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The Motivating Operations Concept: Current Status and Critical Response

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Abstract This paper reviews the current status of the Motivating Operation Concept (MOC), followed by a critical response to Whelan and D. Barnes-Holmes (2010), who argued against the MOC and proposed an alternative analysis of motivation, the *Consequence-Valuing Operation* (*CVO*). In this paper, we: (a) review the MOC and discuss its conceptual and empirical status, (b) clarify certain aspects of the MOC, (c) correct Whelan and D. Barnes-Holmes's inaccurate descriptions of the MOC, and (d) critique the CVO and related concepts. We demonstrate that the MOC is a high-impact innovation in behavior analysis that provides a useful theoretical framework for analyses of operant (instrumental) behavior. In contrast, the case made by Whelan and D. Barnes-Holmes for the competing CVO concept suffers from a range of problems. We, therefore, conclude that the MOC provides a superior and more useful behavioral analysis of motivation.

Keywords Motivating operations · Conditioned motivating operations · Establishing operations · Abolishing operations · Evocative effect · Abative effect · Motivation · Consequence valuing operations

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Behavior-analytic formulations of motivational concepts have evolved since the field's founding. For example, Skinner (Skinner 1938, 1953, 1957) described motivational variables such as deprivation, satiation, and painful (aversive) stimulation as important controlling variables of operant behavior. In 1950, Keller and Schoenfeld introduced the motivational concept *Establishing Operation* (*EO*), which was later expanded upon by Millenson (1967). Building on these earlier treatments, Michael alone wrote several papers that expanded the EO concept (e.g., Michael 1982, 1988, 1993c, 2000) and contributed to articles with multiple authors that did the same (e.g., Iwata et al. 2000; Laraway et al. 2003; Sundberg and Michael 2001). We use the term *Motivating Operation Concept* (*MOC*) to refer to the most recent version of the general approach to motivation that emerged from Michael's work in order to distinguish this approach from the controlling variables it describes. The MOC has provided important conceptual tools for behavior analysts in a variety of areas, especially Applied Behavior Analysis (see Iwata et al. 2000; Miguel 2013). For examples and relevant data, see Table 1 (which provides citations to articles from different areas that have used the MOC), Fig. 1 (which depicts cumulative citations to Michael 1982, 1993c; and Laraway et al. 2003), and Fig. 2 (which shows categories of articles that have cited Laraway et al. 2003), the Reference section, and the Bibliography (which lists the articles that have cited Laraway et al. 2003). We include data on

Table 1 Areas of selected publications using the MOC

Research Area	Publication
Academic Behavior	Allday and Pakurar 2007
Behavioral Pharmacology	Byrne et al. 2000; Northup et al. 1997; Poling 1986; 2001; Poling and Byrne 2000; Poling and LeSage 1992; Raiff and Dallery 2008
Clinical Psychology and Clinical Behavior Analysis	Lejuez et al. 2005; Ringdahl and Falcomata 2009; Wilder and Wong 2007
Consumer Choice	Fagerstrøm 2010; Fagerstrøm et al. 2010
Environmentally Friendly Behaviors	Manuel et al. 2007
Feeding Behavior	Rowland et al. 2008; Tapper 2005
Gene-Environment Interactions	Langthorne and McGill 2008
Intimate Partner Violence	Bell and Naugle 2008
Joint Attention	Dube et al. 2004; Naoi et al. 2008
Organizational Behavior Management and Workplace Safety	Agnew 1998; Goltz 2003; Olson et al. 2009; Olson and Winchester 2008
Self-Injurious Behaviors	Worsdell et al. 2000; Langthorne and McGill 2008
Stereotypy and Other Challenging Behaviors	Gould 2005; Langthorne et al. 2007; Lanovaz et al. 2009; Rapp 2004

the citations of Laraway et al. (2003) because this was the last major revision of the MOC.

Based on its growing usage in the empirical and conceptual literature, the MOC has emerged as a robust and useful approach to understanding motivation in the context of operant conditioning processes. Indeed, from reviewing the literature, it is clear that the MOC has facilitated successful action on the part of behavioral scientists. As Zuriff (1980) noted, in Skinner's pragmatic view of science a "good" or "true" concept allows behavioral scientists to "operate successfully on [their] material" (Skinner 1945, p. 293) and "respond effectively to the situation it describes" (Skinner 1974, p. 235). Given our review of the literature, the MOC meets these pragmatic criteria. Although we do not believe that the current iteration of the MOC provides some sort of "final" or "ultimate" description of motivational variables, it does provide a set of "rules for effective action" with respect to these variables (Skinner 1974, p. 235). Of course, we anticipate that the MOC will undergo additional refinement

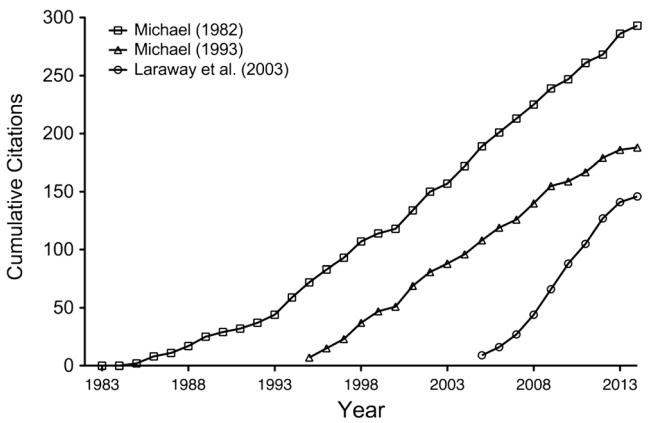

Fig. 1 Cumulative citations to Michael (1982), (1993c), and Laraway et al. (2003) as of April 2013

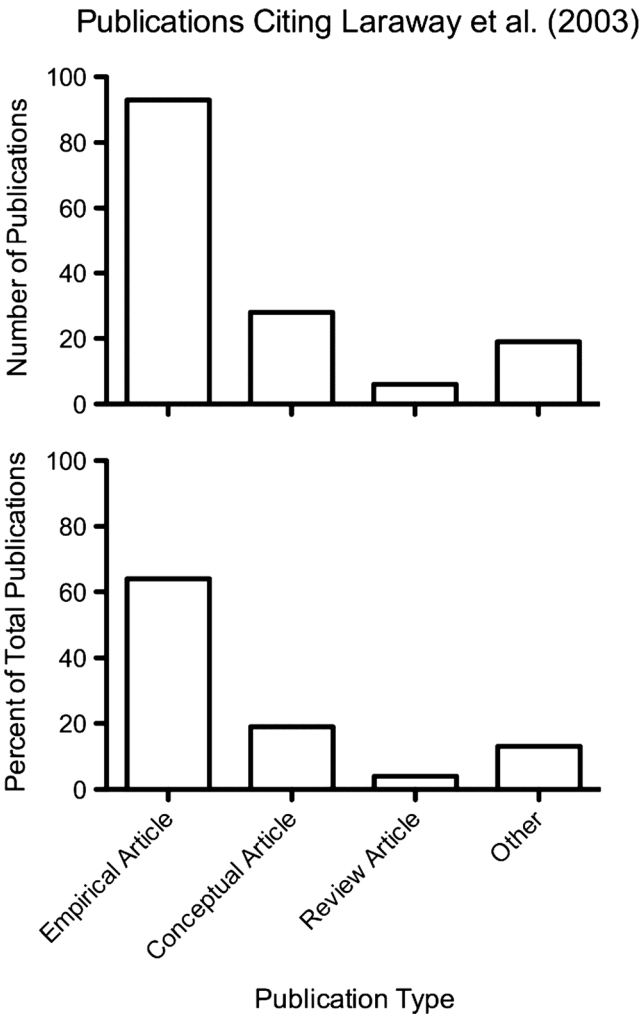

Fig. 2 Types of publications citing Laraway et al., (2003) as of April 2013. The top graph depicts the total number of each type. The bottom graph depicts the percentage of each type.

based on conceptual and empirical work. We hope that this paper will spur on such work.

In 2010, Whelan and D. Barnes-Holmes provided a critical assessment of the conceptual and empirical status of the MOC and proposed an alternative motivational concept, the *Consequence Valuing Operation* (*CVO*). Their paper invites a thorough response. In the current paper we provide arguments in favor of the MOC and against the CVO and related concepts. In this paper we: (a) review the MOC and discuss its conceptual and empirical status, (b) clarify certain aspects of the MOC, (c) counter Whelan and D. Barnes-Holmes's main criticisms of the MOC, and (d) critique their alternative motivational concept, the CVO. To assist scholars and researchers interested in the MOC, we have also provided a comprehensive bibliography of publications (as of Spring 2013) that have cited Laraway et al. (2003).

The Conceptual and Empirical Status of the Motivating Operations Concept

Motivating Operations (MOs) have two primary effects. They (a) 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*). The term *MO* encompasses two types of motivational variables: Establishing Operations (EOs) and Abolishing Operations (AOs). EOs make consequences more effective, whereas AOs make them less effective. Although response rate has served as the traditional dependent variable in behavior analysis, researchers may measure response strength in different ways, including effort, choice, schedule control, and resistance to extinction (or other disrupters) (e.g., Millenson 1967, Ch. 15; Nevin and Grace 2000; Poling 2001). We feel that the effects of MOs on various measures of response strength should be investigated more thoroughly. MOs may also influence the current strength of behavior by influencing the behavior-altering effects of relevant discriminative stimuli (S^Ds) (Laraway et al. 2003; Lotfizadeh et al. 2012a; McDevitt and Fantino 1993; Michael 1988, 1993c, 2007) because stimulus control generally requires effective consequences. Systematic studies of the effects of MOs on stimulus control are rare, and this is an important area for future research, both basic and applied.

