Author's Response

Jack Michael
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Henry D. Schlinger

If someone asked for a brief description of the main points of my paper and their possible relevance to the overall field of behavior analysis, I could do no better than to refer to Schlinger's commentary. If I had it to do over again, I would adopt much of his language and his organization of the topics. I agree with all aspects of his interpretation and thank him for his clear and very readable summary. His work with Blakely (Blakely & Schlinger, 1987; Schlinger & Blakely, 1987, 1992) is also an attempt to classify behavioral functional relations, and my own efforts in this direction have benefited considerably from the important distinctions they make with respect to rule-governed behavior.

Mark L. Sundberg

It is gratifying to read about the various ways a more explicit recognition of motivational relations can be put to practical use. Sundberg's work in developing more effective verbal repertoires with autistic, developmentally disabled, and brain-injured children and adults is widely known in the area of applied behavior analysis. As founder and editor of the journal *The Analysis of Verbal Behavior*, he has played a major role in encouraging experimental and conceptual analysis of verbal relations, especially with respect to Skinner's (1957) classification of elementary verbal operants. His manual (Sundberg, 1987) makes extensive use of various establishing operations (EO) as important variables to be used in training, and to some extent offsets the neglect of mand relations in traditional language training.

Bruce Hesse

In addition to identifying the main points of my paper in very understandable language, Hesse makes an important general point and, in addition, raises three issues that require more of an analysis than they received. The general point is that this paper, by its title, is concerned with establishing operations, but its actual purpose is to consider EOs in the broader context of multiple controlling variables. Some readers may be somewhat confused by the attention given to behavioral relations other than the establishing operation, for example the discriminative stimulus (S^D), respondent relations, and unconditioned and conditioned reinforcement. A briefer and possibly clearer treatment of EOs could have been accomplished at the cost of neglecting the broader issue of multiple control.

One point raised by Hesse questions the ultimate practical and/or theoretical value of my concern for complexity. He asks "whether all the theoretical possibilities produced by the logical extension of a concept have significance in reality. For example, is anything gained by considering every possible stimulus function . . . ?" (p. 216). He concludes that for a unified theory of behavior the answer may be yes. I think so too, but the ultimate value of this type of intellectual exercise will depend upon whether the resulting verbal behavior permits those who are influenced by it to behave more effectively with respect to their subject matter, and only time will tell.

A small point, but one that I am not satisfied with, is my statement that some punishing stimuli function as their own EOs. Hesse refers to this as "an apparent redundancy in terms," and I agree. I'm not sure how to deal with this issue, and have simply postponed a more careful treatment.

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Finally, he comments on the general difficulty that has surrounded efforts to demonstrate empirically the transitive conditioned establishing operation (CEO) with nonhuman subjects. The notion that there are stimulus changes that function as conditioned reinforcement in one stimulus context but not in another seems uncontroversial. Further, it seems reasonable that a change from a stimulus context in which such a stimulus has not been effective as conditioned reinforcement to one in which it has been effective will fit the definition of an establishing operation, if it (a) alters the reinforcing effectiveness of a stimulus and (b) results in an increase in the frequency of behavior that has been reinforced by that stimulus. It has been fairly easy to demonstrate the second effect, but a methodology for demonstrating the first effect independently of the second has yet to be developed. Even demonstrations of the second effect have not yet excluded the possibility that the supposed CEO is simply functioning as an S^D for a chain of responses rather than as a CEO for the first element in the chain. I think this problem will soon be solved with nonhuman subjects by the use of more effective experimental designs. With respect to the human situation, as will be discussed in connection with the comments by McDevitt and Fantino below, there seems to be no question as to the existence of controlling relations that fit the definition of the transitive CEO. The critical question is whether it is better to interpret the controlling variable as motivational, or to enlarge upon or complicate the usual discriminative interpretation.

A. Charles Catania

Throughout his comments, Catania expresses a strong concern for "how those outside our field look upon what we do" (p. 219), and he believes that my treatment of establishing operations will contribute negatively to the outsider's view of us. My introduction of a number of terminological innovations and abbreviations, my use of neologisms (*motivative*), and my failure to explain some terms

(*activation syndrome*) will decrease "accessibility of behavior-analytic materials to those outside of behavior analysis" (pp. 220–221). In addition, my neglect of the findings of those outside of behavior analysis (e.g., Premack, 1971; Rescorla & Wagner, 1972) will contribute to the view that we are insular in our perspective. I confess that the way outsiders view us has not been one of my major concerns. This paper is an effort to introduce a behavior-analytic conceptual approach to topics that have been neglected, and my main priority is to communicate as effectively as possible with insiders. However, I am not at all sure that any outsiders will attempt to contact our field by reading this type of article, or that they would be offended by the issues raised by Catania if they did read it.

