

Advances, Problems, and Challenges in the Study of Emotion Regulation: A Commentary

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Published online: 10 November 2009
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Abstract As observed by Zinbarg and Mineka (*American Psychologist* 62:259–261, 2007), the study of emotion regulation faces significant challenges that need to be addressed. In reviewing the articles in the present issue, we comment upon how researchers have attempted, with varying degrees of success, to meet these challenges. We suggest that the articles in the present volume demonstrate important conceptual and methodological advances in emotion regulation research, but that the most central problems noted by Zinbarg and Mineka have yet to be fully remedied. In highlighting both the progress made and problems faced by the field of emotion regulation, we suggest new directions for future research in this area.

Keywords Emotion regulation · Emotional reactivity

In a recent issue of the *American Psychologist*, Olatunji et al. (2007) suggested that the concept of emotion regulation might enhance current contemporary learning theory perspectives on the etiology and maintenance of anxiety disorders. Within the same issue, Zinbarg and Mineka (2007) responded by calling for a clearer definition of emotion regulation, a better distinction between emotion activation/reactivity and emotion regulatory processes, and more rigorous experimental research suggesting that the concept of emotion regulation provides incremental utility above and beyond learning theory models of emotional

disorders (Mineka and Zinbarg 2006). Since that time, emotion regulation researchers have attempted to meet these challenges with varying degrees of success. Examining relevant behaviors and concepts in children and adolescents, the articles in the present volume represent important conceptual and methodological advances in the field of emotion regulation. Namely, they recognize the importance of studying the regulation of emotion in a development context, and use more sophisticated measures of emotion and regulatory strategies. Nonetheless, it is clear that this area of research still faces significant challenges that should be addressed in future studies.

Advances in the Study of Emotion Regulation

Recognizing the Importance of a Developmental Context

An important contribution made by several papers in this volume is a recognition of the usefulness of developmental models (Carthy et al. 2009; Morris et al. 2009; Sulik et al. 2009; Suveg et al. 2009; Waters et al. 2009) and longitudinal data (Cisler et al. 2009; Morris et al. 2009) for testing claims regarding the causal nature of emotion regulation variables in psychopathology. Too often, theoretical discussions of the role of putative emotion regulation deficits in the etiology of various disorders have inferred that emotion regulation is implicated in psychopathology because individuals with various disorders are often characterized by salient affective symptoms. Importantly, longitudinal designs allow for sharper distinctions between factors that are epiphenomena of a disorder and those that are distinct from, but causally related to, the development of the disorder. Studies of child and adolescent samples have the additional advantage of allowing for measurement

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of emotion regulation constructs before the onset of frank disorders (ideally) or, less ideally, earlier in the course of those disorders. Moreover, research in basic developmental science has clearly established that the underlying neuro-cognitive mechanisms necessary for the regulation of behavior in general, and emotion-related behavior in particular, undergo significant maturation over developmental time (e.g., Kochanska et al. 2000; Rothbart and Bates 1998). In addition, as well exemplified in the paper by Sulik et al., there is also considerable evidence for individual differences in these factors in both childhood and adolescence (Rothbart et al. 2001). Thus, developmental periods characterized by both intra- and interindividual variability and change in behavioral regulation/executive functioning processes (such as childhood, but probably also late adulthood) provide important windows for mapping causal relationships between regulation and psychopathology indices.

It will be important for future empirical studies of emotion regulation constructs to more actively consider the role of developmental processes in regulation as they relate to the disorder(s) of interest. For example, although many theorists of emotional development (e.g., Campos et al. 2004) argue that the reactive and regulatory components of emotion may be impossible to separate, if such discrimination were possible, developmental periods characterized by immature regulation abilities (such as early childhood) may present opportunities to more cleanly tap individual differences in emotional reactions that are less subjected to regulation processes.

