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Neuroscience and organizational behavior: Avoiding both neuro-euphoria and neuro-phobia

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Summary Organizational neuroscience has great promise for advancing organizational research and practice. The field, however, is developing rapidly and has also become the subject of technological and methodological challenges that must be considered when conducting or interpreting neuroscience research as applied to organizational behavior. We explore four issues we deem to be important in understanding the role of neuroscience in organizational behavior research: (i) neuroscientific research and reductionism; (ii) the need to address methodological and technological challenges in conducting this type of research; (iii) how neuroscientific research is meaningful in organizations (the "So what?" issue); and (iv) neuroscience as just another management fad. In addressing these issues, we hope to set out a roadmap that will enable organizational scholars to avoid past mistakes and thus serve to advance multidisciplinary research in organizational behavior using neuroscientific approaches. Copyright © 2014 John Wiley & Sons, Ltd.

Keywords: organizational neuroscience; management fads; electroencephalograph; fMRI

Scholars have recently called attention to the potential of neuroscience to inform organizational research and theory (Becker, Cropanzano, & Sanfey, 2011; Beugré, 2010; Senior, Lee, & Butler, 2011). Moreover, there have been a number of excellent examples to date where neuroscience methods have been applied to organizational settings. Waldman, Balthazard, and Peterson (2011a), for example, used quantitative electroencephalogram (qEEG; a form of neurofeedback that uses multiple EEG nodes) to investigate visionary leadership behaviors. Bagozzi and colleagues (2013) coupled functional magnetic resonance imaging (fMRI) and behavioral measures to investigate Machiavellianism and organizational outcomes. Most recently, Akinola and Mendes (2014) used neurobiological measures to investigate police officers' decision making. This variety of methods and applications reflects the expansive view of organizational neuroscience encouraged by its proponents (Becker & Cropanzano, 2010; Lee, Senior, & Butler, 2012).

Interest in neuroscience has also exploded in the popular press and public consciousness (Poldrack, 2008), including in best-selling books (e.g., Gladwell, 2005; Pinker, 2002). As a result, public interest and fascination has created pressures to commercialize methods and findings from neuroscience (Caulfield & Ogbogu, 2008). Regrettably, efforts to commercialize neuroscience have frequently outpaced the underlying science (Owen et al., 2010; Vernon, 2005). Cropanzano and Becker (2013) noted that even scholars have not been immune to over-exuberance regarding the implications of neuroscience-based findings (e.g., see Ringleb & Rock, 2008).

This seeming euphoria regarding the implications and application of neuroscience to organizational behavior theory and practice has given rise to a fear by some that organizational neuroscience research is headed in the wrong direction (Lindebaum, 2013). On the basis of our reading of the literature, there seem to be four main issues of contention. The first is a fear of reductionism, that organizational neuroscience seeks to reduce organizational behavior

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to activity in particular brain regions or even neurons; as well as related concerns about the potential value of neuroscience to inform organization behavior theory. Second, there are legitimate concerns with limitations of the methods and technologies that underlie neuroscience research, including whether non-significant findings can be meaningful. The third issue is that, even if the first two concerns prove unfounded, practical applications of neuroscience may be inappropriate or meaningless, and the potential for application to issues beyond individuals (e.g., team-based phenomena) is non-existent. This is the "So what?" issue. The fourth and final issue we address is that some applications of neuroscience (especially with regard to leadership) have all the characteristics of (yet) another management fad. We acknowledge that some of these concerns have been already addressed to some degree (e.g., see Cropanzano & Becker, 2013), so we focus in this article on offering some additional considerations as a means to contribute further to the ongoing debate.

