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SOCIAL NEUROSCIENCE: THE FOOTPRINTS OF PHINEAS GAGE

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Social neuroscience is the most important development in social psychology since the "cognitive revolution" of the 1960s and 1970s. Modeled after cognitive neuroscience, the social neuroscience approach appears to entail a *rhetoric of constraint*, in which biological facts are construed as constraining theory at the psychological level of analysis, and a *doctrine of modularity*, which maps particular mental and behavioral functions onto discrete brain locations or systems. The rhetoric of constraint appears to be mistaken: psychological theory informs the interpretation of biological data, but not the reverse. And the doctrine of modularity must be qualified by an acceptance of a domain-general capacity for learning and problem-solving. While offering tremendous promise, the social neuroscience approach also risks accepting a version of reductionism that dissolves social reality into biological processes, and thus threatens the future status of the social sciences themselves.

Lives of great men all remind us / We can make our lives sublime
And, departing, leave behind us / Footprints on the sands of time.
Henry Wadsworth Longfellow, "A Psalm of Life" (1838)

Phineas Gage was not a great man in the way that Goethe and George Washington (whom Longfellow likely had in mind when he wrote his poem) were great men, but he left his mark on history nonetheless—as the index case stimulating the most important development in social psychology since the Cognitive Revolution: its embrace of neuropsychological and neuroscientific methodologies and data (Adolphs, 1999; Cacioppo, Berntson, & McClintock, 2000; Klein & Kihlstrom, 1998;

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Ochsner & Lieberman, 2001). This new state of the science has been marked by the publication of special issues of social-psychological journals devoted to neuroscientific research (e.g., Beer, Mitchell, & Ochsner, 2006; Harmon-Jones & Devine, 2003; Heatherton, 2004; Lieberman, 2005), including the present one, comprehensive handbooks (Cacioppo et al., 2002; Cacioppo & Berntson, 2004a; Harmon-Jones & Winkielman, 2007), and now even a dedicated journal (Lieberman, 2006). How did we get here, and what is this new social neuroscience all about?

A SHORT HISTORY OF SOCIAL NEUROSCIENCE

The social sciences got their official start in the 19th century, as August Comte invented sociology and foresaw the emergence of a "true final science"—which he refused to call *psychology*, on the grounds that the psychology of his time was too metaphysical (Allport, 1954). His preferred term was *la morale* (nothing metaphysical about that!), a science of the individual combining cognition, emotion, and motivation with action. But he really meant psychology, and especially social psychology—which, unique among the subfields of psychology, links the individual's mind to action in the real world of other people. Our metaphysical days are over (mostly), and modern psychology has links to both biology (through physiological psychology) and sociology (through social psychology).

For its part, neuroscience got its start, and its name, only in the early 1960s, with the Neurosciences Study Program sponsored by Rockefeller University (Quarton, Melnechuk, & Schmitt, 1967; Schmitt, 1970a, b; Schmitt, Melnechuk, Quarton, & Adelman, 1966). Before that there was just *neurology*, a term dating from the 17th century, *neurophysiology* (first appearing in English in 1859), and *neuroanatomy* (1900). As a biological discipline, neuroscience was initially organized into three branches: *molecular and cellular neuroscience*, concerned with neurons and other elementary structures of the nervous system—the whole legacy of the 19th-century "war of the soups and the sparks" (Valenstein, 2005); then there was *systems neuroscience*, concerned with how the various pieces of the nervous system connect up and interact with each other; and finally *behavioral neuroscience*, concerned with everything else—but in particular with sensory mechanisms, basic biological motives such as hunger and thirst, and motor activity—mostly without reference to mental states as such.

Pretty quickly there began to emerge a fully *integrative* neuroscience (Gordon, 1990), concerned with making the connections between the micro and the macro, the laboratory and the clinic, and between neurobiology and psychology. First to make its appearance was cognitive neuroscience (Gazzaniga, 1988), concerned with the neural bases of cognitive functions such as perception, attention, and memory (see also Posner & DiGirolamo, 2000; Posner, Pea, & Volpe, 1982). Some practitioners of cognitive neuroscience defined cognitive broadly, so as to include emotional processes as well—for that matter, any internal state or process that intervened between stimulus and response. But a full-fledged *affective neuroscience* soon began to emerge as well, running parallel to cognitive neuroscience, and intending to do for feelings and emotions what the cognitive neuroscientists had done for perception, attention, and memory (Davidson, Jackson, & Kalin, 2000; Panksepp, 1992, 1996, 1998). The rise of a *conative neuroscience*, concerned with the neural basis of complex social motivation, not just survival motives like hunger, thirst, and sex (Higgins & Kruglanski, 2000), cannot be far in the future, and will complete the neuroscientific analysis of Kant's trilogy of mind (Hilgard, 1980).

In addition, what began as a proposal for a social-cognitive neuropsychology (the term I at least still prefer, because it puts equal emphasis on mind and brain; Klein & Kihlstrom, 1998; Klein, Loftus, & Kihlstrom, 1996), morphed into social-cognitive neuroscience (Blakemore, Winston, & Frith, 2004; Heatherton, 2004; Lieberman, 2005, 2007; Ochsner & Lieberman, 2001), and then evolved into a full-fledged social neuroscience (Cacioppo & Berntson, 1992b; Cacioppo et al., 2000).

When Klein and I suggested that social psychology take an interest in neuropsychology, our idea was mostly that brain-damaged patients might provide an interesting vehicle for advancing social-psychological theory (Klein & Kihlstrom, 1998).

But social neuroscience is about more than expanding the subject pool for social psychology beyond college sophomores and people waiting in airport departure lounges. Rather, social neuroscience seems to represent a new point of view about how to do social science—just as cognitive neuroscience presented itself as a new way of doing cognitive psychology, by looking at the brain as well as the mind.

