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Of BOLD Claims and Excessive Fears: A Call for Caution *and Patience* Regarding Political Neuroscience

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Published and informal assessments of the prospects for neuroimaging in political science have tended to range from overexuberant to reflexively dismissive. We seek to present a cautious but fair middle ground in considering this new methodology, primarily from an epistemological perspective. Our examination centers on the relationship between two levels of analysis, focusing on the potential for connection between behavior-based theories of political psychology and cognition and the neural processes and systems involved in generating behaviors and states of mind. We explore the place of each level of analysis on its own, as well as the potential for the fruitful interaction of the two. This analysis brings together opinions and ideas presented by others in various forums and across multiple disciplines, offers a discussion of the the promises and perils of neuroimaging in its application to social science, as well as some practical thoughts regarding its early-stage incorporation into political psychology. We argue in favor of proceeding with more substantial incorporation of neuroimaging into political psychology's methodological arsenal, but note that this will initially require both (1) greater acceptance of work more focused on presenting empirical results than on providing dispositive evidence in broader theoretical debates and (2) a commitment on the part of those conducting this research to refrain from overstating the definitiveness of its theoretical implications.

KEY WORDS: social neuroscience, neuroimaging, fMRI, political behavior, reverse inference

Introduction

Recent articles have expressed enthusiasm for the use of emerging techniques (including Functional Magnetic Resonance Imaging, or fMRI) in neuroscience and cognitive psychology for the study of political psychology (Fowler & Schreiber, 2008; Lieberman, Schreiber, & Ochsner, 2003; McDermott, 2004, 2009). While publication of new research of this kind in political science journals has been limited, working papers, conference presentations, a special issue of this journal and at least one dissertation suggest that numerous studies are in the pipeline (Cacioppo & Visser, 2003; Schreiber, 2005). This, along with the explosion of neuroimaging work in social psychology and its rapid growth in economics, suggests that an exploration of the question "Where Do We Go From Here?" should consider the potential and appropriate place of these methods in political psychology. Informal dialogue among scholars on this topic ranges from overexuberant to reflexively dismissive, and it can often seem that most of the voices in the conversation come from one of these end points. As Aue, Lavelle, and Cacioppo (2009) put it: "Overreaching enthusiasm such as the idea that fMRI can reveal lies and political attitudes are as common as the opinion that fMRI, in its current form, is useless for the advancement of psychological theories" (p. 10). Those

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political psychologists who have knowledge of these techniques and have invested time in familiarizing themselves with the neuroimaging literature and methodology can find themselves in the position of advocating for their use. In practice, this may mean overstating the current applicability of neuroimaging in political psychology and overselling the extent to which it is likely to change the way we define knowledge in our subfield. There is also a tendency to highlight the ways in which these new methods are superior to existing ones. After all, many of the proponents of their use were initially motivated by a frustration with the ability of standard methods to address questions in their research programs. Some scholars, on the other hand, seem perhaps too eager to dismiss the possibility that neuroimaging might be meaningfully brought to bear in our discipline. In practice, this can mean highlighting limitations of these new techniques while simultaneously ignoring the same or similar limitations of more standard methods. It is easy to imagine how these opposing sides might feed on each other, producing a vicious cycle. This is not atypical of methodological discourse within disciplines. But, perhaps some of the negative consequences can be avoided with regard to political neuroscience.

Recent methodological evolution in political science has tended to not simply add to the arsenal but to alter the standard of proof. When the latter happens, existing empirical work can sometimes be "relegated" by adherents of a new method to the level of theoretical claim or even folksy conjecture. This is, understandably, a threatening proposition for those whose life's work is rooted in certain methodological paradigms. The future role of neuroscience in political psychology need not be viewed as a higher standard of proof looming on the horizon that will transform work done to date into early theoretical building blocks. Rather, we believe neuroscience has the potential to be integrated into political psychology primarily as a window into a different level of analysis. The sociopsychological level of analysis has been the mainstay of work on political behavior. Modern imaging techniques and neuroscientific advances have now begun granting access to a neurobiological level of analysis. Our discussion is focused on the near-term potential for the fruitful interaction of the two levels (rather than the prioritization of one over the other), and offers some thoughts regarding how the field might deal with access to this relatively new level of analysis in the immediate future.

Psychology bridges the divide between social science and biological science, and so should *political* psychology. This can include the examination of behaviors and cognitive processes from the perspective of the physiological substrates from which they emerge. As epidemiology benefits from both biostatistics and microbiology, so can political psychology benefit from analysis of political cognition and behavior at different levels. Behavioral studies have and will continue to shape the direction of neuroscientific work. Eventually, work in social neuroscience on the microfoundations of political cognition may help inform behavioral work. Getting to the point when the latter is true will require a certain amount of patience. We are likely to see many studies on political subject matter with interesting empirical results but light on clear theoretical implications. Such work may be a necessary step in the development of a political neuroscience that is directly relevant to the larger theoretical debates in our discipline. Allowing it to develop properly may require us to be more accepting of studies that largely present methods and findings, rather than fully exploring theoretical and causal significance. Such tolerance might encourage less overselling of implications on the part of those applying these methods to politics.

