

Commentaries

Some Key Research Questions for Mindfulness Interventions

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For industrial and organizational psychologists who are unfamiliar with the mindfulness literature, Hyland, Lee, and Mills (2015) nicely introduce the concept by highlighting key findings from prior studies. Although their review focuses on the many benefits of mindfulness, we believe that mindfulness research should address certain questions that will help us understand whether mindfulness interventions result in a cost-effective positive return on investment. In alignment with the perspective of evidence-based practice (Briner & Rousseau, 2011; Pfeffer & Sutton, 2006), we call for a holistic evaluation of mindfulness, including a consideration of when or how unintended side effects emerge. Importantly, we discuss the potential mechanisms by which mindfulness generates valued outcomes (e.g., performance and collective psychological climate) and the need for more sophisticated research to isolate these causal effects. We also consider how the judicious use of utility analytics (e.g., cost effectiveness and return on investment) might help demonstrate the value of mindfulness interventions while also acknowledging questions of causality that must be addressed for such value to be experienced. We close by clarifying that we have the intention of promoting research to further evidence-based practices. There are organizations that

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have already begun providing mindfulness meditation interventions, and it is our hope that our commentary will help practitioners in these settings to consider the evidence suggesting that there may be unknown nuances regarding mindfulness practice. Ultimately, we believe that mindfulness is an important burgeoning area of research deserving of more scholarly attention.

Toward a More Holistic Understanding of the Mindfulness Construct

The holistic perspective on mindfulness research that we articulate acknowledges both the benefits of mindfulness and the possible unintended side effects, both of which seem suggested by prior research. Indeed, Hyland et al. review several studies suggesting that mindfulness has promising outcomes for key organizational decision makers to take seriously. However, they omit studies suggesting the possibility of unintended side effects, which a more recent critique from the popular press suggests (Crane & Grosso, 2013/2014). Thus, we seek to draw attention to scholarly critiques of mindfulness interventions.

Criticism of mindfulness stems primarily from the clinical literature, where mindfulness-based meditation interventions have been applied with increasing frequency. Hickey (2010) criticized empirical research for using small and demographically homogenous samples (i.e., White, educated, middle-class women). Chiesa and colleagues (Chiesa & Malinowski, 2011; Chiesa & Serretti, 2009) pointed out other problems, such as the diversity of mindfulness conceptualizations, which have produced interventions that vary in the way that mindfulness is practiced. These authors also noted that few mindfulness studies use random assignment or adequate controls and that those that do use a wait-list control, which is simply a group of people waiting to receive the treatment. A more ideal use of controls would involve similar treatments that differ from mindfulness interventions only in terms of content that directly stems from the operationalization of mindfulness. Indeed, such a study has been conducted. In the largest experimental study of mindfulness-based cognitive therapy to date ($n = 255$), which was applied to individuals with recurrent depression, mindfulness-based cognitive therapy was not found to be generally more effective than cognitive therapy without the mindfulness component or treatment as usual (Williams et al., 2014). However, it *did* help vulnerable individuals with a history of child trauma (Williams et al., 2014). As an explanation for this finding, Williams et al. suggested that mindfulness-based cognitive therapy might facilitate adaptive forms of emotional processing (e.g., recognizing emotions, avoiding rumination), which has been suggested elsewhere (Chiesa, Serretti, & Jakobsen, 2013).

Given the use of inconsistent conceptualizations of mindfulness noted by Chiesa and colleagues (Chiesa & Malinowski, 2011; Chiesa & Serretti,

