*TERMINAL TERMINOLOGY: A SELF-REGULATED
RESPONSE TO GOLDDIAMOND*

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For me, it has been a reassuringly welcome sight to observe the increasing attention paid to self-regulatory patterns by "operant" researchers over the past few years. I can still vividly recall an evening in the Spring of 1971 when, on a visit to Lawrence, Kansas, Montrose Wolf asked me if "there was really anything to this stuff called self-control". My response was affirmative, but I must confess having felt some frustration at the reluctance of applied researchers to delve into this new realm. Having been raised a devout operant conditioner, I believed (then and now) that the parameters and processes involved in behavior patterns labelled "self-control" could best be unravelled by functional behavior analysis, an undertaking unlikely to occur so long as individuals with these skills refused to examine the phenomenon critically. Several of my own early studies in self-control were deviously motivated by the hope that their operant and infrahuman nature might seduce a few of my respected colleagues to turn some empirical attention to this area. The increasing frequency of JABA articles dealing with self-regulatory patterns therefore has my strong support.

Unfortunately, as I think is reflected in Goldiamond's article, operant researchers may have some homework to catch up on before they embark on this new area of inquiry. The literature on self-regulation has mushroomed over the last half decade and a new international journal (*Biofeedback & Self-Regulation*) has been inaugurated to help communicate this work. Goldiamond was himself a valuable contributor to early excursions into the analysis of self-control. However, judging from his comments on self-reinforcement, his reading has not kept apace

with the research. For example, an integral aspect of his criticism deals with the distinction between dispensing consequences and determining contingencies. That distinction was dealt with at length by Morgan and Bass (1973) and Thoresen and Mahoney (1974). Likewise, although I am not in entire agreement with either of their analyses, Premack and Anglin (1973) and Rachlin (1974) have already offered stimulating commentaries on the concept and procedures of self-reinforcement. And these are only a few of the many such contributions whose consideration would, I think, have improved Goldiamond's addition.

It would be impossible to elucidate my objections to Goldiamond's analysis in a few short paragraphs. Many of these issues have been touched upon by Carl Thoresen in his own response, and are dealt with at greater length in our book, as well as a later manuscript (Mahoney, 1974). I shall therefore pick only a few items for comment.

First, I would be the last person to deny that the area of self-control is plagued by linguistic and terminological problems. We apply the label "self-control" to those behavior patterns where immediate or externally visible influences are inconspicuous. Thus, there is no qualitative difference between self-regulated behaviors and those that are ostensibly determined by external contingencies. As Skinner, Goldiamond, and others have cogently argued, the "inner man" often gets credit when the controlling parameters are not obvious. In agreement with them, I do not believe in an inner entity or agent called "self". The denial of self is not, of course, a recent phenomenon:

Man has no "self" claimed David Hume,
 And, truly, it's a blessing;
 For what he might have done with one
 It's only better guessing.

While I concede the problems incurred by the terminology applied to self-regulation, I would also defend them on several grounds. They can (and have been) used in operational ways to describe behavioral patterns that are culturally deemed "selfy". The use of the term "self" to describe these patterns need not connote acceptance of free will or "self-determination", and it need not be problematic so long as clear operational definitions are provided.

It is interesting that many operant researchers seem to feel that workers in self-regulation are claiming a violation of long-revered principles of behavior, a misbehavior of organisms in which the individual can divorce himself from external influence and/or override causal chains. I know of no such contention in the literature on behavioral self-control. Molecular instances of self-regulation are part of an infinite series of presumably deterministic events. If they violate molecular analyses, then it is time that we look at molar variables—past, present, and future influences (many of which are mediated by the central nervous system). For a variety of unfortunate and illogical reasons, the central nervous system has remained a mysterious and ignored influence by most operant researchers. Hopefully, the "Cognitive Inquisition" is now moribund (Mahoney, 1974).