Intentions and Caveats

This article is intended to be relevant to political psychology as a whole, but particularly addresses those, like us, early in their careers contemplating the likely place of neuroimaging in the field during the coming decades. Individual scholars and the discipline generally must decide whether to invest the substantial time and human capital required to gain fluency in the latest

neuroimaging methods and neuroscience literature; and we, individually and collectively, must determine how readily we incorporate findings arising from these methods into our compiled knowledge, and with what weight. When we discuss *political* behavior we are referring generally to actions by individuals and groups related to collective outcome generation in the public sphere. When referring to *political* psychology we have in mind the interdisciplinary effort to examine political behavior in a psychological light.

There is a tendency to group many applications of methods and theory from the biological sciences together when discussing their relevance for political science and political psychology (Fowler & Schreiber, 2008; McDermott, 2009). Behavioral genetics, evolutionary psychology, biological anthropology, endocrinology, and neuroscience are often bundled. There are certainly overlapping issues when grabbing arrows from the quivers of disciplines more squarely rooted in biology. But, while we understand the inclination to consider them in concert, we have tried to repress it here. Such a large group of fields and their implications for political psychology cannot receive any meaningful treatment in a single article. That task is likely impossible even when considering neuroimaging all by itself. Furthermore, the promises and challenges associated with these many approaches do not reduce cleanly to a few dimensions. Trying to consider them all, or even more than one or two of them at once, leads not only to insufficient examination of important issues, but to confusion and incorrect association. Instead we focus our attention narrowly on neuroimaging. Our greatest familiarity is with fMRI, which also is the fastest growing and most discussed method of neuroimaging today. Much of what is written in this article likely applies more broadly to other neuroimaging methods, but none of it should be construed as an effort to comment on genetics or anthropology or endocrinology.

We believe the ideas expressed here can contribute to a discussion about the direction of research heading forward in the near or intermediate term. We are not making predictions regarding the interaction of neurobiology and social science at a time when the methods, mapping, and models have granted us full understanding of neural function, from the level of the neuron and synapse to the level of full neural networks and systems. That bridge should be crossed when we come to it, by scholars with the benefit of knowing what it looks like. None of us knows how fast such a time approaches, but it seems clear that it is not in the immediate future.[1] Thus, some of our discussion undoubtedly has a relevance half-life, albeit a relatively long one in the context of academic careers.

fMRI Basics

Neuroscience has been transformed in recent decades by advancements in the ability to measure neural activity in healthy humans. Earlier research was often limited to studies of animals or individuals with known neurological deficits and lesions. Noninvasive techniques allow research to overcome these limitations. The development of fMRI has played an important role in this progress. MRI technology allows for the imaging of soft tissues based upon the physics of nuclear resonance. The technique exploits the physical properties of atomic nuclei to create images.

Blood flow in the brain is regulated to account for differential neuronal activation. Neurons require oxygen and glucose drawn from the blood to function, particularly during periods of peak activity. fMRI measures hemodynamic changes in the brain by repeated scanning and exploitation of the difference in magnetic properties between oxygenated and deoxygenated blood. The data generated are based upon the oxygenation of blood flowing through the brain, which is referred to as the Blood Oxygenation Level Dependent (BOLD) signal. At the most basic level, it is observed to

[1] In fact, the method with which we are primarily interested (fMRI) does not currently have the capacity to examine neural activity with the spatial (and certainly not temporal) resolution required for a complete understanding.

produce a dip when a particular part of the brain is activated (the neural activity deoxygenating the surrounding blood) and a spike following activation (as blood is redirected). Hemodynamic changes in the brain appear to be closely related to neural activity (Logothetis, Pauls, Augath, Trinath, & Oeltermann, 2001).[2]

fMRI offers several advantages over other methods of observing brain activity. It is non-invasive. It does not expose patients to ionizing radiation, which positron emission tomography (PET) or computed tomography (CT) scanning do, making it safe for prolonged and repeated use on subjects. It offers greater spatial resolution than electroencephalography (EEG) techniques used in evoked potentials (EP) or event-related potentials (ERP) studies. EEG, on the other hand, produces data with far greater temporal resolution.

