

# A CRITIQUE ON NEUROSCIENTIFIC METHODOLOGIES IN ORGANIZATIONAL BEHAVIOR AND MANAGEMENT STUDIES

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## Abstract

Organizational neuroscience continues to flourish in organizational behavior and management studies as indicated by the growing number of publications. However, with a few exceptions, substantive critiques of organizational neuroscience are conspicuous by their absence. In this point-counterpoint article, we aim to redress this imbalance. We do so by asking two significant yet neglected questions: (i) how strong is the science behind this domain, and (ii) why we are doing this type of research (the *so what?* question)? Our analysis shows that the science behind organizational neuroscience is far less rigorous than currently advocated (due to low statistical power of some neuro-imaging studies plus an inability to locate mental phenomena precisely in the brain). In terms of the *so what?* question, we encourage researchers to move away from general statements and become more specific about the phenomena they research. We contend that the practical implications of this research, as well as inferences of the link to behavioral changes, are currently overstated. We also underscore the importance for these studies to become contextually sensitivity in order for the researchers to capture important events in the workplace. Finally, we offer some suggestions for future research.

**Keywords:** Organizational neuroscience, low power, small samples, weak management theories, generalizability

## Introduction

Organizational neuroscience is swiftly consolidating its influence in organizational behavior and management studies (Ashkanasy, 2013; Balthazard, Waldman, Thatcher, & Hannah, 2012; Becker & Cropanzano, 2010; Becker, Cropanzano, & Sanfey, 2011; Cropanzano & Becker, 2013; Laureiro-Martínez, Brusoni, Canessa, & Zollo, 2014; Scherbaum & Meade, 2013; Senior, Lee, & Butler, 2011)<sup>1</sup>. This development has captured the imagination of practitioners as well, who often invoke references to ‘science’ or ‘studies’ in order to show that many important phenomena in the workplace, such as leadership or learning and development, can be better understood and manipulated through neuroscientific theories and methods (Balthazard, 2011; Ringleb & Rock, 2008; Stuart, 2014). To date, this excitement around neuroscience research has advanced largely unrestrained, with only some ethical and philosophical critiques emerging (Healey & Hodgkinson, in press; Lindebaum, 2013a, b; Lindebaum & Zundel, 2013).

In this article, we argue that the rapid adoption of organizational neuroscience, defined as “a deliberate and judicious approach to spanning the divide between neuroscience and organizational science” (Becker & Cropanzano, 2010, p. 1055), has neglected two key questions. Therefore, we urge researchers to take a step back and to answer two basic questions that are paramount importance in all social science. That is, (i) how strong is the science behind the discoveries we have made to date, and (ii) why we are doing this type of research (the *so what?* question)? These two questions provide the organizing framework for the remainder of this article. To briefly explain,

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<sup>1</sup> For consistency, we employ the term *Organizational Neuroscience* (Becker et al. 2011) only, while acknowledging the complementary perspective of *Organizational Cognitive Neuroscience* (Senior et al. 2011). While for some research problems or questions some researchers argue it is necessary to clearly distinguish them, for our purpose “all of the various approaches face a fundamental question . . . what do brain scans really tell us?” (Lee et al. 2012a, p. 924).

the rapid expansion of organizational neuroscience research without a rigorous process of establishing the *so what?* question requires us to examine the science first to better grasp what this science can offer to the study of organizational behavior and management phenomena, and if there is any way to establish a clear *so what?* question. While this may not seem logical to some, there are various fields in which the *so what?* has eventually caught up with the science. For instance, in creativity solutions often find problems that only become evident subsequently (e.g., 3M's discovery of post it notes) and the same happens in medical science (e.g., the discovery of penicillin). Unfortunately, however, we are not quite as optimistic with the domain of organizational neuroscience, so let us turn to the science of organizational neuroscience.

