

# The Salmon of Doubt

## *Six Months of Methodological Controversy within Social Neuroscience*

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There was something fishy going on ...

Ed Vul, in an interview with Jonah Lehrer (2009a)

In the final week of 2008 a controversial article began swiftly circulating through the neuroscience community (Vul, Harris, Winkelman, & Pashler, 2009). Although it is generally uncommon for articles in press to slip far beyond personal correspondence, the implications of this publication were such that science blogs immediately broke the story just before the new year (Bell, 2008; joneilortiz, 2008; *Neurocritic*, 2008; Roberts, 2008). Word spread that over two-dozen articles, many in the highest-ranked journals, were openly accused of invalid results. Certainly, for anyone who read the decidedly combative title, “Voodoo Correlations in Social Neuroscience”, this urgent manner of dissemination was understandable. Social neuroscience, the most rapidly emerging field of the past decade, entered 2009 in a rather vulnerable position.

Through the month of January researchers within the community, myself included, worried that our methodological foundation was in jeopardy. This chapter will follow the aftermath through the subsequent six months. It was an interval of heated public controversy that ultimately climaxed, I will argue, in a brilliant display of neuroscientific irony. The scientific debate itself focuses on proper statistical practice in functional magnetic resonance imaging (fMRI) research. However, even if the content of the debate may be quite methodologically oriented, it is my hope that the various formulations of criticism, the conventions of debate (and their disregard), as well as the process of ensuing resolution may offer a worthwhile narrative case study to those embarking on analogous campaigns.

## PART I: Voodooos and Don'ts

### January: The scientific record

For cognitive neuroscientists around the world, the new year in 2009 began with a surprising email attachment. Although it has become common practice for journals to publish articles electronically, sometimes several months before they appear in print, when “Voodoo Correlations in Social Neuroscience” (henceforth “Voodoo Correlations”) began circulating, its wide dissemination was reportedly a surprise even to the lead author (Lehrer, 2009a). More damaging for social neuroscientists than even the content of the article—many had not yet read it—were the sensational headlines appearing throughout the blogosphere, such as: “Scan Scandal Hits Social Neuroscience” (*Neurocritic*, 2008) and “Vul on fMRI Abuse in the Cognitive Neuroscience of Social Interaction” (Joneilortiz, 2008). A response, any response, was called for from the social neuroscience community. Making matters more complex, the unfamiliar terrain of the internet-based scientific discourse would render the usual debate strategies ineffective, thus demanding novel approaches for the forum of the 24/7 online community. With the field under attack, responses and rebuttals were published more rapidly than the time usually taken to return page proofs.

The mordant title alone was certainly enough to produce a flurry of reactions. However, at the core of “Voodoo Correlations” lay a nuanced criticism of statistical practice in neuroimaging studies. As Christian Keysers was quoted later in the month in *Nature News*: “We all agree that there is a kernel of truth in what Vul and his colleagues write about some of the literature being shaky ... We can never be reminded often enough of the importance of good statistical practice” (Abbott, 2009). Considering the subsequent impact of media sensationalism, which largely obfuscated the core issues, we should begin by outlining the statistical criticism.

Vul and colleagues introduced their article by giving examples of the “puzzlingly” high correlation values between behavior and fMRI measurements found in numerous social neuroscience studies. There is no doubt that social neuroscience had been remarkably successful in describing the relationship between behavior and brain activity, publishing numerous articles in the most high-ranking journals. Examples cited in the introduction of “Voodoo Correlations” include the 0.88 correlation between anterior cingulate activity during a social rejection game and subsequent self-reports of the amount of distress participants felt (Eisenberger, Lieberman, & Williams, 2003), correlations of 0.52–0.72 between anterior cingulate activity during empathy manipulations with two scales of emotional empathy (Singer et al., 2004), and a massive 0.96 correlation between a scale measuring proneness to anxiety and cuneal activation during angry speech (Sander et al., 2005).

