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Target Article

A Neuroskeptic's Guide to Neuroethics and National Security

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This article—informed by science studies scholarship and consonant with the emerging enterprise of "critical neuroscience"—critiques recent neuroscience research, and its current and potential applications in the national security context. The author expresses concern about the subtle interplay between the national security and neuroscience communities, and the hazards of the mutual enchantment that may ensue. The Bush Administration's "war on terror" has provided numerous examples of the abuse of medicine, behavioral psychology, polygraphy, and satellite imagery. The defense and national security communities have an ongoing interest in neuroscience too—in particular, neuroimaging and psychoactive drugs (including oxytocin) as aids to interrogation. Given the seductive allure of neuroscientific explanations and colorful brain images, neuroscience in a national security context is particularly vulnerable to abuse. The author calls for an urgent reevaluation of national security neuroscience as part of a broader public discussion about neuroscience's nontherapeutic goals.

Keywords: critical neuroscience, fMRI, national security, neuroimaging, neuroskepticism, oxytocin, psychoactive drugs

It is hard to imagine anywhere darker, more esoteric, and—to be frank—more thrilling than the domain of national security neuroscience. In this article, I explore that intriguing place where neuroscience and national security intersect, each enchanting to the initiates of the other, and both somewhat mysterious to the rest of us. I confess that my aim here is to puncture that aura of mystery and enchantment, to defuse the understandable thrill, and to offer some words of caution—in particular to scientists, ethicists, research funding bodies, policymakers, and anyone else who may play a significant role in shaping the kinds of neuroscience research that will be conducted in the years ahead. Before proceeding, however, I should make two things clear.

First, I readily acknowledge that neuroscience offers unparalleled opportunities to transform our lives, and (for some) it has already done so. Few of these opportunities are more dramatic than the potential use of functional magnetic resonance imaging (fMRI) to identify patients with impaired consciousness who might be candidates for rehabilitation (Owen and Coleman 2008), and of deep brain stimulation to release them from imprisonment in hitherto unresponsive bodies (Schiff et al. 2007). However, my thesis here is premised on what might be called *neuroskepticism*—that is, a perspective informed by science studies scholarship that views with some healthy skepticism claims about the practical implications and real-world applications of recent developments in neuroscience. The need to probe and question is, I contend, especially acute in the context of national security neuroscience—where the translation from research laboratory to real life may involve great leaps, among them the troubling jump from brain scanning to terrorist screening.

The approach I adopt here is consonant with and sympathetic to the goals of "critical neuroscience"—a multidisciplinary project recently defined as "a reflexive scientific practice that responds to the social and cultural challenges posed both to the field of science and to society in general by recent advances in the behavioural and brain sciences" (Choudhury et al. 2009). The proponents of critical neuroscience aim to bridge the gap between science studies and empirical neuroscience by engaging scholars and practitioners from the social sciences, humanities, and empirical neuroscience to explore neglected issues: among them, the economic and political drivers of neuroscience research, the limitations of the methodological approaches employed in neuroscience, and the manner in which findings are disseminated. The project's avowed and worthy goals include "maintaining good neuroscience, improving representations of neuroscience, and . . . creating an awareness of its social and historical context in order to assess its implications" (Choudhury et al. 2009, 66).

Second, I acknowledge the legitimate aims and objectives of the national security enterprise and of the officials solemnly charged with its pursuit. However, sometimes national security threats may be overstated or invoked for political ends, and the means employed in the pursuit of

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these objectives are often fundamentally violative of the human rights of others (for a more detailed explication, see Marks 2006). In addition, as I outline later, there are many examples from the Bush Administration's war on terror of medicine, other health sciences (including behavioral psychology), and polygraphy being abused in the name of national security. So, while there are risks that the national security community may be misled about what neuroscience can offer, I am also concerned about the ways in which national security may pervert neuroscience.

NEUROSCIENCE NARRATIVES AND SECURITY SEMANTICS

Neuroscience and national security both jealously guard their own argot. In the case of neuroscience, the lexicon is replete with Latin and Greek and innumerable portmanteau constructions that fuse (or confuse) both classical languages. Consider, for example, the subthalamic nucleus, a neuroanatomical term that sandwiches a Greek derivative between two Latin ones, and is (infelicitously) susceptible to the translation "the nut under the bedroom." For some cognoscenti, there may be a familiar poetry in the classical language of the brain's anatomy—the *sulci* and *gyri* becoming, perhaps, the neuro-topographical analogs of William Wordsworth's "vales and hills." But those who do not possess either training in neuroscience or an anatomical dictionary are lost.

National security too has its special (albeit less colorful) language, consisting—for the greater part—of cryptic and somewhat intimidating initials, acronyms, and not-quite-acronyms, such as HUMINT (human intelligence) and BSCT (standing for behavioral science consultation team, and pronounced "biscuit.") For readers unfamiliar with both these terms, the former is commonly defined as a category of intelligence derived from information collected and provided by human sources. This includes interrogations (as well as other forms of overt or clandestine conversations with sources that may be considered "neutral," "friendly," or "hostile"). BSCTs are teams of psychologists and/or psychiatrists, and their assistants (often mental health technicians), tasked by the Department of Defense with advising interrogators how to ramp up interrogation stressors at Guantanamo Bay, Abu Ghraib, and elsewhere (see, e.g., Mayer 2008).