Issue 1: Reductionism

The first issue that we address is *reductionism*, which has been a common concern and point of contention within the philosophy of science for many years. It is frequently raised by those who seek to reject efforts to investigate the role of individual genes, neurons, or brain regions (i.e., sets of neurons) in explaining human characteristics or behaviors (Kaiser, 2011). This concern has been largely fueled by attempts to characterize neuroscience as the localization of cognitive and emotional function to specific neural regions within the brain (Bickle & Hardcastle, 2003). We argue, however, that the reductionist conflict between psychology and neuroscience has been largely overstated (Marshall, 2009). Indeed, it is becoming clear that neuroscientists themselves recognize the fallacy of attempting to reduce human behavior to levels of activity or inactivity in specific brain regions. Instead, neuroscience theory and research would suggest that the neurological basis of complex behavioral phenomena can best be considered using network-based approaches based on multiple brain regions working in unison (Cacioppo, Berntson, & Nusbaum, 2008). Lindquist, Wager, Kober, Bliss-Moreau, and Barrett (2012), for example, used (what they term) a "psychological constructionist" approach to suggest that various networks of the brain produce psychological and behavioral phenomena. Lindquist and colleagues make the case in particular that a given network might be simultaneously pertinent to different psychological events and phenomena that, in turn, are relevant for various aspects of emotion, cognition, and perception. In short, through brain network theory and research, reductionism becomes at least a bit less "reduced."

We argue in particular that the problem is not so much the use of neuroscience *per se*, but how it has been applied conceptually. We see two problems in this regard. The first concerns exactly how far reductionism should go toward suggesting that neurological processes do in fact underpin organizational behavior. Indeed, we concur that localization within the brain *can* go too far. But the same criticism can be linked to a plethora of localization research reviewed by Lindquist et al. (2012), not just a targeted criticism of neuroscience applications to organizational behavior. The challenge for organizational researchers, as with neuroscience researchers in general, is therefore to begin to think more in terms of networks, rather than the strict localization of mental and behavior processes. We begin to see such thinking in the work of Hannah, Balthazard, Waldman, Jennings, and Thatcher (2013), who examined a theoretically derived brain network index of leader complexity.

Second, we concur with Healey and Hodgkinson (2014) who refer to socially situated cognition as a means to understand how brain activity might interact with environmental context. In this regard, Healey and Hodgkinson make the case that, instead of viewing the brain as the sole cause of employee behavior in organizations, a more fruitful approach may be to also examine how brain functioning is influenced by organizational socialization processes. Nonetheless, they recognize that it is also an inescapable fact that behavior in organizations, like human behavior generally, must ultimately be derived from neurobiological processes. In short, we see a return to one of the oldest considerations in organizational behavior: the interplay between the individual and context. The difference

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here refers to how context may not only affect individual cognition, emotions, and behavior but may also have physiological effects in terms of brain function.

Many of the most promising applications of neuroscience, particularly as recently applied within the growing field of organizational neuroscience, involve the use of deductive logic derived from existing theories to test hypotheses about previously unobservable mental processes. One advantage of the application of neuroscience to organizational phenomena is the ability to understand constructs of interest better and to form more precise measures or comprehensive models of prediction (Powell, 2011). In this regard, Senior et al. (2011, p. 806) proposed that neuroscience-based approaches can allow organizational researchers to "examine problems within a wider analytic framework, which in turn allows for the development and testing of additional hypotheses." Returning to the work of Hannah et al. (2013), Senior and colleagues showed how their neurologically based index of leader complexity was related to, but still differentiated from, a psychologically based measure of complexity. Further, the neurological measure enhanced their prediction of adaptive leadership.

Senior et al. (2011) also considered how neuroscience measures can be incorporated in a symbiotic manner with more traditional measures to provide increased understanding. Accordingly, it would appear to be an exaggeration for critics to suggest that those who have applied neuroscience methods to organizational phenomena do so solely to get around the problems associated with self-report inventories. The implication here seems to be that the ultimate goal might be to replace survey-based measures or other traditional assessment techniques with neuroimaging. In reality, there is no evidence of an attempt toward, or even talk of using neuroimaging techniques to replace traditional measures such as surveys.

Further, proponents of neurobiological approaches in organizational behavior (e.g., Becker & Menges, 2013) emphasize the role of neuroscience as a *complementary* rather than competing method of explaining and measuring complex organizational behaviors, and have urged researchers to simultaneously employ both traditional and neuroscience methods. In addition to the Hannah et al. (2013) study, we point to recent examples that have heeded this advice (e.g., see Akinola & Mendes; 2014; Bagozzi et al., 2013; Waldman et al., 2011a) as evidence that investigating organizational phenomena allows researchers to combine both neuroscience measures and more traditional measures in field settings. We believe that these types of efforts have great potential to advance both fields separately, as well as providing *interdisciplinary* insights in a broader sense.

