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Joint Control: A Discussion of Recent Research

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The discrimination of the onset of joint control is an important interpretive tool in explaining matching behavior and other complex phenomena, but the difficulty of getting experimental control of all relevant variables stands in the way of a definitive experiment. The studies in the present issue of *The Analysis of Verbal Behavior* illustrate how modest experiments can take their place in a web of interpretation to make a strong case that joint control is a necessary element of such phenomena.

Key words: joint control, verbal behavior.

When one is in a familiar context, and in the presence of a familiar audience, many verbal operants may be strong concurrently, for each feature of such an environment, by itself, is likely to have pervasive evocative effects on verbal behavior. The combined effect of many such features is no doubt incalculably complex. In relation to this welter of interacting variables, verbal behavior, as emitted, is remarkably coherent. Sometimes one's utterances display slips or blends, but more commonly, only one verbal operant is emitted at a time. Despite the unity implied by the dominance of one verbal operant over all others at any moment, that operant is likely to have been potentiated by more than one variable. No matter how much we would like a glass of water, we are not likely to ask for one in the absence of an audience or in the absence of a source of water. One can plausibly assume that all verbal behavior, to varying degrees, is evoked by concurrent variables.

Sometimes the form of a verbal response betrays its multiple sources of control. When we read that *Representative Ball dismissed the president's recent bounce in the polls,* we suspect that *bounce* was emitted in preference to *jump* or *rise* because of the intraverbal control from *Ball.* Similarly, the statement that a track star *jumped at the chance* to compete suggests at least two sources of control of the metaphor. When writing, I often find myself repeating distinctive response forms when many adequate synonyms are available, indicating that the form is under both thematic and echoic control. For example, in the paragraph above, I originally wrote the phrase *at any time* on two successive lines. Since such repetition of a conspicuous phrase "clangs in the ear," I changed the second to *at any moment*. The "clang" is a distraction to the reader for the very reason that one source of control, the echoic, is irrelevant to the theme, and consequently, writers usually try to avoid iterations of distinctive phrases. Skinner (1957) devoted most of Chapter 9 of *Verbal Behavior* to similar examples culled from his own experience, and doubtless the reader can supply many more.

The concept of joint control—the confluence of two controlling variables on a single response form—is therefore not new. Rather, it is a special case of the nearly ubiquitous phenomenon of multiple control, which has been discussed at length by Skinner. What is new is an appreciation of the role that joint control plays in complex behavior. Specifically, Lowenkron has proposed that the onset of joint control can be a discriminable event; moreover, that event can serve as a controlling variable in novel delayed matching-to-sample performance and other complex behavior. (See Lowenkron, 1998, for a comprehensive account.) In the light of my comments above that multiple control is the nearly universal rule rather than the exception, it might be hard to see how joint control could be of interest. However, important cases arise when we confine our consideration to those examples of joint control in which each of two stimuli can plausibly be assumed to exert control over a single dominant response rather than over broad classes of responses of roughly equal strength.

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That the onset of joint control, so defined, can be a discriminable event is not in doubt. Surely everyone has had an experience like that recounted by Skinner:

> In the grade school that I attended as a child, a single teacher taught two grades in the same room. While one class recited, the other worked on its assignments. One day in third grade, when the teacher was talking with the other class, I raised my hand, waved it wildly to attract her attention, and said, "I was *reading* the word 'middle' just when you *said* it." (Skinner, 1978, p. 171)

This anecdote illustrates not only that joint control is discriminable but that it can serve as a controlling variable for a self-report. Something of the sort is all that is required by Lowenkron's analysis of selection-based performance on matching tasks.