Procedural Challenges

There are practical challenges inherent to the method still being resolved by fMRI practitioners. These have been discussed at great length elsewhere (McDermott, 2009; Tingley, 2006), so we will provide only a brief overview. The technological infrastructure needed is expensive, so start-up costs are prohibitive for many researchers. EEG is far less expensive. There are also limitations on the subject pool. Subjects cannot be claustrophobic or have pacemakers, metal implants, and some types of tattoos or piercings. Subjects must remain still in an enclosed, noisy tube during the course of the experiment. fMRI experimental paradigms require greater attention to details of design, such as temporal sequencing and precision of control conditions, than is necessary for more standard methods in our field. For example, unlike instruments employed in survey research, the use of fMRI requires that the researcher design an experiment that induces theoretically compelling temporal sequencing of neural events associated with political behaviors. Our analysis of survey responses typically does not rest on certainty that all respondents are processing the key stimuli at precisely the same instant. This sort of precision is often essential in fMRI paradigms.

While there is substantial evidence that the hemodynamic response is a proxy for electrical neural activity, it is important to note that fMRI is measuring the BOLD signal, not the underlying electrical and chemical processes that make up brain function (Heeger & Ress, 2002).[3] This requires great attention to details of structural features of blood flow in the brain and the complex statistical modeling involved in analyzing the very large data sets generated by fMRI studies.

This statistical analysis has been the subject of extensive discussion, criticism, and defense (Dobbs, 2005; Lieberman, Berkman, & Wager, 2009; Logothetis, 2008; Nichols & Poline, 2009; Poldrack & Mumford, 2009; Vul, Harris, Winkielman, & Pashler, 2009; Yarkoni, 2009). It is not within the scope of this article to wade deeply into that debate, and the volume of ink spilled elsewhere suggests correctly that a few sentences could not do such an effort justice. However, our discussion of the method generally requires a brief comment. The widely circulated Vul et al. critiques especially highlight the importance of further development and testing of statistical correction techniques demanded by the nature of fMRI data. As in other fields, there is still clearly work to be done in bolstering confidence regarding the methods used to ask questions of data. The critiques are not, however, damning of all social and cognitive neuroimaging analysis. They most directly call into question the *magnitude* of some of the correlational claims common in social neuroscience. There remains widespread consensus in the neuroimaging community on the use of the general linear model for examination of data. The critique is important, however, as a reminder that the statistical tools undergirding the analysis of these data will continue to be developed. We do not see the Vul

[2] For background on fMRI technology, see: Cabeza and Kingstone (2001); Huettel, Song, and McCarthy (2004); Jezzard, Matthews, and Smith (2001).

[3] Progress is now being made in efforts to simultaneously measure electrical signals and hemodynamic flow, which could maximize overall spatial and temporal resolution.

et al. critique as evidence that findings in social neuroscience should be ignored. As in other domains of social inquiry, the development of standard methods and user-friendly analysis packages has enabled the employment of techniques without full recognition of the assumptions inherent in them. This produces substantial variance in the quality of scholarship. Given this and the relative youth of this field, it is particularly important that consumers of neuroimaging work demand great transparency in the reporting of methods with findings.

Growth of Social Neuroscience

Social neuroscience has emerged as a boom field in the last dozen or so years (Adolphs, 2003a, 2003b; Blakemore, Winston, & Frith, 2004; Cacioppo, 2002; Cacioppo & Berntson, 2005; Decety & Keenan, 2006). This emergence has produced new journals, numerous handbooks and textbooks, scholarly societies and meetings, as well as dedicated graduate programs. The bulk of this work has been in social psychology. Neuroscience has also found application in behavioral economics (Camerer, Loewenstein, & Prelec, 2005). However, the use of imaging technology has been limited thus far in political psychology, so political neuroscience remains a smaller movement than its equivalents in economics and psychology.

