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# The Appeal of the Brain in the Popular Press

**Diane M. Beck**

Department of Psychology and Beckman Institute, University of Illinois, Urbana-Champaign

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## Abstract

Since the advent of human neuroimaging, and of functional magnetic resonance imaging (fMRI) in particular, the popular press has shown an increasing interest in brain-related findings. In this article, I explore possible reasons behind this interest, including recent data suggesting that people find brain images and neuroscience language more convincing than results that make no reference to the brain (McCabe & Castel, 2008; Weisberg, Keil, Goodstein, Rawson, & Gray, 2008). I suggest that part of the allure of these data are the deceptively simple messages they afford, as well as general, but sometimes misguided, confidence in biological data. In addition to cataloging some misunderstandings by the press and public, I highlight the responsibilities of the research scientist in carefully conveying their work to the general public.

## Keywords

fMRI, neuroscience, media, public

In recent years, even a cursory perusal of the science sections of our national newspapers will reveal a healthy proportion of brain-related findings. These articles boast such intriguing titles as “Cells That Read Minds,” to the more quirky “Neuron Network Goes Awry, and Brain Becomes an iPod.” Many of these articles report findings from the functional magnetic imaging (fMRI) literature. Indeed, Racine, Bar-Ilan, and Illes (2005) reported that press coverage of fMRI research in major newspapers and magazines increased exponentially from two in 1994, a couple of years after the blood oxygenation level dependent (BOLD) fMRI signal was first successfully measured in the human brain (Kwong et al., 1992), to over 40 in 2004 when the data were collected. In this article, I explore the press’ interest in the neurosciences and in fMRI in particular.

There are no doubt a number of reasons for the interest in neuroscience in the popular press. Recent studies suggest that at least one factor may be the degree to which the public find fMRI images and neuroscience language more convincing than results that do not make reference to the brain (McCabe & Castel, 2008; Weisberg et al., 2008). Weisberg and colleagues (2008) showed naive adults and students from an introductory cognitive neuroscience course descriptions of psychological phenomena that were followed by “good” or “bad” explanations of the phenomena. The good explanations were genuine explanations given by researchers in the field, and the bad explanations were “circular restatements of the phenomena”

(Weisberg et al., 2008, p. 471). The participants’ task was to rate how satisfying the explanations were. It is important to note, however, that half of the explanations were accompanied by neuroscience information that specified the brain region known to be involved in the task. Participants rated the bad explanations as less satisfying, but this effect was significantly reduced in the presence of the neuroscience information for both the naive adults and students of the neuroscience course. The researchers verified that the addition of neuroscience language did not provide any explanatory power by running the same experiment with neuroscience experts, defined as individuals beginning, currently pursuing, or having completed an advanced degree in cognitive neuroscience or a related area. These experts did not rate the “bad” explanations as being more satisfying when the neuroscience information was included. The authors conclude that nonexperts are “fooled” by scientific-sounding, but uninformative, neuroscience language.

Similar conclusions were reached by McCabe and Castel (2008) in regard to brain images. In their study, undergraduate participants read brief articles summarizing fictitious brain

## Corresponding Author:

Diane Beck, 2147 Beckman Institute, University of Illinois at Urbana-Champaign,  
405 N. Mathews Avenue, Urbana, IL 61801

E-mail: dmbeck@illinois.edu

imaging findings that included claims not necessitated by the data. Having read the article, the participants had to rate, among other things, whether the scientific reasoning in the article made sense. Each article was accompanied by either no visual image, a bar graph depicting the result, or a brain image. Participants rated the scientific reasoning in the text accompanied by a brain image as making significantly more sense than the same article accompanied by either the bar graph or no graphical representation. There was no difference in ratings between the articles with a bar graph and the articles without any graphical depiction of the data. It is interesting to note that this effect could not be attributed to the greater visual complexity of brain images over bar graphs. Participants did not rate articles with topographical maps, such as those depicting the scalp distribution of event-related brain potentials, as making as much sense as those accompanied by a brain image of the type obtained in fMRI and MRI experiments.

