
Critical Essay: Building new management theories on sound data? The case of neuroscience

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Abstract

In this critical essay, I contend that accelerating *demands* for novel theories in management studies imply that new methodologies and data are sometimes accepted prematurely as *supply* of these novel theories. This point is illustrated with reference to how neuroscience can inform management research. I propose two demand forces that foster the increased focus on neuroscience in management studies, these being (i) the direction of public research funding, and (ii) publication bias as a boost for journal impact factor. Looking at the supply side, I note that (i) the statistical power of studies using functional magnetic resonance imaging (or fMRI, the ‘gold’ standard) is unacceptably low, (ii) the use of imprecise ‘motherhood’ statements, and (iii) the dismissal of ethical concerns in the formulation of management theories and practice informed by neuroscience. I then briefly outline the bad consequences of this for management theory and practice, emphasize why it is important to prevent these consequences, and offer some methodological suggestions for future research.

Keywords

management, methods, neuroscience, practice, theory

Introduction

Ongoing debates within *Human Relations* and beyond reflect a keen interest among management scholars in the future of theory building (e.g. Cornelissen and Durand, 2014; Healey and Hodgkinson, 2014; Lindebaum and Jordan, 2014b). Recently, however, some scholars have expressed concern about the current state of building theories

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in the management literature, thereby indirectly increasing demands for novel theories. Writing in *Academy of Management Review*, for instance, Devers and colleagues (2014: 248) suggest that ‘we appear to be moving toward a future where theory is of less importance’. In a similar vein, Alvesson and Sandberg (2013: 128), writing in the *Journal of Management Studies*, lament that there is a ‘troubling shortage of novel ideas . . . within management studies’. Suddaby and colleagues (2011) would concur that this dearth of novel theory affects management research more broadly speaking. For instance, they argue that the simplicity and static nature of extant management theories implies that dynamic changes in the complexity and size of modern organizations are difficult to capture.¹ From a theoretical perspective, I agree that this simplicity and static nature can pose challenges, for instance, when we fail to better understand ever more dynamic and frequently changing organization–employee relationships (in neglected areas such as multiple jobholding, see Sliter and Boyd, 2014), or in terms of not fully understanding within-leader behavior variance in relation to situationally appropriate leadership (Jordan and Lindebaum, in press).

Owing to the influential standing of the journals that published the articles by Devers et al. (2014) and Alvesson and Sandberg (2013), it is plausible to suggest that their articles calling for novel management theories and ideas can create a certain demand whose supply is not (yet) met. This directly relates to the motivation of this essay; that is, when perceptions of the *demand* side reach a given magnitude, it can potentially create an atmosphere where new theory – especially when based upon novel yet problematic data – may be accepted too readily in order to ensure the necessary *supply* to meet this demand. For some scholars, this lies at the heart of the scientific project: that is, ‘social scientists . . . will naturally want to underpin [i.e. supply] their theories with more empirical data’ (Runkel and Runkel, 1984: 130).

For illustration, I discuss how neuroscience, particularly functional magnetic resonance imaging data (fMRI, the ‘gold standard’ of neuroimaging techniques, see Cui et al., 2011), can inform management studies. The combination of these two disciplines has given rise to the new domain of organizational neuroscience, briefly defined as ‘a deliberate and judicious approach to spanning the divide between neuroscience and organizational science’ (Becker and Cropanzano, 2010: 1055). However, I speculate that the case of neuroscience may merely be indicative of a larger phenomenon in social science, where sometimes demands for theoretical advances influence theory-building efforts that, over the years, are increasingly questioned by empirical findings (i.e. the supply). This very development can also be observed in the literature on boundaryless careers (see Rodrigues and Guest, 2010).

Before I proceed, I would like to briefly offer four important clarifications. First, while Kanter (2005) focuses upon the ‘demand’ side of ‘bad theories’, Ghoshal (2005) focuses upon the ‘supply’ side of it. However, I appreciate both demand and supply forces in relation to novel theories in management studies that are informed by neuroscience. For organizing purposes, I treat demand and supply as distinct forces, whilst not neglecting their cyclical interaction (Kanter, 2005).