My intention here is not to take an easy shot at the neuroscience and national security communities, and the linguistic practices in each of these domains. Rather, I wish to express concern about the naming of things in these contexts and, in particular, about the hazards—practical and ethical—that arise from the deployment of opaque terminology. It is easy for outsiders to the world of neuroscience to believe that, because there is a polysyllabic name for some part of our brain, we have a deep understanding of what it does and how it does it. This is, of course, not necessarily the case. To give just one example, physicians can sometimes achieve dramatic improvements in the motor symptoms of some Parkinson's patients by using small electrical pulses to stimulate the subthalamic nucleus (the "nut" mentioned earlier). However, the mechanism by which this effect is achieved is still being explored. In addition, as one neurosurgeon colleague recently made clear to me, our stimulators are not "smart." They do not monitor what is occurring in the subthalamic nucleus and respond to it. Nor do they monitor the response of the nucleus to their stimuli.

Some might argue that, in this clinical example, the intervention works, and while we should seek to refine the technique and our understanding of the efficacy of the intervention, the limited nature of our current understanding should not bar its use. I do not intend to address that claim here. However, the nonclinical application of neuroscience in the murkier national security context creates serious potential hazards, and these risks are amplified in the absence of solid theoretical models and robust empirical data. Not least, there is a real danger that pseudo-neuroscience will become a vehicle for the abuse of those who are perceived as a threat to national security.

Although I will substantiate this point shortly, allow me briefly to explore the foundations of neuroscience's linguistic hazards. When the language of neuroscience is used to construct explanatory narratives, the results can be unduly persuasive due to a phenomenon the philosopher J. D. Trout has termed "explanatory neurophilia" (Trout 2008). A recent study indicates that nonexperts—including college students taking a cognitive neuroscience class—are not very good at critiquing neuroscience narratives. Deborah Weisberg and colleagues explored the hypothesis that even irrelevant neuroscience information in an explanation of a psychological phenomenon may interfere with a person's ability to consider critically the underlying logic of that explanation (Weisberg et al. 2008). Weisberg found that nonexperts judged explanations with logically irrelevant neuroscience information to be more satisfying, particularly in the case of bad explanations. The authors try to explain the results in a number of ways. They suggest that the *seductive details effect* may be in play. According to this theory, seductive details that are related—but logically irrelevant—make it more difficult for subjects to code and later recall the main argument of a text. They also hypothesize that lower level explanations, in particular, may make bad explanations seem connected to a larger explanatory system and therefore more insightful.

Contemplating the implications of the Weisberg study, J. D. Trout has argued that placebic neuroscientific information may "promote the feeling of intellectual fluency" and that "all too often humans interpret the positive hedonic experience of fluency as a mark of genuine understanding" (Trout 2008). Trout suggests that "neurophilic fluency flourishes wherever heuristics in psychology are reductionist," that is, where they focus on a small number of local factors with apparent causal significance in order to explain a complex problem. Although more work may be required to provide a full account of explanatory neurophilia—that is, our blind (or at the very least blinkered) love for neuroscientific explanations—there is little doubt that the phenomenon has been persuasively demonstrated.

In the national security domain, there is also a temptation to believe that a claim is true because it carries the label HUMINT, or is similarly packaged in the specialist language of national security. Many senior administration officials appear to have believed (or wanted to believe) that "EITs"—so-called "enhanced interrogation techniques"—were, as the name suggested, "enhanced." But, as experienced interrogators have repeatedly asserted, the products of aggressive interrogation tactics such as waterboarding, exposure to temperature extremes, stress positions, and the like tend not to be reliable, whatever one calls them (see, for example, Bennett 2008; Soufan 2009). This is because interrogatees under pressure tend to say whatever it is they believe their captors want to hear, or anything just to stop their abuse. Numerous detainees in the Bush Administration's "war on terror" retracted claims they had made during torturous interrogations, once they were removed from their high-pressure interrogation environments—most notably, Khalid Sheikh Mohammed, who was waterboarded 183 times in March 2003 (CIA Inspector General 2004), and later told the Red Cross:

> During the harshest period of my interrogation I gave a lot of false information in order to satisfy what I believed the interrogators wished to hear in order to make the ill-treatment stop. . . . I'm sure that the false information I was forced to invent in order to make the ill-treatment stop wasted a lot of their time. (ICRC 2007)

Psychologists James Mitchell and Bruce Jessen were the principal architects of the post-9/11 interrogation regime to which Khalid Sheik Mohammed and others were exposed (Mayer 2008; SASC 2008). But as Scott Shane observed in the *New York Times*:

> [Mitchell and Jessen] had never carried out a real interrogation, only mock sessions in the military training they had overseen. They had no relevant scholarship; their Ph.D. dissertations were on high blood pressure and family therapy. They had no language skills and no expertise on Al Qaeda.
>
> But they had psychology credentials and an intimate knowledge of a brutal treatment regimen used decades ago by Chinese Communists. For an administration eager to get tough on those who had killed 3,000 Americans, that was enough. (Shane 2009)