We agree nonetheless that steps need to be taken to pursue more concrete theory development when attempting to link neuroscience with organizational phenomena. With that said, such possibilities will only occur through a more in-depth consideration of the meaning of terms that are oftentimes thrown around quite loosely, such as "brain activity." As summarized by Waldman, Balthazard, and Peterson (2011b), much neuroscience research to date, including organizational applications (e.g., Boyatzis et al., 2012), has attempted to understand the neurological basis of behavior through experimental methods in which visual stimuli are introduced and changes in activity in various regions of the brain are recorded. These changes are typically operationalized in terms of blood flow measures associated with fMRI scanning methods. Such research fits "well with the view of the brain as driven by the momentary environmental demands" (Raichle & Snyder, 2007, p. 1084). In contrast, a growing body of research would suggest that the brain at rest (but in an awake state) is not passive and is actually more active than when presented with stimuli or during goal-directed tasks (e.g., Buckner, Andrews-Hanna, & Schachter, 2008). At the same time, it is becoming increasingly recognized that the brain at rest can be used to represent the relatively enduring, intrinsic activity of the brain that reflects meaningful individual differences (Lindquist et al., 2012; Waldman et al., 2011b). Accordingly, any move toward better theoretical development will need to take into account stimulated versus at rest or intrinsic brain activity.

Relatedly, "brain activity" can be operationalized in terms of different variables, depending on the technology. For example, through the use of EEG, variables can be quantified to reveal the sheer amount of electrical energy in particular brain regions, as well as the extent of connectivity between regions (cf. Hannah et al., 2013). Research should pursue the meaning of these different types of variables when forming neuroscience indices to predict cognition, emotions, and behavior.

In sum, organizational neuroscience recognizes that complex phenomena are shaped by lower level neural processes, as well as higher level social interactions, so that any explanation that ignores either level is incomplete

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(Cacioppo & Decety, 2011). This line of argument, based on considering levels of analysis, will be familiar to readers who study teams. Although it is important to learn as much as possible about individual team members, we must also observe how members interact and also how the team is influenced by the larger organization it is part of. This is also consistent with Ashkanasy's (2003) multilevel framework of emotion, which implies that researchers need to consider emotion variables across multiple levels of analysis. In short, although the aim of neuroscience is reductionism within the brain level of analysis, its purpose is to illuminate not eliminate higher level psychological and organizational theories (Marshall, 2009). Furthermore, we agree with Healey and Hodgkinson (2014) that any neuroscience-based emphasis on reductionism should be tempered by considering that cognition and underlying brain activity are, at least to some degree, socially situated. Thus, any attempt to connect brain activity to organizational phenomena should consider that those phenomena may themselves affect the nature of brain activity.

Issue 2: Technological and Methodological Challenges

The second line of attack against neuroscience research that we identified in the introduction to this essay can be found in attempts to discredit its methods. Because fMRI technology and methodology has become prominent in neuroscience research (Poldrack, 2008; Racine, Bar-Ilan, & Illes, 2005), it has been the most frequent target of these questions. It is important to note that the original intent of these criticisms was not to suggest that fMRI-based studies should be abandoned but rather that they should be conducted and interpreted with care (Sanders, 2009). Nonetheless, it is worth familiarizing readers with the most recent critiques of fMRI in order to illustrate this point. In this regard, Vul, Harris, Winkielman, and Pashler (2009) introduced a broader psychology audience to issues that were already familiar to most neuroimaging scientists and students. They pointed to published fMRI studies in which reported correlations between brain activity and behavioral measures were improbably high and likely caused by non-independence and sample size issues. The Vul et al. article sparked a vigorous debate around fMRI statistical methods, focusing primarily on how best to perform and to report fMRI analyses (e.g., Lieberman & Cunningham, 2009; Nichols & Poline, 2009). At no point, however, did any of the authors suggest that fMRI was not a valid and important method for investigating the brain.

Sample size and power are other well-known methodological issues with fMRI and other neuroscience methods that were resurrected in a recent article by Button et al. (2013). That study used available meta-analyses to perform *post hoc* power calculations of the studies included in those meta-analyses to show that the average power of the studies was relatively low. It is worth noting here that the lowest power estimates came largely from animal and brain volume studies, and not from fMRI. Nonetheless, the authors' primary point was that as neuroscience investigates more subtle effects, the sample sizes should increase accordingly. Once again, the authors *did not* argue that fMRI and other neuroscience methods should be abandoned, but rather that they should be conducted, reported, and interpreted with greater care. In fact, their own recommendations focused on greater disclosure in the reporting of study design and analysis and incentivizing replication. Although most authors agree that guarding against Type I errors is an important concern, others worry that, in the case of fMRI in particular, a single-minded focus on avoiding Type I errors might be counterproductive (Lieberman & Cunningham, 2009).

