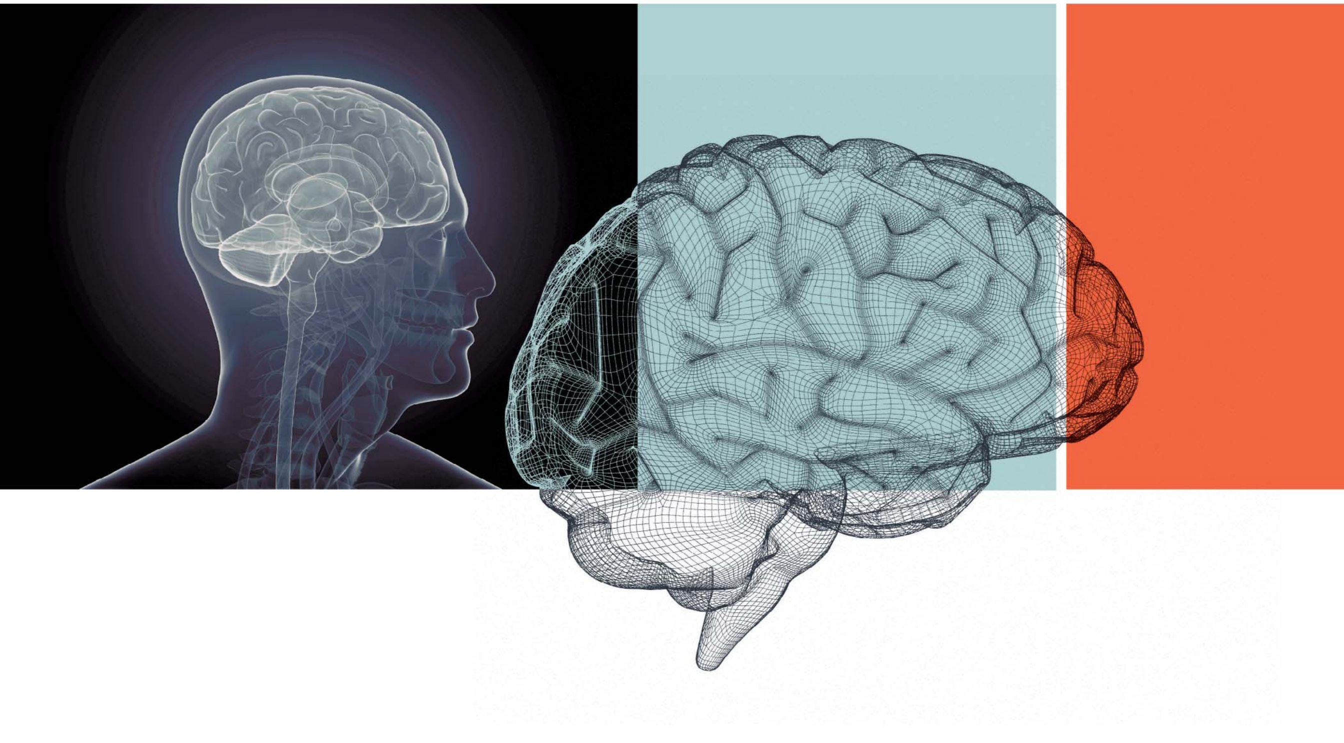
WILLIAM R. UTTAL

Mind and Brain

A Critical Appraisal of Cognitive Neuroscience



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Mind and Brain

A Critical Appraisal of Cognitive Neuroscience

William R. Uttal

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For Mitchan

"Were our minds and senses so expanded, strengthened, and illuminated, as to enable us to see and feel the very molecules of the brain; were we capable of following all their motions, all their groupings, all their electric discharges, if such there be; and were we intimately acquainted with the corresponding states of thought and feeling, we should be as far as ever from the solution of the problem, 'How are these physical processes connected with the facts of consciousness?' The chasm between the two classes of phenomena would still remain intellectually impassable. Let the consciousness of love, for example, be associated with a right-handed spiral motion of the molecules of the brain, and the consciousness of hate with a left-handed spiral motion. We should then know, when we love, that the motion is in one direction, and, when we hate, that the motion is in the other; but the 'Why ?' would remain as unanswerable as before."
—John Tyndall, 1871, p. 87[1]

"That the mental processes may be due to cerebral activities we may believe, but with what anatomical elements the individual mental processes may be connected we do not know. Notwithstanding our ignorance, it would appear best and most scientific that we should not adhere to any of the phrenological systems, however scientific they may appear to be on the surface. We should be willing to stand with Brodmann, believing that mind is a function or an attribute of the brain as a whole, or is a concomitant of cerebral operations, but I at least am unwilling to stand with the histological localizationists on the ground of a special mental process for special cerebral areas or for special cerebral cell groups."
—Shepard Ivory Franz, 1912

"There is no greater impediment to a unified cognitive neuroscience than our inveterate Aristotelian tendency to consider cognitive functions as separate entities. For more than a century, experimental psychologists have been successfully dissecting them. It should be obvious, however, that the success in defining, classifying, and experimentally manipulating any given cognitive function does not imply a separate neural structure for it. Common sense, psychophysics, and experimental psychology provide ample evidence that all cognitive functions are interdependent. Perception depends on memory and attention, memory depends on perception, language depends on all three, and intelligence is served by all of the above plus reasoning, and so on. Also interdependent must be, of course, their neural foundations."
—Joaquin M. Fuster, 2000, p. 52

"There is an explanatory gap between our knowledge of the brain and what we know first-hand of ourselves, and it is difficult to imagine what kind of finding would bridge that gap. That there should be a neurological basis for our mental life is not controversial. But that beginning insight also seems to exhaust the contribution of brain scans to our self-understanding."
—Matthew B. Crawford, 2007, p. 78

[1] I am grateful to Stanley Klein of the University of California-Santa Barbara for calling this prescient quotation to my attention.

Contents

Foreword

Everyone knows that the brain is in the head and that the mind is the product of the brain—no brain, no mind; malfunctioning brain, defective mind. But reflection on this unique organ—this “enchanted loom,” or “great raveled knot,” to use Sherrington’s (1942) colorful metaphors—prompts many questions. Why, for example, is the brain all “scrunched up” in one spot within the skull? Might there not be some advantages to having it distributed throughout the body, as the nervous system, as a whole, is? Distributed systems work perfectly well in the world of computers, even without any centralized control element. Might it not be advantageous to have pockets of computing power located close to the extremities where much of the action is, rather than having all that power located in one small area at one end of the body. (It was once held that Diplodocus, a dinosaur about 80 to 90 feet in length, had one brain in its head and another at the base of its spine because, given the relatively leisurely rate at with which neural impulses travel, the head was too far away from the tail to control it in a timely fashion. Whether the bulge at the end of the end of the spine actually housed a second brain is now disputed.) Another of the mysteries of the brain is why its surface has such a convoluted shape, with more of the cerebral cortex—the two-millimeter thick layer of gray matter that covers the entire cerebrum—within the fissures than on the surface.

Presumably, the brain is in the head, and the cortex is folded in on itself, as it is, because there is some biological advantage of it being in this location and shaped this way. One advantage of having the cortex in the smallest possible space is that of minimizing the time required for the transmission of neural impulses between its most widely separated parts. Laid out flat, the cortex would cover an area of about two square feet; scrunched up in the skull, the greatest distance between any two points is roughly 6 inches, and the average distance between random points is, of course, much less than that. A possible cause of the convolutions is that the cortex has experienced an explosive growth relative to the brain’s inner portions, and there is some reason for it to remain relatively thin. The only way this could happen within the confines of the skull would be for the outer layer to fold in on itself. This idea gains plausibility from the fact that the human brain differs from those of other mammals more with respect to its gray matter than with respect to other parts.

Whatever the reasons for the brain's location and shape, the question that trumps all others is: just how does the brain make the mind? Many inquisitive minds have pondered this question over the ages—minds pondering the basis of the existence of minds. I said that everyone knows that the mind is the product of the brain, but perhaps in that I misspoke. The nature of the relation between brain and mind is an old and abiding enigma. Some would say that the brain *is* the mind. Others would deny the existence of the mind, dismissing its apparent existence as an epiphenomenon. I take it as the prevailing view among modern neuroscientists, however, that the brain does, indeed, make the mind. Few, if any, would dispute the assumption that the brain, somehow, gives rise to sensation, perception, cognition, volition, and other psychological experiences or states. The persisting challenge is to determine precisely how the brain does this.

Exactly when people began to suspect that the brain had something to do with sensory experience and thinking is unknown, but it probably predates recorded history. Did neolithic humans have enough of an idea of the role of the brain—or whatever they thought inhabited the head—in determining mental states to motivate them to develop the practice of boring a hole in the skull for therapeutic or other purposes? No one knows. One guess is that they adopted the practice in the belief that it relieved severe head pain or lessened the occurrence of mysterious behavior, such as might be caused by epilepsy.

The ancient Greeks entertained a variety of notions regarding the function of the brain. Among the first to identify it as the organ essential to sensation, intelligence, and thought was Alcmaeon, who lived during the fifth century BC. Plato endorsed this idea in *The Republic*, locating reason (one part of the tripartite soul, the other two parts being spirit and desire) in the head. Aristotle described the function of the brain—which he believed to be the coolest organ of the body—as that of cooling body moisture, which had been turned into vapor by the sun's heat. He, like the fifth-century BC Sicilian philosopher-scientist Empedocles before him, considered the seat of intelligence to be the heart.

During the second century AD, renowned Roman physician Galen took exception to Aristotle's description of the brain as a cooling system and proclaimed it to be the source of mental activity. Not surprisingly, this did not settle the matter, and the Alcmaeonic-Platonic-Galenic view did not become universal, or nearly so, until relatively modern times. In common parlance the heart is still cited as the organ that houses our innermost beliefs, knowledge, and passions; and it is not always clear to what extent such references are intended to be metaphorical. In the seventeenth century René Descartes famously proclaimed the pineal gland to be the abode of the soul and the mediator between mind and body. His basis for this view was belief that this gland is the only structure within the brain that does not exist in duplicate and his mistaken assumption that animals do not have pineal glands.

Dissections of the brains of cadavers (some claim also of those of live prisoners) were performed as early as the third or fourth century BC by the Greek philosopher-physicians Herophilus and Erasistratus (the latter is credited with distinguishing between sensory and motor nerves). Although outlawed at times in certain places (e.g., ancient Rome), autopsies and dissections provided increasing knowledge of human anatomy and physiological functions throughout the Middle Ages and until the present day.

The idea that different regions of the brain might serve different purposes dates back at least to the Middle Ages (Kemp, 1996). Interest in the possibility of associating specific functions with different brain areas began in the nineteenth century, when localizing was done primarily by studying the effects on various psychological functions of injuries to different parts of the brain and sometimes by creative speculation. Thomas Willis, a seventeenth-century English physician and author of *Cerebri Anatome* (1664), believed memory to be located in the cortical gyri of the cerebrum (rather than the cerebellum), because, he observed, it is the temples and fore part of the head, not the back, that we rub when we wish to remember something. German physician Franz Joseph Gall (1758–1828) developed the idea of inferring psychological properties from the detailed shape of a person's skull into a pseudoscience, remnants of which persist.

Perhaps the first serious attempts to localize functions in the brain by empirical means should be considered to be those of the French physician, Paul Broca (1824–1889), who performed postmortem examinations of the brains of people suffering from paralysis and aphasia. Broca's pioneering contribution has been acknowledged by the designation of a region of the left (or dominant) temporal lobe of the brain just posterior to the inferior frontal gyrus, critical to speech, as Broca's area. Carl Wernicke (1848–1905) pointed to an area posterior to Broca's as essential to language understanding; again the basis of the localization was study of the effects of localized injury.

Undoubtedly the most widely publicized incident involving the study of localized brain function as a consequence of an accident was the horrific injury suffered by Phineas Gage in 1848. Gage, about 25 at the time, was using a straight iron bar to tamp blasting powder in a hole in preparation for blasting rock from a roadbed at a railroad construction site. The powder exploded prematurely, launching the bar, which passed through Gage's head destroying much of his frontal cortex. Gage remained conscious through the ordeal and lived as a physically able person for an additional 12 years. Much has been written about the effects of the trauma on Gage's personality and mental capability. According to a recent account (Macmillan, 2000), reports of its effects include many claims for which there is little factual evidence, and the actual effects were not as devastating as the more sensational reports portrayed them to be.

As medical knowledge increased and surgical procedures became more sophisticated, it became feasible to study brain functions in less opportunistic and passive ways. Pioneering work was done by Canadian neurosurgeon Wilder Penfield (1891–1976), who made a practice of stimulating the exposed cortex of persons undergoing brain surgery under local anesthesia and recording their responses. In this way he was able to pinpoint areas that were to be excised to control epileptic seizures. Over time he compiled detailed maps of primary sensory and motor areas of the cortex.

The emerging belief that the frontal, or prefrontal, cortex was unessential to normal mental functioning, but at the same time was somehow responsible for various types of psychotic behavior or states, led in the 1930s and 1940s to a rash of frontal lobotomies, a procedure for the development of which in 1936 Egas Moniz received the Nobel Prize in 1949. A stunningly dark chapter in the annals of twentieth-century medicine is the story of Walter Freeman's infamous "ice-pick lobotomies," of which he is said to have performed between 3,000 and 4,000, often in showman style, over a period of less than 20 years. Lobotomies typically had the desired effect of quelling psychotic episodes—replacing violent behavior with docility—but at the cost of destroying people's personalities. They fell out of fashion with the appearance of antipsychotic drugs in the mid-1950s.

Development of the ability to deliver a mild electrical impulse to precise areas of the brains of animals via implanted electrodes led to much experimental exploration of the brain in efforts to associate specific areas with specific functions. Among the better-known results of these efforts was the serendipitous discovery by James Olds and Peter Milner (1954; Olds, 1956, 1958) that rats would relentlessly press a bar in order to experience whatever they were experiencing when a mild current was delivered to the septal area of their brains. This discovery yielded the notion of a pleasure center within the brain and launched a to-date unsuccessful search for a reliable method of stimulating it chemically in humans for therapeutic or hedonistic purposes (Stix, 2009).

Among the more interesting discoveries in brain science to have been made in the twentieth century was the effect on brain function of the separation of the two cerebral hemispheres by transection of the corpus callosum, as a means of controlling otherwise uncontrollable epileptic seizures. Roger Sperry (1913–1994) and colleagues explored the effects of this operation on the subsequent functioning of the "split brain," many of which gave a picture of two brains working independently. This work, for which Sperry received the Nobel Prize in 1932, and research following directly from it have been documented extensively by Sperry's student and collaborator, Michael Gazzaniga (e.g., 1970, 1998).

Despite the crude techniques available, the study of brain function over the centuries produced considerable knowledge about, and immense fascination with, the brain. Wouldn't it be wonderful and illuminating to be able to observe what is going on in one's brain *while* one is sensing a touch on one's arm, perceiving the red of a strawberry, recalling yesterday's baseball game or an event in one's childhood, searching one's memory for a temporarily forgotten name, or feeling euphoric or sad or guilty or proud. Thanks to the breathtaking strides that have been made in neural imaging, a technology that barely existed a few years ago, this idea has morphed from the realm of fantasy to that of working science. But what does it mean to observe the activity of the brain? At what precisely should one want to look? The firing of individual neurons? The activity at neural synapses? The patterns of signal transmissions among neuron clusters? Changes in the gross level of activity of specific regions of the cortex or of identifiable "organs" within the brain?

What does one look for in the hope of finding the biological resting place of a specific memory? A reverberating neural circuit? Changes in the diameters of axons or dendrites with resulting changes in propagation characteristics such as resistance or conductance? Evidence of proliferation of synaptic terminals? Establishment of new intercellular connections? Changes in the properties of the conducting surfaces of neurons? Changes in the rate of production of different types of neurotransmitter substances? Changes in the arrangement of nucleotides on RNA or DNA molecules? Effects of the "second messenger" nucleotide cyclic adenosine monophosphate (AMP)? Patterns of activity of hormones such as adrenocorticotropic hormone (ACTH) and the melanocyte-stimulating hormone (MSH)? The chemical activity of glial cells? The operation of isolatable subsets of neurons—"neural cliques?" Something else?

The questions seem endless, but one is inclined to assume that whatever one chooses to observe, one is sure to learn much that is both intrinsically interesting and useful. Little wonder that the development of sophisticated noninvasive techniques for observing ongoing brain activity has everyone wanting to take a look. The opportunities presented by these techniques have been seized enthusiastically by researchers, and a great deal of work is exploiting them for the purpose of identifying areas and structures within the brain that are involved in the production of specific psychological states and experiences. But the apparent richness of the possibilities prompts other questions. Are there limits—practical and/or theoretical—to what one can discover about the mind by observing the behavior of the brain? What can one reasonably hope to learn about psychological processes and experiences through the use of brain imaging techniques?

In *Mind and Brain: A Critical Appraisal of Cognitive Neuroscience*, William Uttal addresses questions of these sorts and casts a sharp critical eye on what appears to be a trend toward more and more work on brain mapping, spurred by technological developments, at the expense of less and less utilization of conventional experimental studies of psychological phenomena per se. He does not argue that brain imaging devices and techniques are worthless. To the contrary, he characterizes their development as among the more important diagnostic and scientific developments ever and acknowledges their unique and revolutionary role in alleviating human suffering. But he raises many questions regarding their use in the study of psychological—especially cognitive—processes. What, he asks, is the likelihood that brain imaging techniques will add value to existing behavioral science research? Is this type of research worth the amount of attention it is receiving relative to that being received by other types of research aimed at understanding cognition? Are realistic assumptions being made about the knowledge and understanding of cognitive processes and experiences that these techniques are likely to produce? Are the results of the research that is being done being interpreted objectively and the practical implications of those results being assessed and reported responsibly?

Mind and Brain is an ambitious and gutsy effort to confront these and closely related questions. In Uttal's words, his aim is "to critically evaluate to what extent brain imaging and other recording techniques have informed scientific psychology"—to consider, in particular, whether "these new technologies offer us an expedited pathway to the great question of how the brain makes the mind."

One learns early in the book—in the Preface—that Uttal's answer to the last question is unequivocally negative; in the remainder of the book he makes a case to convince the reader that that conclusion is warranted. Readers of *The New Phrenology*, Uttal's earlier book on the same topic, will not find this surprising. *Mind and Brain: A Critical Appraisal of Cognitive Neuroscience* is an update of *The New Phrenology* in that it covers a great deal of work involving the use of brain imaging techniques that has been done since the latter was published, but it is more than an update by virtue of addressing a broader range of relevant topics.