2009) and the frequent use of meditation interventions to cultivate mindfulness, it is worth noting that there is evidence of inconsistent outcomes attributable to meditation practice as well. For instance, a 6-month longitudinal study of 27 long-term meditators found that approximately half of individuals experienced adverse effects (e.g., increased relaxation-induced anxiety and panic, increased tension, lower motivation in life; Shapiro, 1992). Interestingly, Shapiro noted long-term meditators experienced more adverse effects than those who have practiced meditation for a shorter period of time. A more recent systematic review of 24 mindfulness-based stress reduction and mindfulness-based cognitive therapy studies, which are meditation-based interventions, noted that practice effects were observed in only half of all studies that reported homework practice (Fjorback & Walach, 2012). Still other researchers testing the stress-buffering role of mindfulness observed that mindfulness meditation training, compared with a cognitive training program, reduces self-report psychological stress but increases cortisol reactivity in response to social evaluative stress (Creswell, Pacilio, Lindsay, & Brown, 2014). Interestingly, individuals low in preexisting levels of mindfulness reported the strongest cortisol reaction to the aforementioned stress test.

Putting this research together with Hyland et al.'s review suggests the need for research into how much mindfulness practice is needed in order for the practice to become functional and to consistently provide value. In order to address this, researchers should consider conducting longitudinal studies with samples of participants undertaking more specific guidance on mindfulness practice. It is still unclear whether the quantity or quality (or both) of mindfulness practice matters. Such research will bring clarity both to the mindfulness construct and to mindfulness practice. An example of this sort of research can be found in Hülshager, Alberts, Feinholdt, and Lang (2013), who in two studies (a diary study and field experiment) examined the effects of mindfulness on both emotional exhaustion and job satisfaction. Consistent with the role of emotion regulation strategies discussed previously, these researchers observed that mindfulness had a causal effect on emotional exhaustion and job satisfaction, which was mediated by surface acting. In other words, mindfulness decreases the likelihood of engaging in surface acting as an emotion regulation strategy, which prevents emotional exhaustion and promotes job satisfaction.

Organizational researchers will benefit from adopting similar designs in their research, but expanding the outcome variables of interest to job performance and behaviors that foster a positive psychological climate is needed. We believe research examining the perceptions of close others in the workplace is an important future research direction to undertake in order to understand the broader impact of frequent mindfulness practice in

organizations. Of course, care must be taken to avoid capitalizing on employees' implicit theories that mindfulness should improve their functioning at work, a plausible source of method bias (Podsakoff, MacKenzie, Lee, & Podsakoff, 2003). Given the adverse effects discussed earlier, it seems that the positive effects of mindfulness meditation discussed by Hyland et al., like enhanced executive functioning, may require nuanced practice to achieve positive outcomes. Thus, there may be specific ways in which mindfulness is conducted in order for organizations to systematically experience productivity gains. Again, conducting longitudinal studies in diverse samples of individuals practicing various forms of mindfulness should allow for this more holistic perspective to develop. In keeping with this notion of a more holistic evaluation and given that researchers and practitioners will likely be most concerned with the degree to which mindfulness interventions positively affect individuals in the general population (e.g., working adults), the limitations noted by clinical researchers (e.g., demographic homogeneity, applications guided by different conceptualizations) should be an impetus for organizational researchers to be thorough in reporting the details of their work so that a future meta-analysis of the literature can both summarize the causal role of mindfulness in the workplace and identify sources of moderation. In short, longitudinal, multisource designs using experience-sampling methodologies should add to our understanding of the impact of mindfulness as a practice, with specific parameters around how it should be practiced in order to be effective. A future meta-analysis should then be able to identify the possible boundary conditions and nuances surrounding mindfulness meditation practice across multiple criteria, allowing for the very holistic evaluation that we are advocating.

Encouraging Evidence-Based Practice Using Utility Analytics

Echoing researchers who encourage evidence-based practice (Rousseau & McCarthy, 2007), we believe that practitioners, especially those inclined to view mindfulness meditation favorably, should consider acting with what Pfeffer and Sutton (2006) refer to as wisdom; that is, acting with the best information on hand while doubting what you believe to be true. In the context of applying mindfulness research, this involves using the best causal knowledge of the mindfulness construct to design interventions while doubting what may be true. This might involve certain subpractices of mindfulness meditation being called into question. Practitioners could also take the role of testing any mindfulness meditation practices that are based on poorly supported causal reasoning. Such research can be invaluable and contribute to our collective understanding of mindfulness, especially if effects emerge linking mindfulness to outcomes of financial value (e.g., performance).

