Rethinking Motivating Operations:

A Reply to Commentaries on Edwards, Lotfizadeh, and Poling (2019)

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As we acknowledged in our initial article (Edwards, Lotfizadeh, & Poling, 2019), the MO concept as popularized by Michael and his supporters, which we hereafter call the "current" concept, has served behavior analysts rather well. For that reason, the revisions of the concept that we proposed in our initial article, no matter how slight, should be challenged. We are grateful to the commentators, most of whom trained with Michael, for exploring the limits of our reconceptualization and for raising issues that we failed to consider. Herein we address what we view as their primary concerns. It is not our goal to argue that they are wrong and we are right, but rather to further discourse in the hope that it helps behavior analysts better understand some of the factors that modulate certain aspects of behavior.

All scientists attempt to account for variability in their subject matter, and their concepts help them to do so. Behavior analysts focus on interactions between and among changes in the environment and changes in the actions of living creatures, and the MO concept is of value to the extent that it helps them discover and organize some of these interactions. The MO concept, both in its current and re-conceptualized form, emphasizes that certain kinds of changes in the environment, MOs, alter the likelihood that actions which historically produced a particular kind of outcome (e.g., food, water, or sexual stimulation) will occur in the present situation and, if they do occur, their relative strength. Our revised definition, contrary to the current one, specifically indicates that these effects are mediated through changes in the control of behavior by discriminative stimuli (S^Ds). We defined MOs as "*operations that modulate the reinforcing or punishing effectiveness of particular kinds of events and the control of behavior by discriminative stimuli historically relevant to those events*" (p. 1). We intended that definition to be brief, clear, and useful to minimally trained behavior analysts in understanding MOs in a way that may help them predict much of the variance in relevant behaviors.

Although all of the scholars who commented on our article clearly understand the current MO analysis, each of them is a senior academic with extensive relevant training. Our experience in analyzing articles published in the *Journal of Organizational Behavior Management* (Lotfizadeh, Edwards, & Poling, 2014) and in training and working with Board Certified Behavior Analysts® (BCBAs®) suggests that more than a few behavior analysts have difficulty with the concept. Our goal in writing about MOs was to make the concept readily accessible by highlighting MO effects of clear practical significance, avoiding potentially confusing language, and foregoing complex analyses appropriate to very few everyday situations.

Although we did not point this out in our first article, the current definition of MOs is potentially misleading in that it explicitly distinguishes between a "behavior-altering effect" and a "value-altering effect." According to Laraway, Snycerski, Michael, & Poling (2003), MOs "influence the capacity of operant consequences (reinforcers and punishers) to alter the strength of future behavior (the value-altering effect) and (b) change the current strength of behaviors related to the consequences affected by the MO (the behavior-altering effect)" (p. 603). Both effects are evident solely in changes in behavior and there is no good reason to designate only one of them as "behavior-altering."

But the use of those terms is not the primary weakness of the current definition. Rather, as we pointed out, that definition provides only the vaguest suggestion of how behavior will change when a relevant MO is active. We believe that our definition, which focuses attention on the role that S^Ds play in modulating the effects of MOs, at least partially alleviates this difficulty. According to some commentators, however, our emphasis on S^Ds was unnecessary because the current account deals with them adequately.

Stimulus Control in the Existing Motivating Operations Concept

Three commentators suggested that the current MO conceptualization adequately accounts for the mediational role of S^Ds in determining the response form that is ultimately emitted when a relevant MO is in effect (Miguel, 2019; Carbone, 2019; & Pilgrim, 2019). As we acknowledged in our original proposal, the role of stimulus control has not been overlooked by theorists who have written on this topic (e.g., Michael, 1993; McDevitt & Fantino, 1993; Whelan & Barnes-Holmes, 2010). Instead of arguing that this point has been utterly ignored, our aim in reviewing relevant theory and experimental evidence was to establish the importance and ubiquity of stimulus control in determining the specific effects of MOs. This analysis was required to justify the promotion of this aspect of the concept to a higher rank, that is, to the definition itself. We proposed that altering the definition of MOs to include the mediational role of S^Ds achieves two things at once. First, it accounts for changes in behavior prior to contact with the relevant reinforcer (or punisher), as does the "behavior-altering" component of the existing definition. Second, it also specifies that these changes are mediated by S^Ds (or, more generally, antecedent stimuli). Given the central role of stimulus control in determining the specific response form that will be produced when an MO is in effect, we made the case that it is necessary to specify its role within the definition.

Carbone (2019) suggested that "appropriately trained behavior analysts" should be aware of the role of S^Ds in determining the effects of MOs on behavior. We agree with Carbone's sentiment that behavior analysts *should* know this stuff. However, what evidence do we have, other than the verbal behavior of the commentators in this issue (who are highly skilled behavior analysts and extremely familiar with the conceptual work on MOs) that researchers and practitioners are aware of the importance of stimulus control when considering the influences of MOs on behavior? Influential summaries of the MO concept do not highlight stimulus control,

and those summaries are likely to be the only relevant publications that many BCBAs® encounter.