Although previous authors' writings on the MOC (e.g., Michael 1993c) have described behavior-altering effects as "momentary" or short-lived, these effects may be relatively long-lasting or even permanent (Dougher and Hackbert 2000; Langthorne et al. 2007; Poling 2001). For example, individuals with Prader-Willi syndrome engage in excessive food-related behaviors, including overeating and hording. The brain satiation mechanisms of these individuals do not function properly, so food ingestion does not serve as an effective AO for subsequent food consumption (i.e., these people do not "feel full," so eating behavior continues). This permanent EO effect, termed *hyperphagia*, continues throughout these individuals' lifespan (Cassidy and Driscoll 2009). As another example of a long-term MO, consider congenital indifference to pain, in which individuals do not experience painful stimulation as aversive and will not seek to escape from such stimulation (Nagasako et al. 2003). In other words, these individuals have a persistent AO for pain as an aversive stimulus, and this condition may last across an individual's lifespan. Because of such examples, we see no reason to restrict a priori the time frame in which MOs can produce their effects, and we expect differences will exist across MOs and individuals in the duration of these effects.

The value-altering effect is seen in changes in the strength of a behavior after consequences have been delivered following that behavior. In other words, the value-altering effects of MOs make consequences more or less effective at changing the behaviors they follow. For example, Lattal and Williams (1997) found that rats maintained at lower percentage of free-feeding body weights (i.e., more food-deprived animals) consumed the same number of pellets in magazine training in a shorter period of time than did rats maintained at higher percentages of free-feeding body weights. That is, increasing food deprivation increased how fast the rats ate. In addition, lower body weights generally were associated with quicker acquisition of a new operant response (lever pressing) and engendered higher response rates compared to those observed at higher weights. Thus, manipulating the level of food deprivation changed the capacity of the reinforcer to strengthen and maintain behavior; this change exemplifies the value-altering effect. As this example shows, the value-altering effect makes operant conditioning possible because, by definition, consequences must have an effective behavioral function to change or maintain behavior. More generally, one might say that MOs change the appetitive or aversive value of events, which makes these events more or less effective as controlling variables in operant relations.

MOs have two potential behavior-altering effects: the *evocative* effect and the *abative* effect. As their names imply, these effects refer to MO-influenced increases and decreases in the *current* strength of behavior, respectively. Michael (2000) and Klatt and Morris (2001) noted that behavior-altering effects of EOs for positive reinforcement are most clearly observed during extinction. This is because, when reinforcer delivery is ongoing, the behavior-strengthening (or maintaining) effects of reinforcer delivery occur simultaneously with the behavior-altering effects of the relevant EO (Michael et al. 1997). During extinction, the behavior-altering effect can be isolated because reinforcer delivery does not occur. Therefore, researchers can use responding in extinction to assess the behavior-altering effects of relevant MOs uncontaminated by the current effects of reinforcer delivery. Of course, to attribute such changes in behavior to the

actions of an MO, discriminative stimulus (and other) conditions must be held constant.

Skinner (1938, Figure 137, p. 388) reported data demonstrating the behavior-altering effect in extinction. He trained rats to lever press using food delivery as a reinforcer and then fed them varying amounts of food (0, 2, 4, or 6 g) prior to an extinction session. As the amount of food consumed before testing increased, rats emitted fewer responses in the extinction sessions, thereby demonstrating the behavior-altering effect of prefeeding greater amounts as AOs. In terms of methodology, when attempting to isolate the behavior-altering effects of MOs, experimenters should control other variables that might affect the current strength of the relevant behavior (however measured), such as the presence of the proper tools/operanda, physical barriers, capacity to respond, behavioral history, response force/effort, discriminative stimuli, and so on.

Apart from extinction, the behavior-altering effect can be observed in discrete-trial situations or in response chains, in which the behavior occurs, a reinforcer is delivered, and the behavior for that reinforcer stops (although other behaviors may continue). Manding provides one example. Skinner (1957) defined mands as verbal operants under the primary control of motivational variables (see also Michael 1988). That is, the mand specifies its own reinforcer (i.e., we "ask for what we want"). When the relevant MO is in effect (making some event function as a reinforcer), mands that have produced that event will occur more often (the evocative effect), assuming the presence of an audience and possibly other discriminative stimuli. Once that event occurs, manding ceases (the abative effect). There is also evidence that "derived" mands developed via conditional discrimination procedures may occur without previous reinforcement for those mands (e.g., Barnes-Holmes et al. 2000; Rosales and Rehfeldt 2007), so reinforcer delivery at that time cannot account for their occurrence. Regardless of whether an individual learns a mand through direct contact with relevant contingencies or via derived stimulus relations, the relevant MO evokes the behavior prior to reinforcer delivery and is, therefore, an example of the behavior-altering effect. Indeed, in reading Skinner's (1957) treatment of the mand, the behavior-altering effect (especially the evocative effect) is inherent to the concept.

The effects of MOs for aversive stimuli are less well characterized, reflecting the relative lack of research on punishment and aversive control compared to reinforcement (Baron 1991; Cipani and Spooner 1997; Poling 2001). As one example, the onset of an aversive stimulus (e.g., electric shock) can serve as its own EO, evoking escape behavior that has resulted in its offset with the offset serving as the reinforcer for that behavior. As another example, a sufficient drop in blood levels of opioids after a history of repeated administrations of opioids (such as morphine) can function as an EO for painful stimuli by increasing subjects' sensitivity to such stimuli, thereby making these stimuli more aversive and evoking increased escape responses (for a review of such *opioid-induced hyperalgesia*, see Angst and Clark 2006).

Unless studied during extinction for the measured operant, examining the effects of MOs on punishers requires an ongoing schedule of reinforcement to maintain the behavior to be punished (Pierce and Cheney 2008, p. 124). MOs that affect the value of the reinforcer maintaining a behavior may also influence the effectiveness of punishers delivered for that behavior. For example, Azrin, Holz, and Hake (1963) examined the effects of food deprivation (60, 65, 70, 75, and 85 % of free-feeding weight) on the ability of electric shock to punish pigeon's responses maintained by food delivery. Lower percentages of body weight systematically increased responding for food, counteracting the punishing effects of electric shock (see Figure 5 in Azrin et al. 1963). Therefore, lowering the percentage of free-feeding body weight served as an AO for the punishing effectiveness of electric shock, whereas increasing the percentage served as an EO for shock as a punisher. From these results, we can see that one MO may affect different consequences at the same time (Michael 2000; Laraway et al. 2003; Poling 2001) and that the study of punishers and their MOs must consider the characteristics of the reinforcement contingencies (and related MOs) that maintain the target behavior. Given its importance and prevalence in controlling everyday behavior and aberrant behavior in special populations (Cipani and Spooner 1997; Pierce and Cheney 2008; McGill 1999; Poling 2001; Sidman 2001), a renewed interest in aversive control and related MOs is warranted (O'Reilly et al. 2007a).

In many empirical applications and studies of the MOC, researchers have not attempted to isolate the two effects of the MOs they studied (Klatt and Morris 2001), but this has changed in recent years. For example, O'Reilly, Edrisinha, Sigafoos, Lancioni, and Andrews (2006a) experimentally demonstrated both the behavior-altering and value-altering effects of access to social attention with a man diagnosed with autism. A functional analysis revealed that attention served as a positive reinforcer for challenging behavior (e.g., bizarre speech). Prior to some sessions, the participant received attention, but before other sessions he did not. When pre-session access to attention was restricted, and challenging behavior was followed by attention, the challenging behavior occurred at a frequency higher than when pre-session access to attention was available. Thus, the manipulation of pre-session attention changed the reinforcing value of attention delivered dependent on challenging behavior (a value-altering effect). O'Reilly et al. (2006a) also manipulated the MO prior to extinction sessions and measured behavior *in the absence of reinforcer delivery* (i.e., in extinction), thereby isolating the behavior-altering effect of the MO. With the stimulus conditions held relatively constant, the participant engaged in less challenging behavior during extinction sessions when he

received pre-session attention than when he did not receive pre-session attention. These behavior-altering effects are termed *abative* and *evocative*, respectively. Several other studies have empirically demonstrated the behavior-altering effect separately from the value-altering effect (e.g., O'Reilly et al. 2006b, 2007a, b; 2008).

Unconditioned and Conditioned Motivating Operations

We can distinguish MOs based on whether or not they require a learning history to make them effective. *Unconditioned Motivating Operations* (*UMOs*) do not require a learning history to exert their value-altering effects, whereas *Conditioned Motivating Operations* (*CMOs*) do. In this context, *unconditioned* and *conditioned* are synonyms for *unlearned* and *learned*, respectively. This usage is comparable to the description of *unconditioned* and *conditioned* reinforcers and punishers (cf., Michael 1993b; Pierce and Cheney 2008; Skinner 1953). UMOs and CMOs comprise both EOs and AOs. Some events serve as UMOs by making their own termination effective as a reinforcer or punisher. For example, the onset of an unconditioned aversive event (e.g., pain, excessive temperature change, loud noise) may function as a UEO by making its offset more reinforcing and evoking behaviors that produce this result (escape). The onset of an unconditioned appetitive event (e.g., eating) makes its offset (e.g., loss of food, time out from reinforcement) punishing and will abate behaviors that have resulted in its offset. Two general UMOs include deprivation (UEO) and satiation (UAO) of unconditioned reinforcers (e.g., food, water, sex). Other UMOs include the administration of certain drugs, hormonal changes, circadian rhythms, gene expression, brain damage, and other biological phenomena that change the effectiveness of operant consequences. For example, the genetic makeup and resulting biological processes responsible for Prader-Willi syndrome, discussed above, would be considered a UEO for food as a reinforcer.