The case that Uttal makes is many faceted. It deals with technical and methodological issues that are relevant to the application of brain imaging to the study of cognitive processes, including the costs and cumbersomeness of imaging equipment, difficulty in controlling independent variables, questionable reliability of data processing (the subtraction technique) and interpretation, difficulty of measuring the intensity or amplitude (as distinct from location) of brain activity, uncertainty regarding precisely what type of neural activity brain images represent, temporal differences between experienced cognitive states and presumably associated brain image patterns, inter- and intrasubject variability in recorded brain activity ostensibly representing the same cognitive states or activities, and the complexity of techniques that are used to analyze brain imaging data.

He argues that the brain's neural network is too complex and the observational instruments at our disposal too blunt to permit study at a level of specificity that would warrant hope of discovering how the brain makes the mind. Moreover, the limitation, he contends, may be more than a temporary one; the complexity of brain networks may be beyond analysis:

> It may represent an intractable problem that neither new measuring devices nor computational engines could ever begin to unravel. There are too many uncertainties, too many neurons, too many idiosyncratic connections (e.g., the brain is not neatly organized as is a simple crystalline structure) for us to ever be able to understand its detailed organization and how, specifically, this complex information pattern produces the reality we call mind.

Uttal considers most cognitive constructs (attention, memory, emotion) to be insufficiently precisely defined to admit of matching them up with specific brain locations or processes. He is skeptical of the numerous taxonomies of learning and memory that have been proposed, questioning whether the various "types" that have been identified are truly distinctive. He contends that the functions that researchers of brain imaging generally are trying to locate have been identified by specific psychological models, and if the models were to change, it would make for considerable confusion.

Among several important distinctions that Uttal makes, none is more central to his position than the distinction between the questions of *where* and *how*. Much of his critique deals with the prospects of answering the *where* question—the localization of brain regions responsible for specified mental states or experiences—and his conclusion is that the prospects of answering it satisfactorily are not good. He argues that it is theoretically impossible to associate any particular brain activation site or pattern of activation uniquely with any particular cognitive state because even the simplest of thoughts typically activates many areas of the brain, and any given area is likely to be activated by many different psychological processes. "In fact, as some scholars have pointed out, brain images tell us little more than that there is some brain activity when our minds are active. . . . However, as far as specifying the specific neural processes that are the coded equivalents of mind, virtually all such cumulative measures of brain activity are bankrupt."

But suppose we *could*, or sometime in the future *will*, answer the *where* question. Would this bring us any closer to an answer of the question of *how*? As Uttal puts it, "What does knowing what part of the brain is activated by some stimulus or task tell us about how that part encodes mental activity?" What if we had (or sometime in the future acquire) the techniques to observe the activity of neurons and their interactions at whatever level of detail desired? Is it reasonable to suppose that at some point we would see that the activity that is observed must give rise to mental states we all experience?

What would it mean to *explain* consciousness and subjective experience in terms of neural activity? That learning does not require consciousness has been known for some time. In *Design for a Brain*, W. Ross Ashby (1952/1960) attributes his failure to use "consciousness and its related subjective elements" in his book, despite his intention to deal with how the brain learns, to the fact that "at no point have I found their introduction necessary" (p. 11). Norbert Wiener (1964) addressed the possibility of the existence of machines that learn and reproduce themselves in his book, *God and Golem, Inc*. Many instances of machine learning have been documented in the meantime, and the self-replicating capability is an active area of research. The possibility of machines becoming self-aware, once the purview of science fiction, has been getting increasing attention from scientists with a futuristic bent. The June 2010 issue of *Scientific American* includes the development of machine self-awareness in its list of "12 events that will change everything" (Greenemeier, 2010). If machines become self-aware, some argue, they will surely discover and act upon the possibility of reproducing themselves in ever better (more intelligent, more powerful) forms, and what this would portend for human beings, who can say?

For present purposes the question of interest is this: how could one ever know for sure whether a machine was aware of itself or conscious in the sense in which people are aware of themselves and conscious? Its ability to claim to be conscious and to behave *as though* it were conscious is hardly compelling evidence. Imagine not knowing what a brain is but having the opportunity to observe one (natural or artificial) in action at any level of detail one desires. What might one expect to see in the activity of the molecules that make up the neurons, and those that carry the electrochemical signals down the axons and across the synapses—individually or collectively—or in the detailed behavior of complex neural circuits, that would convince one that consciousness is the necessary result? I find it difficult to imagine what it could be. It seems to me not unlikely that we will never solve the mystery of consciousness. To some, acceptance of this possibility undoubtedly would be a cause for disappointment or despair. I am not among them.

Uttal objects to what he sees as overstatements of what has been learned about human cognition through brain imaging studies, and in doing so he does not mince words. “[A]ll putative extrapolations from behavioral to neuroscientific mechanism are vastly underdetermined inferences from data that do not provide the logical or empirical constraints necessary to draw robust conclusions.” Further, he contends “Cognitive neuroscience, despite considerable ballyhoo, does not yet have the tools with which to deal with a complex system such as the brain. . . . Of particular concern are the unjustifiably strong conclusions drawn from noisy data, a characteristic of much of the research currently being carried out in this field.”

We psychologists (including neuroscientists among us), Uttal says in a variety of ways, are good at fooling ourselves into believing that we have made progress in understanding details of the neural substrate of behavior and cognition when we really haven’t. He speaks of the seductiveness of brain images and their tendency to add persuasive weight to a report. (It seems a safe bet that *neuroscience* sounds more scientific than *psychology* or even *cognitive science*, at least to the general public and probably to many scientists as well.)

It is arguably a major failing of human reasoning that in judging the benefit that has been derived from any effort to achieve some goal, or good, we too often fail to consider nonobvious negative effects the effort may have had, or what might have been accomplished if the same energy had been expended on alternative goals. Uttal argues that to the extent that work on brain mapping is motivated by the assumption that this is an effective avenue to a better understanding of cognition, much of it is not worth the opportunity costs (represented by alternative paths not taken) it incurs.

Is Uttal right in this assessment? I don’t know. What I do know is that *Mind and Brain: A Critical Appraisal of Cognitive Neuroscience* is a scholarly, incisive, thought-provoking book. This will surprise no one who has read any of the many other substantive provocative books Uttal has written. In this book, Uttal raises hard questions about the nature of science, about how one should decide what is worth doing, about prioritizing among the possibilities, about how to determine whether progress is being made—especially about whether progress is being made in understanding cognition. Whatever the future of brain mapping, I believe that in *Mind and Brain: A Critical Appraisal of Cognitive Neuroscience* Uttal has done an extraordinarily valuable service in articulating a host of issues that deserve attention from anyone with a more-than-passing interest in the age-old and ever-new question of how the brain makes the mind.

Raymond S. Nickerson
Tufts University

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Preface

There is no greater fascination on the part of humanity than with the brain mechanisms that might explain our minds. What, we all ask, could possibly account for our personal awareness of the world of which we are a part? There are so many examples of this fascination in both popular culture and the scientific literature that it would be impossible to catalog them. Whether our personal answers to the great question of what we are mentally are to be found in religion, spirituality, philosophy, physiology, or psychology, the question is undoubtedly asked by virtually all of us at one time or another.

One of the most important developments in scientific history is that increasing numbers of people are attempting to answer this age-old question in terms of the biology of the nervous system and, in particular, of the nature of that magnificent organ—the human brain. The brain is probably the most complex system that has ever been discovered. Its complexity possibly rivals that of the universe and probably exceeds it in terms of the potential range and diversity of interconnections and interactions.

Cognitive neuroscience is the current manifestation of the theologies, philosophies, and sciences that have long been concerned with the relation between our minds and our brain. It asks (or should ask) questions such as:

1. How does the brain make the mind and control behavior?
2. What is the level of analysis of the brain that is the most likely basis of our cognitive processes?
3. How do traumatic brain injuries inform us about the relation of the mind and the brain?
4. How do surgically induced lesions inform us about the relation of the mind and the brain?
5. How do EEGs and brain imaging techniques inform us about the relation of the mind and the brain?
6. What is the significance of different patterns of activity on the brain when a person is stimulated or tasked?
7. Can brain imaging provide an alternative approach (to behavioral techniques) with which to measure, control, and predict behavior? Does it add value to the behavioral measures?

8. Given that the dominant current theory is that the mind can be parsed into relatively independent "modules" whose mechanisms can be localized in circumscribed regions of the brain, what is the current state of this theory?
9. What implications do new findings on the distribution of neural responses, brain "holism," and cognitive inaccessibility have on the dominant theories of cognitive psychology and their efforts to discover the underlying neural mechanisms of our cognitive activities?
10. What is the current state of the empirical findings from brain imaging–cognitive comparisons? Does their lack of reliability and modest correlations justify their use as predictors of performance and abilities? Do they provide a coherent pathway to understanding the mind-brain problem?
11. Are we drawing appropriate interpretations and inferences from the empirical findings that have been forthcoming over the last two centuries in particular?
12. What does the future hold for cognitive neuroscience? Is it reasonable to think of a non-neuroreductive scientific psychology? In other words, can psychological science exist and prosper without neuroscience?
13. What is the likelihood that cognitive neuroscience as we conceptualize it today will be applicable to the many social and medical problems facing humanity?

The goal of this book is to consider some of the many alternative answers that have been provided to some of these questions. The strategy used here is framed in the form of a critical review of both cognitive neuroscience's past history and its modern developments. A particular interest is considering the possible role of the newest technological development—brain imaging—in studying the relationship between the mind and the body. Unfortunately, the explosive growth of this new mode of research has not been accompanied by a comprehensive and synoptic evaluation of the huge number of studies that have been published in the past two decades. To do so coherently, however, requires that we also consider the history of cognitive neuroscience prior to the invention of modern brain imaging devices, especially functional magnetic resonance imaging (fMRI). This includes consideration of the psychological and more conventional approaches to what used to be called physiological psychology.

My purpose, therefore, is to take a cut at a synoptic synthesis of this substantial body of scientific literature. This is not going to be an easy task; results are varied and numerous, and, as I point out in the body of this book, not only is there a substantial amount of inconsistency in the research findings, but there is also great conceptual confusion about the significance of virtually every one of the multitude of reported experiments.

At the outset I must accept the fact that it is impossible to cover all of the relevant literature. However, by selecting appropriate exemplars, I hope that it will be possible to come to a reasonable conclusion about the current status of what clearly is a time of major developments in cognitive neuroscience. Having no vested interest on my own part with regard to particular findings or specific theories, I hope what I can offer is an objective evaluation of the state of the field a decade or so into the twenty-first century.

Nevertheless, I also have to acknowledge that I come to this project with a somewhat negative bias. It appears to me that there has been far too much hyperbole and far too little critical analysis of what our experimental outcomes really mean. This has been a major problem throughout the history of cognitive neuroscience and promises to continue to be one well into the current period. I am not now convinced of the validity (defined in its most formal sense) of much of these data and even less so of the interpretations that are often attached to them.

Despite this skepticism, there has been enough empirical progress to support a major transformation in thinking about the role of brain imaging in cognitive neuroscience. That change has been from an almost phrenological orientation in which separate cognitive modules were thought to be localized in narrowly circumscribed locales in and on the brain. Recent research studies, especially those in which the results of many different experiments were combined (meta-studies), have shown that the response to even the most carefully controlled stimulus is much more widely distributed on and in the brain than hitherto thought.

In an older work (Uttal, 2001), I argued against localization on technical and conceptual grounds. In a more recent work (Uttal, 2009), I showed how recent research made this point even more emphatically and led to the conclusion that the modular-localization hypothesis had to be replaced by one that emphasized both widespread distribution of brain representations and a more unified view of psychological mechanisms.

If there is an overarching assumption driving the ideas expressed in this book, it is my focus on the failures of reductionism—both to cognitive modules and to neural mechanisms. I am now convinced that finding support for both cognitive modularism and neuroreductionism is a much more difficult task than hitherto assumed and that we actually know far less in both domains than many think we do.

In my earlier books I tried to identify the pitfalls associated with efforts to proclaim the nature of hypothetical cognitive processes on the basis of behavioral observations. In this present work I emphasize the search for some explanation of the increasing variability of empirical findings with regard to their reduction to neural mechanisms. There is still too much uncertainty about some of the most basic findings from studies that attempt to assign specific functions to specific brain regions (or to systems made up of localized functions) to uncritically accept much of the present literature. This book is a modest effort to resolve some of the present problems generated by mental inaccessibility and neural complexity.

This present work has a somewhat different orientation than the two earlier ones. After introducing my philosophy of mind-brain relations and discussing some of the general problems faced by cognitive neuroscience, I carry out a review of specific brain-behavioral studies to see how well they have informed us in our search for mind-brain relations. Although a major effort will be directed at recent imaging studies, the present situation becomes understandable only in the context of the history, both psychological and neuroscientific, that has led us to the present situation.

There are a few general points that I should like to make in these introductory comments. First, as a psychologist, I must express the opinion that scientific psychology is at risk in the current context of brain imaging—the newest fad in its long history.[1] An important question is—do neuroscientific findings inform psychology, or does psychological knowledge inform neuroscience? As I progress through the discussions in this book, I have become increasingly convinced that neuroscience is much more dependent on psychology than psychology is dependent on neuroscience and that with the uncertainties of precise cognitive process definitions and the innate problems we have controlling cognitive states, there is substantial reason for caution. Indeed, beyond the sensory and motor systems, neuroscience has done little, in the opinion of many of us, to resolve any of the great questions of psychology. On the other hand, it is virtually impossible to carry out a meaningful experiment in cognitive neuroscience without guidance from psychological findings and theories.

Am I biased? Of course, I am. I am coming at the problems of cognitive neuroscience from a perspective that may be unusual among my colleagues. Where they seek affirmation of their findings and theories, I seek negation; I challenge the empirical outcomes and interpretative theories. Where their work is based on a set of usually implicit assumptions, I seek to make the assumptions explicit and in doing so often find contradictions at the most basic level of understanding.

Despite some who would read this criticism of cognitive neuroscience as a generalized attack on both experimental psychology and neuroscience, I hope I can make it clear to my readers that I am a strong and positive proponent of the kinds of empirical research that I discuss in this book. Much of the data, the findings, and the results obtained over the last century are relatively solid scientific facts that provide us with a picture of human nature that was not possible in the speculative periods that preceded it. As I show throughout this book, however, there is somewhat less assurance that PET or fMRI images are reliable and valid indicators of psychologically meaningful patterns of brain activity. The difficulty is in large part with the theories that have proliferated over this same time period—theories that have been proposed to explain psychological processes with neural mechanisms. I argue that most of these theories cannot be discriminated from each other for a number of reasons. First, the anatomic structures to which they refer are rarely adequately demarcated, and their activities are, to a degree not yet fully realized, unreliable and unreplicated.

Second, the data are complex, and the systems involved not adequately simplified by assumptions such as “pure insertion”—the idea that the removal of one portion of a complex process leaves all of the other components in their original state.

Third, none of the neuroscientific theories so far proposed is sufficiently quantitative or precise to account for the vagaries of the data. Indeed, most psychological theories do not have identifiable neural postulates that can be tested. Therefore, at the same time, they all permit too much leeway in accommodating contradictory information and do not constrain our theories when opposing discoveries occur. Furthermore, without specific neural postulates, it is rare, indeed, when psychological controversies can be resolved by neurophysiological techniques. The major exceptions to this generalization occur in the sensory and motor domains where the research issues are of neural transmission codes rather than of the neural equivalents of our cognitive processes. In general unless a psychological theory has specific neurophysiological postulates, it is neutral with regard to underlying mechanisms. By "neutral" I mean that it is underdetermined; that is, it does not contain sufficient information to discriminate between plausible neural mechanisms. Underdetermination also plagues any reductionist approaches using behavior and mathematical models as well.

Fourth, there is a lot of cherry picking exhibited in the field; references are selected to provide support for arguments that on close inspection are only a small part of the relevant literature. I must admit that I cannot avoid this problem; my strategy is also to select a few particularly salient reports and deal in depth with each of them for each of the topics considered here. My bibliography will happily be shortened to the hundreds from the tens of thousands by this selective approach; however, more important is the fact that a detailed dissection of a few studies will often uncover hidden design flaws, internally inconsistent findings, and illogic that might have otherwise been overlooked.

This then brings me to a highly personal admission. Whenever one attempts to survey such a broad and complex field of science as cognitive neuroscience, it is very difficult to be sure that one has interpreted all of the reports within the frame of reference intended by the authors. I am sure that there may be discrepancies between their stated conclusions and my own evaluations of their findings. In some cases, I am probably to blame, but in others I am convinced that some investigators have read far too much into what are variable and inconsistent results. In some cases I am sure that differences in initial assumptions may also account for differing interpretations. I also apologize in advance to all of those authors whose publications are overlooked because of the sheer volume of the literature, as well as to those who may feel I have not expressed their point of views correctly.

Obviously, when one samples from such a broad literature, the selection may be unbalanced. I am aware of that problem and admit that I have often sought out articles and reports (the number of which is growing every week it seems) that were critical or that illustrated the variability or uncertainty of the empirical findings. However, the huge variety of stimuli, analytical methods, and experimental conditions makes the results far more variable and complex than anticipated. Indeed, if one examines the literature very carefully, there is a remarkable absence of real replication. This problem is exacerbated by the fact that very small changes in experimental protocol can produce very large changes in results. This problem is even further compounded by the complexity of the brain itself. An emerging generalization is that even the most peripheral parts of the brain are so heavily interconnected with higher levels that it is often difficult to tease out their separate roles.

Finally, another personal note: I am fully aware that the strongly critical approach I take in this book will not be well received by many of my fellow cognitive neuroscientists. However, I am becoming increasingly aware that the field in which we labor is heavily contaminated with both our hopes and our implicit, a priori assumptions. This does not mean that the study of behavior or brain anatomy, chemistry, and physiology will not continue to lead to understanding about their respective fields. It is the current failure to establish robust links between the cognitive and neural domains that is the problem.

If the analysis I present here provides the basis for a more realistic, constructive, and conservative evaluation of what we have accomplished in cognitive neuroscience, or even stimulates some discussion about the possible flaws in traditional and modern research, I will feel that the effort has been worthwhile. With these caveats in place, I now turn to the task at hand—a critical appraisal of the field of cognitive neuroscience in the twenty-first century.