THE RHETORIC OF CONSTRAINT

When cognitive neuroscience began, it was little more than an umbrella term, collecting all the individual disciplines interested in the brain in all its aspects—*the* neurosciences, like the physical sciences, or the social sciences (Quarton et al., 1967; Schmitt, 1970a, 1970b). If you pick up the followup to the initial *Neuroscience Study Program* volumes (Gazzaniga, 1995), the first thing you notice is its title: *The Cognitive Neurosciences*, plural; and when you look at the table of contents of the first edition, you see large sections devoted to neural plasticity and development, sensory and motor systems, attention, memory, language, thought, imagery, consciousness, even emotion. There was no mention of social interaction (an omission somewhat corrected in subsequent editions), but there was also no attempt to characterize *cognitive neuroscience*, singular, as a field as such. There was just the first sentence of the preface: "At some point in the future, cognitive neuroscience will be able to describe the algorithms that drive structural neural elements into the physiological activity that results in perception, cognition, and perhaps even consciousness" (p. xiii).

But identifying the neural substrates of cognition cannot be the primary motivation for cognitive neuroscience, because that has been the historical agenda of physiological psychology—which is classically defined as "the study of the physiological mechanisms of behavior" (Morgan, 1943, p. 1) see also Milner, (1970; Teitelbaum, 1967, p. 2). Of course, there is more to neuroscience than physiology; it includes genetics, molecular and cell biology, endocrinology, and evolution; but even these topics fall under the broader rubric of biological psychology (Rosenzweig, Leiman, & Breedlove, 1996). Admittedly, there was not much cognitive content in physiological psychology before 1967; but then again, there was not much cognitive psychology, either.

Gazzaniga and his colleagues did somewhat better in their undergraduate textbook, where they pointed to how "the disciplines of cognitive psychology, behavioral neurology, and neuroscience now feed off each other, contributing a new view to the understanding of the mechanisms of the human mind" (Gazzaniga, Ivry, & Mangun, 1998, p. xiii)—implying a new relationship between the study of mental life and the study of its underlying neural mechanisms. To illustrate the difference between the older physiological psychology and the newer cognitive neuroscience, they invoked Marr's (1982) hierarchical analysis of information processing. This consisted of a computational level that operates on input representations to generate output representations, an algorithmic level that specifies the processes to be performed at the computational level; and an implementational level that embodies the algorithms in a physical system. But Gazzaniga et al. balked at Marr's assumption that the computational and algorithmic levels could be understood without reference to the implementational level. "Any computational theory," they asserted, "must be sensitive to the real biology of the nervous system, *constrained by how the brain actually works*" (p. 20, emphasis added).

Similarly, Kosslyn and Koenig (1992, p. 4) portrayed the "dry mind" approach of traditional cognitive psychology and cognitive science as similar to "the attempt to understand the properties and uses of a building independent of the materials used to construct it." This was in contrast to the wet mind approach of the new cognitive neuroscience: the kinds of designs that are feasible depend on the nature of the materials. By analogy, "a description of mental events is a description of brain function, and *facts about the brain are needed to characterize these events*" (emphasis added).

Along the same lines, Ochsner and Kosslyn (1999, p. 324), illustrated the cognitive neuroscience approach with "the cognitive neuroscience triangle" (p. 325), with cognitive abilities at the apex, and computation and neuroscience at the bottom vertices. As they put it (referring to a label in the accompanying diagram), "Abilities is at the top because that is what one is trying, ultimately, to explain, and neuroscience and computation are at the bottom because the *explanations rest on conceptions of how the brain computes*" (p. 324, emphasis added). And very recently, the introduction to a wide-ranging survey of neuroimaging findings ended with the take-home message, repeated in the abstract, that the method "promises to advance psychological theory by suggesting functional representations and processes, by imposing significant constraints on these processes, and by producing not only new behavioral hypotheses but also new means of falsifying theoretical hypotheses" (Cacioppo, Berntson, & Nusbaum, 2008, p. 67).

When some neuroscientists assert that psychological theories are constrained by neuroscientific evidence, the idea is that evidence about brain structure and function will somehow determine which theories of cognitive function are right, and which are wrong. It is this idea—that knowledge of biological structure and function will constrain theories at the psychological level of analysis—that is the key feature distinguishing the new cognitive neuroscience from the older physiological psychology. This *rhetoric of constraint* has been echoed by some social neuroscientists as well. For example, Cacioppo and Berntson wrote that "knowledge of the body and brain can usefully constrain and inspire concepts and theories of psychological function..." (Cacioppo & Berntson, 1992b, p. 1025; see also Cacioppo & Berntson, 2004b, p. 978). Even earlier, in a manifesto promoting social psychophysiology, Cacioppo had hailed "The recent effort by social psychologists to bring their theoretical constructs into line with knowledge about the structure and function of the human nervous system…" (Cacioppo, 1982, p. 250).

Similarly, Ochsner and Lieberman argued that "cognitive psychology underwent [a] transformation as data about the brain began to be used to constrain theories about the cognitive processes underlying memory, attention, and vision, among other topics" (Ochsner & Lieberman, 2001, p. 726)—with the implication that social psychology would undergo a similar transformation as data about the brain began to be used to constrain theories about the cognitive processes underlying social interaction. For Goleman (2006, p. 324), "new neuroscientific findings have the potential to reinvigorate the social and behavioral sciences," just as "the basic assumptions of economics . . . have been challenged by the emerging 'neuroeconomics', which studies the brain during decision-making." And in a recent report on the progress and prospects of the new field, Cacioppo and his colleagues asserted that "Social neuroscience capitalizes on biological concepts and methods to inform and refine theories of social behavior" (Cacioppo et al., 2007, p. 99).

