behavior or cognition?" I hesitated, because it was immediately obvious to me that this was a litmus test that would set the tone for the entire defense. I replied, "No," and I stopped. (It was a yes-or-no question, after all.) Estes smiled, and things went smoothly after that.[1]

But there was indeed something remarkable about the various experiments Fred and I conducted together for several years, namely, that in many of them we had set aside – possibly for the first time in the long history of Harvard's operant laboratory – the opaque, sound-proof chamber in favor of thin, clear acrylic walls through which we could both watch and film our birds.

When you watch, and especially when you have the luxury of replaying a performance in slow motion, you see things that could never be detected in procedures that aggregate the characteristics of operationally defined responses across trials or organisms. Do "responses" even exist? Behavior is continuous, after all. How are we justified in calling two separate lever presses instances of the *same* response? As William James (1890) pointed out in his classic *Principles of Psychology*, we would never call two ticks of a clock "the same" tick. Skinner wrestled with this very issue in one of his earliest and most thoughtful papers (Skinner, 1935).

One of the first things you notice when you conduct experiments in open chambers (or, for that matter, in the natural environment outside the laboratory) is that organisms continue to behave even when you are not prodding or rewarding them. What's more, much of what they do is new, at least in some small way. The point was driven home to me on the day Fred and I reached a critical point in an experiment we ultimately called "The Spontaneous Use of Memoranda by Pigeons" (Epstein & Skinner, 1981). In the "symbolic communication" procedure, Jill had been a "speaker" (passing along a "message") and Jack had been a "listener" (receiving that "message"). At some point we decided to train each bird in the opposite role. Then, on that critical day, we removed the clear partition that separated the two sides of the chamber and put just *one* bird inside.

At first the bird behaved erratically, but, after a few minutes, a new and stable performance emerged "spontaneously" – that is, without any intervention on our part. In a sequence of steps that proved to be quite stable, the bird first behaved as a speaker; that is, it looked behind the curtain where a color was illuminated, then pecked the letter corresponding to the hidden color (the letter now lit up), and then *crossed the chamber* to the listener's side. Now the bird oriented toward and sometimes stretched toward the illuminated letter – sometimes repeatedly – and then pecked the corresponding color on the listener's panel. If the colors matched, a feeder was then operated automatically on the listener's side. Precisely the same performance emerged when we repeated the procedure with the other bird, and both birds repeated this sequence many times – again, with very few errors.

With each bird, two repertoires of behavior had come together spontaneously to produce a new sequence of behaviors that was meaningful in human terms. The birds were now, in effect, speaking to *themselves*, using the letter keys as memoranda to help them remember what color was behind the curtain. When we removed the curtain, the birds stopped using the letter keys; they eventually just went from color to color. When we restored the curtain, after first making a number of errors, they started pecking the letter keys again.

But how, precisely, could one account for the emergence of the new sequence?

A number of remarkable performances I had witnessed during my collaboration with Fred – especially the memorandum experiment – got me thinking obsessively about this issue for years.

3.1. *"Insight" in the pigeon*

The next experiment – the first of many I conducted along these lines with the help of a number of talented students – took on the gold standard of chimpanzee performances: the remarkably "insightful" problem-solving performances observed by Wolfgang Köhler in the early 1900s (Köhler, 1925). In one of the most memorable events that Köhler described, after a period of apparent confusion and frustration, a chimpanzee named Sultan was successfully able to grab some bananas that were hanging out of reach by placing a crate beneath the bananas and climbing – something he had never been taught to do. (Nearby chimps had

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[1] Even though all 10 faculty members asked me questions, my orals did go remarkably smoothly, in part because of one professor's kindness. A few days before the orals, he asked me to come to his office and furtively handed me a piece of paper on which he had written three questions. "These are the questions I'm going to ask you at your oral defense," he said. "Years ago, a professor did this for me, and now I'm doing it for you." At the orals, he sat in the back of the room and said nothing for just over an hour. Then he raised his hand to get Estes' attention and proceeded to ask me an impossibly difficult question, which, after a pause for effect, I answered confidently. Estes responded: "Well – I think we've all heard enough, don't you?" There were nods around the room, and that was that.

previously tried to reach the bananas merely by jumping, but their attempts had been fruitless.)