Several studies in this issue (and in the larger literature on emotion regulation) focus primarily on describing the correlates of individuals' dispositional use of particular putative emotion regulation strategies, such as distraction (e.g., Suveg et al. 2009; Rydell et al. 2003). It is worth noting that the regulation constructs typically explored in studies of adults (such as reappraisal) also likely have a developmental trajectory, such that the use of particular strategies is constrained by developmental abilities. For example, while infants are capable of simple strategies such as attention redirection, toddlers and older children have access to a broader range of possible emotion regulation skills, owing to their greater motor and cognitive development (Mangelsdorf et al. 1995). If emotion regulation variables represent a diathesis for emotional disorders, then they would be expected to have some degree of temporal stability. Empirical data are lacking regarding the stability of emotion regulation variables, their developmental specificity, and mean level changes across development. It will be important to understand the developmental precursors of these more mature emotion regulation mechanisms. Although the articles in this volume are significant in that they recognize the importance of examining emotion regulation

constructs in a developmental context, further research is clearly needed.

Utilizing Observational and Experience Sampling Measures

Importantly, the articles in this volume also demonstrate significant strides in the measurement of emotion and emotion regulation strategies. Specifically, the use of observational and experience sampling measures in these articles represents a commendable and necessary step forward in investigations of regulatory behaviors. As aptly noted by Morris et al. (2009), much of the previous research in this area has measured emotionality and emotion regulation strategies using self- or other-report measures and this has been particularly true in studies involving children (Rothbart and Bates 2006). Although the accuracy of such self measures has not been extensively studied within the emotion regulation literature, research on another subtype of affect regulation (Gross et al. 2006), namely coping, suggests that self reports of affect regulation strategy utilization are frequently quite inaccurate (Ptacek et al. 1994; Stone et al. 1998).

When reporting upon their experiences and behaviors over longer intervals, individuals often rely on heuristics, basing their reports on knowledge about the effects or conclusions of their experiences (Brewer 1994; Brown and Harris 1978) and on potentially biased beliefs about how they generally behave (Bradburn et al. 1987). In fact, Stone et al. (1998) found that even retrospective reports of coping strategies made within 48 hours of a stressful event of interest were only moderately correlated with the use of such strategies as reported using an experience sampling method.

Additionally, there is reason to believe that the accuracy of self-reports of affect regulation strategies is related to the degree of affect that an individual experiences. Smith et al. (1999) found that, among individuals preparing for an exam, individuals who reported experiencing a greater degree of exam-related stress on a daily measure exhibited a greater tendency to systematically overestimate the degree to which they used affect regulation strategies on retrospective reports when daily and retrospective reports of strategy use were compared. Thus, using conventional self-report measures may be particularly problematic for studies attempting to distinguish emotional reactivity from emotional regulation (see below for discussion of the distinction between emotional reactivity and emotional regulation).

Given that many of the biases believed to contribute to the inaccuracy of self reports may also come into play when retrospectively evaluating the experiences and behavior of others, studies of emotion regulation should not rely solely on conventional peer- or parent-report measures of emotionality and regulatory strategies to supplement self-reports. In this respect, the studies presented in this volume

exhibit an improvement over much previous emotion regulation research. While many of these studies still utilized self- and parent-report measures, a number of them also employed observational and experiencing-sampling measures with interesting results.

For example, Suveg et al. (2009) assessed emotion intensity using both a conventional self-report measure and an experience sampling method to study the relationship between emotion intensity and parent- and youth-reports of emotion regulation. Supporting the notion that self-report measures are often inaccurate representations of individuals' actual experiences, self-reported intensity of positive emotions was not significantly correlated with an electronic diary measure of this variable within this study, and self-reported intensity of negative emotions showed only a moderate correlation with a diary measure of this variable. Further, while no significant findings were obtained using the conventional self-report measure of emotion intensity as a predictor, Suveg et al. found that their experience sampling measure of intensity of positive emotion was positively associated with child-reported emotion regulation difficulties and negatively associated with parent reports of child emotion regulation. This suggests that, despite the poor rates of diary completion within this study, the electronic diary measure employed captured meaningful variance in emotional intensity not captured by the self-report measure.

Likewise, Waters et al. (2009) used observer ratings of children's emotional states to supplement their comparison of mother and child reports of children's emotion in a particular situation, finding that observer reports were more concordant with child reports than were maternal reports. Given that even momentary self-reports made by four- to five-year-old children are, in and of themselves, potentially inaccurate, future studies of parental accuracy in assessing their children's emotions might extend this paradigm by examining the discrepancies between maternal reports and experimenter-coded observations of children's emotions.