For instance, Michael's seminal and widely-cited 1982 article explicitly focused on distinguishing between S^Ds and what are now termed MOs, and their potential interaction was totally ignored. Langthorne and McGill's (2009) tutorial on the MO concept and its applications contains clear descriptions of the concept and includes many good examples but makes no mention of the mediational role of S^Ds. Michael's (2007) chapter in Cooper, Heron, and Heward's (2007) influential book dedicates one sentence to the mediational role of S^Ds when describing the behavior-altering effect: "The alteration in frequency can be the result of (a) a direct evocative or abative effect of the MO on response frequency and (b) an indirect effect on the evocative or abative strength of relevant discriminative stimuli (S^Ds)" (p. 375). Throughout the rest of the chapter, the MO is described (and defined) as altering "the current frequency of all behavior that has been reinforced by [the relevant reinforcer]" (p. 389). Likewise, in Michael and Miguel's (2019) chapter in the just-published third edition of Cooper, Heron, and Heward's (2019) book, the MO is described as altering "the current frequency of all behavior that has been reinforced by [the relevant] stimulus, object, or event" (p. 374). Encouragingly, in this new chapter, a paragraph rather than a sentence is dedicated to describing the mediational role of S^Ds in determining the form of the response that will be emitted following an MO, but descriptions of direct evocative effects prevail throughout the chapter. The consistent lack of emphasis on the critical role of S^Ds in determining MO effects in key source materials suggests that Carbone's (2019) claim that well-trained practitioners are likely to be fluent with this aspect of the concept is unfounded.

Pilgrim suggested that the role of S^Ds is already implied in an earlier definition of the MO (Michael, 1993) if we consider the "repertoire" of the organism to include both responses and relevant stimulus control (i.e., the three-term contingency). Michael (1993) described MOs as altering "the frequencies of occurrence of that part of the organism's repertoire relevant to those events as consequences" (p. 192). This definition is closer to our proposed definition than those that have appeared more recently, but it fails to explicitly emphasize the importance of antecedent stimuli. Moreover, "repertoire" does not appear in more recent (and more popular) definitions of MOs. For example, Laraway, Snycerski, Olson, Becker, & Poling (2014) describe the behavior-altering effect as changes in "the current strength of behaviors related to the consequences affected by the MO" (p. 603).

In line with Pilgrim's (2019) description of the repertoire, Donahoe and Palmer (1994) state that "responses are not selected or strengthened; what is selected is the ability of particular environments to guide those responses" (p. 71). We suspect that Pilgrim would agree with this characterization of behavioral selection, as do we. Although it makes no reference to repertoires, the revised definition of MOs that we proposed is consistent with Donahoe and Palmer's analysis and emphasizes that MOs act upon an "*n*-term" contingency, where $n > 2$.

Evocative Effects of MOs?

Laraway and Snycerski (2019), Lechago (2019), and Miguel (2019) suggested that the current definition of the MO – specifically the component describing the behavior-altering effect – is preferable to what we have proposed because MOs, they argue, *can* evoke behavior in the absence of any apparent mediation by S^Ds. As evidence, Laraway and Syncerski and Miguel describe a rat pressing a lever under constant stimulus conditions but varying food deprivation (MO) levels. Response rate changes as a function of level of deprivation, which they present as

evidence that the MO directly alters the frequency of a response in the absence of any mediational influences of S^Ds.

On the face of it, their example is compelling. There is no experimenter-arranged S^D at work, hence the effect of the MO appears to be direct evocation of food-maintained behavior. It is, however, possible to propose a molecular analysis of behavior in this situation, similar to that Michael (1993) used when describing transitive conditioned motivating operations, which provides evidence of an S^D that is likely to mediate the MO effect. For example, if the rat is oriented towards and in close proximity to the lever, raising and lowering a forepaw will move the lever downward, close a microswitch, and produce food. If, however, the rat is not in this location and so oriented, the same movements will not produce food. Given this arrangement, visual, tactile, and perhaps other stimuli associated with being "in the correct position" are established as a complex S^D. Paw movements occur only when the S^D is present, and the level of food deprivation modulates the evocative function of the S^D. Although the experimenter did not explicitly arrange for discrimination training, it has occurred nonetheless, and is relevant to the effects of food deprivation. There is little doubt that stimuli in the operant chamber function as S^Ds and guide behavior under even the simplest experimental arrangements.