The basis for Vul and colleagues’ suspicion, as they explained, is that the strength of a correlation between two variables is not simply a result of their direct relationship, but also a factor of the independent reliability of both measures. For instance, if you want to explore if the intelligence of a carp predicts the intelligence of the fisherman who can catch it, the theoretical maximum correlation value would be based on the reliability of both intelligence measures. As Vul and colleagues applied the calculation,

claiming the most optimistic test-retest reliability of psychological scales ranging 0.7–0.8, and fMRI test-retest peaking at approximately 0.7, correlations between the two should have an upper bound of 0.74. The authors thus argued that the less-than-perfect reliability of these two measures rendered many of the reported correlation values, often exceeding 0.8 in social neuroscience studies, to be “impossibly high.”

In order to investigate the origins of such correlation values, the authors selected 55 articles for a detailed investigation of the methods. However, upon surveying the publications, they often encountered a lack of clarity in the methods section, and thus conducted a four-question, multiple choice email survey aiming to clarify the analyses underlying the data presentation. An impressive 53 of the 55 authors responded.

Based on the responses, Vul and colleagues concluded that the core statistical error driving the “impossibly high” correlation values was “non-independence error.” The basis of such error results from a two-step procedure, in which voxels marked during a first analysis are *selectively* analyzed in a second analysis. This procedure is of significant concern in fMRI analysis due to the large number of variables included. With up to 60,000 voxels in the brain, the likelihood of randomly encountering a significant relationship in any single voxel is rather high. Thus, several solutions to the problem of “multiple comparison correction” have been developed specifically for fMRI data. The non-independence error is committed when selection criteria for a second analysis on a data set are based on results from a first analysis on the same data. In such cases analyses favor areas that have already been demonstrated to be related. Vul and colleagues then compiled a “red-list” of 28 articles, which were deemed guilty of non-independence error, hereby, they claimed, invalidating the results.

When attack is still diffuse and indirect, guilty parties can pretend that the bullets are not intended for them. However, Vul and colleagues had wisely taken a more sniper-like tactic. Their unequivocal critique resounded with the concluding statement: “At present, all studies performed using these methods have large question marks over them. Investigators can erase these question marks by re-analysing their data with appropriate methods.” Names had been named; allegations had been made, and drastic terms for exoneration had been laid out. Authors of the cited articles were thus faced with the options of: (1) admitting the wrong and re-analyzing their data; (2) pleading innocence and demonstrating the error in the “Voodoo Correlations” critique; or (3) hoping that nobody had noticed.

Although for weeks “Voodoo Correlations” had been discussed amongst blog-savvy neuro-enthusiasts, on the morning of Friday, January 9, the article finally reached the mainstream. Sharon Begley, author of the *Newsweek* blog Lab Notes, posted “The “Voodoo” Science of Brain Imaging” (Begley, 2008), in which she described the situation for social neuroscientists in terms that leave little wiggle room: “a bombshell has fallen on dozens of such studies: according to a team of well-respected scientists, they amount to little more than voodoo science.” Those frustrated with the sensationalism surrounding the rapidly growing field of social neuroscience considered “Voodoo Correlations” a coveted victory (“Editorial: What were the neuroscientists thinking?” 2009; Giles, 2009); those attacked by the article considered it an offense to proper scientific discourse. Regardless of perspective, the article had been noticed, and the indictment of an entire field (not to mention publications in the most

reputable scientific journals such as *Nature* and *Science*) made this accusation difficult to shrug off quietly.

As Begley parenthetically suggests: “in fairness, the skewered authors should be given a chance to defend themselves,” several accused authors responded by attacking the validity of Vul and colleagues’ claim through both conventional and Internet-guerilla tactics. On January 13, Christian Keysers began energetically posting links throughout the blogosphere to a response article he had co-authored with Mbemba Jabbi, Tanya Singer, and Klaas Enno Stephan (Jabbi, Keysers, Singer, & Stephan, n.d.; Lieberman, 2009; *Neurocritic*, 2009). Much of “Response to ‘Voodoo Correlations in Social Neuroscience’ by Vul et al.—summary information for the press” takes the form of accusations of libel for the indiscriminate and unwarranted criticism made against social neuroscience. As they explain, the field is larger than the details of a specific statistical technique: “statistical arguments that are partially flawed, and misleadingly implies that social neuroscience studies rest entirely on the sort of brain-behaviour correlations that are criticised.”