This is despite the fact that many findings in social neuroscience would seem to have implications for political psychology. A great deal of research has focused on identifying the neural structures involved in processes at the heart of social cognition, such as self-awareness, self-referencing, self-regulation, and mentalizing (e.g. Adolphs, 2009; Decety & Sommerville, 2003; Fletcher et al., 1995; Gusnard, Akbudak, Shulman, & Raichle, 2001; Heatherton, 2011; Heatherton et al., 2006; Jenkins & Mitchell, 2010; Johnson et al., 2006; Kelley et al., 2002; Mitchell, 2008; Mitchell, Banaji, & Macrae, 2005; Mitchell, Macrae, & Banaji, 2006; Mitchell, Cloutier, Banaji, & Macrae, 2006; Moran, Macrae, Heatherton, Wyland, & Kelley, 2006; Northoff & Bermpohl, 2004; Northoff et al., 2006a,b; Saxe & Kanwisher, 2003; Schulte-Ruther, Markowitsch, Fink, & Piefke, 2007; Turk, Heatherton, Macrae, Kelley, & Gazzaniga, 2003; Völlm et al., 2006). Research has also associated neural structures with phenomena such as attributional inference (e.g. Blackwood et al., 2003; Lieberman, Gaunt, Gilbert, & Trope, 2002), group-based social perception (e.g. Eberhardt, 2005; Harris & Fiske, 2007; Phelps & Thomas, 2003; Phelps et al., 2000), stereotype threat (e.g. Derks, Inzlicht, & Kang, 2008), social decision making, moral judgment and attitude processing (e.g. Cunningham, Raye, & Johnson, 2004; Forbes & Grafman, 2010; Rilling & Sanfey, 2011), disgust (e.g. Moll et al., 2005), and dynamic social interaction (e.g. Rilling et al., 2008; Turk et al., 2004). Also, a great deal has been learned already about the neural processes defining the role of affect and emotion in cognition, perception and memory (e.g. Beaucousin et al., 2007; Davidson, Jackson, & Kalin, 2000; DeYoung & Gray, 2009; Northoff et al., 2006a; Ochsner, 2004; Phelps, 2004, 2006). This fast-developing branch of social neuroscience has already spread into political science in certain ways (Marcus, Sullivan, Theiss-Morse, & Stevens, 2005; Marcus, Neumann, & Mackuen, 2000). Lieberman (2007) provides a rich review of the neural processes associated with various elements of social cognition.

The remainder of this article explores some key issues in using theory from political psychology to drive neuroscience research and critical steps in incorporating results from neuroscience into our theories of political behavior. We assess the potential for fruitful interaction between the neurobiological level of analysis and the sociopsychological one.

Requirements for the Ideal Application of fMRI Methods to Political Psychology

Social neuroscience is generating a wealth of information about the location of neural activation when subjects perform various tasks and are presented with certain stimuli. Political psychology

contains a tremendous body of theory about political behaviors and the thought processes involved in them. For a coherent link to be made between the two fields, we must steer clear of the common pitfalls associated with this interdisciplinary endeavor—in particular, the misstep of too readily taking neural correlates of politically relevant behavior as clear evidence regarding theories about political phenomena. In full maturity, scholarship in political neuroscience might meet these criteria:

(1) We must have precisely articulated theories regarding political behavior.
(2) We must identify clear hypotheses at the level of neuronal activation that are directly linked to the aforementioned theories. This requires significant confidence in the neuroimaging, lesion, and animal studies that have produced the function-to-structure mapping we are seeking to exploit (Henson, 2005, 2006). That confidence should extend beyond mere correlation between behavior and region of brain activation, to the elimination of confounding descriptions of neural function. Confidence in functional exclusivity is necessary for structure-to-function induction or reverse inference (Henson, 2005, 2006; Poldrack, 2006).
(3) We must design experimental paradigms that, given the constraints imposed by imaging methods, effectively isolate elements of theories we are examining with a level of conceptual and temporal precision far greater than is demanded by most other methods prominent in political psychology.
(4) The collection, analysis, and interpretation of imaging data are a product of a host of assumptions. We must have full confidence in the suite of techniques used in the analysis of fMRI data and the premises upon which these techniques are based.

Shortcomings at any stage discount the reliability of research and the weight scholars can afford evidence gathered. There are challenges and reasons for concern at each point. We focus primarily on issues associated with items 1 through 3, with special attention paid to item 2. The ideal presented here is likely an impossibly tall order for political neuroscience in the near future. It is intended to represent a high standard for the use of fMRI in political psychology.

We are not aware of any currently published work that fully brings neuroimaging to bear within political behavior in the manner described above. However, political topics have already made their way into the larger field of social neuroscience. Much of the published work that could be described as early political neuroscience has been produced by scholars in other fields and published in journals outside of political science.

Westen, Blagov, Harenski, Kilts, and Hamann (2006) use political stimuli to analyze the "Neural Bases of Motivated Reasoning." The authors conducted a study during the 2004 election in which they expose a sample of strong partisans to contradictory (threatening) and exculpatory statements regarding the presidential candidates. They claim to show distinct neural correlates of motivated reasoning. Rule et al. (2010) discovered increased amygdala activation in subjects when they considered candidates for whom they voted in a simulated electoral task. Kaplan, Freedman, and Iacoboni (2007) examine neural activation patterns among partisans looking at images of the 2004 presidential candidates. The authors "found that compared with viewing one's own candidate, viewing the candidate from the opposing political party produced signal changes in cognitive control circuitry in the dorsolateral prefrontal cortex and anterior cingulate, as well as in emotional regions such as the insula and anterior temporal poles. BOLD signal in these regions correlated with subjects' self-reported ratings of how they felt emotionally about the candidates" (p. 55). Knutson, Wood, Spampinato, and Grafman (2006) present a similar study, but examine subjects in the process of performing an Implicit Association Test using candidate images. Kato et al. (2009) examine neural correlates of attitude change. Subjects were shown negative political advertisements. They find differential activation patterns between subjects who maintained their candidate ratings and those who changed them. Zamboni et al. (2009) use multidimensional scaling, survey and fMRI data "to

identify which criteria/dimensions people use to structure complex political beliefs and which brain regions are concurrently activated" (p. 367). In an EEG study, Amodio, Jost, Master, and Yee (2007) examine neural evidence regarding divergent cognitive style between liberals and conservatives, and Kanai, Feilden, Firth, and Rees (2011) identify corresponding structural differences in young adults.