That joint control can be a controlling variable may not be controversial, but that Lowenkron's work has only slowly begun to generate further research and discussion suggests to me that many behavior analysts are not aware that there is a thorny problem to be solved and that a consideration of the role of joint control solves it. Certainly the lay person thinks there is nothing to explain: In a matching-to-sample task, we pick the correct comparison because it *matches* the sample; in a delayed task, we pick the correct comparison because we *remember* the sample. Nothing could be simpler. But that is no explanation at all; its force derives entirely from the circumstance that the relevant performance is commonplace and automatic. To put it loosely, in a novel example how do we *know* that one stimulus matches another? It is not enough that two stimuli are in fact identical in form, color, pattern, and so on; "identity" itself is not a stimulus property. When stimuli become sufficiently complex, we are unable to tell whether they are identical. For most westerners, Chinese characters or Arabic script would serve as an example. We can discriminate similar symbols of that sort only by examining them closely, that is, by engaging in sequences of systematic coding responses. It is this scrutiny that controls the judgment of identity. Conversely, two stimuli that are not identical might be identified as such; a counterfeit copy of the *Mona Lisa* might be discriminable to an expert but not to the rest of us.

It is perhaps more plausible to dismiss the concept of "identity" entirely and to assume that we match stimuli according to the discriminated responses they evoke: two stimuli might evoke the same response and therefore be judged identical. If we see a pattern as a star, we might match it with another pattern that we see as a star, even if the stars are different. We might tact a wide variety of physically different paintings as *Mona Lisa* and match them accordingly. However, although this interpretation solves some problems of interpretation, it does not solve them all, for response-produced stimulation is still stimulation; we are left with the puzzle of how to tell that two stimuli, response-produced or otherwise, have "matched," in the absence of a specific history of matching such stimuli. Lowenkron's analysis solves the problem in a general way that is independent of the particular stimuli in any example. There is a saltation in response strength when the second of two discriminative stimuli that control a given response form is introduced. This saltation itself has stimulus properties that putatively generalize from one example to the next.

Lowenkron's most easily grasped example of joint control is illustrated in the Gutierrez paper (cf. Lowenkron, 1998, 2004). When we attempt to find a string of random digits in a table of many such strings we find ourselves rehearsing the target string while attempting to tact the strings in the array. In only one case are the tacts compatible with the echoics; that is, the same response form is evoked by both variables. The effect is usually conspicuous.

The particular stimuli that jointly control a response are specific to the example at hand, but the saltation in response strength is general from one example to the next, and it is presumably this stimulus property that serves as the controlling variable for selection responses in matching tasks. The suggestion that a change in response strength is a discriminable property of an event is itself not new. In his discussion of the descriptive autoclitic, Skinner (1957) wrote, "Another group of autoclitics describe the state of strength of a response" (p. 315). Autoclitics such as *I guess, I hesitate to say, I hear, I insist, I swear,* and so on, all suggest control by response strength. Joint control is simply the control of a behavior in certain contexts by a jump in response strength arising from the confluence of two variables.

The role of joint control is itself conditional,

of course. The strength of many responses in one's repertoire is constantly in flux, so such changes in themselves would have no general significance; they merely attest that the environment is always changing. However, in certain contexts, such as those in which a problem has been posed, such fluctuations can be an important component of the sequences of behavioral and contextual events that evoke the solution. Thus, in matching tasks, we learn to exploit joint control, for only the matching comparison enters into joint control in that particular context.

Unfortunately, as Lowenkron notes in his paper (this issue), a joint control interpretation of matching-to-sample is partly interpretive. Although saltations in response strength may be discriminable to the subject, they are covert to other observers. Moreover, the history responsible for establishing joint control as a discriminative stimulus for correct matching is usually buried in the subject's past. Science must attack phenomena of this sort with a combination of experimental and interpretive work, as Lowenkron has done. A complete experimental analysis is probably out of reach, but research can become persuasive when it takes its place in an interpretive scheme.

The research in the present issue of *The Analysis of Verbal Behavior* illustrates this point. Several of the studies were conducted by graduate students working under deadlines, without grant support, with only modest resources, and were imperfectly controlled. Nevertheless, although alone they are narrow and sometimes ambiguous, they each take their place in a broad interpretive scheme and offer incremental support for it.