Admirably, Morris et al. (2009) used observer ratings of both child anger reactivity and anger regulation strategies in assessing the relations between reactivity, regulation, and externalizing behavior. Morris et al. concluded that their study supported the claim that emotion regulation is an important predictor of the development of externalizing problems. However, as they report in their results section, their study actually showed that when entered simultaneously as predictors, observed anger reactivity was a significant predictor of concurrent and future externalizing problems while observed use of anger regulation strategies was not. In our opinion, the results of this study stand in contrast to previous research findings (reviewed extensively by Morris et al.) suggesting that externalizing problems are predicted by self-report measures of emotion regulation.

Hence, in all three of the studies described above, the use of experience sampling and observational measures yielded findings about emotion regulation that likely would not have been detected had only conventional self- and other-report measures been used. We strongly recommend that future research emulate, and even improve upon, these studies in using more observational and experiencing sampling measures when examining the relationship between emotion regulation and psychopathology in youth.

Notably, the research reported by Sulik et al. (2009) in this volume provides an exemplary model of how future emotion regulation research should utilize observational measures. Not only did these investigators employ observational as opposed to self-report measures to assess effortful control, but they also modeled effortful control as a latent variable constructed on the basis of several measures believed to assess this construct. Further, they tested and empirically validated the invariance of a measurement model of effortful control for individuals of different genders and ethnicities. This is an important first step in evaluating whether the construct validity of this construct is equivalent across populations.

Continuing to apply methodological strategies like utilizing multiple measures and establishing the invariance of measurement models formed using multiple measures will be important for future emotion regulation research. Because it is unlikely that a single observation or observational measure can adequately capture forms of emotionality or emotion regulation, such as trait anxiety or a tendency to engage in avoidance, multiple measures should be used to comprehensively assess constructs of interest and correct for unreliability in measurement.

Ongoing Challenges in the Study of Emotion Regulation

Improving Statistical Analysis and Interpretation

Although incorporating sound measures of constructs of interest is important, even the most rigorously designed studies can yield misleading conclusions if the data collected is not analyzed appropriately. As regards the statistical analyses reported in the articles in this special section/issue, we suggest alternative strategies that would enhance or complement those reported in some cases.

For example, two of the studies incorporated designs that would have permitted sequential analyses related to dynamic questions such as whether (a) the mother's validation and acceptance were followed by the child's being less likely to be avoidant or vice versa, or both (Waters et al. 2009) and (b) attention refocusing was followed by less anger or whether anger was less likely to be followed by attention refocusing, or both (Morris et al.

2009). Unfortunately, both of these studies instead used data aggregated over assessment waves and simply computed a static, cross-sectional correlation. In fact, in the only study in the literature to use sequential analyses to test whether children's use of putative regulation strategies actually resulted in significant changes in their degree of observed anger or fear (Buss and Goldsmith 1998), there was limited support for the notion that the regulatory behaviors examined did in fact change the intensity of either emotion. Rather, findings were more consistent with the argument that these "regulatory" behaviors marked greater intensity of the emotional response, and therefore are perhaps best viewed as part of the emotion itself. Other studies in the special section utilized analysis of covariance/partial variance without incorporating any strategies that might help to minimize the well-known biases that are inherent in such analyses when the covariate is not perfectly reliable (e.g., Huitema 1980; Kahneman 1965; Kenny 1979; Maxwell and Delaney 2004). Examples of some of these strategies include using multiple measures that are heterogeneous with respect to their sources of error and then either forming more reliable and valid aggregates or analyzing the data using structural equation modeling, adjusting for the degree of (a) bias in estimates of the unique effect of emotion regulation and (b) type I error rate inflation arising from unreliability of measurement of the covariate, or conducting a sensitivity analysis to determine how sensitive one's results are to unreliability in the covariate and different assumptions regarding the true strength of association between the latent covariate variable and the latent dependent variable (Zinbarg et al. 2009).

