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Organisational Cognitive Neuroscience drives Theoretical Progress, or: The Curious Case of the Straw Man Murder

Correspondence:
Dr. Michael JR Butler*
Aston Business School
Aston University
Aston Triangle
BIRMINGHAM
B4 7ET
UK
Telephone: 07919 926362
E-mail: m.j.r.butler@aston.ac.uk

Professor Nick Lee
Warwick Business School
Warwick University
COVENTRY
CV4 7AL
UK
E-mail: nick.lee@wbs.ac.uk

Dr. Carl Senior
Life and Health Sciences
Aston University
BIRMINGHAM
B7 4ET
UK
E-mail: c.senior@aston.ac.uk

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Abstract

In this critical essay, we respond to Lindebaum's (2016) argument that neuroscientific methodologies and data have been accepted prematurely in proposing novel management theory. We acknowledge that building new management theories requires firm foundations. We also find his distinction between demand and supply side forces helpful as an analytical framework identifying the momentum for the contemporary production of management theory. Nevertheless, some of the arguments Lindebaum (2016) puts forward, on closer inspection, can be contested, especially those related to the supply side of organizational cognitive neuroscience (OCN) research: fMRI data, motherhood statements and ethical concerns. We put forward a more positive case for OCN methodologies and data, as well as clarifying exactly what OCN really means, and its consequences for the development of strong management theory.

Keywords

Management, methodology, organizational cognitive neuroscience, practice, theory

Introduction

Human Relations has a distinguished track record of publishing emerging debates related to theoretical innovations in management research, often derived from biological and psychological advances. For example, *Human Relations* published an exchange on evolutionary psychology (EP) in the context of management and organizations. Nicholson (1997: 1053) set out his argument that 'evolutionary psychology offers a radical and challenging new perspective on human nature and organizational society.' Later, Sewell (2004a: 923) countered that EP 'is found wanting…it cannot satisfy the rigorous demands of experimental evolutionary biology and does not deal well with some of the key problems faced by mainstream psychologists.' (see also Sewell, 2004b). Markoczy and Goldberg (2004: 1045) in a nuanced response conclude their article by asking for a more balanced critical debate: 'We are looking forward to the day when a discussion of EP and EP's role in the study of management and organizations can be discussed and debated like any other approach, on its merits and on its flaws. Until that day arrives, we must continue to refute criticisms based on misrepresentations of our work.' We know how they feel.

It seems to us that the self-same debate is still raging, but with EP replaced by neuroscience as the *bête noir* of Dirk Lindebaum and colleagues (e.g. Lindebaum, 2016; 2014a, b; 2013a, b; Lindebaum and Zundel, 2013). Specifically, Lindebaum (2016) has recently argued that neuroscientific methods and data may have been accepted prematurely, and inappropriately privileged, in proposing and supporting novel management theory. More widely, Lindebaum (2016; Lindebaum and Jordan, 2014b; Lindebaum and Zundel, 2013) in a series of essays in *Human Relations* and other august journals (e.g. Lindebaum, 2013a, b; Lindebaum and Jordan, 2014a), has repeatedly critiqued the application of neuroscientific concepts to organizational, management, and especially leadership, questions. In the aforementioned papers, Lindebaum and colleagues have on various occasions cited the work

of Lee and colleagues (2012a, b; Senior et al., 2011a), and that of several other scholars (e.g. Ashkanasy et al., 2014; Balthazard et al., 2012; Becker et al., 2011; Waldman et al., 2011), which – over the course of nearly a decade since 2007 (e.g. Butler and Senior 2007a, b; Senior et al., 2008a, b) – has attempted to find an appropriate place for neuroscientific theories, methods, and concepts in organizational research (see also Ashkanasy, 2013; Waldman et al., 2016, Ward et al., 2015).

Of course, it is true that unjustifiably privileging neuroscience in management research could lead to the problems pointed out by Lindebaum (2016). However, it is equally clear that misinformed and inaccurate criticism of neuroscience and related work will also lead to problems with knowledge creation. We appreciate that not all scholars will see the utility of incorporating neuroscience-based understandings of social behavior into management and organizational research, in the same way that not all scholars will agree that a post-positivist, subjective, contextually-situated, approach to research is of any merit as a basis for knowledge-creation. However, to criticize one for not being the other seems to us to be broadly meaningless.

Here, we respond to, and elaborate on, Lindebaum's (2016) criticisms of neuroscientific research in organizations. We feel it necessary to provide a more in-depth examination of many of the claims made in Lindebaum's (2016) essay, to a) correct a number of basic factual inaccuracies, b) clear up some important misinterpretations, and as a result c) provide a more nuanced understanding of the key issues involved, to avoid unnecessarily chilling the advance of research in this area. In essence, our conclusion is that Lindebaum's (2016) essay has gone to great trouble to construct an elaborate straw man. In overcoming this, we focus heavily on exploring the assumptions that appear to underlie Lindebaum's criticisms, showing that for the most part they are typical of criticisms made of so-called 'scientific' research methods by those who tend towards a more subjective view of

knowledge, many of which have long-since been superseded (Healey and Hodgkinson, 2014). While these criticisms may have some merit, depending on one's point of view, they are not particularly critical of neuroscience per se, but of *any* research method which aims to quantify and objectify human social behavior.

