Reply to Beale and Winton

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While the last word about the varieties and conditions of behavioral contrast, positive or negative, cannot yet be said, the notion put forward by Beale and Winton deserves a reply.

In the first place, a decreasing rate of responding may not be evidence of inhibition. A description in terms of inhibition of rates of responding which decrease can as well be stated in terms of decreased facilitation. Similarly, a description in terms of facilitation of rates of responding which increase can as well be rephrased in terms of decreased inhibition, and so forth. The problem of inhibition in operant conditioning is compounded since negative rates of responding do not exist. They may be inferred, but this also is difficult to support, as I have elaborated in detail elsewhere (Reynolds, 1964). Inhibitory effects are inferred not from a measurable decrease in the rate of responding to a value below zero, but rather from a decrease in one rate of responding relative to one or more other rates of responding. In these cases, it often makes no more sense to speak of inhibition of the decreased responding than to speak either of a decrease in facilitation of that responding or of an increase in the facilitation of the other responding. In some cases (e.g., Reynolds, in press), an experimental analysis can suggest one or another of the alternatives as likely; in others, it probably cannot.

In the case of multiple schedules, what seems to exist is a fair correlation between the relative rate of responding in one component and the relative frequency of reinforcement in that component (Reynolds, 1961b). This relationship, it should be noted, breaks down at both ends of the continuum of relative frequency of reinforcement, as I have elaborated elsewhere (Reynolds, 1961b); and Terrace's data, to which Beale and Winton refer, on the functions of an "errorless" S- destroy the generality of this as a "cause" of contrast. Be those as they may, when we change one of two reinforcement components of a multiple schedule to extinction and notice that the rate of responding in the other component increases (positive behavioral contrast), we have one instance of this correlation. To speak of this as a relief from inhibition previously generated by reinforcement in the other component, while perhaps sensible in the inhibitory view of the world, contributes nothing to our knowledge beyond what we

already know from the existence of the correlation itself. Further, when we reinstate reinforcement in both components and notice that the increased rate of responding now decreases (negative behavioral contrast), we likewise gain no additional knowledge by speaking of a reinstatement of inhibition by reinforcement.

Now it is difficult to tinker with the interpretation of the unpublished data referred to by Beale and Winton in their comment. But I suspect that they may have found V-shaped gradients of generalization around a stimulus which was just previously the occasion for a negative contrast. This, as they seem to feel, may be interpreted as an instance of relative inhibition, but it may instead involve the facilitation of responding (e.g., Reynolds, 1961c). The reinstatement of reinforcement in Component A of a multiple schedule results not only in a negative contrast in Component B but also in an unusually high rate of responding in Component A—what I have called *successive contrast* (Reynolds, 1961a). The rate in Component A *temporarily rises above* the decreasing rate of Component B (see Reynolds, 1968, p. 44), and after a few sessions, decreases to equality with the rate in Component B. The rate in Component B seems low because other rates are high. The facts are more complicated, though extraordinarily orderly, than the simplistic notion of inhibition implies. Whichever tack is taken, however, all of us who have worried about contrast, generalization, and the process of discrimination will look forward to seeing those data.

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