Finally, we believe that a few the studies in this special section drew premature conclusions based on the statistical analyses that they and previous researchers conducted. For example, though we applaud Morris et al. (2009) for conducting a longitudinal study, we also note that their design omitted a time 1 measure of externalizing behavior. Unfortunately, in the absence of this time 1 measure, the information that can be gleaned from a longitudinal study is greatly limited. The reason for this is that in the absence of a time 1 measure of externalizing, it is impossible to rule out the possibility that the association between time 1 anger and time 2 externalizing is entirely due to a cross-sectional association between time 1 anger and time 1 externalizing combined with temporal stability of externalizing.

Likewise, each of the studies cited by Cisler et al. (2009) as showing that emotion regulation interacted with various other variables failed to account for the fact the emotion regulation variable correlated with the variable it was purported to moderate. Each of these interactions could therefore actually be due to quadratic effects of the emotion regulation variables or to the quadratic effects of the variables purported to be moderated by emotion regulation

(Aiken and West 1991, pp. 92 – 93). That is, when an emotion regulation variable is correlated with a second, putative moderator variable, the product of those two variables will be correlated with the emotion regulation variable squared (i.e., the emotion regulation variable times itself). Thus, it might be that the product of the emotion regulation times the putative moderator variable is acting as a proxy for the effect of the emotion regulation variable squared. In the future, such studies need to include these quadratic effects of the emotion regulation variable in their statistical models to ensure that the interaction terms make unique contributions above and beyond the quadratic effects.

Defining and Distinguishing Emotion Regulation and Reactivity

However, perhaps the most serious problem with the studies in this issue is not statistical, but conceptual. Admirably, these studies showed an increased recognition of the need to define emotion regulation and to distinguish it from emotion reactivity. Most provide clear definitions of what they mean by emotion regulation and explicit arguments as to why it might be related to psychopathology rather than just assuming this to be the case. The consensus that these definitions converge on is that emotional reactivity refers to the initial strength of emotional activation to a stimulus (either internal or external) whereas emotional regulation refers to the processes that modulate emotional responses (e.g. Carthy et al.; Cisler et al.; Morris et al.). Further, many of these arguments attempt to draw a conceptual distinction between emotion reactivity and emotion regulation (Carthy et al.; Cisler et al.; Morris et al.). Nonetheless, we find the arguments offered to be somewhat lacking, and suggest that researchers have often failed to distinguish between regulation deficits and excessive reactivity at a methodological level.

Establishing Whether Emotion Regulation is a Distinct Construct Of the articles in this volume, the review article by Cisler et al. (2009) provides the most detailed argument that emotion reactivity and regulation are distinct constructs. While this represents an admirable initial effort to differentiate regulation from reactivity, we assert that it does not provide a sufficient answer to this challenge and must be improved upon if psychologists are to continue to discuss emotion regulation as if it were a construct. To further clarify this assertion, we explain the reasons why we still continue to disagree.

Within this article, Cisler et al., (2009) use definitions of emotion regulation that we believe are contradictory, describing it as both a latent variable that explains observed

behaviors and as the same observed behaviors that it is theorized to explain. Namely, they propose that emotion regulation is one of the determinants of residual variance in lower order indicators of a hierarchical fear construct, with the lower order indicators of fear including behavioral, physiological, and verbal-cognitive tendencies. At the same time, they define emotion regulation as “a heterogeneous set of actions that are designed to influence which emotions we have, when we have them, and how we express them.” They further note that this set of actions can include behaviors they had previously been described as behavioral indicators of anxiety.

From our perspective, this presents a conceptual problem in that emotion regulation cannot be both a heterogeneous set of actions, and something that explains variance in these very actions that is not explained by a higher order fear construct. Likewise, two distinct constructs cannot have precisely the same indicators. That is to say that anxiety regulation cannot be viewed as a latent variable distinct from fear and anxiety if the only indicators of anxiety regulation are also indicators of anxiety.