Mitchell and Jessen drew on their experience of the SERE training program—a program designed to inoculate U.S. service personnel against abusive treatment at the hands of enemy captors by exposing them to the kinds of treatment that they had historically received, for example, during the Korean War. Mitchell and Jessen reverse-engineered those techniques and used them as the basis for a new aggressive interrogation regime in the "war on terror" (Mayer 2008). But the reverse engineering of SERE tactics not only violated fundamental human rights norms, and the baseline protections for detainees found in Common Article III of the Geneva Conventions (Marks 2007a); it was also premised on a fundamental strategic error. As several experienced interrogators have repeatedly made clear (see, for example, Bennett 2008; Soufan 2009) and as some psychologists tried to warn the Bush Administration (Fink 2009), these techniques are not reliable methods for the extraction of intelligence. On the contrary, as the North Koreans demonstrated in the 1950s (Margulies, 2006) and the British government discovered in the wake of several questionable convictions for terrorism in the 1970s (Gudjonsson 2003), these techniques tend to be excellent methods for extracting sham confessions and getting detainees to say whatever they believe their captors want to hear. While this was the intended effect in the former case, in the latter it was not. As a result, several Irish Republican Army (IRA) suspects were falsely convicted, while those responsible for the mainland terror attacks in Britain continued to roam free. However, this vital element was lost in the translation of stress tactics from the SERE training program to the U.S. detention and interrogation operations.

In my view, this account demonstrates the perils arising from a lack of critical engagement with purported scientific expertise, in this case behavioral psychology, in a national security context. These perils arise, in part, from the seductive nature of national security terminology—such as "enhanced interrogation techniques"–that exaggerates or misrepresents the scientific foundations of a particular practice. Two related examples from the interrogation context are "truth serum" and its even more troublesome cousin, "lie detector." These terms reflect a profound lack of precision and tend to reinforce the operation of mental heuristics that deprive us of the opportunity to think critically.

The U.S. military and intelligence communities have long had a fascination for psychoactive drugs as interrogation aids (see, for example, ISB 2006, 73–74). The term "truth serum" is loosely used to describe a variety of psychoactive drugs including scopolamine, sodium pentothal, and sodium amytal. The colloquialism seems to promise the Holy Grail—detainees in a drug-induced state of compliance, unable to resist imparting explosive nuggets of actionable intelligence. However, there is, to date, no drug that can live up to this title. These drugs may make some people more talkative, but there is no guarantee that what they say is either accurate or useful. It appears that this did not prevent the reported use of psychoactive drugs as interrogation aids in the "war on terror." Several detainees have claimed that they were drugged prior to interrogation (Warrick 2008). The CIA and Defense Department dispute these claims. However, the Bush Administration commissioned and received legal opinions that took a permissive approach to the use of drugs in interrogation.[1] The CIA has acknowledged that detainee Abu Zubaydah was

1. See, for example, Jay Bybee's Memorandum for Alberto Gonzales, dated August 1, 2002, available at http://www.washingtonpost.com/wp-srv/politics/documents/cheney/torture_memo_aug2002.pdf (last accessed January 4, 2010) and John Yoo's Memorandum for William Haynes of March 14, 2003 at http://www.aclu.org/files/pdfs/safefree/yoo_army_torture_memo.pdf (last accessed January 4, 2010).

waterboarded 83 times in August 2002 (CIA Inspector General 2004). It has also been reported that Zubaydah was drugged with sodium pentothal (see, for example, Follmann 2003). If this allegation is true, Abu Zubaydah's abusive interrogation may speak as much to the efficacy of so-called "truth serums" as to the utility of waterboarding.

Not surprisingly, the search for the Holy Grail continues, and neuroscience cannot resist stepping up to the plate. There has been much discussion in both academic journals and the media recently about oxytocin. This hormone is released in the bodies of pregnant women during labor (see, for example, Lee et al. 2009) and there is some evidence to suggest that "intranasal administration of oxytocin causes a substantial increase in trusting behaviour" (Kosfeld et al. 2005; see also Baumgartner et al. 2008). As a recent report of the National Research Council acknowledged, the drug is of particular interest to the defense and national security communities, not simply because of its implications for soldier performance, but also because it might allow for "new insights into adversary response" (NRC 2009). Although the report does not expressly discuss this, one potential use is its administration as an aid to interrogation—perhaps covertly prior to interrogation in aerosolized form. This may sound fanciful. However, aerosolized oxytocin is already being marketed by one corporation, Verolabs, as "Liquid Trust," promising to deliver "the world at your fingertips, whether you are single, in sales, an unhappy employee who wants to get ahead"[2]—or perhaps all three of these. So we should expect interrogators to be tempted to use it if they have not already done so.

If there were such a thing as a "truth serum," of course, there would be little need to direct intelligence efforts toward the detection of lies. But that too is an enterprise with a long and colorful history—discussed in more detail than is possible here in a 2003 report of the National Research Council (NRC 2003). For the greater part of the last century, "lie detector" was the monicker associated with the polygraph, although the device does nothing of the kind suggested by the term. The polygraph does not detect lies; on the contrary, it only measures physiological changes that tend to be associated with anxiety. This is problematic because for many polygraph subjects the experience of being tested itself is sufficient to cause considerable anxiety. As a result, the NRC concluded, the polygraph is "intrinsically susceptible to producing erroneous results"—in particular, false positives (NRC 2003, 2). In addition, "countermeasures" are possible: People can be trained to beat the polygraph by reducing external manifestations of anxiety, creating false negatives. In spite of these limitations, the "lie detector" label has stuck, reinforced by countless television series and movies—and figurative labels, like their literal counterparts, are often hard to peel away.

