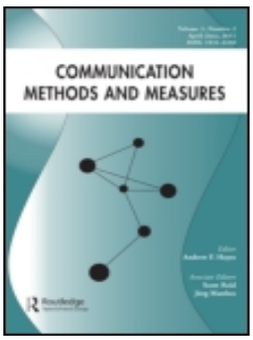

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Communication Methods and Measures

ISSN: 1931-2458 (Print) 1931-2466 (Online) Journal homepage: https://www.tandfonline.com/loi/hcms20

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To cite this article: René Weber, J. Michael Mangus & Richard Huskey (2015) Brain Imaging in Communication Research: A Practical Guide to Understanding and Evaluating fMRI Studies, Communication Methods and Measures, 9:1-2, 5-29, DOI: 10.1080/19312458.2014.999754

To link to this article: https://doi.org/10.1080/19312458.2014.999754

Communication Methods and Measures, 9:5–29, 2015

ISSN: 1931-2458 print/1931-2466 online
DOI: 10.1080/19312458.2014.999754

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Brain Imaging in Communication Research: A Practical Guide to Understanding and Evaluating fMRI Studies

René Weber, J. Michael Mangus, and Richard Huskey
University of California Santa Barbara

Core communication research questions are increasingly being investigated using brain imaging techniques. A majority of these studies apply a functional magnetic resonance imaging (fMRI) approach. This trend raises two important questions that we address in this article. First, under what conditions can fMRI methodology increase knowledge and refine communication theory? Second, how can editors, reviewers, and readers of communication journals discriminate sound and relevant fMRI research from unsound or irrelevant fMRI research? To address these questions, we first discuss what can and cannot be accomplished with fMRI. Subsequently, we provide a pragmatic introduction to fMRI data collection and analysis for social-science-oriented communication scholars. We include practical guidelines and a checklist for reporting and evaluating fMRI studies.

Brain imaging methods are on the rise in communication. Inspired by a paradigmatic shift toward process driven, materialist communication science (Lang, 2013; Lang & Ewoldsen, 2013; Weber, Sherry, & Mathiak, 2008; Weber, 2015), and enabled by innovations in brain imaging technology (Mather, Cacioppo, & Kanwisher, 2013), we are beginning to see an increasing number of studies that examine core communication research questions using functional neuroimaging. Examples, which are by no means exhaustive, include studies on television violence (Murray et al., 2006), on playing video games (Bavelier et al., 2011; Mathiak & Weber, 2006; Weber, Ritterfeld, & Mathiak, 2006), on deception detection (Langleben et al., 2002), on affective responses while watching movies ("neurocinematics"; Anderson, Fite, Petrovitch, & Hirsch, 2006; Hasson, et al., 2009; Hasson, Nir, Levy, Fuhrmann, & Malach, 2004), on persuasion (Falk, Berkman, Mann, Harrison, & Lieberman, 2010; Berkman & Falk, 2013; Weber, Huskey, Mangus, Westcott-Baker, & Turner, 2015), on health messages (Falk, Berkman, Whalen, & Lieberman, 2011), and on strategies in marketing communication (Ariely & Berns, 2010).

Most of these studies, as well as many other communication brain imaging studies, use a functional Magnetic Resonance Imaging (fMRI) approach. The recent trend of fMRI studies in core areas of communication research raises two main concerns. First, will this trend lead to an increase in useful knowledge in our field or will "neurobabble" take over and perhaps even displace sound traditional communication research? Considering the past 20 years of fMRI research may help here. The first fMRI study was published in 1992 (Kwong et al., 1992; Mather, Cacioppo, & Kanwisher, 2013). Since then, research using fMRI has exploded—in 2010 the

Correspondence should be addressed to René Weber, Department of Communication - Media Neuroscience Lab, SSMS 4405, University of California Santa Barbara, Santa Barbara, CA 93106-4020. E-mail: renew@comm.ucsb.edu

journal *Nature* counted 1,500 fMRI publications per year with an increasing trend across all empirical sciences (Smith, 2012). During its infancy, some cognitive scientists criticized fMRI research as a "new phrenology" (e.g., Uttal, 2001) and as a "fancy methodology" that failed to resolve conceptual or theoretical controversies (e.g., Page, 2006). Within the broader field of communication, many continue to raise similar concerns. However, others have demonstrated that fMRI studies provide crucial additional dependent variables that help to constrain theory testing (White & Poldrack, 2013). Greenwald (2012) analyzed 13 major theoretical controversies in cognitive and social psychology since the 1950s and concluded that only one can be counted as resolved, and that this resolution was a consequence of neuroimaging data and not based on behavioral or self-report data.

Today there is little doubt that fMRI has come a long way since the early studies of the 1990s (see Smith, 2012), and when the study is properly designed and conducted, fMRI data can provide a valuable contribution to communication theory (for more discussion on this issue, see Weber, Falk, & Eden, in press). However, the question of what exactly is a properly designed and conducted fMRI study is difficult to answer for many communication scholars, which leads to our second concern: How can our field discriminate good, relevant fMRI research from bad, irrelevant fMRI research? Is there a critical mass of qualified editors, reviewers, and scholars in our discipline to assure high-quality fMRI research in our communication journals? The answer is probably "not yet." As a consequence, communication scholars often perceive fMRI studies as too difficult, esoteric, and risky.

At the same time, editors of major communication journals are receiving an increasing number of submissions with fMRI study designs but find themselves confronted with the problem of conducting a first, informed review of submissions in order to decide whether or not to send submissions to reviewers (J. B. Walther, personal communication, May 26, 2012). After that, editors find it difficult to locate a diverse set of qualified reviewers in our field who are familiar with both the communication literature and fMRI methodology. As a consequence, editors of communication journals have repeatedly requested fMRI workshops at conferences of the International Communication Association or the preparation of guidelines by communication scholars familiar with fMRI research. This article is our response to these requests.

We realize that fMRI research can be difficult, and conducting these studies requires substantial training and experience. However, we also believe that communication researchers who are trained in social scientific methodology already have many of the skills needed to thoughtfully evaluate and critique fMRI studies, which build on core scientific principles familiar to many scholars. We hope that our article serves communication scholars as a first place to look for advanced (and not trivial) guidelines that can be readily understood with some effort. Accordingly, this article offers practical recommendations for fMRI research with the specific methodological background and research questions of communication scholars in mind.

In this spirit, we have tried to select resources and tools for acquiring fMRI skills which are, for the most part, freely available and are appropriate for scholars trained in communication programs with a social-scientific orientation. At the same time, this article cannot (and should not) replace a comprehensive textbook on fMRI methods. For readers interested in learning more, we recommend Huettel, Song, and McCarthy (2014), a well-written and easy-to-process fMRI textbook. Additionally, Poldrack, Mumford, and Nichols (2011) provide a practical introduction to fMRI data analysis, while Ashby (2011) offers a comprehensive overview of the statistical methods that underlie most fMRI studies.

With these considerations in mind, the first section of our paper discusses the most common conceptual fallacies and misguided expectations of fMRI. This section is a useful reading even for scholars with only a general interest in fMRI methodology. Then, we provide a "jump start" in fMRI for communication scholars, including concrete guidelines for reporting and evaluating an fMRI study, and conclude with an outlook for the future of fMRI in communication research. We hope to provide communication scholars some knowledge of what fMRI research entails and where to find high-quality resources for the self-education on fMRI. A summary of practical guidelines for reviewing fMRI research is also provided as a condensed checklist in the Appendix.

WHAT CAN AND CANNOT BE ACCOMPLISHED WITH FMRI?