In defense of a pure evocative effect of MOs, Laraway & Snycerski (2019) refer to previous work in which this analysis was defended. In the most thorough analysis, Laraway et al. (2014) referred to experimental results obtained by Gutierrez et al. (2007), O'Reilly et al. (2006a, b, 2007a, b, 2008), and Skinner (1938) in which an MO manipulation changed behavior in the absence of an S^D manipulation. Laraway et al. also referred to data obtained by Edrisinha, O'Reilly, Sigafoos, Lancioni, & Choi (2011), which convincingly demonstrated changes in

behavior resulting from MO manipulations in both S^D and S^Δ conditions.[1] In all of these examples, although the influence of MO manipulations on behavior was demonstrated, S^Ds or other stimuli associated with the experimental context could have mediated this effect.

In a hypothetical example of a drug addict engaging in drug-seeking behavior while undergoing withdrawal, Laraway et al. (2014) noted that such behavior would be likely even in the absence of relevant S^Ds, such as a drug dealer. This hypothetical example serves to illuminate the point of disconnect between the analysis of Laraway and Snycerski and the present analysis. We suggest that the drug-seeking behavior of a drug user would fall under the control of an impressively large array of S^Ds (including complex, conditional S^Ds) and that these stimuli would be pervasive throughout the individual's normal environment. Responses collectively labelled as drug seeking and drug taking involve extended behavioral chains. For example, one chain might include locating a mobile phone, messaging a dealer, walking to a meeting point, and purchasing the drug. The stimuli associated with these complex chains serve as S^Ds (or conditional S^Ds).

Just as there are many stimuli associated with an operant chamber that are not specifically noted or controlled by the researcher (i.e., contextual stimuli), there are myriad stimuli in applied settings that guide behavior (i.e., function as S^Ds) even though they have not been identified by an observer. In the experimental examples and in this hypothetical example, contextual stimuli (i.e., unidentified S^Ds) are present and can serve a mediational role in determining the precise effects of MO manipulations. Indeed, one of our main goals with this reconceptualization is to draw attention to the critical role of these stimuli.

[1] Laraway et al. (2014) stated that, "this is an empirical demonstration of a 'pure' behavior-altering effect of an MO with the presence or absence of a discriminative stimulus having no discernable effect on behavior" (p. 609). However, examination of the relevant figures (Edrisinha et al., 2011, Figures 5 & 6) reveals a discernable effect of the discriminative stimulus for both participants.

Lechago (2019) and Miguel (2019) question how the emission of automatically reinforced behavior can be accounted for with our revised definition. Lechago describes a child clapping her hands when a relevant MO is in effect. What stimuli could be related to the differential availability of stimuli produced by hand clapping? With some automatically reinforced behavior, such as visual self-stimulation or skin picking, S^Ds can be identified. For example, an illuminated environment and an absence of skin covering may come to control behavior in these two examples, respectively. In Lechago's example, it may be that we can identify relevant stimulus conditions, such as the absence of gloves or any item in one or both hands, which might prevent the production of the reinforcing stimulus.

We agree, however, that there are some MOs associated with reinforcers that are available in many contexts. A response consistently reinforced in a wide variety of stimulus conditions (contexts) does not come under clear stimulus control and, as Miguel (2019) indicated, under these circumstances the MO, if it can be identified, would be a better predictor of the behavior than the stimulus context. But we must ask if the exceptional cases in which MOs affect behavior in the absence of demonstrable stimulus control justify proposing that MOs *generally* have unmediated evocative effects. Many examples of purely evocative effects involve an operant response class with a single member and, in such cases, predicting the specific aspect of behavior (i.e., operant response) that the MO will affect is not difficult, because there is a single relevant response topography. The situation is very different when the operant response class has many members. Here, the current MO concept emphasizes that *all* members of the response class are affected. For example, Michael and Miguel (2019) wrote, "food deprivation (MO) increases the value of food, as well as *all* [emphasis ours] behaviors that have led to food, for as long as the organism is deprived" (p. 373). It is easy to find data that disprove this

contention and demonstrate that antecedent stimuli determine the specific responses altered by an MO.

Many of the commentators agreed with our assessment that the mediation of MO effects by S^Ds is the rule rather than the exception. Moreover, the general value of considering stimulus control in analyzing behavior is well established. Sidman (1986), for instance, indicated that if behavior were not controlled by antecedent stimuli, then: "Response would succeed upon response, the sequence being determined solely by momentary reinforcement probabilities, deprivation states, energy requirements, etc. This, of course, is not what happens" (p. 218). Stimulus control of MO effects is ubiquitous enough to merit inclusion in the definition of the concept. Exceptions, such as those that occur with extreme generalization training, can easily be covered in supporting information. It is better to emphasize stimulus control in the definition, at the risk of not including a few exceptions, than to ignore stimulus control, which is evident in most instances. The current definition does the latter.