Acknowledgments

The initial stimulus for this book came from a conversation I had with two of my University of Hawaii colleagues, M. E. (Jeff) Bitterman and Peter Balsam (also of Columbia University). Jeff, Peter, and I had gotten into the habit of discussing outlandish ideas about psychology and neuroscience. Despite the enormous activity in the field of cognitive neuroscience, it quickly became apparent that there was a lacuna to be filled—it seemed that there was a need for a critical evaluation of the field from an objective point of view. I owe much to those original conversations. Indeed, my last eight summers have been spent as a visiting colleague at the Bekesy Laboratory of Neurophysiology at the University of Hawaii. These visits were made possible through the efforts of my friend and colleague, Patricia Couvillon. I am deeply grateful to Pat for her intellectual and administrative support during this period. Nor can I ignore the warm aloha hospitality of the many members of the laboratory staff who made this such a rich personal experience.

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Many others have influenced me over the years sometimes by their writing and sometimes by means of a brief comment expressed in much less formal settings. It is amazing how a few words can stimulate a decade-long research project.

For the last 11 years, since my retirement from Arizona State University, I have enjoyed the continued support of my home department—Industrial Engineering. I am deeply grateful to a succession of department chairs and college deans for their willingness to provide a home for my work. It could not have continued without their confidence that what I was doing was worthwhile, however distant it may have been from their organizational charter.

My appreciation for my colleagues and our interactions in our little "metaphysical society" continues unabated. My discussions with Peter Killeen, David Hestenes, Mike McBeath, Art Glenberg, Federico Sanabria, and Warren Van Egmond, played an important role in the development of my ideas. John Reich's invitation to join a comparable social-science–oriented discussion group also helped me to develop some of the strategies of critical thinking that were useful in writing this book.

Finally, my dear wife continues to provide a home environment and what seems to be an unending amount of patience with me. Without her, nothing would have happened. And so, once again, I lovingly dedicate this book to her.

1 Introduction

1.1 Some Background

In the past decade and a half important new developments in instrumentation capable of studying the functioning brain have appeared. These devices, most notably positron emission tomography (PET) and functional magnetic resonance imaging (fMRI) but also now including such exotic tools as magnetoencephalography, now unquestionably make it possible to study the anatomy and physiology of the brain (among other organ systems) better than ever before. There is no question that brain imaging devices represent one of the most important diagnostic and scientific developments of all time. Human suffering has been alleviated to a degree because of these devices in a way that is comparable only to the introduction of anesthesia or the purification of public water supplies. MRI machines are capable of tracking brain transmitter distribution, spotting potential weaknesses in circulation, defining the tracts connecting distant regions, and, to an as yet unknown degree, determining regions of heightened metabolic activity that may be associated with cognitive activity.

Despite this abundant progress, it must be clearly understood that anatomic and physiological images are not direct measurements or indicators of cognitive processes. Indeed, their meaning as correlations of our mental life is open to a wide variety of disputes, empirical inconsistencies, and internal uncertainties.

Nevertheless many researchers in the field of cognitive neuroscience argue that brain images can be used to study the neural foundations of our mental activities in a way that had hitherto been beyond the hopes of even the most imaginative researchers. Supplanting the older techniques of the electroencephalograph (EEG) and the event-related potential (ERP), these new techniques promised to provide a means of studying the function of the brain as it carries out its adaptive cognitive processes. However, many of us are beginning to believe that it is a promise yet to be fulfilled. In this book I critically examine just what has happened and what we have learned from the astonishingly large corpus of published experiments in which brain images are compared to cognitive processes.

The argument that brain imaging techniques will revolutionize cognitive science is based on the idea that they are direct measurements of salient brain activity during controlled cognitive activities. Many applications of brain imaging have been proposed, some of which are thought to offer alternative, if not better, means of measuring cognitive states and processes than those provided by the traditional psychological methods. Many of these suggestions thus promise what are considered to be objective measures of what had traditionally been limited to subjective measures of mental states.

However, there are others who believe that there are substantial unresolved problems with and limitations of this approach that suggest that some of the initial expectations may be unachievable not only in the short run but also in the distant future. The problems arise in many guises and include some that are conceptual, some that are technical, and some that are empirical. This gradually emerging awareness has led to a somewhat belated critical examination of the plausibility of the assumption that brain imaging techniques will permit us to "read the mind" of a human being or even to find adequately correlated biological markers for particular cognitive states.

The current book considers the role that brain imaging has made or might make to cognitive neuroscience. It is a new embodiment of what had hitherto been known as physiological psychology. The complexity and variability of human behavior have made progress in this field difficult to evaluate and even to conceptualize. Clearly, any novel method of evaluating, predicting, and controlling behavior would be of extreme interest—if it could be shown that these methods work or are likely to work in the future. This is the crux of the problem faced by modern cognitive neuroscience—what is the likelihood that brain imaging techniques will be able to bring added value to the existing behavioral science research? How deep should be our science's commitment to techniques that many researchers believe are deeply flawed and, despite their popularity, are neither theoretically nor empirically seminal nor even, in some cases, possible?

An important goal of this book, therefore, is critically to evaluate the extent to which brain imaging and other recording techniques have informed scientific psychology. This is not just an empirical problem; there is a profound theoretical question lurking in the background—what is the likelihood that we will be able to add to the fundamental theory of the mind-brain question using these powerful methods? In other words, do these new technologies offer us an expedited pathway to the great question of how the brain makes the mind?

Although the ultimate answer to the possibility of supplementing, if not substituting, brain research for behavioral research is going to be primarily empirical (will it work?), it must also be appreciated that there are major philosophical and logical issues raised whenever one has the audacity to compare mental and neural activities. There is no denying that this is a task of universal interest and monumental implications, but at least a few scholars agree that at present there has been limited conceptual progress despite the great diversity and number of empirical studies. We can no more ignore some of the imponderable foundation philosophical concepts that are involved than we can the limits of the technology. Given the current state of our knowledge, it may indeed be that our choice of the "correct" level of analysis, whether it is behavioral or neural, may be instrumental in interpreting the meaning as well as the applicability of what are complex and indirect experiments. Clearly, this is a problem of extreme complexity, and resolving it will be instrumental in the future development of scientific psychology as well as neuroscience.

The primary question—how does the brain make the mind?—cannot be studied in isolation. Cognitive neuroscience may have a short history, but it evolved not only from a century or so of physiological psychology but also from a longterm concern with the basic question. Therefore, other tasks in this book will be to review and evaluate the history of the cognitive neurosciences that preceded the invention of the imaging devices. The observation that much of this earlier work is also deeply flawed adds some depth of understanding to why modern imaging techniques have so far failed to achieve some of their most extravagant claims.

1.2 The Great Question—The World Knot

The greatest scientific question of all time, the one to which most human attention has been directed over the millennia, is—how are our minds and our brains related? The profundity of the question has led to its being referred to by Arthur Schopenhauer (1788–1860) as the "world knot."

Although there is considerable debate about the reality of the mind (see for example the article by Schlinger, 2005), to deny its reality or to declare it merely epiphenomenal would be to make human existence meaningless. Furthermore, there is at least one piece of solid evidence that the mental processes are real. That singular piece of evidence is that each of us is endowed with a personal awareness, a process that has come under many names. Whatever the term used—mind, mentality, soul, ego, self, intellect, consciousness, awareness, sentience, psyche, or cognition—we all have first-hand knowledge of what it is that we are talking about when we use any one of these words. There is no way that we could deny the reality of the mind because proof positive exists within each of us—our own sentience. We could not do so without destroying the meaningfulness of our ability to converse and interact at many different social levels.

However, as much as I am convinced that my mind exists, I have long ago resigned myself to the fact that defining the mind is an unachievable goal. These days I look upon it as a process of the brain analogous to rotations being a feature of material devices called wheels—albeit infinitely more complicated. In other words mind is neither nothing more nor nothing less than a function of the material brain.

Despite this near universal appreciation of the reality of mental activities, the description of the mind and the explanation of its neural origins have proven to be extremely difficult challenges for the science that has grown over the centuries to study them. That science is psychology, not the psychology of the therapeutic couch or inferred, but inaccessible, cognitive entities but the hard science of observable behaviors. If the interests of psychological science are combined with neurophysiological science, we refer to this science as physiological psychology, psychobiology, or, most recently, cognitive neuroscience. A major question that is implicit in this discussion is—can (or should) psychology exist without its neural co-studies? More precisely, what do psychology and neuroscience offer each other?

Psychology, confronted as it is by enormous obstructions and difficulties in constructing explanations of mental phenomena, has fractionated into a number of subsciences that have taken many different and often idiosyncratic directions over the years. Schools of thought have proliferated, and over time, strategic collaborations with other sciences have repeatedly formed.

At the root of cognitive psychology, however, has been the assumption that the nature of the mind (or its effects) can be studied experimentally. A further special assumption of modern cognitive neuroscience is that we will also be able to determine the neural conditions that lead to the mind. On the basis of this premise it is argued that, in principle, we should be able to understand the neural mechanisms that account for mental and behavioral activities. It is not yet clear whether or not this goal can or will be achieved; only time will tell. What we can discern now are the intellectual and philosophical roots that underlie the neuroreductionist goal of explaining mind in terms of the brain and the many obstacles that prevent us from achieving that goal.

The most fundamental root of all of these questions lies not in the laboratory but in speculative ontology—a major division of metaphysical philosophy. Ontology is that branch of metaphysics that deals with the philosophy of reality, of the nature of existence itself. The ontology of cognitive neuroscience is especially complex for two reasons: first, we have no direct access to or empirical evidence of the mind (Uttal, 2007); we have only indirect evidence from which we must infer its nature and construct hypotheses concerning its function. Second, mental activity is not sufficiently constrained by behavioral observations so that a robust analysis can be made of it into modular elements: in other words, all of our cognitivist-reductionist theories of mind are underdetermined.

Many questions for which we have no current answers, therefore, lay solely within the confines of the speculative philosophy that we call ontology. In the place of specific empirical answers to some of these most profound questions, philosophers have over the centuries tried to establish certain beliefs about the nature of reality that are based on whatever relevant knowledge is available and rational and logical arguments and derivations that may make these beliefs plausible, even if pure speculation cannot confirm them.

In cognitive neuroscience there is a major ontological assumption that, however controversial, guides the day-by-day activities of laboratory researchers as well as those who conjure up new theories of the relation between the mind and the brain. That basic assumption is that, however inexplicable it may be at the moment, the brain makes the mind. Although we do not know how, it is widely accepted that a complete neural explanation is, in principle, possible. Those who labor in the laboratory rarely make this monistic assumption explicit, and yet few cognitive neuroscientists would challenge this fundamental idea.[1]

Nevertheless, the assumption of mind-brain equivalence is without any compelling empirical foundation; none of the required tests of necessity and sufficiency have ever been carried out to confirm it generally or specifically. However likely it may seem, there is no evidence other than plausibility and reason to support this foundation assumption.

This profound foundation assumption comes in two parts (box 1.1). The first part is a general hypothesis, implicitly honored by all cognitive neuroscientists. It asserts that any mental or cognitive activities and processes as well as all of those that control behavior are the functions, the outcomes, or the results of the activities of the nervous system. Herein is the foundation assumption of what ontologists would refer to as monism or physicalism or mind-brain neuroreductionism.

Only those who believe in some kind of dualism would deny this part of the basic ontological postulate. (See Uttal, 2004, for a more complete discussion of the impact of dualistic thinking throughout history on theology, philosophy, and psychology.) This assumption links the worlds of the mind and the nervous system into a single inseparable reality; one part is structure, and the other is function. We can no more conceptually separate the two than we can separate the circular motion of a wheel from the wheel itself. This does not mean, however, that the two sciences—psychology and neuroscience—are inseparable empirically. Despite the ontological, in principle, inseparability, practical considerations (e.g., complexity) may keep these two scientific paths separate. Examining this issue is also a part of the challenge faced in this book.

The essential point of the first part of the basic ontological postulate is that the function cannot exist without some kind of equivalent physical structure. Our minds are products of our nervous system, and any idea of the consciousness or mind existing after the deterioration of the brain is without merit. Indeed, without this kind of mind-brain[3] monism the whole cognitive neuroscience enterprise would be meaningless and pointless; we could never be sure that our studies were not contaminated by other forces that were totally out of our control and totally unaccounted for in our experimental protocols.

Beyond the general mind-brain, monistic postulate just described lies the second part—one that is much more specific. It is the hypothesis that our minds are not just functions

Box 1.1

The Two Parts of the Basic Ontological Postulate

1. All mental processes are the outcome of neural activity.
2. All mental processes are the outcome of the microscopic interactions and actions of the great neuronal networks of the brain.[2] This is the proper level of analysis of the mind-brain problem.

of our material nervous system (the first part) but that they are the specific result of the cumulative integration and interaction of complex and innumerable neuronal activities that go on in the brain as opposed to other levels of neural activity.

It is this complex and intricate pattern of neuronal activity and interactions that cognitive neuroscientists assert becomes or *is* mind; it is in the complex network of neurons that memories are stored, that decisions are made, that personalities are forged, and that behavior is controlled. It is there that the physiological actions are transmuted in some mysterious way into all of the many kinds of mental states, processes, feelings, and faculties that grace human existence. The mind, according to this postulate, arises out of the complex interactions of billions of component parts in ways that we do not now know and possibly may never to be able to know.[4] The relation between the brain and the mind, cognitive neuroscientists agree, is something akin to the Sherrington's (1940/1963) "enchanted loom":

> The brain is waking and with it the mind is returning. It is as if the Milky Way entered upon some cosmic dance. Swiftly the head-mass becomes an enchanted loom where millions of flashing shuttles weave a dissolving pattern, always a meaningful pattern, though never an abiding one; a shifting harmony of sub-patterns. (p. 178)

This is beautiful poetry but hardly a rigorous scientific finding; it is simply a vague metaphor for the point being made by the second part of the basic ontological postulate.

This piece of poetry by Sherrington aside for the moment, the general principle expressed in the second postulate is widely held by contemporary psychologists and neuroscientists. The modern version of the idea was probably first expressed by McCulloch and Pitts (1943) and Pitts and McCulloch (1947) in their pioneering work on the logic of networks and then in a follow-up on form recognition by such networks. However, the first specifically neuroscientific expression of the second postulate was published by Hebb (1949). In it he suggested specific patterns of neural interaction as the basis of cognitive activities. His theoretical neurophysiology was based on his elaboration of what had originally been a psychological principle—Thorndike's (1931) "Law of Effect."[5] This purely psychological observation was that repeated practice led to enhanced behavioral strength. Hebb argued that this law must also have a neural equivalent and in 1949 presciently formulated the following neural equivalent of it:

> When an axon of cell A is near enough to excite a cell B and repeatedly or persistently takes part in firing it, some growth process or metabolic changes takes [*sic*] place in one or both cells such that A's efficiency, as one of the cells firing B, is increased. (Hebb, 1949, p. 62)

Hebb then went on to suggest that the "growth process" was the increased growth of synapses as they were exercised. This idea—that it is the change in synaptic conductivity that accounts for the changes in the neural network—is the basis of most physiological theories of learning and memory. Synaptic conductivity changes can account for short-term memory by invoking reverberating circuits that fade as the temporary synaptic changes lose the transient "potentiation." Long-term memories are accounted for by permanent changes in conductivity so that the information in the synaptic patterns becomes locked in. Martin, Grimwood, and Morris (2000) present a compelling argument that such synaptic changes are necessary but that “little data currently support the notion of sufficiency” (p. 649).

Hebb, nevertheless, made some remarkable speculative leaps from this foundation idea of neural networks. He postulated the existence of “cell assemblies”—a “diffuse structure” of neurons in the brain that was created as a result of use and one that could encode complex responses. To this construct he added the notion of the “phase sequence”—a series of cell assemblies that actually was the level at which “thoughts” became extant. It is important to reiterate that the empirical evidence for these hypothetical neuronal net structures is as nonexistent now as it was then.

Many contemporary scholars also followed in Hebb’s footprints. In a recent debate in a popular magazine Koch and Greenfield (2007)[6] argued from two opposed speculative points of view in attempting to answer the question—“How does consciousness happen?” Their answers were almost the same but differ in one main way; Koch argues that consciousness occurs when a specific set of neurons in a specific part of the brain fires in a specific manner. Greenfield argues that the brain produces consciousness when neurons in all parts of “the brain are synchronized into coordinated assemblies, and then disband.”

Both hypotheses share a common principle, what I have referred to as the second part of the basic ontological postulate—the plausible, but unsubstantiated, idea that it is the arrangement of the great neuronal networks in the brain that accounts for consciousness, their term for the mind.

The distinctive anatomical attribute that distinguishes between their two theories, on the other hand, is the degree to which the neural network of the brain producing consciousness is localized or distributed. However, they do not differ with regard to the level of analysis; both assume that it is based on the detailed state and interactions of the neuronal network of the brain.

Despite the disclaimer in Koch and Greenfield’s joint paper that neither one is “is attempting to explain how consciousness arises” (p. 83), in fact both are actually operating at pretty much the same level at which Hebb was at the beginning of the neural network days. Both are proposing ingenious, but nonspecific and untestable, hypotheses that closely conform to the ontological postulates presented earlier. Both make the same foundation assumptions, and both suggest ways that such assumptions might be implemented. However, neither makes any specific statements about the details of how the neural networks produce the mind. Their speculative contributions are plausible and reasonable; however, they are without any empirical support. Koch is very explicit about this in their joint article when he says:

> Neuroscience does not yet understand enough about the brain’s inner workings to spell out exactly how consciousness arises from the electrical and chemical activity of neurons. Thus, the first big step is to determine the best neural correlates of consciousness (NCC)—the brain activity that matches up with specific conscious experiences. (pp. 76–77)

In this manner he retreats back to confront the traditional problems faced by all cognitive scientists. First, all of the problems faced by correlation methods are once again brought to our attention; second, the brain measures—the NCCs—to which he alludes are generally drawn from irrelevant levels of analysis such as brain images, EEGs, and other cumulative methods; and,third there is no direct access to the conscious experiences that permits us to directly compare mental and neural events. The details of the neural networks, however gracefully and eloquently expressed, are totally finessed. Indeed, with considerable justification, we may conclude that Koch and Greenfield's ideas are, perhaps, less specific than were those of Hebb!

It is in this context that the greatest misunderstanding of the current brain imaging approach becomes crystal clear. In a recent paper (Posner & Rothbart, 2007) extolling the importance of the Hebbian tradition dealing with neuronal networks, it was suggested that brain imaging "also probes neural networks that underlie all aspects of human thought, feeling and behavior" (p. 5). Unfortunately, there is a disconnect here between the Hebb *neuronal* network model and the *neural* network of brain regions at which brain imaging techniques operate: the Hebbian network is a network of *microscopic* neurons. The networks that are studied with, for example, an fMRI system, are combinations of *macroscopic* brain regions that actually tell us very little about how the brain makes the mind. Nor do brain images tell us anything about the details of the network of neurons. In fact all of the salient details of their function are lost by the processes of accumulation and summation that characterize such techniques as fMRIs and EEGs. It is entirely possible for two totally different neuronal network states to produce the same fMRI response. Thus, there is no functional relation between an fMRI image and the activity of the critical and essential network of neurons that Hebb so presciently pointed out must be the psychoneural equivalent of a cognitive process. In short, the macroscopic neural networks studied with fMRI systems are not the same as the microscopic neuronal networks that cannot (because of their complexity) be studied at all. To link them together is nothing other than a neuroscientific pun.