Could pigeons solve the classic box-and-banana problem? My students and I were able not only to get pigeons to solve the problem in a human-like way – first displaying signs of confusion, then rapidly moving a box beneath a small toy banana, then climbing onto the box and pecking the banana – but also, by varying the training histories of other birds, to show how various kinds of experiences contributed to the emergence of the solution. Birds that solved the problem had first had four kinds of experiences: they had been fed for pecking the toy banana when it was within reach; they had been taught to push a box toward green targets at ground level; they had been taught to climb onto a box and peck a toy banana suspended above it; and they had been exposed to the toy banana when it was out of reach, learning that jumping and flying to reach the banana never produced food.

Birds that had never been taught to climb pushed the box toward the banana but never managed to get on the box and peck the banana. A bird whose jumping and flying toward the banana had never been eliminated jumped and flew toward the banana in the test situation for several minutes before solving the problem. Birds that had never been taught to push were completely helpless. Our report appeared in *Nature* in 1984 (Epstein, Kirshnit, Lanza, & Rubin, 1984).[2]

The study not only demonstrated an elegant case of "the spontaneous interconnection of repertoires" – the phenomenon Fred and I had observed in the memorandum experiment – it also suggested that there is an orderly relationship between the particular experiences an organism has had and the type of novel behavior that emerges in a new situation.

But were the performances themselves orderly? In 1984, in a talk I gave at the annual meeting of the International Society of Comparative Psychology (ISCP), I offered moment-to-moment accounts not only of pigeons' performances in the box-and-banana experiments but also of the performances of humans attempting to solve Maier's (1931) classic "two-string" problem (Epstein, 1984b). The same principles, I argued, could help us understand why both pigeons and people behaved as they did moment to moment in time in new situations – and why such performances often resulted in the emergence of genuinely novel behavior. I also presented the basics of generativity theory.

3.2. Moment to moment

My students and I learned how to give running accounts of novel performances mainly by videotaping performances and then studying them repeatedly in slow motion, often reviewing the same recording dozens of times. The goal was to be able to apply well-established principles of behavior to what we were seeing with at least a reasonable degree of confidence. For the most part, we avoided "coding" the performances. Although coding sometimes has value, it necessarily simplifies what one is observing, directing attention away from all aspects of behavior that don't fit the coding scheme. In that sense coding is bit like putting the bird back into an opaque box.

Here is what happens when a pigeon that has had the four optimal experiences described earlier is faced for the first time with an out-of-reach banana and a small box located about a foot away on the floor of the chamber (for a more detailed account with supporting diagrams, see chapter 2 of Epstein (1996a); cf. Taylor et al., 2010; Wimpenny, Weir, Clayton, Rutz, & Kacelnik, 2009):

From the outset, the pigeon is presented with "multiple controlling stimuli" – stimuli which, because of the pigeon's training history, get multiple behaviors competing with each other: climbing, pushing, and orientating toward the toy banana. The pigeon has seen three kinds of stimuli that occasion those behaviors: the box alone, the box with the green spot present, and the banana over the box. Now it is seeing distorted forms of all three of those stimuli.

Onlookers will tend to view the pigeon's vacillations between box and banana as a sign of "confusion," because people often feel confused when faced with multiple controlling stimuli (imagine how you would feel and behave as you were approaching a defective stoplight on which the red and green lights were both illuminated). At first, the relative strengths of the competing behaviors are determined largely by the distance between the box and the banana, predictable from known parameters of a phenomenon called "stimulus matching" (Epstein, 1990, 1996a).

As the pigeon continues to look back and forth between banana and box, however – notably, with no food being delivered – each behavior rapidly grows weaker (the process of "extinction"), which in turn stimulates the resurgence of other behaviors that have been reinforced in this setting until, predictably, one repertoire necessarily wins the competition: pushing. Some climbing might occur first, but climbing will disappear rapidly because the stimulus that controls climbing (box under banana) is present only in a very distorted form. Pushing must inevitably win the competition also because jumping and flying toward the out-of-reach banana had recently been eliminated.

The pigeon pushes *toward* the banana because of the manner in which pushing blends with orienting toward the banana. (When behaviors – or "ideas," for that matter, which are just covert behaviors – compete, new sequences, blends, or both can occur; the particulars are determined by current stimuli and the topographies of the behaviors. Some behaviors cannot blend; others – such as spoken words – blend easily.) A type of generalization called "functional generalization" might also be occurring – a spread of effect from the green targets used in training to the toy banana (cf. Bird & Emery, 2009; von Bayern, Rutz, Heathcote, & Kacelnik, 2009).