This positive effect of a brain image even persisted for an actual BBC article that either included or did not include a paragraph from another researcher criticizing the articles' conclusions; that is, for both articles containing a criticism and those that did not, participants rated themselves as agreeing more with the conclusions of articles accompanied by a brain image than with the conclusions of articles without a brain image. This significant effect of the brain image is particularly startling given that the presence of the expert criticism did not have a significant effect on participants' ratings of agreement. In other words, it would appear that the brain image carried more weight than a paragraph relating an expert's criticism of the article's conclusions.

Why are images and language that relate to the brain so influential? One possibility is the visual nature of the brain images or the visual imagery associated with the neuroscience language (Weisberg et al., 2008). However, this cannot be the entire story because, as McCabe & Castel (2008) showed, neither bar graphs nor colorful topographical maps engendered the same increased confidence that the fMRI and MRI images did. Indeed, fMRI research is not always accompanied by brain images in the press. Thus, the appeal of fMRI research must go beyond the visual nature of the data they produce. In this article, I discuss a number of possibilities for the appeal of fMRI research to the press and the general public.

## Provides a Simple Message

One factor that seems likely to account for some of the popularity of brain references or images is the simplicity of the message that they afford (i.e., complicated behavior  $X$  lights up area  $Y$ ). This can be a highly palatable statement to someone with little knowledge of fMRI or neuroscience. It sounds both definitive and scientific. As one op-ed columnist put it, "The hard sciences are interpenetrating the social sciences" (Brooks, 2009). Indeed, both McCabe and Castel (2008) and Weisberg et al. (2008) suggest that part of the allure of these kinds of statements is that, on the surface, they sound like the reductionism prevalent throughout the sciences. Chemistry can be

reduced to atomic physics, and behavior can be reduced to a brain region. Of course, the difference is that the latter reduction, on its own, lacks any explanatory power. Knowing, for instance, that individuals suffering from a protracted unabated bereavement exhibit activity in the nucleus accumbens when viewing pictures of the deceased (O'Connor et al., 2008) does nothing on its own to explain complicated grief (see *The Brain as Explanation* for more on this).

There is another way in which the apparent simplicity of a brain image, or resulting  $X$  activates  $Y$  statements, can be misleading. It obscures the complicated processes of arriving at these images. An fMRI image is not a photograph, even in the sense in which an X-ray image can be said to be a photograph. Instead, fMRI images are constructed from signals derived from the complex interactions of radio waves and the magnetic properties of hydrogen and deoxygenated hemoglobin. Thus, a great deal of sophisticated mathematics and signal processing is performed even before the fMRI researcher obtains his or her raw fMRI images. Then, before the researcher can even begin to assess the presence of activity, these raw fMRI images must be submitted to a number of preprocessing steps that include spatial realignment and signal normalization, to name just a few. Ultimately, activity is assessed by modeling the conditions of interest (typically in a general linear model framework) and by submitting the resulting model to statistical testing. Of course, all of this cannot be conveyed in a typical press article. Indeed even the fMRI research team itself is unlikely to have the physics, mathematics, and statistical expertise to perform every step in the construction and processing of their data without the help of existing software, written by outside experts. Nonetheless, it is worth pointing out that the construction of the colorful images we see in journals and magazines is considerably more complicated, and considerably more processed, than the photo-like quality of the images might lead one to believe.

Although the press may be forgiven for ignoring the complex processing involved in producing fMRI images, there is an even more fundamental and easily conveyed aspect of fMRI analysis that is very often omitted from press articles: fMRI "activity" can only be obtained by performing some kind of contrast. fMRI does not measure neural activity directly—it measures a correlate of neural activity, the proportion of deoxygenated hemoglobin relative to oxygenated hemoglobin in the blood. Because deoxygenated and oxygenated blood are present throughout the brain, in order to determine whether a region is "active," one must look for an increase in oxygenated blood in a particular region as a function of time, and, more specifically, as a function of conditions that change over time. In other words, the choice of tasks or conditions that the researcher will contrast is absolutely critical to obtaining activity, as well as any inference that can be made about this activity. By omitting this fact from a press article, one gets the impression that participants are asked to do a single task in the scanner, such as view pictures of women in bikinis, and voilà, a set of areas light up (Landau, 2009). In fact, the condition of interest must always be contrasted with another condition. In the bikini example, participants' viewing of pictures of women in bikinis needs to be

contrasted with their viewing of something else. Was it pictures of fully clothed women, pictures of scantily clad men, or a blank screen? Clearly, the choice made by the researcher would change how we interpret the “activity” associated with viewing scantily clad women. The omission of the control or comparison condition prevents the reader from critically assessing not only the suitability of the control task but also the corresponding inferences that can be drawn from the activity.