Through eight brief counter-arguments the authors offer broad-spectrum critiques of “Voodoo Correlations,” which basically aim to undermine the “outsider” understanding of social neuroscience held by Vul and colleagues. For example, while Vul and colleagues argue that secondary analyses based on regions selected during an initial analysis constitutes non-independence error, Jabbi and colleagues claim that the correlation coefficients and p-values are reported for the purpose of illustrating effect size alone, and thus do not constitute secondary analyses. Furthermore, the response explains that the question underpinning social neuroscience studies is not the strength of correlations between brain and behavior, but rather where in the brain such correlations occur. Finally, there is frustration with the brevity of the questionnaire, and that based on such minimal data Vul and colleagues “flag a set of studies as ‘problematic’ without discriminating when non-independence errors were committed and when not.” The response, aimed at the same lay audience that had embraced the sensationalism of the initial article, established a counter-argument to “Voodoo Correlations,” while launching the two-sided debate within the public sphere.

The response, however, came too late for the accelerating media aggression. On January 14, the *Wall Street Journal* quoted the senior author of “Voodoo Correlations,” Harold Pashler, in an article about the use of neuroimaging technologies in the courtroom: “In the law, individual differences are the main focus ... and it often could come down to these voodoo statistics” (Hotz, 2009). *New Scientist* had published an editorial on the same day entitled: “What were the neuroscientists thinking?” (2009). The author laments the journal’s own involvement in promulgating many of the criticized studies: “We have to eat a little humble pie and resolve that next time a sexy-sounding brain scan result appears we will strive to apply a little more skepticism to our coverage.” These same publications that were so quick to embrace social neuroscience’s successes were just as quick to propagate its alleged failures—after all, they had been duped as well.

Amidst the popular media hubbub, a more technical dialogue persevered in the blogosphere with a rebuttal by Vul and colleagues. The reply (“Reply to Jabbi et al.”) can often be found immediately succeeding Keyser’s posts on numerous blogs on January 15. It begins with a disclaimer that Jabbi and colleagues’ response has an

“evolving rebuttal, but it has changed at least once since we replied to it, so we can’t be sure whether our comments below will address the points in this version” (Ed Vul, 2009). They are referring to the removal of two-thirds of the introduction, which in the final version condenses the introduction into a two-sentence lead-in for the summarized points. Vul and colleagues’ reply, likewise, contained eight corresponding counterpoints. They argue that multiple comparison correction does not safeguard against the inflation of secondary correlation analyses. And although social neuroscience may not fixate on the amplitude of correlations, Vul and colleagues respond that the scientific literature should nonetheless be free from such statistical errors.

A more thorough critique of “Voodoo Correlations” was posted online beginning on January 27 (Lieberman, 2009). “Correlations in social neuroscience aren’t voodoo: A reply to Vul et al.” by Matthew Lieberman, Elliot Berkman, and Tor Wager (2009), went as far as to mimic the long list of acknowledgements on the cover page of “Voodoo Correlations.” The reply presents a far more exhaustive treatment of “Voodoo Correlations” than the admittedly rushed previous attempt of Jabbi and colleagues. As stated on the cover page, it is an “Invited reply” under submission at the same journal which had accepted “Voodoo Correlations.” In general terms Lieberman and colleagues attacked several weaknesses in the methods, including the unexplained absence of 54 correlation values from the meta-analysis conducted in “Voodoo Correlations.”

Their primary concern, returning to an outstanding debate, involves the magnitude of the statistical error. While Jabbi and colleagues argued that enhanced correlation values were not of primary concern to social neuroscientists, Vul and colleagues countered that no statistical error belongs in scientific practice. Here, a novel question is raised: if the statistical error only results in minor increases in significance values, does that really merit the designation of “voodoo?” Connoting magical practice and the absence of any genuine scientific support, the term “voodoo” may have been used unfairly.

I have focused the discussion of January on the technical foundation of the debate, along the way highlighting certain players in the story. However, it was not only the accused who felt their fates to be in a tenuous position—cognitive neuroscientists around the world were concerned for the future of their profession. Many had heard the title before working through the paper, and perhaps all were unknowingly guilty of such an error. Nonetheless, a rigorous methodological rebuttal by Lieberman and colleagues brought the story back into perspective by the end of January—and rightly, the optimistic conclusion of their article announces the onward march of social neuroscience:

There are various ways to balance the concerns of false positive results and sensitivity to true effects, and social neuroscience correlations use widely accepted practices from cognitive neuroscience. These practices will no doubt continue to evolve. In the mean time, we’ll keep doing the science of exploring how the brain interacts with the social and emotional worlds we live in.