In advancing the argument that emotion regulation is a distinct latent variable with several lower-order indicators, Cisler et al.'s (2009) main line of evidence appears to be that anxiety has been associated with neural activity in the amygdala while attempts to deliberately regulate emotion have been associated with activity in the prefrontal cortex (PFC). We are unconvinced because we believe that this argument takes an overly simplistic view of the neurological substrates (or indicators) of anxiety and provides an insufficient basis for claiming that PFC activity is an indicator of a latent emotion regulation variable. Biological models of anxiety in no way confine the neural substrates of this emotion to the amygdala. Although it is generally acknowledged that the amygdala plays a role in anxiety and fear, both current and historical models of this emotion have also accorded important roles to other brain regions (e.g., Bannermann et al. 2003; Gray 1970, 1982; Gray and McNaughton 2000; LeDoux 1995). It is also widely acknowledged that amygdala activation is not specific to fear and the processing of aversive stimuli but is also involved in positive affect and the processing of positively-valenced stimuli (e.g., Davis and Whalen 2001). Moreover, some models of the neuropsychology of anxiety (e.g., Gray and McNaughton 2000) include interactions of the amygdala not only with the septo-hippocampal system, but also with the anterior cingulate cortex, PFC, and frontal cortex as part of the substrate of anxiety. Of course, even if the PFC is the anatomical substrate of emotion regulation, for an emotion regulation system to work it must have some connections with the emotion reactivity systems that it regulates and this may account for its interconnections with the other structures that Gray and McNaughton identify as

the substrate of anxiety. However, the fact that the PFC is thought by Gray and McNaughton to be a component of the anxiety system itself at the very least raises some additional questions regarding Cisler et al.'s identification of the PFC as the substrate of emotion regulation.

Further, we believe that the fact that PFC activity is elevated during efforts to engage in various emotion regulation strategies does not provide sufficient reason to suggest that a latent emotion regulation factor underlies these strategies or that PFC activity is the neural substrate of emotion regulation. As Cisler et al. (2009) in fact note, the PFC has been broadly associated with goal-directed behaviors, deliberate actions, and executive functioning (Miller and Cohen 2001). Although Cisler et al. suggest that emotion regulation strategies “may be relatively automatic or habitual occurring in or outside of awareness,” thus far studies suggesting that elevated PFC activation occurs in conjunction with emotion regulation have all utilized paradigms in which participants were instructed to deliberately attempt to control their emotions or utilize particular regulation strategies. Thus, the PFC may have been activated during these studies not because it is specifically associated with emotion regulation but because participants were engaging in goal-directed behavior.

In other words, while PFC activation occurs during the deliberate use of actions that fall under the rubric of emotion regulation strategies, it is certainly not specific to these actions. It is also active during a wide range of other cognitive tasks (Miller and Cohen 2001), such as attempting to memorize and retrieve words by organizing them in semantic categories (Fletcher et al. 1998). If one infers a latent emotion regulation construct underlies emotion regulation strategies such as re-appraisal and emotional suppression simply because attempts to use these strategies are associated with PFC activation, then by that logic, deliberate word memorization should also be viewed as indicator of emotion regulation. We are confident that Cisler et al. (2009) would agree with us that an assertion along those lines would be preposterous. If so, we believe that this illustrates that the logic of identifying the PFC as the neurological substrate of emotion regulation on the basis of findings that it is activated during emotion regulation is problematic.

Interestingly, in contrast to the overarching concept of emotion regulation, the concept of effortful control can clearly be viewed as an underlying construct, as evidenced by Sulik et al. (2009) and previous studies (Eisenberg et al. 2004; Rothbart et al. 2001). Further, this construct appears to be related to but distinct from negative emotionality, associated with activity of the PFC, and related to other cognitive tasks that also prompt PFC activation (see Rothbart and Rueda 2005, for a good review). Hence, it is feasible that effortful control may predict residual variance

in some lower order indicators of anxiety in a manner akin to that suggested by Cisler et al. (2009). However, as emotion regulation is used to refer to a number of behaviors and strategies, not all of which involve engaging in effortful control, the concepts of effortful control and emotion regulation should not be conflated. In the absence of further evidence that there is a latent emotion regulation variable or dimension that is distinct from anxiety, researchers should at least consider that emotion regulation may be nothing more than a descriptive label for a broad range of behaviors and strategies.