Discriminative vs. Antecedent Stimuli

Several commentators suggested that describing MO effects as mediated only by S^Ds may be too restrictive and that the mediational influences of conditional S^Ds and contextual stimuli must also be considered (Petursdottir, 2019; Pilgrim, 2019; Rehfeldt, 2019). As implied in our initial discussion of the conditioned motivating operation (CMO) concept (Edwards et al., 2019), we agree that the mediational role of such stimuli must be considered and had no intention of excluding their mediational role from our conceptualization.[2] Instead, we had considered them to

[2] *Contextual stimulus* appears to be used in two ways. First, it is used in place of *discriminative stimulus* when a complex array of stimuli is serving as a discriminative stimulus (procedurally and functionally) but the specific subset of stimuli within the array that are serving this function are unknown (e.g., Petursdottir, 2019). Second, the term is used in the same way as *conditional stimulus* when the "condition" is a complex stimulus array whose specific controlling elements are unknown (e.g., Griffee & Dougher, 2002).

fall within the larger class of stimuli described as "S^Ds," with the understanding that some S^Ds are conditional. In the spirit of our analysis of definitions with respect to their effects on the listener, it appears that the term *discriminative stimulus* does not serve this function for many proficient behavior analysts. Additionally, in some conceptual and experimental work on conditional discrimination, the conditional stimuli in the presence of which conditional S^Ds are effective (i.e., have evocative functions) have been conceptualized as an additional term in the *n*-term analytical unit (Sidman, 1986). In an analysis of the role of sample stimuli (i.e., conditional stimuli) in a matching-to-sample procedure, McIlvane (2013) suggested that "sample and comparison stimuli may have logically and perhaps empirically separable functions" (p. 136).

In the process of developing our proposed definition, in place of *discriminative stimuli* we had used *antecedent stimuli* up until our two most recent drafts, but we were concerned that the scope of such a description would be too broad. Upon further reflection, and in light of our commentators' concerns, we now feel that this broader scope is warranted and that "antecedent stimuli" is a better term. We also note that, with this modification, we can more readily incorporate mediational control by dissimilar stimuli when stimulus generalization is observed and also account for mediational control by stimuli that would not normally be classified as "discriminative" because of their omnipresence (Miguel, 2019).

Lechago's (2019) observation that "releasers" or "sign stimuli" associated with fixed (or modal) action patterns, such as those described in our description of extreme stimulus generalization and "vacuum activity," do not qualify as S^Ds is an astute and appropriate point. We agree with Carbone (2019) that a better example of extreme stimulus generalization would be Skinner's (1957) example of the emission of a mand in the apparent absence of any relevant S^D. Regarding Lechago's discussion about the relevance of the MO concept to respondent

behavior, we suggest that MOs also modulate the eliciting function of conditioned and unconditioned stimuli, and we are not the first to do so. Michael (1993) stated that, "respondent evocation, like operant evocation, should be conceptualized as jointly controlled by the [MO] and [unconditioned stimulus] (or [conditioned stimulus])" (p. 198). By expanding the scope of the definition to include mediation by "antecedent stimuli," our proposed definition would encompass antecedent stimuli associated with respondent behavior, and we feel that this is an additional advantage of defining the MO in this way.

Pilgrim (2019) suggested that schedules of reinforcement under which reinforcement has historically been produced might also serve a mediational role in determining the behavioral characteristics that are observed when an MO is in effect. We do not agree that events preceding the MO can serve a mediational role, but a related example may serve to illustrate how our proposed definition would apply when predicting specific behavioral characteristics when an MO is in effect. If a response was previously reinforced under a multiple variable-interval 30-s fixed-interval 5-min (mult VI 30-s FI 5-min) schedule, the rate of response when the MO is in effect will depend upon which stimulus is present (i.e., a relatively high rate of response in the presence of those stimuli associated with the VI 30-s schedule and a relatively low response rate in the presence of those stimuli associated with the FI 5-min schedule of reinforcement).

Transitive Conditioned Motivating Operations

We now move on to the issue of the CMO and its utility for behavior analysts. We begin with the "transitive CMO" (CMO-T) because this variant of the CMO was defended most vociferously (Carbone, 2019; Laraway & Snycerski, 2019; Lechago, 2019; Miguel, 2019; Petursdottir, 2019). There is no need to break down another behavioral chain example because it is abundantly clear that the commentators who addressed this concept did not disagree with our

description of the behavioral chain in terms of conditional discrimination, neither did we disagree with their analyses in any of the examples provided. Instead, the point of contention is situated on the issue of whether a (complex) label is useful when describing a change in the functions of a conditional S^D with the onset of the associated conditional stimulus. Only when a conditional stimulus is present will a conditional S^D function as an S^D and, hence, a conditioned reinforcer.

In Petursdottir's (2019) example, "wallet in hand" is an S^D in the presence of which going out onto the street is reinforced with ice cream, but only in the presence of an ice cream truck (the conditional stimulus). We argue that this analysis is complete, *given that currently effective S^Ds have conditioned reinforcing properties*, which is a well-established phenomenon. In the previous example, because "wallet in hand" functions as an S^D, it also functions as a conditioned reinforcer (and, therefore, S^Ds historically associated with finding the wallet will guide "wallet-seeking behavior"). None of this is controversial, even among the commentators who advocate for the CMO-T.