Second, consistent with previous writers on the topic (Ghoshal, 2005; Kanter, 2005), I employ the terms ‘demand’ and ‘supply’ in this essay. However, instead of a perhaps more restrictive economic interpretation of these terms, the reader will encounter several

occasions where I (or indeed others) are explicit in recognizing ‘agency’ aspects of researchers’ activities.

Third, it is germane to indicate what constitutes ‘good’ or ‘bad’ management theory in the context of this essay. Precisely, I adopt an ‘industry’ standard of how theory can be defined, namely, as a ‘system of constructs . . . in which the constructs are related to each other by propositions’ (Bacharach, 1989: 498). While perspectives can differ on what constitutes theory (Sutton and Staw, 1995; Weick, 1995; Zundel and Kokkalis, 2010), adopting the above-mentioned definition helps demonstrate that, in terms of supply, neuroscience often struggles to measure up to this standard (i.e. owing to the operationalization of ‘constructs’ based upon problematic data). At the same time, it remains popular for demand-related reasons. In addition, what constitutes ‘good’ or ‘bad’ management theory raises ethical aspects too, especially when these aspects matter little in the formulation of management theory as espoused by empiricists or rationalists. Thus, I concur with Suddaby’s (2014) exegesis of normative theories to indicate what ‘good’ theory might look like. Here, ethics and values of actors are appreciated in management theories as an engine that constructs reality, rather than a camera that captures it (MacKenzie, 2006). This differentiates normative theorists sharply from empiricists and rationalists, the latter two harbouring the assumption of an ‘objective distance between research and practice and who believe that their research is largely free of political and moral assumptions’ (Suddaby, 2014: 2). Interestingly, Suddaby (2014: 2) adds that normative theorists oftentimes ‘contradict the (often dismal) view of the world given to us by both empiricists and rationalists’. This clarification has particular bearings on the discussion on the dismissal of ethical concerns in the formulation of new theories in management (i.e. as part of the discussion on the supply side).

Fourth, I disagree with the contemporary advocacy of neuroscience in management studies (e.g. Becker et al., 2011; Senior et al., 2011; Waldman et al., 2011) for a number of ethical, logical and empirical reasons (as detailed in Lindebaum and Jordan, 2014a; Lindebaum and Zundel, 2013). At the same time, I am heartened that more balanced (and sometimes sceptical) views on the merit of non-clinical applications of neuroscience in management are emerging (Tracey and Schluppeck, 2014), or sociological inquiries on what kinds of issues arise as neuroscientific data is produced (Pickersgill, 2013). However, compared with the main thrust of advocating articles, these studies pale in numbers.

In the remainder of this essay, I first identify further *demand* forces closely linked to the domain of organizational neuroscience in addition to general demands for novel management theory already detailed. These forces are described in terms (i) direction of public research funding, and (ii) presence of publication bias to boost journal impact factors. Second, I underscore that the *supply* side of management theories currently informed by neuroscientific methods and data (especially fMRI data) is affected by (i) the usage of invalid data/discrepant analytical strategies, (ii) imprecise theoretical statements, and (iii) a dismissal of ethical concerns in the formulation of theories and practice in management. The way that research institutions, funding organizations and individual scholars help produce and reproduce these demand and supply forces then leads to the emergence of ‘bad’ consequences for management theory and practice (e.g. when it is assumed that method *X* is the most effective tool to recruit key personnel when, in fact, it is not). In

light of these persisting problems, I close this essay by offering some suggestions for future research to help limit the bad consequences for management practice already mentioned above.

Characteristics of the demand side

The question as to *why* there is such a receptive audience in relation to neuroscientific theories and methods as a conduit to inform new management research is a relevant and under-explored one. In this essay, I argue that there are at least two relevant demand factors.