It should be clear now that the second part of the ontology postulate is widely, but not universally, held among cognitive neuroscientists. For reasons that have more to do with available technology than with either philosophy, logic, or empirical findings, many other theorists place the essential transformation process between brain and mind at other levels of activity such as single neurons, wavelike fields of activity, or activated chunks of the brain. These alternative hypotheses must be appreciated to be temporary surrogates for the impenetrable neuronal net hypothesis. It is the information processing by highly complex microscopic neural networks, rather than any of these alternative measures cum theories, that is the core of the foundation premise of modern cognitive neuroscience.

The main difficulty that makes the neuronal network hypothesis into a postulate (as opposed to a robust empirically observed fact) is that the combinatorial complexity of the brain's neuronal network is so extreme that it cannot be studied directly. The true psychoneural identity level of activity—the idiosyncratic and exceedingly complex interaction of so many neurons—at which brain becomes mind is computationally intractable. As a result, alternative theoretical approaches such as single-cell or field measurements are called into play simply because they are measurable with available measuring instruments. Ethologists refer to this kind of behavior as displacement activity—one does what one can when one cannot do what one should do!

Complexity being what it is, this is not necessarily a criticism of the state of our science. It is no more a problem than the relativistic limit on the speed of light or the second law of thermodynamics' prohibition of perpetual motion machines. Complexity is becoming better understood and increasingly appreciated as being supremely frustrating to the kind of scientific analysis to which cognitive neuroscientists aspire. However, it is important that we realize the implications of the inaccessibility and noncomputability of the neuronal network. These constraints have profound implications for theory and practice in cognitive neuroscience, the way it has developed, and how it will continue to do so in the future.

However widespread is the acceptance among cognitive neuroscientists of this second part of the ontological postulate—the mind is an emergent factor from the interactions among the vast number of neurons[7] that make up the brain—it must also be reiterated that there is no proof of it, and it has to be considered as an unprovable assumption rather than a provable fact. There is no empirical proof in which necessity and sufficiency of the network have been proven; nor is there any computer simulation that exhibits any of the properties of consciousness.[8] Although we cannot prove the second part of the ontological postulate, there is no plausible alternative explanation available at the present time, only details of how neurons might interact at local levels or speculations about the overall nature of the network in the style proposed by Koch and Greenfield (2007). This is why it is presented here as an ontological postulate or presumption, plausible and reasonable, but not proven and probably not provable.

This second part of the basic ontological postulate is critical (along with the practical limits of what we can do) in determining not only the nature of our theories but our day-to-day activities in the laboratory. The elusiveness of empirical answers to the question of the essential level of analysis is the basic reason that the mind-brain problem (how does the brain make the mind?) remains unanswered and why there is such an abundance of questionable theoretical speculation and flawed empirical research in this field.

Unfortunately, the presumed level of brain activity (the interactions among a vast number of neurons) at which we believe the salient information processes are carried out that become sentience, consciousness, and mental activities of all kinds is exactly the level at which our research techniques are least adequate; the most fundamental reason, as noted, being the extraordinary complexity and numerousness of the involved neurons and the idiosyncratic nature of every neuron-to-neuron interaction.

As a result, neuroscientists have turned to other techniques to provide grist for their theoretical mill. All of the most frequently used methods epitomized by the fMRI or the

EEG, however, share a common difficulty—they pool the responses from the many neurons that make up the brain into cumulative, global measures. In doing so, they lose all of the critical information about the neuron-to-neuron interactions that make up the crucial activities of the brain. All, therefore, are not operating at the level of analysis at which the predominant current opinion suggests is the one at which mind and brain activity are most closely associated—the details of the interactions between myriad individual neurons of the brain. It is a practical problem—there are just too many of them—that restricts this approach.

Another main technological alternative is the use of microelectrodes to study the action of individual neurons. This method has been a powerful tool in helping us understand the nature of the components of the neural networks of the brain (the individual neurons) but from the very narrow perspective of the one-micron-wide point of a microelectrode. However microelectrodes offer little information about the interactive organization of the great numbers of neurons that are involved in even the simplest thought. Indeed, the ability to record from a single electrode has driven a major theoretical tradition based on the idea that single neurons can encode complex cognitive processes. This theoretical hypothesis seems also to be based on a flimsy empirical foundation.

In summary, these two ideas—the general first part of the ontological postulate stating that the mind is a function of the brain and the second more specific part that it is the detailed pattern of neuronal interactions that represents or encodes mental activities and processes—with all of their uncertainties seem to be our best current answers to the mind-brain problem.

It should not be inferred that these two postulates are merely topics for philosophers to mull: they exert an enormous influence on the development of theory and the choice of experimental protocols. By so specifying the relevant level of analysis, we can see that two corollaries immediately emerge. First, assigning mind to very complex neural interactions suggests that almost all of the work that has been done and can be done in the future using brain images is aimed at the wrong level of analysis. Because the old phrenological idea of localized cognitive process encoding modular cognitive processes also seems to be on its last legs, it can be expected that attempts to correlate cognition[9] with brain images will also diminish just as the enthusiasm for the EEG as an entrée into the mind has moderated over the years. Rather than these cumulative, pooled, and integrated signals, we should be attending, if we could, to the detailed patterns of activity of a myriad of neuronal interactions.[10]

Second, the computational and combinatorial aspects of the neuronal net hypothesis suggest that the problem as posed by the ontological postulates is intractable. The best that can be hoped for is that there will be some neural correlates of cognitive processes observed with these integrated global measures that may serve as useful biological markers in certain restricted situations (box 1.2).

To summarize, the main point made here is that a priori no macroscopic brain imaging or electrical recording activity, no matter how direct it may seem to be in recording the activity of the brain, can *in principle* provide solutions to the mind-brain problem. The basic

Box 1.2

Two Corollaries of the Basic Ontological Postulate

1. Brain imaging techniques are formulated at the wrong level of analysis and thus cannot provide answers to the mind-brain problem.
2. The neuronal network approach is computationally intractable and thus cannot provide answers to the mind-brain problem.

reason for this conceptual barrier is that the current technology used by these methods is operating at the wrong level of analysis. Whereas brain imaging produces cumulative signals, the essence of the mind is more likely to be found in the microdetails of idiosyncratic neuronal interactions. This clash is between the innumerable states of a discrete network and a single cumulative state in which all of these microscopic activities have been pooled at the cost of great information loss.

In the section that follows, I show how these basic postulates can have a far-reaching effect on our thinking about the relation between the mind and the brain.

1.3 Implications of the Ontological Postulate

This section is concerned with the implications of the two parts of the ontological postulate; first the brain is the organ of mind, and, second, the level of analysis at which that equivalence is manifested is to be found in the details of the great network of interacting neurons. Of the first part, little more need be said. One is either a monistic physical materialist or one is a nonmaterialist dualist.[11] If one denies the idea expressed in this part of the postulate, then the whole enterprise of cognitive psychology is a meaningless and uncontrollable mess. For purely theoretical reasons, the second part of the ontological postulate should be the focus of the entire cognitive neuroscience enterprise. That it is not is due to the practical problems dealing with its great complexity.

1.3.1 Implication for Philosophy

Dualism comes in many guises: one can resort to theology or such traditional philosophical concepts as Descartes's substance dualism, Geulincx's and Malebranche's occasionalism, or Leibniz's parallelism, on the one hand, or turn to more modern ideas such as Eccles's tripartite reality, Chalmers's naturalistic, or Kripke's versions of dualism, as well as certain interpretations of Davidson's supervenience.[12]

The adoption of any of these dualistic stances by some philosophers, although this point is likely to be disputed, is inconsistent with the whole motivation behind cognitive neuroscience. It seems completely illogical to expect that one could carry out experiments attempting to study the mind-brain relation guided by the presumption that they represented two different kinds of reality. To suggest that mind and brain, in fact, are not causally or otherwise intimately related to the degree of identity or equivalence would invalidate the very essence of cognitive neuroscience.

This then brings us to the second part of the ontological postulates. Having accepted the proposition that the mind and the brain are two parts of the same basic reality, what more can be said about the specific nature of that relation? The answer to this query, the second part of the ontological postulate asserts, is that they are related in the way that a mechanism and its function are related. In this particular case the ontological assertion is that the salient mechanism is the intricate interconnection pattern of the myriad of neurons that make up the great networks of the brain. This level of analysis is complex and is probably beyond analysis and specific explanation for the reasons I have already mentioned. Indeed, it may represent an intractable problem that neither new measuring devices nor computational engines can ever begin to unravel. There are too many uncertainties, too many neurons, too many idiosyncratic interconnections (e.g., the brain is not neatly organized as is a simple crystalline structure) for us to ever be able to understand its detailed organization and how, specifically, this complex information pattern produces the reality we call mind.

This approach has profound implications for understanding what cognitive neuroscience has achieved and what still remains mysterious and unknown. If we are to accept the second part of the ontological postulate, then almost all of the other approaches to studying the relation between the mind and the brain are being carried out at the wrong level of analysis.

For compelling practical and historical reasons neuroscientists have turned to other more global measures such as the EEG, the ERP, and most recently the brain imaging procedures such as fMRI and PET. All of these methods, however, share a common difficulty—they pool the responses from the many neurons that make up the brain into cumulative, global measures. In doing so, they lose all of the critical information that makes up the salient activities of the brain. All, therefore, are not operating at the level of analysis at which the predominant current opinion suggests is the one at which mind and brain activity are most closely associated.

If this analysis is correct, then all of the work using the molar, integrated, cumulative measures of brain activity is misdirected, and the resulting findings must be considered to be irrelevant in the search for solutions to the mind-brain problem. This is as serious an impediment to scientific understanding as acceptance of dualism would be.

There is implicit in these comments another important and essential point—the fact that it is the pattern of information transactions, not the biochemistry of individual neurons, that accounts for the emergence of consciousness or mind. For example, although we know a lot about the biochemistry of the neuron and of the synapse and can explain the details of the transfer of information along an axon or from cell to cell, these are properties of the microscopic components of the nervous system; the particular technology is not essential to an information-processing system's function. By themselves, therefore, these properties tell us little about the mind-brain problem—only about the details of the particular biochemical mechanisms of the components of which the brain is made. Just as a material from which a transistor is made tells us nothing about the program that is being run on a computer, even the most detailed knowledge of the biochemistry of a neuron tells us nothing about how the overall system is representing mental processes. In some unlikely ideal world, should we be able to build a brain-like structure capable of mimicking all of the complex informational processes and interactions, albeit with a completely different technology than the sodium-potassium-chloride chemistry our brains use, such a system would presumably be able to "think" or "perceive" as well as we do and may even be "conscious." The point is that it does not matter what component technological units are being used; only the arrangement and interactions of those elements are of consequence in representing "mind."

This, then brings us to the next step in this preliminary philosophical study of the nature of mind-brain reality—that of the epistemology of cognitive neuroscience. That is, how can we know (i.e., what strategies can we use to learn about) the critical mind-brain interactions. Epistemologists have different goals and consider different topics then do ontologists. Rather than contemplating what is, they are concerned with the limits on our ability to know what is; that is, given postulates such as those proposed by the ontologists, what does this mean to our ability to study the mind-brain problem (among many others) and to understand, to learn, to describe, or to explain the properties of the mind and the brain. In other words, what is it logically possible for psychologists to do given the ontological postulates as starting points?

Few practicing experimental psychologists or cognitive neuroscientists struggle with such questions. They go about their various projects questioning only how data can be gathered or explained. Their epistemology is an empirical one; decisions are made on the basis of what works or what appears to work—their epistemology is an unredeemably practical one. Nevertheless, they, too, are bound by the epistemological constraints. More or less implicitly, without overt awareness, all cognitive neuroscientists and psychologists constantly make certain practical assumptions within this context of unspoken and implicit epistemological conundrums about what they can learn from their experiments.

In the following pages I distinguish between those epistemological issues that guide psychology and those that guide the neuroreductionist efforts of cognitive neuroscientists.

1.3.2 Implications for Psychology

To understand how the two ontological postulates affect the course of scientific psychology, it is important to express a major epistemological principle (box 1.3). Before dealing with the impact of this postulate on psychology, I must consider a very special idea—accessibility and its antithesis, inaccessibility. The issue being dealt with here is how much access do we have to mental processes? Can we gain access to (i.e., measure) the nature of mental

Box 1.3

The First Epistemological Postulate for Psychology

> Mental processes are private and are not accessible to any form of measurement, either experimental or introspective.

processes, or are we forever constrained from any direct measurement of the mind? Let me now spell out in detail some of the arguments for both of the two interpretations.

Arguments for Inaccessibility

The argument for the inaccessibility of the mind revolves around the fact that the mind, by definition, is a private personal experience. Since there is no detailed explanation (beyond the second part of the ontological postulate) of the relation between the physical structure and processes of the brain and our individual and private experience of being, sentience, or awareness, there is no instrument that we can attach to or scan the head that will directly measure mental processes. As many psychologists have written previously, mind is an *intrapersonal* and not an *interpersonal* phenomenon. The only possible way that we can begin to get any, however defective, insight into the mind of another person is by means of that individual's introspective reports or by observing that person's behavior.

However, both introspection and drawing inferences from behavioral observation are well known to be deeply flawed methodologies. Researchers such as Nisbett and Wilson (1977) had shown four decades ago that people are not aware of their own logical processes and mental strategies. The reasons for this introspective blindness are manifold but include false memory construction (Loftus, 1996) and the automatic (i.e., unconscious or preconscious) nature of many behavioral processes (Bargh, 1997). Whatever the reasons, and there are many others, it is clear that people cannot always accurately report the logic or reasons they used to arrive at a decision. For some phenomena, for example those associated with the basic qualitative state of such experiences as color or pitch, it is not possible to reconstruct with words what it is that a person is experiencing. Introspection, therefore, must be ruled out as an effective means of accessing mental states.

The main alternative means of accessing mental states is to draw indirect inferences from publically observable behavior. However this strategy, too, is deeply flawed. The reasons behind this assertion are equally numerous and include these:

- There is a well-known engineering principle (that holds for human behavior as well as pieces of electronic equipment) that the inner workings of an unopened "black box" cannot be determined by comparing the box's input to its output. The relation between input and output cannot in principle tell us anything definitive about the functional changes that may be occurring in the box. This is well known to engineers and should be to psychologists.

- The reason for this generalization is that the mechanism inside either a piece of hardware or the human mind is underdetermined by the behavioral observation. That is, there is not enough information in behavior to precisely and uniquely determine inner mechanisms. There are many possible (and far too many plausible) explanations for each behavioral observation. No convergence of observations can lead to an answer to the problem of internal structure. Indeed, additional observation often leads to even more plausible and possible explanations than had been contemplated originally.
- Human behavior is characterized by enormous variability when compared to other sciences. Individual results are not sufficiently repeatable. Therefore, there remains a serious question concerning the reliability as well as the validity of many psychological measurements.
- Human behavior is not bound by robust, stable, universal laws of time, space, and number in the way physical phenomena are. There is, therefore, no way that an entirely external observation can be associated with an inaccessible experience. Physics can generally do this, but its success depends on the assumption that the laws of physics are the same everywhere—in the internal microscopic world as well as distant macroscopic universes.[13]
- Behavior, as expressed in the literature of experimental psychology is not adequately linked to the associated mental activities. People can intentionally or unintentionally display behavior that is quite contrary to what they are really thinking. Questionnaires, stage plays, and the courtroom all present examples that illustrate how separated one's thoughts can be from one's utterances. Even the best experimental protocols do not provide robust constraints or necessarily even plausible links between behavior and the underlying thoughts.
- Mental faculties and components are "hypothetical constructs" (MacCorquodale & Meehl, 1948) created by psychologists to describe behavior and, therefore, may not exist in some physical or psychobiological sense.
- Finally, the complexities of both behavior and the neuronal mechanism are so great that there is no computational way in which they can be linked. There is no one-to-one correspondence among measurable behavior, mental activity, and brain responses.

Arguments for Accessibility

The honorable epistemological opposition argues that these arguments are too stringent and demanding and that much is lost when we limit ourselves by assuming mental inaccessibility. Although the words may differ in the many arguments, the ubiquitous core argument for accessibility is that consciousness exists, and without assuming accessibility we would be denied any hope of measuring and explaining it. By denying accessibility, they argue, we lose one of the main raisons d'être of psychological science as well as basic matters of our own humanity. In any event proponents of accessibility argue that the links between mind and behavior are solid enough for us to draw good inferences.

Herein lies the core of the greatest debate in psychology—the one between behaviorism and cognitive mentalism. It is here that the empirical and theoretical strategies of these two great schools of psychological thought diverge. My argument in the present context is that they so diverge primarily because of their differing stances on the epistemological question—are the process and activities of the mind accessible?

The issue has been debated for many years, and the opposing answers to it are based on beliefs and assumptions more comparable to ontological and epistemological speculation than on any empirical evidence. On one side of the debate are those who traditionally have been called mentalists and more recently cognitive mentalists. Mentalism is based on an initial epistemological assumption asserting that mind is sufficiently directly accessible to be studied by introspective or experimental assay techniques. Behaviorists, in opposition, accept that the mind is not directly accessible and, therefore, that we can only observe and measure the final outcome of mental activity—behavior. From that point the respective strategies of the two approaches to the study of the problems of interest to psychology are set in a conceptual concrete.

The arguments for and against behaviorism and mentalism, respectively, can be summed up in the following brief lists abstracted from my earlier work (Uttal, 2000).

The Essential Arguments against Mentalism

1. There is a lack of public availability, objectivity, and repeatability for metaphysical or mental processes.
2. Mentalism leads to homunculus or infinite regression arguments.
3. Mentalism produces unprovable hypothetical constructs.
4. The empirical data argue against the accessibility of mental processes.
5. Mentalism requires complex experimental designs and unprovable assumptions that produce fragile data.
6. Mentalism arises because of the vested interests of its humanist, theological, and personal protagonists or from the professional needs of psychotherapists.

The Essential Arguments against Behaviorism

1. There is only a limited range of behaviorist psychology.
2. Behaviorism dehumanizes humans.
3. Behaviorism is too “mechanical” or is “not sufficiently mechanical.”
4. Behaviorism is not a step forward.
5. Behaviorism overemphasizes the environment and underemphasizes heredity as a source of behavior.
6. Behaviorism is nothing more than common sense.
7. Behaviorism is antidemocratic.
8. Behaviorism is antireligious.