The remarks above are not meant to preclude the possibility that, even for insiders, some of my terminological innovations are cumbersome, inconsistent, or confusing. Catania's comments relevant to this effect will now be addressed. I will in what follows have to comment on a number of matters in detail, and I hope this does not seem to be nitpicking.

With respect to respondent conditioning and Rescorla, I have approached this work with a strong bias against the cognitive language of expectancy and surprise and a moderate bias against molar explanations in general. From the perspective of this bias, an emphasis on respondent contingencies, as opposed to stimulus pairings, amounts essentially to a reminder that in a conditioning experiment the unconditioned stimulus (US) is being paired with various contextual stimuli in the presence and absence of the conditioned stimulus (CS). The research of Rescorla and his colleagues has been valuable in directing our attention to the importance of this often-neglected form of pairing, and in developing a quantitative account of the cumulative effects of these pairings in various conditioning arrangements. This work does not imply, at least to me, that these two kinds of stimulus contiguities are not the relevant independent variable, irrespective of the complexity of their ultimate

effects on the dependent variable. My paper did not seem to be the place for a presentation of the recent literature on respondent conditioning, especially because it would have to be accompanied by a rather complex analysis aimed at counteracting the cognitive and molar implications of such work. However, I can see that I should have included a footnote to the effect that the pairing of CS and US is not the only important pairing that occurs in respondent conditioning. I thank Catania for his sensitivity to this topic, and I will certainly include such a footnote in future treatments.

I don't understand the concern with my use of *conditioned* and *conditional* in an operant context. The first term occurs often as a modifier for *reinforcer*, *reinforcement*, *punisher*, or *punishment*, which certainly qualify as operant contexts, but I think this use is fairly widespread. *Conditional* occurs exclusively as a modifier for *conditioned reinforcer* or *conditioned reinforcement*, or in describing this type of learned establishing operation. Here the usage is identical to that in *conditional discrimination*, also an operant concept, but I am not familiar with any well-accepted substitute. Before leaving respondent conditioning, I must thank Catania for directing my attention to the unsuccessful attempt by Young to replicate the earlier reports of pupillary conditioning (1965), so in the final version of the article, the example was changed to peripheral vasoconstriction.

With respect to *motivative*, I have to credit Ernie Vargas for suggesting the term as a nice companion for *discriminative*. Even though it does not appear in the dictionary, it is certainly not misleading, although it may be irritating to outsiders and curmudgeons. Catania's criticism of my reference to pain as a stimulus is quite reasonable, but if one wishes to speak in general terms, mentioning instead the various kinds of physical events (heat, cold, pressure, electric stimulation, etc.) that cause the pain seems cumbersome. And I apologize for slipping up once again and reinforcing or punishing an organism rather than a response.

As to *reinforcement* and *reinforcer*, I admit to a somewhat nonstandard use, which consists of a general reluctance to use the term *reinforcer*. *Reinforcement* refers to a type of environmental change (an operation) that increases the future frequency of the type of behavior that precedes or preceded it. (The term may also refer to the change in behavior that is due to a reinforcement operation, but I seldom use it this way.) *Reinforcer*, then, refers to a stimulus, irrespective of any particular operation involving that stimulus. For example, food as a stimulus is a reinforcer, but the delivery of food is reinforcement. In an earlier paper (Michael, 1975), I questioned the wisdom of assigning behavioral functions to static or nonchanging stimulus conditions, and I have been avoiding the use of *reinforcer* as much as possible ever since. The argument is somewhat complex, and I cannot, of course, expect the readers of the present paper to have read the earlier one. The issue will be considered in some detail under the heading of *stimulus change* in an upcoming publication (Michael, 1993). I do not think this idiosyncrasy leads to any misunderstanding in the present paper.

With respect to my abbreviations, they might constitute a problem for the reader encountering them for the first time, but there is a logic to them that should make them fairly easy to master. In general, the use of *unconditioned* and *conditioned* is equivalent to *unlearned* and *learned* and is not meant to imply respondent processes. My invention of separate terms and abbreviations for the evocative (UE and CE) and function-altering (UC and CC) respondent relations arises from the necessity of separately identifying these two quite different effects of stimuli. Irrespective of their wide currency and accessibility to those outside our field, US and CS cannot accomplish this purpose. Although two terms are now necessary where one previously sufficed, an ambiguity has been eliminated, and in the long run I believe that our verbal behavior will be more effective.