While we are thoroughly optimistic about the ability of fMRI to provide new insights into communication, it is no panacea. Readers—and, crucially, editors and reviewers—should maintain realistic expectations and a healthy skepticism regarding fMRI studies. Of course, any such study inevitably rests on underlying philosophical premises about the nature of the mind, for instance, the belief that mental states supervene on brain states (for a strong version of the reductive physicalist view, see, e.g., Dennett, 1991; for softer alternatives to reductive physicalism, see, e.g., Davidson, 1992; Searle, 2004). While debates over the mind-body problem are both interesting and valuable, we have no reason to expect them to be resolved by any contribution we could make here. Instead, this paper focuses on the practical and methodological considerations that are crucial for conducting a sound fMRI study. Research using fMRI can fall victim to fallacious expectations, poorly-executed methods, aberrant analysis, and overstated conclusions. In this section, we first explain the conceptual capabilities and limitations of fMRI; the sections that follow will provide practical guidelines for experimental design, data collection, and analysis.

Fallacious Expectations—Questions to Address with fMRI

For the most part, fMRI studies address basic and very specific questions. A common misconception is that "looking into people's brain" will quickly provide conclusive answers for conceptual or theoretical controversies. For instance, consider the cognitive construct of *presence* (Lee, 2004) resulting from media exposure. If presence is taken to be a multi-dimensional and, in terms of mental processes, vaguely conceptualized construct, simply sliding someone in a brain imaging scanner and then expecting to see "presence" and the involved mental processes is ill-advised and unlikely to increase knowledge. Put differently, testing a theoretical prediction using fMRI requires that the concept in question has been clearly defined in such a way that observing differing patterns of brain activity would meaningfully test the prediction.

Affective, cognitive, and behavioral processes categorically involve the human brain. Thus, simply observing that the brain responds to communication task X is of no interest and a waste of resources because it tells us little about what psychological process might be occurring (Cacioppo et al., 2003). Similarly, that brain images look sophisticated or interesting is not a valid indicator of the significance of fMRI for communication theory. In fact, it has been demonstrated that using brain images in reporting research findings can be seductive and even deceiving (McCabe & Castel, 2008; Weisberg, Keil, Goodstein, Rawson, & Gray, 2008; but Farah & Hook, 2013). Falk (2012) outlines a number of specific communication research questions that can be

addressed (or enhanced) with neuroimaging methods. We recommend this reading as a source of ideas for concrete and reasonable fMRI communication studies including high-quality studies on presence (see also Falk, Cascio, & Coronel, 2015). On a more conceptual level, there are essentially three basic research questions for which fMRI can provide useful answers with the potential to advance communication theory (Aue, Lavelle, & Cacioppo, 2009; Mather, Cacioppo, & Kanwisher, 2013):

First, can a specific mental process involved in a communication phenomenon be localized to a specific brain network? While we agree with Coltheart (2013) in saying that simply localizing neural activity is in itself of little theoretical value, such brain-mapping studies are the necessary precursor for testing theory. In other fields, many foundational brain-mapping studies have already been conducted, but the cognitive processes of interest in those studies may not always be directly applicable to communication. Therefore, we believe that localization studies, which dominated neuroimaging research in its initial phase, still have their place today to provide a better groundwork for explanation and prediction of communication phenomena. For instance, if communication researchers are able to demonstrate that attitude change selectively engages specific brain systems, then we know that the mind contains a specialized and dissociable mechanism for attitude change and that brain activity in this system can be used to predict attitude change (e.g., Falk et al., 2010, 2011). This logic should not be confused with simply stating *where* activity in the brain has occurred as response to a study task and concluding that this activity is indicative for a mental process (see the section on reverse inference below). It should also be noted that state-of-the-art localization studies rarely identify brain activity in one isolated cortical structure—this is not how the brain is wired for processing information. The brain is a complex network of interconnected neurons, and as such conducting a localization study usually means that communication scholars have to identify dissociable connectivity patterns among distributed cortical networks (e.g., Ramsay, Yzer, Luciana, Vohs, & MacDonald, 2013).

Second, can a localized mental process X be found during communication task Y? For instance, to the extent it has been demonstrated that cognitive aggression selectively engages specialized brain systems, there is value in studying if markers of cognitive aggression can be found when participants play a violent video game or if other well-established mental processes are more involved in this task (Weber, Ritterfeld, & Mathiak, 2006).

Third, do different communication tasks engage distinct or common processing mechanisms? For this question, the requirement of selectivity and functional specialization is not as important as for the first two questions, because conclusions are inferred by comparing two or more brain states and not by interpreting one particular brain activity pattern. For instance, scholars interested in computer mediated communication can investigate whether processing faces of real persons engages the same mechanisms as processing faces of avatars and to what extent *realness* or *mediation* is an important factor in this task (e.g., Churches, Nicholls, Thiessen, Kohler, & Kaege, 2014; James et al., in press).

The justification for addressing any of the aforementioned research questions with fMRI is even stronger if an argument can be made that the mental processes under investigation are difficult to access via conscious reasoning, and that bypassing the conscious system when collecting data improves the accuracy of behavioral predictions (e.g., due to the lack of memory bias, socially desirable response patterns, testing effects). For instance, Falk et al. (2010, 2011; see also Weber et al., 2015) were able to show that adding fMRI signals in theoretically relevant

brain regions to traditional self-report measures significantly improved the prediction accuracy of attitude and behavior change in independent samples after exposure to health messages.

Finally, fallacious expectations can arise at a practical level. Anderson et al. (2006) summarize potential pitfalls when communication scholars are interested in using functional neuroimaging and seek opportunities for collaboration with cognitive psychologists and neuroscientists. From our own experience, it is important that communication scholars engage in collaborations with some knowledge about fMRI research, as provided by this article, and with realistic expectations. Unfortunately, finding scholars with advanced fMRI skills in other disciplines who welcome the specific training and ideas of communication scholars, or finding fMRI facilities with a research focus and with personnel who have confidence in the skillset of communication scholars, is difficult and rather the exception than the rule. Nonetheless, we do not feel that this exchange is wholly one-sided either. For instance, many fMRI studies utilize high-control stimuli (e.g., checkerboards, Stroop tasks) that are optimized to investigate tightly constrained research questions. By comparison, communication researchers have considerable expertise dealing with the complexities inherent to low-control and naturalistic stimuli (for instance, see Mathiak & Weber, 2006). These skills may be of great value to potential collaborators.

Overstated Conclusions—Subtraction, Reverse Inference, and Consistency Fallacy

Oftentimes, fMRI research reports state that communication task X (e.g. watching a persuasive message) activates brain region Y which has previously been associated with mental process Z. The important question here is: compared to what? Standard fMRI analysis follows a subtraction logic, that is, brain activation patterns are a result of comparing one or many experimental conditions with a control (also called rest or baseline) condition. But what is a meaningful baseline? Frequently, fMRI designs use an inactive baseline condition which typically comes in the form of a black screen (sometimes with a centered cross or other shape in order to focus eye gaze and to reduce head motion). The implied assumption is that during this inactive baseline brains return to a resting and inactive state. Whether this inactive state is indeed inactive and can be considered as a useful baseline has been frequently challenged (e.g., Morcom & Fletcher, 2006). An alternative, and presumably superior control condition is an active baseline. For instance, short video sequences with persuasive content could be compared to similar video sequences (e.g. in terms of average sound and brightness amplitudes) but this time with persuasive or sense-making content removed by scrambling the audio and video tracks. Conclusions that follow from analyses with subtraction logic crucially depend on the type and content of baseline conditions which must be clearly defined and included in the interpretation of fMRI results.