The omission of the comparison critical to obtaining the images promotes an even more fundamental misunderstanding. It implies that the failure of an area to light up means that there is no activity in that area when, in fact, the failure simply means that there was no difference in activity in that region for the conditions being contrasted. Take, for instance, the following statement from the Australian Broadcasting Corporation’s online news service (<http://www.abc.net.au>) regarding a study of male and female voices in the scanner: “The scientists found that female voices activate the brain’s auditory section, but male voices activated the area at the back of the brain called the mind’s eye” (Viegas, 2005). “What?” you say. Male voices do not activate auditory cortex? Of course they do. The problem arose because the press failed to understand the nature of the contrast used. The study contrasted male and female voices, and thus what they really found was that female voices activated the auditory cortex more than male voices, not that males voice failed to activate the auditory cortex (Sokhi, Hunter, Wilkinson, & Woodruff, 2005). This kind of error underscores the need for researchers to be more careful in ensuring that the message relayed to the press via press releases and article titles cannot be misinterpreted.

In summary, the de-emphasis on the behavioral contrasts at the core of fMRI research falsely depicts images of the brain as concrete and directly observable. However, these images of the brain in action are only as valid as the behavioral assumptions made in producing them. What does it mean to say that moral decisions are associated with activity in regions implicated in emotional processing (Greene, Sommerville, Nystrom, Darley, & Cohen 2001)? What exactly is meant by moral decisions or emotional processing? The only way to really understand these statements is to also know what are not moral decisions, and how emotional processing is being defined. What these concepts actually mean, in the context of the research, is completely dependent on the contrast the experimenters used.

## Confidence in the Biological

In reading various descriptions of fMRI research by the press, there is another theme that emerges: There is greater confidence in “biological” images than in the behavioral phenomena on which the images are based. One sees statements such as “Chocoholics really do have chocolate on the brain” (Farrow, 2007). Do people really doubt that chocoholics love chocolate? I suppose some people may be suspicious of the term *chocoholic*, thinking “I like chocolate as much as the next guy. I just have self control.” The fact that the sight and taste of chocolate activated the brain’s reward system more in admitted chocolate cravers than in noncravers does lend credence to the idea that some people find

chocolate more rewarding than others (Rolls & McCabe, 2007). But this is probably something most people already believe without the need for a corroborating brain scan.

This tendency to put more faith in brain images than in behavior can be seen even for phenomena met with more skepticism than the term *chocoholic*. *The New York Times* took a “neuroscientific look at speaking in tongues,” also referred to as *glossolalia* (Carey, 2006). Glossolalia is a practice in the Pentacostal and charismatic Christian churches in which practitioners produce fluent, but unintelligible, speech-like sounds that they describe as God speaking through them. In the press article, the author describes a study that used single photon emission computed tomography to compare regional cerebral blood flow in women speaking in tongues to cerebral blood flow in the same women singing gospel music (Newberg, Wintering, Morgan, & Waldman, 2006). Although glossolalia and singing both involve verbal utterances and evoke religious meaning in practitioners, the women described a lack of voluntary control over their vocalizations only during glossolalia. The study found decreased activity in the prefrontal cortex during glossolalia, a result that the study’s authors describe as consistent with the women’s descriptions of a lack of intentional control over their utterances. The press article does not explicitly endorse this conclusion but instead chooses to quote Andrew Newberg, the lead author on the study: “The amazing thing was how the images supported people’s interpretation of what was happening. The way they describe it, and what they believe, is that God is talking through them” (Carey, 2006). To an uncritical reader this statement sounds like the data support the position that glossolalia is the word of God. Moreover, by failing to raise an alternative, or even question this interpretation, the press article itself implicitly endorses this conclusion. The study’s result, however, is neither amazing nor does it support any particular cause of glossolalia. Decreased activity in the prefrontal cortex could be due to any number of reasons. For instance, unintelligible speech is likely to entail a lighter memory load than does recalling the words of a gospel song. Indeed, it is not at all surprising that brain activity differs under two conditions in which the practitioner claims to be in a very different state, regardless of whether the state reflects divine intervention, spontaneous vocalization, reduced memory load, or anything else. Instead, the most we can conclude from this research is that, when practitioners of glossolalia claim they are in a different mental state when they speak in tongues than when they sing, their brains corroborate that claim. Why then did such research make it to the national press? Part of the answer is that the topic of glossolalia is intrinsically interesting to people. However, as a newspaper’s goal is to report news, it would seem that *The New York Times* felt that this study added something to the discourse on glossolalia. Thus, it would seem the reporter and/or editors felt that a brain image showing that glossolalia differed from gospel singing was somehow more convincing than the practitioners’ own claims that this was the case.