As the box nears the banana, the relative probabilities of the competing behaviors change because of stimulus matching: the closer the box gets to the banana, the more salient the banana becomes, which increases the probability that the bird will orient and stretch toward it. What's more, the closer the box gets to the banana, the more likely it is that *climbing* will occur because the bird is creating closer and closer approximations to a stimulus it has seen many times: box under banana. Indeed, some birds will climb and stretch toward the banana prematurely and then resume pushing. When the box is beneath the banana, the old configuration is perfected, and the bird climbs and pecks. This last step is a superb example of what I have long called "automatic chaining" (or "autochaining"), the inadvertent production of stimuli that control other behaviors (imagine opening a refrigerator door and seeing a slice of chocolate cake that you did not know would be there).

Automatic chaining is a nearly ubiquitous phenomenon, facilitated by even a small movement; when a bird (or person) turns its head even slightly, its visual field changes, producing new stimuli that can change the probabilities of subsequent behaviors. We saw evidence of precisely that phenomenon repeatedly on our videos. And behavior not only *produces* stimuli; it also *is* a stimulus. When we speak or think or imagine or hum a tune, our actions serve as stimuli that can occasion other actions, just as surely as if we had turned the page of the book; automatic chaining, in other words,

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[2] To view an exceptionally human-like example of a pigeon solving the box-and-banana problem, visit http://www.youtube.com/watch?v=mDntbGRPeEU; at this writing, the video has been viewed more than 200,000 times. To watch a 30-min film about the Columban Simulation Project, visit http://www.youtube.com/watch?v=QKSvu3mj-14.

might be an important mechanism in associative thinking, dreaming, and other cognitive phenomena (Epstein, 2008).

In subsequent procedures with pigeons, my students and I were able to engineer “spontaneous tool use” (Epstein & Medalie, 1983), the “spontaneous interconnection of three repertoires” (Epstein, 1985c; cf. Luciano, 1991), and the “spontaneous interconnection of four repertoires” (Epstein, 1987). Over time, we were becoming better at producing more complex performances because the dynamics of the interconnection process were becoming clearer. In particular, we were becoming more adept at reproducing and controlling the two basic scenarios that get multiple behaviors competing: first, when recently effective behavior is now ineffective (extinction-induced resurgence), and second, when organisms are exposed to multiple, novel, or ambiguous stimuli that are related to stimuli known to control specific behaviors.

3.3. A formal theory

Verbal accounts are necessarily imprecise, which is why important relationships among variables studied in the natural sciences are virtually always expressed mathematically. The more adept I became at giving moment-to-moment accounts of continuous performances, the more frustrated I became by the lack of precision in those accounts – an extinction phenomenon. Behavioral processes do not operate one at a time, as my accounts mistakenly suggested, and a rich environment is always present which is not adequately described as consisting of a few “stimuli.” Most important of all, behavior is *continuous in time*. There *are* no responses; the “response” is just a construct, not a measurable property of the behavior of organisms (Epstein, 1982; Skinner, 1935, 1938). Could I replace or at least supplement my verbal accounts with formal theory?

In 1982 or 1983, I began experimenting with different types of equations and modeling techniques that might allow me to do so. An important step was to conjecture that *multiple behavioral processes are operating continuously on the probabilities of occurrence of multiple behaviors*. Those behaviors are occasioned by an interaction between the organism's behavioral history and the rich environment surrounding that organism, the appearance and composition of which are constantly changing, in part as a result of the organism's own behavior.

At some point, I settled on four simple equations that represented basic processes that probably operate simultaneously in most awake organisms most of the time: extinction, reinforcement, automatic chaining, and resurgence (Fig. 1). What would

(1) *Extinction:* $y_{n+1} = y_n - y_n * \varepsilon$

(2) *Reinforcement:* $y_{n+1} = y_n + (1 - y_n) * \alpha$

(3) *Resurgence:* for $\lambda_{yy'} < 0$ and $y'_n - y'_{n-1} < 0$,

$$y_{n+1} = y_n + (1 - y_n) * (-\lambda_{yy'}) * y'_n$$

(4) *Automatic Chaining:* for $\lambda_{yy'} > 0$ and $y'_n - y'_{n-1} > 0$,

$$y_{n+1} = y_n + (1 - y_n) * \lambda_{yy'} * y'_n$$

Fig. 1. *The transformation functions of generativity theory*. According to generativity theory, multiple behavioral processes operate simultaneously on the probabilities of multiple behaviors. In one possible instantiation of the theory, four basic behavioral processes are represented (above). y_n is the probability of behavior y at cycle n of the algorithm, y'_n is the probability of behavior y' at cycle n of the algorithm, ε is a constant for extinction (it determines the rate at which the probability of behavior y decreases over cycles of the algorithm), α is a constant for reinforcement (it determines the rate at which the probability of behavior y increases over cycles of the algorithm as a result of certain environmental events), and λ'_{yy} is the constant of interaction between behaviors y and y'.