We begin with discussion of a fundamental mistake made by Lindebaum's (2016) work, the conflation of cognitive neuroscience with biological neuroscience under the general term 'neuroscience'. This mistake seems to lead Lindebaum to make several erroneous assumptions, and draw a number of conclusions that are flatly incorrect when applied to cognitive neuroscience. Following this, we explore further Lindebaum's (2016) elucidation of demand and supply-side forces, and explain how a number of Lindebaum's (2016) criticisms depend on basic misunderstandings of the plurality of neuroscientific methods and neuroimaging techniques. In doing so, we clarify the importance of defining appropriate nomenclature and provide an overview of contemporary ethical positions with regards to such research. Along the way we also use the opportunity to rectify various misconceptions of previous work and provide clarity on a number of the debates raised by Lindebaum (2016).

What is organizational *cognitive* neuroscience?

On numerous occasions, the work of Lindebaum and colleagues (e.g. Lindebaum, 2016; Lindebaum and Zundel, 2013) appears to conflate the work of authors such as Waldman et al. (2011), Becker et al. (2011), Becker and Cropanzano, (2010), and Balthazard et al. (2012), with Lee et al. (2012a, b; Senior et al., 2011a), under the same general umbrella. However, while there are overlaps, the two streams diverge in important ways. Indeed, Lee et al. (2012a) have gone to considerable trouble in the past to make this distinction clear. More specifically, Becker et al. (2011) term the approach *organizational neuroscience* (ON), and Lindebaum (2016) also uses this term repeatedly. In their work, Butler and Senior (2007a, b;

see also Lee and Chamberlain, 2007), however, were consistent in using *organizational cognitive neuroscience* (OCN) to frame the emerging field of study (e.g. Senior et al., 2011a; Lee et al., 2012a). OCN though, is never used by Lindebaum (2016), implying that he considers OCN to be synonymous with ON.

It is important to recognize that this is not a mere semantic game, or an attempt to claim one's scientific patch and force our preferred nomenclature to become dominant. Rather, the two terms demarcate two substantively different approaches to incorporating the brain into management and organizational research (Lee et al., 2012a, b); just as cognitive neuroscience has a specific meaning within the broad neuroscience umbrella. Further, many of the criticisms made in Lindebaum's (2016) work are primarily relevant to the ON approach, and it is simply incorrect to conflate this with OCN. In this section, we will explain why Lindebaum's (e.g. 2016) criticisms are particularly misguided when applied to OCN rather than ON. First though, since it appears that the distinction is more difficult to comprehend than we expected, we will go into some depth to discuss why the two approaches are different in important ways. This is particularly important in light of Ward et al.'s (2015) recent defense of ON, which appears to exhibit a similar tendency to conflate ON and OCN, this time for the purposes of defending ON rather than criticizing it.

While neither cognitive neuroscience nor neuroscience itself can truly be considered to have coalesced as unified disciplines of study until the 1970s or even 1980s, their various foundational fields (e.g. cognitive psychology, neurology, biological psychology, neurophysiology, medicine, and so forth) have far longer histories. Indeed, the roots of a differentiation between the cognitive neuroscience and neuroscience approaches can be seen to emerge at the 1906 Nobel Prize ceremony, where the prize for physiology or medicine was given jointly to two great histologists, Camillo Golgi, and Santiago Ramón y Cajal. Both made seminal contributions to understanding brain function, but nevertheless were

fundamentally at odds in perspective. Ramón y Cajal argued that the individual neuron was the fundamental unit of the brain, while Golgi maintained that the nervous system was a continuous network. Such was their disagreement that they each used their Nobel prize-winning speeches to argue against the other position.

Of course, over time, the ideas of both now form the foundation of neuroscience in general. The brain is a large network, consisting of individual neurons linked together and transmitting electrical impulses through a complex process of ionic permeability through the cellular membrane. At the end of the neuron, the electrical impulse, which has been literally bounced along the cell body via a process known as saltatory conduction, reaches the synapse, and chemical substances (neurotransmitters) released across the synaptic gap initiate a post-synaptic impulse in adjacent neurons. Individual neuron activity can be measured by the insertion of micro-electrodes, and a region of the brain is considered active if the networked neurons in that area are synchronizing their discharge frequency (Uhlhaas et al., 2009). While neuroimaging techniques such as Magnetoencephalography (MEG), functional Magnetic Resonance Imaging (fMRI) and others (of which more will be said later) can provide an indirect indication of neuronal activity, it is the case that micro-electrode insertion is the current method by which we can record actual discharge frequency and phase synchronicity of groups of neurons – rendering it inapplicable to human research and obviously outside the reaches of the management scholar (see e.g. Senior et al., 2011a: Figure 1).

As was clarified in Lee et al. (2012a: 923), ON is ‘a fundamentally neuroanatomical perspective in that it is concerned with the role that brain anatomy plays in the mediation of organizational decisions. Becker et al. (2011) exemplify this perspective – even the title of their report refers to the “neural black box”’. A close reading of Becker et al.’s (2011: 936-937) paper in ON will confirm this conclusion, turning up multiple references to individual

neurons, neuroanatomy, and neurophysiology, and a clear definition of ON which advocates 'levels of reduction that deconstruct individuals to discrete brain processes', even to the level of 'molecular explanations'.