The Sidener and Michael study is a replication of Lowenkron (1984), and the Wright study is a replication, with improvements, of studies by Whitehurst, Ironsmith, and Goldfein (1974) and of Silvestri, Davies-Lackey, Twyman, and Palmer (in preparation). Replication of experiments is a central and necessary part of science, and such studies always serve a useful purpose in confirming and generalizing results, but they are particularly necessary in studies of human behavior, in which histories are variable and tight control is seldom possible. The Sidener and Michael study confirms that relational matching-to-sample in pre-school children is impaired when one of two sources of control is eliminated. The results also illustrate the importance of evaluating the data in the context of a broad interpretive scheme, for the data cut both ways: By hypothesis, joint control is a *necessary* controlling variable for correct performance and explains how correct performance emerges; however, when coding responses were prevented, subjects still performed correctly on 40% of the trials, on average. My back-of-the-envelope calculations suggest that the deviation from chance accuracy (25%) is not quite statistically significant, but it raises a possible challenge to the joint control account. However, I believe that Sidener and Michael have interpreted the results correctly: The contingency imposed by the experimenters was not the only source of joint control. They point out that "as overt forms of 'coding' are prevented, discouraged or punished, increasingly covert forms of similarly functioning behavior might emerge." Since this is just what one would expect within the broader interpretive scheme, this suggestion is not circular. Moreover, it is supported by the observation that some children who were prevented from using the experimentally controlled coding response attempted to use *ad hoc* coding responses.

The Gutierrez study is a good example of a study that necessarily dovetails with an interpretive scheme, and because of the complexity of the behavior under consideration, cannot stand independent from it. The subjects were adults, and it was assumed that correct sorting of four pictures would emerge from the joint control by covert rehearsal of the target sequence and covert tacting of cards as they were arranged on the table. The only relevant observable responses were the sorting of cards and engagement in the distraction task. Since much of the behavior of interest was covert, the data necessarily lend themselves to a variety of interpretations. However, the study supports the joint control interpretation in this way: It was designed to falsify the account, and it did not. When rehearsal was prevented, correct performance deteriorated to chance levels in five out of six subjects. As was the case in the Sidener and Michael study, one subject appeared to surmount the obstacles the experimenter put in the way of covert rehearsal, and her performance on the target task was well above chance.

The results of the experiment permit an alternative interpretation, however. One might

argue that the distraction task of reciting a nursery song interfered with performance not because it disrupted covert rehearsal but simply because it was a distraction; on this view, any competing behavior might have been equally disruptive. I think this alternative is implausible, but it could be evaluated by adding a control condition in which the distraction task did not compete with covert rehearsal. For example, subjects could tap out a pattern of beats with their feet as a distraction task. Such a task would plausibly be equally distracting but would not compete with covert rehearsal of the target sequence.

The five papers in this series are unusual in that only one of them used subjects with disabilities. That is a welcome trend, but an important advantage of working with children with disabilities is that some behavioral events, ordinarily covert or fleeting, are more conspicuous in that population. The Tu study illustrates this advantage, for she was able to build up effective performance in her subjects by adding, step by step, the putative components of the discrimination of joint control. Only when all components were in place were the children consistently successful in the manded selection task. In typical children such components are acquired very early, and relevant behavior may be covert. It might be possible to do a comparable study with very young children of normal abilities, but such children often lack the kind of secondary skills that would make them tractable experimental subjects (sitting still, waiting, taking turns, etc.). Notwithstanding the logic of the experimental design, I was surprised by the robustness of the effect and was oddly relieved that two subjects in the second experiment performed accurately without engaging in self-mimetics. It is experimentally convenient when all behavior of interest appears in a measurable way in its expected place in a complex performance, but much human behavior is not so accommodating. Tu suggested that the discrepant data might be due to the emergence of covert responding (which presumably mediated performance in the other studies discussed) or by changes in the size of the effective unit of response. There is a third possibility that I will discuss later.

The Lowenkron study is another example of the effective interplay of interpretation and experimental analysis. Lowenkron's appreciation for the role of joint control in selection behavior emerged inductively from early experiments on selection-based verbal behavior (e.g., Lowenkron, 1984, 1988, 1989) and was formulated in increasingly complete conceptual analyses (Lowenkron, 1991, 1998, 2004). The account is elegant but difficult to demonstrate unequivocally, since experimental control over all relevant variables is typically unattainable. However, owing to its parsimony and plausibility, there is a strong presumption in its favor. Lowenkron's study (this issue) is deductive, in the sense that it asks what must follow if the joint control account is correct; it them puts those deductions to experimental test. Deductive, theory-driven research is the norm in psychology, but interpretation-driven research differs from most such work in an important way: All of the terms in the account have an independent empirical foundation; no elastic, free-standing hypothetical constructs are invoked.