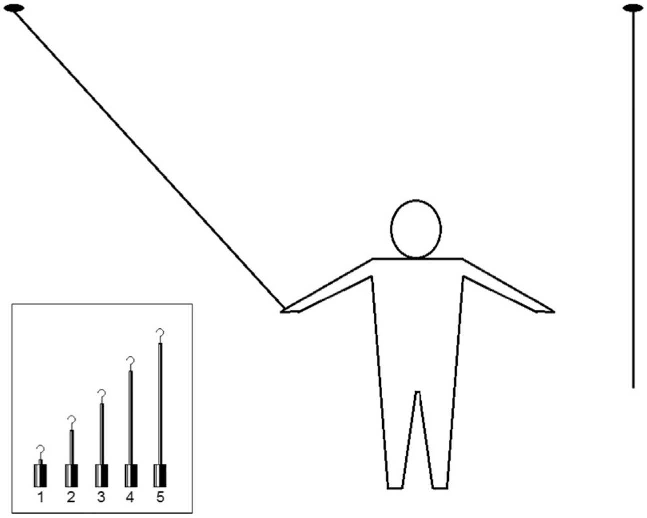

Fig. 2. Maier's (1931) *two-string problem*. Subjects are instructed to tie the two ends of the strings together, but they quickly learn that they cannot reach both strings at once. They learn this by pulling one string toward the other and reaching. Most people then try pulling the second string toward 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, if a subject is able to solve the problem at all, automatic chaining is usually involved. The person ties the long object to the end of a string and then pulls the object toward the second string; this is one way of using the object to extend one's reach. When that fails, the subject often lets go of the object, which causes the attached string to swing in a pendulum motion. The solution follows rapidly. Objects of intermediate lengths produce predictable outcomes according to those lengths.

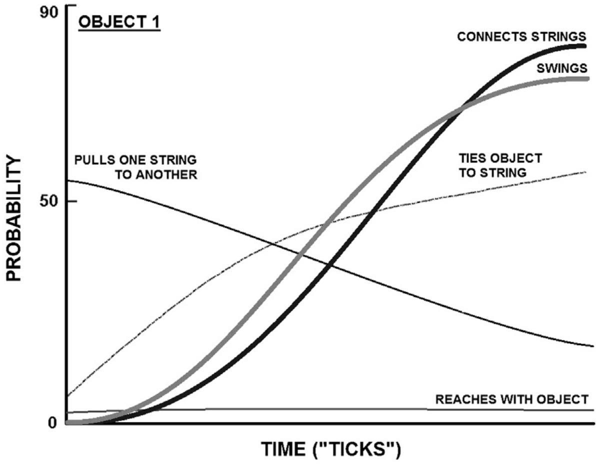

Fig. 3. *Probability profile for* Maier's (1931) *two-string problem*. A probability profile generated by the transformation functions shown in Fig. 1, generated for five behaviors relevant to Maier's (1931) two-string problem. The abscissa is labeled “ticks,” which are cycles of the computer algorithm, each a scalable moment of unspecified duration. The profile was generated with parameters for a short object (#1 in Fig. 2), which generally produced rapid solutions to the problem and no irrelevant reaching. Note that pulling one string toward 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. 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.

happen, I wondered, if all of these equations were operating simultaneously on some initial probability values for behaviors that were known to be under the control of stimuli present in the test situation? Just before the moment when I first activated the computer program that instantiated these ideas, I would have bet a large sum of money that the output of the program would be a meaningless array of random points on a graph. Instead, the program produced smooth, overlapping curves for each behavior, and the process of change over time was surprisingly interpretable. I called the resulting graph a “probability profile” and published the details of the theory about a year after my presentation at ISCP (Epstein, 1985b).

I had almost immediate success with the new modeling technique when I applied it to an analysis of video recordings I was accumulating of people trying to solve Maier's (1931) two-string problem in which subjects are asked to connect two strings that they cannot reach simultaneously (Fig. 2). With parameters appropriate to the structure of the problem, the algorithm quickly generated profiles appropriate to the variants of the problem I was presenting to different subjects. Fig. 3 shows a probability profile appropriate to a performance in which an individual is supplied with an object (“Object 1,” which is the shortest of five different custom-made objects we employed in the study) that makes solving the problem fairly easy (see the caption for details).