That said, it seems that even advocates of ON are occasionally prone to over-extending the notion of ON towards OCN, in their defense against critiques. For example, Ward et al. (2015: 26) claim that 'Becker et al. (2011) presented the brain-level as an additional level, rather than as the preferred level, of analysis. … Indeed, Healey and Hodgkinson [2015] and others (e.g., Lindebaum & Zundel, 2013) have taken statements about using brain-level data and misinterpreted them as claims to uncover fundamental and implicit causes.' However, this line of defense for ON seems somewhat revisionist, and unlike Ward et al. (2015), without first-hand knowledge of what Becker et al. (2011) *wanted* to say, readers must rely on what they *actually* said. Of course, Becker et al. (2011: 937) do not omit entirely any mention of different levels of explanation for organizational behavior, and indeed note the 'potential concerns…that more reductionist or molecular explanation [may] supplant other accounts'. Yet such statements may appear somewhat prosaic compared to the many others presenting neuroscience as the 'most fundamental level of analysis', discovering the 'most basic building blocks of nature' (Becker et al., 2011: 934), and the numerous neurophysiological examples of potential ON research. Indeed, Becker et al. (2011: 951) specifically claim that they 'observed that hierarchical reductionism promises a new, deeper level of analysis'. As such, Ward et al.'s (2015: 26) claim that critics of ON have 'misinterpreted Becker et al.'s (2011) advocacy for ON, as hierarchical reductionism' seems somewhat unfair. Indeed, Ward et al.'s (2015: 26) claim that 'the ON perspective is that neural activity is necessary, but not sufficient, in understanding sensation, perception, and behaviors…we recognize that behavior takes place within, and is partially shaped by, the social context' does not seem consistent with ON's foundational expositions (see also Lee et

al. 2012a). Rather, in attempting to defend ON, Ward et al. (2015) appear to have redefined it, as OCN (see Butler and Senior, 2007a, b; Lee and Chamberlain, 2007 Senior et al., 2011a).

Of course, while basic neurophysiological knowledge provides foundations to cognitive neuroscience, the cognitive neuroscience perspective is fundamentally different in that it is a multilayered approach concerned with human knowledge systems, rather than just brain structure and activity itself (Gazzaniga, 2000) – recalling Golgi's work in a metaphorical sense at least. Further, inherent to the study of human knowledge systems 'is the notion of social embodiment. It goes without saying that one cannot have [knowledge systems] without a description of the social environment within which [they develop]' (Lee et al., 2012b: 214; see also Healey and Hodgkinson, 2014; 2015). As such, OCN, as an applied subfield of cognitive neuroscience, is concerned with exploring human behavior within and in response to organizations as a set of theoretical layers, of increasing abstraction; from evolved adaptation, to biological/physical brain activity and anatomy, to cognition (i.e. mental), to social, to organizational as a subset of social theory (see Butler and Senior, 2007a; Lee et al., 2012a, b; Senior et al., 2011a).

It can be seen that OCN is a framework that – explicitly, from its very first origins (e.g. Butler and Senior 2007a; Lee and Chamberlain, 2007) – does not attempt to reduce organizational behavior to basic neural activity or structure. This is opposed to ON which, drawing from Becker et al.'s (2011) original work, seems to take this as its *raison d'être*. Indeed, it is clear that it is not possible to fully understand a given organizationally-relevant behavior by ignoring the various interwoven layers of theory posited by OCN (Butler and Senior, 2007a; Lee et al., 2012a, b; Senior et al., 2011a). Although they are not synonymous, OCN shares this holistic perspective with Healey and Hodgkinson's (2014; 2015) critical realist view, which is another perspective that provides strong criticism of Lindebaum's arguments (e.g. Lindebaum and Zundel, 2013).

Healey and Hodgkinson (2014; 2015) also robustly criticize ON's reductionist focus. While Ward et al. (2015) attempt to defend ON against these criticisms, it should be clear that in doing so they also are forced to conflate ON with OCN, because without the more neurophysiological focus, ON simply becomes another term for OCN. As such, it can be seen that the OCN approach dealt with much of Lindebaum's intended critique of ON before it was even made, given that the foundational work on OCN predates both Lindebaum's work and the original work on ON. To be clear, OCN does *not* attempt to reduce organizational behavior to just brain activity, ignoring the wider social context; 'the organizational cognitive neuroscientist is interested in understanding the molecular logic of organic knowledge systems only when placed in their natural social ecology' (Lee et al., 2012b: 216). Importantly, OCN does not ignore any particular level of analysis, but rather explicitly recognizes the symbiotic relationship between the layers of theory, and in doing so develops more rigorous testable hypotheses, and ties this to advances in research methods that can more accurately test these hypotheses (Butler and Senior, 2007a; Lee et al., 2012a, b; Senior et al., 2011a).

Of course, the OCN view necessitates taking account of our existing knowledge about cognition, brain structure, neural activity, and the like, in deriving hypotheses, rather than ignoring it and pretending it is irrelevant to explaining organizational behavior (Senior et al., 2011a). Even so, this is not the same as strict upward entailment, where higher-level theories must be explained in terms of lower-level ones. At this point in our understanding, this is an impossible task. OCN rather advocates a gentler variety of entailment, where theories at one level must not directly contradict what we already know about other layers (Senior et al., 2011a). This is most clearly seen when positing higher-level (e.g. social) theories while taking account of existing knowledge about lower-level (e.g. cognitive or neural) systems.

However, higher-level investigations can provide important tests of lower-level theories as well, meaning entailment can sometimes be a double-edged concept.

Take for example the finding that the neurophysiological mechanisms involved in the perception of pleasant or rewarding stimuli (Senior, 2003) are also activated when individuals are paid to complete a task, and that the activity is predictive of success on that task (Longe et al., 2009). These findings suggest that a subcortical network exists that may drive completion of tasks when an individual receives an extrinsic motivator (common practice in the workplace). They also provide a clear example of theoretical entailment, with both the low-level theory impacting our understanding of the higher and *vice versa*. To be clear though, the OCN approach makes no claim as to the utility of identifying a region in the brain that can be artificially activated to increase task completion. Simply because the brain is involved gives OCN no more unique power to create legions of workers tirelessly completing tasks in an endless cycle of productivity than any other insight into motivation and performance, from Taylor's scientific management onwards (Senior and Lee, 2008). Indeed, we clarified the role that OCN could and does play in driving the development of theory a number of years ago: 'The OCN approach affords the researcher the ability to understand in greater detail the physical and contextual mediators of these different motivational drives.' (Senior et al., 2011a: 5). In this sense, OCN can both inform our understanding of management and organizational behavior, but also cognitive psychology, and neuroscience itself (Lee et al., 2012a).