Furthermore, the basic subtraction paradigm must be understood as a tool for brain-mapping, not mind-reading. A typical, simple statistical analysis of fMRI data will yield contrasts that indicate which brain regions are significantly more activated by condition A than by condition B or vice-versa. Suppose that the study is exploratory and has no specific brain region of interest (ROI) about which to hypothesize. Instead, the researchers image the entire brain and observe a significant cluster of activation in brain region X. Perhaps that activation seems to make sense; prior studies have associated region X with cognitive process P, which might plausibly be engaged during condition A but not B. Frequently, the researcher will be tempted to assert that, based on their brain-imaging data, cognitive process P took place under condition A, but not condition B. However, fMRI brain-mapping data can provide only very limited support for such a claim.

The relationship between cognitive processes and brain activity is not bijective; that is, cognitive processes do not map one-to-one onto certain brain regions. Brain region X may be activated by cognitive process P, but also process Q, R, S . . . Similarly, cognitive process P might activate region X, but also regions W, Y, Z . . . The fact that the region was active does not guarantee that the purported cognitive process took place.[1]

This limitation is known as the problem of reverse inference, and it can be ameliorated in two general ways (Poldrack, 2006). The first way is within the control of the experimenter: combine brain-imaging data with more traditional measurements. Brain-imaging data can be gathered in conjunction with other measures, whether observational or self-report, which can provide further evidence to help triangulate the active psychological processes. Researchers in communication and media science should be highly familiar with the development of such measures, and brain-mapping should be seen as a complement to those tools, not a competitor. The second way to avoid the reverse-inference problem is beyond the control of any particular experiment: establishing that a certain region exhibits high selectivity can strengthen the justification for a reverse inference conclusion. Researchers have to demonstrate that certain brain regions exhibit highly-selective responses that are consistently associated with one cognitive process but not others (see above, conceptual research question one). Data-sharing and replication are crucial to establish the selectivity of a region by observing trends across numerous studies (e.g., http://neurosynth.org).

Finally, it is quite common in fMRI articles to conclude that the consistency between theoretical propositions and fMRI data demonstrate the validity of the theory. In many cases this conclusion is overstated and known as *consistency fallacy* (Mole & Klein, 2010). The observation that fMRI data is consistent with a theory cannot solely be used as evidence for the theory. In order to avoid a consistency fallacy, fMRI researchers have to demonstrate that their study could have produced some specified alternative brain activation patterns that are inconsistent with the theoretical predictions and, despite all methodologically sound effort, these brain activity patterns were not among the observed results (Coltheart, 2013).

PRACTICAL GUIDELINES FOR CONDUCTING AND CRITIQUING FMRI STUDIES

The brain is an interconnected network of neurons that manipulate electrochemical gradients. This collective system of coordinated signal processing is the measurable physical substrate of mental activity. In medicine, brain imaging might be used to diagnose some anatomical abnormality and a single highly detailed still image will suffice for this purpose. However, in functional brain imaging, researchers are interested in how a certain psychological state or behavior is related to brain activity. Because the brain is a dynamical system with distributed interactions across multiple anatomical regions, activity must be measured in both spatial and temporal dimensions.

In order to grasp the capabilities of fMRI as a measurement tool, it is first necessary to understand what fMRI actually measures. When a group of neurons fire, blood flow to that region of the brain increases in order to deliver oxygen and energy in the form of glucose. Increases in neuronal activity correspond with a rapid (0.5–2.5 seconds) increase in oxygen consumption. A corresponding increase in cerebral blood flow supplies additional oxygen, although at a slower

[1] We do believe that some informed speculation using the logic of reverse-inference may be useful in the discussion section of a paper, provided that the authors make it clear that any such conclusions are, in fact, merely speculative.

rate (peak occurs about 5-6 seconds after onset of increased demand). This increased blood flow is known as the hemodynamic response (HR). Fluctuations in blood oxygenation level alter the local magnetic properties of the active brain region. This is referred to as the blood-oxygenation-level dependent (BOLD) response (Ogawa, Lee, Kay, & Tank, 1990; for an excellent review, see Raichle & Mintun, 2006). Importantly, the amount of oxygen supplied exceeds the amount of oxygen consumed by the increased brain activity. Given that there is more oxygen supplied than consumed, increases in neural activity correspond with an increase in oxygenated hemoglobin. This difference in oxygenated (oxyHb) and deoxygenated (deoxyHb) hemoglobin is known as the BOLD contrast. In fMRI, strong static magnetic fields (typically between 1.5 and 3 tesla) are used in conjunction with varying magnetic fields (field gradients) and radio-frequency (RF) pulses to localize changes in BOLD contrast. The spatial resolution of fMRI data is typically 1–5 cubic millimeters, and the temporal resolution is about 2–3 seconds. All this requires a large, noisy, and expensive scanner in which participants lie on their backs inside a narrow bore.

It is important to point out that the BOLD signal is only an indirect measure of neural activity characterized by low signal and considerable noise. However, BOLD signals have been shown to closely correlate with local neuronal activity (Logothetis, Pauls, Augath, Trinath, & Oeltermann, 2001; Logothetis & Pfeuffer, 2004). Thus, communication studies using fMRI are based on the following logic: (1) communicative stimuli or events lead to (2) localized increases in neural activity in participants' brains which, in turn, (3) result in changes in metabolism (primarily oxygen and glucose) which (4) prompts an increase in blood flow that (5) changes the ratio between oxygenated and deoxygenated hemoglobin (BOLD), which (6) can be detected by magnetic resonance imaging techniques (DeYoe, Bandettini, Neitz, Miller, & Winans, 1994) within a magnetic resonance imaging (MRI) scanner.

With these important first considerations in mind, we now turn our attention to the practical aspects of conducting a study utilizing fMRI technology. In the following sections, we provide a general overview of the essential components of fMRI studies including model design, data collection, preprocessing, common analytic procedures, and specific methods that are particularly useful for testing communication theories. At each step along the way, three basic questions will be examined. First, what is the purpose of this step? Second, how should this step work? Third, how should this step be reported and what should reviewers look for to be sure it has been carried out in a reasonable, valid way? This three-step progression should help organize the most relevant information for different types of readers—first an overview for a general audience; second, technical details for those interested in conducting fMRI research; and finally, guidelines for readers and reviewers to assess a study's quality. This structure necessitates some overlap between each section, however we have endeavored to keep this as minimal as possible. A condensed checklist of key issues is available in the Appendix.

Experimental Design and Data Collection

What Is the Purpose?

To detect brain activity associated with a task or stimulus of interest, a sound experimental design must be used. The requirements for using appropriate stimuli and designs are higher in fMRI compared to more traditional experiments because of the aforementioned low signal, high noise characteristics of the data. Typical experimental paradigms for neuroimaging research can

be broadly classified into two types: block designs and event-related designs. In a block design, participants cycle through experimental conditions in separate periods of time. For instance, several trials of stimuli thought to activate some cognitive process of interest are presented in one block, followed by a neutral baseline period, followed by another set of treatment stimuli, and so on. Alternatively, using event-related design, different conditions can be presented in arbitrary order with a variable interstimulus interval (ISI; Dale, 1999).

Neither approach is inherently superior. A block design facilitates more convenient study design optimization, data collection, and analysis because the design is simpler. Theoretically, a block design also provides greater statistical power (more similar stimuli lead to higher signal within in a block) than an event-related one, although, depending on the nature of the study, this is not necessarily relevant in practice (Chee, Venkatraman, Westphal, & Siong, 2003). On the other hand, event-related designs allow for experiments where the sequence of events is driven by the participant rather than predefined by the researcher (e.g., labeling persuasive sequences in a message). Event-related designs should also be considered if randomizing the sequence of conditions is important, for instance, if the predictability of stimuli within a block might interfere with the process of interest through boredom, habituation, strategizing, or the like (Chee et al., 2003).