This is not to say that the behaviors and strategies that fall under the rubric of emotion regulation are not interesting and worthwhile objects of study. Moreover, we welcome the opportunity to continue the debate on the status of the emotion regulation construct and its incremental predictive validity. Such debate often leads to stronger work on a construct which, in turn, leads to enhanced recognition of the importance of the construct such as was the case in the debate over whether anxiety sensitivity had incremental predictive validity above and beyond the effects of the closely related constructs of trait anxiety or neuroticism (e.g., Eke and McNally 1996; Lilienfeld et al. 1989; McNally 1989; Rapee and Medoro 1994; Zinbarg et al. 2001). Regardless of whether the behaviors that emotion regulation researchers would currently place under the rubric of emotion regulation are distinct from emotional disorders or emotion reactivity, it is clear that many of them play a role in the course emotional problems, and that their study may, at the very least, help us understand and refine current treatments. For example, given that facilitating reappraisal is one of the central goals of cognitive behavioral therapy, one of the most well-validated treatment for emotional disorders (Hollon et al. 2006), it seems likely that failure to engage in reappraisal might be associated with the continuation of anxiety and depression.

Establishing Whether Emotion Regulation Deficits have Incremental Validity We do not question the conclusions that emotion regulation strategies can influence emotional responding and that anxious patients choose different regulation strategies than controls. As suggested by Cisler et al. (2009), the body of evidence from studies that manipulate emotion regulation efforts is impressive and the conclusion that emotion regulation strategies can influence responding should now be considered to firmly supported and established. Cisler et al. and Carthy et al. (2009) also present convincing evidence that anxious patients choose different regulation strategies than controls and establish that such differences are reliable. We do not debate this. What we have questioned in the past (i.e., Zinbarg and Mineka 2007), and continue to question, is whether emotion regulation

deficits contribute to anxiety disorders above and beyond excessive emotion reactivity.

To begin with, we do not interpret the term deficit as a difference in preference among different emotion regulation methods, but rather, as an impairment in a functional capacity. That is, according to our way of thinking, a deficit is present when one attempts to apply a skill or strategy but does so in a less than adequate (or perhaps less than average) manner. The one study cited by Cisler et al. (2009) and the only published study we are aware of that asked anxious patients and controls to both use a particular mode of emotion regulation (i.e., cognitive reappraisal) found no evidence that the patients were any less able than the controls to use cognitive reappraisal to reduce negative emotion (Goldin et al. 2009).

Carthy et al. (2009) cited some findings under review showing a similar pattern in children. According to Carthy et al. “anxious children demonstrated ... difficulty activating cued reappraisal in order to decrease their negative emotion. *However, once applied, this strategy was equally successful in reducing negative emotion for both groups* [italics added for emphasis].” In our opinion, the fact that anxious children had difficulty activating cued reappraisal following the brief training administered in this study is neither surprising nor indicative of an inherent deficit in activating and effectively using reappraisal. Indeed, following cognitive behavioral treatment protocols, it is often necessary to spend several weeks encouraging patients to consistently and systematically engage in reappraisal (e.g., Zinbarg et al. 2006). However, after this extended training, a large percentage of extremely anxious and depressed patients prove capable of initiating and using successfully using reappraisal to reduce their negative emotions. Hence, we suggest that it is premature to conclude that the anxious children evaluated by Carthy et al. exhibited an emotion regulation deficit simply because they did not easily adopt a new emotion regulation strategy following a very brief training session. Moreover, the fact that they appeared equally adept in using reappraisal to reduce negative emotion once they did apply this strategy seems totally at odds with the notion of a deficit in reappraisal.

Likewise, in our view, none of the studies cited by Cisler et al. (2009) as demonstrating incremental validity of emotion regulation in predicting anxiety disorders actually support the conclusion that emotion regulation *deficits* contribute to anxiety disorders above and beyond excessive emotion reactivity. Rather than incorporating an objective measure of actual efficacy in deployment of emotion regulation, each of the studies cited by Cisler et al. in this regard incorporated measures of either the individual’s (a) preferred modes of emotion regulation such as the Salovey et al. (1995) Trait Meta-Mood Scale (“No matter how badly I feel, I try to think about pleasant things”) or (b) *perceived*

efficacy in influencing their emotion via emotion regulation such as the Gratz and Roemer (2004) Difficulties in Emotion Regulation Scale (“it takes me a long time to feel better”) or the Catanzaro and Mearns (1990) Negative Mood Regulation scale (“I can usually find a way to cheer myself up”). The measures of perceived emotion regulation efficacy come closest to measuring emotion regulation deficit but, unfortunately, in our view self-report efficacy measures are not up to the task of demonstrating actual deficit.