I first saw the S^r abbreviation for conditioned reinforcement in Keller and

Schoenfeld (1950), and I just naturally assumed S^R for unconditioned reinforcement, although Keller and Schoenfeld did not use S^R in their text. I have extended this convention to S^P and S^P for conditioned and unconditioned punishment. I suspect that the S^I and S^R contrast has been and is currently used by many others who teach behavior analysis, and I am surprised by Catania's characterization of this as "too major an innovation to introduce in passing" (p. 221).

My use of the terms *improvement* and *worsening* came about because in teaching I have frequently had to point out that a stimulus change from one condition (S_1) to another (S_2) could function as conditioned reinforcement for a number of reasons in addition to the availability of unconditioned reinforcement in S_2 and not in S_1 . Even with unconditioned reinforcement available in both conditions, it might be of better quality or larger quantity in S_2 ; it might be available for less effort, with less delay, or with less accompanying punishment in S_2 , and so on. In other words, so long as the change from S_1 to S_2 is an *improvement* of some sort with respect to reinforcement, punishment, and so on, the stimulus change will work as conditioned reinforcement. In lecturing, I then go on to talk about conditioned punishment being related to *deprovement*, but I have not wanted to use this term in writing; thus, *worsening*. I will take Catania's suggestion as a friendly amendment, and see how I like replacing *improvement* with *bettering* in future use. (Considering Catania's reaction to *motivative*, I will certainly not, in this treatment, suggest *deprovement* as the appropriate parallel to *improvement*, although I have used this term in my classes with success.)

I cannot agree with Catania that *unconditioned* and *conditioned* are appropriate modifiers only for the reinforcing properties of the relevant stimuli. These modifiers are, of course, already used in this sense, as with *unconditioned reinforcers* and *conditioned reinforcers*. But I also need a way of distinguishing between two kinds of establishing operations—those that alter the reinforcing properties

of certain stimuli because of the evolutionary history of the species, or, as one might say, innately, and those that alter the reinforcing properties of certain stimuli because of the specific organism's own history. The terms *unconditioned* and *conditioned* (or *unlearned* and *learned*) can serve this function quite well. If the term *conditioned establishing operation* doesn't seem quite right for imprinting, this is because *conditioned* is being interpreted too narrowly. When I read the term *conditioned reinforcer*, I don't think of this as a reference to respondent conditioning, but only as a synonym for *learned*. Some environment-behavior interactions that occur during the lifetime of the individual organism may establish the reinforcing effectiveness of other stimuli or events and yet not be easily referred to as conditioned or learned (e.g., prolonged drug use leading to addiction). There are probably other interactions that also defy a satisfactory assignment in terms of the categories being suggested in this paper, but the present system may still be an improvement over no system at all.

Catania's reference to a difficulty in reading passages involving abbreviations such as UEO seems to ignore the fairly common strategy of reading the letters rather than the words for which they stand, as when the abbreviation S^P is read as "ess dee." UEO is most easily read, and referred to in speech, as "you-ee-oh."

Catania notes my failure to include a discussion of response deprivation as an operation that alters the reinforcing effectiveness of other events or stimuli, especially as dealt with by Premack (1971). Because McDevitt and Fantino raise a similar question, I will postpone a consideration of this matter until I discuss their comments.

A final point by Catania is to criticize my treatment of UEOs as being far too superficial. As he says, "a lot is known about the variables that are effective as establishing operations in areas that are traditionally spoken of as hunger, thirst, sexual and maternal behavior, thermo-regulation. . ." (p. 223) and so on. I agree. The detailed workings of the various

UEOs are often well covered in the traditional literature of psychology and physiology. I certainly did not intend my treatment to be a substitute for that literature. However, I think such treatments often fail when they deal with general behavioral issues, such as the distinction between evocative and reinforcer-establishing effects, and they are often cluttered with inferred mental processes. My purpose with respect to the literature outside of behavior analysis is only to provide someone within our field with a set of behaviorally consistent concepts for dealing with these topics.