There is strong evidence that polygraphy was abused in the "war on terror" and, in my view, this misuse is attributable to misunderstandings of the technology reinforced by the "lie detector" monicker. Documents obtained by M.Gregg Bloche and me pursuant to Freedom of Information Act (FOIA) requests reveal that between August 2004 and October 2006, the U.S. Air Force Polygraph Program conducted 768 polygraphs in Iraq.[3] According to an internal summary, 47% of the polygraph tests indicated no deception, while 46% purported to indicate some form of deception. This was interpreted by the drafter of the summary in the following way: "Detainee personnel are just as likely to have committed the suspected act as not." (Of course, this might equally have been interpreted to mean: "Detainee personnel are just as likely *not* to have committed the suspected act.") But further reading suggests that the attribution of guilt—defined as "involvement in multiple acts of anti-coalition force activities"—on the basis of these results requires an unjustifiable leap of faith. The summary itself offers one important reason why the report's conclusion is unwarranted. It states (without acknowledging the implications of this) that "only 10% of requests for polygraph support contain sufficiently detailed information for specific issue exams." The remainder of the polygraph tests were (according to the summary) "by definition screening examinations wherein the examiner is called to resolve numerous and divergent issues based on extremely generic, anonymous and perishable reporting."

In polygraphers' feedback forms accompanying this summary, many respondents complained that the polygraph technology was either "over utilized or not utilized properly." Two described the use of a failed polygraph test as "a hammer to be used against the detainee." One said this never resulted in anything positive, while another said that, having participated in 240 polygraph examinations in Iraq, on only one occasion did he witness this approach produce "anything of value." Despite this, detainees were "regularly" hammered with polygraph results—even when deception was not indicated (that is, even when they had "passed" the polygraph test). In such cases, the only clear evidence of dishonesty was on the part of the interrogators.

Not surprisingly, many polygraphers complained about the way their services had been deployed. One noted that interrogators "did not fully understand how to use our services despite multiple briefings and pretest coordination discussions." The polygraph was often used as a "crutch" to avoid unnecessary interrogations, one polygrapher claimed. Another complained about the use of what s/he considered to be worthless questions, and estimated that in 70% of cases interrogators asked: "Have you ever been involved in attacking coalition forces?" Others described larger issues that the military failed to address. Most notably, one concluded:

> I encountered nothing but difficulties with the exams and have no reason to have any confidence the results were valid. I attribute these problems to a host of reasons: bad environment,

2. See http://www.verolabs.com/news.asp (last visited September 20, 2009).

3. Documents on file with author.

problems with interpreters [who were used in most interrogations], and cultural differences.

Even in its traditional use in the United States, it is clear that the polygraph does not merit the moniker, "lie detector." (The National Research Council has noted that while the technology performs better than chance, it is far from perfect [NRC 2003].) But in the national security context, where most interrogations are mediated through an interpreter, and cultural issues are often ignored, the label is surely even more problematic.

The language of lie detection is more worrisome still when it is deployed to describe the use of brain imaging and related technologies that do not patently rely on external manifestations of anxiety. Newer technologies purport to show us what is going on in someone else's brain and—if one were to believe much of the press coverage—in their mind. As the British experimental psychologist Dr. Richard Henson has succinctly observed, "There is a real danger that pictures of blobs on brains seduce one into thinking that we can now directly observe psychological processes" (Henson 2005, 228). It is to the seductive power of brain images that I now turn.

NEUROIMAGING AND NEUROSCIENTIFIC IMAGINERIES

Brain images are ubiquitous. They are no longer the sole province of medical and scientific journals. Viewers of cable news and print media alike are frequently shown brain images. They usually accompany features breathlessly reporting that brain imaging has heralded the end of lies or finally lifted the shroud to reveal "how the brain handles love and pain."[4] But functional neuroimages are not images insofar as that word is used to connote optical counterparts of an object produced by an optical device.[5] Brain activity does not have an optical component. We cannot literally see people think—although these images may suggest as much to those with little or no understanding of how they are produced. Rather, neuroimages are carefully constructed representations of the brain and its functions. When the results of fMRI-based cognitive neuroimaging studies are presented to us in image form (as is almost invariably the case), tiny changes in blood oxygenation levels (less than 3%) are represented by bright colors (usually reds, yellows, and blues). These changes are the product of a comparison between levels of blood oxygenation for a chosen cognitive task and those for an activity considered a suitable baseline. These changes are interpreted as markers of local activation or inhibition in the regions of the brain in which they occur.

Many science studies scholars and bioethicists have critiqued the manner in which brain images are produced, constructed, and interpreted (see, for example, Dumit 2003; Joyce 2008; Wolpe et al. 2005; Marks 2007b). I do not review all these critiques here. Instead, I wish to highlight a simple methodological point that is often not appreciated. A functional MRI image—the kind so frequently reproduced in glossy magazines for lay readers—is usually not a single image of one person's brain. There are two reasons for this. First, the changes represented in brain images are often not those of a single experimental subject. More commonly, they are representations of composite data from a small experimental cohort. Second, these color images are superimposed on a higher resolution structural brain image, just as Doppler weather radar images are superimposed on geographic maps. The higher resolution image is intended to reveal the topography of the brain—just as the geographic map (on to which the constructed Doppler image is superimposed) is intended to show that—for example—the latest hurricane is 50 miles off the coast of Georgia. However, the structural image of the brain need not be taken (and is often not taken) from any of the subjects of the experiment. This is important because the neurological analog of the state of Georgia in my brain is (like the lyrical Georgia on my mind) not necessarily the same as yours. Put another way, there is considerable variation in the anatomical structure of the human brain—variations that occur even as between identical twins, and the right and left hemispheres of the same brain (Weiss and Aldridge 2003). So the colored area of activation or inhibition may not correlate precisely with the area represented in the structural image.