We point out here that proponents of organizational neuroscience have always advocated bringing a wide variety of neuroscience methods and findings to bear on organizational questions (Becker & Menges, 2013). In fact, fMRI is not the only promising methodology for organizational settings. Recent work is demonstrating the viability and promise of EEG and qEEG technology in organizational science research (e.g., see Balthazard, Waldman, Thatcher, & Hannah, 2012; Hannah et al., 2013; Waldman et al., 2011a). Because of such factors as cost effectiveness and portability, EEG methods readily allow for larger sample sizes. Although not as spatially precise as fMRI, the temporal precision of EEG is superior. Furthermore, as argued by Waldman et al. (2011b), the increased spatial precision of qEEG in recent years has allowed the technology to become a viable means of examining brain activity in a way that might be relevant to organizational behavior.

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Organizational scholars will immediately recognize that the types of methodological concerns raised earlier are not unique to neuroscience (Fiedler, 2011). Increased power, greater transparency in reporting study design and analysis, and incentives for replication are also needed in other domains of organizational research (Murayama, Pekrun, & Fiedler, 2014; Scherbaum & Ferreter, 2009). We point out that recent proponents of an organizational neuroscience perspective have not been blind to these issues, and they have urged caution in interpreting any single neuroscience study (Becker et al., 2011).

As we have already discussed in this essay, neuroscience is a relatively new and rapidly evolving scientific field. At the same time, we also acknowledge that its great promise is accompanied by technological and methodological challenges (Cacioppo & Decety, 2011). We therefore suggest that organizational neuroscience should be employed with cautious optimism. Going further, we believe that it would be irresponsible for organizational scholars to remain on the sidelines until the field of neuroscience has overcome all of its challenges. A primary reason here is that organizational science is in the midst of an affective revolution (e.g., Ashforth & Humphrey, 1995; Ashkanasy & Ashton-James, 2005; Barsade, Brief, & Spataro, 2003). This has led scholars to recognize that there is currently an under-appreciation for the role of implicit affect and automatic processing in current organizational theories (e.g., Barsade, Ramarajan, & Westen, 2009; Dane & Pratt, 2007; Woiceshyn, 2011). We argue that an interdisciplinary organizational neuroscience perspective provides the most promising means of conceptualizing and investigating previously invisible drivers of organizational behavior. Organizational scholars are also uniquely positioned to ground neuroscience research in behavioral measures and practical settings that will ultimately help overcome many of the current challenges and concerns within basic neuroscience research (Cacioppo & Decety, 2011; Henson, 2005).

The final issue we address with respect to methods is the notion that non-significant findings cannot be meaningful. Indeed, it is true that some of the research using neuroscientific methods has produced non-significant findings (e.g., Bagozzi et al., 2013). As such, we acknowledge that non-significant findings *prima face* essentially reflect confirmation that there is insufficient evidence to reject the null hypothesis (Cortina & Folger, 1998). Nonetheless, when compared with significant findings in a greater theoretical context, non-significant findings can still provide evidence of discriminant validity. For example, if a neurological index is associated empirically with a particular aspect of leader behavior, but not other aspects of leader behavior (i.e., other aspects not theoretically relevant to the index), then there would be evidence of discriminant validity (Cortina & Folger). In such a case, non-significant findings become meaningful.

Issue 3: Addressing the So *What* of Neuroscience

Much of the critique of neuroscience in organizational behavior research centers around the "So what?" question. The basic message here has been that even if technological and methodological challenges could be addressed, there is little to be learned or applied to practice on the basis of neuroscience applications to organizational behavior. We wholeheartedly disagree and wish to point out three examples of how neuroscience can inform organizational science right now. First, as described earlier, neuroscience has the potential to inform management theory and prediction. Again, we refer to the work of Hannah et al. (2013) as an example.

