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On Terms

Stimulus Control Shaping and Stimulus Control Topographies

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Some time ago, Sidman and Stoddard (1967) suggested the term "stimulus shaping" to refer to certain procedures that sought to establish new controlling environment-behavior relations. In their proposal, the term would directly parallel "response shaping." Whereas response shaping refers to gradual changes in the response requirements of a reinforcement contingency, stimulus shaping would refer to gradual changes in the stimulus control requirements. Consistent with this usage, Ray (1969) sought to introduce the term "stimulus control topography" into the language of behavior analysis. The term was intended to parallel "response topography." Whereas response topography differentiates members of a functional response class, stimulus control topography could serve to differentiate members of a functional stimulus class.

These two proposals were straightforward, potentially useful, and seemingly obvious extensions of already familiar behavior analytic concepts and terms. But they were not well received. Few behavior analysts today use the term "stimulus shaping," and when they do the usage is often not consistent with Sidman and Stoddard's proposal (see below). "Stimulus control topography" appeared in a few early papers (e.g., Stoddard & Sidman, 1971), but it has largely disappeared from the verbal behavior of behavior analysts—even those sympathetic to Ray's proposal.

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This history notwithstanding, we continue to find it useful to talk and think in terms of "stimulus shaping" and "stimulus control topographies" in our laboratory research. When we write papers, however, we often find ourselves adopting terms that seem less precise merely because we judge them less likely to mislead or confuse our audience. Given the interests of many behavior analysts in stimulus control and the high level of related research activity, the time seems ripe to reconsider and expand upon the proposals of Sidman and Stoddard (1967) and Ray (1969).

Stimulus Control Shaping

Sidman and Stoddard's "stimulus shaping" proposal was made while the authors were reporting an early study of fading in teaching form discriminations. Their paper clearly identified stimulus shaping as a generic term for a class of procedures that are used to develop new stimulus-response relations. The defining property of that class was gradual stimulus change arranged to help the subject learn. Subsequently, stimulus shaping was defined in another way as a label for a specific teaching procedure that gradually transformed stimulus shape (e.g., Schilmoeller, Schilmoeller, Etzel, & LeBlanc, 1979). This definition eliminated the generic character of "stimulus shaping," and encouraged further terminological differentiation. On its face, further differentiation might be seen as helpful by increasing the likelihood that experimental procedures would be communicated precisely. But not everyone has found it helpful. For example, Deitz and Malone (1985) suggested that the shaping/fading distinction was unnecessary. They proposed that one term be selected as *the* term for teaching procedures that featured a program of gradual stimulus changes.

Deitz and Malone's paper also identified another terminological issue—the appropriate classification of delayed prompting procedures (Touchette, 1971). Delayed prompting procedures do not program gradual changes in stimulus size, shape, intensity, or other features. Rather, these procedures merely superimpose an effective controlling stimulus (the prompt) on an ineffective one (the target) and then delay the prompt (typically gradually) to encourage transfer of stimulus control to the target. Deitz and Malone concluded that the delayed prompting procedure differed sufficiently in its requirements to merit classification separate from procedures that gradually transformed stimulus size, shape, or intensity. Progressive delay programming, however, seems based on the supposition that gradual increases in the delay prior to the prompt gradually influence some aspect of behavior related to observing the target.[1] In our view, therefore, it would be reasonable to use one term to refer generically to *all* procedures that arrange gradual, progressive changes to promote stimulus control transfer.

We propose *stimulus control shaping* as the generic term. This term has been selected to retain Sidman and Stoddard's parallel to response shaping and also to separate it from specific types of programmed stimulus changes. Under this proposal, procedures termed stimulus fading, stimulus shaping (with reference to stimulus shape transformations), and delayed prompting may all be seen as equally valid exemplars of stimulus control shaping.

As a generic term, stimulus control shaping need not cloud distinctions among various procedures. Indeed, it will likely prove useful to continue differentiating subclasses of major stimulus control shaping procedures. The generic definition, however, permits one to use stimulus control shaping freely to refer to procedures that do not fit comfortably into one or another of these subclasses. For example, we are currently working on techniques for teaching people with severe mental retardation to discriminate circumstances in which a given form discrimination is possible (e.g., A vs. B) from those in which it is not (A vs. A). One procedure under study uses new computer methodology to arrange within-trial changes in stimulus intensity ("dynamic fading"); the initiation of stimulus change is scheduled in a manner analogous to that used in delayed prompting. Because our procedure combines several features of existing methods, it is not readily classifiable under the current terminological framework.