(Lieberman et al., 2009)

## February: The scientific discourse

The heated controversy of January did finally find resolution towards the end of the month. I’ll summarize the general sentiment with a personal anecdote. In the first

week of February, I received a text message from a colleague in New York: “Went out for drinks with the stats department. Comfortable now that voodoo correlations argument is bullshit.” Researchers, even those not directly attacked by “Voodoo Correlations”, were relieved that the criticism was not as grave as it initially appeared.

Scientific discourse can take various forms. With the theoretical debate ebbing, the practical implications of the public’s involvement became the focus of discussions. Let us examine briefly Tania Singer’s assertion in a *Nature News* article on January 15. In what ways was the experience with “Voodoo Correlations,” as she claimed, “not the way that scientific discourse should take place” (Abbott, 2009)? The *Nature* article offers a suggestion of the meaning implicit in Singer’s comment:

The swift rebuttal was prompted by scientists’ alarm at the speed with which the accusations have spread through the community. The provocative title ... and iconoclastic tone have attracted coverage on many blogs ... Those attacked say they have not had the chance to argue their case in the normal academic channels.

(Abbott, 2009)

Three particular issues emerge here regarding the wider shifts at play in the scientific discourse: (1) the increased speed of dissemination; (2) the sensational title aimed at media coverage; and (3) the role of public debate in discussing complex methodological topics. While these observations had been aimed as accusations against Vul and colleagues, both parties appeared to be equally guilty.

The issue of speed is crucial. “Voodoo Correlations” is often accused of having being released before journal publication, but whilst such statements may be subtly underplayed, and although the article had in fact been accepted by the time of release, the suggestion is that “Voodoo Correlations” leaked in a “scientifically inappropriate” manner. While there was certainly much surprise at the speed of dissemination, it is important to note that there was nothing unusual about the release protocol of the article. Oddly, the most classically “improper” scientific proceeding during the month of January was the release of Jabbi and colleagues’ rebuttal, which contained a seemingly self-aware admission of its own prematurity: “A detailed analysis will be submitted to a peer reviewed scientific journal shortly” (Jabbi et al., 2009). Strangely, the eventual details of their analysis do not extend beyond the original online publication, although a second online version was released shortly thereafter with a revised introduction.

With respect to sensationalism in the article title, Ed Vul was candid about the choice: “We wanted to make the paper entertaining and to increase its readership. We wanted our paper to have some impact. If people don’t know about these statistical problems, nothing will be done to fix them.” The accusation there is certainly justified. However, the necessity of such sensationalism in order to elicit a response from the research community raises a question with respect to the role of the public voice in motivating innovation in science.

The public’s role in the debates surrounding “Voodoo Correlations” was a contentious topic for those accused by the article. The authors of the reply rejected an invitation to openly debate the issues on a public blog, claiming instead that “the critique will be dealt with in peer-reviewed literature in forthcoming papers by the scientific community” (Klincewicz, 2009). They went on to state that they would

postpone further discussion until “a proper scientific dialogue occurs; not a dialogue by press and anonymous blogs who cannot evaluate the statistical claims made by Vul et al. Popular opinion asserts that the way in which the paper is discussed does not support fair and suitable scientific manner” (Klincewicz, 2009). At the heart of such debate tactics is the trope of “proper science” and the discursive weight of the objective scientist. “Science” in this context implies a private, “expert,” insular discourse, that is, not for laymen—but with “valid scientists” on both sides of the debate, such expertise cancels itself out. For instance, in her *Newsweek* blog, while buttressing the criticism by Vul and colleagues by describing the authors as “well-respected scientists,” Begley also makes claims that seemingly contradict the trope of objectivity in scientific practice:

If you were wondering how, exactly, problematic studies got past the peer review at these top journals, that’s a clue: scientists no less than other mortals love to have their hunches, prejudices and stereotypes validated by empirical evidence. Maybe they didn’t look too critically at studies that did exactly that.