Demand-side forces

With the distinct nature of OCN clarified, we can now move to more specific aspects of Lindebaum's (2016) critique, while considering how accurately they apply to OCN versus ON. In considering what constitutes useful management theory, Lindebaum's (2016)

distinction between demand and supply side forces is helpful. We agree with the view 'that there are at least two relevant demand factors.' (Lindebaum, 2016: 540) which could maintain OCN research. The first is the emergence of substantial research funding, notably in the US and Europe, and Lindebaum (2016) notes President Obama's *Brain Initiative* and the EU's *Human Brain Project*. The second demand factor is the search by publishers and academic journals, including *Human Relations*, for 'the highest quality original research to advance our understanding of social relationships at and around work through theoretical development and empirical investigation.' (http://www.tavinstitute.org/humanrelations/submit_paper/guidance.html). This is supported by willing academic researchers who focus both on what they publish and where, 'for career progression, promotion and kudos' (Lindebaum, 2016: 541).

Despite our agreement on the nature of the forces though, we strongly differ from Lindebaum (2016) in our evaluation of the impact that these two demand factors may have in driving OCN based research. Lindebaum (2016) highlights the possibility of a trend towards reductionist science (quoting Duster, 2006), and a bias in favour of publishing 'neuroscientific advocates in management studies in relation to publications in leading US journals' (Lindebaum, 2016: 541). Here again, we can see clear echoes of already well-discussed debates involving biological approaches to social research, such as Nicholson's (2005) three themes of opposition to evolutionary psychology. The first theme, 'Reductionist imperialism' (Nicholson, 2005: 400), is that 'the biologists are taking over and EP is their Trojan horse.' As a concomitant to reductionist science is the second fear of 'Mechanistic determinism' (Nicholson, 2005: 400), that 'our genes make us do x or y.' The third and last theme, 'Normative naturalism' (Nicholson, 2005: 401), captures the idea of a social agenda, expressed as 'a present danger for EP writers to be too assertive about "fit" and "misfit" between human nature and contemporary life'.

Of course, in the present context, one can replace ‘EP’ with ‘OCN’, and see the same picture being painted again, over a decade later. Yet, genuine understanding of how biological/evolutionary theory, which Lee et al. (2012a, b) show is one level of theory taken in by OCN, can inform the development of management theory and practice, showing the feebleness of some of the above objections. Here, we refer to work on developmental stability and transformational leadership (e.g. Senior and Yeats, 2010). Developmental stability is a biological term used to describe how genes can facilitate phenotypic development (Clarke, 1998). Effective phenotypic expressions can be biased by a range of environmental stressors, which often manifests itself in the form of an asymmetrical morphology (Kowner, 2001). Such developmental idiosyncrasies occur throughout the biological kingdom but in human cultures we tend to assign asymmetrical looking people with a range of negative social attributes such as being less dominant, less intelligent, physically unattractive, less popular with peers etc. (Mazur et al., 1984; Mazur et al., 1994; Mueller and Mazur, 1996). What is of interest to the current debate is that managers who are more asymmetrical tend to show a higher degree of task satisfaction and work related well-being when leading a small group of workers on a year-long project. Perhaps more interestingly, the work produced by the respective groups was of a higher quality that the work produced by groups led by a leader with a more symmetrical physical morphology (Senior et al., 2012). Thus it seems that asymmetrical people may develop compensatory personality strategies to help overcome the perceived misgivings from their peers when younger, and that these strategies may manifest themselves as effective relationship-based leadership skills in later life. As such, it is not the case that one’s biology (or ‘genes’) ‘forces’ us to take one path or another. Rather, our biological makeup interacts with our physical and social environment over time, to some given effect. Ignoring the biological foundations is equally as myopic as presenting them as the only influence on management and

organizational behavior. Further, the practical impact of the above is evidenced in the interest from key practitioner-oriented journals (e.g. Senior et al., 2011b).

Of course, considering Lindebaum's (2016) censure of reductionist science, we can see again that when applied to OCN this is unfounded. Indeed, as shown earlier in this paper, it has been stressed over and over again that OCN is based around including, but also taking account of layers of theory beyond, the purely biological/neuroanatomical. Furthermore, research has repeatedly cautioned against over-interpretation of purely biological and neuroscientific empirical findings, and set out clear concerns over the interpretation of neuroimaging data in management and organizational research (e.g. Brieter et al., 2015; Lee et al, 2012 a, b; Senior et al., 2011a).

Supply-side forces

If Lindebaum's (2016) presentation of the demand-side forces has some merit – although we disagree with the conclusions – his presentation of the supply side forces is less helpful. He focuses on three characteristics: 'the validity of fMRI data', 'the use of imprecise motherhood statements', and 'the supply of ethically devoid theoretical and empirical advances' (Lindebaum, 2016: 541). Each of these forces is misconceived and contains substantial inaccuracies. The validity of fMRI data is tackled first, followed by ethical issues, and finally a discussion of motherhood statements.

The validity of fMRI data

Lindebaum (2016) appears to suggest that fMRI occupies some kind of privileged position in the cognitive neuroscience toolkit. We take this implication from the fact that any other neuroscientific methods are only rarely mentioned, and Lindebaum (2016: 537) directly quotes from Cui et al. (2011) that fMRI is the 'gold' standard neuroimaging technique.