Ochsner and Lieberman (2001) were surely right that social psychology "gained greater conceptual and explanatory power" (p. 726) as it stepped down from the behavioral level to the cognitive level—thus replacing the objective situation of classical experimental social psychology with the *subjectively perceived* situation of modern cognitive social psychology (Jones, 1993; Zajonc, 1980). Without some concept of intervening mental activity, experimental social psychology—like behaviorism did little more than record functional relations between social stimuli and individual responses. But it is not at all clear that either cognitive or social psychology gain additional conceptual and explanatory power by taking a further step down from the cognitive to the neural level of analysis. After all, *pace* Goleman (Goleman, 2006), the real revolution in economics flowed from the observational field studies of Simon (e.g., 1947, 1955) and the paper-and-pencil questionnaires of Kahneman and Tversky (e.g., Kahneman, 2003; Kahneman, Slovic, & Tversky, 1982)—and they have the Nobel prizes to prove it.

THE EXAMPLE OF MEMORY

But it is one thing to assume that mental life is a product of brain activity, and another thing entirely to assert that knowledge of how the brain works constrains the kinds of theories we can have about mental life. In fact, it appears that precisely the reverse is true: psychological theory constrains the interpretation of neuroscientific data. Consider, for example, the amnesic syndrome, as exemplified by the late patient H.M., who put us on the road toward understanding the role of the hippocampus in memory (e.g., Milner, Corkin, & Teuber, 1968; Scoville & Milner, 1957). But what exactly is that role? The fact is, our interpretation of the amnesic syndrome, and thus of hippocampal function, has changed as our conceptual understanding of memory has changed. First, amnesic patients were thought to suffered a kind of consolidation failure, such that new memories were simply not stored (Scoville & Milner, 1957); then to have lost long-term but not short-term memory (Atkinson & Shiffrin, 1968); then to have suffered a retrieval rather than an encoding failure (Warrington & Weiskrantz, 1970); then to be capable of shallow but not deep processing (Butters & Cermak, 1975); then to have impaired declarative but spared procedural memory (Cohen & Squire, 1980); then impaired episodic but not semantic memory (Schacter & Tulving, 1982a, 1982b); then impaired explicit but not implicit memory (Graf & Schacter, 1985; Schacter, 1987); then impaired declarative but not nondeclarative memory (Squire & Knowlton, 1994); and now, most recently, impaired relational but not nonrelational memory (Cohen et al., 1999). Here, clearly, neuroscientific data has not done much constraining: the psychological interpretation of this neurological syndrome, and its implication for cognitive theory, changed almost wantonly, as theoretical fashions changed in psychology, while the neural evidence stayed quite constant.

Look at it another way: Suppose we had no idea where H.M. had sustained his brain damage. We were just presented with a patient who could not seem to remember recent events. However, further, careful testing, of H.M. and of other patients like him, employing the paradigms of cognitive psychology, revealed that he suffered a specific impairment of conscious recollection that spared priming effects. The conclusion that there are two expressions of memory, explicit and implicit, and that they are dissociable, might be (and indeed I think it was) a substantial advance in cognitive theory. But note that this advance comes from behavioral data—from how H.M. and others like him performed on tests of free recall, stem-completion, pursuit-rotor learning, and the like. The theoretical conclusion that explicit and implicit expressions of memory are qualitatively different does not depend on the location of the brain damage.

If we are interested in the neural bases of memory, then neuroscientific evidence is obviously critical. Once we have a good description of some process at the psychological level of analysis, then we can try to determine how the brain does it, and the presence of a valid psychological theory allows us to make valid interpretations of what we see at the neural level. But if the psychological analysis is wrong, the analysis of neural function will be wrong as well. That is because cognitive and social neuroscience depend on cognitive and social psychology; but cognitive and social psychology do not depend on neuroscience. The constraints go down, not up. As Gallistel has stated: "An analysis at the behavioral level lays the foundation for an analysis at the neural level. Without this foundation, there can be no meaningful contribution from the neural level" (Gallistel, 1999, p. 843). Or, as I like to put it, psychology without neuroscience is still the science of mental life, but neuroscience without psychology is just a science of neurons.

THE RHETORIC OF CONSTRAINT IN COGNITIVE NEUROSCIENCE

Given that the rhetoric of constraint first emerged in cognitive neuroscience, it may be instructive to determine how it has fared so far in that field before turning to its application in social neuroscience. First, it might be claimed that research on amnesic patients like H.M. did introduce the principle that memory is not a unitary entity, but comes in various forms, like declarative and procedural, or episodic and semantic, or explicit and implicit (Schacter & Tulving, 1994; Schacter, Wagner, & Buckner, 2000). In that way, neuroscientific data would constrain psychological theory by forcing us to appreciate the existence of multiple memory systems, even if further psychological research were needed to determine exactly what those systems were.

The first point to be made in response is that, as a matter of historical fact, the idea of multiple memory systems was already present in memory theory before any neuroscientific evidence was available. Thus, Bergson (1911) distinguished between two forms of memory, *independent recollections* and *motor mechanisms*, while Ryle (1949) distinguished between knowing that and knowing how. Both of these line up with the declarative-procedural distinction popularized by Cohen and Squire (1980)—which, in any case, had already been imported to cognitive psychology from artificial intelligence (Anderson, 1976; Winograd, 1972, 1975) without the benefit of data from neurological patients. Similarly, Tulving's (1972) distinction between episodic and semantic memory was justified mostly on logical grounds. And while the distinction between explicit and implicit memory was, arguably, directly inspired by studies of amnesia dating back to Korsakoff himself (Korsakoff, 1889a/1996), Schacter's (Schacter, 1987) seminal review makes clear that the essential idea of implicit memory had been around since roughly the time of Ebbinghaus (1885/1964)—who introduced savings in relearning precisely because he wanted a measure of memory that was not limited to conscious recollection. Moreover, it must be said that most of the subsequent battle over the nature of implicit memory has taken place on a field of neurologically intact subjects (e.g., Roediger & McDermott, 1993).