For those practitioners hoping to capitalize on mindfulness research, utility analytics such as those put forward by Boudreau and Cascio (2008) can be used to acknowledge the promise of mindfulness meditation interventions while also acknowledging where our causal knowledge is lacking. There are key parameters in utility analytics that will require extensive research, such as (a) the causal effect of various forms of mindfulness meditation on valued outcomes in the workplace (e.g., performance) within a diverse employee population, (b) the duration of these effects, (c) the cost of the intervention, and (d) the participation rates across time. Given the research linking mindfulness to work attitudes, and research linking work attitudes to performance (Ricketta, 2008), we can develop a quantitative prediction linking mindfulness to performance. This is consistent with strong theory testing (Velicer et al., 2008). Of course, this estimate will need to be tested. Although some researchers may currently struggle to communicate the value of field experiments to organizational stakeholders at present, expert opinion and/or longitudinal multisource designs using experience-sampling methodologies can convincingly furnish the estimates to each of these parameters, allowing for a stronger case to be built for creating such experiments. Thus, researchers hoping to encourage mindfulness meditation interventions should acknowledge where our causal knowledge is imperfect and communicate the value of studies that address these imperfections. If researchers grow to understand the underlying mechanisms of mindfulness, businesses can begin to implement more nuanced and specific mindfulness interventions, which will have a stronger link to key business outcomes. It is our hope that understanding the causal mechanisms of mindfulness will allow companies to compare mindfulness programs with other programs with similar causal mechanisms (i.e., those involving emotion regulation) in terms of cost effectiveness, thus allowing for a more rational decision-making process on investments to take place. Such research should allow us to be able to reliably estimate and understand the causal impact of the mindfulness construct as well as allow us to estimate the cost effectiveness of the intervention.

Conclusion

We have attempted to encourage more thoughtful research on mindfulness in the workplace. Specifically, we argued for a holistic evaluation of the mindfulness construct, noting studies with interesting findings (such as null or adverse effects), and called attention to methodological issues within the mindfulness literature. We pointed out where more research is needed to understand the value of mindfulness interventions. Encouraging evidence-based practice, we called practitioners to doubt what they believe to be true about mindfulness and to design studies to address the specifics of

mindfulness practice. Using a utility framework, we pointed out how practitioners can estimate the value of these mindfulness interventions while also acknowledging where our causal knowledge is limited. Longitudinal field experiments with experience-sampling methodologies and third-party reports of workplace outcomes will be needed to tease out the effects of enhanced mindfulness on performance. In short, we echo Hyland et al.'s encouragement of mindfulness research in the workplace and encourage practitioners to wisely consider the value of related interventions.

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Mindfulness, Flow, and Mind Wandering: The Role of Trait-Based Mindfulness in State-Task Alignment

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Mindfulness—present moment attention and awareness (Brown & Ryan, 2003)—is commonly proposed as a productive state of consciousness in the workplace. Unfortunately, being mindful at every moment of the workday is fairly uncommon. Research suggests that people engage in *mind wandering*—a lack of attention to and awareness of the present (Smallwood & Schooler, 2006)—for the majority of their day (in every task except making love; Killingsworth & Gilbert, 2010). Further, there is another state of consciousness called *flow*—an intense sense of concentration and control over activities—that has also been linked to workplace performance (Nakamura & Csikszentmihalyi, 2002). Interestingly, whereas mindfulness facilitates higher performance by being aware of external stimuli, flow enables higher performance by doing the opposite—blocking out external stimuli. These findings suggest that mindfulness is neither the most common psychological state nor the only productive psychological state for the workplace.

Hyland, Lee, and Mills (2015) suggest broadening investigations of mindfulness by considering its relationship with other consciousness constructs. Aligning with this directive, I offer arguments suggesting that

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