Indeed, it seems that at times Lindebaum (2016) appears to use the terms neuroscience and fMRI as synonyms. While of course we agree that fMRI is the most well-known technique, especially to the layperson that may have only read lightly or approached second-hand the scientific literature around the area, to equate virtually all neuroscience with fMRI, and to quote a single minor source as evidence that it is a 'gold standard' is misconceived to say the least.

In fact, even fMRI itself is not a single technique, but an umbrella term that encompasses several different types of neurophysiological data collection. Presuming Lindebaum (2016) was referring to the BOLD (Blood Oxygenation Level Dependent) imaging method (given that was what Cui et al. (2011) were using as their baseline standard), this metric is but one of many different possible measurements possible with an MRI scanner, such as baseline cerebral blood volume (Rosen et al., 1991), changes in the degree of cerebral perfusion (Detre et al., 1992), the degree in which oxygen is extracted from the cells (van Zijl et al., 1998) as well as the metabolic rate of cerebrovascular oxygenation or CMRO2 imaging (Hoge et al., 1999). Each of these distinct indices has strengths and weaknesses, and it is not difficult to find substantive and important criticism of fMRI and the BOLD response as a measure of brain activity in the specialized literature (Logothetis, 2008). In fact, in their previous work, Butler et al. (2015) show that the BOLD response is but a single method, and one which is subject to key drawbacks. Certainly, we accept that much recent management and business literature has used fMRI methods, and we agree this can be questioned – indeed it has already been so (Lee et al., 2012a). However, we would suggest that continuing to discuss neuroscience as virtually analogous to fMRI is far more likely to result in poor theory development than any of the work that he criticizes.

Lindebaum (2016) also provides strident criticism of the sample size of neuroscience studies. This is a criticism we have heard over and over again (see e.g., Zandbelt et al., 2008),

and it rests on a set of misinterpretations of statistical concepts. Specifically, Lindebaum (2016, drawing heavily from a single source, i.e. Button et al., 2013) contends that most fMRI (again, appearing to implicitly conflate fMRI research with *all* neuroscience) studies are of very low power, meaning that they are less likely to detect true effects in the population. The implications of this criticism for statistical theory and research practice are however not as simple as presented in Lindebaum (2016), and much space has been devoted in the statistical, medical, and neuroscientific literature to the issue, which is covered briefly here.

It is important to remember that for any given study, the appropriate sample size for a given power depends on the size of the effect one is hoping to uncover (Lenth, 2001; Schulz and Grimes, 2005) – with larger hypothesized effects needing smaller sample sizes for a given power (i.e. it is simply impossible to decide a blanket rule for what is 'too low' in terms of sample size). Further Friston (2012: 1301) explains that 'a significant result (properly controlled for false positives), based on a small sample indicates the treatment effect is actually larger than the equivalent result with a large sample', and that large samples are at risk of returning statistically significant but scientifically trivial results[1].

Friston's (2012) admittedly rather flippant approach to these issues did lead to a number of critical responses (see, e.g. Ingre, 2013; Lindquist et al., 2013), which were responded to by Friston (2013), who pointed out that in essence there was quite a strong consensus that 'in an ideal world, one should acquire as [much] data as possible…[but]…significant results from small sample studies are valid and should be reported. Furthermore, they are more likely to replicate…small studies automatically protect against inferring trivial effect sizes' (Friston, 2013: 503). While we would certainly add the qualification *properly conducted* to Friston's (2013) comments about small studies, we have to assume he felt this was a given. This directly contradicts Lindebaum's (2016: 542)

argument that low power neuroscience studies mean that it is questionable that we can 'be sure to a sufficient degree' in existing neuroscience results, and that we cannot be sure that work building on them is 'based on a true effect reported previously'. In fact, closer to the opposite is true (Friston, 2012).

Button et al. (2013) give more details on the potential problems of small sample sizes. Apart from the obvious low power issue already covered, they argue that the positive predictive value (PPV, i.e. the likelihood that a positive research finding reflects a true positive effect) is lower in a small sample (see also Ioannides, 2005). However, Friston (2013) shows that this result is simply a function of larger sample sizes being increasingly sensitive to trivial effects, and as such PPV adds little new to any particular line of argument against small samples. Button et al. (2013) also suggest that the significant effects uncovered in small sample studies – even if true – are inaccurate, and likely to be exaggerated (otherwise they would not be significant in the small sample). Of course, again, this is statistically correct, but is dealt with formally by providing confidence intervals for the effect. As long as the confidence interval does not include the null (which by definition it cannot if it is statistically significant, whatever the sample size), then there is evidence for an effect. It is up to the researcher to interpret the utility of any given statistical procedure, in light of its precision (i.e. confidence interval). Button et al.'s (2013: 365) other criticisms appear to primarily be about badly designed studies in general, which 'tend to co-occur with studies of low power or that become worse in small underpowered studies'. However, this is speculation at best, and such problems can also occur in large sample studies.