Some of this research comes as close as can currently be expected to the standard we set above. But, for the most part, it does not seem to be directed substantially at an audience of political behavior scholars and does not explicitly address debates in that subfield. Some of these projects are designed and presented in such a way that they speak somewhat to theory in political psychology. Others, though, do not and are best described as making forward inferences (in the direction of function-to-structure) about neural activation using political stimuli. Work in this genre might be viewed as unsatisfying by many political psychologists and lacking in terms of immediate applicability to existing research programs in our field. It is, however, a type of analysis relatively common in social neuroscience, and it may represent an essential step in the development of political neuroscience. One might argue that such work should simply be allowed to develop in these other disciplines. However, the scholars pursuing it are not usually concerned with the same questions motivating those of us whose primary interest is politics. There are likely ways in which the active participation of political behavior scholars might help make the results of such studies more relevant to the field. Such involvement could also encourage those scholars to develop theories more precisely conducive to exploration through neuroimaging, aiding the proper development of a more mature political neuroscience.

Levels of Analysis and Constraint

In political psychology, our theories and models are developed from and designed to deal with phenomena at the level of observed and measurable behavior. Even responses to survey items measuring underlying attitudes are themselves a behavior. Centuries of research have contributed to the language and conceptual framework used to describe and consider behaviors. This is true even when the topic of discussion is implicitly the cognitive mechanisms underpinning them. Our theories are rooted at this level, and one might argue this is the level about which we as political psychologists ought to care the most. It is by no means a given that the actual physical neural processes behind the attitudes and behaviors we care about will map neatly onto the way we discuss behavior and states of mind. If we develop a model of mind based upon external observation of behavior and subsequently observe that this model is not reflected in neuronal activity, would this mean that our model was misguided? We do not believe this is necessarily so. We also argue, though, that such inconsistencies should provide fodder for further exploration and consideration, and ideally refinement, of our models, concepts and theories.

Mitchell (2010) compares this issue regarding levels of analysis in social neuroscience to the classification of beetles. Whereas the division of the Coleoptera order into species and subspecies was long based on morphology, classification can now be made using genetic code, which has not always corroborated the appearance-based findings (e.g. Hunt et al., 2007; Maddison, Baker, & Ober, 1999). Mitchell believes that neural evidence can be similarly instructive with regard to our theories and conceptualizations of behavior and states of mind. Our observation-based classifications of behavior and mental states may not match up with the neurophysiology behind them. Neuroimaging evidence has the potential to help us identify inconsistencies and highlight areas of future theoretical focus. But the beetle analogy likely overstates the position of neuroimaging in political psychology. Since taxonomy seeks to classify organisms on the basis of evolutionary proximity, the genetic classification is, in that case, a more definitive level of analysis. We do not believe this strict hierarchy applies to the relationship between levels of analysis in political

psychology. In fact, it may work the other way, with theories regarding political behavior driving neuroscientific research.

Edward O. Wilson (1998) argues for the dominance of biological approaches:

> The full understanding of utility will come from biology and psychology by reduction to the elements of human behavior followed by a bottom-up synthesis, not from the social sciences by top-down inference and guess-work based on intuitive knowledge. It is in biology and psychology that economists and social scientists will find the premises needed to fashion more predictive models, just as it was in physics and chemistry that researchers found the premises that upgraded biology. (p. 206)

This passage is used as an epigraph by Glimcher and Rustichini (2004) in a piece describing neuroeconomics from a moderately bottom-up perspective. Kihlstrom (2010), on the other hand, takes great exception to the implied hegemony of neural data relative to psychological theory. He describes this as the "rhetoric of constraint" and argues that when cognitive neuroscientists "assert that psychological theories are constrained by neuroscientific evidence, the idea is that evidence about brain structure and function will somehow determine which theories of cognitive function are right, and which are wrong" (p. 760). His quarrel is with:

> . . . *eliminative materialism*, sometimes disguised as *intertheoretic reductionism*, which asserts that the language of psychology and the other social sciences is at best an obsolete *folk-science*, and at worst misleading, illegitimate, and outright false. In this view, psychological concepts such as belief, desire, feeling, and the like have the same ontological status as *vital essence*, *the ether*, and *phlogiston*—which is to say they are nonexistent, and should be replaced by the concepts of neuroscience (Churchland, 1981, 1995; Churchland & Churchland, 1991, 1998; Churchland, 1986; Stich, 1983). (p. 773)