This reinforces two points recently made by the neuroscientist and philosopher Adina Roskies. First, as Dr. Roskies notes, "the conventions of the brain image are representational translations of certain nonvisual properties related to neural activity in the brain"—that is, the comparative magnetic properties of oxygenated and deoxygenated hemoglobin; and second, "the choices that are made [in the construction of a brain image] are not visible in or recoverable from the image itself" (Roskies 2008). In my view, functional brain images might properly be considered the product of "neuroscientific imaginaries" comprising the values, beliefs, and practices of the neuroimaging community.[6] This is not to say that functional neuroimages are complete fabrications, entirely divorced from reality (in this case, brain functions). Rather, brain images are the product of decisions about scanning parameters and the criteria for statistical significance, in conjunction with acts of interpretation and representation that may tell us as much about the imagers as they do about those who are imaged (although we must look beyond the images to learn much about either of them).

This analysis is important because it highlights the chasm between the manner in which brain images are constructed and the way lay viewers in particular comprehend them. Many of us appear to assume that visually arresting brain images share the evidential characteristics and epistemic status of photographs (see Roskies 2008). Empirical support for this view has been provided by David McCabe and Alan Castel, who have shown that readers attribute

4. See, for example, http://www.msnbc.msn.com/id/4313263/ns/technology_and_science-science/ (last visited September 20, 2009).

5. I draw here from the definition of "image" at www.m-w.com.

6. The term is my own, but it draws some inspiration from the notion of "technoscientific imaginaries." See Marcus (2005).

greater scientific value to articles summarizing cognitive neuroscience research when those articles include brain images than they do when the articles include no image, a bar graph, or a topographical map of the brain. They found that this effect (albeit not large) was demonstrable regardless of whether the article included errors in reasoning (McCabe and Castel 2008). One explanation for this may be, as Roskies contends, that neuroimages are "inferentially distant from brain activity, yet they appear not to be." Put another way, she argues, brain images are "seemingly revelatory." In my view, this latter claim needs to be probed a little further. To whom is what revealed, and by what means? The answer to this question (one I next endeavor to provide) is vital to a nuanced understanding of some of the potential hazards of brain imaging in the arsenal of national security neuroscience.

In spite of their ubiquity, brain images are essentially meaningless to the uninitiated. When we look at these images in isolation, there is no "aha!" moment, no epiphany. The images require the explanation of experts. However, at the same time, the images also tend to reinforce the expert narrative. It is hard not to be impressed by the expert (real or apparent) who can guide us through the images—who can bring us to the point of revelation. And if the expert is compelling enough, it can be hard to look at the image again without relying heavily on the expert's interpretive framework. In this way, brain images and neuroscientific narratives rely on each other to work their persuasive and pervasive magic. This point may be illustrated by a notable recent analog regarding the use of a different kind of image in a national security context (the run-up to the invasion of Iraq in 2003). I have chosen this example not simply to illustrate how images and narratives work together, but also to demonstrate the potential hazards when science is purportedly deployed in a national security context.

In February 2003, then U.S. Secretary of State Colin Powell made a presentation to the United Nations Security Council that was intended to substantiate the United States' argument that Iraq presented an imminent threat to regional and global security.[7] Most people recall Powell's presentation—which he subsequently described as a permanent "blot" on his record (Weisman 2005)—because of a theatrical flourish: He held up a small vial of white powder while describing the threat that a similar quantity of weaponized anthrax might present. Fewer may now recall some of the other visual elements of his presentation—in particular, his reliance on IMINT (or image intelligence—the term used in "national securitese" to denote aerial and satellite photography). Powell showed time-sequenced satellite images of buildings whose functions were described in yellow text boxes—among them a chemical munitions bunker.[8] He also showed computer-generated images of trucks and railway carriages that were described as "mobile production facilities for biological agents." Before displaying the images, Powell warned that we could not understand them, but that imaging experts had shed light where, otherwise, there would only be darkness:

> The photos that I am about to show you are sometimes hard for the average person to interpret, hard for me. The painstaking work of photo analysis takes experts with years and years of experience, poring for hours and hours over light tables. But as I show you these images, I will try to capture and explain what they mean, what they indicate, to our imagery specialists.

As the filmmaker Errol Morris subsequently noted in a *New York Times* blog:

> I don't know what these buildings were *really* used for. I don't know whether they were used for chemical weapons at one time, and then transformed into something relatively innocuous, in order to hide the reality of what was going on from weapons inspectors. But I *do* know that the yellow captions influence how we *see* the pictures. "Chemical Munitions Bunker" is different from "Empty Warehouse" which is different from "International House of Pancakes." The image remains the same but we *see* it differently.[9]

The interpretations of these photographs offered so convincingly by Powell have, of course, failed to stand up to scrutiny. Embarrassingly for the Bush Administration, the Iraq Survey Group failed to find any evidence of an active program for the development of weapons of mass destruction.[10] But, by that time, the images had done their work. According to Morris, the captions did the "heavy lifting," while the "pictures merely provide[d] the window dressing." But this account does not give full credit to the powerful way in which the images and the narrative work together, each reinforcing the other. The image, requiring interpretive expertise, validates the expert interpreter. And the act of interpretation gives meaning to the image that is otherwise incomprehensible to the lay viewer. In this deceptively attractive circularity (that I call the "image-expert bootstrap"), the image tells us how important the expert is, while the expert tells us how important the image is. Since nonexperts are hardly well placed to provide an alternative interpretation of the image, or to challenge the interpreter's expertise (or his expert interpretation), it is extremely hard for them to break the circle.