It should be said that writing and publishing this article was, of course, the choice of the writer and editor. Thus, it may reflect

their own biases, or what they believe to be the biases of their readers, more than those of the actual public-at-large. We do not know the public's reaction to such an article or whether the data convinced anyone who had doubts about the ultimate source of glossolalia. Indeed, not everyone in the press accepted this story uncritically. Daniel Engber (2006) of *Slate.com* was not only rightly critical of the research, but also of *The New York Times's* portrayal of it, aptly comparing it to a fMRI study on eating ice cream and asking "If your test subject tells you he likes ice cream, what do we learn from the fact that his brain thinks so too?"

This is not to say that it is never useful to acquire brain data that corroborate a behavioral claim. Everyone is intimately acquainted with the subjective nature and fallibility of behavior, and thus the interest in the brain may be the result of a healthy skepticism of claims made on the basis of what someone says (e.g., "I am a chocoholic"). For example, although a great deal of research and effort goes into the classification of mental disorders in the *Diagnostic and Statistical Manual of Mental Disorders* on the basis of behavior, it is nonetheless comforting that there are a number of biological markers that correlate with depression, for example, even if those markers are not robust enough to serve as diagnostic criteria (Mössner et al, 2007).

### Confusing Biological With Innate or Inevitable

Related to a confidence in the biological is a misunderstanding of what a biological result indicates. In particular, there is a common tendency to confuse *biological* with *innate*. For example, the Associated Press (Schmid, 2005) reported on a PET study showing that when homosexual men sniffed a derivative of testosterone, their hypothalamus responded more like that of heterosexual women than heterosexual men (Savic, Berglund, & Lindstrom, 2005). The Associated Press rightly stated that "the findings clearly show a biological involvement in sexual orientation." However, they then make an erroneous jump from describing homosexuality as being biological to being innate, primarily in the form of a quote from Dr. Sandra Witelson: "It is one more piece of evidence . . . that is showing that sexual orientation is not all learned" (Schmid, 2005). A difference in the brain in no way indicates that the behavior under study is not learned. In fact, all learned behaviors will in some way change the brain. The brain is, after all, the source of all behavioral learning. Homosexuality may very well be innate, but a functional brain scan could not prove, nor disprove, that position.

### The Brain as Explanation

Another common mistake made in the press is illustrated by an article in *The Guardian* newspaper entitled "Brain Scans Pinpoint How Chocoholics Are Hooked" (Farrow, 2007). As mentioned above, the article describes research in which chocolate cravers show greater activity in reward structures of the brain than noncravers when viewing and tasting chocolate. There is no explanation of "how" chocoholics are hooked, just a correlation between chocolate craving and activity in certain regions

of the brain. We do learn that reward structures are implicated, but once again we probably did not need a brain scan to tell us that chocolate cravers find chocolate rewarding.

Why do such articles get picked up by the press? Do the editors and writers really feel they understand chocolate craving better? Part of the explanation may be that people have a general interest in human behavior, regardless of whether the article allows for any real insight. However, the language used to sell the story also indicates a confusion about what constitutes an explanation.