Our second example concerns the role of *neurofeedback*, which involves measuring brain activity using EEG and feeding this information back to modify behavior (Kaiser, 2005). Although critics such as Lindebaum and Jordan (2014) have questioned the potential of neurofeedback and its use in organizations (e.g., for leader development), we argue that there is already compelling evidence in support of this approach (e.g., see Hanslmayer, Sauseng, Doppelmayr, Schabus, & Klimesch, 2005). Waldman et al. (2011a), for example, cited qEEG research that has identified neural patterns empirically associated with desirable behaviors. They argue in particular that neurofeedback training protocols, such as playing video games with the brain (rather than using hands), can be developed and used to develop leaders. Using such methods, for example, a video game can be designed to accompany feedback in real

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time from a computer-based trainer to a trainee (e.g., by rewarding targeted performance with a clear picture or auditory sound), while penalizing undesired brain patterns (e.g., providing a fuzzy picture or unpleasant sound). The computer trainer applies such rewards and punishments on the basis of whether the trainee's brain is moving in the direction of neuro-circuitry associated with desirable behaviors. Accordingly, neurofeedback of this nature represents a form of operant conditioning. By repeating the process multiple times, and if the trainee is motivated to cooperate with the computer trainer, the brain eventually learns the desired pattern, and the new intrinsic circuitry of the brain becomes relatively permanent. This process is based on the principle of neuroplasticity, which accounts for the ability of the intrinsic brain to change and suggests it is possible for brain circuitry to change over time (cf. Bavelier, Levi, Li, Dan, & Hensch, 2010, on brain plasticity in adults).

Third, in considering the *So what?* question, Lindebaum and Jordan (2014) also lamented that we have yet to develop technology that will allow for brain scans to inform us about the complex nature of social interactions at work, and moreover that we do not yet understand how such data might be used to improve individual and group effectiveness. In this regard, they posited that a major limitation of neurological assessment is that it is restricted to individual behavior in laboratory settings (i.e., it does not apply to group behavior). As noted by Healey and Hodgkinson (2014), however, technology is already being developed to record simultaneous qEEG data on multiple actors engaged in group activities (see, e.g., Berka et al., 2004; Berka, Stikic, Korszen, & Johnson, 2014). In this regard, Waldman et al. (2013) recently showed how, via advanced qEEG technology, real-time neurological assessment can occur simultaneously for each member of a five-member team engaged in a problem-solving task. Waldman and his associates showed specifically that emergent leaders were able to generate more (neurologically assessed) engagement on the part of other team members when they spoke in team meetings.

Finally, regarding neurofeedback, we need to comment on critics' suggestions that proponents of neuroscience applications argue in essence for neurofeedback as a sort of "magic bullet" (our term, not theirs) of leader development. In other words, as a result of leader development, an individual might suddenly be turned into a "great leader." Nothing could be further from the truth, however. Waldman et al. (2011a) clearly pointed to the possibility of combining neurofeedback with established or traditional leader development tools (e.g., multisource feedback, executive coaching) with the premise that those tools might become more effective through the added incorporation of neurofeedback. Moreover, these authors are also quite clear that systematic research is needed to determine if this premise will hold up in the longer term.

Issue 4: Overreacting to Overselling—Emotional Intelligence and Neuroscience

So far, we have addressed issues of reductionism, methodological and technological challenges, and the "So what?" question with regard to the role of neuroscience in organizational behavior research. The final question that we address is why ground-breaking ideas in organizational research such as neuroscience can produce overreactions for and against the idea, such as what we see today with organizational neuroscience. To examine this question, we examine the ongoing controversy concerning another (once) new idea introduced into the organization disciplines: emotional intelligence. Originally introduced in Salovey and Mayer (1990),[1] the construct was subsequently formally defined by Mayer and Salovey (1997). Following its introduction, however, emotional intelligence was popularized in a book by Daniel Goleman, a *New York Times* social sciences columnist, entitled, *Emotional intelligence: Why it can matter more than IQ* (Goleman, 1995). This book subsequently became a *New York Times* bestseller and even made the cover of *Time* (Gibbs, 1995). In effect, emotional intelligence became the next major management fad, which is a "widely accepted, innovative intervention into the organization's practices designed to improve some aspect of performance. Fads evolve into new management practices or are abandoned as failures" (Gibson & Tesone,

[1]The term "emotional intelligence" first appeared in a dissertation by Payne (1985), but Salovey and Mayer (1990) were the first to publish a workable functional definition of the construct.