The curves predict a clear transition from one behavior to another: pulling one string toward the other (which doesn’t work and therefore decreases in probability throughout the session), tying the object to the string (highly likely with a short object, unlikely with a long object), setting one string in motion (creating a pendulum, the end of which one can easily catch while holding the other string), and then connecting the strings. The most important curve in the profile, however, is the lowest one, which indicates that the probability of using a short object to try to extend one's reach stays low throughout the session. Subjects who are supplied with a long object have trouble solving the problem because they repeatedly use the object to try to extend their reach, which can never result in a solution; a probability profile for this case shows a rapid increase in the probability of reaching. Until that probability begins to decrease again (extinction), probability curves for solution-related behaviors (tying and swinging) stay low. In this case, small parameter changes determine the difference between failure and success.

A probability profile predicts not only how multiple behaviors will change continuously in time, it also predicts arousal. Where a number of different curves overlap – and specifically where the sum of the probabilities exceed a value of 100% – the nervous system is probably overwhelmed in some sense, and people feel the competition among different behaviors (or “ideas”) as confusion or frustration. We found some anecdotal support for this hypothesis by interrupting performances on several occasions and

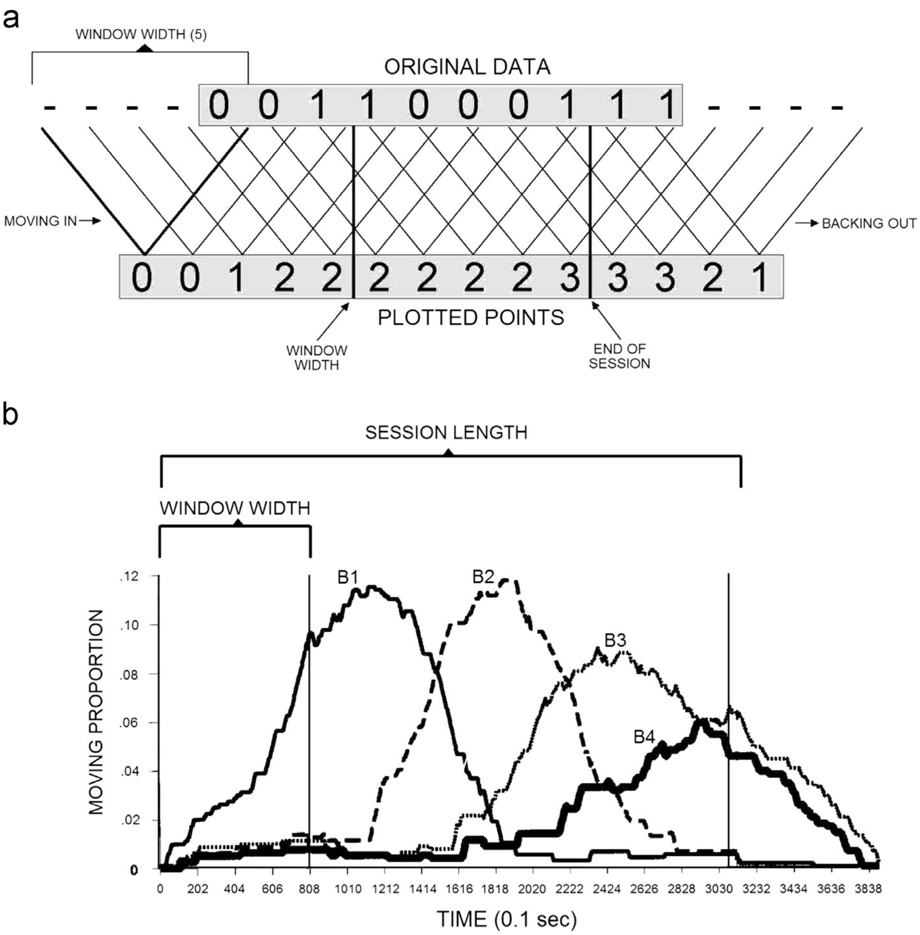

Fig. 4. *How to construct a frequency profile*. A frequency profile reveals the orderliness in the moment-to-moment behavior of an individual subject by plotting occurrence/nonoccurrence data (1 s and 0 s) in a way that maximizes the discriminability of curves that represent the various behaviors occurring in a given setting. (a) The curve for each behavior is constructed by computing a moving proportion (or moving sum) for the occurrence/nonoccurrence data for that behavior, ideally recorded in bins of 0.1 s or smaller. Three different parameters – window size, resolution, and step size – can be varied. (b) Plotting the moving proportion (or sum) yields a three-part graph in which the first third, which matches the window size, provides information about the beginning of a session and the second third shows clear transitions from one behavior to another during the session.