Of course, associating a brain region with a behavior can potentially lead to an explanation. Indeed, for those of us using neuroimaging, we pursue such knowledge in the hope that it will provide a bridge to other knowledge that may ultimately help to explain a condition or behavior. For example, a brain region may be associated with other behaviors, neurotransmitters, or single cell data that in turn might shed light on the original behavior of interest. Previously, I stated that knowing that complicated grief is associated with greater activity in the nucleus accumbens (O'Connor et al., 2008) does nothing, on its own, to explain complicated grief. However, knowing that the nucleus accumbens is highly associated with pleasure and reward raises the interesting possibility that individuals suffering from complicated grief may actually experience a kind of reward in ruminating over their loss. Similarly, knowing that attending to multiple items, as opposed to a single item, reduces activity in regions of the visual cortex known to exhibit competitive interactions among cells suggests that the difficulty in attending to multiple items simultaneously may stem in part from these competitive interactions (Scalf & Beck, 2010). It is important to note, however, that, in both of these examples, it is this additional knowledge associated with the brain region described that ultimately affords any explanatory power.

### Responsibilities of Behavioral Scientists

Thus far, I have concentrated on misunderstandings regarding fMRI in the press: The press provides a simple (perhaps, too simple) message; the press exhibits a confidence in the biological, but at times that confidence is misplaced and stems from misunderstandings of the term *biological*; and the press sees neuroimaging as explaining behavior, even when the information obtained lacks any explanatory power. Of course the public's interest in fMRI is not entirely misguided. There is a positive side to all of these factors. It is useful for both science and society for the public to be informed about ongoing research. To ensure that such information reaches a wide audience, it is helpful when the research can be distilled into a simple message. The difficult part is making sure that the simple message is still correct, and this task starts with the researcher. It is a boon to all of the behavioral sciences that the press and the public find our work interesting and that that interest is rekindled by "biological" data; they too find it remarkable that we can peer into the human brain. However, it is the responsibility of all of us in the behavioral sciences, and especially those whose research is being highlighted, to accurately



communicate what can and cannot be concluded from our data. We should be careful not to encourage portrayals of our research as explaining a behavior or condition when it does not. We must provide the press with descriptions of our research and interpretations of our results that are carefully crafted to be clear, relevant, and scientifically accurate.

Some may argue that an occasional overreaching in the press is an acceptable price to pay to keep our research in the public eye. It is beneficial to us all to raise public awareness of the behavioral sciences. However, a lack of rigor in this endeavor on the part of the scientific community not only misinforms the public, but also, over time, has the potential to undermine the public's confidence in our research. This distrust by the public is already observable in relation to the health sciences. The press often presents the latest news in health as a breakthrough or as incontrovertible truth. This, after all, is what sells newspapers. But those of us in science know that breakthroughs are actually very rare; that all results require interpretation; and that scientific knowledge is actually built up very slowly over time, with path corrections along the way. This disconnect between the goals of the press and the result of the scientific process leads to often contradictory headlines, where coffee consumption, for example, is good for you one week and bad for you the next. Without an understanding of the scientific process, it can appear to the public that the facts keep changing. The public's reaction to this apparent inconsistency is not to assume that the press has mischaracterized the results or that this is the natural back and forth of the scientific process. Instead, they conclude that the supposed experts simply do not know what they are doing. Such distrust of science is not particularly damaging to society when it comes to things such as coffee consumption, but it can be when the people decide not to vaccinate their children against measles, mumps, and rubella (i.e., the MMR vaccine), because of unfounded fears that persist in the media despite scientific evidence to the contrary.

In short, every time we allow the press to mischaracterize our results or overstate our conclusions, we run the risk of damaging the reputation of our entire field in the eyes of the public. It is not only our responsibility to educate the public, allowing for an increasingly sophisticated understanding of the brain and behavioral sciences, but we must also be mindful of the fact that the "dumbing down" of science may diminish our impact and the perceived importance of science to society as a whole.

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The author declared that she had no conflicts of interest with respect to her authorship or the publication of this article.

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