Beyond handling complex procedures, the generic term "stimulus control shaping" would be convenient when different but conceptually related teaching approaches are employed in the same study. For example, we recently reported a study of novel stimulus control procedures for computer-assisted teaching of spelling prerequisites to mentally retarded students (Dube, McDonald, McIlvane, & Mackay, 1991). The procedures are complex and need not be detailed here. The important point is that lacking a widely accepted generic term, we felt compelled to use "fading" to refer to two procedures that had different characteristics and stimulus control requirements. The use seemed defensible because both employed relative intensity as a prompting stimulus difference. One procedure, however, "faded out" a stimulus to accom-

[1] A continuing empirical question is whether the progressive delay does actually have this effect or whether transfer of stimulus control occurs abruptly at some point in the delay series, merely through superimposition. Less widely appreciated, however, is the possibility that other procedures (fading, stimulus shaping, etc.) may also rely mainly on superimposition; gradual stimulus changes may in fact mask equally abrupt transfers of stimulus control (Stoddard, McIlvane, & de Rose, 1987).

plish stimulus control transfer (vanishing the prompt; cf. Skinner, 1968) and the other "faded in" a stimulus so as not to disrupt existing control by another simultaneously displayed stimulus. Our use of fading, therefore, clearly had a generic flavor. And as we have argued, stimulus control shaping is probably a preferable generic term, given its relation by analogy to response shaping.

Stimulus Control Topography

Ray's concept of stimulus control topography was developed in the context of an intensive group effort that sought to extend the limits of behavior analytic thinking about stimulus control. The findings of that program led to the conclusion that stimulus control research concepts and practices then extant did not fully capture the complexity and dynamic character of the subject matter. For example, rate of response was identified as a potentially misleading dependent variable, despite its utility in other behavior analytic enterprises (cf. Ray & Sidman, 1970). Perhaps more important was empirical demonstration of qualitative differences between the controlling stimulus–response relations that were intended by experimenters' contingency arrangements and those that actually developed. The dimension of line orientation, for example, was and continues to be a favored stimulus control research preparation (cf. Honig & Urcuioli, 1981). But tilted lines were shown to be complex stimuli whose controlling properties could not be assumed (Touchette, 1969). Seemingly continuous dimensions of stimulus control were shown to be discontinuous upon close analysis (e.g., Stoddard & Sidman, 1971; cf. Bickel & Etzel, 1985). Discriminative baselines were shown to involve mixtures of different stimulus–response relations (Sidman, 1969). And controlling stimulus–response relations were shown to have the properties of operant responses more generally (Ray, 1969). "Stimulus control topography" was chosen to help communicate these and other aspects of the subject matter. In addition, the term carries with it the ongoing implication that behavior analytic concepts and descriptive language are readily applicable to so-called "cognitive" phenomena (cf. Sidman, 1978).

Reviewing Ray's (1969) study may help to place her proposal in further context. Rhesus monkeys learned to select either the left or right of two keys, depending upon the colors or line tilts that were displayed on them. If both keys displayed either red (R) or a vertical line (V), for example, the left key was defined as correct. If both keys displayed either green (G) or a horizontal line (H), the right key was correct. After the monkeys acquired these "original" discriminations, Ray presented "conflict compounds" that superimposed line tilts on colors and reversed the reinforcement contingencies for either one element type or the other. For example, V was superimposed on G (G + V) and H was superimposed on R (R + H); reinforcers followed selection of the left and right keys, respectively. In this example, therefore, reinforcer relations were reversed for two elements (R and G) and unchanged for two others (H and V). Conflict compounds initially disrupted discrimination, but the monkeys quickly acquired discriminations consistent with programmed contingencies (in the example, $G + V \rightarrow$ left and $R + H \rightarrow$ right).

Ray's primary concern was control by the elements after the monkeys' behavior conformed to the conflict-compound contingencies. When those elements were presented separately, would they exert control consistent with the original-discrimination or the conflict-compound contingencies? Sixteen replications of the procedures demonstrated that separate element control was not consistent with the conflict-compound contingencies. Instead, control was typically consistent with the original contingencies. Apparently conflict-compound training had not reversed or abolished stimulus control by the reversed elements; instead, that stimulus control topography was merely not occurring when conflict-compound contingencies were in effect. Ray's terminology proposal followed logically from these and other similar research findings. Perhaps the clearest subsequent example was Stoddard and Sidman's (1971) study of the virtually instantaneous removal and restoration of stimulus control by circle-ellipse differences in rhesus monkeys.