First, if Duster (2006: 1) is correct, public funding to support key research priorities is increasingly channeled toward 'reductionist science for which partial evidence is found in the strikingly imbalanced allocation of research funding for "causes" of wide-ranging problems' and an 'expansion of databases on markers and processes "inside the body"'. Consistent with this, several neuroscientific advocates in management studies have proclaimed 'a biological turn in order to understand the underlying processes concerning markets and organizations' (Butler, 2014: 720) or desire for a 'more biologically informed view of business and organizations' (Senior et al., 2011: 813). Empirical studies reflecting these sentiments have already been published, such as genetic and neurological foundations of customer orientation (Bagozzi et al., 2012) or the quest for a leadership 'gene' (while not neglecting environmental factors, see De Neve et al., 2013).

Further indicators of this trend emerge from President Obama's *Brain Initiative* announced in April 2013, which allocated \$100m worth of research funding to five federal research agencies (White House, 2014), with contributions from commercial organizations, universities or foundations exceeding Obama's pledge significantly. Likewise, the EU-funded flagship project the *Human Brain Project* has secured funding in excess of €1b in 2013² in order to develop the necessary technology to create a computer simulation of the brain. At around the same time in the USA, funding for the Humanities seems to have decreased progressively since 2009, and in 2011, it amounted to less than 0.5 percent compared with grants dedicated to science and engineering research and development (Delany, 2013). It is for this reason that the availability of funding can be seen as a legitimate demand force that exercises influence over which research projects are funded. As Waldman (2013: 2) notes, it is his 'experience that grant agencies and foundations increasingly seek interdisciplinary research proposals that involve co-investigators from diverse backgrounds [including neuroscience]'. Of note, in the realm of neuroscientific leadership studies, the first studies funded by the Defense Advanced Research Projects Agency (or DARPA – one of the five federal research agencies mentioned earlier within Obama's *Brain Initiative*) have been published (e.g. Balthazard et al., 2012; Hannah et al., 2013). In light of the research priorities (and associated funding flows) of major funding organizations, it is likely that neuroscience (more generally and in the context of management studies), will remain on the research agenda for some time. Two points are worth highlighting here. On the one hand, in the context of clinical applications, it is clear that appropriate funding levels are of vital importance. On the other hand, there might be a temptation to categorize neuroscience as yet another management fad, but this view has been challenged (Lindebaum, 2013b).

Second, publication bias as a boost for journal impact factor is likely to play a role in creating a demand for novel theories, and the case of neuroscience is a prominent example. For instance, I recently provided a count of 15 to 0 in favor of neuroscientific advocates in management studies in relation to publications in leading US journals (Lindebaum, 2014). Adding recent publications (of which I am aware, see Ashkanasy et al., 2014; Lindebaum and Jordan, 2014a; Molenberghs et al., 2015), the score has changed to 17 to 1. Indeed, the count would also appear differently if high-quality European journals or journals of highly respectable but lower impact factors are included as well.³ Further to this, when advocating articles on organizational neuroscience are fast-tracked in prestigious journals despite years of backlog articles (e.g. an article appearing ‘on-line’ first in March of a given year, and then included in the April issue of the same year), it raises the question for me as to whether publication bias does not play a role here.

A closer look at the clinical literature further helps illuminate the link between publication bias and a journal’s impact factor. For instance, Dubben and Beck-Bornholdt (2005) argue that positive findings stand a better chance of being published, being published earlier, and being published in journals with higher impact factors (with the concomitant risk that conclusions can be misleading: this is especially the case for neuroscience, see Button et al., 2013). The former editor of the *British Medical Journal*, Richard Smith (2015), agrees, positing that “‘top journals’ select studies that are new and sexy rather than reliable’. However, given the competition among journals for professional prestige, being frequently read and noticed as indicated in a higher impact factor is crucial for the journal, as publishers and advertisers tend to support financially a high-impact journal more compared with a journal with a waning impact factor (Joober et al., 2012). Therefore, articles presenting positive and clean results, or those that represent novel and attractive topics, are more likely to get published, not least because it will help boost the journal’s impact factor. This argument is also reflected in the recent meta-analysis on fMRI data by Button and colleagues (2013). Given the crucial implications for career progression, promotion and kudos of *where* we publish (as opposed to *what* we say), social science runs the risk of becoming ever more normative in the topics it addresses, and the way we engage with these topics (either as advocates or dissenters – see Duster, 2006; Holt and den Hond, 2013).