In either case, brain mapping is most effective when using high-control stimuli that are carefully calibrated to exclusively engage a certain process. This poses a major problem for most mainstream communication research both theoretically and methodologically. Theoretically, communication models tend to focus on high-level psychological processes of self-referencing, perspective-taking, social learning, cognitive dissonance, or any number of other multifaceted constructs. Isolating this sort of construct into specific cognitive processes is inherently challenging (see our first section in this article). Methodologically, communication research frequently uses stimuli that offer comparatively low control. For instance, suppose a researcher wants to compare the persuasiveness of two different public service announcements. The messages might vary in dimensions that are relevant for persuasion, the use of scientific evidence versus individual testimonials, for instance, but are likely to also vary in many other ways that could yield extraneous brain activity and should be controlled for, like the amount of motion or luminosity. Effective fMRI for higher-order processes requires carefully controlling for unrelated lower-order processes induced by the experiment itself.

How Does It Work?

Once the experimental procedure has been thoroughly prepared, data collection can begin. An experimental session usually begins with a series of so-called structural scans to calibrate the scanner and generate a static but high-resolution anatomical image of the participant's brain. Then, during so-called functional scans, three-dimensional scans are taken at regular intervals with a period defined by the repetition time (TR) and converted into an image. Each three-dimensional image is referred to as a volume. A volume is composed of thousands of voxels (volume-elements, the 3D equivalent of a pixel), its atomic components, which represent activation at a point in space.

When conducting studies with a scanner that is primarily utilized for functional imaging, the scanner technician will be able to recommend settings that meet the needs of your study. For instance, a typical TR for functional neuroimaging is 1–3 seconds, although TRs of less than

1 second are possible today. If a study design necessitates alternate settings, a good scanner technician should be able to inform researchers about the best approach to achieve particular research objectives.

What Should Be Reported and What Can Go Wrong?

Poldrack et al. (2008) offer a detailed explanation of some common issues associated with reporting fMRI data. However, this and other resources (e.g., the fMRI methods wiki, http://www.fmrimethods.org/) are targeted toward readers who are already quite familiar with conducting research using brain imaging techniques. Given that the use of fMRI in communication research is a more recent development, it may be useful to expand on several issues that are treated as tacit knowledge in other resources.

Participants. Communication scholars are already quite familiar with reporting information about subjects and sample size. In addition to demographic information, brain imaging studies must also disclose information about a sample that may not be immediately obvious. For instance, the inclusion of both right- and left-handed subjects in a study raises potential issues as hand movement (e.g., a button press) is associated with contralateral neural activation. Presenting another challenge, individual differences in cognitive style may result in differential activation patterns (Miller et al., 2009; Miller, Donovan, Bennett, Aminoff, & Mayer, 2012). Therefore individual characteristics such as subject handedness, gender, and cognitive style should either be standardized as best as possible across subjects, modeled as a covariate, or used as a variable for examining group differences. As a final consideration, while many communication studies may have large samples that seek to approximate a given population, the practicalities of conducting fMRI studies (e.g., cost and time) often constrain sample size. Thus, it is common to see brain imaging studies with a small number of participants that do not necessarily reflect a larger population. Meticulously specifying subject inclusion and exclusion criteria helps inform the sort of inferences that might be made about a given sample in an fMRI study. Despite small sample sizes, however, researchers have developed procedures that maximize statistical power and efficiency in fMRI experiments (Liu & Frank, 2004; Liu, 2004).

Experimental Paradigm. Once the sample has been described, researchers must specify the nature of the experimental paradigm. For studies that utilize a block design, researchers should report the number and duration of all blocks and periods of rest within a given procedure. In addition, researchers should specify the psychological process each block sought to elicit as well as how periods of rest were defined (e.g., closed eyes, black screen, black screen with a fixation point). Answering these questions should include detailed information about the stimuli utilized, behavioral measures, and the extent to which stimuli were repeated. Studies that employ an event-related design should include similar information about the nature and meaning of stimuli and elicited behavior. These studies must also explain how events are conceptually and operationally defined as well as the duration of each event and the ISI. In some less common cases, studies may combine both block and event-related designs, which requires reporting all of the above information.

Studies that utilize block or event-related designs rely on an analytical logic where multiple trials are aggregated in order to achieve sufficient statistical power. In some designs, researchers may choose to expose subjects to all trials all in one, uninterrupted sequence (called a “run”

in fMRI experiments). However, this approach has two potential drawbacks. First, long runs consisting of multiple trials may unnecessarily fatigue subjects. Second, the fMRI scanner heats up over time and this heat creates magnetic field distortions that add additional noise to data. In order to avoid these complications, subjects are often exposed to trials across a number of runs. When multiple runs are used in a study, researchers must report the number of runs, the duration of each run, and what trials occurred in what run.

fMRI Data Acquisition. Although, as mentioned above, a skilled technician should be trusted to select appropriate scanner settings, it is relevant to report information about the scanner settings as this provides readers with an understanding of what sort of machine the data were collected with and a variety of information that assists in interpreting study design, data analysis, and results. For instance, the TR between full brain scans provides readers with information about spatial and temporal tradeoffs and the number of scans per stimulus presentation. For example, if relevant stimulus features (e.g., shooting vs. non shooting when playing a violent video game) vary at a higher frequency than the scanner is recording data (e.g., shooting every 1s, but scanner TR is 2s), then individual shooting events cannot be resolved with this fMRI sequence.

Other scanner settings may have less impact on the way in which a reader might interpret the results of a given study but are still relevant for replication purposes. Therefore, a manuscript should report the following information: scanner manufacturer and model, magnetic field strength, TR, echo time (TE; the time between a RF pulse and magnetic resonance signal sampling), flip angle (the degree to which a RF pulse knocks atomic nuclei off the axis of equilibrium), number of slices, slice thickness, gap between slices, slice order, field of view (FOV; the physical size of an image, reported in cm^2), and matrix size (FOV for a given slice, reported as a grid). Communication scholars can simply ask the fMRI technician for a printout of these scanner parameters. For a more detailed overview of these settings and their implications, see chapter two in Lazar (2008).

Preprocessing

What Is the Purpose?

A series of image preprocessing steps must be carried out prior to statistical analysis (see Strother, 2006, for an overview). In order to meaningfully aggregate and compare fMRI data, the brain must have the same alignment and internal structure in each volume to be analyzed. Furthermore, some basic transformations are needed to account for quirks in how the scanner collects the images, and filters can be applied to improve the ratio of signal to noise in the data, easing subsequent analysis. Commonly referred to as a "preprocessing pipeline", this procedure consists of any data treatment that does not depend on specific hypotheses. For a more detailed overview of the statistical underpinnings of these preprocessing steps, we direct interested readers to Ashby (2011, chap. 4).

How Does It Work?

In what follows, we describe a preprocessing pipeline that may be well suited for many communication studies. Of course, no single solution will apply to all studies, and these steps may

become outdated as newer methods emerge. However, as of this writing, these steps represent a benchmark by which fMRI studies in communication research should be evaluated.