(Begley, 2008)

Contradictions such as this appear throughout the debates surrounding “Voodoo Correlations.” After all, both the accused and accusing parties are equally well-respected scientists, and both the accused and accusing articles are published in peer-reviewed scientific journals. The traditional strategies used in public scientific debate—namely, attempting to invalidate the scientific legitimacy of your opponent—are at times attempted, but are largely ineffective. In the case of the “Voodoo Correlations” debate, the non-scientific public was given the role of the invalid scientist, unequipped to engage in thoughtful criticism.

Behind all the well-worn polemics, what was at the core of the frantic opposition to the publicity received by “Voodoo Correlations”? Again, one indication can be found towards the conclusion of the *Nature News* article, where Chris Frith, author on several of the red-listed articles, strips away the rhetoric of “proper scientific discourse,” revealing the issues in more realistic terms: “We are not worried about our close colleagues, who will understand the arguments. We are worried that the whole enterprise of social neuroscience falls into disrepute” (Abbott, 2009). Disrepute can indicate many things, but the sentiment takes another form during interviews with Tor Wager, who was also one of the authors of the reply article with Lieberman. Wager makes clear that “Voodoo Correlations” negatively (and unwarrantedly) biases funding and top journals (Lehrer, 2009b). Even if at times researchers may want the public to remain out of specialized debates until resolution within the community can be achieved, public opinion still contributes to the practical outcome for the field as a whole. Interestingly, although scientists are usually also the advisors or leaders of funding agencies and top journals, it is made clear here how biased they too become through the public discourse.

Perhaps *Seed Magazine* contributor, Jon Bardin, summarized the events of February best: “When findings are debated online, as with a yet-to-be-released paper that calls out the field of social neuroscience, who wins?” (Bardin, 2009).

### March: The scientific agenda

With little changes in the “Voodoo Correlations” story during the month of March 2009, perhaps this presents an ample moment to reflect on the distance that criticism travelled within cognitive neuroscience over that year. This was not the first time social neuroscience had been under attack—the statistical debate of 2009 was, in many ways, a reincarnation of former critiques of overvaluation and over-interpretation of social neuroscience imaging results. If in 2009 the critical approach to cleaning up statistics was challenged, so in 2008 the over-interpretation of fMRI results was contested. Articles with self-reflective titles were being published that year, such as “What we can do and what we cannot do with fMRI” (Logothetis, 2008) and “The role of fMRI in cognitive neuroscience: Where do we stand?” (Poldrack, 2008). Where in 2009 the propriety of using Internet media as a forum of scientific debate was disputed, in 2008 the focus was on printed, but no less public forms.

Much was spurred by the ground-breaking *New York Times* op-ed piece “This is your Brain on Politics” of November 11, 2007 (Iacoboni et al., 2007), which set a precedent for self-publishing results in the popular media in advance of journal acceptance. Marco Iacoboni and colleagues presented an fMRI study of 20 swing votes viewing the political candidates (almost replicating a study they had published three years previously; see Kaplan, Freedman, & Iacoboni, 2007). The piece was written in the standard journalistic style and reflected extensively how brain data impacted on the perceived status of the various presidential candidates. It was sensational in the extreme and, needless to say, provoked a commensurate retaliation from the neuroimaging community in *The New York Times* a few days later, signed by no fewer than 17 leaders in the field (Aron et al., 2007).

A poignant generational gap is evident in the comfort with which the individuals involved in the “Voodoo Correlations” debate navigated the social community on the Internet. One telling example is the alacrity of Vul and colleagues’ reply to the Jabbi and colleagues’ response article. Within 24 hours they had published a response online.

Unlike “Voodoo Correlations,” the controversy in the *New York Times* piece was spurred by the true lack of peer-review in the article’s publication. Perhaps some residual strategies had made their way into the Vul debate without recognition of the numerous differences. While a shift in critical priorities occurred from 2008 to 2009, the underlying motivation may have persisted: a general concern over the increasing popularity and ubiquity of social neuroscience.