His study evaluated the role of both legs of his joint control account, tacting and self-echoics. Experiment 1 showed that effective performance of the selection task depended on accurate tacting, a necessary result if the joint control account is correct. The data in Experiment 2 were less clear. In one condition the prevention of rehearsal did not lead to any decrement in accuracy. However this finding serves as the foundation for a distinction between stimulus selection in conditional discrimination and stimulus specification under joint control. Lowenkron's discussion of this distinction is both original and thought-provoking.

Lowenkron speaks of tacting and self-echoics as the two components of joint control. He is being careful and precise in doing so, since his work has shown the relevance of those behaviors in the account. Although there are many examples of joint control by self-echoics and tacts, I believe that the phenomenon is more general and that it extends to any case of a discriminable saltation in response strength.

As an illustration of the point, consider the following demonstration I carried out a few minutes ago. My 13-year-old daughter, like most American children, at one time had learned the names of the capitals of all the states, but I thought it unlikely that all of them were currently strong in her repertoire.

"What is the capital of North Dakota?" I asked.

"Um . . . Hmm . . .," she mumbled.

"Never mind. Forget it . . . Now I'm going to list some famous 19th-century Europeans. Here we go: Napoleon, Tallyrand, William Gladstone, Benjamin Disraeli, Otto von Bismarck, Gordon . . . ."

"Bismarck!" she cried.

Such examples of the stimulating effect of a supplementary cue are everyday phenomena. It is evident that *Bismarck* was uttered in strength not because it was a mere echoic to a name—she did not echo *Disraeli* or *Tallyrand*—but because it satisfied the previous contingency. But since the question alone did not evoke the target response, it is equally clear that both sources of control were necessary. That is, the response was under joint control.

One source of control was the spoken word *Bismarck,* but it is evident that the second source of control could not be a covert self-echoic, for she had not yet emitted it in any form. Moreover, it isn't clear what the second source of control could be. Any account of this commonplace performance must be very speculative, but we must not shrink from speculating. I believe that our account will turn out to be common to "insight," "recognition," and many examples of problem solving.

My own interpretation is that the question *What is the capital of North Dakota?* did indeed alter the strength of the response *Bismarck,* along with a host of other related operants. (See Palmer & Katz, 2005, for a related discussion and some empirical support of this thesis.) However, a response can be increased in strength without actually being emitted. (This is necessarily true, since we are continually bombarded by countless discriminative stimuli evoking, in many cases, mutually incompatible responses.) *Bismarck* was strong—it was "on the tip of her tongue," as we say—but it was not quite strong enough to be the prepotent response at that moment; competing responses were stronger. Likewise, the list of names of famous Europeans potentiated other responses, such as the echoics *Napoleon, Tallyrand,* and *Gladstone,* which all must have been at extremely low strength a few moments previously. However, when the word *Bismarck* was presented as an auditory stimulus, it potentiated a response form that was already strong. The discrepant jump in response strength was a salient event that identified the response as "the answer" (and no doubt initiated a cascade of other discriminative behavior that may be relevant as well).

On this account, joint control does not necessarily require the simultaneous presentation of discriminative stimuli. The question about the capital of North Dakota preceded the name *Bismarck* by a half-minute or so. Such examples suggest that the potentiating effect of a discriminative stimulus on a response must endure for that long and possibly for much longer. You have probably not thought about Benjamin Disraeli for many a month, but if you encounter the name tomorrow, or next week, will you not "recognize" it with particular strength? The example is inexact, since some new learning is probably occurring in the context of this discussion. However, there are anecdotal reasons to think that a strong response remains potentiated for a period of time, even if it is not emitted. People commonly report that when they work on a problem but do not solve it, a supplementary stimulus encountered hours or even days later can evoke relevant discriminative responses with great strength. ("Eureka!")