Basic Quality Control. As an initial step, common-sense data quality control should be applied – checking the images for major acquisition artifacts (e.g., thin bands of light and dark across the entire image) and excluding participants with obviously questionable data (e.g., those who fell asleep or disengaged mid-session, which is not uncommon). Additionally, image orientation should be checked and corrected if necessary to match standard convention, and brain extraction should be performed to strip out the skull, dura, and other extraneous non-brain tissue.

Slice-Timing Correction. Another fundamental step in preprocessing is slice-timing correction (Sladky et al., 2011). While each volume is considered to represent a single time point, the entire volume is not captured instantaneously by the scanner. Rather, data are collected as a set of individual two-dimensional slices during a TR, which are then assembled into the full three-dimensional volume for that TR. Slice-timing correction should be applied to account for small differences in the timing of slice acquisition. The details of how slices are acquired depend on the configuration of the scanner. For instance, slices might be gathered consecutively from 1 to n, or in an interleaved pattern where all odd-numbered slices are collected, then all even-numbered ones. Suppose that a particular scanner takes 32 slices per TR and that a subject is exposed to a stimulus at the onset of a given TR. We know that the hemodynamic response is slow, therefore the BOLD response in once slice may differ from BOLD responses in other slices within the same TR as well as across multiple TRs. Slice timing correction works to correct this inconsistency by making assumptions as to what a BOLD response should be for a given slice within a given TR.

Motion Correction and Standardization. All the brain images must all be aligned to a common space so that specific regions of that space can be compared. However, several mathematical transformations are necessary to accomplish this. Head motion inside the scanner is inevitable, so a motion-correction procedure must be applied to keep head alignment consistent throughout the time-series of volumes for a given participant. Differences in overall mean signal intensity between volumes would be problematic for analysis as well, so signal intensity standardization is usually applied to maintain a consistent mean.

Registration

A two-step registration procedure is commonly employed to maintain a consistent space for analysis. First, a participant's functional images are aligned with the high-resolution anatomical scan collected at the beginning of the scanning session. Functional scans provide useful information about neural activity but poor information about neural structure. This can make it difficult to identify the exact structural location of neural activity identified during functional scanning. To solve this issue, the high-resolution structural scans must be coregistered to the comparatively low resolution functional scans. Since voxel sizes and imaging parameters differ between both scans, modern automated algorithms apply a series of transformations to maximize image-intensity correlations between the two images. The boundary-based registration (BBR) procedure (Greve & Fischl, 2009) implemented in most fMRI analysis software packages (see below) is a good coregistration solution. Second, because the precise size and shape of brain regions vary

substantially across individuals, the images are then registered with a nonlinear transformation to match a "standard brain," such as the common MNI152 template (http://www.bic.mni.mcgill.ca/ServicesAtlases/HomePage). This process is called *normalization*.

Spatial and Temporal Filtering. Spatial and temporal filters are applied to improve signal quality in the data. The BOLD signal in one voxel may differ substantially from BOLD signal in neighboring voxels. Such differences introduce noise into the data and violate assumptions about the normal distribution of signal and noise common to statistical tests employed in most brain imaging analyses. Spatial smoothing is a process for dealing with these issues. For instance, a spatial filter can be applied to smooth areas of activation by taking a weighted average of intensity in a neighborhood of voxels of a certain size, commonly 5mm or 7mm.

Temporal filtering is used to remove noise from sources unrelated to the process of interest. The period of the signal induced by communication science studies is typically on the order of many seconds—if the stimulus changes every 15 seconds, for example, the response signal should have an expected frequency of approximately 0.067Hz. Much lower frequencies will contribute noise and should be filtered out. For instance, physiological changes in the body as well as thermal and magnetic changes in the scanner produce low-frequency noise. This noise is typically less than 0.015Hz (Smith et al., 1999), including steady (0Hz) linear drift, and can be removed by applying a high-pass temporal filter.

What Should Be Reported And What Can Go Wrong?

Generally speaking, preprocessing steps will remain the same between experiments (Ashby, 2011) and will vary slightly among different research labs and scanner configurations. Researchers should first specify what software package was used to preprocess fMRI data. Two common choices are the freely available Oxford Center for Functional MRI of the Brain (FMRIB) Software Library (FSL; http://www.fmrib.ox.ac.uk/fsl) and the Wellcome Trust Centre for Neuroimaging Statistical Parametric Mapping software package (SPM; http://www.fil.ion.ucl.ac.uk/spm/); however, other packages such as AFNI (National Institute of Mental Health; http://afni.nimh.nih.gov/afni/) and BrainVoyager (Brain Innovation; http://www.brainvoyager.com/) are also available.

These software packages may be used in a variety of combinations to perform the most common preprocessing steps outlined above. Each step ought to be reported in an fMRI study, but this description might consist simply of mentioning that the step was carried out using default settings in a certain software package. Nonetheless, some specific configurations and their implications are important to mention here.

Slice-Timing Correction. Slice-timing correction can be accomplished by a variety of interpolation methods. Sinc interpolation is a popular choice, implemented by default in both SPM and FSL, as it also functions as an effective low-pass filter. Slice-timing correction can also be omitted during preprocessing and instead occur during statistical analysis in the form of a temporal derivative of an estimated hemodynamic response function. This process adds weighted regressors to a design matrix that shift the predicted BOLD response forward or backward in time to account for the order in which slices were collected. Slice-timing correction is crucial for studies that employ an event-related design where subjects rapidly shift between tasks. Without

correction, these rapid shifts complicate estimates of the hemodynamic response. By comparison, slice timing is less critical for block designs with a slow time-course separated by periods of rest.

Head Motion Correction

Head movement can dramatically impact the strength of a BOLD signal when blood saturated brain regions are shifted outside of the brain or to a location previously occupied by bone or air (Huttel et al., 2014). Fortunately, SPM, FSL, and MATLAB have procedures that attempt to resolve head motion issues by realigning each volume along the x, y, and z, axis. Researchers should report what algorithms were used to correct for head motion as well as instances where particular volumes were omitted due to excessive head motion that could not otherwise be corrected.

Registration

Analysis often requires that brains are not only rotated, but warped to fit a standard size and shape. Linear transformations represent a useful first-step in accomplishing this goal but the process of registering an individual brain to a standard template benefits from the use of nonlinear transformations. Generally speaking, the default settings are sufficient to carry out these procedures.[2]

Spatial Smoothing. There are a handful of approaches for dealing with spatial smoothing but the most common kernel utilizes a full width at half maximum (FWHM) Gaussian function. Determining the correct size of a spatial smoothing kernel presents a challenge to researchers. Applying too large a kernel may combine distinct signals thus limiting the ability to detect unique activation in neighboring structures. Choosing too large a kernel can also increase noise in the data. If one voxel containing task relevant signal is surrounded by others containing only noise, a large kernel will essentially replace signal in the voxel of interest with noise. Similarly, studies interested in examining very small brain structures must select a kernel small enough to distinguish activation in these structures. On the other hand, choosing too small a kernel may insufficiently reduce noise, thereby suppressing task related activation. Absent any special considerations, Ashby (2011) recommends choosing a kernel that is 1–3 voxel widths in size. In many of today's scanners, this is a 7 mm FWHM kernel.

Temporal Filtering. Recall that temporal filtering accounts for small low-frequency noise that may result from gradual changes in the scanner's magnetic field (Smith et al., 1999). When applying a temporal filter, researchers must carefully specify what frequency ranges are filtered out. For block designs, a general rule of thumb (see Ashby, 2011) is that a temporal filter should account for twice the period of the task plus rest. So, a block design with a 15-second task followed by 15 seconds of rest should filter out frequencies longer than one minute (0.0167Hz). A shorter filter risks excluding task-related data. This issue is less problematic in event related designs where periods of task and rest shift rapidly.