Clearly, none of these arguments is compelling by itself. They all depend in large part on an original decision to accept or reject accessibility. Having said that, it is important not to try to finesse this issue, but, instead, to take the bull by the horns and make a value judgment. In my opinion most of the arguments against behaviorism and its attendant inaccessibility are wishes and hopes rather than scientific arguments. They dote on humanistic judgments about the desirability of understanding the human condition.

The arguments against mentalism, on the other hand, have a greater degree of scientific robustness and support (or are supported by) the idea of inaccessibility. Without any question, however, it is the acceptance or denial of the epistemological postulate of accessibility that directs and guides one to either behaviorism or mentalism. My opinion is that psychology would be better off scientifically being behaviorist rather then mentalist. I go so far as to make it an epistemological postulate asserting my preference for behaviorism (box 1.4).

There is another way in which these two postulates impact on thinking in psychology. Because, according to the second part of the ontological postulate and the first epistemological postulate, the brain level at which mind is embodied is that of the great and unanalyzable neural network and that mental processes are not directly accessible, there are few constraints on how we might assume the mind-brain to be structured. Coupled with the great complexity of the system, this means that we are relatively unfettered in making certain further assumptions concerning the nature of the organization of the mind-brain. Two of these assumptions stand out in the history of psychology—separability and analyzability. Because we cannot deal with the whole complex system with all of its interacting parts and variables at once, we fall back on Descartes's admonition to break the system into parts. This leads to two extremely potent, but highly questionable, governing assumptions. The first is that the mind is modular; that it is made up of quasi-independent units—the faculties and processes that are explored in conventional psychological experiments. The second is that the neural equivalents of these mental modules are located in particular parts of the brain.

Throughout the history of cognitive psychology and its predecessors, up to and including the early days of the brain imaging movement, experiments have been based on these two assumptions. The search was on—find the areas of the brain that were activated by such cognitive processes as "solve a problem," "decide which candidate you prefer," "think about a loved one," or "think about a cow." As the discussion in this book progresses it will become clear that modularization and localization are no longer tenable interpretations. In their place two alternative statements must be substituted. First, mental components cannot be

Box 1.4

The Second Epistemological Postulate for Psychology

Psychology is better served by a behaviorist approach that dotes on the observable parameters of human activity rather than the inferences of a reductionist mentalism.

analyzed into independent and separable cognitive modules; to do so in a Cartesian sense belies the interactive complexity of our thoughts. It is likely that we will begin to realize the mental modules represent a convenient organizing principle but do not necessarily reflect the actual nature of our mental activities. Because compelling evidence for neither modularization nor holism is yet available, I also characterize this idea as an epistemological postulate (box 1.5).[14]

1.3.3 Implications for Neuroscience

Just as the choice of one's theoretical psychological stance depends on certain assumptions about the accessibility and modularity of the mind, the choice of one's neuroreductionist stance depends on the second part of the ontological postulate—that the instantiation of the mind is to be found in the actions and interactions of the many neurons of the great networks in the brain.

There is rapidly accumulating empirical evidence that the range of brain regions involved in even the simplest thought is widely distributed throughout the brain as summarized as The First Epistemological Postulate for Neuroscience (box 1.6). This assertion, however, is much less speculative and represents the first of the postulates driving neuroscientific research.

As these holist ideas (the mind must be treated more as a whole than as a system of separable modules, and the brain activities associated with a thought are widely distributed) have increasingly begun to percolate into experiment and theory, the epistemological situation has gotten much worse. A diffusely distributed system is not conceptually simpler than a system of discrete nodes. Furthermore, distribution complicates the search for an objective neural correlate of any behavioral activity. Since multiple regions are involved, distribution has led to the use of complex pattern recognition analysis methods that were far more challenging and the results of which were far less certain than those based on the

Box 1.5

The Third Epistemological Postulate for Psychology

> Although convenient as a means of experimental protocol simplification, mental processes are not modular and cannot be divided up into quasi-independent entities.

Box 1.6

The First Epistemological Postulate for Neuroscience

> Brain activity associated with mental activity is broadly distributed on and in the brain. The idea of phrenological localization must be rejected and replaced with a theory of broadly distributed neural systems accounting for our mental activity.[15]

simple modular and localizationist ideas of the past. Some investigators (e.g., Hilgetag, O'Neil, & Young, 1996) have suggested that the situation is actually much worse than just being "more complicated." They argued that the analysis of a heavily interconnected system of cooperating and interacting regions in the manner cognitive neuroscience may require might not be possible in fundamental principle. Instead, they argue that the situation would get increasingly complicated (not less so) as more and more experiments are carried out.

Brain images, it must be clearly understood, still provide us only with the capability to search for highly variable locales of activation that may be associated with vaguely defined cognitive processes. It is important to remember that no matter how complex the analysis, brain images essentially search only for answers to the "where" question. The essence of the mind-brain problem, however, is still the "how" question, and it is not yet clear just what the "where" question tells us about the mind-brain problem.

Just as it is necessary to invoke an additional epistemological postulate in order to understand the roots of psychology, it is also necessary to invoke another basic assumption to define one's approach to neuroscience. In the case of psychology, as I noted earlier, the additional epistemological assumption concerns the inaccessibility of the intrapersonal events we designate as mental. Depending on one's choice, it was a more or less logical progression from the respective assumptions of accessibility or inaccessibility to the kind of empirical and theoretical research to be pursued. If one accepted the intrinsic arguments for inaccessibility, the challenges to cognitive psychology were not just practical but of deep principle; inaccessibility denied even the hope of a remote future in which we might find some way to even indirectly measure the attributes of the mind.

The comparable supplemental epistemological assumption underlying modern cognitive neuroscience, however, is a practical one rather than one of deep principle. It is the respective answer to the query—is it possible to measure or examine the details of the neural network that is the basic psychoneural equivalent of mental activity? This supplementary epistemological postulate for neuroscience can be formalized as shown in box 1.7. Although this postulate may well run counter to the current Zeitgeist, a strong argument supporting this postulate can be made based on combinatoric arguments.

Inherent in any such postulate, of course, is the possibility that, at some unforeseeable future time, unexpected developments may make possible what is currently impossible in

Box 1.7

The Second Epistemological Postulate for Neuroscience

> Because of their great complexity and number, it is not possible for us to analyze the great neuronal networks of the brain in a way that would permit us to identify the neural equivalent of any kind of mental activity at this microscopic level of analysis.

practice. Nevertheless, the reality today is that there is no practical way to analyze such a complex and irregular network, and some mathematical arguments concerning combinatorics and computability strongly suggest that this is a reasonable working rule for the near and perhaps even for the far-distant future.

What are these arguments supporting the second epistemological postulate for neuroscience? Some of them are these:

- The kinds of network problems that are observed in the brain are known to be computationally intractable *in practice*. They are not infinitely complex (which would introduce an *in principle* constraint) but merely so consuming of any conceivable computational power that they could never be solved. This is equivalent to what complexity theorists call an NP complete problem, a problem that cannot be solved in any determined amount of time.
- Few of our neural network simulations scale up. That is, the simple models we are able to program onto a computer typically fall apart or saturate in one way or another when we try to increase the number of interacting simulated neurons beyond a few hundred.
- Efforts to simplify the difficulties inherent in these problems (e.g., by assuming regularity, adding additional nonbiological constraints, or by breaking it up into smaller parts) do not work.[16]

There are really no good counterarguments to these practical constraints on understanding the neuronal basis of the mind. What actually happens is that investigators implicitly accept the limitations and then turn to alternative experimental and theoretical strategies. They implicitly accept the monumental barriers to the direct evaluations of these complex neuronal networks and utilize whatever measuring devices are available (e.g., the EEG or the fMRI), whether or not these devices are operating at the appropriate level of analysis. In so doing, often without realizing it, they are accepting the unanalyzability of the neuronal network—the second epistemological postulate for neuroscience—and opt for some alternate, but questionable, strategy that does not even promise to answer the essential mind-brain problem. It is important to point out again that this does not imply an “in principle” rejection of the second ontological assumption; instead it is simply a practical and necessary response to the fact that studying the microdetails of the neuronal network is not an effective strategy.

1.4 Some Relevant Conceptual Issues

1.4.1 The Seductive Attractiveness of Brain Images

A major issue in cognitive neuroscience concerning the use of brain images such as the fMRI is that their impact on our science may be far more than they deserve. That is, we are seduced by the pretty pictures and the seeming “face validity” that these images seem to offer. For example it is now established (McCabe & Castel, 2008) that people are more likely to accept the credibility of a published report when a brain image rather than an informationally equivalent graph or table is used. Roskies (2008) referred to brain images as perpetuating an “illusion of inferential proximity” that makes us feel we know something about something that, in fact, actually remains inscrutable.[17]

Weisberg, Keil, Goodstein, Rawsdon, and Gray (2008), arguing in a similar vein, suggested that neuroscientific information itself, even if it is not relevant, made a theory more acceptable than when that kind of information was withheld. This effect was maximum when the judging subjects were not expert in the field beings discussed. These authors warned especially against the problems posed for nonexperts in evaluating neuroscience information added as decorations to scientific story.

The point is that the attractiveness and the seeming, but illusory, directness of these images give them a conceptual and scientific impact that they may not entirely deserve. Their charm, their novelty, and their pictorial splendor tend to overwhelm critical consideration of the serious epistemological issues revolving around the limits of what these images can actually tell us. It is only in recent years that the empirical facts have begun to raise further questions about some of the facile misinterpretations of their meaning.

It can be argued that the widespread and uncritical acceptance of the brain image as a measuring tool of cognitive processes is based on a widespread misunderstanding of the actual progress that has been made in linking results from the two fields. Although cognitive neuroscience journals have been flooded with publication of what are often very preliminary reports, the neural basis of cognition and the neural activity depicted by brain images operate at vastly different conceptual levels. We do not yet understand what either of these differences means or how we might link them together.

In short, there is no theory or putative explanation that yet explains how mental processes emerge from neural ones. There is, instead, an emerging corpus of scientific opinion that the mind-brain problem is intractable due to the complexity of the neural interconnections that actually lie at its core. The misunderstanding that we have made more progress on this fundamental issue than we have is also exacerbated by the hyperbolic and exaggerated popularization of very preliminary or unsubstantiated scientific findings by the press and the lay community.

1.4.2 The Problem of Defining Mental Processes

One of the most serious impediments to unraveling the mind-brain problem is that mental states are very difficult to precisely define. In fact many of the cognitive processes that we wish to correlate with either surgical interventions or brain images are merely neologisms for experimental results or hypothetical constructs used to flesh out some speculative psychological theory. To compare the objective neurological data with such poorly defined, and often arbitrary, mental entities stretches logical analysis to its limits. The actual connection is so loose that it is all too easy to carry out what are, in retrospect, misleading comparisons. It is also possible in systems as complex as this to find empirical support for almost any theory.

The problem is that the history of psychology is filled with a huge vocabulary for a large number of different psychological faculties, traits, or modules. Yet there has been no clear development of a coherent taxonomy or classification system for psychology comparable to the Linnaean one in biology or Mendeleev's in chemistry. Instead, idiosyncratic and obscure terms come and go as psychologists suggest new hypothetical entities, study them until they are no longer of interest, and then move on to some other topic. Words like "acquisitiveness" or "ego" have now been replaced by such equally vague concepts as "consciousness" or "attention." Only operationally defined terms such as "reaction time" or "percent correct" tell us anything and then only about the behavior of the organism.

The issue of definition becomes extremely vexing when a comparison is made between a mental activity and a brain response, especially if it is uncertain just what mental processes are being invoked and how such obscure processes as "attention" and "perception" actually are different or independent of each other (if they are). The point is that it becomes difficult to locate in the material brain what are little more than hypothetical constructs or tags attached to experimental protocols.

This issue raises serious practical problems of interpretation in any mental process-brain response comparison. For example, a study purportedly of people's preferences for a cola or a political candidate may end up measuring a brain response that has nothing directly to do with our preferences per se but may be measuring some subtle aspect of a general emotional response or of some previous experience. This misdirection to an irrelevant aspect of the cognitive state plays havoc with any attempt to use a brain image as an indicator of mental activity as well as any effort to develop a coherent theory of mind-brain relations.

In a more general sense, it raises questions about the validity of any purported neural measure of any cognitive process. If there is always the possibility (because of poor definitions) that we are measuring something other than what we thought we were, no matter how reliable the findings, those findings may be theoretically meaningless. In short, poor definition of mental entities degrades the validity of any neural correlations with those nebulous cognitive processes. To note that it also makes independent stimulus control more difficult is simply to restate the obvious.

1.4.3 The One-to-Many Issue

It is becoming increasingly clear that many different cognitive processes can activate the same area or system of areas of the brain. (For example, see the work of Culham & Kanwisher, 2001.) Thus, if the available findings are limited to answers to questions about "where" a response is occurring, it is theoretically impossible to exclusively associate any particular brain activation site or pattern of activation with any particular cognitive state. In the words of Poldrack (2006), it is extremely difficult because the putative location of a cognitive module is not unique, to use "reverse inference" to assign specific mental meanings to even the most discrete and reliable brain activations. Any attempt to do so, according to Poldrack, is “deductively invalid.” He goes on to say that it “still can provide some information,” but this depends on the empirical “selectivity of activation” (p. 59).

This is a very important, but largely overlooked, point. It raises severe limitations for any attempt to “read a person’s mind” by measuring brain responses. First, multiple functionality of single brain regions disassociates specific brain responses from particular cognitive processes as a matter of principle. That is, activation in any particular brain region cannot be solely assigned to any particular cognitive process when that brain area is involved in representing many different cognitive processes.

Second, the one-to-many problem adds to the practical difficulty of assigning either qualitative or quantitative significance to what are often only modest correlations in brain image-cognitive process comparisons. No matter how carefully an experimenter controls the salient experimental variables,[18] there must necessarily always be other forces operating to modulate the response of a given brain region. Efforts to use brain imaging in legal proceedings as a “lie detector” to mitigate culpability are invalidated from the outset by this principle.

1.4.4 The Many-to-One Issue

Just as the fact that many psychological tasks and stimuli can simultaneously activate a single brain region (and, therefore, we cannot in principle say that any particular neural activity or place is a unique indicator of any particular kind of mental activity), it must also be remembered that it is likely that many behaviors or cognitive processes may be instantiated by a number of different and redundant brain mechanisms. We have little knowledge about the full range of brain regions that may be equivalent or substitutable for each other. However, we do have plenty of evidence that many different regions of the brain are activated during any kind of cognitive task. Furthermore, we also know that under extreme conditions (such as damage due to ischemic stroke) some regions of the brain are capable of taking over functions of damaged regions. Whether or not this redundancy under the extreme conditions of a stroke is also implicitly or explicitly present under normal conditions remains an important question for cognitive neuroscience.

The potential for redundant representation strongly suggests that cognitive processes need not be encoded by the same neural mechanisms in different people. Just as there are different cognitive strategies to solve a particular problem, it is probably the case that many different brain regions or clusters of brain regions may account for a particular behavioral outcome. This is what we refer to as the many-to-one principle. This is also what is meant by the general underdeterminative nature of behavioral responses—behavior cannot tell us what brain mechanism is active just as activations of brain regions do not tell us which mental process is active. Behavior by itself is neutral with regard to underlying mechanisms.

This limitation on our understanding is also known, as I have discussed earlier, as the “black box problem.” To know precisely what mechanism is inside the black box, one must open the box. Unfortunately, for mind-brain theorists, even after imaging the brain, the complexity of the system and the variability of the responses are so great that our path to understanding is blocked by another kind of virtual “closeness”—complexity.

1.4.5 The Sign-Code Distinction

Over the years (starting with Uttal, 1967) I have repeatedly pointed out that there are two possible meanings—signs and codes—of the correlated neurophysiological responses that are obtained when one compares brain activity to cognitive processes. A “sign” is a correlate of brain activity that indicates that something is happening neurophysiologically, but it is only a candidate to be the “psychoneural equivalent” of the associated mental activity. That is, a sign may be correlated neural activity in response to stimulus or mental task. However, it has not been established that it is *the* neural activity that specifically results in or *is* the cognitive experience itself. It does not encode, represent, or in any way is it the equivalent of the mental experience. All that a sign does is to tell us that there is a recordable brain response to some stimulus or cognitive state.

A sign may be used in powerful ways to measure some property of brain activity and possibly even someday serve as a biomarker of some dysfunctional cognitive activity. However, it does not necessarily explain or represent the mechanism by means of which brain activity is transmuted into mental activity. In short, the concept of a sign reminds us that not all neural responses recorded from the brain are psychobiologically relevant.

On the other hand I have designated a correlated neurological response that *is* the psychoneural equivalent of some mental activity as a “code.” A code is a measure of neural activity that is the actual mechanism of whatever cognitive process is being manipulated. It is the necessary and sufficient mechanism, not merely a concomitant or correlated sign, of some mental activity. In short, it is the neural activity whose activity *is* the mental activity.

Distinguishing between a sign and a code is not an easy task given that either may correlate highly with brain activity. To determine that something is a code requires that we prove both its necessity and its sufficiency, an empirical task of considerable difficulty. Clearly, because the requirements for a code are so high, there are very few that have been robustly identified in the cognitive neuroscience literature beyond the transmission codes of the sensory and motor systems. The study of higher-level cognitive processes remains virtually untouched by such progress.

It now seems clear that most of the molar,correlated brain responses, whether they are EEGs or fMRIs, are signs. In fact, as some scholars have pointed out, brain images tell us little more than that there is some brain activity when our minds are active, an idea that is hardly surprising given the ontological postulates discussed earlier. However, as far as specifying the specific neural processes that are the coded equivalents of mind, virtually all such cumulative measures of brain activity are bankrupt. The persisting question is—what does knowing what part of the brain is activated by some stimulus or task tell us about how that part might encode mental activity?

1.5 Some Relevant Technical Issues

The conceptual, occasionally philosophical, issues discussed in the previous sections raise serious questions about the applicability of brain imaging devices to the measurement of cognitive processes. There are, in addition, a number of purely technical issues that complicate the matter further. These issues and challenges arise not because of any subtle logical or epistemological uncertainties but because of well-documented and tangible issues with the day-to-day details of using brain imaging devices to conduct research on cognitive processes. The technical issues collectively also provide serious challenges to any optimism to the use of brain imaging as a means of evaluating such poorly defined psychological faculties as learning, attention, perception, personality, thinking, intelligence, level of learning, decision making, or other complex, high-level cognitive states. The following paragraphs discuss some of the technical issues that still bedevil research efforts to correlate brain images and cognitive states.