Of course, it is one thing for theorists to postulate the existence of multiple memory systems on rational or introspective grounds, and it is another thing entirely to demonstrate empirically that such entities actually exist. In this respect, it can be argued that the proof of the pudding is in the neuroscience. Which leads to the second point: Even though evidence from amnesic patients gave empirical support to what had mostly been mere speculation, the fact remains that it was *behavioral* evidence—not specifically biological evidence about brain structure and function—that led to the distinction—evidence that, for example, amnesic patients could learn new motor skills, or new vocabulary, even if they could not remember the learning experience. Neurobiological findings—pertaining, for example, to the site of the lesion(s), or the activation of certain brain areas—had nothing to do with the theoretical advance. If amnesic patients show spared priming in the face of severe deficits in recall and recognition, it does not matter whether their lesion is in the hippocampus or the amygdala—or, for that matter, whether they have any lesion at all (Kihlstrom & Schacter, 2000).

Perhaps the most frequently posited example of neuroscience constraining psychological theory is the debate over the status of mental imagery—or, more broadly, whether there are two distinct forms of knowledge representation in memory, propositional (verbal) and perceptual (imagistic). After Paivio (1969, 1971) proposed his dual-code theory of memory, some theorists countered that there was only a single propositional code for knowledge representation (e.g., Pylyshyn, 1973, 1981). Some theorists, such as Anderson (1978), argued that the issue was ultimately undecidable—though that did not prevent Anderson himself from including both meaning-based propositional representations and perception-based analog representations in his theory of memory (Anderson, 1983, 1995). In 1994, Kosslyn (1994) announced the resolution of the mental imagery debate, based on Farah's research on visual agnosia (Farah, 1988, 1990), as well as his own early brain-imaging work (1988). But the fact is that Farah's work involved behavioral data from brain-damaged patients. And besides, long before this time most people had already been convinced that propositions and images were distinct forms of mental representation, again based on behavioral evidence (Finke, 1980, 1985)—including Kosslyn's own extremely clever experiments on image-scanning (e.g., Kosslyn, Ball, & Reiser, 1978). Moreover, if anyone remained unconvinced by these prior behavioral experiments, they probably remained unconvinced by the newer imaging experiments as well (Pylyshyn, 2003a, 2003b). So, in the final analysis, neuroscientific evidence was neither necessary nor sufficient to resolve the theoretical dispute over the nature of knowledge representation.

And that is the situation wherever we look in cognitive neuroscience. Hatfield (2000), a philosopher trained in experimental psychology, surveyed a number of different areas, including sensation and perception as well as memory, and concluded that, in each case, psychology led and constrained neurophysiology rather than the reverse, and went so far as to argue, based on abstract principles, that psychology *must* provide the functional vocabulary for describing much of the brain's activity" (p. S396) and that "psychology leads the way in brain science" (p. S397; see also Hatfield, 1988). Coltheart, a cognitive scientist who has effectively used behavioral data from neuropsychological cases to develop a theory of reading (e.g., Coltheart, 2006a), while leaving open the theoretical possibility that neuroscientific data might be decisive in the future, considered more than a dozen purported examples where neurophysiological (particular functional neuroimaging) data constrained psychological theory, and found each case unpersuasive (Coltheart, 2006b, c; see also Uttal, 2009).

THE RHETORIC OF CONSTRAINT IN SOCIAL NEUROSCIENCE

Turning to social neuroscience, so far there have been relatively few attempts to deploy the rhetoric of constraint. In one example, Mitchell and his colleagues performed an imaging study that revealed increased hippocampal activation when subjects memorized sentences describing individuals, but increased activity in the dorsomedial prefrontal cortex when subjects formed impression of these same targets (Mitchell, Macrae, & Banaji, 2004). Because two different neural systems seemed to be at work, depending on whether the encoding goal was social or nonsocial in nature, Mitchell et al. concluded that social cognition (i.e., impression formation) "recruits distinct mental operations" (p. 4912) from nonsocial cognition. If so, then neuroscientific evidence would seem to constrain psychological theory, because it would imply that principles such as elaboration and organization, which apply to memory encoding in the nonsocial domain, do not apply to memory encoding in the social domain. Unfortunately, the design of their study included a confound that precluded any such conclusions: the nonsocial task involved rote memorization, while the social task involved inferential reasoning. In the absence of a nonsocial task whose cognitive demands were comparable to those of the social task, we cannot truly say that social and nonsocial cognition recruit distinct neural subsystems.

Subsequent research by Mitchell and his colleagues amassed more compelling evidence for the role of the medial prefrontal cortex in certain aspects of social cognition (Mitchell, Banaji, & Macrae, 2005; Mitchell, Cloutier, Banaji, & Macrae, 2006; Mitchell et al., 2004; Mitchell, Macrae, & Banaji, 2002; see also Mitchell, Macrae, & Banaji, 2005). But these papers did not evoke the rhetoric of constraint: they were solely concerned with localizing the neural substrates of certain social judgments, largely in response to other investigators who implicated the temporo-parietal junction (Saxe & Kanwisher, 2003). And the matter of localization (to which I will turn shortly) is, logically, unrelated to the question of constraint. The specific mental processes entailed in various social-cognitive tasks must still be discovered by the usual sorts of behavioral experiments.