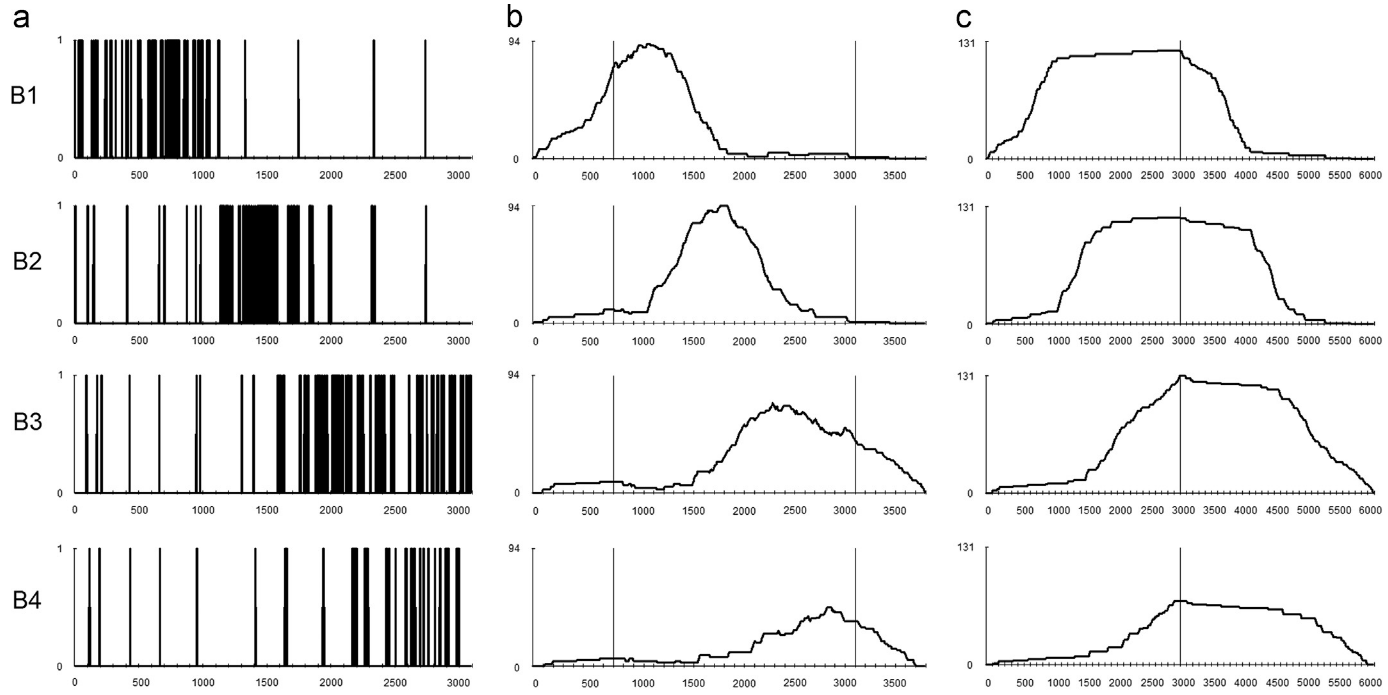

Fig. 5. *Optimizing frequency profile parameters*. With both resolution and step size held constant at 1 (to preserve as much information as possible about the recorded occurrence/nonoccurrence data), changing the window size has a dramatic effect on the resulting curves. (a) A window size of 1 shows the raw data – the equivalent of check marks on a behavior checklist. (c) A window size equal to the total number of bins (the observation intervals) in the session yields the traditional and marginally informative cumulative record (evident to the left of the vertical line in the center of the graph). (b) A window size of roughly 25% of the total number of bins in the session yields curves that optimally differentiate the behaviors, that suggest their dynamic interaction, and that reveal what appears to be an orderly transition from one behavior to another over the course of the session. These curves also resemble the probability curves produced by the transformation functions of the generativity model (Fig. 6).