Taken together, I argue that there are note-worthy demand forces for management research informed by neuroscientific theories and methods. However, as I will detail hereafter, there are lingering issues with the quality of the supply and its appeal.

Characteristics of the supply side

When it comes to the supply of novel management theories informed by neuroscientific data, there are several characteristics that are worth highlighting. As indicated, the first concern is related to the validity of fMRI data. A second concern pertains to the use of imprecise motherhood statements. And third, I discuss the supply of ethically devoid theoretical and empirical advances mirroring the ‘pretense of knowledge’ (Ghoshal, 2005).

By all accounts, the use of fMRI data is becoming more widespread in management research broadly defined (e.g. Bagozzi et al., 2012, 2013; Laureiro-Martínez et al., 2014; Molenberghs et al., 2015), largely because it offers management scholars the confidence

to rely upon 'hard' and 'objective' data, which is said to reduce the error margins and levels of ambiguity to be found in more traditional research design (Lindebaum and Jordan, 2014a). More precisely, the putative 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). Therefore, neuroscientific imaging technologies capture both 'controlled' and 'automatic' processes of social cognition (Lieberman, 2007). In consequence, neuroscience offers the promise to make visible what has been hidden, insofar as unconscious processes can be readily made evident in ways that normally eludes traditional modes of data collection (Becker et al., 2011).

As one of many recent examples, consider the fMRI study on 'Machiavellianism' and 'mentalizing' by Bagozzi and colleagues (2013). The authors invoke prior research suggesting that 'mentalizing' (i.e. 'the ability to read the desires, intentions, and beliefs of other people', see Frith and Frith, 2008: 504) is located in several specific brain regions. Bagozzi and colleagues (2013) refer to four studies to cement their argument. Of these four studies, three are review articles, and only one study is based upon empirical fMRI data (with a sample size of 12 individuals, see Saxe and Wexler, 2005).

While my intention here is not to call into question the quality of theorizing in the Bagozzi et al. (2013) study, a recent meta-analysis published in *Nature Reviews Neuroscience* offers a cautionary tale on the interpretation of fMRI data. Specifically, it reports that the estimated statistical power of fMRI studies using human subjects is only 8 percent, owing to small sample sizes and inconsistent analytical strategies (Button et al., 2013). That is, if a study were to be designed with a power of 8 percent, it entails that – if there were 100 genuine non-null effects to be detected, this study can only be expected to detect eight of them (Button et al., 2013). Statistical conventions typically locate the requisite statistical power at .8 (and not .08, see Funder et al., 2014). This, in turn, has significant consequences for the advancement of theory in management research and beyond. Recall how theory was defined previously in this essay, namely, as an arrangement of constructs in which these constructs are associated by way of propositions. Put differently, proposing a complete theory requires the identification of *which* factors (e.g. constructs, variables or concepts) are logical constituents of the explanation of the phenomenon under investigation (Whetten, 1989). Therefore, given the low statistical power of fMRI studies, how can we be sure to a sufficient degree that the relating of mental phenomena (e.g. mentalizing, attitudes, cognition or emotions) to specific brain regions via specific propositions is based upon a true effect reported previously?