So, what does the CMO-T concept bring to the table? "MO" indicates that such a change satisfies the definition of an MO (and it does appear to do so), "C" indicates that the effectiveness of the operation is a result of some conditioning history, and "T" tells us that this has something to do with conditional discrimination (clarification of which requires analysis in the terms of conditional discrimination, as in the previous paragraphs). In a recent review of research involving the interrupted chain procedure, only in the discussion section did Carnett et al. (2017) mention the CMO-T concept, which they described as "environmental variables that, due to prior learning history, establish the reinforcing effectiveness of a previous neutral stimulus and evoke the behavior that has been reinforced by the other stimulus" (p. 217).

They then went on to say, “at a practical level, practitioners should evaluate the value of the terminal reinforcer to ensure the person wants to access and that this reinforcer can only be obtained by producing the targeted mand” (p. 217). This implication that the CMO-T concept does not have practical value corresponds with our own assessment of the concept and does not align with Carbone’s (2019) assessment that describing this procedure as a “transitive conditioned motivating operation” is “clearly uncomplicated verbal behavior about controlling variables” (p. X). Instead, we suggest that scientists and scientist-practitioners who have a solid grasp of the concept of conditional discrimination will be able to analyze, adapt, and apply these procedures effectively.

With respect to the scope of the CMO-T concept, from our experience as instructors and supervisors, from the examples provided by the commentators, and from the range of procedures described as involving a CMO-T in the relevant literature, the concept appears to have become entirely operationalized, that is, synonymous with the interrupted chain procedure. A conditional discrimination perspective offers greater scope and precision. For example, it is clear that these same principles apply to matching-to-sample arrangements in which, after a period of training, the presentation of the sample stimulus establishes the “correct” comparison stimulus as an effective S^D and conditioned reinforcer.

The MO’s greatest contribution to our analysis of behavior under situations involving conditional discrimination is that it prompts us to identify and gain control of the variables that modulate the reinforcing effectiveness of the terminal stimulus change. With an enhanced understanding of the accompanying changes in stimulus control by S^Ds, conditional S^Ds, and conditional stimuli (including higher order conditional stimuli), the MO concept allows for a

precise analysis of behavior under such circumstances, but we do not see a strong case for the utility of the CMO-T concept.

Reflexive Conditioned Motivating Operations

Several commentators questioned our dismissal of the "reflexive CMO" (CMO-R) concept (Carbone, 2019; Lechago, 2019; Laraway & Snycerski, 2019; Petursdottir, 2019). In line with Michael's (1982) original analysis, these commentators' defense of the CMO-R is based on the reasoning that a conditioned aversive stimulus should not be classified as an S^D because "in the absence of the [conditioned aversive] stimulus, there is no possibility that the [response] could have failed to terminate the [conditioned aversive] stimulus" (Lechago, p. X). We have identified three main issues with the CMO-R concept. First, we suggest that the validity of the main argument for the CMO-R, as stated above, is fragile because simply rewording it reveals that it is not necessarily true. For example, if we consider that the reinforcer is a stimulus change, from conditioned-aversive-stimulus-present to conditioned-aversive-stimulus-absent, an account that is consistent with Michael's (1993, p. 8) description of reinforcement, this stimulus change is only possible in the presence of the conditioned aversive stimulus. When worded in this way, the S^D account is not problematic. We are not suggesting that the conditioned aversive stimulus is best classified as an S^D but are merely pointing out that the main rationale for its classification as an MO is weak. Second, we suggest that the adding a new term to describe what must be logically true (for a conditioned aversive stimulus to be terminated it must be presented) represents an unnecessary proliferation of jargon.

Third, this invocation of the MO concept distracts us from its actual utility when analyzing (predicting and controlling) behavior under relevant circumstances. In response to our suggestion that the CMO-R analysis can be replaced with a logical analysis, Laraway and

Snycerski suggest that this is true of all functional stimuli and that "the mere onset of a stimulus before or after behavior does not tell us anything about the possible behavioral functions (if any) of that stimulus for a given organism without, for instance, knowledge of its behavioral history or a functional analysis" (p. X). We agree with this point, except we would add that the MO concept is specifically of value in predicting the functions of stimuli prior to their onset (see Poling, Lotfizadeh, & Edwards, 2017), and it is precisely for this reason that we suggest that the CMO-R concept adds nothing of value to our analysis. C*onditioned aversive stimulus* is a functional term which is synonymous with *conditioned punisher*. Presentation of a conditioned aversive stimulus functions as a punisher (by definition); termination of a conditioned aversive stimulus functions as a reinforcer (by definition). The CMO-R concept is based on the assumption that the function of the putative conditioned aversive stimulus has been confirmed as such. This is a problem because the MO concept was developed specifically for the purpose of understanding and classifying changes in the reinforcing and punishing effectiveness of stimuli.