This proposal is seemingly incompatible with evidence from the present studies that self-echoic behavior is *required* if imposed delays are to be mediated. When echoics were disrupted, performance deteriorated. However, I am not suggesting that discriminative stimuli invariably have enduring effects, only that they can have them under some conditions. In the studies under discussion, the responses being echoed were weak, and they evoked other behavior only weakly. Mandarin words, the orientation of symbols, dots, and lines, "Checksol-clip," and so on are neither distinctive nor evocative for typical subjects. If quite evocative stimuli had been used, such as pictures of family members, accurate responding would surely have emerged without rehearsal. Nevertheless, the proposal that discriminative stimuli can have enduring effects, even when not rehearsed, is compatible with the serial-position gradient that Lowenkron observed in his study, and it may account for some of the anomalous results of the studies in which the disruption of rehearsal did not have expected effects. Thus, in my opinion, joint control, as formulated by Lowenkron, is just the most ex-

perimentally tractable end of a range of important related phenomena.

This digression has prepared the ground for a discussion of the Wright study, for the role of joint control in her study is quite different from its role in the others. Her study was not a test of joint control; rather, its purpose was to show that a grammatical construction (the passive) can be acquired through modeling in the absence of explicit shaping contingencies. The shaping appears to have been accomplished by the reinforcing properties of "achieving parity" with the behavior of the model. I believe that this is an important demonstration, but I will confine my remarks to those aspects of the study relevant to the topic of joint control.

Joint control helps explain the behavior of subjects in this experiment, behavior that, although commonplace, would otherwise be difficult to interpret. Perhaps it is best to use an analogy. When we listen to a familiar tune, the music evokes some sort of conditioned behavior. Ordinarily this behavior is unobservable, but it need not be. Suppose, for example, that you are tapping your fingers to the tune of the "William Tell Overture." Tapping is a kind of overt listener behavior. The tapping is rhythmic and is partly controlled by the auditory stimulus and partly by the conditioned rhythmic pattern; that is, the tapping is under joint control. In the middle of an exciting phrase, if the music were instantly to change to "Greensleeves," the fact of joint control would be conspicuous; your tapping would not instantly track the new tune; there would be a jarring discontinuity. When we listen to familiar music, or familiar songs, or familiar verbal expressions, our "listener behavior" is jointly controlled by the familiar pattern of stimuli and by the current auditory stimulus.

Now consider the case of the child who sets out for the first time to play a familiar tune on an unfamiliar instrument, say, a xylophone. When the child hits the correct keys, his "listener behavior" is jointly controlled by the resulting auditory stimulus as well as the covert (or perhaps overt) behavior of singing along. When the correct key is hit, behavior is under joint control; when the wrong key is hit, joint control ceases abruptly. Thus joint control as a discriminable event may be the conditioned reinforcer putatively underlying imitation, generalized imitation, and all conformity to patterns established by a social community. To put it loosely, we "recognize" that we have matched when joint control occurs; we cry, "That's it!" The establishment of joint control as a conditioned reinforcer in such contexts doubtless occurs very early for children and is presumably conditional upon other variables. (Sometimes conformity is reinforced and sometimes it is not.)

In the Wright study, the repetitive presentation of the passive construction established it as a familiar intraverbal frame. The child's turn to speak was analogous to sitting down in front of a xylophone. When the child uttered a pattern that conformed to the familiar frame it was under joint control, hence was reinforced. When the child deviated, there was no joint control, and this was a conditioned punisher.

On this account, the acquisition of patterns of verbal behavior—patterns conventionally said to reflect grammar—can be understood as the outcome of moment-to-moment fluctuations in conditioned reinforcement, partly owing to joint control. If this account is correct, the domain of joint control is great indeed.

Joint control is a tool in the workshop of the behavior analyst who would understand complex behavior. It is not a new phenomenon, nor does an analysis of joint control invoke new principles. It has been lying in the toolbox all along, but we are only beginning to appreciate its role in the control of human behavior. I believe that it will have a distinguished future. But it is not easily studied. The authors of the papers under discussion are to be commended for bringing an important but formidable phenomenon under the lens of experimental analysis.

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