7. The full text of Colin Powell's presentation to the United Nations is available at http://www.cnn.com/2003/US/02/05/sprj.irq.powell.transcript/ (last accessed September 20, 2009).

8. The images are available at http://www.state.gov/secretary/former/powell/remarks/2003/17300.htm (last accessed September 20, 2009).

9. See http://morris.blogs.nytimes.com/2008/08/11/photography-as-a-weapon/?ref=opinion (last accessed September 20, 2009).

10. See the Report of the Iraq Survey Group (also known as the Duelpher Report), available at https://www.cia.gov/library/reports/general-reports-1/iraq_wmd_2004/index.html (last accessed January 10, 2010).

NATIONAL SECURITY NEUROSCIENCE: PERILOUS TO THE VULNERABLE?

A National Research Council report on "Emerging Cognitive Neuroscience and Related Technologies" issued in August 2008 cautioned that when using neurophysiology data to determine psychological states, "it is important to recognize that acceptable levels of error depend on the differential consequences of a false positive or a misidentification" (NRC 2008, 19). The authors expressed particular concern that "the neural correlates of deception could be construed as inconvertible evidence of deception and therefore (mistakenly) used as the sole evidence for making critical legal decisions with lasting consequences," and noted that "insufficient high-quality research using an appropriate research model and controls has been conducted on new modalities of credibility assessment to make a firm, data-driven decision on their accuracy" (34).

These concerns were exacerbated the following month when reports emerged that a woman named Aditi Sharma had been convicted in India of killing her former fiancé with arsenic, and that her conviction relied principally on "evidence" from brain electrical oscillation signature testing (EEOS)—purportedly a variation on the electroencephalograph (EEG)-based technique of so-called "brain fingerprinting" developed and aggressively marketed by Lawrence Farwell (Giridharadas 2008). Many commentators have been understandably appalled that a judge would permit such a travesty of justice, noting hopefully that such an outcome should not be possible in the United States. Judicial gatekeeping may well prevent such an incident from recurring in Europe or North America. A more immediate concern in the United States, however, is the use of neurotechnologies in the national security context, where there is no judicial gatekeeper.

In particular, an fMRI "test result" could be used to label a detainee as a terrorist—a troubling prospect described more fully elsewhere (see Marks 2007b). One can imagine without great difficulty that an intelligence operative—seduced by colorful brain scans, pseudo-scientific explanatory narrative, and media hype—might say "the fMRI picked him out as a liar"—or, worse still, "as a terrorist." It is not difficult to see how that could influence the subsequent treatment and interrogation of such a detainee. Labels such as the "worst of the worst" (used to described to detainees at Guantanamo Bay), and—a fortiori—specific labels such as the "mastermind of 9/11" and the "20th hijacker" (ascribed to detainees Khalid Sheikh Mohammed and Mohamed Al Qahtani, respectively) led to abusive and, in the most extreme cases, life-threatening treatments. But a label invoking a much-hyped and little-understood technology, such as fMRI, is likely to be all the more powerful. These concerns are highlighted by two factors.

The first is the aggressive marketing of these technologies, which may further exacerbate the undue confidence in and uncritical assessment of these technologies fueled by the specialist language and images I have already described. We have seen this in the case of so-called fMRI-based "lie detection"—the worst offender in the field being No Lie MRI, a corporation that claims accuracy rates of 90% or more and asserts (without a solid evidential basis) that its proprietary technology is "insensitive to countermeasures."[11] A similar point can be made about aerosolized oxytocin too, and the colorful marketing strategies described earlier. The second factor is the interest of the national security community in neuroscience. Although it has been difficult to corroborate the claim that fMRI has already been used—in conjunction with EEG—to screen suspected terrorists (see Marks 2007b), there is clearly interest in its use for this purpose (see ISB 2006). In January 2007, the Department of Defense's Polygraph Institute was renamed the Defense Academy for Credibility Assessment. There appear to have been two rationales for the new title—first, an expansion of the portfolio of the institute to encompass the use of newer technologies including but not limited to fMRI and, second, a shifting of the institute's mandate to address counterterrorism.

The National Research Council's recent report "Opportunities in Neuroscience for Future Army Applications" acknowledged that there were challenges to the detection of deception due to individual variability and "cultural differences in attitudes to deception" (NRC 2009, 96). But the report continues to pose the question "is there some kind of monitoring that could detect if a subject being interrogated is responding in a 'contrary to truth' manner?" (NRC 2009, 96). This should serve as a potent reminder. LSD and, increasingly, the polygraph may seem consigned to the annals of U.S. national security. But no one should doubt that the concerns that have motivated their use are alive and well in the neuroscience and national security communities.