Like some UMOs, some CMOs can be described as involving deprivation (CEO) or satiation (CAO) of their conditioned reinforcers. As mentioned previously, O'Reilly et al. (2006a) showed that deprivation (or satiation) of attention made it more (or less) effective as a conditioned reinforcer. Other CMOs, however, do not seem to involve deprivation or satiation. Michael's (1988, 1993c, 2007) original taxonomy described three CMO subtypes. In their critique of the MOC, Whelan and Barnes-Holmes (2010) were dismissive of the utility and empirical status of these CMO subtypes. Therefore, we will provide definitions and, to the extent possible, empirical examples for each subtype to establish their explanatory power and conceptual status. Although we do not assume that Michael's subtypes exhaust all possible learned motivational variables, they do provide a useful starting point for behavior analysts. Future empirical and conceptual work may suggest additional learned motivational variables and/or ways that Michael's subtypes should be revised. We hope that this paper proves helpful for such work. For readers interested in a wide variety of examples and additional detail on this topic, we refer them to articles by Langthorne and McGill (2009), McGill (1999), Michael (1988, 1993c, 2007), Olson, Laraway, and Austin (2001), Sundberg (1993), and Tapper (2005).

Several authors (e.g., Blakely and Schlinger 1987; Dougher and Hackbert 2000; Poling 2001; Poling and LeSage 1992; Roche et al. 2002; Schlinger and Blakely 1988; Valdivia et al. 2006) have noted that some verbal stimuli, including rule statements, can also function as learned motivational variables. At this point, it is unclear the extent to which verbal MOs fit the description of the three CMO subtypes described below, and more research on the motivational functions of such stimuli is needed to clarify how such stimuli fit the MOC framework. Relational Frame Theory (RFT) appears to be a promising approach to studying the motivational effects of verbal stimuli (e.g., Roche et al. 2002; Valdivia et al. 2006), as it includes the formative and motivative augmental concepts that describe verbal stimuli that serve as MOs. As Rosales and Rehfeldt (2007) noted, an integration of RFT and the MOC appears a worthy endeavor; we agree.

In any given behavioral episode, there are likely multiple, possibly interacting variables (including MOs, S^Ds, etc.) influencing behavior at any given time, and these variables may change in strength across the episode. This is especially true for human CMOs, given the likely involvement of language and complex learning histories in their development. Therefore, in our definitions and examples below, we will focus on one relevant CMO at a time, while recognizing the likelihood of complex, multiple controlling variables operating in the background and possibly in combination. This simplification is merely for explication purposes and does reflect a view that the variable under discussion is the only one operating in the behavioral episode.

Surrogate CMO (CMO-S)

The CMO-S is a formerly neutral stimulus (NS) that becomes effective by being associated with an already effective MO, thereby acquiring the same motivational properties as that MO (i.e., it substitutes for the original EO or AO). This association may involve a direct pairing between the NS and original MO or through some other process, such as derived relational responding (e.g., Valdivia et al. 2006). As an example of events that could be described by the CMO-S, consider the conditioned abstinence (or withdrawal) phenomenon (for a review, see Childress et al. 1988). Conditioned abstinence occurs when formerly NS associated with drug self-administration or withdrawal later come to elicit withdrawal symptoms, which may be behavioral, affective, or physical in nature. Relevant to the CMO-S effects of these stimuli, conditioned withdrawal symptoms include self-reported drug

craving and seeking and subsequent self-administration. Kenny, Chen, Kitamura, Markou, and Koob (2006) provided an empirical example, in which they demonstrated a CEO-S for heroin as a reinforcer. These researchers paired a light and tone, presented together, with naloxone-precipitated withdrawal (a UEO for heroin as a reinforcer) in rats physically dependent on heroin. After the pairing procedure, when presented by itself, the compound stimulus increased rats' self-administration of heroin. Therefore, the compound stimulus functioned as a CEO-S in that it increased the reinforcing effectiveness of drug delivery and evoked self-administration responses. That is, it served the same behavioral function as did naloxone administration. Although these authors did not use the MOC terminology, and described these stimuli in Pavlovian terms, the compound stimulus in this study fits the conceptual definition of the CEO-S.

Not surprisingly, CMO-Ss for punishers are less well understood, but the research on conditioned hyperalgesia may provide an example of this type of CMO. Formerly neutral stimuli that reliably predict the onset of morphine administration can later produce conditional tolerance to the analgesic properties of the drug, with subjects showing a greater sensitivity to pain after exposure to these stimuli (e.g., Krank 1987; Krank et al. 1981). Thus, these drug-related stimuli increase the aversive value of painful events and evoke escape responding, such as removing paws from a hot plate and licking them. In terms of the MOC, these stimuli function as CEO-Ss for painful events. Although these researchers did not examine the punishing effects of the hot plate in the context of operant conditioning, presumably the increase in the aversiveness of pain would make such stimuli more effective as punishers, given that events that evoke escape responding often also function as positive punishers (Baron 1991). Of course, this hypothesis needs to be tested experimentally.

Reflexive CMO (CMO-R)

The CMO-R makes its own offset a reinforcer or punisher (e.g., due its predictive relationship with appetitive or aversive stimuli). Carbone, Morgenstern, Zecchin-Tirri, and Kolberg (2010) described several applied examples of the CMO-R. For instance, they discussed the findings of Smith, Iwata, Goh, and Shore (1995), who manipulated aspects of task demands (requests by experimenters for the participant to engage in behavior) and observed changes in self-injurious behavior (SIB). Smith et al. had determined that SIB was functionally maintained by escape from these demands. Characteristics of the task demands the researchers manipulated included novelty of the required task and rate of requests to engage in tasks. In one experiment, Smith et al. found that presenting task demands at a high rate generally produced more SIB than when they presented tasks at a lower rate. Thus, compared to a lower rate of task demands, a high rate of task demands functioned as a CEO-R, making escape from those demands more effective as a reinforcer and evoking higher rates of escape responding. Compared to a high rate of demands, a low rate functioned as a CAO-R, making escape from the demands less effective as a reinforcer and abating escape responding.

As an example of a CEO-R for which its offset functioned as a punisher, consider the time-out ribbon procedure used by Foxx and Shapiro (1978). As long as the participants behaved appropriately, they were allowed to wear the time-out ribbon, which gave them access to reinforcers for good behavior. If they misbehaved, they lost the ribbon for a short period of time, during which they could not earn reinforcers. In this situation, the correlation between wearing the ribbon and backup reinforcers made the loss of the ribbon aversive. In MOC terms, having the ribbon functioned as a CEO-R in making its loss function as an effective punisher and abated misbehavior that resulted in its loss. This case is also a good example of a stimulus having multiple behavioral functions, in that having the ribbon simultaneously functioned as an S^D for the availability of reinforcement for desirable behaviors and as a CEO-R that established ribbon removal as an effective punisher.

Transitive CMO (CMO-T)

The CMO-T affects the conditioned reinforcing or punishing effectiveness of some other event, often occurring within a behavior chain. The most commonly studied CMO-T changes the conditioned reinforcing effectiveness of stimuli or objects that eventually lead to some other effective consequence (sometimes called the "blocked-response CEO," Michael 1988; Pierce and Cheney 2008, p. 273). In considering this kind of CMO-T for reinforcers, we need to realize that at least two separate motivational variables are involved. One is the *starting MO* (a UMO or CMO), or the "reason" for starting the chain of behavior that influences the effectiveness of the terminal reinforcer at the end of the chain. The second is the *intermediate MO* (a CMO-T), which moves the individual closer to the terminal reinforcer controlled by the starting MO. In addition to influencing the effectiveness of the original consequence, the starting MO influences the function of any intermediate CMOs. Skinner (1957, pp. 253–254) discussed this type of conditioned motivational variable in an example in which a person was offered a "handsome reward" for a drawing of a cat, which then made writing tools effective as reinforcers and evoked behaviors that had produced such tools in the past (e.g., searching one's bag, asking someone for a pen). Not surprisingly, CMO-Ts for punishers have not been studied or discussed as often as those for reinforcers, although relevant studies may have been conducted. To our knowledge, there is no clear published example of a CMO-T for punishment. Again, further research in this area may be of value.

Consider an everyday example of the CMO-T for a reinforcer. You have a nice bottle of wine that you want to share with your lover. The starting MO is the condition that made you desire to drink the wine with your lover, such as an anniversary to celebrate or a reunion after time apart. The terminal reinforcer is drinking the wine, which occurs at the end of the behavior chain. Having the unopened bottle of wine functions as a CEO-T that makes corkscrews (and wine glasses) valuable as conditioned reinforcers and evokes behavior that leads to obtaining them (e.g., search behavior). Once you have opened the bottle, corkscrews become less valuable and behaviors that obtain them stop. That is, the opened bottle functions as a CAO-T that abates corkscrew-seeking and -using behaviors (at least until that bottle is empty). Of course, there are likely other MOs, both verbal and nonverbal, that might influence behavior in this example (e.g., private rules regarding the effects of alcohol on inhibitions). Regardless, corkscrews generally are not valuable in and of themselves and we do not go around seeking them until there is a reason for doing so.

Hall and Sundberg (1987) empirically demonstrated the effects of a CMO-T in the context of mand training. They trained two students with hearing impairments and severe developmental disabilities to emit mands for missing items required to complete a chain of behaviors that led to an already effective reinforcer. For example, one chain involved making instant soup. When the hot water was missing, participants had to mand for the hot water to complete the chain. Thus, the act of removing the hot water, which was required to produce an already effective reinforcer (the soup), functioned as a CEO-T by making the hot water more valuable and causing the participants to ask for it (for an additional example, see Sigafoos et al. 1989). The starting MO was food deprivation that made soup an effective (terminal) reinforcer. Note that obtaining the hot water served as a CAO-T in that it made hot water less effective as a reinforcer and abated mands for hot water. In other words, once the participants got the hot water, they no longer needed it to complete the chain, so they stopped asking for it.