[2]Klein et al. (2009) conducted an analysis of normalization algorithms. While the normalization procedures utilized in FSL are outclassed in accuracy by other algorithms, they should suffice for typical studies in communication research.

Other Preprocessing Steps. There are several other steps that can be included a part of a preprocessing pipeline. For instance, in addition to temporal filtering, magnetic field inhomogeneities resulting from pockets of air in subjects (e.g., sinus cavities) and scanner drift (Smith et al., 1999) can be corrected using a B0 unwarping procedure. In this process gradient field maps are captured at various time points during a scanning sequence, and these field maps are then used to correct for magnetic field distortions. While this may be a crucial step for data collected on older scanners, the increased number of steps and computational time may not be justified on more modern systems.

Grand-mean scaling is another process that normalizes the overall mean BOLD response across all scanning sessions. This process corrects for session-by-session inhomogeneities in magnetic field strength as well as differences individual differences cerebral blood flow, thereby improving subsequent combination of results across sessions and subjects. Grand-mean scaling is implemented by default in both SPM and FSL. Whereas grand-mean scaling should be carried out in most communication studies, other approaches such as *global normalization* procedures should be avoided as they have been shown to bias experimental results in fMRI studies (Gavrilescu et al., 2002; Murphy, Birn, Handwerker, Jones, & Bandettini, 2009). Therefore disabling grand-mean scaling or applying global normalization techniques in fMRI studies requires additional justification.

Basic Data Analysis

What Is the Purpose?

Once preprocessing is complete, researchers can turn their attention to data analyses designed to test study hypotheses. Just as with any other study, the raw data of fMRI do not speak for themselves—they must be interpreted by applying an analytical procedure. Under the traditional paradigm of fMRI, brain activity is contrasted between conditions using straightforward subtraction logic: the process of interest is described by the difference in activation between treatment and control conditions. A General Linear Model (GLM) regression analysis is applied to model and assess the statistical significance of those differences. The researcher creates a standard GLM design matrix which models the timing of the stimulus, and that model is used to generate a set of parameter estimates (PEs) to predict BOLD signal change (brain response).

Broadly speaking, fMRI analysis typically adopts either a *whole-brain* approach or a *region of interest (ROI)* approach. On one hand, whole-brain analyses attempt to identify the specific brain regions activated by a given stimulus from among all possible regions across the entire brain. The corresponding statistical technique has been termed statistical parametric mapping (SPM). SPM is mostly an explorative procedure; it is performed without prior hypotheses about likely brain activity patterns. On the other hand, a ROI analysis usually relies on theoretically or empirically derived *a priori* assumptions about what brain structures should be associated with the process at hand. Having a well-defined ROI reduces the number of voxels or brain areas to be analyzed and provide a stronger theoretical basis than exploratory whole-brain analysis.

There are two major procedures to select ROIs. First, specific anatomical ROIs can be identified based on the findings of previous brain imaging studies or meta-analyses that summarize brain activity patterns from other studies using similar stimuli (e.g. activation in response to a cognitive control task; see http://neurosynth.org for a database of brain imaging meta-analyses).

Second, functional ROIs can be identified within the same study (see Berkman & Falk, 2013). Suppose a communication researcher wants to examine whether playing a racing video game has an effect on cognitive control. In this scenario the researcher can expose participants to two independent tasks or stimuli within the same study: one to localize the relevant functional ROI with an independent, well-established go/no-go *localizer task,* and another that contains the concept of interest (playing a racing video game). Subsequently, time-series analyses can test the hypothesis whether playing a racing game causes changes in the independently localized region that processes cognitive control.

How Does It Work?

In fMRI research, data analysis is often broken down into a series of smaller analyses. Data for individual subjects are first fitted to a model in a first-level analysis. Subsequent higher-level analyses may combine data for the subjects across multiple runs or combine data for multiple subjects within the same group. The first step involves modeling preprocessed data for an individual subject. In studies where subjects are exposed to just one run, there is just one first-level analysis per subject. Conversely, in studies where subjects are exposed multiple runs, there are multiple first-level analyses per subject. But what exactly is modeled?

The hemodynamic response and corresponding changes in BOLD signal should increase in task sensitive voxels during periods of task compared with periods of rest. Therefore a simple hemodynamic response estimate might look like a boxcar; elevated and sustained activation during task followed by lesser activation during periods of rest. Such a model predicts task relevant increases in activation that start immediately during a stimulus block and end immediately during periods of rest. In actuality, the hemodynamic response is delayed; it can take up to six seconds before a region reaches full oxygen saturation. Moreover, the hemodynamic response tapers off over time. Accordingly, hemodynamic response models are made more accurate when a boxcar function is convolved with a hemodynamic response function (HRF) that accounts for delayed changes in blood oxygen saturation levels.

These predicted responses are modeled as regressors in a standard GLM. In fMRI analysis these regressors are commonly referred to as explanatory variables (EVs). If a study has two experimental conditions, each condition may be modeled as an EV. These EVs, as well as any other variable a researcher want to account for (e.g., confounding variables, corrections for typical fMRI noise sources such as scanner drift) comprise the design matrix of a basic GLM. To provide a basic example, consider the process for conducting a simple GLM analysis using, for instance, the popular fMRI analysis package FSL. Imagine a model using a single factor with two levels. This model yields a design matrix with two EVs and as many rows as there are volumes (or time points) in the data. Each row of the design matrix encodes the factor level at the time that volume was collected: [1 0] for condition A, [0 1] for condition B. Significance testing is then carried out in accordance with some defined contrasts of parameter estimates (COPEs). Each contrast is a set of weights to be applied to the EVs of the design matrix. In our example, a [1 -1] contrast corresponds to the difference in activation when subtracting condition B from condition A, that is, what brain regions are more active during condition A than condition B. The output of this procedure is a map of t-statistics, which can be transformed to z-statistics (z-map) and then thresholded at a certain significance level to identify voxels with statistically significant

activation. Those areas of activation will ostensibly indicate the brain regions associated with condition A but not condition B.

What Should Be Reported and What Can Go Wrong?

When reporting a standard GLM analysis, researchers should report information about first- and higher-level analyses. These aid readers in understanding how models were calculated, how contrasts are calculated, and how relevant parameters were estimated, both for whole-brain as well as ROI analysis.

First-Level Analysis. Data for each run for each subject should be fitted to a GLM that at a minimum includes EVs, their temporal derivatives (to account for response latency), confound variables (head motion, covariates), and an error term. Contrasts must also be specified in a first-level analysis.

Each EV must be convolved with an HRF. The simplest strategy models an estimation of what a hemodynamic response might look like (e.g., a gamma function). However, this approach may rest on shaky assumptions. Different brain regions (Schacter, Buckner, Koutstaal, Dale, & Rosen, 1997) and subjects (Aguirre, Zarahn, & D'esposito, 1998) may have different hemodynamic responses to the same stimulus. These differences add noise to the data that may: (1) reduce the ability to detect task related activation, and (2) pose challenges for studies attempting to discriminate which neural structures are associated with a given mental process (Ashby, 2011). Nevertheless, this is among the most commonly applied approaches and is most likely suitable for block designs. Event-related designs may consider a more sophisticated strategy. Flexible HRF models apply several free parameters to HRF estimation in an attempt to correct for different hemodynamic responses across brain regions and individual subjects.