A second characteristic of the supply side can be found in the use of so-called 'motherhood statements'. That is, statements that lack clear conceptual and theoretical directions enabling a better understanding of how precisely brain networks account for (and are influenced by) behaviors that have practical relevance in the context of work. Lindebaum and Jordan (2014a) refer to the evident use of these statements in advocating research on organizational neuroscience. For instance, Goleman and Boyatzis (2008: 2) maintain that,

. . . great leaders are those whose behaviour powerfully leverages the system of brain inter-connectedness . . . If we are correct, it follows that a potent way of becoming a better leader is

to find authentic contexts in which to learn the kinds of social behavior that reinforce the brain's social circuitry.

Another example is provided by the Chartered Institute for Professional Development (or CIPD, 2014), when they suggest 'how HR [Human Resources] can use neuroscience to boost learning and development, cut staff turnover and enhance customer service', adding that one way to achieve this is to help 'staff to gain knowledge of how the brain is structured [which] can help learners build self-awareness and improve their personal effectiveness'. This is a prominent motherhood statement, and its validity is questionable; as if *structural* knowledge of the brain alone – without any deeper understanding of how the brain holistically *functions* – could ever explain such complex issues such as self-awareness or effectiveness in the context of a particular work setting or situation (Lindebaum and Jordan, 2014a; Lindebaum and Zundel, 2013).⁴

Third, and consistent with the pretense of knowledge, it is evident that organizational neuroscience is firmly embedded within the positivist paradigm (for an exception, see Healey and Hodgkinson, 2014), with its ontological and epistemological key tenets of reality as real and apprehensible as well as findings being objectively true, respectively (Guba and Lincoln, 1994). As an example to this end, consider Becker and colleagues (2011), who argue that 'neural mechanisms are largely homogenous across all individuals' and that 'all brains are organized in a similar fashion' (p. 936).⁵

The positivist ideal is also captured in Ghoshal's (2005) cautionary essay on bad management theories. For him, the pretense of knowledge results from a 'partialization of analysis, the exclusion of any role of for human intentionality or choice, and the use of sharp assumptions and deductive reasoning' (pp. 76–77). For the pretense of knowledge to gain legitimacy in management studies and beyond, theorizing must be value-free and, worse still, involves a dismissal of ethical concerns (see also Suddaby, 2014). This is especially apparent in the application of neuroscience for enhancement purposes rather than clinical or therapeutic purposes (as an example of this, see Waldman et al., 2011). In addition, many ethical problems, such as those of potentially harmful management practices, are the direct consequence of methodological problems (Lindebaum and Jordan, 2014a; Lindebaum and Raftopoulou, 2015). This, in turn, suggests that grounding questions philosophically and ethically is of great significance in management studies. But advocates of organizational neuroscience seem unimpressed by these concerns. As one advocate of neuroscience once wrote to me, it is more an empirical – rather than an ethical or philosophical – question as to whether individuals participating in studies using neuroscientific methods feel any discomfort about this kind of data collection. I disagree with this view. Grounding questions philosophically and ethically is of great significance in social science research, for otherwise empirical efforts are 'understood to solidify and legitimize existing dogmas and prejudice' (Alvesson and Willmott, 1992: 435). Like others (e.g. Hallinan et al., 2014), I have detailed previously why ethical concerns do arise in the context of applying neuroscientific methods to the study of management phenomena (Lindebaum, 2013b).

Perhaps even more noteworthy is that neuroscientists themselves (see e.g. Farah, 2005; Farah et al., 2008; Fuchs, 2006; Ilieva et al., 2015) – as well as philosophers and historians (Harari, 2014; Levy, 2009) – have advanced the debate on the ethics of