In anticipation of discussing this issue further, we note and agree with Petursdottir's observation that our criticism of the CMO-R should also apply to its "unconditioned" counterpart, that is, to the onset of an (unconditioned) aversive stimulus (or "painful stimulus" in Michael's [1993] words). The greatest potential contribution of the MO concept to situations in which a potentially punishing stimulus (unconditioned or conditioned) is presented or terminated is in determining the degree to which the stimulus functions as an aversive stimulus (i.e., punisher). For example, administration of a painkiller functions as an abolishing operation that reduces the punishing effectiveness of shock onset and also reduces the reinforcing effectiveness of shock offset. This appears to be a more appropriate application of the MO in this type of situation.

Returning to situations involving a conditioned aversive stimulus in which the "conditioning" was respondent in nature, how should this situation be conceptualized? In this case, unless we consider respondent conditioning to be an MO (see relevant discussion below), we need not evoke the MO concept, as it only complicates what is an otherwise straightforward analysis. When a task demand (e.g., an instructor pointing to a worksheet and saying "do the next problem") functions as a conditioned aversive stimulus, we know that its onset will function as a punisher and that its offset will function as a reinforcer. Rather than saying that a "reflexive conditioned motivating operation" is in effect, we can say that an aversive task demand is present. We can also ask why the task is aversive and what we might do to alter its function in this respect. The MO concept may have some utility in addressing this question. Because the presentation of aversive stimuli has been described as an MO since well before Michael's original reintroduction of the MO concept (e.g., Keller & Schoenfeld, 1954) and because they are commonly described as such in the current behavior-analytic literature, a more thorough analysis of this point seems to be warranted and is currently under preparation.

Surrogate Conditioned Motivating Operations

With respect to the "surrogate CMO" (CMO-S), Laraway and Snycerski (2019) questioned our dismissal of this concept based on its status as a respondent conditioning procedure. We agree that dismissal on these grounds requires additional explanation. Respondent mechanisms are relevant to many MOs, and it is critical that the role of respondent conditioning is understood when analyzing the conditions under which MOs are effective and predicting the various outcomes of MOs. The CMO-S is described as the presentation of a stimulus (S1) that alters the reinforcing or punishing effectiveness of another stimulus (S2) as a result of a previous respondent conditioning procedure in which S1 was correlated with an MO. Although

respondent conditioning is not currently conceptualized as a motivating operation (e.g., see Michael, 1988), the respondent conditioning relevant to the CMO-S is not considered to be the MO itself but, instead, it is conceptualized as the history of conditioning that is required for the stimulus (S1) to function as an MO.

The main issue with the CMO-S concept is that it has limited empirical support. Although Laraway et al. (2014) described several examples that suggest that the CMO-S is "real," they did not explore the multiple failures to demonstrate CMO-S effects under conditions which should have produced such effects (e.g., Cravens & Renner, 1975; Mineka, 1975). We find it interesting that the CMO-S, the existence of which is in question, is included in the current MO conceptual framework, but a related operation with clear MO effects is not. Presentation of stimuli that have been historically correlated with reinforcers (in a respondent conditioning arrangement) reliably increases the effectiveness of the relevant reinforcer and the evocative function of discriminative stimuli associated with the reinforcer. These pairing procedures and their associated outcomes are commonly referred to as "Pavlovian-instrumental transfer" (see Cartoni, Balleine, & Baldassarre, 2016). Given the other issues with the CMO concept that we have discussed, we suggest that, rather than generating another CMO subtype, it may be most beneficial to conduct further conceptual and empirical work on the topic of MOs and respondent conditioning before considering how to incorporate such effects into our account.

General Utility of the CMO Concept

Putting aside the technical issues that we have just explored, some commentators suggested that the CMO concept (particularly the CMO-T and the CMO-R) has inspired valuable research and, for this reason, the CMO concept should not be discarded. We do not wish to discourage researchers and practitioners from evaluating, refining, and applying the procedures

that are currently associated with the CMO-T and CMO-R concepts. Instead, we are suggesting that these procedures can be more clearly understood when analyzed in other ways, for example, as some of our commentators have analyzed the interrupted chain procedure. In the history of our discipline, and other disciplines, findings that have been generated when exploring concepts subsequently rejected are not discarded. In the case of the CMO-T, we should continue to conduct research on the interrupted chain procedure to develop a better understanding of the conditions under which such a procedure is likely to be most beneficial to clients, including the specific history of conditioning that is required for successful application of the procedure. Furthermore, going beyond this specific procedure, we suggest that the importance of conditional discrimination to verbal processes and other complex behavior in applied settings cannot be overstated and that additional research in this area is required (see McIlvane, 2013). In the case of the CMO-R, we should continue to conduct research on the presentation and termination of conditioned aversive stimuli in applied settings and, relevant to the current analysis, we should also consider operations (MOs or otherwise) that can alter the function of the relevant aversive stimuli.