Kihlstrom argues that even within cognitive neuroscience, neural evidence has not provided the definitive evidence to constrain psychological theory:

> Once we have a good description of some process at the psychological level of analysis, then we can try to determine how the brain does it, and the presence of a valid psychological theory allows us to make valid interpretations of what we see at the neural level. But if the psychological analysis is wrong, the analysis of neural function will be wrong as well. That is because cognitive and social neuroscience depend on cognitive and social psychology; but cognitive and social psychology do not depend on neuroscience. The constraints go down, not up. (p. 762)

He points to what many would argue is among the clearest cases of neural evidence proving the validity of a theory based upon behavioral evidence. H.M., one of the best known lesion subjects in the history of neuroscience,[4] had most of his hippocampus removed to halt debilitating seizures. This procedure left him able to perform tasks that required very short-term memory, but not able to form longer-term episodic memories. This provided evidence of the role of the hippocampus in encoding long-term memories, valuable data linking structure and function. Researchers also observed that he *was* able to form procedural or motor memories. He would gain proficiency by repeating motor tasks, despite an inability to recall performing them in the past, which provided evidence of a neural basis

[4] See, for instance, Corkin (2002); Schacter and Tulving (1994); Scoville and Milner (1957); Squire (2009); Squire and Zola (1996).

for the multiple memory systems about which psychologists had previously theorized and observed behaviorally. Kihlstrom, for one, is not convinced that the neuroscientific evidence from such amnesiac subjects was definitive in this case, arguing that psychological evidence had substantiated the claim. Furthermore, he asserts that evidence from amnesiac patients is fundamentally behavioral. We disagree somewhat with these assessments. It is difficult to say exactly what sort of evidence satisfied the appropriate burden of proof. But we believe that the lesion study evidence in this case was both neural and more compelling. Lesion patients are unique in that their pathology allows for isolation of brain activity; the lesion affords the ability to "see" that a particular region is not active during the completion of a particular task.

What is more significant is that there is any debate at all, seeing as this example is drawn from a domain within psychology fundamentally concerned with describing how the brain accomplishes specific tasks. One can certainly develop theories of memory focused on the way we experience it and related behavioral outcomes, but the debate over memory systems was more about neural structure. Most theories in political psychology, and even political cognition, are less likely to evoke such a tight link between a function and neural structure.

This points to an important nuance in the relationship between sociopsychological theories and neurobiological analysis—a nuance that makes a generalizable position on "constraint" too stark. Some theories are more proximate to neural function than others. Some psychological theories imply description of brain function, while others deal with broader issues of social behavior and cognition. The former are more subject to "constraint" than the latter. In psychology, much behavioral analysis has bordered on neuroscience. This is also the case for some areas of behavioral economics, such as examination of reward processes (which, not coincidentally, has been one of the topics most effectively investigated by neuroeconomics).[5] Such a continuum of constraint should also apply to political psychology. However, by their very nature, political behaviors will tend to be of the more complex, social variety. While political cognition is based upon the building blocks of cognition generally, the element that makes it uniquely political will tend to mean it involves a constellation of these building blocks often over a significant period of time.

There are some models and theories of political behavior that could be construed as models of brain function. Zaller's Receive-Accept-Sample model is one such example (Zaller, 1992). The literature in voting behavior on online processing is another (Lodge, McGraw, & Stroh, 1989; McGraw, Lodge, & Stroh, 1990). (There generally seems to be interest in topics related to cognitive processes, especially when we face an issue of observational equivalence in distinguishing between models.) Even these examples, though, do not approach the level of neural evocation involved when studying something like memory systems.

Therefore, we believe that, for the foreseeable future, political psychology will not be a domain generally subject to bottom-up constraint. Understanding the political brain[6] relies on understanding the political mind. The dependence is not as strong in the reverse direction. Not only is analysis of the social brain not likely to render examination at the level of behavior and state of mind extinct, but the former is only interesting in light of the latter. Theories and concepts in social sciences are interesting *because* they mean something in the real world. Neural structure and function are important to social neuroscience, as opposed to just plain neuroscience, when the same can be said of them. This is a good thing for political neuroscience, because logic dictates that there would be little to study in the absence of those descriptions and explanations.[7] In the case of politics, this

[5] An excellent example of neural evidence contributing to richer theory can be seen in the way in which Levy, Snell, Nelson, Rustichini, and Glimcher (2010) improve upon the work of Huettel, Stowe, Gordon, Warner, and Platt (2006), adding nuance to our understanding of the way in which individuals process risk and ambiguity.