A patient’s self-report of low perceived efficacy with a particular regulation strategy may not be indicative of a deficit in using that strategy because the patient may not have used that strategy frequently enough in the past to know that it can work for him/her. In addition, a patient’s self-report of low perceived efficacy at emotion regulation in general may not indicate a generalized deficit because all the patient knows is that he or she is left with more anxiety than he or she would like (or perhaps in comparison with others) but cannot possibly know the extent to which this is due to excessive initial reactivity versus deficient regulation.

To better appreciate this last point, please consider a metaphor in which initial reactivity is analogous to the amount of gas reaching the engine of a car and emotion regulation is analogous to the car’s brake system. Imagine that the drivers of two cars of the same make and model, one traveling at 30 mph and at 60 mph, apply equal pressure to the brakes. The second car, which is traveling faster since the driver is applying more pressure to the accelerator thereby sending more fuel to the engine, will come to a stop after the first. Does this indicate a deficit in the braking system of the second car? It is certainly possible that the second car has faulty brakes, but this is by no means necessarily the case as even if its brakes work just as well as the first car’s it would be expected to stop more slowly simply due to its greater momentum.

We contend that individuals asked to rate their emotion regulation efficacy are in the same position that passengers in the two cars in our examples would be in if they were not aware of the difference in speed of the two cars at the point at which pressure was first applied to the brake. Such passengers would know that the second car took longer to stop and might therefore be tempted to conclude that its brakes do not work as well as the first car’s brakes. They would not be justified in this conclusion, however, unless they could repeat the test of the brakes making sure that the two cars were traveling the same speed. And so too, neither our patients nor we can know that our patients are deficient at emotion regulation until we test their ability to regulate emotional arousal of the same intensity as that of a nonpatient using the same mode of emotion regulation.

To muddy the theoretical waters even further, we believe that initial reactivity is likely to influence the choice of

mode of emotion regulation. Thus, avoidance seems to be consensually regarded as one mode of emotion regulation and a great deal of theory and evidence suggests that anxiety and fear are causal influences on avoidance and escape (for a review see Zinbarg 1998). Indeed, the primary finding of one of the studies that Cisler et al. (2009) cited as demonstrating the incremental validity of emotion regulation is that fear (of bodily sensations) predicts experiential avoidance and non-acceptance (Tull et al. 2008). It seems to us that the primary conclusion to be drawn from Tull et al. is that reactivity (fear of bodily sensations) predicts choice of regulation mode. If it is true that initial reactivity influences the choice of modes of emotion regulation, then it seems likely to be the case that individual differences in initial reactivity at least partly account for the differences between anxious patients and controls in their choice of emotion regulation modes and may even entirely account for these differences. This is a question that needs to be examined in future empirical studies.

Conclusions

In summary, the articles in this issue represent important steps forward in the study of emotion regulation variables. The conceptual and methodological advances that they exhibit should be extended by future research. Namely, future research should continue to examine emotion regulation variables in a developmental context and should utilize longitudinal designs to better understand the developmental time course of variables of interest. Further, studies should emulate many of the articles in this issue in incorporating observational and experience sampling measures, and should also use such measures to model latent variables.

However, the field of emotion regulation continues to face significant challenges that should be addressed. Despite the good faith efforts of investigators to more clearly define emotion regulation, it is clear that the definitional and conceptual problems noted by Zinbarg and Mineka (2007) have not yet been fully remedied. Thus far, it remains unclear whether emotion regulation is a latent variable distinct from emotion activation/reactivity or merely a descriptive label for a wide variety of behaviors. In referring to emotion regulation as if it were a latent variable, the onus is on researchers to either more firmly establish it one or change the language that they employ. Further, although it is now clear that emotion regulation strategies can influence emotional responding and that anxious patients choose different regulation strategies than non-anxious individuals, researchers have yet to show that deficits in emotion regulation contribute to disorders above and beyond excessive reactivity. We echo

the sentiments expressed by Zinbarg and Mineka (2007) in highlighting the need for empirical evidence that emotion regulation deficits a) exist and b) have incremental predictive power.

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