Rule-governed Behavior

In our original article (Edwards et al., 2019), we acknowledged that rules can alter the reinforcing or punishing effectiveness of other stimuli and, therefore, appear to qualify as MOs, but the CMO concept is not helpful in clarifying the effects of these rules. Lechago (2019) disagreed and provides an example in which, as the result of a rule statement, the functions of a red stapler and several other stimuli are altered. Lechago says that “the rule establishes the sight of red stapler as a reinforcer for search behavior…” (p. X). We agree with this analysis and it aligns with Schlinger’s (1993) analysis of such an event. Nevertheless, we disagree with the

claim that the analysis employs “CMO terminology” and that “the CMO analysis represents a viable and straightforward approach to analyzing the effects of verbal behavior as an MO” (p. X). A similar approach to describing rules is taken in one of the studies cited by Lechago (Belfiore, Kitchen, & Lee, 2016). In this study and in Lechago’s example, the authors suggest that the rule functions as a CMO-T. No attempt is made to reconcile this description with Michael’s (1993) definition of a CMO-T: “When a stimulus condition (S1) is correlated with the correlation between another stimulus (S2) and some form of improvement (or worsening), the presence of the S1 establishes the reinforcing (or punishing) effectiveness of S2 and evokes (or suppresses) the behavior that has been followed by that reinforcement or punishment” (p. 203). Not only do rules of this sort fail to qualify as CMO-Ts, but the CMO (or CMO-T) label brings us no closer to understanding the effects of rules than the description provided by Schlinger, who conceptualizes rules as function-altering, contingency-specifying stimuli (see also Schlinger & Blakely, 1987). Pointing out that one such function-altering effect appears to satisfy the definition of the MO may be helpful, but to enhance our understanding of the effects of rules, we need to delve into relevant research on stimulus equivalence, derived relational responding, associative learning, and other related fields of inquiry (for additional discussion see Dixon, Belisle, Rehfeldt, & Root, 2018; Palmer, 2012).

As an aside, Michael’s (1993) above-cited definition of the CMO-T clearly illustrates one of our general concerns with the current MO concept: it is unnecessarily complex and difficult to understand without dedicated study. “Correlated with a correlation” is an unusual phrase that is far from clear. “Some form of improvement (or worsening)” begs the question: By what standard? Michael (1993) goes on to explain what he means by these terms, and people who exert sufficient effort – especially those who have had the good fortune to actually study with

him, like several of our commentators – certainly can understand the CMO-T. We fear, however, that many behavior analysts will not master the concept, but rather will use "CMO-T," as well as related terms, imprecisely to refer to any of a variety of variables that change behavior in a variety of ways consistent with everyday conceptions of "motivation." As noted previously, our analysis of the way in which authors of *Journal of Organizational Behavior Management* articles use these terms certainly supports this view (Lotfizadeh et al., 2014).

Other Points to Consider

In his review of our initial submission, and in his published commentary, Killeen (2019) rightly suggested that behavior analysts interested in advancing the MO concept need to consider data and concepts from outside behavior analysis and pointed out a directly relevant body of research (see Adams & Dickinson, 2014). We did not attempt to summarize this body of literature in our original manuscript, nor will we attempt to do so here, but a summary of one study might whet the reader's appetite for additional contact with this area of research. Dickinson, Balleine, Watt, Gonzalez, & Boakes (1995) reinforced lever pressing in rats and found that, under extinction conditions, rats that were food deprived did not respond more frequently than rats that were sated, unless they were previously exposed to the response-produced food while sated. This finding can neither be explained nor predicted by the current MO concept or by our revised MO concept. Clearly, further research is needed. Behavior analysts conducting basic research have not extensively examined MOs; we encourage them to do so.

Killeen (2019) rightly pointed out that we, like other behavior analysts, ignored private events in our analysis of MOs. We did so not because we viewed internal events such as those he termed "introspectively observed feeling[s] or state[s] of mind" (p. X) as unimportant, but rather

because we felt that incorporating them in our analysis would surely add complexity, but probably not add explanatory power. Perhaps we were wrong. In Skinner's (1957) example of a man dying of thirst and gasping "water" in the absence of any relevant stimuli, the poor unfortunate might well be imagining a person who had provided water in the past (i.e., an S^D). That S^D, a private event, evoked the response. Such an analysis is difficult to test, but that does not render it false or useless.

It is perhaps worth noting that our analysis of MOs, like the current one, ignores the physiological state of the organism. This is consistent with the level of analysis characteristic of behavior analysis, but MOs work by changing physiology, which in turn alters how organisms interact with their environment (i.e., behave). As we suggested elsewhere (Poling et al., 2017), analyzing MOs at the physiological level may provide a useful means of categorizing them, but that is a topic for another forum.