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2001, p. 122). Characteristic of management fads, the overblown interest in emotional intelligence resulted in ever-increasing exaggerated claims (Jordan, Ashton-James, & Ashkanasy, 2006) and a proliferation of questionable measures.

As a consequence of the proliferation of definitions and measures of emotional intelligence, scholars inevitably began to push back (see, e.g., Antonakis in Antonakis, Ashkanasy, & Dasborough; 2009; Landy, 2005; Locke, 2005). Given that these authors were prominent scholars in the field of industrial and organizational psychology, this led in turn to a movement across the discipline to reject emotional intelligence in its entirety, a movement that continues to this day. Antonakis, for example, stated (p. 256) that emotional intelligence "Looks like *g*" (intellectual intelligence). Ashkanasy (2013, p. 311) has subsequently characterized this as "throwing the baby out with the bathwater."

In defense of emotional intelligence, Ashkanasy and Dasborough (in Antonakis et al., 2009) pointed out that emotional intelligence is essentially an individual difference variable that, like any other such variable, differs between people, just like physical height, personality, and IQ. More recent meta-analyses by Joseph and Newman (2010) and O'Boyle, Humphrey, Pollack, Hawver, and Story (2011) confirm that emotional intelligence is significantly related to important organizational variables (including leadership) over and above the effect of personality and IQ.

Ashkanasy (2013) argued that scholars need to resist the temptation to overreact to new concepts and ideas (as many of them did and some still do in the case of emotional intelligence). He commented in particular that "This kind of kneejerk reaction is really no more excusable than fad behavior" (p. 312). Thus, although we acknowledge that some aspects of neuroscience are showing signs of becoming faddish (e.g., like the exaggerated claims made in Ringleb & Rock, 2008), we argue here that recent, peer-reviewed research into neurobiological determinants of behavior at work is both justifiable and undoubtedly useful.

Extending this line of argument, we posit that neuroscience provides important theoretical insights and opportunities that can move organizational research in emotional intelligence forward. Empathy is a critical component of emotional intelligence. In recent years, neuroscience has helped to demonstrate that empathy in not a unitary process but rather is composed of two broader but related sub-processes. This multidimensional view of empathy has existed for many years (Davis, 1983). With the rise of social cognitive neuroscience, interdisciplinary evidence for the multidimensional view of empathy began to mount (Decety & Jackson, 2004). Dziobek and colleagues developed a picture-based measure of cognitive and emotional empathy using behavioral measures (Dziobek et al., 2008). Then they employed behavioral, fMRI, and skin conductance measures to demonstrate that individuals with borderline personality disorder showed unique deficits in emotional and cognitive empathy compared with healthy controls (Dziobek et al., 2011). In addition, the results of meta-analytic studies (e.g., Fan, Duncan, de Greck, & Northoff, 2011) have confirmed that cognitive empathy and emotional empathy are distinct but related components of empathetic behavior that are supported by unique brain networks. We note further that, in line with the previously mentioned work of Lindquist et al. (2012), we are not necessarily interested in activity in a particular brain region or structure. Rather, neuroscience findings indicate that emotional empathy originates primarily in a network of automatic brain systems, whereas cognitive empathy relies heavily on controlled brain processes. Both types of empathy interact to produce other-oriented feelings and behavior (Shamay-Tsoory, 2011).

Indeed, adopting a multidisciplinary approach that includes neuroscience also has broader implications for organizational research. In the example that we discussed earlier, emotional empathy is closely related to emotional contagion, and neuroscience methodology may finally provide a reliable means of investigating contagion processes in organizational settings (Barsade, 2002). Mirror neurons also appear to play a role in contagion and emotional empathy, and brain imaging technologies allow scholars to investigate the role of these processes directly in organizational phenomena (Rizzolatti & Fabbri-Destro, 2008).

Conclusion

In conclusion, we remain optimistic that neuroscience will revolutionize organizational research in ways that we cannot fully anticipate at this point. These advances will certainly occur in both theory and practice. We temper

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our long-term optimism with a prudent appreciation for and understanding of the rapid development and current limitations of the field of neuroscience. We also believe that organizational scholars have an important role to play in shaping the ethical and practical development of neuroscience technologies. We can however only play this role if we heed the lessons of past "management fads" and become knowledgeable and engaged in the science behind the fad.

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