neuroscience (or bio-technology more generally) much further than those who import it into management studies. Recently, I wrote an article (and rejoinder, see Lindebaum, 2013a, 2013b) on the effects of pathologizing healthy but ineffective leaders owing to their ‘brain profile deficiencies’ (a label used by Waldman et al., 2011). That is, only because a brain profile of an arbitrarily selected category of effective leadership (i.e. inspirational leadership, see Waldman et al., 2011) was not detected in some participants (i.e. those with ‘anger management problems’), these participants underwent a series of neuro-feedback sessions to become more inspirational (and, therefore, more effective). To the best of my knowledge, there is little – if any – deeper conversation about the ethics of neuroscience as such among advocates of organizational neuroscience, especially if neuroscience is applied to emotional or cognitive enhancing in *healthy* individuals rather than clinical purposes. It should be added, however, that Waldman and colleagues (2011) caution against the use of neuroscience to boost the ‘darker side’ of leadership, such as manipulative skills. Of course, that might just as well be achieved by watching Kevin Spacey depicting Frank Underwood in *House of Cards*.

Bad consequences for management practice

Based upon the preceding sections, I discern the danger that we create bad rather than good theory. However, beyond offering incomplete or ill-guided direction for future empirical efforts, it has also significant consequences for management practice, especially when ineffective management practice (e.g. in terms of selecting key-decision makers) is considered effective. As McDaniel and colleagues (2006) show in the context of employment interviews, prior meta-analyses have shown that structured interviews had a higher validity than unstructured interviews (.27 versus .19, uncorrected). However, they also discuss subsequent studies on publication bias suggesting that the validity of structured interviews is likely to be lower (i.e. .21), placing it in close proximity to the validity of unstructured interviews. For McDaniel and colleagues (2006), one key consequence of this has been that many practitioners have invested considerable efforts to create rather laborious structured interviews in order to select employees, assuming that these had a higher validity compared with unstructured interviews. With the alleged ‘objective’ and ‘rigorous’ nature of neuroscientific data, it is not hard to imagine that future employee selection processes will harness expensive fMRI technology to ensure the ‘best’ candidates are hired for the job. First studies following the logic of ‘objective’ and ‘rigorous’ data are already emerging (see e.g. Waldman et al., 2011). However, pointing to the ethical problems arising from low statistical power of fMRI studies, Button and colleagues (2013: 365) conclude that ‘unreliable research is inefficient and wasteful’. Therefore, like others, I argue that (at least for now) we should not be ‘blinded by neuroscience’ (Wastell and White, 2012: 399), even though the advocacy of applying neuroscientific theories and methods is sharply increasing as demonstrated by the growing number of publications in top-tier North-American management journals (Lindebaum, 2014).

It follows that there is an imperative to ensure that the new theories we plausibly seek to generate should be based upon valid and strong data in order to prevent any negative ramifications for management theory and practice. By extension, theories are required to

possess a wider theoretical grasp of phenomena under examination and their practical relevance (Edwards, under review) in order to generate, for instance, a better understanding of social relations in and around work (Edwards, 2011).

Some suggestions for future research

The question then arises what scholars can do *en route* to developing new and strong management theory informed by neuroscience? In this regard, I advocate *direct* replication, coupled with an increase in sample sizes, in order to augment the statistical power of these studies. Direct replication refers to the repetition of an experimental procedure to as precise a degree as is feasible (i.e. using identical equipment, material, stimuli, design and statistical analysis where possible) to attain more confirmatory power compared with conceptual replication. The latter refers to the application of different methods or parameters (e.g. different questionnaires to examine transformational leadership) to repeat tests for specific hypotheses or experiments (see Schmidt, 2009, for a review on the difference between direct and conceptual replication). However, direct replication appears crucial in the context of neuroscientific studies and, by extension, for management studies drawing upon this empirical body in order to advance knowledge. This is because a recent study demonstrates significant inconsistencies in terms of analytical strategies. Precisely, Carp (2012) reports that, of 241 fMRI studies examined, 223 unique analytical strategies were employed, so that almost no analysis was used more than once. Relevant factors here concern definitions of key variables, the statistical model employed, adjustments undertaken (or lack thereof) to account for potentially confounding factors, and the usage of filters to exclude some observations from the analysis. Particularly in small sample studies, this can generate different estimates of an effect size depending upon the analytical choices made. Even minor adjustments can readily render a non-significant result significant (Button et al., 2013). Therefore, I concur with Funder and colleagues (2014), who stress that reproducibility of findings helps establish a firm bedrock upon which reliable findings can accumulate.