From a technical standpoint, Goldiamond's insistence that there is no operant contingency unless it is independently defined is itself problematic. Independence of events denotes total lack of correlation so that, supposedly, the hapless pigeon has no potential influence on the "if-then" relationship between his operant responding and the machine-mediated consequences. However, it is ludicrous to make such a contention. The pigeon *does* influence his own contingencies (less powerfully, perhaps, but the influence is still there). In the procedures of

shaping or stretching an intermittent schedule, the pigeon influences the experimenter. A bad pecker gets different contingencies than a good pecker. One need not talk of "intention" on the part of either party here; the point is that both often show changes in their behavior following changes in the other party's behavior. As Bandura (1971) pointed out, from a deterministic analysis, it is irrelevant how we label our horizontal and vertical axes. Whether we plot the pigeon's behavior as a function of the experimenter's (machine-executed) behavior or *vice-versa*, we still get the same curve form and it is arbitrary to call one the "cause" and the other the "effect".

This raises what I think is the central issue in the criticisms of Goldiamond and others. Behaviorists have long been embroiled in an argument regarding the "prime mover" of behavior. While mentalists, humanists, and other such "soft-heads" have claimed that action springs from within, the behaviorist has painstakingly traced all causal chains to an external impetus. There is a logical irony in the fact that both are equally arbitrary. Reminiscent of David Hume's insightful destruction of Thomas Aquinas' defense of a "first cause" (god), one can show that it is indefensibly arbitrary to stop at any given point in a regress of events and award that event (or category of events) primacy. Human behavior is a long chain of complex interplay between both internal and external events.

We waste our time in moot debate—
 Where lie behavior's sources?
 Within? Without? We seldom doubt
 The strength of our pet forces.
 But causes can have interchange
 By logic's bare directives;
 And thoughts of reciprocity
 Might widen our perspectives.

Paraphrasing Goldiamond, what I am saying here is that total environmental control is as much an explanatory fiction as is self-reinforcement. I am certainly not inviting a return to nebulous mentalism or analyses that stop at one

of Skinner's "mental way stations" and hold them supreme. However, as methodological (rather than radical) behaviorists, we should bear in mind (*sic*) that our research preoccupation with observable events is a *methodological* preference, not an *ontological* commitment. While we must presume some form of molar and at least probabilistic determinism, it need not (and logically ought not) be unidirectional. Behavior may be said to be a function of environment [$B = f(x)$], but environments may also be said to be a function of behaviors [$X = f(b)$]. The same data apply to either argument. It seems more reasonable (and, yes, parsimonious) to talk of a *reciprocal determinism* that recognizes the continual interaction of events in the phenomenon called behavior (Mahoney, Kazdin, and Lesswing, 1974). Wherever those events take place, in one of the nervous systems or in the physically external world, it is time that we devote our attention to their complex interdependence without presuming *a priori* any deterministic primacies.

My comments are certainly not intended to discourage continued and (hopefully) expanding applications of functional behavioral analysis to those patterns labelled self-control. I concur with Goldiamond in the need for ter-

minologically rigorous examination of self-regulatory phenomena, and as I mentioned earlier, I believe that the skills of the operant researcher are uniquely valuable and direly needed in advancing our understanding of this important area.

REFERENCES

- Bandura, A. Vicarious and self-reinforcement processes. In R. Glaser (Ed.), *The nature of reinforcement*. New York: Academic Press, 1971. Pp. 228-278.
- Mahoney, M. J. *Cognition and behavior modification*. Cambridge, Mass.: Ballinger, 1974.
- Mahoney, M. J., Kazdin, A. E., and Lesswing, N. J. Behavior modification: Delusion or deliverance? In C. M. Franks and G. T. Wilson (Eds.), *Annual review of behavior therapy theory and practice*, Vol. 2. New York: Brunner/Mazel, 1974. Pp. 11-40.
- Morgan, W. G. and Bass, B. A. Self-control through self-mediated rewards. In R. D. Rubin, J. P. Brady, and J. D. Henderson (Eds.), *Advances in behavior therapy*. Vol. 4. New York: Academic Press, 1973. Pp. 117-126.
- Premack, D. and Anglin, B. On the possibilities of self-control in man and animals. *Journal of Abnormal Psychology*, 1973, 81, 137-151.
- Rachlin, H. Self-control. *Behaviorism*, 1974, 2, 94-107.
- Thoresen, C. E. and Mahoney, M. J. *Behavioral self-control*. New York: Holt, Rinehart, & Winston, 1974.