As another instance, Lieberman (2007) concluded that social neuroscientific research had revealed a distinction between externally focused processes (associated with lateral fronto-temporoparietal regions) that operate on external, physical, typically visual features; and internally-focused processes (associated with medial frontoparietal regions) that operate on mental, emotional, and experiential characteristics; and, further that it is hard to imagine such a distinction "in the absence of existing neurocognitive data" (p. 279). On the contrary, the distinction between analog, perception-based and verbal, meaning-based knowledge representations has been almost universally accepted in cognitive psychology for almost 40 years (Anderson, 1976; Paivio, 1971)—a consensus that, as indicated earlier, had nothing to do with neuroscientific evidence. Accordingly, it is hard to imagine that such a distinction, so well established in the nonsocial domain, would not prove important in the social domain as well.

Theorists will continue to argue the rhetoric of constraint in the abstract—and, it might be noted, a similar debate has now begun to take place within economics (Maskin, 2008). But based on available evidence, the rhetoric of constraint suffers the same fate in social neuroscience as it has in cognitive neuroscience. The notion that neuroscientific evidence can constrain, or even inform, psychological theory seems dubious, for the kinds of reasons outlined by Hatfield (2000). But as an empirical matter, in terms of currently available research, there seems to be no compelling evidence that this is the case.

TWO KINDS OF CONSTRAINT: A CLARIFICATION

Although neuroscientific evidence does not—indeed, I believe it cannot—constrain psychological theory, that does not mean that biological processes play no role in social cognition and behavior. There are certainly biological constraints on human experience, thought, and action, just as there are social constraints. The relative magnitude of these influences may vary, depending on a host of factors, including the domain under consideration—as is the case for nature and nurture, genes and environment, the person and the situation, and other familiar theoretical dualisms. More important, as we have long understood from the biopychosocial model (Cacioppo & Berntson, 1992a, b; Cacioppo, Berntson, & McClintock, 2000; Crawford, Luka, & Cacioppo, 2002; Engel, 1977, 1980), biological and social influences interact, with each other and with behavior, in a relation that Bandura characterized as reciprocal determinism (Bandura, 1978; see also Kihlstrom, in press). The issue addressed here is not whether biology influences mind and behavior, much less the magnitude of that influence compared to others. The issue is whether data about the structure and function of the brain can constrain theories advanced at the psychological level of analysis.

THE DOCTRINE OF MODULARITY

The defining mission of cognitive and social neuroscience cannot be the search for the neural substrates of cognitive and social processes, because that has been the mission of physiological (or, more broadly, biological) psychology for more than 150 years. Nor can it involve the rhetoric of constraint, because that is dubious on theoretical grounds, and unproven empirically. What does seem to be distinctive about cognitive and social neuroscience, rather, is a particular approach to the identification of neural substrates: the *doctrine of modularity*.

Klein and I began our paper on social neuropsychology with the observation that "For a very long time psychology thought it could get along without looking at the brain" (Klein & Kihlstrom, 1998, p. 228). Everybody understood that the mind is what the brain does, but very few people tried to figure out the details. Partly the reasons were practical: many psychologists assumed (and many still assume) that complex human behavior, including higher cognitive and social processes, were simply too complex to reveal their neural substrates given the methods available. And partly the reasons were ideological: first there was the Skinnerian black box doctrine of the empty organism, which held that the internal structure of the organism was irrelevant to its behavior. And later, as indicated earlier, the computational functionalist notion of the brain as a universal Turing machine supported the notion that the biological structure of the brain was irrelevant to cognitive theory.

But the reasons did not lie just in ideology. As Posner and DiGirolamo (2000) noted, Lashley's doctrine of mass action and equipotentiality reinforced the idea that cognitive processes were not, and thus could not be, localized. At mid-century, in the years immediately preceding the cognitive revolution, the consensus among physiological psychologists (e.g., Morgan & King, 1966) was that, aside from small areas of each cortical lobe devoted to particular elementary functions (motor control in the frontal lobe, touch in the parietal, audition in the temporal, and vision in the occipital), and "symbolic speech" (p. 713) in Broca's and Wernicke's areas, the rest of neocortex was "association cortex" (p. 710)—with the anterior portion perhaps specialized for thinking and problem-solving, and the posterior portion having to do with complex perceptual functions. The whole scheme followed from traditional stimulus-response associationism: learning, thinking, and all the rest were mediated by associations, and associations were formed, and stored, in the association cortex as a whole. If learning and thinking were governed by the Law of Mass action, then there was no point in searching further for the neural substrates of cognition and social behavior.

MODULARITY IN COGNITION

The acknowledgement of specialized areas for speech and language, of course, were the cracks in the dike, and soon this scheme began to break up. Inspired by Chomsky's idea that there is a language organ in the mind, Fodor (1983) postulated the existence of a set of mental modules interposed between transducers (that make representations of sensory stimulation available to other systems) and central systems (that form inferences and beliefs). These modules had a number of properties, such as automatic evocation, and characteristic patterns of development and breakdown, but for our purposes two features are paramount. First, modules are domain-specific—there might be a visual module for the analysis of three-dimensional spatial relations, and another module for the acoustic and phonetic analysis critical to speech perception. Second, and more important for present purposes, each mental module is associated with a fixed neural architecture—there is some part of the brain that has the neural machinery that implements the module's activity.

While it is the goal of cognitive psychology (and cognitive science more broadly) to work out how these modules work at the psychological level of analysis, the defining agenda of cognitive neuroscience is to identify the neural correlates of these modules in some centers, or systems of centers, in the brain. Without something like the doctrine of modularity, cognitive neuroscience does not make any sense. If all mental life were just a matter of associations formed by a general-purpose information-processor—or, for that matter, systems of productions operating on symbolic representations (Anderson, 1976), or even patterns of activations across a connectionist network (McClelland & Rumelhart, 1986; Rumelhart & McClelland, 1986)—there would be no reason to be interested in the neural bases of different mental functions, and we would not need any neuroscience beyond molecular and cellular biology.