## PART II: Critical Tactics in Action

### April: Naming names

With the controversy of “Voodoo Correlations” as yesterday’s news, Nikolaus Kriegeskorte and colleagues at the National Institute of Mental Health published a second, albeit gentler, reprimand of the neuroimaging community in the high-impact journal *Nature Neuroscience* (Kriegeskorte, Simmons, Bellgown, & Baker, 2009:



online publication April 26, 2009). Much theoretically aligned with “Voodoo Correlations,” the article also carried the requisitely catchy title “Circular analysis in systems neuroscience: The dangers of double dipping.” The content of the critique centers on selection bias in analyses—almost identical to the critique of non-independence error. Thus, *voodoo correlation* was rechristened the less exotic, though equally condescending *double dipping*. Nonetheless, the determinedly gentler title, invoking ice-cream or Seinfeldesque taboos, did not usurp voodoo correlation as the descriptive term within the field—nor did any comparable media coverage follow.

The lack of reaction is probably attributable to the lack of direct finger pointing, characteristic of “Voodoo Correlations.” With nobody feeling the need to defend his or her careers, nobody really did. When all were arguing about the proper *place* to debate science in the early part of the year, they may rather have meant to say that proper science simply should not get personal.

### May: Name dropping

By May, the anxiety had subsided—scanners were still running, funding had not been revoked, and social neuroscience had not been disbanded—just in time for the relatively quiet publication of volume 4, issue 3 of *Perspective on Psychological Science*. On the surface the final version of Vul and colleagues’ article is unrecognizable with reparations apparent in the new title, “Puzzlingly High Correlations in fMRI Studies of Emotion, Personality, and Social Cognition” (Ed Vul, Harris, Winkielman, & Pashler, 2009). An awkward title footnote marks the emasculation: “This article was formerly known as ‘Voodoo Correlations in Social Neuroscience.’” The text remains largely unchanged, however “social neuroscience” is nowhere to be found, and in its place is a thorough substitution of “fMRI studies of emotion, personality, and social cognition.” The issue also contained six response articles, a “reply to comments” from Ed Vul and colleagues, and an editor’s introduction.

In addition to referring to the rechristening in final publication of “Voodoo Correlations,” the title of this section also refers to another theme raised in May 2010: the suggestion that other issues should also be marked for urgent critical discourse within the neuroimaging community. While the question of statistics was of crucial importance, its relative resolution gave the illusion that social neuroscience had been packaged up neat and tidy again.

Of all the contributions, only that of the editor Ed Diener, commented on the peripheral issues raised by the community’s handling of the article, suggesting “that the debate can itself stimulate useful discussions about scientific practices and communication” (Diener, 2009). He then suggests that his journal is not an appropriate forum to continue the debate, instead asking that “further discussion of the issues should now take place in journals that are focused on imaging and neuroscience.” However, just before distancing himself, he manages to slip a brief editorial comment into the penultimate paragraph: “In addition, there are questions related to what relative blood-oxygen levels actually signify about the mind when they are uncovered.” The suggestion here is that amidst the hubbub surrounding “Voodoo Correlations,” there are other fundamental issues of cognitive neuroimaging with fMRI that may reflect equally unfavorably if treated with similar attention.

Diener significantly calls attention to the fact that just because the crisis caused by the “Voodoo Correlations” article has been resolved does not mean that all is well and good in fMRI studies in social neuroscience research. Many more important questions and assumptions remain untested and unanswered. The question raised by Diener, though tactfully underplayed, may be of too grand a scale. Greater impetus, on the scale of “Voodoo Correlations,” may be truly necessary to engage a community in earnest self-reflection and productive criticism.

### June: Swimming upstream

Perhaps the most globally appreciated prank to ever make use of an fMRI scanner was brought to the attention of the neuroimaging community at the Organization for Human Brain Mapping’s annual conference. During the final lecture on June 22, the past chair, Rainer Goebel, delivered his “closing comments and meeting highlights” to a full auditorium in San Francisco. After reviewing many of the emerging directions in the field, he displayed what he described as one of his favorite posters from the conference: “Neural Correlates of Interspecies Perspective Taking in the Post-Mortem Atlantic Salmon: An Argument for Multiple Comparisons Correction.” It was greeted with a cathartic laughter of recognition.