THE WAY FORWARD

In the limited space available, I cannot attempt a comprehensive ethical critique of national security neuroscience—a project to which Canli and colleagues (2007) and their commentators make important contributions. Nor can I address all the potential perils of national security neuroscience. I have focused on the population I consider most vulnerable, detainees, and I have discussed two core examples in relation to that population. But there are clearly other vulnerable populations, most notably, soldiers (or warfighters, as they are now called), whose freedom is constrained not simply by their enlisted status, but also by their natural desire for recognition and advancement in the military. A fuller discussion of the potential hazards in relation to that population can be found elsewhere, in particular in Jonathan Moreno's book *Mind Wars* (Moreno 2007).

In his book, Moreno calls for the creation of a national advisory committee on neurosecurity, staffed by professionals who possess the relevant scientific, ethical, and legal expertise. The committee would be analogous to the National Science Advisory Board for Biosecurity established

11. See http://noliemri.com/products/Overview.htm (last accessed September 20, 2009).

in 2004, which is administered by the National Institutes of Health (NIH) but advises all cabinet departments on how to minimize the misuse of biological research. Such a committee could certainly play a role in the oversight of national security neuroscience if it had the authority to monitor the misuse of neuroscience within—as well as outside—government. But in my view, the time has also come for a broader public debate about the legitimate nonclinical applications of neuroscience—one that takes into account the concerns addressed here, and seeks to learn from the abuse of medicine, behavioral psychology and polygraphy in the national security context.

If we are to have a meaningful discussion, we will have to ask ourselves some difficult questions that explore the kinds of neuroscience research that are being funded and address the broader context in which that research takes place. For example, many of us do not blink at the use of brain imaging to detect lies in detainees. In contrast, no one advocates the use of the technology during U.S. Senate hearings for nominees to the federal judiciary (although it did briefly occur to me to write an op-ed during the confirmation hearings of Justice John Roberts making this suggestion, tongue firmly in cheek). Surely, the detection of an answer "contrary to truth" in such a context might be of some interest to the senators charged with the confirmation of federal judges.

I am, of course, not the first to make this kind of point. Commenting on fMRI-based research to screen children for potentially violent behavior, the sociologist Troy Duster has noted that studies are "not designed to capture the kind of diffuse, anonymous violence reflected in the behavior of unscrupulous executives, traders, subprime lenders and so on" (Duster 2008). It is tempting to add to the list the arm's-length architects of torturous interrogation, and the legal and health professionals who—purportedly exercising their professional skill and judgment—approved their use.

Duster continues with more than a hint of sarcasm:

> But for the sake of argument, suppose we could monitor children and determine that greater activity in the prefrontal cortex means that they are likely to exhibit violent behavior. Surely, then, we should scan preteens to intervene in the lives of potential Enron-style sociopaths before they gut the pensions of the elderly, right? Oops, I guess I have the wrong target group in mind. (B4)

In this piece, Duster applies to recent neuroscience research some of the criticism that social activist Martin Nicolaus directed at sociologists and criminologists in 1968, roughly paraphrased as "you people have your eyes down and your hands up, while you should have your eyes up and your hands down." He explains:

> "Eyes down" meant that almost all the research on deviance and crime was focused on the poor and their behavior, while "hands up" meant that the support for such research was coming from the rich and powerful—from foundations, the government, and corporations. Conversely, of course, "eyes up" meant turning one's research focus to the study of the pathological behavior of the elite and privileged, and "hands down" meant giving more of a helping hand to the excluded, impoverished, and disenfranchised. (B4–B5)

Although Duster does not address how neuroscience might help the disenfranchised, it is not difficult to conjure other uses of MRI technologies that might redound to their benefit (including perhaps the provision of government-funded diagnostic services to the uninsured and the underinsured or a broader use of the technology to rescue and rehabilitate patients trapped in minimally conscious states). But how we use these technologies, "the wicked problems"—to use the language of Frank Fischer (Fischer 2003, 128–129)—that we call on them to address, should be a matter of informed debate in which experts engage with and listen closely to the public. The neuroscience and neuroethics communities must be frank with the public about the potential, the limitations, and the perils of neuroscience. We should empower the public to challenge decisions regarding the development and application of neuroscience (see Dickson 2000), and engage with them in figuring out the road ahead. The educational and communication challenges in this exercise should not be underestimated. But we should rise to them. If we fail to reconsider and refocus the gaze of neuroscience, we risk abandoning or—worse still—imperiling the vulnerable. And if we do that, tomorrow's historians and science studies scholars will, rightly, not look kindly on us. ■

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Neuroconcerns: Some Responses to My Critics

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Neuroscience has its limits. Perhaps oxytocin can diminish neuroskepticism. But no psychoactive drug or neurotechnological innovation can enable me to engage meaningfully in around a thousand words with more than a dozen commentaries—embracing the work of Foucault (Thomsen 2010) and Spinoza (Bentwich 2010), in addition to the latest scholarship in neuroscience and neuroethics, some of which appeared after my article was submitted for publication (e.g., Illes et al. 2010). Although my responses are shaped by these thoughtful commentaries, the latter raise many important points to which I cannot do justice here.