The idea of functional specialization was foreshadowed in the work of Emanuel Swedenborg (1740/1845-1846), who dabbled in anatomy before he became a Christian mystic (Gross, 1998b), and has its more recent origins in phrenology (Finger, 1994; Gross, 1998a). For the most part, well into the 19th century the brain was considered to be a single organ—as in Pierre Flourens' doctrine of equipotentiality. But first Gall, and then Spurzheim (both of whom were distinguished neuroanatomists in their day, and not quite the quacks of modern legend), identified some three dozen separate mental faculties, including propensities like secretiveness and acquisitiveness, sentiments like cautiousness and self-esteem, perceptive abilities like size and weight, and reflective abilities such as comparison and causality—each said to be localized in a different part of cerebral cortex, as revealed by bumps and depressions in the skulls of those who either lacked such abilities or possessed them in abundance (Gross, 1998c).

The phrenologists' evidence was terrible, of course, and their assertions were vigorously challenged by Flourens and others, who argued for an early version of the Law of Mass Action. But the tide turned when Broca correlated motor (expressive) aphasia with damage to the left frontal lobe—inciting what Finger (1994, p. 376) dubbed "the revolution of 1861." Modern cognitive neuroscience has now gone on to identify dozens of brain centers for specific functions (e.g., Cabeza & Nyberg, 1997, 2000). Unfortunately, until recently, none of these areas had much or anything to do with social behavior per se.

By contrast with the modules identified by modern cognitive neuroscience, one of the most striking things about the classical phrenologists' map is how *social* many of their faculties were. Indeed, more than half of the three-dozen or so faculties listed by Spurzheim (1834) were affective as opposed to intellectual in nature, and almost half of them were legitimate topics for personality and social psychology. Social neuroscience really begins here.

PHINEAS GAGE REDUX

And here is where Phineas Gage comes in. We all know the basic story—although, as Macmillan has cogently demonstrated, many modern commentators exaggerate the extent of Gage's personality change, perhaps engaging in a kind of retrospective reconstruction based on what we now know, or think we do, about the role of the frontal cortex in self-regulation (Macmillan, 1986, 2000, 2009). What is not fully appreciated is that, more than a decade before Broca and Wernicke, the Gage case played a role in the debate over phrenology and localization of function (for references to the quotations that follow, see Macmillan, 1986, 2000; see also Barker, 1995). Harlow's initial (1848 and 1849) reports of the case merely emphasized the fact that Gage had survived his accident. Bigelow, the Harvard professor of surgery who also examined Gage and was eventually to acquire his skull and tamping iron for what is now the Countway Medical Library at Harvard Medical School, called it "the most remarkable history of injury to the brain which has been recorded" to date—remarkable because Gage survived at all, much less continued to function. All these accounts could be interpreted as consistent with Flourens' holistic view of the brain—that you could lose a lot of brain tissue and still function adequately.

But already in 1848, Harlow was hinting that while Gage's intellectual capacities were unaffected by the accident, he had observed changes in his mental manifestations—a piece of phrenological jargon that referred to the affective (and social) as opposed to the intellectual faculties. In 1851, an anonymous article in the *American Phrenological Journal* (APJ) insisted that Gage was, in fact, changed by his accident. Harlow himself, in his final report of 1868, described the "mental manifestations" in some detail, with particular respect to the "equilibrium . . . between his intellectual faculties and his animal propensities." Nelson Sizer, a prominent American proponent of phrenology (and probably the author of the 1851 *APJ* article), concluded that the tamping iron had passed out Gage's head "in the neighborhood of Benevolence and the front part of Veneration" (Macmillan, 2000, p. 350). This was 10 years before Broca refuted Flourens. Unfortunately, Gage's brain (as opposed to his skull) was not available for examination, or Harlow and Bigelow might have beaten Broca to the punch, and Gage, not Tan, might have provided the milestone demonstration of cerebral localization. Still, just as H.M. arguably became the index case for the new cognitive neuroscience, so Phineas Gage may serve as the index case for our new social neuroscience.

THE MODULARITY OF SOCIAL INTERACTION

If, as I argue, the defining feature of cognitive neuroscience is the search for dedicated cognitive modules in the brain, then the defining feature of social neuroscience is the search for dedicated social modules. The phrenologists had some ideas about what these might be, and so have some more recent social scientists. For example, in his theory of multiple intelligences, Gardner (1983) explicitly cited Gage as evidence for an interpersonal form of intelligence, defined as "the ability to notice and make distinctions among other individuals," and isolable by brain damage from other intellectual abilities. Gardner also proposed an intrapersonal form of intelligence, defined as "the ability to gain access to one's own internal, emotional life."

Even earlier, Taylor and Cadet (1989) had offered a somewhat more differentiated view of the neurological basis of social intelligence, suggesting that three different social brain subsystems were involved: a *balanced/integrated cortical subsystem* that employs long-term memory to make complex social judgments; a *frontal-dominant* *subsystem* that organizes and generates social behaviors; and a *limbic-dominant subsystem* that organizes and generates emotional responses.

Based on his analyses of autistic children, Baron-Cohen (1995) suggested that the capacity for mindreading—by which he really means a capacity for social cognition—is based on four cognitive modules—an intentionality detector, an eye-direction detector, a shared-attention mechanism, and a theory-of-mind mechanism. Each of these is, presumably, associated with a separate brain system, impairments in one or more of which presumably cause the "mindblindness" characteristic of autism.