1.5.1 Cumbersome Procedures

However beautiful a colorful brain image may eventually turn out to be, it is the final result of a massive investment in time and money. The PET system, for example, not only requires the detector system itself, but also a radioisotope- generating capability to produce the injectable radioactive materials and a computer facility to process the raw data from that detector. (The medical and ethical issues of using such an invasive procedure, furthermore, should not be minimized.)

The complexity and expense of MRI systems are also well appreciated, but the major issue of invasiveness associated with the PET procedure is largely overcome by MRI systems. Functional MRIs are totally noninvasive; no one has ever shown any deleterious physical effects from the large magnetic fields used to orient the protons of the body's atoms other than being hit by an errant piece of metal attracted by the powerful magnetic fields surrounding the device. Nevertheless, there are a number of practical issues in their use that also make the process cumbersome, complicated, and expensive.

It takes an extended period of time to produce a single fMRI image with most current techniques. Furthermore, subjects must cooperate to an extreme degree including remaining motionless and attending to a single cognitive theme for the duration of the measurement in what can be an acoustically noisy and highly constricted environment. Even the slightest head or respiratory movements can distort the final image (Raz et al., 2005). Furthermore, because of the extended time required to collect the data in an fMRI-based experiment, the number of subjects is usually relatively low compared to behavioral evaluations.

Perhaps most significant, however, is the remarkable lack of control over the cognitive tasks that serve as the independent variables in experiments comparing cognitive states and brain images—a lack of control that is also the bane of many psychology researchers. It is not always possible to know that the instructions to carry out a specific cognitive activity are being followed. Both effortful (is the subject trying to play some other game than that designed by the experimenter?) and inadvertent (did the subject not understand the instructions?) actions can lead to a lack of control of both the experimental and control conditions.

1.5.2 The Subtraction Issue

Although there have been recent improvements in quantification of the responses obtained with brain imaging devices, the basic research paradigm remains: determine how the brain response differs when a subject is thinking about "nothing" (the control condition) and when the subject is carrying out a specified mental task (the experimental condition). Examples of the latter are making a decision, carrying out mental arithmetic, or thinking about a specific object such as a "cow." The general procedure is to subtract the control condition ("don't think about a cow") from the experimental condition ("think about a cow"). The idea is that only the salient aspects of the response will show up in the difference image; all irrelevant and unchanging portions will disappear because they remain the same and sum to nothing. Despite the array of modern statistical approaches and analytic methods for producing a brain image, in the final analysis it is the subtractive difference between the control and experimental conditions that is the methodological kernel of this or, for that matter, any other cognitive neuroscience experiment using imaging techniques. The frailties of this kind of subtractive logic have been more or less obvious to many investigators in the field since Van Orden and Paap's (1997) cogent criticism, but many still ignore its basic limitation. Most important of all is the fact that the resulting brain images are themselves the cumulative activity of uncountable numbers of neuronal responses. Thus, the observation that an area may null out and leave no trace in the difference image does not mean that its detailed activity was the same in both the experimental and control conditions. The nature of the underlying neuronal network state may change considerably and still produce zero difference scores. Furthermore, the logic of the subtraction process can be subverted because it is highly likely that the detailed activity in two sequential (i.e., the control and experimental) conditions would be different simply because time had elapsed between the two measurements.

However, there is something even more basic and fundamental at work here that should perplex anyone using the subtraction method. The assumption that the baseline conditions during the "control" condition remain stable is highly questionable. In an article that deserves much wider attention, Stark and Squire (2001) pointed out that one possible control condition—what was supposed to be the inactive rest period between experimental conditions—was highly unstable in learning experiments. That is, what were assumed to be repeatable baseline conditions that one could subtract from the experimental condition were, in fact, very variable. Stark and Squire pointed out that in some experimental protocols (those involving the medial temporal lobe among others) it was possible for the baseline rest condition to be so variable that it could actually change the sign of the difference between the control and the experimental activations.

Closely related to this problem with the subtraction process is the prevailing but erroneous idea that the brain is inactive when not involved in mental activity. The whole thrust of the subtraction process is that no change in the activation pattern indicates no difference in the salient neural activity. In a recent article Raichle (2010) summarized the earlier discovery (Binder et al., 1999; Fransson, 2006) of the "default" mode—extensive amounts of brain activity during rest. Raichle pointed out that "60 to 80% of all energy used by the brain—occurs in circuits unrelated to any external event" (p. 47). If this is so, it raises questions about what the absence of an activation measured with an fMRI machine actually means in terms of the blood oxygen level dependent (BOLD) level itself and the fundamental idea that blood oxygenation varies with neural activity in the way we thought it did. What the concept of resting or default activity further raises is that this ongoing activity is being confounded with the evoked activations! If the subtraction method is thus flawed and BOLD measurements are associated not only with stimulus-evoked neural activity but also with background activity, the whole edifice of this kind of brain imaging could be called into question.

It must also be appreciated that whatever advanced data-processing techniques (for example, multidimensional scaling or general linear models) are used to analyze the effects of a number of different variables are only methods that help to organize the data; they are not reductive analyses of the anatomy or structure of the involved brain mechanisms. (This insightful comment has been attributed to Professor Anne Anastasi.)

If one adds to this situation the problems of the loss of information when one pools data and the fact that different regions of the brain may interact by reinforcing, inhibiting, or disinhibiting another region's response, it is clear that we are confronting a tangle that at least a few of us now believe to be bordering on the inscrutable and unanalyzable. The bottom line is that the basic subtraction method is so deeply flawed that it makes much of the research using this method highly questionable.

1.5.3 The Paucity of Quantification

Another major issue faced by any investigator who wishes to use brain image responses as indicators of changes in behavior (for example, degree of learning) is that the magnitude of most brain images and measures has not yet been adequately related quantitatively to the cognitive responses these images are supposed to measure. Indeed, it may not be possible to do so. There are many discontinuities, thresholds, multiple influences, and nonlinearities that make it extremely difficult to use variations in brain image contrast as a quantitative scale of cognitive activity. Because of the many factors that can distort the amplitude of the fMRI signal, there is a paucity in the literature of results in which the magnitude of the brain image response has been used as a scale of different levels of either activation or cognitive activity. In most cases the brain image is used as an indication of where something is occurring rather than how much of that activity is occurring.

Specific calibrated values for the extent and magnitude of the brain response are regularly confounded by the arbitrary choice of thresholds at which focal responses are accepted as being biologically significant. So far, because the brain image device is primarily used to determine the location of activations, and because the brain responses are magnified or suppressed in a highly nonlinear manner, it will be extremely difficult to directly relate the amplitude of a brain response to the subjective magnitude of a perception or the degree of learning a subject has achieved.[19] The best we can do is to say that this or that brain area may be involved in some way.

A typical approach at the present time is to scale (and artificially color) a brain image based on statistical tests such as the *z*-score or the number of voxels that are at or above some criterion level. (The images may be presented as contour maps in which the color or height corresponds to intensity differences.) However, should the relation be grossly nonlinear, such a measure would lose much of its integrity as an indicator of the amplitude of a response. Such a distortion should be expected in a complex and presumably nonlinear system such as the brain. Other problems arise when one attempts to use tests of statistical significance because of their sensitivity to deviations from normality and because normality is unlikely in the processes mapped by brain imaging or EEG systems. Behavioral measures, on the other hand, are far more direct and scalable than brain responses.

A related problem concerning the use of a brain image as a measurement of cognitive function is that the degree of contrast is determined by a substantial number of different factors, some known and some unknown, that prohibit simple interpretations of the quantitative relation between the cognitive state and the neural response. The strength of the magnet, the material being scanned, and the choice of the time constant measured all can and do introduce nonlinearities that preclude simple scaling. The implication is that brain images are actually most often qualitative and rarely quantitative. In short an irregular scale devoid of a specific metric defining the interval size does not provide the necessary basis for quantification of the magnitude of a brain response, regardless of method.

It is also important to appreciate that the mere fact that numbers can be assigned to some variable does not mean that a quantitative relation exists between the numbers and that variable. An irregular interval or the absence of a nonarbitrary zero can lead to gross distortions of the meaning of a series of data points. This lack of robust quantitative scaling should affect any decision to supplement neural responses for behavioral ones in any serious way. Ignoring these limits on measurement can lead to mistaking an illusion of measurement for a quantified scale simply because numbers can be "assigned." Such a mistake would be especially significant in the context of learning; it could lead us to ignore a precise behavioral measure (for example, "percent correct") for an imprecise, seductive, and ill-quantified, variation in a brain image. Although the brain image promises to add objectivity to the behavioral measure with its panoply of high-tech equipment, it is, in fact, far more removed from that which is to be measured (the cognitive process) than is the behavioral measure.

The situation is much more complex and challenging than usually appreciated. To prove that there is a causal relation between two variables requires tests of sufficiency and necessity that are elusive. To prove that a measure is valid (actually measuring what you think is being measured) is also a demanding and difficult task. Philosophers and logicians have tangled with the problem of validity for centuries without complete success. It is all too easy to succumb to the siren call of "face validity" and to assume that *what is being measured* is *what is intended to be measured* simply because *it can be measured*.

In the context of this background the task of relating neural responses and cognitive processes for psychology is far more complex than generally appreciated. Dingman and Sporn (1964), for example, in a specific effort to consider how we might confirm that a particular molecule, neuron, or locus in the brain was the locus of the memory engram presciently proposed the following tests:

> We suggest that the following criteria must be satisfied in order to demonstrate that a given molecule, set of molecules, structure, or set of structures is indeed [the site of] a permanent memory trace: (i) It must undergo a change of state in responses to the experience being remembered. (ii) The altered state must persist as long as the memory can be demonstrated. (iii) Specific destruction of the altered state must result in permanent loss of the memory. (p. 26)

Such rigorous tests for cognitive neuroscientific relations of any kind are rarely, if ever, satisfied. Therefore Dingman and Sporn concluded that all such suggestions that there is a specific memory storage region or mechanism must be "highly circumstantial" (p. 26). Considering the way data are collected today in many comparable kinds of experiments, we must also agree that many of the reported relations using imaging techniques also remain "circumstantial."

Other authors (e.g., Martin, Grimwood, & Morris, 2000) have also described a set of criteria that they feel must be met to establish a particular neural mechanism as the site of a memory. Their criteria are paraphrased in table 1.1.

Martin, Grimwood, and Morris's criteria are even more stringent than Dingman and Sporn's. Indeed they appear to be based on the assumption that it is possible to manipulate the synaptic connectivity of a complex neuronal system in a detailed way—an assumption that is clearly untenable at present. This impossibility is especially evident in the mimicry criterion. Therefore both sets of criteria are actually inappropriate to the problem at hand. Each represents the protocol of an unexecutable Gedanken experiment, rather than a scientific plausibility.

A more subtle problem concerns the misidentification of neural activity that seems superficially to have the same shape and dimensionality as a behavioral response.

Table 1.1
Stringent criteria for establishing neural-cognitive correspondences

Detectability Some change in synaptic efficiency must be detected somewhere in the nervous system.

Mimicry If we could imitate the synaptic pattern, the memory would be the same as the real pattern.

Anterograde alteration Anything that prevents the synaptic pattern from forming should prevent the formation of a memory.

Retrograde alteration Anything that alters the synaptic pattern should change the memory.

Paraphrased from Martin, Grimwood, & Morris (2000).

Isomorphism has been used by cognitive neuroscientists for years as an acceptance criterion of a putative relation between neural and mental variables. If there is a similarity in the shape or time course of two functions, then this similarity is taken as evidence that one represents the other. Nevertheless, if there is any single principle we have learned from the study of sensory processes, it is that there is no need to assume that the dimensions used by the stimulus are the same as those used by the neural responses; similarity of functional shape or even of dimensionality is not good evidence of a causal relation. Thus, for example, although a stimulus may be continuously varying in magnitude, it is not only possible but well established that the actual neural representation of the associated cognitive response may be encoded by some other neural dimension such as recruitment, spatial location, or even temporal sequence.

The bottom line of this lack of robust quantification is that it is unlikely that we will find a means of manipulating brain images so that they can be used as a scaled quantitative measure of a cognitive activity such as the degree of training, intelligence, aptitude, or personality. Although some coarse and indirect measures may be observed to correlate (typically with rather low coefficients) with cognitive capabilities, the search for a brain image displaying a quantifiable scale that meets all of the conditions for good measurement of such subtle psychological properties as "leadership" is unlikely to succeed.

1.5.4 Indirectness of Measurement

A related technical issue concerns the degree to which brain images are direct or indirect measures of the neural codes for cognitive processes. As with the earlier methods such as EEG, there has always been a presumption on the part of researchers that the brain images are especially potent tools because they are "direct" measures of the brain's activities. Unlike many of the earlier methods that used autonomic indicators (e.g., the polygraph), which were obviously secondary and indirect measures of brain and cognitive activity, the fMRI, for example, seems at first glance to be a direct measure of the activity of the brain. Thus modern brain images have assumed a kind of face validity that belies what we now know to be their actual indirectness as measures of brain activity.

There remains considerable controversy in the field concerning the exact relation of the brain image both to the brain's metabolism and to the information processing carried out by neuronal networks. The chain of logical and functional connections between a thought and an fMRI brain image is much longer than generally appreciated. In retrospect we now appreciate that the fMRI is as distant as the galvanic skin response or pulse rate from cognitive processes. Although there is little question that the brain images are measures of some aspect of the brain's neurobiological activity, how direct or indirect they may be remains open to question. The issue of directness becomes even more controversial when we make the leap to cognition.

An argument can be made that the responses captured by fMRI systems are logically quite distant from the essential information processes that are assumed to be the true psychoneural equivalents of cognition. Just how distant is the connection is suggested by table 1.2, which lists some of the many gaps that must be leapt to link the brain image to cognitive activity.

In other words, despite many attempts to link fMRI images directly to cognitive processes, there remain serious gaps in the logic connecting cognition and its neural substrates. The illusion of directness is based more on the general ontological assumption that the brain is the seat of our minds rather than on any convincing empirical evidence.

Very recently, the robustness of the "→" (i.e., "which is assumed to lead to") between steps has itself has been challenged. The basic assumption in this chain of logic has been that the fMRI image, known to be sensitive to changes in blood oxygenation (the blood oxygen level dependence, or BOLD), is also closely enough related to the neural activity to be used as a measure of that neural activity. A few researchers have begun to question this most basic assumption. For example, in a recent research report Maier et al. (2008) considered a pervasive discrepancy in the neuroscientific literature. Previous research had shown that human fMRI signals were strongly correlated with a subjective suppression of a visual stimulus, whereas neurophysiological activity from the monkey's brain was not under what seemed to be nearly identical stimulus situation. To unravel this discrepancy Maier and his colleagues compared two situations (no perception due to the real absence of a visual

Table 1.2
Inferential steps from cognition to fMRI

Cognitive processes are encoded by patterns of local field potentials (synaptic activity) →
Increased glucose (?) metabolism →
Increased oxygen demand →
Increased oxygenated blood flow →
Decreased deoxygenated blood level →
Changed fMRI signature

Where "→" means "which is assumed to lead to."

stimulus and no perception due to cognitively suppressed vision) and discovered that the fMRI signals did not always match the neural signals. They put it very succinctly in their concluding comments:"Our results demonstrate that the very same signals that correlate strongly with the BOLD signal in one context (physical stimulus removal) fail to do so in another (perceptual suppression)" (p. 1197).

Furthermore, Sirotin and Das (2009) also showed that the hemodynamic activity reflected in the BOLD measure may not be as closely related to the neural activity as it has generally been assumed. These researchers used an optical method of measuring the blood volume and its level of oxygenation at the same time that neural activity was recorded. The neural activity was picked up by electrodes that recorded both multiunit records and local field potentials. Their results indicated that the blood volume and oxygenation measures seemed to be composed of two different signals—one that was correlated with the neural activity during on and off visual stimulation and another that was apparently unrelated to whether or not a stimulating light was on. They attributed this to anticipation on the part of the animal to the subsequent onset of a stimulus. This result suggests that the blood volume and oxygenation levels are not always related to the induced neural activity.[20] The very important conclusion of this study was to disassociate the blood measures and the neural measures at least in the context of this visual experiment. The authors concluded: "These results raise the further possibility that there may be other, hitherto uncovered exceptions to the assumption that hæmodynamic signals uniformly imply equivalent underlying neuronal activity" (p. 478).

Finally, even some of the most committed pioneers in the use of fMRI (Bartels, Logothetis, & Moutoussis, 2008) have now concluded that the measurements provided by this device do not reflect the cumulated spiking activity of neurons in the brain. Therefore they cannot be used to test for specific neuronal sensitivities such as directional sensitivities. They noted that ". . . most [fMRI] studies fail to convincingly demonstrate the directional sensitivity of its neurons" (p. 451.) Unfortunately we do not know to what property of the brain these images do reflect. Bartels and colleagues suggested that it may provide a "complementary" measure of brain activity but one whose relation to cellular neurophysiology is yet to be established.

The importance of all of these studies cannot be overestimated. Since the fMRI can be dissociated to at least some degree and in some contexts from the neural activity, the most fundamental premise of the enterprise—fMRI signals represent blood levels correlated with neural activity—is called into question. At the very least this is another variable that must be explored and that may help to explain the wide variability of responses observed in this kind of cognitive neuroscience.

These studies have been carried out in the sensory pathways, and all involve some kind of subjective variable (e.g., perceptual suppression and/or anticipation). Indeed it is with these complex, high-level cognitive states that the BOLD measure may fail most completely. It is the fact that these high-level cognitive variables, which most clearly characterize the enormous enthusiasm for brain imaging work these days, most demand our critical attention. It is important, therefore, to constantly ask—are we dealing with a distant epiphenomenon—a sign—or are we dealing with a valid measure—a code—of the cognitive phenomenon under study? The current status of brain imaging technique as it is used by so many cognitive neuroscientists these days does not yet provide an answer to the fundamental question of directness.

1.5.5 The Timescale Difference

In addition to the problems of measurement validity and directness, there is also a massive discrepancy between the time course of fMRI responses and our cognitive processes. This discrepancy raises additional questions about the meaning of these seductive brain images. Our thoughts seem to function at what appear to be millisecond timescales as do the responses of individual neurons. For example we are able to discriminate between two sequential visual stimuli when the interval is as little as 10 msec. As another example retinal disparities can be processed to produce the experience of stereoscopic depth with stimulus exposures of only a few microseconds.

However, since the metabolic processes (such as oxygen depletion) on which fMRI images are based take much larger fractions of a second—several hundred milliseconds to several seconds—it is obvious that the timescale of cognition and of the hemodynamic processes underlying fMRI brain images are not directly comparable. This, too, adds to the discrepancies and suggests that any attempt to use the brain image as a simple measure of cognitive activity is fraught with serious technical as well as conceptual difficulties.