An additional concern that must be overcome in a first-level analysis is autocorrelation within the time-series data. The state of the brain does not reset from one TR to the next; BOLD signal is correlated with itself over time. However, accurate parameter estimation requires that the error term in the correctly-specified GLM contains "white," or random, noise. There are two basic approaches to handling this problem: precoloring and prewhitening. Precoloring uses low-pass temporal filtering to induce known autocorrelation that will overwhelm other noise. While the result is unbiased, it is necessarily inefficient (Woolrich, Ripley, Brady, & Smith, 2001). The alternative, prewhitening, attempts to estimate and then remove the autocorrelation from the data. While accurate estimation of autocorrelation was once seen as somewhat problematic, well-tuned estimation techniques have made prewhitening a widely-used solution to correct for autocorrelation (Woolrich et al., 2001).

Higher-Level (Group) Analysis. Once data for each participant are fitted to a GLM, these individual first-level analyses must be combined in higher-level group analyses. There are several statistical procedures for combining data in higher-level analyses. Generally speaking, analyses that rely on a mixed effects approach are more readily generalizable than those that rely on a fixed effects procedure (Beckmann & Smith, 2004; Woolrich, Behrens, Beckmann, Jenkinson, & Smith, 2004; Woolrich, 2008).

Additional EVs and contrasts may be specified at this stage. For instance, researchers testing group differences may choose to model each group as an EV and then contrast one group against

another. This contrast between groups is a crucial step because it avoids a common statistical fallacy. Suppose that group A shows one activation pattern to a given stimulus and group B shows a different activation pattern to the same stimulus. While the activation patterns are visually different, they are not statistically different as they were never directly compared. These differences in activation between groups can occur for a variety of reasons. Contrasting activation associated with group A from that associated with group B is the only way to tell if one group is statistically different from another.

Multiple Testing. The simplest formulation of the GLM-based significance test contains a crucial flaw which must be avoided. Each fMRI data volume has a huge number of voxels, and therefore the significance threshold must be adjusted to account for multiple comparisons: with around 100,000 voxels being tested, an uncorrected p-value threshold of 0.05 is essentially meaningless, since thousands of voxels will appear to be significant purely by chance. To demonstrate the extent of this problem, a highly entertaining study from Bennet, Baird, Miller, and Wolford (2010) imaged a dead salmon's brain during an "open-ended mentalizing task" and found an 81mm^3 area of (obviously spurious) activation with a significance level of $p < 0.001$. After correcting for multiple testing using family-wise error rate (FWER) based on Gaussian random field (GRF) theory, they found no results. GRF-based correction, particularly cluster-based thresholding, has become standard, offering a less-conservative alternative to simple Bonferroni correction (Smith & Nichols, 2009). Because the GRF approach utilizes resolution elements (resels) that depend on the smoothness of the image, applying an appropriate spatial filter during preprocessing is especially important when using this method of correction.

Whole Brain and ROI Analysis. Whole brain analyses often report neural activation that survives statistical thresholding after correction for multiple comparisons. These activations occur in so-called *clusters*. Clusters represent the location of statistically significant neural activity associated with a specific contrast. The anatomical basis of these clusters is then determined by cross-referencing either the maximum z-statistic or center of gravity coordinates with an atlas. Importantly, this only works when results have been normalized to a standard brain template (e.g., MNI152).

Researchers who conduct ROI analyses are often interested in understanding signal changes within a given ROI. Both FSL and SPM have procedures for calculating signal change within a ROI. These signal change estimates are quite small; a change in signal of just 1.5% is generally considered a large change. While signal change is easily calculated for block designs, it is more difficult to calculate in event-related designs. Ashby (2011) offers a more detailed discussion of approaches for calculating percent signal change for event-related designs.

Visualizations. Visual presentations of fMRI results can help readers understand the location and intensity of neural activation. The interpretability of these figures and tables is greatly enhanced by including a few critical pieces of information. When reporting activation tables, authors should include information about the cluster size, coordinates of maximum z-statistic (Max-Z) and/or center of gravity (COG), structure at Max-Z or COG, and specify what atlas was used. Figures that visualize neural activation should include information to orient readers (e.g., sagittal, coronal, and axial views). When significant clusters are overlaid on a standard brain template, researchers should specify the statistical threshold and multiple comparison correction applied as well as the normalized coordinate space (most likely MNI152). The BrainNet (Xia,

Wang, & He, 2013) visualization tool is free and useful for demonstrating these results. If sufficient space is available, researchers may also choose to include a figure detailing the experimental procedure. This visualization should include information about the order and duration of periods of task and rest. Finally, fMRI studies often contain more information than can be reported in the standard journal article. These data should be included as supplementary data, if the journal allows for it.

Voodoo Correlations. One issue in fMRI data analysis that readers should be aware of is so-called "voodoo" correlation (Vul, Harris, Winkielman, & Pashler 2009a, 2009b). Vul et al.'s controversial paper demonstrated that studies correlating neural activation with nonindependent behavioral measures are at risk of reporting inflated or possibly even spurious results (but see Lieberman, Berkman, & Wagner, 2009). Small sample sizes may also account for these inflated correlations (Yakoni, 2009). Even more troubling, the effects identified in small sample studies may under represent the true effects present in a population and give the false impression that a neural region is highly selective for a specific cognitive process. In this context, it has been suggested that a sample size of 50 or more may be necessary for studies seeking to test individual differences in neural activation. Given that a standard fMRI study in communication needs an average of about $1,000 per participant, a budget of up to $50,000 would be needed for just using the MRI facility to meet this requirement.

Advanced Data Analysis

While the GLM-based subtraction logic has provided many important insights and is oftentimes the most sensible approach, we would be remiss to neglect the many alternative approaches, some of which are of particular importance for communication science. In this section, we provide a brief overview of alternatives to the GLM approach. To introduce these alternatives, a distinction should be made between models that *encode* or *decode* brain states (Naselaris, Kay, Nishimoto, & Gallant, 2011). We explained that brain mapping through techniques like SPM do not constitute mind-reading: experimenters present a stimulus designed to induce a given mental state and then observe brain activity that *encodes* that state. In other words, this approach can predict brain states based on mental states, but cannot predict mental states based on brain states. Only recently have researchers begun to reverse the procedure – using brain activity to *decode* mental states.

The most well-known technique for brain decoding is multi-voxel pattern analysis (MVPA). In contrast to the traditional voxel-by-voxel significance-testing approach explained above, MVPA searches for response patterns that span multiple voxels, training a statistical classifier that is sensitive to distributed patterns that represent information in the brain (Norman, Polyn, Detre, & Haxby, 2006). One common technique to develop MVPA is the use of searchlight algorithms, which look for spheres of activation around each voxel (Etzel, Zack, & Braver, 2013). In the simplest case, the MVPA approach first builds a model of how a certain stimulus is encoded in the brain then uses that model to discriminate between different stimulus types. Even more sophisticated implementations are concerned with not only simple classification but full reconstruction – for instance, predicting the details of a natural image participants looked at based on their brain activity, even if the model was not trained using that specific image.

The Gallant lab at UC Berkeley has conducted a program of research that utilizes decoding extensively and to great effect (Huth, Nishimoto, Vu, & Gallant, 2012; Naselaris, Prenger, Kay, Oliver, & Gallant, 2009; Naeselaris et al., 2011), including predicting visual features of movies from brain activity with surprising fidelity (Nishimoto et al., 2011). For instance, using a collection of natural images collected from the Internet, they produced a Bayesian reconstruction algorithm that selects the known image which is most structurally and semantically similar to a randomly-chosen image presented to a participant in an fMRI scanner. Similarly, Haynes and colleagues have conducted decoding studies focused around free-will and hidden intentions, using brain activity to predict attentional salience and decision-making behavior (Bogler, Bode, & Haynes, 2011; Chen et al., 2010; Haynes et al., 2007; Soon, Brass, Heinze, & Haynes, 2008). These results demonstrate that, by working beyond the subtraction paradigm, "mind-reading" studies are now an extant, albeit nascent, area of research. At this point, we can only speculate about the scientific and ethical implications of this innovative work for core research areas in communication science that investigate deception detection and persuasion.