At the end of his commentary, Catania suggests that I should have written a quite different kind of article, and that I would be well qualified to do so. I appreciate his confidence, but I believe that he, himself, is far more qualified than I am to provide the field with a survey and behavioral interpretation of the literature outside of behavior analysis—more qualified in terms of knowledge and scholarship, but also in terms of belief in the value of such a project.

*Margaret A. McDevitt and
Edmund Fantino*

McDevitt and Fantino provide a valuable service by identifying areas of research and theory that should be included in any behavioral approach to motivation, but that I completely failed to consider. The first, response deprivation theory, was also mentioned by Catania, and it will certainly be included in future treatments, including those that occur in my various courses and workshops. Let me partially overcome this deficit with a brief consideration at this point. Briefly stated, response deprivation research (Timberlake & Allison, 1974) begins by observing the time an organism (e.g., a rat) spends in various activities (eating, drinking, running in a running wheel, etc.) when there are no artificial constraints on any of these activities. It then turns out that any constraint that reduces the occurrence of an activity below its naturally occurring time expenditure results in the opportunity to en-

gage in that activity becoming more effective as a form of reinforcement, and behavior that has been so reinforced becoming more frequent. This sometimes occurs even if the naturally occurring time is only a relatively small proportion of the total time available. Response deprivation, then, would seem to be an establishing operation. With responses such as eating and drinking, this is not much different than identifying food and water deprivation as UEOs. Preventing an animal from drinking, for example, typically results in water deprivation in the ordinary sense. If a particular type of response is a means to an end, however, and if the end can be obtained without the response, one would not expect response deprivation to have much effect; of course, this is well recognized by those who work in this area. How to incorporate response deprivation and behavior regulation theory into an orientation such as my own, one that attempts to formulate all functional relations in terms of the effects of momentary environmental changes on highly specific features of behavior, is at present unclear to me, but the empirical findings must certainly be accounted for if they have not been already. I must admit that I have trouble reacting effectively to a language shot through with metaphors about organisms attempting to maintain their natural flow of activities, defending against challenges to their behavioral bliss points, striving to distribute their responses among alternatives so as to make the best of their opportunities and circumstances, and so on.

The behavioral economics literature (e.g., Hursh, 1984) is another very molar orientation, and I have for that reason paid less attention to it than I should. As a result of McDevitt and Fantino's reminder of its clearly motivational implications, I will spend more time on it in the future. As with behavior regulation theories and results, I find myself trying to understand the results in my own terms, and there is no question that research in this area has produced some new results to understand. A good example is the fact that when all food is

obtained during the experimental sessions, as one changes from a richer to a leaner schedule of food reinforcement the food-reinforced behavior occupies more of the organism's available time, not less (as when extra food is provided outside the sessions). Still, this doesn't seem surprising from a more molecular perspective, because spending the same time responding on the leaner schedule as was spent on the richer one leads inevitably to increased food deprivation, and one would suppose an increase in the frequency of food-reinforced behavior relative to other behavior, as well as an increase in the effectiveness of food as reinforcement relative to other forms of reinforcement. It may well be, as is claimed by researchers in this area, that the use of concepts and quantification methods from microeconomics will be a valuable source of new ideas in behavior analysis. My admittedly biased view is that the benefits will probably work in the opposite direction, with behavior analysis providing an experimental context in which some empirical evidence for economic theory can be obtained. Even this possibility seems unclear, however, because the human events involving demand curves, elasticity, and so on would seem to be largely the result of relatively complex verbal repertoires and may be only analogues to basic principles of individual behavior, but again this is clearly another type of bias.

As McDevitt and Fantino point out, the areas discussed above are clearly concerned with the relativity of stimuli as reinforcers or punishers depending upon context, and thus are squarely in the middle of the topic of motivation. Delay-reduction theory, however, seems to be more concerned with the relations between neutral stimulus changes and unconditioned reinforcement. I would not interpret the example of the red light associated with a fixed-interval 5-min schedule as a case of reinforcing potency being established by context. More specifically, I would not identify the red light as the relevant stimulus, but rather the change from the pre-red condition to the

red condition. For me this is the actual event that functions as conditioned reinforcement for the response that produces it. I would then go on to claim that the reinforcing potency of a stimulus change is as much determined by the prechange condition as the postchange condition. It is true that the red light bears a fixed relation to food across conditions, but what happens when a response produces the red light is that it causes a change from one stimulus condition to another. Delay-reduction theory evaluates the value of a stimulus change accurately by taking into consideration the value (in terms of time to reinforcement) of the prechange stimulus condition as well as that of the postchange stimulus condition (the red light) and quantifying the value of the change appropriately.