An even more differentiated view has been offered by Daniel Goleman (2006). As he imagines it, the social brain is not a discrete clump of tissue, like MacLean's (1970) reptilian brain, or even a proximate cluster of structures, like the limbic system. Rather, the social brain is an extensive network of neural modules, each dedicated to a particular aspect of social interaction. For example, Goleman speculates that there are modules for primal empathy, empathic accuracy, listening, and social cognition, among other abilities involved in social awareness. And there are modules for interaction synchrony, self-presentation, influence, and concern for others, among other abilities involved in social facility (or relationship management).

TROUBLE WITH MODULES

According to some evolutionary psychologists, the kinds of modules postulated by cognitive, affective, and social neuroscience evolved in the Environment of Early Adaptation (EEA)—roughly the east African savannah during the Pleistocene Era, in which modern humans emerged about 300,000 years ago (Barkow, Cosmides, & Tooby, 1992; Pinker, 1997, 2002). And social neuroscientists, like cognitive and affective neuroscientists, have proceeded apace to identify modules, centers, or systems in the brain that appear to be dedicated to particular cognitive, affective, or social processes. A recent review by Lieberman counted no fewer than 21 brain areas differentially associated with such functions as visual self-recognition, dispositional attribution, reflected self-appraisals, affect labeling, and attitude processing (Lieberman, 2007).

At the same time, the program of identifying social or even cognitive processing modules in the brain has come in for some scrutiny. Even Fodor (2000) has expressed doubt about what he has called Massive Modularity—the idea that the mind, and thus the brain, is nothing but a collection of a vast number of modules, each dedicated to performing a different cognitive activity (see also Barrett & Kurzban, 2006). Some of these criticisms have been methodological in nature—stemming partly from ambiguities in the description of the mental processes being localized, and partly from long-understood problems with the logic of subtraction that brain-imaging inherited from Donders (Donders, 1868/1969) and Sternberg (Sternberg, 1969)—not to mention the very likely possibility that any such modules as might exist are so densely interconnected and reciprocally interacting that it might not be possible to dissect them with even a 16-Tesla magnet (e.g., Uttal, 2001, 2009). Recently, for example, Vul and his colleagues have identified a procedural anomaly in many brain-imaging studies that has artificially inflated many correlations between patterns of brain activation and particular social-cognitive activities (Vul, Harris, Winkielman, & Pashler, 2009a, b).

The enterprise of brain-mapping is made even more difficult by the possibility of one-to-many, many-to-one, and many-to-many relations between anatomical structure and psychological function (Cacioppo et al., 2008; Sarter, Berntson, & Cacioppo, 1996).

Beyond these sorts of methodological difficulties, enthusiasm for the doctrine of modularity should be tempered by the additional need for a mechanism for general-purpose information processing—the role once assigned to association cortex. This is because we can solve problems other than those that confronted our ancestors in the EEA—like how to jury-rig carbon-dioxide scrubbers to keep the crew of Apollo XIII alive after their command module lost power and heat. To take another example: the modern phrenological head locates a brain system for the semantic processing of visual words, somewhere in the left fusiform area: but it seems extremely unlikely that evolution produced a specialized brain system for processing visual words, for the simple reason that writing was only invented some 6,000 years ago, and the brain has not yet had enough time to evolve one.

Moreover, it seems likely that the greatest gift of evolution was not a mental toolbox of dedicated modules: it was language, consciousness (which gave us something to talk about), and the general intelligence needed to solve problems other than those posed by the EEA. After all, humans expanded beyond the EEA as soon as they could—a movement that would not have been possible had they not had the capacity to adapt quickly, over the course of a single life cycle rather than the course of evolutionary time, to new environments. What made that quick adaptation possible, of course, was general intelligence and a powerful capacity for learning in general. To the extent that social behavior is mediated by a general-purpose information-processing system, the project of identifying specific neural correlates of social behavior will fail, because the models and methods of social neuroscience are geared toward identifying domain-specific modules.

A MIDDLE WAY

So if the brain is not just an equipotential blank slate, neither is it likely to be composed exclusively of dedicated mental modules. What is needed is a system that lies somewhere between Gardner's proposal for a single module for "interpersonal intelligence" and Goleman's "far-flung neural networks" (p. 324)—a kind of basic level analysis that encompasses a limited number of dedicated modules, but still leaves ample neural space for general problem-solving.

One such proposal comes from Jackendoff (1992, 1994, 2007), a cognitive scientist much influenced by Chomsky and Fodor, who has argued since 1992 that certain aspects of social cognition may be modular in nature.[1] For example, he has argued that because social organization is unrelated to perceptual structure—that is to say, the interpersonal meanings assigned to objects and events are not highly

1. In our early presentations of social-cognitive neuropsychology, Klein and I unaccountably failed to discuss Jackendoff's work. Jackendoff himself was too polite to ever mention it, but I take this opportunity to correct the record.

correlated with their physical attributes—the same modules that process perceptual information cannot process information relating to social stimuli.

What kinds of social information-processing modules might there be? Based on considerations of specialized input capacities, Jackendoff has suggested that there might be dedicated modules for face and voice recognition, affect detection, and intentionality. Based on considerations of developmental priority, he has suggested that children have an innate capacity to distinguish between animate and inanimate objects, and to learn proper names—that is, to think about individuals as such. And based on the work of Fiske (e.g., Fiske, 1992), he has suggested that there are modules dedicated to processing such universal cultural parameters as kinship, ingroup-outgroup distinctions, social dominance, ownership and property rights, social roles, and group rituals. Now, to be clear, Jackendoff does not think that the Liturgy of the Eucharist is hard-wired into anybody's head. But he does think that we come into the world innately equipped to pick up on such things—just as we come into the world innately equipped to pick up Russian, if that is the language our parents happen to speak. And that innate equipment comes as a set of brain modules.