Another important emerging method in fMRI analysis is inter-subject correlation (ISC) analysis, which measures between-subject voxel-wise correlations in the BOLD signal time-series (Pajula, Kauppi, & Tohka, 2012). Like the GLM, ISC can be used to study encoding processes, but from a different perspective. Whereas standard GLM analysis builds a model using a design matrix to define the time-course of stimuli, ISC simply looks at commonalities in hemodynamic response across individuals exposed to the same stimulus. This makes ISC especially well-suited for the low-control stimuli commonly used in communication research, for which it is difficult to define a precise *a priori* model of brain responses. Furthermore, a broad framework for understanding communication as interbrain coupling has emerged in recent years, most notably in the work of Hasson and colleagues (Hasson et al., 2004; Hasson et al., 2008; Hasson et al., 2012; Hasson, Ghazanfar, Galantucci, Garrod, & Keysers, 2012). According to this view, communication selectively aligns brain states across individuals, so ISC analysis is a critical tool for neuroimaging analysis in communication (e.g. Hasson et al., 2008; Stephens, Silbert, & Hasson, 2010; see also Weber, Eden, & Mathiak, 2011). Hyperscanning techniques (Montague et al., 2002), wherein multiple participants simultaneously undergo fMRI scanning while interacting with each other, present additional exciting opportunities for communication researchers to extend this line of research.

It is also worth noting another technique conceptually similar to ISC, functional connectivity analysis, which compares time-series correlations in patterns of neural activation *within* individuals (Friston, 1994). Connectivity between multiple brain regions is the norm in higher-order cognitive processes. Functional connectivity analysis can be used to study how the brain integrates information, while the standard subtraction logic focuses on how the brain segregates information.

Finally, the application of a brain-as-predictor approach (see Falk, Cascio, & Coronel, 2015, for more procedural details and specific examples) has yielded impressive results in studies of media persuasion. In one notable example, the application of neuroimaging using a brain-as-predictor approach doubled the explained variance in real-world health behavior compared to self-report measures (Falk et al, 2011). Similarly, a recent study from our lab found that while conventional non-neuronal measures could not significantly predict the persuasiveness of anti-drug ads for high-risk targets, a brain-as-predictor model could (Weber et al., 2015).

CONCLUSIONS AND OUTLOOK

Methods specific to fMRI data collection and analysis are in a continuous state of development. In this exciting time, no single paper can capture the expansive nature of fMRI research, nor can any paper hope to foresee important changes in methodological possibilities and best-practices. Instead, our primary goal for this paper is to provide communication scholars with reasonably advanced guidelines for understanding and critiquing studies that utilize fMRI. We urge communication scholars to extend the ideas discussed herein and draw upon existing expertise to contribute methodological advances to brain imaging research.

Moreover, we hope that this article motivates and enables communication scholars to engage in the first steps towards designing and conducting an fMRI study. We realize that for many communication scholars fMRI data collection may not seem to be within their reach, but with the advent of publicly available brain imaging data repositories ("Focus on Big Data," 2014; Gomez-Marin, Paton, Kampff, Costa, & Mainen, 2014; Turner, Eickhoff, & Nichols, 2014) it is not unlikely that communication scholars will soon be able to analyze existing fMRI datasets and take advantage of this new methodology for addressing original communication research questions.

More broadly, we hope that this paper demonstrates that the ability to conduct brain imaging research using fMRI is attainable for communication scholars. Whereas communication scholars began to insource physiological research in the past few decades, we hope that these next few decades are ones where our discipline begins to insource and innovate various brain imaging techniques, fMRI included. As shown in this article, fMRI studies cannot resolve all communication questions. Yet, looking at recent research in communication and media neuroscience cited in this article as examples, we are confident that this methodological tool provides a new avenue for testing and refining communication theory. We hope that our fellow researchers read this article as an invitation to engage and collaborate in fMRI research.

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APPENDIX A
Checklist for the Evaluation of fMRI Studies in Communication

Evaluation Criteria	☑
Required Content	
Rationale	
Uses fMRI to advance communication theory and research (p. 7)	☐
Uses fMRI to answer appropriate research question(s); *select at least one* (p. 8)	
Is a mental process involved in communication localized to a specific network and is there a strong justification for the relevance of a localization study?	☐
Is activation for a previously localized mental process X associated with communication task Y?	☐
Do different communication tasks engage the same/different neural systems?	☐
Justifies any of the above by demonstrating that fMRI overcomes limitations inherent to other methodological approaches (not strictly required but would strengthens the rationale)	☐
Procedure is reported with sufficient detail to allow for replication	
Reports participant demographics (p. 13)	☐
Uses an adequate sample size (p. 13)	☐
Includes details on scanner configuration (p. 14)	☐
Pre-processing steps are well specified and include	
Slice-timing correction (p. 15–16)	☐
Head motion correction (p. 15, 17)	☐
Coregistration and normalization procedures (p. 15–17)	☐
Spatial smoothing (p. 16–17)	☐
Temporal filtering (p. 16–17)	☐

(*Continued*)

APPENDIX A
(Continued)

Required Content	
Main analysis includes details regarding	
Design matrix for first- and higher-level analyses, including EVs (p. 19–20)	☐
Type of inference for group-level analyses (p. 20–21)	☐
Correction for multiple comparisons (p. 21)	☐
(If applicable) How ROIs are identified and defined (p. 18–19)	☐
(If applicable) How signal change parameter estimates are calculated (p. 21)	☐
Results & interpretation	
Contrasts are well defined and easily understood (p. 19–21)	☐
If the study makes a reverse inference claim; *select at least one* (p. 8–10)	
Demonstrates that the ROI is highly-selective for a specific mental process	☐
A cautious interpretation of results is recommended	☐
If the study reports group differences in neural activation; *select at least one* (p. 20–21)	
Differences between groups are statistically tested	☐
A cautious interpretation of results is recommended	☐
Avoids common fallacies	
Consistency fallacy (p. 9–10)	☐
Mind reading (controversial, may be appropriate) (p. 9, 22–23)	☐
Integrates results into the larger body of Communication theory and research (p. 7–8)	☐
Additional Recommended Content	
Provides a visual representation of study design including order of stimuli (p. 22)	☐
Provides activation tables showing detailed cluster information including: coordinates of Max-Z/COG, structure at Max-Z/COG, Max-Z statistic, cluster size, and specification of atlas used (p. 21–22)	☐
Provides activation maps which include statistical thresholds (p. 19)	☐
Discusses the inherent limits of fMRI and considers or incorporates non-fMRI data (p. 9–10)	☐
Makes supplemental study materials available to other researchers (p. 22)	☐
Makes pre-processed and/or raw fMRI data available to other researchers (p. 22)	☐

Note. This checklist is designed to assist authors, reviewers and editors in the process of reporting and evaluating an fMRI study. No checklist can include an exhaustive list of requirements for every study and not every requirement on this checklist may be necessary for all fMRI studies. Therefore, we invite fellow researchers to extend or modify our checklist. With this in mind, studies that do not include one or two of the requirements should not necessarily be viewed as invalid or otherwise flawed. Instead, missing requirements should prompt requests for clarification, additional analysis, further justification, or the cautious interpretation of results.