Where we agree with Lindebaum (2016) is that there is a trend towards bigger sample sizes as the OCN field matures. This trend, however, is set in the context of what Hayasaka et al. (2007: 721) call 'paradoxical facts'. By this they mean: 'It is important to have a sufficiently large number of [participants][2] to detect the signal or effect of interest. On the

other hand, it is also important to include as few [participants] as possible in order to avoid unnecessarily exposing [participants] to unforeseen risks and to reduce the costs associated with the study.' (Hayasaka et al., 2007: 721). Friston, Holmes and Worsley (1999) argued that there are two classes of inference in neuroimaging research, which has implications for sample size, and the selection of the most appropriate analysis depends on the research question being asked. The first inference is to comment on the typical characteristics of a population, and in this context a relatively small number of participants are required. The second inference comments on the average characteristics of a population, which requires larger numbers of participants. The distinction between the two classes of inference arises from the nature of such research, as Friston et al. (1999: 2) explain: 'any neurophysiological effect can be inferred to be present or absent (in a statistical sense using a single-subject analysis) or characterized in terms of the effect itself (the parameter estimates of the effect's size).' They highlight that three participants could be used to make a population inference, but under certain conditions involving the definition of the criteria of what is a typical characteristic of human brain functional architecture, the sensitivity of the test, and the specificity of the proportion of normal participants which have the characteristic. These conditions are expressed in conjunction analysis. A decade later, Hayasaka et al. (2007) introduced a new approach for power and sample size calculation for neuroimaging studies, non-central random field theory, which was applied to a sample of forty-one participants in order to validate the method. They also reported that the method worked with pilot data (five participants) which helps to estimate power and sample sizes for future study planning.

Taking in hand all of the above, what can we say here about sample size in OCN (and neuroscience in general)? First and foremost, it is incorrect and, worse, harmful to appeal to some blanket rule implying that small sample sizes are invalid (Schultz and Grimes, 2005). Second, criticisms of small sample size are far more complicated than they may initially

appear, and require a more nuanced understanding than has been reported in Lindebaum's (2016) essay. In fact, the statistical facts reported by authors both for (e.g. Friston 2012; 2013) and against (e.g. Button et al., 2013; Ioannides, 2005; Lindquist et al., 2013) small sample sizes are the same – it is the interpretation and implications of these issues that are central to the current debate. Rather than criticizing the small samples that are sometimes seen throughout cognitive neuroscience, Lindebaum (2016) would have been better served in pointing out the need for better reporting of more statistical details in published studies, including *a priori* power calculations, and values for the precision of estimates, as well as arguing (as Friston (2012) does) for better standards of review around these issue. Then, rather than providing a chilling effect on future work, Lindebaum (2016) would have had a significant net positive effect on future knowledge development.

Ethical issues

We now turn to the third of Lindebaum's (2016) supply-side forces; the supply of ethically devoid theoretical and empirical advances. Lindebaum (2016: 543) means 'the pretense of knowledge, it is evident that organizational neuroscience is firmly embedded within the positive paradigm…with its ontological and epistemological key tenets of reality as real and apprehensible as well as findings being objectively true, respectively'. Lindebaum (2016: 543) then links the positive paradigm to ethical concerns: '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…This is especially apparent in the application of neuroscience for enhancement purposes rather than clinical or therapeutic purposes'. Lindebaum (2016: 543) further asserts that 'advocates of organizational neuroscience seem unimpressed by these concerns.'

Again referring to OCN as contrasted with ON, there are two clear issues here, the idea that OCN is embedded within the positive paradigm, and that this leads to a dismissing of ethical concerns. Of course, it is hard to argue against OCN being at least tangentially related to the positivist paradigm, given that OCN has emerged from a neuroscience context where positivistic principles appear dominant. However, it seems unnecessary to engage in debate over whether or not positivism and related paradigms can offer anything to knowledge development. Such arguments have been made over and over again, and it seems futile to rehash them here. Indeed, Healey and Hodgkinson (2014: 6), consider the philosophical devices (e.g. reductionism) employed to criticize ON by Lindebaum (2016, which are in essence drawn from those in Lindebaum and Zundel, 2013) to be 'dated…[and which] contemporary philosophy of science has countered'. Further, as Healey and Hodgkinson (2014) suggest, positivism/reductionism are not the only philosophical frameworks which can be employed to apply neuroscience to management and organizational research.

Rather, we wish to focus attention on the link Lindebaum (2016) draws between positivism and ethics. Lindebaum (2016: 544) ominously warns that '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.' Of course, we are sure that Lindebaum is well aware of the considerable attention paid to such issues within the wider field of cognitive neuroscience (e.g. Roskies, 2002; Greene, 2015; Greene and Haidt, 2002; Moll et al., 2005), including the possible ethical implications of the application of neuroscientific technologies to enhance function in healthy populations. Indeed, such is the groundswell of this debate that it has engendered the development of a new branch of bioethics that is solely dedicated to the discussion around such potential risks – neuroethics (Moreno, 2003). The field benefits from dedicated journals, a unified regulatory body which

also manages an annual international conference in the field and the awarding of annual international prizes for excellence in such endeavors[3]. Further, while Lindebaum (2016) may criticize what he calls ON for lacking consideration for ethics, OCN work (e.g. Senior et al. 2008b; Senior et al., 2011a) has routinely explored the ethical implications of these issues for management research and practice. In this light, it is not clear why it is safe to imply some necessary connection between positive paradigms and a lack of consideration of ethics, when so much evidence is there to the contrary.

In fact, the issue of cognitive enhancement is so controversial that the area itself has generated its own discourse (Sahakian and Morein-Zamir, 2007). However, examination of the actual use of cognitive enhancing applications shows that the phenomenon is likely to be more sensationalist media reporting than actual practice (Partridge et al., 2011), and to our knowledge there has been no reported case of any actual cognitive enhancing study carried out in a management context. However, to assess the potential risk of cognitive enhancement within a management context, Senior and co-authors previously presented a thought experiment which highlighted the most realistic means by which management behavior could be enhanced (Senior et al., 2008b). This early discussion focused on the possible modulation of the neurotransmitter monoamine oxidase inhibitor and subsequent facilitation of moral decision making in effective organizational managers. Yet, despite this presentation of what may be a feasible manner in which such behavior could have been facilitated, putting science fiction aside, there is very little chance that such leadership behavior could be directly modulated at all. Indeed, our caveat at the time 'In this scientific age, to fully understand behavior means that we must explain it as a complex interaction among genes, the brain, its biochemical state, the person's family upbringing, the way society has treated him or her, and the stimuli that impinge upon the person' still remains true (Senior et al., 2008b: 55).