### **THE SCIENCE BEHIND ORGANIZATIONAL NEUROSCIENCE**

Our ingenuity regarding technological developments, as well as the “increased accessibility of the expertise, tools, machines and technology” has seemingly transformed organizational neuroscience from a futuristic ideal to a real possibility whose feasibility has, arguably, “never been greater for management researchers” (Scherbaum & Meade, 2013, p. 139). Studying the brain with a view to understanding human behavior has long been an ideal of social scientists (Oppenheim & Putnam, 1958). Brain imaging techniques, especially but not exclusively functional magnetic resonance imaging (fMRI, the 'gold standard of neuroimaging data' see Cui et al., 2011), now afford seemingly detailed pictures of brain processes. Researchers have argued that this information will influence a broad range of theoretical insights and practical applications. For instance, scholars in the domains of leadership (Ashkanasy, 2013; Waldman, Balthazard, & Peterson, 2011a), strategic decision-making and

intuition research (Akinci & Sadler-Smith, 2012; Hodgkinson & Healey, 2011), or marketing (Bagozzi et al., 2012; Hedgcock & Rao, 2009) suggest that a range of theoretical insights can be gained by turning to organizational neuroscience. In terms of practical management applications, leadership researchers have been vocal in highlighting the putative benefits of organizational neuroscience in the selection and development of effective leaders (Waldman et al., 2011a), with commercial interest in this research increasing at similar speed (Balthazard, 2011; Stuart, 2014).

In establishing the science behind neuroscience, there are two crucial issues we wish to examine. These issues are primarily methodological in nature, and raise questions about neuroscience research that most social scientists would have difficulty accepting in other organizational behavior or management studies. The two issues are (i) low power in existing neuroscientific studies leading to questions over generalizability of results, and (ii) fundamental inability to localize phenomena in the brain (i.e., we really do not know where to find exactly brain correlates of mental phenomena). We will address each of these in turn.

### **Low power and inability to generalize results**

A close examination of general neuroscientific studies using fMRI data leaves one clear impression; that is, these studies are based upon very small samples sizes (Barch & Yarkoni, 2013; Button et al., 2013)<sup>2</sup>. Some researchers in the neuroscience field support the use of samples of 12 to 20 subjects and recommend very liberal

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<sup>2</sup> Note that a detailed discussion on fMRI methodology and data is outside the scope of this article. Introductory and advanced treatments of this topic can be found elsewhere (Lindquist, Wager, Kober, Bliss-Moreau, & Barrett, 2012; Logothetis, 2008). While studies using Electroencephalography (or EEG) technology tend to have larger samples sizes (Balthazard et al. 2012), as a data-generating technique it is not without limitations, especially in terms of locating where activity in the brain occurs (see e.g., Cropanzano and Becker, 2013).

statistical thresholds be employed to attain sufficient statistical significance (Desmond & Glover, 2002). Recently, however, other researchers have questioned these recommendations and argued they are inadequate for a number of methodological reasons (see Barch & Yarkoni, 2013, for more details). Robust findings from a recent meta-analysis published in *Nature Reviews Neuroscience* (Button et al., 2013) demonstrate that research using fMRI data produces results with very low statistical power. In line with Barch and Yarkoni (2013), Button and her colleagues (2013) attribute this to the use of small sample sizes in the research. In fact, Button's meta-analysis suggests that, in neuroimaging studies with humans, the median statistical power was only "8% across 461 individual studies contributing to 41 separate meta-analyses" (p. 369). They argue that low statistical power appears to be an "endemic problem in neuroscience" (Button et al., 2013, p. 365). These possible consequences are that (i) the detection of a 'true effect' is unlikely, (ii) that effect sizes are often overestimated and (iii) that reproducibility of studies is low.

It is for these reasons that researchers are unable to draw generalizations from these data. Indeed, even if researchers were to attempt replication studies they would be bound to fail in the presence of underpowered fMRI studies (Barch & Yarkoni, 2013; Button et al., 2013). Taken together, this situation sensitizes the neuroscientific community to the methodological issues arising from running fMRI studies with small samples and its concomitant low statistical power (see Yarkoni, 2009). Neuroscientists have explored avenues to combat this problem, for example, by using data aggregations across studies to enlarge sample sizes (even though this carries logistic and ethical problems, see Barch & Yarkoni, 2013).