[6] Apologies to Drew Westen (2007).

[7] For a more elaborate exploration of this mostly unidirectional dependence, see Kihlstrom (2010) and also Klein and Kihlstrom (1998).

makes the case for less fear (and boasting) regarding constraint and more hope that neuroimaging work can help provide some illumination for theory development and testing at the psychological level.

Challenge for Theory Building

The challenges for theory building associated with incorporating concepts and findings from another field are by no means new to the study of political behavior. Issues similar to those facing political neuroscience have emerged whenever psychological concepts have been brought into the field. W. Lance Bennett (1981), for example, issued a call for great care in bringing "theories of perception and cognition" (p. 69) into political research. We believe the issues associated with blending political psychology and neuroscience demand even greater than usual theoretical and conceptual rigor.

As we have mentioned, neuroeconomics appears to have enjoyed some early success in linking neuroimaging with social scientific theory. We believe the key to this success is the simplicity of existing theories and the parsimony of existing models in economics. These theories, typically expressed in formal mathematical terms, offer very specific predictions and precise identification of concepts through measurable outcomes and games. On the other hand, the requirement for this very precision often makes the existing models subject to empirical deviation from expectations. In modern behavioral economics, one often begins with a model of "rationality" and then discovers the ways in which actual behavior strays from this model. Neuroeconomics has contributed to this genre of behavioral economics. Economists have provided theory regarding behavior and developed ingenious "games" and lab experimental manipulations to examine that behavior. Social neuroscience is able to exploit these very simple and specific tasks and models in order to identify neural activation correlated with behavioral responses to stimuli (e.g. Wilson, Stevenson, & Potts, 2006). Then, in turn, the models of neural function developed from this and other evidence can be used to build upon theories in economics.[8]

The neuroeconomics example illustrates, in practice, what we mean by "good" or "rigorous" or "robust" theory—precision is the key element. In economics this is often generated through formal or game theoretic modeling. In psychology, such precision is often the product of carefully designed experimental paradigms. These two models suggest the value of further expanding the application of formal theory and experimental methods in the study of political behavior where linkages to the neurobiological level of analysis may exist. Some neuroscientists, on the other hand, have pursued improved precision by developing and testing computational models of neural activation. Theories about *how* neural computation is performed by the brain can not only help identify regions that may be involved in some aspect of the computation, but also serve to test the theory.

Reverse Inference and Modularity

Evidence gained by associating structure with function is sometimes used to identify the behavioral state of subjects through the observation of their brain activation. This is known as "reverse inference" and presents its own sets of problems (Cacioppo & Visser, 2003; D'Esposito, Ballard, Aguirre, & Zarahn, 1998; Poldrack, 2006). It entails (1) establishing the relationship between a mental state and a particular pattern of brain activation and (2) inferring the existence of said mental state when that pattern of activation is later observed. Political psychology can

[8] See, for instance, Camerer et al. (2005); Caplin, Dean, Glimcher, and Rutledge (2010); Glimcher (2003); Glimcher, Camerer, Fehr, and Poldrack (2009); Glimcher and Rustichini (2004); Hsu, Bhatt, Adolphs, Tranel, and Camerer (2005); Rangel, Camerer, and Montague (2008); Rilling et al. (2008); Rutledge, Dean, Caplin, and Glimcher (2010).

contribute to social neuroscience by providing theoretical precision which can be useful for establishing function-to-structure associations of political behavior. Increased confidence in the validity of reverse inferences may eventually allow political neuroscience to speak more directly to broader theories of political behavior. Ideally, we will be able to use knowledge about how the brain works to determine the mental states of subjects, which will assist in efforts to adjudicate between competing theories. The current limits on the ability to make confident reverse inferences is one reason why political neuroscience is not likely to achieve maturity in the near future.