Behavior analysts are now studying new and potentially greater complexities in the stimulus control of behavior. For example, there is a widespread effort to understand controlling relations that develop when humans acquire conditional discriminations in procedures like matching to sample. This effort focuses on stimulus equivalence and other less well known topics. We will further discuss and extend Ray's stimulus control topography concept with reference to an example of the latter type. Consider an arbitrary matching-to-sample task in which A1 and A2 are the sample stimuli and B1 and B2 are the comparison stimuli. When A1 is the sample, the experimenter delivers reinforcers following B1 selections; when A2 is the sample, however, B2 selections produce reinforcers. Accurate matching to sample is often immediately interpreted in terms of relations involving the samples and the positive comparisons (sample-S+ relations). Even talk of "associations" or "links" between samples and positive comparisons has been deemed acceptable in the highest quality behavior analytic journals (e.g., Fields, Verhave, & Fath, 1984; O'Mara, 1991).

For some time, however, it has been known that matching-to-sample performances need not reflect controlling relations involving samples and positive comparisons. Logically (i.e., Cumming & Berryman, 1965) and empirically (e.g., McIlvane, Withstandley, & Stoddard, 1984), one can accomplish the above matching performance by *rejecting* B2 in the presence of A1 and by rejecting B1 in the presence of A2 (sample-S− relations). Both sample-S+ and sample-S− relations may be demonstrable in the same subjects (e.g., McIlvane, Kledaras, Munson, King, de Rose, & Stoddard, 1987; Stromer & Osborne, 1982), clearly implying that each type of relation may occur on different trials in the same matching-to-sample baseline. Moreover, the measured behavior (i.e., touching positive comparisons) does not differentiate these relations. Just as reinforcement does not depend upon response topography (e.g., a touch with the left or right hand), it also does not depend on whether the touch of the positive comparison is part of a sample-S+ or a sample-S− relation. The concept of stimulus control topography seems sensible and straightforward in this light.

A reasonable question is whether reviving stimulus control topography would provide any real advantage over the current practice of referring to different controlling relations (stimulus–stimulus and stimulus–response). We suggest two related advantages. First, stimulus control topography was designed explicitly for application to matters of stimulus control. In contrast, controlling relation is a higher-level term; it also applies to response–reinforcer relations. The term controlling relation has been used by default to fill the void resulting from the lack of an agreed parallel to response topography. Further, the term controlling relation does not really have a consensus definition. Leading researchers of stimulus equivalence, for example, have not yet agreed as to whether controlling relations merely have *properties* (Sidman & Tailby, 1982) or whether there are actually different *types* (Fields & Verhave, 1987). Also part of this same disagreement is whether relations control responses or whether the response is an element of a controlling relation or behavioral unit (cf. Sidman, 1986). Applying the topography concept to stimulus control might help to resolve these disagreements. Whereas a behavior analyst would certainly differentiate response topographies, he or she would certainly not use topography differences to differentiate *types* of functional response–reinforcer relations within a given response class.

The second advantage of reintroducing "stimulus control topography" will be to fulfill its intended function of securely linking behavior analytic research of stimulus control to its roots. Based on the progress so far, we must anticipate ventures into areas of extreme complexity. Efforts to manage less complex subject matter have occasioned brief and relatively trivial departures from behavior analytic descriptive language (cf. McIlvane & Dube, 1990). We believe that conceptualizing different controlling relations as different stimulus control topographies may help to forestall more persistent and serious lapses in descriptive and theoretical rigor.

A final pertinent consideration is the initial discomfort that may result from using topography in relation to stimulus control. Discomfort may arise because topography has been used predominantly in relation to spatial or structural aspects of behavior. One way to avoid it might be to suggest an entirely new term to distinguish one form of stimulus control from another. (One reviewer of this paper suggested stimulus control *topology.*) However, the status of stimulus control topography in the history of behavior analysis and in particular its critical parallel relationship to response topography seem to offer compelling rationale for giving Ray's original proposal another chance.

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