However, the domain of organizational neuroscience has not caught up yet with this development. In fact, a great deal of contemporary theorizing in this domain

is seemingly based upon prior empirical fMRI studies conspicuous by small sample sizes. For instance, the pioneering article by Becker et al. (2011) draws upon evidence from a number of empirical neuroscientific studies with small sample sizes, ranging from 1 to 30 ( $M=14.2$ ,  $SD=7.70$ ) in studies where the sample size and method could be clearly identified. While the Becker et al. (2011) study primarily draws on secondary sources, a more recent empirical contribution is provided by Bagozzi et al. (2012), who draw on a sample of 24 salespeople in a neuroscientific study. Likewise, in another study using fMRI data by Bagozzi et al. (2013) draw on samples of 43 and 24 individuals, respectively. A notable outlier in terms of sample size is a recent fMRI study on decision-making performance, with an  $n$  of 63 (Laureiro-Martínez et al., 2014). While this seems large in from a neuroscience perspective, from an organizational behavior perspective the associated power of such a study is likely to be low (for a discussion of this see Funder et al., 2014). Most quality journals in this domain would not accept samples of this size as providing sufficient power on which to establish valid and reliable findings. Indeed, from our reading several of these studies fall well below the recommended sample size criteria set by the Central Limits Theorem to discuss generalizability or broader application of the results, as we have discussed earlier. Hence, far from producing ‘hard data’, the study by Button et al. (2013) suggests that the findings of neuroscientific studies may introduce ambiguity - rather than a clear direction for future research. Perhaps somewhat ironically, it is the reference to increased rigor associated with neuroscientific data that propels much scholarly efforts in terms of theorizing and empirical testing in the domain of organizational neuroscience (Becker et al., 2011; Waldman, Balthazard, & Peterson, 2011b).

However, beyond a general concern about small sample sizes as a contributor to low power, Button and colleagues (2013) also identify the usage of mixed analytical strategies. These include discrepant definitions of key variables, the statistical model used, any adjustments incorporated (or lack thereof) to account for potential confounding factors, and the use of certain filters to exclude some observations from the analysis. Especially in small samples studies, this can generate different estimates of an effect size depending upon the analytical choices made. Even minor manipulation can readily change results of a study (Button et al., 2013). In this respect, a recent examination of 241 fMRI studies (Carp, 2012) reports 223 unique analytical strategies were employed, so that almost no analysis was used more than once. Carp (2012) argues that the essence of good scientific method is the ability to replicate findings, but notes that this essential element is not available and that “many studies were underpowered to detect any but the largest statistical effects . . . [and that] data collection and analysis methods were highly flexible across studies” (p. 289). Carp (2012) goes on to say that in many cases, the actual data collection methods and the types of statistical analyses were not fully reported. Clearly, if neuroscience and, in particular, organizational neuroscience wants to portray itself as a hard science, then it needs to report these details, and calls to this effect are accumulating (Barch & Yarkoni, 2013; Carp, 2013). Otherwise, we can only concur with Button and her colleagues (2013) that “as unreliable research is inefficient and wasteful” (p. 365).

In summary, there appears a case to argue that current neuroscientific studies suffer from low power and often lack a rigorous methodological framework for data collection and analysis that would enable replication. This becomes an even more

important issue when we are discussing our next area of concern, which concerns a fundamental inability for researchers to localize phenomena in the brain.

### **A fundamental inability to localize phenomena in the brain**

In most endeavors, human beings have a desire to seek out simple solutions. It is a part of the attraction of Occam's razor – simple solutions are the best. Neuroscientific approaches speak to and reflect this desire, for they seemingly grant organizational behavior and /management scholars the confidence to emulate the natural sciences (with their use of 'hard' and 'objective' data), thereby reducing the margins of error and the levels of ambiguity that persist in more traditional research designs (Akinci & Sadler-Smith, 2012; Waldman et al., 2011b). Thus, these methods are frequently portrayed as the 'proper methods' to cope with the increasing complexity of business practices (e.g., Herrmann, 2005). Specifically, advocates repeatedly refer to concrete examples linking specific mental phenomena to specific brain regions or neural networks. For instance, Becker and colleagues (2011, p. 934) argue that neuroscience can "elucidate *particular* networks of brain systems and processes responsible for workplace attitudes and behaviors" (p. 934, italics added). Elsewhere, Becker and colleagues (2011) invoke the example of envy as involving "a distinct neural network including the striatum, the amygdala, the anterior cingulate, and the insula" (p. 936). Similarly, Bagozzi and colleagues (2012) hypothesize that individuals with higher levels of customer orientation, will show more coordinated activation of the insula and amygdala regions of the brain. In the same hypothesis, they also predict that sales orientation will have no significant relationships with the activation of the brain in the same regions (for similar formulations, see Bagozzi et al., 2013; Boyatzis, 2011). There are two broad issues that emerge from such



hypotheses: (i) the ability for researchers to locate activity in the brain in relation to specific orientations, and (ii) the use of (and accepting) a null-hypothesis to draw conclusions. We will address with each in turn.