Drawing from the above arguments, in response to Lindebaum’s (2016) criticisms

regarding the ethics of neuroscientific research in management and organizations, we would go so far as to say that they may be overstated, and indeed the relationship between ethics and our examination of the brain is more symbiotic that previously thought. Indeed, some scholars have even argued that the practice of cognitive neuroscience with this unique technological approach is actually *essential* for the continued development of normative ethics (Greene, 2014).

The use of imprecise motherhood statements

Finally, we briefly turn to Lindebaum's (2016) second supply-side force. By motherhood statements, Lindebaum (2016: 542) means '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.' However, this is slightly distinct from the more formal definition of the phrase; 'adjective platitudinously endorsing that which everyone accepts as worthy: motherhood statement…' (Deverson and Kennedy, 2005), and in light of this we must clarify that our following discussion refers to Lindebaum's (2016) usage. We agree with Lindebaum (2016) that motherhood statements are not trivial, and can have immense power in ensuring that public perception is swayed towards a specific concept. Indeed, it is here that we have a possible mechanism in which social fallacies such as the 'Grandmother Cell' (referring to neurons that are specifically activated on presentation of a specific individual, see Gross, 2002) are born, or indeed the now-infamous claim that we are 'in love in with our iPhones' (Lindstrom, 2011). Needless to say there is already a strong argument against inappropriate use of motherhood statements where scholars have gone to strenuous efforts to debunk the use of statements, such as 'the buy button in the brain' (e.g. Lee et al., 2012b; Senior and Lee, 2008).

Again we find that previous work on OCN already makes similar points to Lindebaum (2016), regarding caution in interpreting neuroscientific research on management and organizational topics. That said, despite what we feel is appropriate concern over motherhood statements, it is worth considering the importance of such statements in any developing field, and the role that they play in driving debate to consolidate any given theoretical position. Indeed, scholars have argued that the generation of such formative language is actually *necessary* prior to the consolidation and development of a specialist vernacular. This is a natural process of evolution in any emerging discipline and one that takes place during the early stages of its formation to ensure that a common trade language within that discipline is created (Levallois et al., 2012). This does not mean that the whole research agenda should be seen negatively, but rather it is an opportunity to create an environment in which informed debate and knowledge exchange can take place. As such, in mitigation of the examples that Lindebaum (2016) highlights, though we would not wish to condone them, in a nascent field like OCN it is likely that there will be such instances as academics and practitioners reach for a deeper understanding of a new subject and its potential applications. As the field grows and develops, we would hope more robust debate over these terms would also develop.

Some suggestions for future research

Having critiqued Lindebaum's (2016) arguments about the demand and supply side of the research economy associated with neuroscientific approaches to organizational research, we now indicate some suggestions for future research. These suggestions go beyond the direction laid out by Lindebaum (2016). Even so, we agree with Lindebaum (2016: 545) about the need for '*direct* replication' of OCN research studies, ideally 'with an increase in sample sizes', but for different reasons to those suggested by him. Earlier, we have shown

that sample size *per se* is something of a red herring in the statistical power debate, but there does appear to be a trend towards an increase in sample size, as neuroscientific technology develops. Direct replication and extension of key OCN research studies will help to tease out further research contributions and potential implications for management practice, as well as – like in any other field – increase confidence in the state of current knowledge. Indeed, many fields of social science, including management and organizational research, are facing similar issues regarding replication. This is not an issue specific to neuroscience.

Unlike Lindebaum (2016), we do not see the burgeoning use of neuroscientific research methods as a negative, but an indicator of an exciting potential to understand the brain in the context of management and organization studies, and by doing so increase knowledge. Note however, that the OCN approach explicitly avoids claiming that neuroscientific methods are to be used in isolation, but instead should be placed in their appropriate context. Because Lindebaum (2016) narrows his lens on fMRI, he misses the breadth of the OCN research landscape; whereas Butler et al. (2015) revealed that there is a plurality of research methods being adopted by neuroscientific studies of decision-making within the organizational behavior cluster. The plurality of methods includes hormonal research, neuroimaging, EEG, as well as the more traditional approach of EP (see Saad and Greengoss, 2015). Collectively, the methods will reveal new knowledge, which of course as one outcome has 'the potential to help academics and practitioners to understand the biological components of decision-making, the mechanisms that link the components, and the outcomes of the decisions.' (Butler et al., 2015: 15), but also to help us understand the wider context that we operate in as biological beings (see Senior et al., 2011a; Healey and Hodgkinson, 2014). In a previous study, Butler (in press), however, is cautious about the uncritical application of OCN research findings to practice, emphasizing the current

limitations of empirical research, which is still gathering evidence about understanding the basic science of OCN.

Butler et al. (2015), in mapping the diffusion of OCN research, found that Nicholson's (2005) fertile area of neuroeconomics has expanded to include neuromarketing and organizational behavior. Nevertheless, Nicholson's (2005) suggestion to explore the two levels of a bi-directional account of individual agency and social context, and the analysis of the co-evolution of human nature and culture, seem ripe for a rich stream of research activity. This is in line with Lindebaum's (2016) advocacy of taking more account of a critical realist perspective within OCN research (Healey and Hodgkinson, 2014). In adopting the two-level approach, it becomes clearer that, from a certain point of view, OCN research is not simply embedded within the positivist paradigm, but takes a more integrative approach.