Catania and St. Peter (2019) raised important points about the complexity of the MO concept in its current form. For example, under the current conceptualization, several different effects of MOs are described. Establishing operations increase the reinforcing effectiveness of a stimulus and evoke behavior relevant to that stimulus. Establishing operations increase the punishing effectiveness of a stimulus and suppress (abate) behavior relevant to that stimulus. Abolishing operations decrease the reinforcing effectiveness of a stimulus and suppress (abate) relevant behavior. Abolishing operations decrease the punishing effectiveness of a stimulus and evoke (?; the opposite of suppress) behavior relevant to that stimulus. In our view, this is a confusing analytical framework.

The difficulties with description of evocative and suppressive functions are avoided, for the most part, with our reconceptualization because we suggest that it is not helpful to speak of

MOs evoking or suppressing behavior directly. Instead, we refer to changes in the functions of relevant discriminative and other antecedent stimuli. With respect to punishment, it is also useful to consider changes in the functions of antecedent stimuli. Importantly, punishment and reinforcement, although polar with respect to their effects on operant behavior (by definition) are not polar with respect to their underlying mechanisms, specifically respondent mechanisms (see Donahoe, Palmer, & Burgos, 1997). Antecedent stimuli that historically have been correlated with punishment can produce relevant respondent behavior (e.g., increased heart rate) and relevant operant behavior (e.g., behavior that has previously resulted in termination of those stimuli). As when predicting changes in behavior as a function of MOs related to reinforcers, when considering changes in behavior as a function of MOs related to punishers, prediction and control is enhanced by considering the functions of relevant antecedent stimuli.

Nevertheless, our proposed reconceptualization did not do away with the establishing and abolishing operations distinction. We agree with Catania and St. Peter's (2019) concern that this distinction generates additional, and perhaps unnecessary, complexity. We have found that motivating operations and their effects can be described without these terms with little trouble and that they add little in the way of precision. For example, when the term *establishing operation* is used, the speaker must specify whether this term is in reference to a change in the reinforcing or punishing effectiveness of relevant stimuli. Additionally, an establishing operation relevant to one stimulus can function as an abolishing operation with respect to another stimulus. For example, methylphenidate can increase the reinforcing effectiveness of academic task participation and decrease the reinforcing effectiveness of edibles (Northup, Fusilier, Swanson, Roane, & Borrero, 1997). If we instead refer to the operation as a *motivating operation* and specify which specific stimulus functions are altered, and in which direction, no additional

specialized terms are required. We will not explore this issue further here but suggest that other behavior analysts may benefit from considering this simplification, although a more detailed analysis of the implications of doing so may be warranted.

Conclusion

We are happy that our article regarding MOs generated thoughtful comments from 11 highly able behavior analysts and we hope that their work, with ours, encourages other behavior analysts to carefully and critically analyze where our field currently stands with respect to understanding the variables that influence reinforcement and punishment. Despite the seminal and valuable efforts of Michael and his associates to develop the MO concept – and we deeply appreciate and honestly applaud that work, work to which one of us (AP) is proud to have contributed – the concurrent concept falls short of being adequate for reasons that we have articulated. We revised the current concept to increase its practical value in predicting specifically how organisms will behave in response to MOs. We also made the concept simpler and easier to understand. But we know well the old adage, often attributed to Einstein, that a wise person "makes things as simple as possible, but not simpler." After considering the nine commentaries, we believe that our initial definition could be substantially improved by changing one word, resulting in the following definition: *MOs are operations that modulate the reinforcing or punishing effectiveness of particular kinds of events and the control of behavior by antecedent stimuli historically relevant to those events*.

There are, of course, other definitions that may serve equally well, or even better. For example, we often hear behavior analysts define MOs as *operations that alter the effectiveness of reinforcers and punishers.* This brief but, arguably, adequate definition of the MO omits any reference to what is now classified as the "behavior-altering" effect and also omits the associated

changes in stimulus control that are described in our proposed definition. Such a definition may be adequate because, as Michael (1993) pointed out, “it seems that most events, operations, or stimuli that alter the reinforcing effectiveness of other events also alter the momentary frequency of occurrence of any behavior that has been followed by those other events” (p. 193). However, they do not always do so, as in Dickinson et al.’s (1995) study. That they often have this effect, and the mediational role of antecedent stimuli, can easily be explained when it is helpful to do so.

Regardless of how MOs are defined, the subtypes considered in the current concept are not particularly useful and, equally importantly, do not cover the full range of operations that serve as MOs. It appears to us that many behavior analysts, including some whose comments we address in this article, are convinced that the current concept provides a fully adequate behavior-analytic account of “motivation.” It does not. Neither does our reconceptualization. More, and more innovative, conceptual and empirical work is needed, and we hope that work soon appears.

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