Some researchers want to break down the complexities of the human brain into more manageable sub-regions in order to better understand the ambiguous or paradoxical findings in social science with a single unit of analysis: the human brain or specific regions therein. In psychological studies, the tendency to link specific mental phenomena to specific brain regions or neural networks is referred to as the locationist approach. This approach hypothesizes “that all mental states belonging to the same emotion category (e.g., fear) are produced by activity that is consistently and specifically associated with an architecturally defined brain locale . . . or anatomically defined networks of locales that are inherited and shared with other mammalian species” (Lindquist et al., 2012, p. 122). For instance, research has suggested that the amygdala is central, following sensory input, in establishing fear responses (LeDoux, 2000). The locationist approach posits that the amygdala is either the most crucial hub in the fear circuit, or the brain center of fear (Lindquist et al., 2012).

A recent meta-analysis questions the decreased complexity argument put forth by advocates of organizational neuroscience (Lindquist et al., 2012). These scholars challenge the widely held view among organizational behavior and management scholars that discrete emotion categories can be consistently and specifically linked to distinct brain regions, an assumption adopted by several management researchers in undertaking neuroscientific studies (see above). Instead, Lindquist and colleagues (2012) provide meta-analytic support for the psychological constructionist hypothesis, which states that discrete emotion categories are made up of more general brain networks, rather than specific locales (see also Lieberman, 2007). Specifically,

Lindquist and colleagues (2012) report evidence that discrete emotion categories (e.g., fear) emerge from more general brain networks (i.e., mental states associated with a particular emotion category (e.g., fear) are not specifically and persistently linked to a specific brain locale). Instead, Lindquist and colleagues (2012) show that large-scale networks essential to brain functioning interact in the production of psychological events. What is really of interest here is “that emotion categories such as anger, sadness, fear, and so forth, are common sense categories whose instances emerge from the combination of more basic psychological operations that are the common ingredients of all mental states” (p. 125). This suggests that basic psychological operations are often common across a range of diverse brain functions, to the extent that the Lindquist et al. study (2012) shows that “categories like *emotion*, *cognition*, and *perception* are not respected by the brain” in terms of specific locations (p. 139 italics in original).

Compelling experimental evidence exists to underpin the psychological constructionist hypothesis. A study by Feinstein and colleagues (2013) demonstrates that the amygdala, in contrast to prior assumptions, is not always essential in eliciting fear or panic. Using a sample of three patients with a rare bilateral amygdala damage (some of whom have never experienced fear before due to the damage), these researchers found that the inhalation of 35% CO<sub>2</sub> not only induced fear, but also caused panic attacks. In consequence, the researchers stress that an important distinction must be made between fear evoked by external events (as taken in via sensory pathways) and fear induced internally through inhalation of CO<sub>2</sub>. Importantly, this study does not necessarily invalidate prior studies that focus on the role of the amygdala in influencing emotional states. Rather, it underscores that considerably

more complex networks may be at play in producing this major emotional state than is currently appreciated.

The conclusion one may draw from this evidence is that there are a number of different processes involved in the production of a specific mental state (e.g., being fearful). This effect has been termed ‘multiple realizations’ in the philosophy of science literature (Putnam, 1979), which essentially argues that distinct processes can lead to identical outcomes. Second, the presence of more generalized (as opposed to specific) neural network suggests that research focusing on activation in one location of the brain may not explain the behavior it purports to explain. Indeed, the inference is that brain regions not captured in the research may be essential to capturing the mental processes that create specific behavior or reactions, such as the panic attacks in the Feinstein et al. (2013) study show.

The presence of more general brain networks has significant implications for deciding upon a specific and distinct brain correlates of mental phenomena in brain imaging studies. Since researchers currently assume locations for phenomena (e.g., in the context of studies on inspirational leadership or Machiavellianism, see Bagozzi et al., 2013; Boyatzis, 2011; Waldman et al., 2011a), we argue based upon the findings of this meta-analysis that a focus on specific locations is far less likely to produce useful knowledge. Importantly, this finding must be considered in light of injunctions that

“the use of any brain imaging or activity measure requires *strong theory* to guide hypotheses about where activity related to a specific construct or process should be occurring in the brain. Without strong theory, activity occurring in a number of

areas of the brain for a number of reasons can be difficult to interpret. In some cases, activation can occur in areas of the brain that were not anticipated” (Scherbaum & Meade, 2013, pp. 137-138, italics added).