Michael's taxonomy of MO subtypes is important in two regards. First, in contrast to traditional approaches to motivation, it focuses attention on measurable (and potentially manipulable) environmental events as determinants of the effectiveness of operant consequences. This focus is consistent with a behavior-analytic approach to the study of controlling variables. Second, it emphasizes the specific operations necessary to alter the reinforcing (or punishing) effects of particular stimuli. Therefore, it may be possible to specify *a priori* whether a given change in the environment will affect the function of operant consequences, and, therefore, to predict (and potentially control) the level of occurrence of the relevant operant responses. Absent this predictive capacity, approaches to motivation can easily become circular. Although Michael's taxonomy has generated considerable interest and proven useful in generating research and conceptual analysis, at this point it is a considerable stretch to extend that taxonomy to account for all of the physical events that can alter the reinforcing and punishing effectiveness of operant consequences. In our view, the general *approach* to the study of motivation championed by Michael is more important than the specifics of his analysis, although as discussed in the next section, that analysis has considerable merit.

A Rebuttal of Whelan and D. Barnes-Holmes's Criticisms of the MOC

Preliminary Remarks

In their critical review paper, Whelan and D. Barnes-Holmes cited three (of six) papers that were written in response to Michael (1993c). In our opinion, the material the authors culled from these papers was selective and does not fairly represent the general positions of the authors who commented on Michael's paper. All six reviews (Catania 1993; Cherpas 1993; Hesse 1993; McDevitt and Fantino 1993; Schlinger 1993; Sundberg 1993) praised the EO concept, and none questioned its usefulness, although the two cited heavily by Whelan and D. Barnes-Holmes (Catania 1993; McDevitt and Fantino 1993) did question some aspects of Michael's analysis and verbiage (we discuss these critiques in subsequent sections).

As far as we can tell, the two papers published by Catania (1993) and McDevitt and Fantino (1993) form the exclusive basis for arguments put forth by Whelan and D. Barnes-Holmes (2010) against the MOC. Their use of selective quotations and commentary imply that Michael's article received only criticism. However, these two reviews did not reject Michael's concept of the EO in general, but merely questioned specific aspects of it. For example, Catania expressed concern over Michael's addition of many new terms and abbreviations, and McDevitt and Fantino expressed concern that the EO might supplant the S^D in terms of describing the evocative effects of antecedent stimuli. Yet, Whelan and D. Barnes-Holmes stated that Catania (1993) "argued that the term establishing operation is inherently misleading" (p. 340). A careful reading of Catania's article reveals that he said nothing of the kind. Catania did not object to the EO concept in general nor did he argue that it is "inherently misleading." He certainly did not argue for its abolition (e.g., he included the concept in his 2007 text). Rather, he objected to particular aspects of Michael's (1993c) presentation of the EO. Similar approval of the Michael's paper can be found in McDevitt and Fantino's (1993) review: "As a fourth term to supplement the traditional three-term contingency of behavior, the *notion of establishing*

operations provides a helpful system for identifying motivational functions affecting behavior" (p. 225; emphasis added).

Whelan and D. Barnes-Holmes (2010) also cited but did not discuss Cherpas's (1993) review. Specifically, Cherpas argued that EOs do not have reinforcer-establishing (value-altering) effects and only have evocative (behavior-altering) effects. As we will discuss later, Whelan and D. Barnes-Holmes argued that the evocative (behavior-altering) effect is not necessary and that motivational variables only have value-altering effects. So Cherpas's view directly contradicts that of Whelan and D. Barnes-Holmes, yet these authors do not discuss this in their paper. They simply cite Cherpas's article in passing as evidence against the MOC and move on. In addition, Whelan and D. Barnes-Holmes did not acknowledge the other three reviews, which were uniformly positive (i.e., Hesse 1993; Schlinger 1993; Sundberg 1993). Nor did Whelan and D. Barnes-Holmes consider or discuss Michael's (1993a) replies to these critiques.

Specific Critiques of the MOC

Whelan and D. Barnes-Holmes (2010, starting p. 339) alleged that there are serious problems with the MOC, arguing that ". . . the MO concept has not gone entirely unchallenged (e.g., Catania 1993; Cherpas 1993; McDevitt and Fantino 1993)" (p. 338). As noted above, the authors only discuss some aspects of the Catania (1993) and McDevitt and Fantino (1993) reviews, while failing to discuss Cherpas's (1993) review. Borrowing heavily from Catania and McDevitt and Fantino, the authors made three main claims, upon which most of their subclaims are dependent. First, they claimed that the MOC uses too many new terms ("resulting in a lack of parsimony") that are ambiguous, that deviate from traditional behavior-analytic verbal practices, and that will confuse behavior analysts. Second, they alleged that the MOC has an unacceptable "dual nature" because it distinguishes between value- and behavior-altering effects. Third, they claimed that the MOC lacks empirical supporting evidence and has not been used by behavior analysts. In what follows, we rebut these criticisms and associated implications.

Terminological Problems: A Rebuttal of Claims about Ambiguity, Parsimony, and Established Verbal Practices

The section entitled "Terminological Ambiguities" (starting on p. 339) contains several arguments, most of which have nothing to do with the word "ambiguous" as normally defined: "open to more than one interpretation; not having one obvious meaning" Ambiguous [Def. 1] (2013). Whelan and D. Barnes-Holmes never specified how the MOC contains ambiguous terms. Instead, they offered two main criticisms about terminological problems of the MOC: (a) it uses too many new terms, which they argue violates the doctrine of parsimony and will create "terminological confusion" (p. 339); and (b) it is inconsistent with "traditional behavior-analytic" verbal practices because it applies the modifiers *conditioned* and *unconditioned* to behavioral operations when such terms should only be applied to modify behavioral processes (p. 340).

By stating that the MOC uses too many new terms, Whelan and D. Barnes-Holmes (2010) repeat Catania's (1993) critique that Michael (1993c) introduced a large number of new technical terms (rather than ambiguous terms) in his formulation of the EO. Whelan and D. Barnes-Homes did not acknowledge or rebut Michael's (1993a) responses to Catania's criticisms. Moreover, these authors did not make a cogent case that these terms are vague or open to multiple interpretations, that these terms have created terminological confusion, or that the mere number of terms violates the doctrine of parsimony. Furthermore, borrowing from Catania, the authors inflated the number of terms in the MOC taxonomy by listing some of Michael's preferred terms for certain functional relations in classical (respondent) conditioning. The MOC as presented by Laraway et al. (2003) does not use any of the terms that the authors admitted "are not directly related to motivation . . . but nonetheless appear important to Michael's conceptual analysis" (p. 339). Whether these terms are important for Michael's conceptual analysis of behavior is irrelevant to the MOC in its current form. With respect to parsimony, the number of terms a theory uses is not, in and of itself, a criterion for parsimony. According to Rosenthal and Rosnow (2008), "Occam's razor [the doctrine of parsimony] requires us to 'cut away' what is unnecessary or unwieldy, because what can be explained on fewer principles or with fewer entities is explained needlessly by more" (p. 48). Similarly, Kantowitz, Roediger, and Elmes (2009) stated that parsimonious theories "explain many results with few explanatory concepts" and that "if two theories have the same number of concepts, the one that can explain more results is a better theory" (pp. 14–15). We will show later that the CVO does not have equal explanatory power compared to the MOC, nor does it truly use fewer terms. Regarding the possible confusion produced by the MOC, Miguel (2013) noted: "Despite the fairly recent change in taxonomy (from EOs to MOs), behavior analysts have had no problem adopting the new term . . ." (p. 9).

Whelan and D. Barnes-Holmes (2010) further criticized the MOC because it uses "terms in ways that depart from traditional behavior-analytic verbal practices" (p. 340). Specifically, echoing Catania (1993), they claimed that the MOC uses the modifiers *conditioned* and *unconditioned* inappropriately because such modifiers are reserved for naming the properties of behavioral processes, not operations. The authors restated one of Catania's critiques and provided no new reasons to reject the use of *conditioned* and *unconditioned* as synonyms for *learned* and *unlearned*, respectively. Catania showed concern that the terms *conditioned* and *unconditioned* might

lead to misunderstandings among behavior analysts due to their association with classical conditioning. In his reply to Catania, Michael (1993a) noted that behavior analysts often use these terms to describe controlling variables in operant conditioning (e.g., *conditioned reinforcers*). To us, it makes just as much sense to use *conditioned* and *unconditioned* to refer to MOs as it does to use these modifiers to refer to reinforcers and punishers, which is standard verbal practice in behavior analysis (e.g., Pierce and Cheney 2008; Skinner 1953). In reviewing the relevant literature, we found no evidence that authors were confused by, or unsatisfied with, the terms *conditioned* and *unconditioned* to refer to MOs in the context of operant conditioning (e.g., Carbone et al. 2010; McGill 1999; Olson et al. 2001; Tapper 2005).