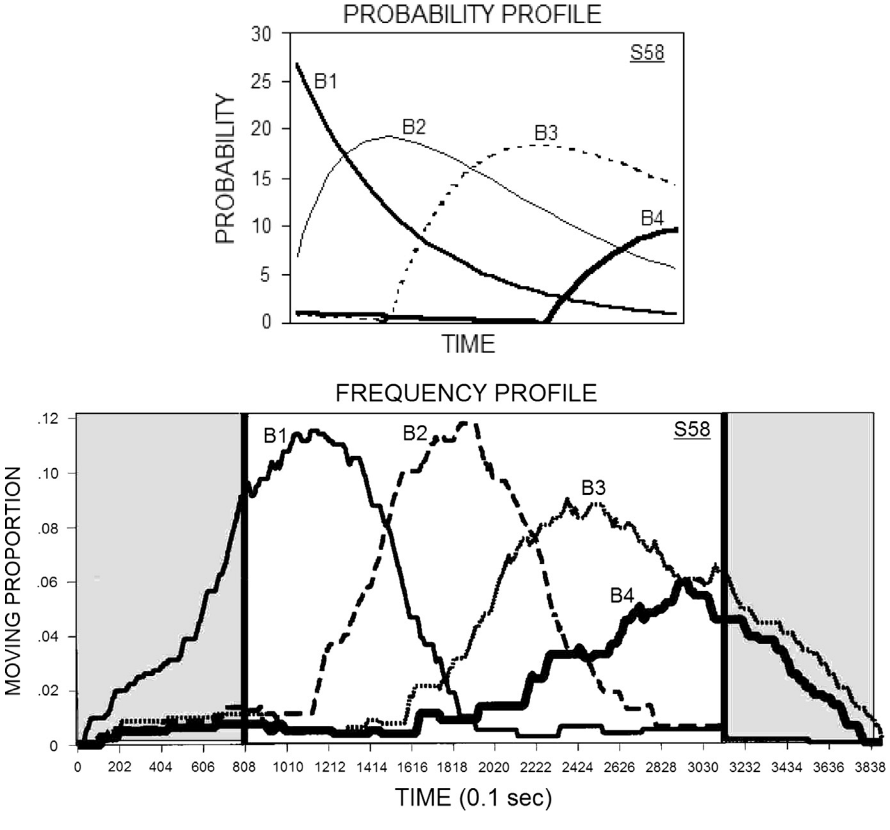

Fig. 6. *Predicting individual behavior moment to moment in time*. (a) This probability profile, produced by the transformation functions of the generativity model, predicts the behavior of a human subject on a touch-screen task. The subject has been instructed to move a spot across the screen into a goal area. Tapping three patches on the screen (B1, B2, and B3) will move the spot in various directions and at varying speeds; tapping a fourth patch (B4) has no effect. The model predicts that the subject will begin tapping B1, then gradually shift to B2, then gradually shift to B3, with responses alternating among the three choices along the way (where the curves overlap). It also predicts that toward the end of the session, the subject will begin tapping B4, even though doing so has no effect. (b) This frequency profile shows actual data obtained from one subject (S58) during a 5-min session. The pattern of responding is predicted well by the probability profile, including the shift to B4.

asking people (a) how they felt and (b) what they were likely to do next. When behavioral competitions were intense, people indeed reported feeling frustrated and, of greater significance, they seemed to have little or no idea what they were about to do.

How *could* they know? A great many computations are required to generate a probability profile; real people are too busy *behaving* to do any computing, assuming they are even capable of such a feat. Our next step, of course, would have been to collect physiological arousal data throughout each session; we never got to that point, unfortunately (see the Postscript below).

3.4. An Informative graphical method

By 1985, I knew a lot about how to graph probabilities, but I had no way to generate a comparable graph for an actual performance by an individual. If four behaviors are in competition at some point during a session, we might see signs of frustration, and we will probably see switching among the behaviors. That kind of performance looks quite disorderly, and it certainly *feels* disorderly to the subject. Was there a way to tease out the orderliness in an apparently disorderly performance, assuming, of course, that the orderliness was there to be found?

I thought about this issue for 5 years before I saw a possible solution to the problem; then it took an *additional* 5 years to bring it to life computationally, even though – as these things almost always turn out in retrospect – the solution was not that complicated (Fig. 4).

I called the graph that reveals the orderliness in a unique performance by an individual a “frequency profile,” which turns out, parametrically, to be a sensible compromise between the old behavior checklist and the cumulative record (Fig. 5). Two of my associates and I described this graphical technique in a paper we presented at the 1999 meeting of the American Psychological Association (Epstein, Thompson, & Crawford, 1999).