The need for strong theoretical underpinnings as highlighted in the above quote is predicated upon sufficient knowledge to precisely locate specific brain regions and to build a model that can link this to specific behavior. It is noteworthy that problems associated with precisely identifying specific mental phenomena as residing in clearly identifiable brain regions are a recurrent theme in the literature (Lindquist et al., 2012; Vul, Harris, Winkielman, & Pashler, 2009). In addition, Lee et al. (2012a, p. 925) highlight that the extent of interaction between different parts of the brain is vast: “with a number of regions throughout the entire brain operating together in a hierarchical fashion for both single, commonplace processes such as face perception and for more complex processes such as social reward”. This brings us to our second issue: that is, drawing conclusions from non-significant results.

Some researchers also use the inverse argument. Indeed, they infer that the inability to locate activity in specific brain regions can also be linked to specific behavior. Drawing conclusions from non-significant findings is a basic error that most doctoral programs emphasize does not lead to good science. Despite this, Bagozzi et al. (2012) develop hypotheses that sales orientation will have no significant relationships with the activation of the brain in specific regions and then draw conclusions from those non-significant results. Even advocates of organizational neuroscience caution that “with the data we have in such cases [i.e., in brain imaging studies], it is impossible to infer a *lack* of involvement in a region which is not shown

as activated”, adding that “the amount of activity detected in X does not necessarily directly correspond to how important region X is for that task” (Lee et al., 2012a, p. 925, italics in original). This, we argue, raises major concerns over the initial development of hypotheses based upon finding no relationship and the subsequent interpretation of non-significant findings reported in some neuroimaging studies (Bagozzi et al., 2012)<sup>3</sup>. Given the intricacy of disentangling how activation (or lack thereof) in one or more brain regions is associated with a particular task, research based upon these methods and statistical analyses are prone to reflect spurious findings (Vul et al., 2009). Now that we have established our skepticism over the science behind current neuroscientific findings, we turn our focus to the ‘so what?’ question for neuroscience.

### **WHAT IS THE *SO WHAT?* FOR NEUROSCIENCE?**

Organizational neuroscience is popular judging by the number of publications in top-tier organizational behavior and management journals (Bagozzi et al., 2013; Becker & Cropanzano, 2010; Laureiro-Martínez et al., 2014; Lee et al., 2012a). As mentioned earlier, we believe part of the reason for the popularity of neuroscience is that it seemingly emulates the natural science by examining ‘objective’ data derived from brain scans. So far we have expressed concerns over the validity and reliability of these ‘objective’ data. Wastell and White (2012) express similar concerns and recently cautioned that brain scans seemingly “shout science” (p. 406), especially into the ears of practitioners.

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<sup>3</sup> While reporting non-significant results can have merit in social science research if done correctly (Kluger & Tikoichinsky, 2001), in the context of neuroscience, we contend this is problematic. This is because it is not logical to conclude that a given brain region *not* showing a statistically significant degree of activity is *significantly implicated* in generating a significant degree of activity in another region (see Lee et al., 2012a).

From an organizational behavior and management perspective, researchers have argued that introducing this element of objective data has the potential to reduce the margins of error and the levels of ambiguity that persist in more traditional research designs (see e.g., Akinici & Sadler-Smith, 2012). It is suggested that, through neuroscience, we can develop “basic processing models that can be used to generate *better* predictions about individual and group performance” (Paulus et al., 2009, p. 1085, italics added, but see also Vul et al. (2009), for a provocative treatment of this argument), thus gaining enhanced methodological rigor compared with traditional questionnaires, which are subject to several perception biases (see also Waldman et al., 2011b).

Specifically, the alleged rigor is attained by virtue of neuroimaging techniques avoiding reliance upon individuals’ self-reports, since they measure *all* brain processes, whether they are conscious or unconscious (Becker et al., 2011). Neuroscientific imaging technologies, therefore, capture both ‘controlled’ as well as ‘automatic’ processes of social cognition (Lieberman, 2007). In consequence, neuroscience promises to render the *invisible visible*, insofar as unconscious processes can be readily made evident in ways that normally eludes traditional modes of data collection. Therefore, some researchers argue that neuroscience can explain paradoxical findings that social scientists have grappled with for some time (see, e.g., Bagozzi et al., 2013).