The final sentence of the analysis of my slotted screw example seems to imply that I would disagree with its conclusion, but it is exactly the analysis that I make. Consider an ordinary student demonstration laboratory experiment with a rat being food deprived, and then having its lever pressing reinforced with food if an overhead light is on, but not if it is off. The overhead light is an S^D for the lever press because in its presence the lever press is reinforced, but in its absence the lever press can occur but is not reinforced. However, the relevant EO is food deprivation, without which lever pressing will not occur even if the light is on. In other words, the ultimate occurrence of the lever press depends jointly upon an EO and S^D . In my slotted screw example, the slotted screws are the EO for asking, in that they alter the value of screwdrivers and evoke the behavior that has obtained screwdrivers, but because asking has not been successful in the absence of the assistant (just as lever pressing under food deprivation has not been successful when the light is off), the sight of the assistant is an S^D for asking. Just as with the rat, the relevant behavior is jointly a function of an EO (UEO for the rat, transitive CEO for the electrician) and an S^D . I should have included mention of the assistant's presence as an S^D . Skinner (1957) dis-

cusses this S^D control in the chapter on the audience relation (pp. 172–184), where an EO determines the *form* of the verbal response (the mand relation), but the audience determines whether or not such a mand will occur in a given situation.

I have trouble with the analysis a little later on in which the expression “the discriminative function of the establishing operation” (p. 226) appears. I consider a UEO or CEO to have several effects at the same time, but I would not refer to any of them as discriminative. Consider food deprivation as the relevant EO. At the simplest level, it increases the effectiveness of food as a form of reinforcement and increases the frequency of behavior that has been reinforced with food. But it also increases the evocative effectiveness of S^D s related to food reinforcement, and these S^D s evoke the behavior relevant to their history, which may or may not be the same behavior evoked directly by the EO. It is these S^D s that have a discriminative function. Food deprivation also increases the reinforcing effectiveness of various stimulus changes as forms of conditioned reinforcement and causes an increase in the frequency of behavior that has been reinforced by those stimulus changes.

The discussion of escape and avoidance puzzled me because it did not seem to be a refutation of my earlier argument against the shock being considered an S^D for the escape response and against the warning stimulus being an S^D for the avoidance response, but it did not seem to acknowledge that argument. I'm not sure that the following analysis deals with their interpretation, but this is the way I would describe the effect of shock onset in a pure escape situation (one with no warning stimulus). Shock onset immediately establishes shock offset as an effective form of reinforcement and evokes the behavior (lever pressing) that has been followed by such reinforcement. But lever pressing cannot occur successfully unless the rat has his paws on the lever and is in an appropriate posture to press. Let us assume that the rat is away from the

lever. In addition to its evocative effect on lever pressing, which is not for the moment possible, shock onset also makes the sight of the lever an effective form of conditioned reinforcement and evokes the behavior that has been followed by that reinforcement (possibly some sort of visual search). Looking for the lever is presumably reinforced most directly by seeing the lever, but I would be unwilling to call the shock an S^D for looking for the lever, because such behavior can be followed by seeing the lever when the shock is off as well as when it is on. The shock-pain stimulus is not a stimulus in the presence of which levers are more easily seen, but in which their sight is more valuable. So the first response in the chain is the visual search behavior, evoked by the shock as a UEO (by making some other stimulus an effective form of conditioned reinforcement). The sight of the lever seems to be a true S^D for approach behavior, which cannot easily occur in the absence of appropriate visual stimulation. As the lever gets visually closer, that visual characteristic is an S^D for successful reaching and touching, and so on until the lever is pressed.

Finally it is proposed that an S^D interpretation can be retained as an alternative to my transitive CEO interpretation if we consider the relevant conditioned reinforcement to be a compound stimulus rather than a simple stimulus change (as in my interpretation). In the human example, this would amount to saying that the reinforcement for the request for a screwdriver is not the simple stimulus event consisting of receiving the screwdriver, but is rather the compound stimulus event consisting of receiving a screwdriver while looking at or having just looked at slotted screws that must be unscrewed. I agree that this compound S^D fits the situation as well as my transitive CEO. Here we are dealing with two somewhat different verbal practices regarding the same event. For me, the one involving the compound stimulus doesn't seem to isolate the relevant controlling variables as well as the CEO interpretation does.

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