A frequency profile is generated by computing a moving proportion (or sum) of occurrence/nonoccurrence values (Fig. 4); for good resolution, one would record occurrence/nonoccurrence data for a number of different behaviors at least every tenth of a second. This means either laboriously coding a video recording by hand (which my students spent many hours doing), or, to the extent that it is possible these days, using software that will code a video recording automatically. As an alternative, one can record tasks that are performed on a touch-screen or that are tracked using other electronic devices; that way, from the outset, one can easily monitor a number of different behaviors (as they are defined by one's equipment) in high resolution – even in real-time.

One could easily attempt the real-time prediction of an individual's behavior using the methodologies I have described. If a number of different behaviors were being coded and tracked digitally in real-time, a frequency profile would be easy to generate continuously. On the prediction side, probability curves could be generated using the generativity model and then continuously refined and updated based on feedback from the actual performance. The accuracy of one's predictions over upcoming intervals of time could then be quantified. This is how real-time prediction is done in weather forecasting, electrical engineering, and other domains. To my knowledge, it has not yet been done with organisms, but it wouldn’t be difficult to attempt it – even with human subjects.

Warning: as I noted earlier, coding *simplifies*. That said, a frequency profile instantly reveals the orderliness in the continuous behavior of a single organism that is, to my knowledge, unparalleled by any other graphical method. What's more, it generates curves that look very much like those of the probability profile, allowing both for statistical comparisons and an immediately informative visual confirmation of the accuracy of one's predictions (Fig. 6).

3.5. Postscript

Although I continue to explore the practical applications of generativity theory (e.g., Epstein, 1996b, 1996c, 1997, 2000a, 2000b, 2011; Epstein, Schmidt, & Warfel, 2008; Epstein, Kaminaka, Phan, & Uda, 2013; Epstein & Phan, 2012), my pertinent laboratory research ended fairly abruptly in the early 1990s after several failed attempts to obtain funding to attempt the real-time prediction of human behavior. I had conjectured that on reasonably complex touch-screen tasks, a real-time model could stay ahead of a subject by 1 or 2 s – possibly longer on simple tasks – and that this technology might improve rapidly over time. Perhaps some people found this idea disturbing. It is one thing to predict that a pigeon will needlessly peck a moving spot of light a thousand times over the next hour and quite another to predict complex human behavior moment to moment in time.

Competency-based creativity training derived from generativity theory has been shown to have value (Bosiok, 2013; DeTienne & Chandler, 2004; Epstein et al., 2008, 2013; Epstein & Phan, 2012; Miller et al., 2013), and generativity theory is sometimes mentioned in articles or books that offer practical advice for boosting creativity or innovation (e.g., DiChristina, 2008; Greene & Rice, 2007; McCorkle, Payan, Reardon, & Kling, 2007; Weinstein & Morton, 2003; Woodman, 2007; Zmuda, 2010). A small but substantive literature has also grown over the years on resurgence (e.g., da Silva, Maxwell, & Lattal, 2008; Reed & Morgan, 2006; Sánchez, & Nieto, 2005; Wacker et al., 2013); Wilson and Hayes (1996) even showed that derived stimulus relations will resurge in human subjects under certain conditions. But in scientific publications generativity theory itself has received little more than mentions, citations, or brief summaries (e.g., Auersperg, Kacelnik, & von Bayern, 2013; Boles, 1990; Bujedo, García, Fernández, Domínguez, & Zayas, 2004; Cook & Fowler, 2013; Hovell, Wahlgren, & Adams, 2009; Johnson & Layng, 1992; Manrique, Völter, & Call, 2013; O’Hara & Sternberg, 2001; Potter & Wilson, 2011; Rastall, 2010; Runco, 1993; Runco & McGarva, 2013; Shettleworth, 2010, 2012; Simonton, 1999; Stahlman, Leising, Garlick, & Blaisdell, 2013). To my knowledge, no one other than I has ever used the transformation functions, the corresponding computer modeling technique, or the frequency or probability profiles to predict or analyze behavior. In short, the full potential of generativity theory has yet to be explored.

In an interview published in 1968, Skinner was asked about an issue that had troubled him early in his career: What do you think about the concept of response? He replied: “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)).

Generativity theory is such a formulation, and even if it is incorrect in its particulars, I believe that there are distinct advantages in trying to understand behavior for what it really is: a natural physical phenomenon that is both continuous in time and probabilistic yet orderly in nature.

Acknowledgments

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