The abstract, by Craig Bennett, Michael Miller and George Wolford (Bennett, Miller, & Wolford, 2009: later to include Abigail Baird on the poster), described a study of social cognition in “one mature Atlantic Salmon (*Salmo salar*).” In keeping with scientific punctiliousness, and no doubt to thwart any appropriation of their study by the overzealous, the authors then noted: “The salmon was approximately 18 inches long, weighed 3.8 lbs, and was not alive at the time of scanning.” The task paradigm was delivered with the familiar laconic methods section:

The task administered to the salmon involved completing an open-ended mentalising task. The salmon was shown a series of photographs depicting individuals in social situations with a specified emotional valence. The salmon was asked to determine what emotion the individual in the photo must have been experiencing. Stimuli were presented in a block design  
(Bennett et al., 2009)

Before proper correction for multiple comparisons, a cluster  $27\text{ mm}^3$  was found to be significant within the brain cavity; however, the authors dutifully noted that “due to the coarse resolution of the echo-planar image acquisition and the relatively small size of the salmon brain further discrimination between brain regions could not be completed” (Bennett et al., 2009). Of course (and thankfully), after proper statistical correction, no active voxels were detected.

To those unfamiliar with the techniques, this appeared to be another successful attack against social neuroscience—it conclusively demonstrated the virtually limitless potential of opaque fMRI statistics. With the same sense of vindication with which “Voodoo Correlations” had previously been disseminated, the “Atlantic Salmon” poster filled inboxes and blogs across the community in the following months. However, those within the community understood that the obvious tongue-in-cheek presentation was far from being an attempt to invalidate fMRI approaches to questions

of social cognition. Rather, it was an example of statistical criticism, which reinforced the validity of correction techniques that have long been argued as essential. In fact, the common statistical error cajoled by Bennett and colleagues was also critically addressed as a subpoint by “Voodoo Correlations” in the results and discussion section.

Before 2009, the nuanced debate over proper multiple comparison correction had rarely leaked beyond the fMRI methods and statistics community. By integrating the criticism into a tongue-in-cheek experimental context, the point was made while garnering public attention. Reiterated here in a more accessible form, perhaps this last rendition provided precisely the emotional closure the fMRI research community needed. The problematic was genuine, but there was an effective solution. The ominous implications of voodoo correlations were finally transformed into a unifying mascot: *the salmon of doubt*. Of course, the content of the salmon poster was not the same precise criticism at the centre of “Voodoo Correlations.” The former addressed the problem of multiple comparisons, while the latter dealt primarily with the non-independence error (although it also addressed problems with certain forms of multiple comparison correction). Nonetheless, a similar approach could well be used to represent the dangers of the biased selection of regions for secondary analysis.

Rooted in an intimate knowledge of statistics, the methodological critique could only come from those who were, at least to some extent, within the field. For instance, Ed Vul explained in a interview with Jonah Lehrer that “Voodoo Correlations” began the year prior with a sense he had that “there was something fishy going on ... despite our suspicions, we didn’t know exactly what that fishy thing was, so we put the topic aside” (Lehrer, 2009a). After joining Nancy Kanwisher’s lab at MIT, he began “working directly with fMRI data” and “learned the relevant jargon and statistics.” Certainly, to understand the criticism, one needs the insider knowledge described by Vul. Thus, Vul also recognized that such knowledge would not be easily accessible to those outside the community.

Vul and colleagues’ criticism was, however, also positioned from outside the field it attacked. While it certainly had impact, that effect may have been limited as researchers quickly scrambled to resolve the methodological dispute (which was statistical, and could eventually be answered). Rather, by sketching the critique within neuroscience, as Bennett and colleagues did with such humor, the argument may have been received more productively. The salmon study does not encourage a rebuttal; simply recognizing its irony is a form of corroboration.

As the 1990s was the “decade of the brain,” the 2000s are already being labeled the “decade of social neuroscience.” Perhaps a bit of unrealistic optimism urges the question: could the looming discomfort in recent years with neuroimaging studies signal the start of the “decade of ironic neuroscience”? The critical strength of irony may be its potential to unify while still making its argument understood within the target community—a valuable tool for any critical neuroscience endeavor.

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