The Dual Nature Problem: A Rebuttal of the Critique of the Two Effects of MOCs

A second major criticism of the MOC offered by Whelan and D. Barnes-Holmes (2010) is that the MOC is a "dual-process" concept, in that it distinguishes between value-altering and behavior-altering effects. The authors argued that the behavior-altering effect (evocative and abative) is unnecessary. They gave three arguments in support of this claim. The main argument against the "dual nature" of the MOC relied on this quote from McDevitt and Fantino (1993): ". . . the evocative function of an EO seems to bring us no closer to predicting the nature of the response, which is usually dependent upon additional contextual stimuli" (p. 226). The claim in the first phrase of the cited sentence, that the evocative function of the EO does not bring us closer to prediction of behavior, is too strong of a conclusion given data from more recent studies. There is evidence that an MO can evoke behavior in the absence of a relevant S^D. Consider the findings of Edrisinha et al. (2011), who found that an effective EO may increase, and an AO decrease, responding in the presence of both an S^D and S^Δ during extinction. That is, the behavior-altering effects of the MO were seen regardless of the presence or absence of the relevant discriminative stimulus. 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. In everyday terms, we may want something and behave in ways to get it even in the absence of discriminative stimuli that indicate that the item is currently available. For example, a drug user undergoing withdrawal will likely engage in drug-seeking behaviors even if the relevant discriminative stimuli for obtaining the drug (e.g., a drug dealer) are absent. In contrast, most events that serve as discriminative stimuli do not exert stimulus control without relevant MOs in effect (Lotfizadeh et al. 2012a), although drug S^Ds may be an exception (e.g., Lotfizadeh et al. 2012b). The evocative effect of an MO can also be readily seen in an isolated fashion during escape or avoidance responding (Carbone et al. 2010). In fact, the evocative effect is necessary to describe the change in behavior produced by the onset of aversive events as well as the occurrence of mands.

The second phrase in McDevitt and Fantino's (1993) sentence is accurate because operant behavior is often multiply controlled. This applies to MOs, but it also applies to S^Ds, which may require the presence of other events to evoke behavior (for a review of the effects of MOs on stimulus control, see Lotfizadeh et al. 2012a). For example, Horner, M. Day, and J. Day (1997) demonstrated a problem behavior that was multiply controlled by an EO and an S^D. In this experiment, problem behavior was highest when both an EO and S^D were present, but occurred at near-zero levels when either variable was presented separately. That MOs and S^Ds may work together to change the current strength of behavior is reflected in a separate quote (not cited by Whelan & D. Barnes-Holmes, 2010) from McDevitt and Fantino that the EO "…seems to have a more general effect, namely that of strengthening related S^Ds" (p. 226). Laraway et al. (2003) and Michael (2007) made a similar point, but Whelan and D. Barnes Holmes (2010) did not acknowledge this or other more current conceptual papers on the MOC.

The second main argument against the MOC's "dual nature" hinges on a single quote from Klatt and Morris (2001), who wrote: "… even though the evocative effect of response deprivation *seems to be necessary for the first occurrence of instrumental responding*, *the effect is often unmeasured*" (p. 177, emphasis added). In their paper, Klatt and Morris noted that studies on response deprivation rarely have attempted to dissociate the value-altering and behavior-altering effects of motivational variables. This alleged criticism of the MOC is actually a comment about the research practices of behavioral scientists, not the ontological status of the behavior-altering effect: "unmeasured" does not mean "nonexistent." In fact, Klatt and Morris's statement that the evocative effect "…seems to be necessary for the first occurrence of instrumental responding…" (p. 177) supports the utility of the behavior-altering effect in describing operant behavior.

Whelan and D. Barnes-Holmes's (2010) third main argument against the "dual nature" of the MOC involves the notion that:

> On balance, perhaps Michael and colleagues are suggesting that the evocative effect occurs at the start of an experimental session before any reinforcer has been delivered *within that session*. However, this approach seems to ignore the fact that reentering the experimental context at the start of the session will likely possess discriminative properties for the availability of reinforcement, based on previous sessions (p. 341; emphasis in original).

There are several problems with this argument. First, multiple control is common and assumed in operant behavior

scenarios (experimental or applied). The MOC concept does not ignore the S^D or behavior-altering properties of contextual variables, but instead provides a more complete description of evocative/abative processes in operation. Consider an everyday example. You are watching hockey and drinking beer with your friends. One of your friends goes to the kitchen to get another beer. Assume that your friend is an S^D for requests for more beer (he is always happy to get you a cold one) and the beer is stocked in the fridge (beer is available). If your can is full, you are not likely to ask him to bring you another one even if the relevant S^Ds are present. According to the argument by Whelan and D. Barnes-Holmes, any request for beer is only due to the contextual variables functioning as S^Ds, but this contradicts daily experience. We do not normally go around requesting things we do not want, even if those things are available. The implications of exclusive S^D control over current behavior demonstrate the need for more sophisticated analyses that include motivational variables that have behavior-altering effects.

Distinctions between the behavior-altering effects of motivational and discriminative properties of stimuli are also supported by scientific evidence. In their operant chamber examples, Whelan and D. Barnes Holmes (2010) suggest that any increase in behavior prior to reinforcer delivery is due entirely to the evocative effect of the experimental setting acting as an S^D, which makes putative evocative effects of the MO illusory. However, even with relatively constant stimulus conditions, the manipulation of an MO can change the current strength of an operant response class (Lotfizadeh et al. 2012a). The data from O'Reilly et al. (2006a, b, 2007a, b, 2008), and Skinner (1938, Figure 137, p. 388) are relevant here because in these studies the stimulus conditions were held relatively constant across manipulations of an MO, yet behavior changed as a function of the MO manipulation. As another example, Gutierrez et al. (2007) manipulated MOs for preferred items in four individuals with developmental disabilities and assessed the occurrence of mands for those items. When the individuals were given free access to one item (satiation) but were restricted from another (deprivation), they emitted more mands for the restricted item than for the free-access item (see also Gutierrez et al. 2010). In these studies, the discriminative stimulus conditions were similar, but an MO was manipulated. Behavior changed as a function of the MO manipulation with the S^Ds for the reinforcer essentially unchanged. The discriminative properties of the experimental setting alone cannot account for the observed changes in behavior. Therefore, logical analyses and available data contradict the notion that behavior-altering effects of MOs are (a) non-existent, or (b) entirely due to the behavior-altering effects of discriminative stimuli in the experimental context.

Interestingly, in an article published after the critique of the MOC by Whelan and D. Barnes-Holmes, Murphy and D. Barnes-Holmes (2010) wrote the following:

> Teaching an individual to emit appropriate mand responses *frequently involves motivating operations* (MOs; Laraway et al. 2003; Michael 1993c) to temporarily alter the reinforcing effects of stimuli. For example, arranging mild water deprivation may enhance motivation when teaching a child to mand for water. The current program of research on derived mands used a board-game format, and absent or surplus tokens on the board were used to *evoke* derived manding for more or fewer tokens. (p. 490; emphasis added)

> A board game procedure was used *to evoke manding* for specific amounts of tokens, similar to a *transitive conditioned motivating operation* (CMO-T; see Laraway et al. 2003; Michael 1993c). A motivating operation refers to the manipulation of setting events (e.g., deprivation to increase the effectiveness of a mand consequence) and is a procedure used frequently in mand training. A *CMO-T* involves procedures that manipulate indirectly acquired or conditioned reinforcers, such as the tokens employed in the current study. (p. 494; emphasis added)

> "To *evoke* derived manding, the test used a board game format similar to that employed for the practice trials . . ." (p. 496; emphasis added).

Although Whelan and D. Barnes-Holmes claimed that MOs do not have evocative effects, D. Barnes-Holmes repeatedly used the evocative effect to describe the changes in behavior that he and Murphy observed in their study. To us, the descriptions and the findings of Murphy and D. Barnes-Holmes provide additional support for our contention that MOs can have behavior-altering effects.

To summarize, the behavior-altering effect has been demonstrated empirically and is consistent with everyday and scientific observations of behavior. Organisms may engage in behaviors when a relevant MO is in effect even in the absence of reinforcer delivery or related discriminative stimuli, although the two types of variables may work together to influence the current strength of behavior. Moreover, the concept is necessary to describe MO-induced changes in behavior in escape and avoidance responding, in discrete-trial situations, in behavior chains, and in manding. Therefore, the concept is not "largely theoretical" and can contribute much to our analyses of operant behavior, including verbal behavior (e.g., manding for items that are not available), as demonstrated, for example, by the work of Murphy and D. Barnes-Holmes (2010). It is noteworthy that authors outside behavior analysis have recognized the importance of distinguishing between value-altering and behavior-altering effects. For example, in her review of the relevance of the MOC to appetite research, Tapper (2005) stated: "The distinction between value-altering and behavior-altering effects is important since it

means the MOs can account for findings in the appetite literature where simple operant conditioning processes cannot" (p. 97).

Empirical Problems: A Rebuttal of Claims for a Lack of Empirical Supporting Evidence

Whelan and D. Barnes-Holmes (2010) stated that: "Perhaps the most serious problem with the concept of MOs is that some aspects of the MO theoretical framework are not strongly rooted in a current and growing body of empirical data" (p. 342). First, our data show that the body of MOC literature is most certainly growing, and a substantial number of empirical articles have used the MOC (see Figs. 1 and 2, the Bibliography, and reviews by Carbone et al. 2010; Langthorne and McGill 2009; Simó-Pinatella et al. 2013). Second, apart from behavior-altering effects, which Whelan and D. Barnes-Holmes claim are "largely theoretical," it is unclear from their article what other "aspects" of the MOC they think do not have empirical support. Regardless, many behavior-analytic concepts could use additional empirical support, but it does not follow that we should jettison these concepts on this basis alone. To take this path would result in a greatly impoverished science.