"Neurolaw" provides one practical illustration of the potential perils of reverse inference. Some studies have associated certain activation patterns with the act of lying, prompting some to argue that fMRI may serve as a sophisticated lie detector. Other studies, though have found similar patterns of activation for a variety of tasks (Langleben et al., 2005; Wolpe, Foster, & Langleben, 2005). Applied within the criminal justice system, such questionable reverse inferences obviously would carry serious consequences. The early use of reverse inferences in seeking understanding of particular political behaviors may be similarly misguided. Adding complexity is the fact that modern neuroimaging tends to depend upon an assumption of modularity, which holds that the brain is organized into regions in terms of both structure and function. If, for example, all of the brain is involved in a particular task or if a particular task is performed by a diffuse network of neurons or neuronal areas across the brain, hemodynamic flow and electrical activation measures with limited spatial resolution may not be useful in associating tasks causing brain activation with underlying neural mechanisms (Kihlstrom, 2010). There is evidence from studies using animals and from lesion studies among humans that modularity may make sense for certain tasks, such as vision and recognition of faces. If we wish to justify the use of fMRI in more social tasks, we must be confident that this assumption maintains some applicability at this higher level or that we are able to reliably measure the complex neural processes involved. Mitchell, and other social psychologists, contend that this is the case, and point to fMRI research showing that several different social forms of cognition activate the medial prefrontal cortex (Mitchell, 2009; Mitchell, Banaji, & Macrae, 2005; Mitchell, Macrae, & Banaji, 2006; Mitchell, Cloutier, Banaji, & Macrae, 2006). Even if certain areas and processes are "dedicated" to social cognition, would we expect the same for political cognition? The answer is "probably not." It is easier to imagine the development of distinctly social, as opposed to political, neural processes and brain regions. But while political cognition is likely built upon a general infrastructure of social cognition, there may also be risks associated with studying it in ways that do not fully account for its uniquely political features. This is a theoretical tightrope walk that political neuroscience faces. As pessimistic as this may sound, it should not be construed as an argument to discourage the pursuit of a distinctly *political* neuroscience by scholars in political science or political psychology.

Discussion and Implications

Despite the challenges, we believe the most relevant question is not whether these methods should be brought to bear on political questions. They already have been and will continue to be. What we must answer now are questions such as: To what extent and when should political psychology engage in, encourage and tolerate the pursuit of these techniques by scholars? How readily should we allow their findings to fundamentally shape our theories?

Unlike archaeologists, for whom the dilemma of the "unrepeatable experiment" (Barker, 1993) means that excavation today is tantamount to destroying evidence that better methods could discern down the road, scholars studying living human subjects face the opposite pressure. There is a permanent loss of data that comes with the passing of time. Political scientists who go to great lengths to creatively use public opinion data from periods before survey research achieved maturity (Berinsky, 2006, 2007; Berinsky, Powell, Schickler, & Yohai, 2011; Karol, 2007; Schickler &

Caughey, 2011) likely wish there had been more early work done to gauge attitudes, even if those efforts were rudimentary. Political neuroscientists may one day wish that we had gathered more data during these early stages despite the limitations we face in its collection and interpretation.

Much work in political psychology implicitly or explicitly seeks to develop a model of the mind as it pertains to political cognition, and there are other areas in which neural data might be useful in adjudicating between conflicting claims. Thus, it makes sense that the field will eventually focus even greater attention on the exploration of the workings of the brain. We believe that research on topics related to political psychology using neuroimaging should be pursued and encouraged wholeheartedly. This is despite the rather substantial challenges which make it unlikely political neuroscience will be able to approach the ideal standard we describe for quite some time. Current work in neuroimaging is essential if for no other reason than its potential role in beginning the process of dealing with many of the theoretical, methodological, and measurement issues that require working out before political neuroscience can contribute significantly to our understanding of political cognition. However, we believe it is important that this early, trailblazing work not be oversold in terms of its implications for our discipline. Placing this demand on the scholars producing this work also requires that other scholars, and the discipline as a whole, not demand such an immediate impact from this new and promising set of methods.

The early advancement of political neuroscience will likely require greater acceptance of work that primarily presents results of experiments and which does not authoritatively address broader theoretical questions. Certainly, fMRI work in political science should be expected to be on firm footing with regard to our discipline's theory (which is one good reason for political scientists to be involved in this sort of research). But, it should be regarded as added, not conclusive, evidence. This will allow such empirical evidence to build in the literature. It will enable researchers engaged in political neuroscience to publish results, thus providing some professional reward for their early efforts in this domain. This will also likely improve the validity of the claims made in this early work, because it will not force scholars to oversell the theoretical implications of their findings.

At a very practical level, this venture would also likely benefit from even greater acceptance of coauthorship. This is an area in which disciplinary norms have shifted substantially already (McDermott & Hatemi, 2010), especially in political psychology. However, papers with multiple (and often long lists of) authors are not as much the norm in political science as they are in psychology and neuroscience. Excessive discounting of professional credit for scholarship produced in collaboration with several authors will discourage work in political neuroscience. More recognition that this sort of research is far more likely to be done in collaborative settings increases the likelihood that political scientists will be involved in collaboration with psychologists and cognitive scientists, even as a third or fourth author.

Finally, theories of political behavior can be informed by research in neuroscience, but caution and patience are advised in this regard. Political theories that rely too heavily on generalizations from neuroscience run the risk of eliminating critical context-specific factors that may be determinants of politically specific behavior. Those who conduct fMRI experiments designed to test for politically specific behaviors by mapping them onto brain areas associated with particular brain states should also proceed with caution. Until we know more about the structural-functional organization of the brain, it will be difficult to advance with *full* confidence.

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