In an attempt to answer the *so what?* question, we note two important general tendencies which need to be overcome. The first is the use of ‘motherhood’ type statements to justify the research and the second is for researchers to ignore establishing practical outcomes from this research. We note many researchers articulate ‘motherhood’ type statements; that is, statements which we argue lack

specificity in order to justify the research. For instance, Scherbaum and Meade (2013) argue that methodologies such as those used in neuroscience can be an “avenue for expanding paradigms and theories in the management sciences” (p. 133). Likewise, Becker and Cropanzano (2010) suggest the incorporation of neuroscientific themes and insights can lead to enhancement of “new and existing theories of organizational behavior” (p. 1055), reflecting an ambition amongst advocates of a “more biologically informed view of business and organizations” (Senior et al., 2011, p. 813). We note that similar statements were made around the time emotional intelligence emerged as a construct that generated interest from both academics and practitioners. In the case of emotional intelligence, the ‘motherhood’ statements involved suggested that emotional intelligence accounts for up to 80% in the variance in life success (Goleman, 1995). The magnitude of these claims was subsequently rebutted by meta-analytic evidence (O’Boyle, Humphrey, Pollack, Hawver, & Story, 2011; van Rooy & Viswesvaran, 2004). What we infer from these motherhood statements around organizational neuroscience is currently more hope than focus.

Resolving this deficiency is, however, not straightforward. What we need is more concrete statements about the links between brain correlates and mental phenomenon based upon well considered theoretical frameworks. However, to date we have rarely seen evidence of researchers linking neuroscience to specific phenomenon without falling into the reductionist trap<sup>4</sup>. In addition, considerable research efforts (such as the Human Brain Project) seek to better understand how neurons of particular brain regions typically connect in order to establish a

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<sup>4</sup> It is notable that some neuroscientists themselves are troubled by purely reductionist approaches to the study of brain. Specifically, Cahill and colleagues (2001) caution against an exclusive focus on reductionist approaches to neuroscientific research at the expense of holistic advances, arguing that “the problem occurs when neuroscientists assume the superiority of one approach over the other” (p. 576).

comprehensive computer model. And yet, as Tretter cautions in a recent interview (in von Lutterotti & Stallmach, 2014), a guiding theory to this end is woefully lacking at the present moment, implying that progress is heavily contingent upon ‘trial-and-error’ approaches. Of course, more concrete theoretical development in neuroscience may render our concerns irrelevant at some point, but we have not seen this to date in organizational neuroscience research.

This leads to the second aspect; if we do finally establish a link – what do we do about this? In other words, what are the practical implications of this research? There are several nuances to this argument, and the best way to explain these is by example. In organizational behavior and management studies, if we are researching Organizational Citizenship Behaviors or Commitment, we are aware of techniques to encourage these by modeling and rewarding such behavior. Indeed, advocates have recently suggested that these behaviors could be addressed in targeted interventions (Scherbaum & Meade, 2013). Proponents of neuroscience (e.g., Balthazard, 2011; Rock, Siegel, Poelmans, & Payne, 2012; Waldman et al., 2011a) argue that, with the aid of neuroscience, we can use brain plasticity to retrain our brains. Indeed, this is suggested in a recent neuroscientific study of inspirational leadership. Here, Waldman et al.’s (2011a) suggests that, following a series of neurofeedback sessions, one participant (who was weak in managing his anger), was able to “rearrange neuro-pathways in the affected area, create new pathways with healthy neighbouring neurons, and largely correct the problem . . . He was able [as a result] to become a more effective leader” (p. 69). This statement is made without the description of any clear pre-post testing regime. Instead, the authors use a slight of hand argument (i.e., therapy sessions suddenly turn into neuro-feedback sessions), and a reductionist argument about the impact of anger on the individual’s ability to lead. They then go



on to offer the inference that by controlling anger the individual turned into an effective leader (without any explanation of the construct definition of 'effective').