Conclusion

In sum, I reiterate that previous contributions on the generation of management theory offer an important analysis that hopefully will spark some imagination among management scholars and beyond in terms of building new theories. With this essay, however, I aimed to add an important twist to their call, namely, that when a certain demand for new theory is plausibly created, that we should be wary not to rush these efforts by taking recourse to kinds of data that require further elaboration and development. In this essay, I have examined both demand and supply forces in relation to novel theories in management studies informed by neuroscience (including an appreciation of ‘agency’ aspects of researchers’ activities). I have shown that the magnitude of the demand is not always met by a corresponding supply of quality data, theoretical precision and an appreciation of harm that the pretense of knowledge can impose upon management practice. Taken together, the more technical aspects of building theories (i.e. especially the kinds of ingredients we use) combine with a dismissal of ethical concerns as a harbinger of bad management theory and practice.

I close this essay by paraphrasing Lewin's (1945: 127) famous statement that 'nothing is as practical as a good theory' to read that *nothing is as impractical as a bad theory*.⁶ I hope this critical essay has contributed a little to the quest for good theories, which, as Kanter (2005) reminds us, will emerge if we actually demand them. Thus, our efforts to generate new and good management theory must be carefully gauged against the danger of generating bad theory based upon data that requires further development.

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Notes

- 1 Suddaby and colleagues (2014) also note that the discipline of 'management research has failed to cultivate any truly indigenous theories of management' (p. 236, emphasis added). This, in turn, relates to the perennial debate on whether – in broader management research or specific subsets thereof – it is wiser and more appropriate to speak of theories *in* management rather than theories *of* management, for management theories cannot be treated as self-contained spheres of social life given their interface with, inter alia, biological, political, economical or social factors (see e.g. Hyman, 2004). Consistent with the definition of organizational neuroscience provided, I interpret the attempt to draw upon neuroscientific theories and data as an effort to create new theories *in* rather than *of* management.
- 2 The information has been retrieved from <http://www.bbc.co.uk/news/health-24428162> and https://www.humanbrainproject.eu/documents/10180/17648/TheHBPRreport_LR.pdf/18e5747e-10af-4bec-9806-d03aead57655 on 20 March 2015.
- 3 Note that I do not equate a journal's impact factor with the quality of research published therein, since the impact factor is determined by technicalities independent of the quality of scientific articles (Seglen, 1997). In addition, I construe a high impact factor as an indicator of article appeal to a broad mainstream readership (unless an article is cited for poor scholarship). It often does not entail that marginalized and dissenting contributions to the literature are of any lower quality.
- 4 These statements also compel the reader to consider the metaphorical versus literal use of neuroscience as a resource in management studies. It appears to me that there is a connection between these motherhood statements and the more metaphorical use of neuroscience, reflecting a loose reliance on analogy and pseudo-science (Wastell, 2013). Several examples of motherhood statements and the more metaphorical use of neuroscience can be found in Jarrett (2015). However, more literal interpretations are also evident in advocating scientific literature on organizational neuroscience, reflecting the assumption that the logics and mechanism operating at one level of analysis (i.e. neural activity) hold and operate in identical ways at higher-levels of analysis too (e.g. the organizational level – for a critique on this argument, see Lindebaum and Zundel, 2013).
- 5 And yet, it is fallacious to assume that only because neural mechanisms and brains work in similar ways across humans, that the phenomenological outcomes are identical too. This

is part and parcel of the multiple realizations phenomenon in the philosophy of science (Lindebaum and Zundel, 2013).

- 6 I gratefully acknowledge a previous conversation with Roy Suddaby, who suggested this paraphrased statement to me at the EGOS Conference 2013 in Montreal.

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Erratum

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The following corrections apply:

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