1.5.6 Variability

Major problems with fMRI data are the variability and lack of reliability of the data gathered when brain images are recorded. Experiments vary; individuals vary; and, especially when data are pooled from a number of subjects, the cumulative results vary considerably. The reasons behind this variability are themselves varied. Obviously many factors that are not adequately controlled impact on the idiosyncratic results from this type of research. Furthermore there are technical reasons that transcend the problem of control. The lack of precision in the definition of the cognitive process under examination itself contributes greatly to the variability of answers to what seem to be even the most straightforward research questions. Maitra (2009) performed a useful service when he discussed some of the technical sources of variability in brain images. In this article he provided fresh empirical evidence that “identified regions of activation can vary from one replication to another” (p. 88). This variability is evident not only in the intrasubject results but also is evidenced between sequential slices of the scans for experiments as simple as measurement of the brain responses to voluntary finger movement. Maitra included the following technical sources of this variability in his discussion (p. 88):

1. The delay in the BOLD response relative to the cognitive process under investigation.
2. Cardiac and respiratory movements of the subject.
3. Voluntary, involuntary, and/or stimulated correlated motion during scans.
4. Scanner variability.
5. Signal difference between activated and control or resting states are small, typically on the order of 1–5%.
6. Subpixel motions can induce large apparent signal changes and result in false positives.

The problem of intersubject variability has also been dealt with extensively in the work of Miller and his colleagues (see, e.g., Miller et al., 2002; and Miller & Van Horn, 2007), who have noted that the individual data are very stable if the same subject is tested at long intervals (such as 6 months). Miller et al. (2002) argued, therefore, that "Exclusive reliance on group analysis may be to the detriment of understanding the underlying cognitive nature of brain activations" (p. 1200).

The problem of variability has not gone unnoticed, and many authors have written on various aspects of it. Many techniques have been suggested to control if not regulate the sources of variability; however there is still no comprehensive way to avoid the most significant sources of variability. Much of the variability is accounted for by psychological and physiological-anatomical factors over which even the most rigorous experimental protocols or stable hardware would not be able to overcome. Sutton et al. (2008), for example, estimate that these physiological-anatomical, stimulus-control issues account for 10 times the amount of variance introduced by the imaging equipment itself.

A result of this innate biological variability of brain images, as well as variable measurements, is that data are noisy, and many, if not most, data-based conclusions may be less than robust. Pooling based on noisy data permits a virtually unlimited number of misleading conclusions to be drawn. Indeed there is always the possibility that even the most fundamental and widely accepted conclusions may be wisps of our imagination or of randomness rather than solid, evidence-driven scientific conclusions. In a world of small and variable experimental outcomes, there are insufficient constraints to theory development. It is not just a matter of reliability, however, but also a matter of a simple lack of consistency. One does not have to do a statistical test to observe the contradictory nature of many of the results that are discussed in this book.

1.5.7 Statistical Errors

Many of the tests of activation in a typical fMRI are based on complex statistical tests that are subject to a number of subtle interpretive errors. Some of these errors are well known and have been a scourge to psychology for years. For example, Rosenthal (1979) summarized what has come to be called the "file drawer problem," otherwise known as "publication bias" (Rothstein, Sutton, & Borenstein, 2005). Rosenthal noted that there is a significant bias in scholarly publications that are dependent on significance tests resulting from the fact that only those experiments whose results arise to the $p < 0.05$ criterion are typically published. On the other hand, a larger number of studies that don't quite make this criterion level are simply cast aside and not submitted or accepted for publication. The problem is that there is a tendency for Type I (false positive) errors to be committed in published reports that are not counterbalanced by the publication of the essentially negative results of those experiments whose findings did not make the 0.05 criterion. Spurious conclusions and theories, therefore, can easily become a part of the corpus of psychological "knowledge."[21] Current investigators such as Nickerson (2000) and Killeen (2005) are among those becoming increasingly aware of the problems associated with significance testing. The statistical problem is exacerbated as the problem has grown in complexity as more and more subtle analyses are carried out by brain image researchers.

However even such simple statistical errors as inappropriately dividing two groups by some obvious factor (e.g., gender) can pollute the experiments of the best-intentioned investigators. Ihnen, Church, Petersen, and Schlaggar (2009) have recently shown how insidious and dangerous such a procedure may be. They were studying gender differences in language processing—a phenomenon that had been repeatedly, but inconsistently, reported by earlier studies. When Ihnen and colleagues carried out the usual basic experiment (dividing their subjects by gender and then using fMRIs to measure the response of a number of identified activation areas), substantial differences between the locations of brain activations were observed for men and women. However when they randomly grouped the men and women into two groups, thus presumably washing out the gender differences, "a similar number of statistically significant regions of 'group difference' in the task associated BOLD signal" were observed (p. 1020). Ihnen et al. (2009) then concluded that,

> . . . these results suggest that one should be cautious when interpreting studies that purport to have identified regions of difference between groups, whether those groups are divided by sex or any other criterion. In particular, generalization or replication of a result in independent data sets is necessary for establishing conclusive support for any hypothesis about differences in brain function between groups. (p. 1020)

This kind of result demonstrates the need for experiments in which the various combinations of studies are permuted to avoid erroneous conclusions. However, even more important is its message that brain image localizations associated with experimental conditions may be illusory and not accurate statements of brain region functionality.

As we proceed in discussing the various topics in this book, the inconsistency of many other reports of brain activity and specific cognitive processes becomes obvious. This may at least partially be explained in the terms of the artifact highlighted by Ihnen, Church, Petersen, and Schlagger's important contribution. The need for totally independent replication and the desirability of permutation analyses of any study of brain imaging are becoming increasingly evident.

If such a simple process as selecting into which group subjects are grouped can produce spurious results, then one can easily understand how a complex data analysis protocol might lead to subtler but even more profound misunderstandings. The following example, taken from the work of Newman, Greco, and Lee (2009), illustrates the complexity of the current levels of analysis used in modern imaging studies and the potential for artifacts that are inherent in such analyses.

> The data were analyzed using statistical parametric mapping (SPM2 from the Wellcome Department of Cognitive Neurology, London). Images were corrected for slice acquisition timing, and resampled to 2 × 2 × 2 mm voxels. Images were subsequently smoothed in the spatial domain with a Gaussian filter of 8 mm at full width at half maximum. The data were also high-pass filtered with 1/128 Hz cutoff frequency to remove low-frequency signals (e.g., linear drifts). The images were motion-corrected, and the motion parameters were incorporated in the design estimation. The EPI data were normalized to the Montreal Neurological Institute (MNI) EPI template. At the individual level, statistical analysis was performed on each participant's data by using the general linear model and Gaussian random field theory as implemented in SPM2. Each event (trial) was convolved with a canonical hemodynamic response function and entered as a regressor in the model. Although there were two phases for each trial (plan and execute), only one regressor that encompassed both phases was used in this analysis. (p. 131)

It is important to stress that in this instance I am not challenging the details of this particular analysis nor any of the conclusions drawn by Newman, Greco, and Lee: I am using it merely as an illustration of the complexity of the analysis techniques now being used in the brain imaging field. Nevertheless the depth of this kind of analysis should at least warn us of the potential for uncritically accepting some really extraordinary findings.

A further demonstration of the virtual certainty that even simple statistical artifacts can distort the seemingly most direct conclusions drawn from the raw data is made clear in the work of Vul, Harris, Winkielman, and Pashler (2009) in their meta-review[22] of fMRI work in the emerging field of social neuroscience. They reviewed 54 articles that had shown what they believed demonstrated "implausibly high correlations" between fMRI images and measures of personality and emotion. They pointed out that correlations between two sets of data are constrained by the reliability of the data sets. Following Nunnally (1970) this constraint can be expressed as:

$$r_{\text{observed A, observed B}} = r_{A,B} * \text{sqrt}\,(\text{reliability}_A * \text{reliability}_B) \qquad (1.1)$$

where $r_{\text{observed A, observed B}}$ is the "true" strength of the correlation between A and B, $r_{A,B}$ is the observed relation between A and B, and reliability_A and reliability_B are the reliabilities of the data sets A and B, respectively.

Vul and his colleagues then referred to the literature to get estimates of the reliability of personality tests and concluded that a range of 0.7 and 0.8 was the best that could be expected. There were few data available that could be used to estimate fMRI reliability, but on the basis of a modest amount of information, it was estimated that ". . . fMRI measures computed at the voxel level will not often have reliabilities greater than about .7" (p. 275).

Given the lack of reproducibility shown by meta-studies of fMRI research that I discuss in later chapters, even this estimate seems quite optimistic.

Based on this analysis of both the behavioral and fMRI reliabilities, Vul and colleagues estimated that even in the case that there was no measurement error (i.e., $r_{A,B} = 1.0$) then the highest plausible correlation between the social or personality tests and fMRI measures would be

$$\mathrm{sqrt}\ (.8 * .7) = .74 \tag{1.2}$$

However when they reviewed the literature, they discovered that a substantial portion of the reported experiments were producing correlation coefficients well above .74. After carefully analyzing the 54 articles in their survey, they concluded that "Over half of the investigators in this area used methods that are guaranteed to offer greatly inflated estimates of correlations. . . . These procedures turned out to be associated with the great majority of the correlations in the literature that struck us as impossibly high" (p. 285).

What was the statistical error that led to these spurious or "voodoo" correlations? According to Vul and colleagues, there was one major factor—the use of a threshold criterion for selecting activated voxels in the fMRI images. That is, only those voxel scores that were above a threshold activity value were correlated with the full range of the behavioral tests. This procedure led to correlating two measures that were not independent of each other, thus producing spuriously high correlations. Such a procedure, they noted, could actually produce positive correlations out of "pure noise." How prevalent this error is throughout current cognitive neuroscience, Vul and colleagues (2009) could not say. However, they believed that it is likely that it is widespread. This is a very important finding just because it attacks the problem at its most basic level—the statistical validity of the empirical data itself.

For obvious reasons, the article of Vul et al. (2009) was met with an extraordinary amount of controversy. If they were correct, then a substantial portion of the scientific literature on the use of fMRI in cognitive neuroscience would have been questionable if not downright susceptible to rejection. Interest was so intense that prepublication copies of the article drew extensive comment. Eventually, an entire section of the journal *Perspectives on Psychological Science* was devoted to arguments both supporting and challenging their criticism. Of the seven contributors to this interesting discussion, four took a middle position agreeing with the main point but considering it to be a well-known argument, two took highly controversial positions, and one not only agreed but said that the problem was far worse than had been suggested by the authors. I deal in turn with the two most contentious responses; one critical of the work of Vul and his colleagues and one that argued they had not gone far enough.

The most spirited rebuttal to the argument put forward by Vul and his co-workers was made by Lieberman, Berkman, and Wager (2009). Their argument is based on what they believe are "misconceptions" on the part of Vul and his colleagues. First they argued that the frail methods criticized actually are not often used in this field, that the Vul et al data collection method (a survey) was incomplete and misleading, and finally that a reanalysis of those data does not support the argument that the results were fallacious.[23]

To the contrary, Yarkoni (2009) not only supported the conclusions drawn by Vul and his colleagues but asserted that the conclusions should have been "even worse" than Vul and colleagues had suggested. Yarkoni, however, did differ with Vul's group when he attributed the problem to "the pernicious combination of small sample size and stringent alpha-correction levels" (p. 294).

In a follow-up article Vul and Kanwisher (2010) extended this critique of the statistical analyses used by many researchers who routinely and somewhat naively use statistical analyses of fMRI data. They reemphasized the fact that a powerful and ubiquitous "selection bias" exists, for example, when information is "thresholded" leading to wide-spread misrepresentation of the findings of this class of cognitive neuroscience experiments. Vul and Kanwisher (in press) characterized a selection bias as simply a poor sample—they considered the sample used not fully representative of the total population. They pointed out that this error is so common that "of the eight papers in a special issue of NeuroImage [one of the leading journals committed to brain imaging], five contained variants of this error." How serious is this problem? Vul and Kanwisher stated that "in some cases (Summerfield et al., 2006), the researchers may have produced their main significant result out of nothing."

My feeling is that the most important contribution of Vul and Kanwisher's extraordinary article is their analyses of why interpretive statistical errors in fMRI studies of cognition are so common. I quote their comments in full here.

> There are three circumstances of neuroimaging that put the field at high risk. First, fMRI researchers work with massively multidimensional datasets, in which only a subset of dimensions contain information that may be relevant to the experiment. This situation encourages researchers to select some subset of the data for analysis, thus to use non-independent selection criteria. Second, fMRI analyses are complicated, involving many steps and transformations before the final statistics may be computed, resulting in confusion (and thus a diminished ability to identify such errors) not only on the part of the researchers themselves, but also on the part of the reviewers. Finally, fMRI research usually asks binary qualitative, not quantitative questions—data are presented as binary values (significant or not significant) further veiling any biases that may lie behind the biases.

Another recent article by Kriegeskorte, Simmons, Bellgowan, and Baker (2009) also developed the idea that there are profound difficulties in the analysis of both single-cell recordings and brain images. The source of these difficulties, they argued, is the inadequate and improper selection of the responses to be analyzed. They note, "In neuroimaging an example of [inappropriate] selection is the definition of a region of interest (ROI) by means of a statistical mapping that highlights voxels that are more strongly active during one condition than another. In single-cell recording, an example of [inappropriate] selection is the restriction of the analysis to neurons with certain response properties" (p. 535).[24]

The problem is that all too often the same data are used, first, for selection, and second, for analysis—a process Kriegeskorte and his colleagues refer to as "double dipping." In agreement with Vul and his colleagues they argued that such a process can lead to mistaken conclusions based essentially on the nonindependent selection of data: essentially selecting a region of interest because it responded in an experimental condition and then using that same region of interest to prove the point. To demonstrate this procedure, Kriegeskorte and his colleagues (2009) analyzed a sample experiment that showed that a significant difference could found between visual stimuli. They then repeated the experiment with random data known to not differ between the two conditions and discovered that the same analysis "also suggested high decoding accuracies, significantly above chance" (p. 537).

Kreigeskorte and colleagues (2009) did not intend to nor did they invalidate all of the 134 papers[25] they reviewed in which they believe there was some sort of "double dipping." Some, they concluded, might have been correct in their conclusions despite the statistical error. However, they did show that there was a pervasive problem throughout the neurosciences with this kind of misuse of sampling statistics. They concluded by suggesting a strategy for avoiding this kind of inadvertent double use of the same data; this strategy is presented in figure 1.1.

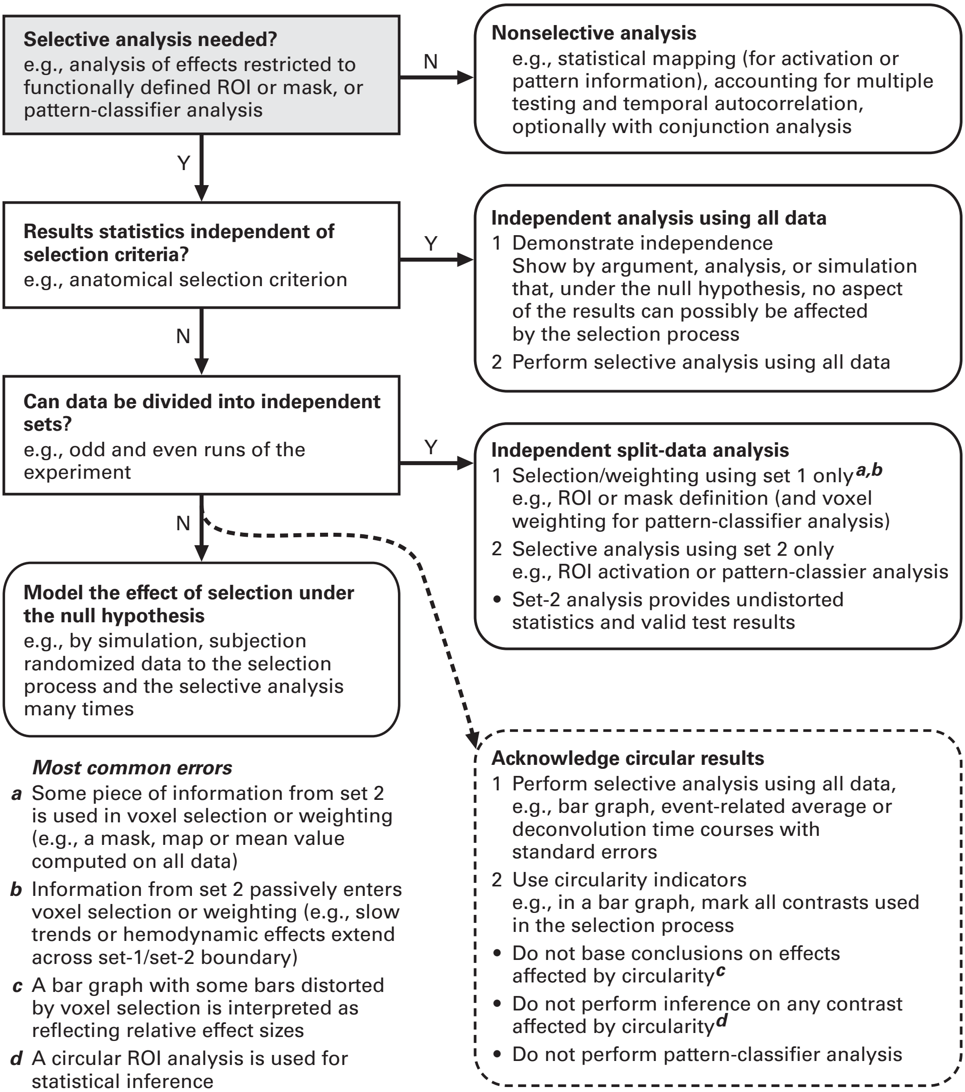
Vul and Kriegeskorte and their colleagues deserve unending commendation for their important contribution to what have become inappropriate interpretations of brain images and cognitive processes in both the lay and scientific literature.

A profoundly disconcerting fact is that this statistical problem was anticipated almost 60 years ago by Cureton (1950). In a recent conference, Vul called our attention to an underappreciated article in which Cureton showed that data known to be random can be manipulated to produce spuriously high correlations. Cureton summed it up quite well when he argued that

> The moral of this story, I think, is clear. When a validity coefficient is computed from the same data used in making an item analysis, this coefficient cannot be interpreted uncritically. And, contrary to many statements in the literature, it cannot be interpreted "with caution" either. There is one clear interpretation for all such validity coefficients. This interpretation is—"Baloney!" (p. 96)

Just how pervasive is the problem highlighted by the Cureton, Vul, and Kriegskorte interpretations is yet to be determined. The proportion of articles reported to be defective in this manner suggests that it should lead to a radical reevaluation of the meaning of the entire brain imaging enterprise.