Our reading of the literature suggests that brain plasticity applies more to addressing neural deficiencies in developing brains in childhood and brains damaged through trauma rather than altering behavior in adult brains (Bavelier, Levi, Li, Dan, & Hensch, 2010). In other words, neuroplasticity occurs following trauma or damage to the brain because the original pathways have been damaged or destroyed. For the individual to regain functions managed by damaged parts of the brain, the brain has no other alternative than to find alternate paths to enable these functions to be managed. In this case, the brain rewires to alternate pathways to allow the function to be regained. Therefore, neuroplasticity does not occur because of a desire by an individual to become an inspirational leader. In addition, it should be borne in mind that radical behavioral changes – such as turning an un-inspirational leader into an inspirational one, see Waldman et al. 2011a), have been a source of significant debate in the leadership field for some time (Bass, 1999; Lindebaum, 2013b). Based upon the evidence provided, we argue that more research is required before we can look to neuroplasticity as the solution for the *so what?* question.

As a further nuance of *so what?* question, we note considerable excitement about what fMRI scans tell us about various phenomena at work, especially with regard to leaders or decision-makers (Boyatzis et al., 2012; Laureiro-Martínez et al., 2014). But can fMRI data reveal the difference between distinctly successful leaders, such as Mahamda Mahatma Ghandi, Winston Churchill, Steve Jobs or Al Dunlap? All had different skills which - when linked to their respective context (i.e., situational and historical context) - made them the right person for the job. In a similar vein, Wastell (2013) refers to the complex interplay of how group members work together

to accomplish the task set by a leader, and he adds the pithy injunction to show “[him] that on a brain scan”. The central question then becomes: what can neuroscience offer us to understand this diversity of leadership behaviors? Is it really the brain alone that determines the success of these individuals, or is it a complex combination of personality, context, opportunity, intelligence, emotion regulation and sometimes just plain luck that determines how successful these leaders were?

### **Implications for theory and practice**

We began this article by highlighting two questions concerning existing neuroscientific studies. The first question was related to the scientific merit behind existing studies. Based upon our scrutiny of this recently emerging body of knowledge we argue that, far from producing ‘hard data’, fMRI data may introduce ambiguity rather than increased rigor and decreased complexity. The second question centered upon the *so what?* in terms of incorporating neuroscientific theories and measurements into organizational behavior and management studies.

In terms of scientific merit, if neuroimaging studies are accompanied by a low probability to detect an effect that is actually true (Button et al., 2013; Vul et al., 2009), in addition to an inability to precisely locate mental phenomena in the brain (Lindquist et al., 2012), then management scholars and practitioners need to be wary not to generate *weak management theory* with findings from unreliable empirical research. Based upon our scrutiny of this recently emerging body of knowledge we argue that, far from producing ‘hard data’, fMRI data may introduce ambiguity rather than increased rigor and decreased complexity. This call for caution stands in direct opposition to the current excitement being generated about organizational neuroscience, which emphasizes the benefits of organizational neuroscience in the

development and extension of management theories (Becker et al., 2011; Lee, Senior, & Butler, 2012b; Scherbaum & Meade, 2013).

In terms of the so-what question, we have highlighted that one key appeal of using neuroscientific methodologies is related to brain plasticity (i.e., the notion that the human brain is capable of learning by way of neuroscientific interventions, even in adulthood). And yet, how this can translate into significant behavioral changes (e.g., turning an un-inspirational into an inspirational leader) is a matter far from being conclusively resolved. In addition, the *so what?* question gains prominence in relation to the inability of brain scans to capture dynamic, reciprocal, relational and recursive interactions between individuals in the workplace, and how this contributes to individual and group effectiveness at work (Fairhurst, 2009; Fairhurst & Uhl-Bien, 2012; Lindebaum & Zundel, 2013).

We do not suggest that all prior neuroimaging studies have no scientific merit. Instead, the analysis provided here and the arguments we have developed underscore the need for researchers to ensure (i) that studies are designed with sufficient sample sizes (and concomitant power) to enable generalizability of the results, that (ii) studies are able to describe the methods in sufficient detail (including locations in the brain) to allow for replication of their work, and that (iii) there is an adequate *so what?* question linked to each study that would allow practitioners to apply the research findings to applied settings. It is from the position, then, that scholars can start to explore more accurately opportunities for theory development, including organizational behavior and management studies. After all, we ought to be mindful of the ‘bad’ consequences for management practice if unreliable empirical data are relied upon to produce weak theoretical frameworks.

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