In terms of the notion of social embodiment, that is, OCN knowledge systems set in the context of their social environment (Healey and Hodgkinson, 2014; Lee et al., 2012b), then there are fascinating new lines of enquiry opening up. OCN research has an important role in conceptualizing further how social embodiment makes life sensible 'in the lived experience of everyday, ordinary interactions and conversations with others and ourselves…across time and space…as we attempt to interweave multiple, alternative and contested narratives and stories…in which we cannot separate ourselves, our senses, our body and emotions.' (Cunliffe and Coupland, 2011: 64). In one example from another field, cognitive science is being used to theorize how artificial intelligence interconnects brain, body and world (Clark, 1998). Another example is architecture, where the work of Robinson and Pallasmaa (2015) and Butler (2016) are exploring how neuroscience, embodiment and the future of design can co-evolve. In other words, matter and mind are being integrated. In a previous study, which introduces the notion of 'The Embodied Machine', Butler (2016) is beginning the process of reflecting on the facets of human interaction with machines and

technology. This will be increasingly important as digital technology and devices keep evolving (Eagleman, 2015).

Conclusion

Lindebaum's (2016) critical essay appeared intent on showing that the emerging neuroscientific positions could be perceived as being in opposition with each other. However, there is much common ground, especially a belief in generating new and, crucially, sound management and organization theory. Common ground also extends to the distinction between demand and supply side forces in acting as drivers for new fields such as OCN research.

Where we differ from Lindebaum's (2016) philosophy is in two areas. The first area is our assessment of the supply side forces. Lindebaum (2016) focuses on three negative characteristics: the validity of neuroimaging data, the use of imprecise motherhood statements, and the supply of ethically devoid theoretical and empirical advances. These were addressed to reveal a more positive interpretation of the practice of OCN research.

The second area is the potential contribution of OCN research to understanding managerial decision-making. We remain steadfast in our belief that continuing investigation into management and organizations incorporating a cognitive neuroscience perspective has significant potential to do good, both in terms of knowledge development, and also management and organizational effectiveness.

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Notes

1. Indeed, issues of over-power (rather than under-power) are routinely considered in fields such as epidemiology, where the N=1 approach avoids the potentially misleading conclusion that a particular treatment is effective based on statistical inference – which may in fact not reveal the complete lack of treatment efficacy in some members of the patient cohort (Guyatt et al., 1986).

2. We thank an anonymous reviewer for suggesting the replacement of 'subjects' with 'participants', including when it appears in direct quotes.

3. Further details of the various activities that are delivered by the International Neuroethics Society can be found here: http://www.neuroethicssociety.org/.

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Dr. Michael Butler is Reader in Transformational Change, and Founder/Director of The TRANSFORMATION Project (www.thetransformationproject.co.uk), Aston Business School. His research interests focus on understanding change from a multi-disciplinary perspective (institutional, biological and historical). He is on the Editorial Board of the *International Journal of Complex Governance Networks* and *Asia Pacific Journal of Business Administration*, and Review Editor, *Frontiers in Psychology, section Educational Psychology*. He publishes in leading management and organization journals (*British Journal of Management*, *International Journal of Operations and Production Management*, *Journal of Management*, *The Leadership Quarterly*, and *Organization Science*). He has co-edited three books (the most recent, the CIPD's *Introduction to Organisational Behaviour*). His engaged scholarship (funded to c£2.5m) has been recognised (CMI and ESRC), and the OECD used his research to evaluate international policy implementation. An ERDF project facilitated measurable growth across 38 SMEs. [Email: m.j.r.butler@aston.ac.uk]

Prof. Nick Lee (BCA, BCA(hons.), Ph.D.) is Professor of Marketing at Warwick Business School. His research interests include sales management, social psychology, research methodology, and ethics. Prof. Lee is the Editor in Chief of the *European Journal of Marketing*, and the Section Editor for Sales Research Methods for the *Journal of Personal Selling and Sales Management*. Nick has received multiple awards for his research, and published over 70 articles since 2005 in journals such as *Organizational Research Methods, Organization Science*, the *Journal of Management*, the *Journal of the Academy of Marketing Science*, the *Journal of Business Ethics*, *Frontiers in Human Neuroscience,* and the *International Journal of Psychophysiology*. His work has received over 3500 citations, while also featuring in popular outlets such as *The Times*, the *Financial Times* and *Forbes*, and on

numerous radio and television programs including *BBC Breakfast*, and *Al Jazeera*. He is also the author of *Doing Business Research* and *Business Statistics using Excel and SPSS.* He received his Ph.D. from Aston University (UK) in 2003. [Email: nick.lee@wbs.ac.uk]

Carl Senior joined Aston University in 2004, after holding a visiting fellowship in Behavioural Sciences at the National Institutes of Health, USA. He has over 100 publications and has contributed to securing over £3M in various research income. He has served as Associate Editor for the journals Cognitive Neuroscience, Frontiers in Cognitive Science, and The Leadership Quarterly and was elected a Fellow of the British Psychological Society in 2015. He is currently a member of the Aston Behavioural Insights Group. [Email: c.senior@aston.ac.uk]

Corresponding author:

Dr. Michael JR Butler

Aston Business School

Aston University

Aston Triangle

BIRMINGHAM

B4 7ET

UK

Telephone: 07919 926362

E-mail: m.j.r.butler@aston.ac.uk

Other author(s):

Professor Nick Lee

Warwick Business School

Warwick University

COVENTRY

CV4 7AL

UK

E-mail: nick.lee@wbs.ac.uk

Dr Carl Senior

Life and Health Sciences

Aston University

BIRMINGHAM

B7 4ET

UK

E-mail: c.senior@aston.ac.uk