One strategy, which is regularly used in an effort to reconcile some of these differences, is to go beyond the individual experiment by pooling the results of many experiments. This is the technique of meta-analysis or meta-review in which a group of what are purported to be similar or related experiments are jointly examined. The idea in a meta-review is that summarizing a large pool of data may provide a more accurate estimate of the properties of the mind-brain relation under study than may be possible with the small sample sizes typical of individual experiments.



Selective analysis needed?
e.g., analysis of effects restricted to functionally defined ROI or mask, or pattern-classifier analysis

Nonselective analysis
e.g., statistical mapping (for activation or pattern information), accounting for multiple testing and temporal autocorrelation, optionally with conjunction analysis

Results statistics independent of selection criteria?
e.g., anatomical selection criterion

Independent analysis using all data
1 Demonstrate independence
Show by argument, analysis, or simulation that, under the null hypothesis, no aspect of the results can possibly be affected by the selection process
2 Perform selective analysis using all data

Can data be divided into independent sets?
e.g., odd and even runs of the experiment

Independent split-data analysis
1 Selection/weighting using set 1 only[a,b]
e.g., ROI or mask definition (and voxel weighting for pattern-classifier analysis)
2 Selective analysis using set 2 only
e.g., ROI activation or pattern-classier analysis
- Set-2 analysis provides undistorted statistics and valid test results

Model the effect of selection under the null hypothesis
e.g., by simulation, subjection randomized data to the selection process and the selective analysis many times

Acknowledge circular results
1 Perform selective analysis using all data, e.g., bar graph, event-related average or deconvolution time courses with standard errors
2 Use circularity indicators
e.g., in a bar graph, mark all contrasts used in the selection process
- Do not base conclusions on effects affected by circularity[c]
- Do not perform inference on any contrast affected by circularity[d]
- Do not perform pattern-classifier analysis

Most common errors

a Some piece of information from set 2 is used in voxel selection or weighting (e.g., a mask, map or mean value computed on all data)

b Information from set 2 passively enters voxel selection or weighting (e.g., slow trends or hemodynamic effects extend across set-1/set-2 boundary)

c A bar graph with some bars distorted by voxel selection is interpreted as reflecting relative effect sizes

d A circular ROI analysis is used for statistical inference

Figure 1.1

A policy flow chart to avoid double dipping.

From Kriegeskorte, Simmons, Bellgowan, & Bake, 2009, with the permission of *Nature Neuroscience*.

Unfortunately, the usual result of a straightforward meta-review is to increase the variability. Most meta-studies show that the activated regions are more broadly distributed than indicated by the results of individual experiments. Rather then converging, meta-studies typically are dispersive in allocating brain regions to cognitive processes. Compelling pictorial evidence for this assertion can found in the meta-reviews carried out by Cabeza and Nyberg (2000), Turkeltaub, Eden, Jones, and Seffiro (2002), Laird et al. (2005), and Neumann, Derfuss, and Von Cramon (2005).

Meta-reviews of the kind mentioned here are increasingly strong evidence for the broad distribution of brain activations. Furthermore, they also are concrete evidence of the very large amount of variation and the lack of replicability among the individual studies that were collected for the meta-review itself. Experiments purporting to be analyzing the same cognitive process indentify activation sites that often are not even overlapping from one study to the next. Clearly the brain is more complicated than we think, or the entire brain imaging enterprise is deeply flawed for methodological reasons of which we are only now becoming aware. It is disappointing that so little attention has been paid to both the content and the significance of these meta-studies and the implications these meta-studies have for the entire imaging program.

A good summary of the problems encountered when the meta-study approach is followed has been provided in an article by Phan, Wager, Taylor, and Liberzon (2002) in which they themselves carried out a meta-study of emotion-driven brain imaging. According to them, the identified problems include:

1. The past and current literature in this field may bias investigators to search for particular behavior-brain region associations rather than to cast their research nets broadly.
2. Experimental protocols differ from one experiment to another so profoundly that it is difficult to assert that two or more experiments are really comparable.
3. The subtraction method, so widely used in brain imaging, is deeply flawed. It “does not identify all the regions that are involved . . . but only those that show a significant difference between the target and reference condition” (Abstracted and paraphrased from Phan et al. 2002.)

In particular reference to their meta-review, Phan et al. also listed the following problems—all of which can be generalized to any similar meta-study.

1. Classifying the variety of conditions is difficult and “somewhat arbitrary.”
2. Not all relevant experiments may be included. Some may be intentionally excluded for reasons that are not valid such as arbitrary classification schemes.
3. Not all emotional or sensory stimulus conditions may be equally represented for practical rather than theoretical reasons.

(Above list was abstracted and paraphrased from Phan et al. 2002.) Whatever, the flaws of their meta-study, the empirical data they summarize are themselves compelling evidence for the some of the problems faced by brain imaging researchers. We do not yet know how devastating these challenging problems may turn out to be as the field matures. For the moment, however, caution seems to be the prudent course of action before we accept the validity of such new fields of inquiry as "social neuroscience," "neuroeconomics," or any of the other neologisms that have sprouted along with the emergence of the fMRI approach to cognitive neuroscience. Indeed, there are even more challenging issues that are only touched by these essentially statistical arguments. Conceptual issues such as the meaning of correlation or the conceptual basis of neuroreductionism itself are even more profound and very likely to rise to increased attention in the near future.

1.6 Some Useful Anatomy

In preparation for the many discussions to be presented in the remainder of this book, it is useful at this time to introduce several different ways with which we can organize the anatomy of the brain. The words used by various authors are not always the same and are not always very precise. Three different schemas are presented in two different figures as follows. First, a general scheme denoting the major lobes of the brain is presented in figure 1.2.

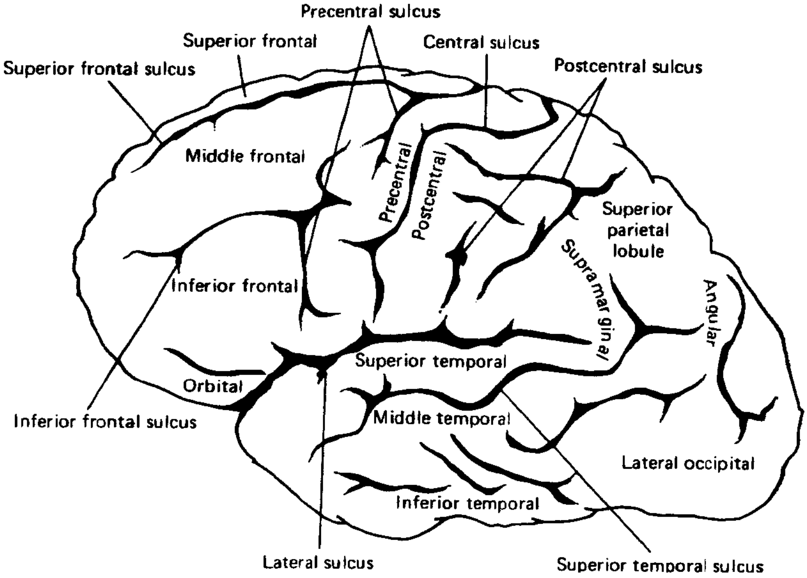


Figure 1.2
A lateral view of the human cerebrum indicating the major gyri and sulci.
From Robinson & Uttal, 1983.

Second, the study of brain anatomy has traditionally been organized in accord with the system proposed by Brodmann (1909/1999). His numbered system was based on criteria of neuronal shapes. Thus Brodmann's numbered areas originally had no special cognitive significance. Nevertheless it has been a mainstay in specifying which areas of the brain are activated or had been surgically removed for many years. The Brodmann system is shown in figure 1.3. This is the classic version of the original depiction by Brodmann a century ago. It is still widely used today.

A third three-dimensional coordinate system for standardizing brain locations based on topological distortions of the brain is increasingly being used. Talairach and Tournoux (1988) proposed such a system anchored to the location of the anterior and posterior commissures, the bands of fibers that (in addition to the great corpus callosum) connect the two halves of the brain. Using these two locations as a reference, they were able to develop a method for stretching an individual brain into something approaching a standard coordinate system that could in principle identify common brain regions. Although this was a step forward from the older anatomical location systems (and there are also new modifications of the original Talairach and Tournoux system), there are still many uncertainties and limitations as the brain is stretched and distorted to fit this "standard coordinate system."

With this preliminary information in hand, we can now consider the role of a number of anatomically specific brain regions in the representation of cognitive processes.

1.7 A Preview of Forthcoming Conclusions

Despite the undeniable fact that imaging techniques are among the most important scientific and medical developments of all time for the study of anatomy and physiology, their application to the problems of cognitive psychology, both theoretical and practical, remains problematic. In the past few years there has been increasing discussion of the lack of rigor exhibited in the flood of research reports now appearing in numbers that may approach tens of thousands a year. Articles in prestigious journals such as *Nature* (Editorial, 2007), *Science* (Miller, 2008a, 2008b), popular magazines such as *Scientific American* (Schermer, 2008), in scholarly journals such as *Trends in Cognitive Science* (Poldrack, 2006), as well as in books (Uttal, 2001, 2009a, 2009b) have begun to raise questions about the limits of the brain imaging approach to the study of cognitive processes. An immeasurable, but significant, portion of the field is now best considered to be exploratory and preliminary. As a result the flood of new observations and suggestive relations being presented almost every day in both professional and lay publications should be critically examined. The rigor of many of these reports has been increasingly challenged by skeptical reanalyses of the empirical findings and their interpretations. Unfortunately there has been a shortage of this kind of critical analysis to match the abundance of hyperbole currently characterizing the field.

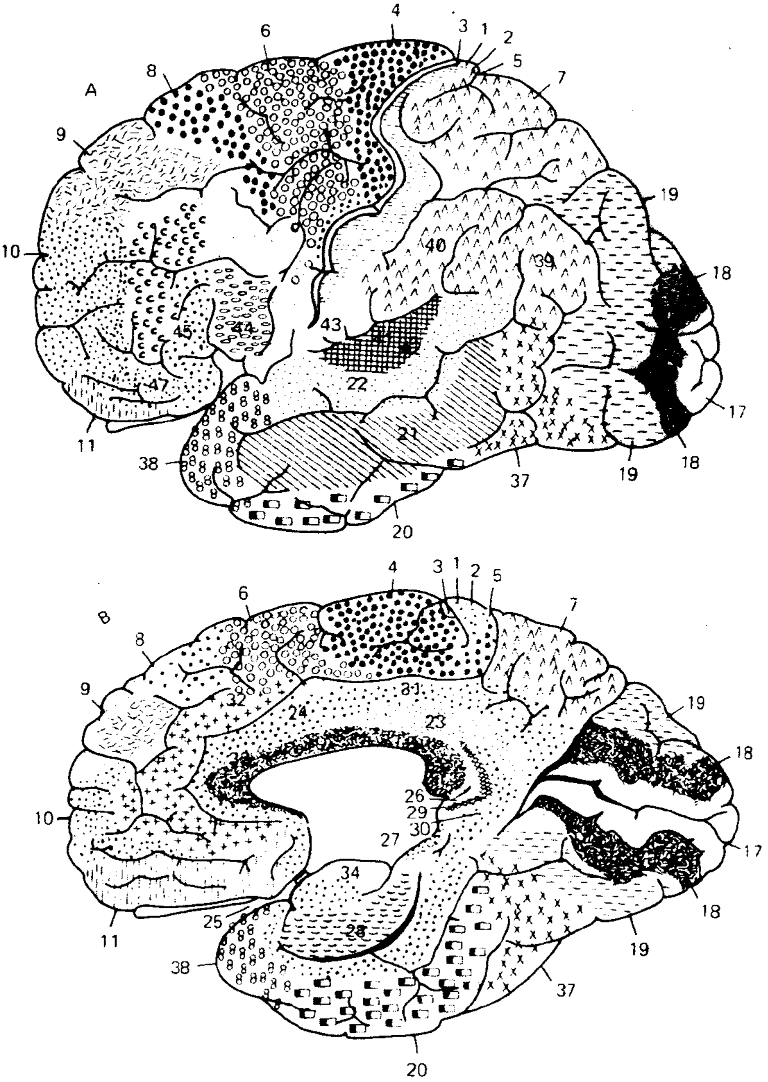


Figure 1.3

The Brodmann regions.

From Uttal, 1973, after Brodmann's, 1909, original drawing.

The empirical facts we encounter in the rest of this book should raise our awareness of the actual complexity of the mind-brain system. As such, they emphasize the increasing difficulty and ambiguity of the scientific situation rather than clarify the nature of the mind-brain relation. The brain probably should not now be considered to be an aggregate of isolated or isolatable components located in a particular place but, rather, as a dynamically changing four-dimensional (*x, y, z, t*), network of interconnecting and interacting components of vaguely defined, redundant, and overlapping functions and, thus, ever-changing spatial limits. No longer is the crutch for simplifying research designs offered by the neophrenological idea of the localized representation of cognitive faculties (either singly or as the separable components of a "system") available to us as a means of simplifying and organizing our findings. It is now increasingly apparent that brain images at the very least reflect the fact that vast regions of the brain, if not all of it, are involved in even the simplest cognitive processes.

Along with our increasing appreciation of the complexity of both the brain and the mind has come an appreciation of the difficulty of analyzing systems as complex as these. Two recent expressions of this new view of complexity (Foote, 2007; Binder, 2008) express this concern in different ways. For example Foote points out that "the seemingly 'deterministic' nature of such foundations may belie their ultimate intricacy and unpredictability" (p. 412), and Binder points out the extreme "frustration" that can arise out of the virtually unanalyzable behavior of complex systems. Not only are some of the problems mathematically intractable, but the problems of even evaluating the nature of their complexity and asking the correct questions can be overwhelming.

The point is—cognitive neuroscience, despite considerable ballyhoo, does not yet have the tools with which to deal with an intricately complex system such as the brain. Much less is it able to link the subtleties of behavior such as intent and perception with that level of neural complexity. Of particular concern are the unjustifiably strong conclusions drawn from noisy data, a characteristic of much of the current research. A close examination of the experimental protocols of a variety of publications is not encouraging; many studies are of low statistical power depending on only a few subjects and are rarely replicated to the extent that is desirable in such a new field of inquiry. Others produce data that are so variable from subject to subject that robust general conclusions are hard to establish. Various kinds of statistical manipulations may appear to define particular prototypical response patterns; however, given their individual variability, all must be considered skeptically.[26]

The basic problem is that this field is too rich and too unconstrained by either the dimensions of the brain responses or of the possible cognitive states to be adequately controlled or measured. As a result there are too many opportunities to try something "novel" or to inadvertently apply questionable statistical procedures. The inevitable outcome is that this phase of cognitive neuroscience shares with psychology in general a lack of specific, universal theory; both remain aggregations of isolated findings in which results have pyramided to evermore inclusive theoretical interpretations rather than follow a systematic science.

What this all means is that the MRI and the EEG are blunt instruments—epistemological sledge hammers—when it comes to understanding or even representing the detailed neuronal network mechanisms that actually underlie cognitive processes. They are techniques that operate at the wrong level of analysis; where we need information about the patterns of microscopic neuronal activity to understand something like learning, we only have available measures that pool all of the truly salient microscopic information into an unanalyzable compound. It may not be too severe a criticism to point out that whatever signs or biomarkers of cognitive activity may ultimately emerge from brain imaging studies, the whole enterprise is theoretically sterile because of this disconnect between the level at which we observe (molar chunks of the brain) and the level at which mind is actually instantiated (the details of the neuronal network). A logical conclusion of this argument is that any hope for practical applications that assume that we will be able to "read the mind" or use these tools as "biomarkers" for either normal or dysfunctional mental processes is unlikely to be realized.

If the macroscopic methods (e.g., fMRI and EEG) provided some additional information that could be of use to behavioral scientists that was not available by any other means, even these limitations would be acceptable. However if one's task is to measure, control, and predict behavioral, cognitive, or mental activities there is, in fact, another better and more direct means of getting much more high-quality information of the kind we need—the behavioral measurements themselves. Given the preliminary, noisy, and low predictive power of the current state of brain imaging and EEG data, my judgment is that the current effort to replace even poorly defined behavioral measures with "neuroscientific" measures would be a substantial misdirection of effort and resources. Little in predictive power would be added to solving the behavioral problems at hand by using these kinds of macroscopic methodologies. It is questionable what even the best neuroscientific knowledge adds to a robust, behaviorally oriented, psychology. Indeed, it may be that the entire current emphasis on imaging has detoured and obstructed psychological science from its main goals. The point is that a psychology without neuroscience may not only be feasible but preferable to the hyperbole of the current chaotic neuroscientific Zeitgeist.

Although no one can predict the future and as remarkable as are its achievements in terms of anatomy and physiology, at present brain imaging is a deeply flawed approach to the study of cognitive processes. This is not to deny the fact that if the optimistic expectations of this new field of cognitive neuroscience are achieved, they might have some role as biomarkers, indicators, or correlates (i.e., signs) of mental activity. However, at present, despite the large number of publications in this field, neuroscientific approaches in general have not come close to the precision of behavioral indicators in predicting human performance or determining the nature of a cognitive state. In short, a purely psychological approach, probably one based on behavioral rather the mentalist foundations, might be a better path to understanding the way our minds work.

This, then, provides a preamble to the main work of this book—an analysis of what neuroscience and psychology have contributed to each other. So far I have dealt mainly with generalities and technical criticisms. Now, I turn to a discussion of the specific empirical findings that have been forthcoming over the years for several of the traditional specialized fields of psychological research. My strategy is to present this material by discussing three topics for each of these standard psychological areas that serve as a primitive taxonomy of psychology. First I consider the purely psychological and behavioral concerns that will later play important roles in guiding the neuroscientific discussion. It is only in this context that the neurophysiological literature becomes meaningful, especially with regard to the inadequacy of the definitions and control of most of the cognitive processes with which this book is concerned. The second stage of discussion reviews the traditional neuroscience—work that was done prior to the advent of the imaging systems. Third, and finally, I consider what brain imaging has brought to modern cognitive neuroscience in the last two decades or so.

Although some neuroscientists may find the psychology portions a bit tedious and the psychologists may be equally challenged by the technical neurophysiology, this strategy is absolutely necessary. Cognitive neuroscience is truly both cognitive and neuroscientific! Both approaches are necessary, and neither is sufficient to make sense ultimately of what we all agree is the greatest scientific issue of all—how does the brain make the mind?

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