The authors' claim that the MOC has little empirical support rests mainly on their judgment that "previous writings on the MO concept" have not discussed findings from a few areas in behavior analysis (i.e., behavioral economics, reinforcement hierarchies, response deprivation, the Premack principle, and UEOs). Apart from being a non sequiter, this is not true (see, for example, Klatt and Morris 2001; Tapper 2005). Moreover, it is not clear (a) how or why these areas were chosen as the baseline for judging the adequacy of the MOC, or (b) why they hold a superior position over the MOC. According to this kind of logic, one could just as easily critique writings in these areas for their failure to discuss the MOC (although we do not think this would be appropriate or profitable). Nevertheless, Whelan and D. Barnes-Holmes (2010) contradicted their own claim: "Klatt and Morris (2001) noted that *response deprivation*, which subsumes the Premack principle, could usefully be described as an establishing operation" (p. 345; emphasis in original). So, Whelan and D. Barnes-Holmes asserted that "previous writings on the MO concept have not incorporated research findings" from the response deprivation or Premack principle literature but then cited and discussed an article that explicitly did just that. Moreover, just because authors who have written on the MOC have not related it to certain arbitrarily selected areas within behavior analysis, it does not follow that the MOC has no scientific value and, therefore, should be rejected.

Because of the apparent lack of discussion of these few topics in previous writings on the MOC, Whelan and D. Barnes-Holmes (2010) hypothesized that "It might be argued, therefore, that the MO approach is too narrow and, thus, has mitigated against the wider application of motivational variables to a wider range of behaviors" (p. 341). The authors cite no evidence in support of this statement, nor do they provide any indication as to how such a mitigating effect of the MOC might be measured. We have identified dozens of articles that have used the MOC to describe, analyze, and change a wide range of behaviors in a variety of settings. From examining the breadth of topics covered in Table 1 and in the Bibliography, the claim that the "narrowness" of the MOC has somehow hampered the "wider application of motivational variables to a wider range of behaviors" is not supported by the evidence.

Alleged Problems with the CMO Subtypes

With respect to the MOC, Whelan and D. Barnes-Holmes (2010) stated: "The specific subdivisions of the CEO [CMO] have rarely been employed . . ." (p. 349) and "Although Michael argued that the MO approach serves to isolate the relevant controlling variables, it remains the case that both basic and applied researchers *have not used his concepts to do so*" (p. 350; emphasis added). These claims are contradicted by their own reference section as well as articles that they have published, which have used the MOC (e.g., Egan & D. Barnes-Holmes, 2009; 2010; Murphy et al. 2005; Murphy and Barnes-Holmes 2009; 2010; Whelan et al. 2006). Furthermore, the claim that researchers have not used Michael's concepts is contradicted by the peer-reviewed research literature published before 2010, the date of their CVO article. For example, a search (April 2013) of the website of the *Journal of Applied Behavior Analysis* (*JABA*; http://seab.envmed.rochester.edu/jaba/jabaindx.asp) conducted using the search terms "motivating operation," "establishing operation," or "abolishing operation" revealed 17 articles published *before 2010* that used at least one of these terms *in the title of the article*. A search (April 2013) using the term "establishing operation" revealed 63 articles published in *JABA* alone that have used the term within the last 20 years. The Laraway et al. (2003) article, which extended and clarified technical language for the MOC, was the 12th most downloaded article from the *JABA* website between April 3, 2008, and April 2, 2009, with 1,694 downloads (http://seab.envmed.rochester.edu/jaba/toc/stats/jaba25mostpop-2apr09.html). To date, we have found nearly 50 articles (including those published in *JABA* and by D. Barnes-Holmes) that have used Michael's CMO subtypes (see Bibliography). Despite the invalidity of their claim that researchers have not used the CMO subtypes, we will review and rebut the Whelan and D. Barnes-Holmes's specific claims regarding each CMO subtype and correct the authors inaccurate descriptions of them.

The Surrogate Conditioned Motivating Operation (CMO-S)

The arguments put forth by Whelan and D. Barnes-Holmes (2010) against the CMO-S are two-fold. They claim that the modifier *surrogate* is unacceptable because: (a) it fails to add

"explanatory precision," and (b) it fails to cover the many different ways in which a neutral stimulus may acquire the behavioral functions of an effective MO, such as through equivalence relations or relational framing. We will discuss these two points in turn, but first we take issue with the authors' example of the CMO-S. The authors wrote: "correlating a neutral stimulus [NS] with painful stimulation may increase the effectiveness of pain reduction as a form of reinforcement and evoke the behavior that has been reinforced with pain reduction" (p. 349). It is unclear why a pairing procedure by itself would influence the effectiveness of pain reduction in general. Regardless, researchers have shown that correlating a particular environmental context (an NS) with painful stimulation (electric shock) actually *decreases* the aversive value of shock when it is later presented in that same environment (i.e., "conditioned hypoalgesia"; Domjan 2005). Perhaps most importantly, this statement ignores a crucial feature – after the pairing procedure, the presentation of the former NS (the putative CMO) may have motivational effects as a result of its association with the original MO. In describing the development and functions of the CMO-S, we would not be concerned with the effectiveness of the original MO or operant consequence due to the pairing procedure, but rather with changes in the ability of the (formerly) NS to exert motivational effects on behavior at a future time. In short, the description of the CMO-S given by Whelan and D. Barnes-Holmes is inconsistent with published descriptions of this motivational variable (e.g., McGill 1999; Michael 1993c; Olson et al. 2001).

Whelan and D. Barnes-Holmes (2010) argued further that the term *surrogate* is merely a synonym for any generic *pairing procedure*, and, thus, does not provide additional "explanatory precision." In response, we note that *surrogate* describes the type of CMO that *can be produced by* pairing an NS with an effective MO. Thus, the term *surrogate* describes a type of controlling variable and the term *pairing* describes how that controlling variable might be produced (i.e., a relevant learning history). In other words, the authors confuse a conditioning procedure with the name of a type of controlling variable that results from that conditioning procedure. Therefore, our (and Michael's) descriptions of the CMO-S actually offer greater explanatory precision because the two terms (surrogate and pairing) identify two different events. The argument that the term *pairing* may not fully describe the diverse learning histories that could produce a CMO-S is reasonable because different methods may transfer the behavioral function from one stimulus to another (Roche et al. 2002). Regardless of what procedure by which the motivational function of one event transfers to another, the term *surrogate* would still be appropriate if the new MO substitutes for the original MO "by altering the effectiveness of the same consequence and evoking [or abating] the same behaviors" (Olson et al. 2001, p. 19). The modifier "surrogate" is consistent with the standard definition of this term: "a substitute" Surrogate [Def 1] (2013). The discovery of methods other than pairing to produce new MOs with similar behavioral functions as the associated MOs would not invalidate the CMO-S concept. Rather these methods would expand our description of the learning histories capable of producing CMO-Ss. Research on the extent to which surrogate CMOs can be developed using methods other than pairing would have practical and theoretical value, and we encourage such work.

The Reflexive Conditioned Motivating Operation (CMO-R)

In critiquing the CMO-R, Whelan and D. Barnes-Holmes (2010) return to the arguments of McDevitt and Fantino (1993). The criticism in question involves an example from Michael (1993c) describing a signaled shock-avoidance procedure. In this procedure, the warning stimulus, due to its relationship with the shock, acquires motivational properties such that its onset makes its offset effective as a reinforcer and evokes behavior that accomplishes its offset (e.g., lever pressing). Michael argued that in this situation the warning stimulus becomes a CEO-R (i.e., it becomes a conditioned aversive stimulus that evokes escape behavior). McDevitt and Fantino contended that the lever is an S^D for responding in the presence of the shock, and this compound stimulus (lever + shock) functions as an S^D in evoking the responses that terminate shock. According to their analysis, "neither stimulus alone – lever or shock – is sufficient to evoke lever pressing" (p. 227). They viewed this stimulus combination as having a discriminative function because lever *plus* shock is uniquely correlated with shock offset, whereas neither the lever nor the shock by themselves are so correlated. McDevitt and Fantino argued that the CEO-R added nothing to the analysis and that the S^D alone could account for the behavior. Nevertheless, their analysis is not relevant to the CEO-R because they never mentioned the role of the warning stimulus (the putative CEO-R) in controlling escape behavior after the warning stimulus had been paired with the shock.

Whelan and D. Barnes-Holmes (2010) concluded that the compound stimulus analysis put forward by McDevitt and Fantino (1993) is preferable over the CEO-R analysis "on the grounds of parsimony" (p. 350). However, the alternative analysis offered by Whelan and D. Barnes-Holmes does not agree with that of McDevitt and Fantino. Whelan and D. Barnes-Holmes state: "In this case the S^D *and the CVO concepts* are sufficient to cover this eventuality, and on the grounds of parsimony, are preferable over the extra concept of the reflexive CEO" (p. 350; emphasis added). Remember that McDevitt and Fantino argued that *only the* S^D *was necessary* to explain the escape behavior in this example and that a motivational term (in this case, the CEO-R) was unnecessary. So, after arguing that we only need one term to explain escape behavior (the S^D), Whelan and D. Barnes-Holmes contend that we really need two terms (the S^D and the CVO). It is unclear how S^D *plus the CEO-R* is less "parsimonious" than S^D *plus the CVO* in that both formulations postulate the joint action of a discriminative stimulus and a motivational variable. Contrary to the opinion of Whelan and D.

Barnes-Holmes, we agree with Carbone et al. (2010) who noted: "With knowledge of the concept of the CMO-R, practitioners may be better equipped to evaluate, select, and implement instructional methods that reduce escape and avoidance behavior exhibited by a large percentage of children with autism and related disabilities" (p. 120).