NEUROSKEPTICISM

Let me begin by saying something about the neuroskepticism that informed my piece (Marks 2010). It is certainly not an ideology, as one of my harshest critics alleges (Keane 2010). Nor is it meant to be construed as some kind of ethical theory. I am even hesitant to elevate it to the status of "method" as the members of the Stanford Interdisciplinary Group in Neuroscience and Law (SIGNAL) suggest in their insightful commentary (Lowenberg et al. 2010). As I said in the article, neuroskepticism is a "perspective." One might also call it a sensibility or an orientation. Since I did not expect all my readers to share the perspective, I wanted to declare it up front. I am glad to acknowledge, as the SIGNAL authors suggest, that I am also "neuroconcerned: concerned about whether even scientifically reliable neuroscience could be used to cause harm or to further harmful ends" (Lowenberg at al. 2010). This acknowledgment should help address the views of some commentators who thought I placed too great an emphasis on bad science as the source of concern (Thomsen 2010; Strous 2010). There are clearly serious ethical issues, whether the science is sound or, as Fisher (2010) puts it, "absolute junk."

ETHICS AND SCIENCE

The purpose of my article was not to provide a comprehensive ethical assessment of neuroscience and national security—an endeavor to which Canli and colleagues (2007) and their commentators make significant contributions. Rather, it was to offer an explanatory account that might inform neuroethical debate. Such a debate must obviously address the legitimacy of the means by which and the ends for which neuroscience is applied (Lowenberg et al. 2010). However, I do not (as Benanti [2010] suggests) consider that "ethical questions remain, in a certain way, external to neuroscience, because they are related only to practical use of neuroscience." Ethics must infuse science from inception—from the first flicker of an idea—through testing and development, to myriad actual and potential applications. As Schienke and colleagues (2009) argue, a comprehensive account of scientific research ethics needs to address three distinct but related spheres of concern: "procedural ethics" (that is, the responsible conduct of research, which includes issues of falsification, fabrication, plagiarism, care of human and animal subjects, and conflicts of interest), "intrinsic ethics" (comprising issues internal to or embedded in the production of a given inquiry or mode of analysis), and "extrinsic ethics" (that is, issues arising from the application of science to policy or from the impact of science and technology on society).

I agree that the slippage from military into civilian applications of neuro-technologies presents ethical issues (see Marchant and Gulley 2010). But, of course, applications that remain within the national security domain still raise important ethical concerns. For example, as Kirmayer, Choudhury, and Gold (2010) note, there is a risk that neuroscience can become "a screen on which to project our prejudices and stereotypes" and that it may shift the focus away from the origins of terrorism as "social and political processes." This has implications for national security too. As John Horgan has argued, recognition of these social processes is vital to our understanding of terrorism and to the crafting of effective counterterrorism policies (e.g., Horgan 2008).

ETHICS AND NEUROHYPE

My paper did not focus on the problem of "neurohype" and the related ethical responsibilities of the scientific community. However, Rippon and Senior "take umbrage with the impression [my article] gives that the neuroscientific community is not 'policing its own house"' (Rippon and Senior 2010)—a phrase my article did not employ. At the same time,

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Rippon and Senior acknowledge that "neuroscientists need to be more aware of the potential dangers of 'overselling our wares"' and that "the brain imaging community needs to be more vigorous in communicating its concerns to the public at large and, more importantly to the policy makers and funders." I am grateful for these acknowledgments and would commend to Rippon and Senior the discussion of "neurohype" in Caulfield, Rachul, and Zarzecny (2010; and the works cited therein). I would also endorse Nagel's "plea for responsibility in science"—a plea that recognizes that some scientists live up to such responsibilities better than others and, more importantly, that there are systemic factors that can promote problematic behaviors (Nagel 2010).

EMOTION, COGNITIVE BIASES, AND COUNTERTERRORISM

Bentwich (2010), drawing on the work of Spinoza, argues I pay too little attention to the "pernicious linkage among fear, superstition, and prejudice" in the national security context. Although I did not explore these issues in my target article, I have done so at substantial length elsewhere. In Marks (2006), I argue that our emotional responses to counterterrorism and associated cognitive biases have an impact not just on individual behavior but also (through a variety of social mechanisms) on counterterrorism policy and practice. Resulting policies tend to violate human rights, and often fail to address real threats: see Marks (2006), where my debt to Spinoza is mediated by the work of neuroscientist Antonio Damasio, on which I draw (Damasio 2003).

ETHICS AND HUMAN RIGHTS

Bentwich (2010) concludes that the answers to these problems are "not found in the jurisdiction of neuroethics, but rather by reasserting human rights and constitutional civil liberties." In other work on neuroscience and national security, I too have emphasized the importance of human rights (see, e.g., Marks 2007b). More broadly, I have also advocated human rights impact assessments of counterterrorism policies (Marks 2006) and used international human rights law to critique health professionals who are complicit in detainee abuses (Marks 2007a). However, I do not believe that professional ethics is an entirely autonomous enterprise. In forthcoming work, I intend to elaborate more fully on the relationship between human rights and professional ethics. For now, I note that the American Association for the Advancement of Science (AAAS) Science and Human Rights Program recognizes the centrality of human rights to the ethics of science, and its new coalition will explore how greater attention to human rights might also result in improvements in scientific process and practice (AAAS 2010).

THE PROSPECTS FOR AND LIMITS OF FURTHER DISCOURSE

I am heartened by the lively debate that my article has provoked in the pages of the *American Journal of Bioethics: Neuroscience*. Consistent with the objectives of my piece, I encourage my colleagues to work with me to take this discussion beyond the pages of this journal and into the larger public domain. I acknowledge, as Lowenberg and colleagues (2010) and Giordano (2010) point out, that national security considerations may place limits on what may be discussed in public. But the presumption should be in favor of openness, and there is—in any event—more than enough information in the public domain about which to conduct meaningful discussions. ■

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