The Transitive Conditioned Motivating Operation (CMO-T)

In arguing against the CMO-T, Whelan and D. Barnes-Holmes's (2010) stated that:

> However, almost all antecedent stimuli act on other events rather than altering their own function. If, for example, a neutral stimulus correlated with painful stimulation increases the effectiveness of pain reduction as a form of reinforcement and evokes the behavior that has been reinforced with pain reduction, then this can be considered both an surrogate and a transitive CEO (p. 351).

Apart from repeating their incorrect description of the CMO-S that we have already discussed, this statement does not capture the difference between the CMO-S and the CMO-T. The key feature is not that they both "act on other events," it is that they have distinguishable and unique functions with respect to other events. To repeat, the CMO-S has the same motivational effects as the original effective MO with which it was associated (i.e., it *substitutes* for the original MO). The CMO-T does not have this feature. As we mentioned above, the most frequently described CMO-T is one that increases the value of a conditioned reinforcer and is "often a stimulus that makes some object or event necessary to complete a behavior chain for a reinforcer, causing the missing object to increase in value" (Olson et al. 2001, p. 25). This scenario does not resemble the description of the CMO-S given by Michael (1993c), and it should be clear that the CMO-T and CMO-S function differently. For other examples of these CMO subtypes, see Langthorne and McGill (2009), Michael (2007), McGill (1999), and Olson et al. (2001).

Many of the arguments of Whelan and D. Barnes-Holmes (2010) against CMO subtypes miss the mark because they do not criticize actual published definitions of the concepts. Although they have used the CMO subtypes in their own work (e.g., Murphy and D. Barnes-Holmes 2010), without noting any problems in doing so, they contend that behavior analysts should reject these subtypes for a variety of unconvincing reasons. In contrast to these authors, we contend that the three different CMO subtypes are different from one another, well worth distinguishing, and helpful to understand motivation. Carbone et al. (2010) clearly agree:

> Conditioned MOs have unique histories related to an individual's ontogeny. In other words, the histories that have led to the development of the many unconditioned and conditioned MOs are remarkably different. Moreover, the mechanisms that account for their effects are all different. Consequently, practitioner efforts to abolish the effects and abate behavior related to any of the unconditioned or conditioned MOs would require substantially different environmental manipulations specific to each type of motivating operation. As a result, Michael (1993a; 2007) provided specific labels for each MO as a way of acknowledging the different histories that have led to their control over behavior. Moreover, he identified different forms of unpairing that can be used to decrease behavior evoked by conditioned MOs. Practitioners who are aware of these differences will certainly be more effective in controlling behavior than those who are unaware. (p. 115)

Consider further this statement by O'Reilly et al. (2007b): "Michael (1982; 1993b; 2000) has provided the behavioral community with a rich description of the possible functional properties of antecedent control." As these quotes illustrate, we are not alone in our view that the MOC provides compelling and useful analyses of behavior (see Langthorne and McGill 2009; McGill 1999; Tapper 2005; Simó-Pinatella et al. 2013; Wilder and Carr 1998). In summary, the critiques leveled against the CMO subtypes by Whelan and D. Barnes-Holmes include inaccurate descriptions of the subtypes, contain logical errors, and lack supporting evidence. Given the successful use of the MOC, including the CMO subtypes, in the scientific literature, (see the References and Bibliography), we argue that these subtypes are useful and should not be abandoned until empirical and theoretical work yields better concepts to replace them.

The Consequence Valuing Operation and Related Terms: A Critique

Preliminary Remarks

We will now scrutinize Whelan and D. Barnes-Holmes's (2010) proposed alternative to the MOC, the *Consequence Valuing Operation* (*CVO*), and related terms. The first point to consider is whether an alternative model of motivation is needed, as they contend: "Given the foregoing criticisms of Michael's treatment of motivation, we have sought to develop an alternative treatment of motivation that derives from Skinner's early work . . ." (p. 342). As we have shown, their criticisms of the MOC are not convincing for a variety of reasons, so we question the necessity of the authors' "alternative treatment of motivation." A second point to consider is that the MOC has been adopted and expanded upon by many

behavior analysts since 1982. In addition, the MOC appears in several handbooks and textbooks, in the curricula for training programs in behavior analysis, and in the Behavior Analysis Certification Board Task List (http://www.bacb.com/). It, therefore, appears that behavior analysts have found Michael's treatment of motivation useful. However, the scientific enterprise welcomes empirical competition among theories. Therefore, in the following sections, we review and consider whether the CVO offers a more useful and parsimonious treatment of motivated operant behavior.

In the quote in the last paragraph, the authors imply that their concept is more consistent with Skinner's analysis of motivation than is the MOC. Although we do not see consistency with Skinner's views as a *sine qua non* for a useful concept in behavior analysis or any other field (this would be a dogmatic rather than an empirical approach), Skinner's writing does not support the implication that the CVO is consistent with his views on motivation. For example, Whelan and D. Barnes-Holmes suggested that the behavior-altering (e.g., evocative) effect is unnecessary for a thorough behavioral analysis of motivation and claimed to exclude the effect from their definition of the CVO. Yet, Skinner repeatedly emphasized the importance of the behavior-altering effects of deprivation, satiation, and aversive stimulation, which are consistent with the MOC. Consider these statements by Skinner (1953; see Sundberg 2004):

> . . . the probability of drinking becomes very high under severe water deprivation and very low under excessive satiation . . . when the organism is deprived of the opportunity to drink – it is obviously important that drinking should be more likely to occur at the *first opportunity* . . . A given act of deprivation usually increases the strength of many kinds of behavior simultaneously. (pp. 141–143; emphasis added)

As another example, Skinner (1957) wrote:

> By reinforcing with candy we strengthen the response *Candy*! but the response will be emitted only when the child is, as we say, hungry for candy. Subsequently, we control the response, *not by further reinforcement*, *but by depriving or satiating the child with candy.* (p. 31; emphasis added)

Note that in the first quote Skinner is obviously referring to the initial occurrence of a behavior as a result of deprivation, and in the second quote he explicitly states that reinforcement is not the controlling variable at the moment the behavior occurs; rather, it is deprivation/satiation that produces the response. Thus, he is clearly referring to a behavior-altering effect of a motivational variable. Egan and D. Barnes-Holmes (2010) noted this: "*Skinner defined mand as a verbal response evoked by conditions of deprivation or aversive stimulation* in which the response is followed by a characteristics consequence" (p. 473; emphasis added).

Skinner (1953) provided many other examples of the evocative effect of deprivation in the section *The Practical Use of Drives*, starting on p. 146. For further discussion of the evocative effect in Skinner's writings, see Cherpas (1993) and Sundberg (2004). It should be clear from these (and other) quotes that Skinner emphasized the importance of the evocative effects of motivational variables in the control of behavior. Given that Whelan and D. Barnes-Holmes (2010) explicitly argued *against the evocative effect* (particularly in the section on the "dual nature" of the MOC), it is not accurate for them to claim that their motivational concept "derives from Skinner's early work" while implying that the MOC does not do so. We feel that the MOC is much more consistent with Skinner's approach to motivational variables and their effects.

The Proposed Alternative Motivational Concept and Terms to Describe It

Following their introductory section, Whelan and D. Barnes-Holmes (2010) describe their motivational concept, the *CVO*, and its behavioral effect, the *Consequence Valuing Processes* or *CVP*. First, they describe their concept as an example of the motivational variable demonstrated by Skinner (1938, p. 393, Fig. 1) that refers to the "effectiveness, or apparent value, of the consequential stimuli that participate in an operant contingency" (p. 342). In this example, Skinner fed rats varying amounts of food (0, 2, 4, and 6 g) prior to placing them under an intermittent schedule of reinforcement. He then obtained cumulative records of lever pressing. According to Skinner these data demonstrate "the interaction of drive and conditioning" (p. 391). In other words, the example used by Whelan and D. Barnes-Holmes shows the combined effects of the manipulation of a motivational variable ("drive") and the resulting changes in the effectiveness of food delivery as a reinforcer. Whelan and D. Barnes-Holmes used these data to support their CVO concept, naming the manipulation of the various levels of food deprivation a *CVO* and the subsequent effects on behavior a *CVP*.

They then defined CVOs as:

> . . . contextual arrangements made by a researcher or practitioner that are designed to either increase or decrease the value of a consequence in an operant contingency without changing or altering that contingency, or by demonstrating some interaction between the factors of consequence value and contingency relations. (pp. 342–343).

We disagree with the authors' notion that motivational variables necessarily entail manipulations by researchers or

practitioners. This is far too narrow – clearly, motivational variables in the natural environment occur without deliberate human intervention, even with many human behaviors (e.g., consider *Norovirus* and the resulting gastroenteritis as an AO for food, sex, exercise, and many other reinforcers).

There are additional problems with the CVO as proposed by Whelan and D. Barnes-Holmes (2010) because they explicitly rejected the behavior-altering effect and defined the CVO solely in terms of the changes in the effectiveness of reinforcers to strengthen or maintain behavior. Based on their definition, the CVO concept can only describe free-operant situations in which repetitions of a response can produce repeated presentations of a positive reinforcer and symmetrically cannot describe or explain any other complex motivational relations. For example, it cannot describe discrete-trial or chaining situations in which only the first occurrence of the behavior is of interest, such as in manding for an item and receiving that item. The CVO concept also cannot describe situations involving aversive stimuli as reinforcers or punishers or in which motivational variables are manipulated when reinforcement is not occurring (i.e., in extinction). The authors also contradict their own position with respect to behavior-altering effects when they stated: "Similarly, a CVP has occurred *if the response rate in the presence of the* S^{Δ} *is increased relative to the response rate in the presence of* S^{Δ} *in the baseline condition* (i.e., greater than *Y*)" (p. 345; emphasis added). By definition, the S^{Δ} signals extinction so no reinforcer is being delivered when it is present. Based on their own position, the term CVP cannot apply to responding under extinction because reinforcers are not being delivered and the authors have argued that there is no evocative effect. As stated previously, the MOC conceptualizes such changes in terms of the behavior-altering effect because no consequences are being delivered, and we discussed that manipulating motivational variables in extinction is an ideal method for assessing the behavior-altering effect (e.g., Klatt and Morris 2001).

The MOC encompasses reinforcement and punishment with appetitive and aversive stimuli in many behavioral situations, whereas the CVO appears to deal only with appetitive reinforcement in the context of free-operant responding. Thus, Whelan and D. Barnes-Holmes's (2010) alternative to the MOC does not account for a wide range of behavioral consequences and their motivational variables, thereby preventing a thorough description of operant behavior. To illustrate, previous writings on the MOC have described the importance of motivational variables in determining the effectiveness of aversive stimuli (e.g., Iwata et al. 2000; Laraway et al. 2003; McGill 1999; Michael 1988, 1993c; Olson et al. 2001; Poling 2001; Smith and Iwata 1997; Wilder and Carr 1998). For example, Poling (2001) described the "antipunishment" effects of certain GABA-ergic drugs, such as diazepam or alcohol (see Commissaris 1993; Rasmussen 2006). Because these drugs reduce the capacity of aversive stimuli to suppress behavior, a person using the MOC would call them *AOs for punishment* and the increase in behavior previously suppressed by punisher delivery an *evocative effect*. (Note that these examples provide additional evidence for the behavior-altering effects of MOs). As another example, variables that make time-out more or less effective as a punisher would be considered EOs and AOs, respectively, as Laraway et al. (2003) suggested.

Although Whelan and D. Barnes-Holmes (2010) claimed to only introduce two new concepts, the CVO and CVP (e.g., p. 348), this is not so. For example, on p. 346 they introduce the *consequence-devaluing operation* (CDO) and its effect, the *consequence-devaluing process* (CDP). The CDO is equivalent to the AO for reinforcers and the CDP is similar to the reinforcer-abolishing effect. The authors describe the satiation conditions from Vollmer and Iwata (1991) and the neutralizing routines from Horner et al. (1997) as examples of CDOs and the subsequent decreases in behavior as examples of the CDP. It should be noted that Vollmer and Iwata and Horner et al., both used the EO concept to describe their results. These are but two examples of Whelan and D. Barnes-Holmes simply replacing MOC terms used in published articles with their own terms. To us, this kind of replacement strategy is unpersuasive.

Later, Whelan and D. Barnes-Holmes (2010) stated: "In the above examples, the concepts of CVO and CVP can be readily applied as terms that serve to describe *the increase and decrease* in the relevant behaviors, independent of other contingencies" (p. 346; emphasis added). Thus, these two terms can apparently refer to variables that *increase or decrease* the value of consequences. The authors did not include *CDO* or *CDP* as "key" terms in their scheme, as they explicitly and repeatedly stated that they are introducing only two key terms (e.g., in their abstract, on pp. 342 and 351). Thus, it appears that the *devaluing* terms are superfluous, as well as redundant with CVO and CVP. If the CVO refers to variables that can *increase or decrease* the effectiveness of consequences, then why introduce the *devaluing operations* term at all? This quote is also confusing because the CVO is supposed to refer to an operation (i.e., an environmental manipulation) that produces changes in behavior, a process termed the *CVP*. The authors appear to suggest the interchangeable use of an operation term (CVO) and a process term (CVP) to describe an observed change in behavior, after stressing the importance of maintaining the distinction between operations and processes. Additionally, using two different terms (CVO and CVP) to refer to the same event (a change in behavior) is confusing and introduces ambiguity into their motivational concept.

Whelan and D. Barnes-Holmes (2010) further stated that:

> In our view therefore, terms such as *learned, unlearned, conditioned, unconditioned, first-order, second-order,*

> and so on can all be employed as modifiers for CVOs, in the same way that we apply these modifiers to antecedent stimuli that are correlated with the differential availability of reinforcement. (p. 351).

So, after arguing that the MOC should be rejected by behavior analysts because ". . . the modifiers *unconditioned* and *conditioned* are not appropriate for the term establishing operation" (p. 340), the authors later state that these same words are appropriate modifiers for the CVO, which is, after all, an *operation like the MO*. The authors do not explain why these terms can adequately modify CVOs but not do the same for MOs. If one accepts the authors' argument (borrowed from Catania 1993) that the terms *unconditioned* and *conditioned* cannot appropriately modify behavioral operations, then we must apply the same logic to the CVO and reject the concept because it conflicts with traditional behavior-analytic practice. Whelan and D. Barnes-Holmes's proposed scheme departs from traditional terminological practice because the authors suggest that we can apply these various modifiers to CVOs "in the same way that we apply these modifiers to *antecedent stimuli that are correlated with the differential availability of reinforcement* [i.e., discriminative stimuli for reinforcers]." In response to this, we are not aware of any published work in which these terms are used as modifiers for S^Ds for reinforcers, which are defined as stimuli that signal the "differential availability of operant consequences." Even more perplexing is the authors' suggestion that we can describe S^Ds as *unlearned*. Given that these stimuli signal the availability of consequences in the context of operant conditioning, they must have acquired their function as a result of learning. Therefore, the term *unlearned* cannot accurately modify or describe S^Ds.

The terminological implications of the quote above from p. 351 also undermines one of Whelan and D. Barnes-Holmes main theses, which is that the MOC is more ambiguous and less parsimonious than is the CVO concept. It is not helpful to propose that behavior analysts use these six terms as modifiers for CVOs with no indication as to how and when to use them. Readers are given no system for distinguishing the various types of CVOs, nor are these terms described in any detail. If these terms identify separate controlling variables and/or the relevant behavioral histories required to produce them, then these variables should be described in detail so we can distinguish among them and test them experimentally. The ambiguity of the CVO in this regard is contrasted with the thoroughgoing definitions provided for every concept in the MOC scheme.

Let us now consider parsimony. If carried out to its logical end point, the scheme proposed by Whelan and D. Barnes-Holmes (2010) results in a similar number of technological terms as the MOC. Namely, they proposed the terms: (a) Consequence-Valuing Operations (CVOs), (b) Consequence-Devaluing Operations (CDOs), (c) Consequence-Valuing Processes (CVPs), (d) Consequence-Devaluing Processes (CDPs), (e) Unlearned CVOs, (f) Unconditioned CVOs, (g) Learned CVOs, (h) Conditioned CVOs, (i) First-order CVOs, and (j) Second-order CVOs (p. 351). We would have even more terms if we applied the last six modifiers to the CDO and CDP. Therefore, Whelan and D. Barnes-Holmes introduce as many terms (~10) to describe their concept as the authors they have criticized for terminological fecundity (Michael 1993c; Laraway et al. 2003). The authors' claim that the CVO concept is "more parsimonious" and represents "a straightforward explanatory framework for describing motivated behavior" because it only includes two terms is not tenable. Either the CVO concept only uses two terms and these other terms are superfluous (thereby introducing ambiguity, redundancy, and terminological confusion) or the CVO uses several terms that are not given proper definitions (thereby introducing ambiguity and weakening any claim to parsimony relative to the MOC). In conclusion, the terminological system for the CVO is ambiguous, confusing, and imprecise, and it provides no benefits over the MOC.

Conclusion

In this paper, we first provided a current description of the state of the MOC in research and practice. The literature and our analyses suggest that the MOC has had a significant and escalating impact on research over the past 30 years that is leading to innovations in behavioral interventions. Next we reviewed and responded to Whelan and D. Barnes-Holmes (2010) critique of the MOC and their proposed competing motivational concept, the CVO. Their criticisms of the MOC are based on an unsystematic, selective, and shallow review of the MOC literature. Their alternative motivational concept suffers from significant logical problems and is less precise and useful than is the MOC. Given the ubiquity of the MOC in behavior analysts' repertoires and its substantial and growing empirical base, we can see no reason to prefer Whelan and D. Barnes-Holmes' concept over the more established, and better researched, MOC. Contrary to their many and varied arguments, the CVO offers no advantages over the MOC, and, in fact, possesses many disadvantages.

Like Whelan and D. Barnes-Holmes (2010), we believe that motivational variables are important and we endorse their call for a "precise and coherent conceptual analysis" (p. 338) of such variables. We have adopted the MOC for our analyses of such variables because, in our opinion, the MOC currently provides the most effective account of motivation from a behavior-analytic perspective. Based on the available evidence, we feel that the MOC: (a) is useful for describing, predicting, and changing behavior; and (b) is precise,

coherent, and parsimonious. As an empirical and testable concept, the MOC should be investigated, critiqued, and modified, as necessary, so we can gain the most complete understanding of behavior as a natural phenomenon as possible. There is important work to be done; gaps exist in the theoretical structure of the MOC and in empirical support for hypothesized functional relations it suggests (particularly with respect to aversive control). To assist this work, we have provided a comprehensive Bibliography of publications that have cited Laraway et al. (2003). Although there are many other publications that have used the MOC without citing this article, this list gives researchers and scholars a starting point for their search for relevant literature.

As members of the scientific community strive to make new discoveries in this area, we and other authors urge scholars to use the MOC terms more consistently (Simó-Pinatella et al. 2013), as this would aid the development of the concept. In our view, the accelerating growth of the MOC literature over the past 30 years is an exciting scientific development, as it represents new and varied applications of the concept to an ever-widening range of behavior and situations. We look forward to the further development of the MOC and its incorporation into new research areas, including those outside of behavior analysis (e.g., Tapper 2005), and we hope that this paper contributes to productive scientific work on motivated behavior.

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