A FACE-PERCEPTION MODULE (A CAUTIONARY TALE)

Without commenting on the specifics, Jackendoff's proposal strikes me as hitting just about the right level of analysis. Certainly there would be good reasons for thinking that evolution might have produced something like a face-perception module, allowing the easy recognition of that most social of stimuli. And sure enough, based on neuropsychological analyses of prosopagnosic patients, as well as neuroimaging studies of neurologically intact subjects, Kanwisher and her colleagues seem to have identified just such a module—a "fusiform face area" (FFA) in the fusiform gyrus (Kanwisher, McDermott, & Chun, 1997)—along with an area in the occipito-temporal cortex specialized for the perception of body parts (Downing, Jiang, Shuman, & Kanwisher, 2001).

The idea that there is a brain module dedicated to identifying faces is one of the most appealing just so stories of evolutionary psychology, but establishing the existence of such a module turns out to be no trivial matter. For one thing, Gauthier, Tarr, and their colleagues have produced quite compelling evidence that the same brain area that is activated in face recognition is also activated when experts recognize all sorts of other objects, including novel biomorphic figures known as greebles; and that prosopagnosic patients, who appear to have a specific deficit in categorizing faces, also have problems categorizing snowflakes (e.g., Gauthier, Behrmann, & Tarr, 1999; Gauthier & Tarr, 1997; Gauthier, Tarr, Anderson, Skudlarski, & Gore, 1999; Tarr & Gauthier, 2000). So, perhaps, the FFA may not be a face-specific area after all, but rather a flexible fusiform area (not coincidentally, also abbreviated FFA; Tarr & Gauthier, 2000) that is specialized for object recognition at subordinate levels of categorization—of which face recognition is a particularly good, and evolutionarily primeval, example.

Gauthier's proposal also remains controversial (McKone, Kanwisher, & Duchaine, 2007), and it is theoretically possible that snowflake-recognition co-opts a brain module that originally evolved for face-recognition. But the larger point is that the accurate assignment of neural function depends not so much on the sensitivity of the magnet, but on the nature of the task that the subject performs while in the machine. If you want to know what the FFA really does, the psychology has to be right; nothing about neuroscience *qua* neuroscience is going to resolve these issues.

WHAT'S *SOCIAL* ABOUT SOCIAL NEUROSCIENCE?[2]

The first glimmers of social neuroscience were pretty exclusively psychological in nature, having their origins in social psychophysiology and cognitive neuropsychology. And based on the research being presented in this and similar venues, social neuroscience is still pretty psychological in nature. Maybe that is simply the way it has to be.

Consider the three basic levels at which we can explain behavior. Psychologists explain the behavior of individual organisms in terms of their mental states. We explain someone's suicide in such terms as his belief that he is worthless, his feelings of depression, or his lack of desire to live. That is what psychologists do. A biologist, by contrast, would explain the same behavior in terms of some biological mechanism—a genetic disposition, perhaps, or anomalous neurotransmitter activity. And a sociologist or anthropologist would explain the same behavior in terms of some structure or process that resides outside the individual's mind or brain—the hothouse atmosphere of a cult, for example, as in the case of the mass suicide at Jonestown in 1978; or a culture dominated by Emperor-worship, in the case of Japanese *kamikaze* pilots in World War II.

Of course, from a strictly psychological point of view both the neurobiological and the sociocultural effects on behavior are mediated through psychology. Diminished serotonin levels, perhaps generated by a particular genetic polymorphism, make people feel depressed and think about suicide; and the cult of the Emperor, or membership in the People's Temple, might make people *want* to sacrifice themselves for a higher cause beyond themselves.

I take it that the goal of cognitive (and affective, and conative) neuroscience is to explicitly link the psychological level of analysis to the neurobiological level; similarly, it is one goal of social psychology to link the psychological and sociocultural levels of analysis. And social neuroscience can serve to link the sociocultural level of analysis through the psychological level all the way down to the neurobiological. But it seems to me that the neuroscientific approach has the potential to extend beyond individual psychology, to encompass other social sciences as well. We see this trend looming on the horizon in such new fields as neuroeconomics (Glimcher, 2003) and neuroethics (Farah, 2005; Gazzaniga, 2005)—not to mention inroads of neuroscience into political science (Westen, 2007; Wexler, 2006). Also on the side of applied social neuroscience are emerging fields like *neuromarketing* (McClure et al., 2004) and *neurolaw* (Rosen, 2007). There is even a *neurophilosophy* (Churchland, 1986) now, and a *neurotheology* as well (McKinney, 1994).

Now much of this work still looks a lot like psychology, focused as it is at the level of individual minds and brains. But it is possible that in the future we will begin to see work that is both neuroscientific and distinctively anthropological or

2. With apologies to Rae Carlson (1984).

sociological in nature. Of course, physical anthropology always implied an interest in neuroscience, and there are a number of anthropologists engaged in a kind of comparative neuroanatomy among primate species, as well as a paleoneurology focused on hominids. But that is pretty much pure evolutionary biology, and it might be really interesting to get the cultural anthropologists involved, looking at the neural underpinnings of culture (Rilling, 2007). Similarly, sociologists might get interested in looking at the neural underpinnings of processes, such as social identification (Berreby, 2005), that emerge at the level of the group, organization, and institution. If Wilson (1998) is right that certain aspects of group behavior have evolved through natural selection and are encoded in the genes, they should be encoded in the brain as well—perhaps we should call it *socioneurobiology*.

WHAT ELIMINATIVE MATERIALISM ELIMINATES

The danger in all of this is reductionism—not so much the everyday causal reductionism implied by the axiom that brain activity is the source of mind and behavior, but in particular the *eliminative materialism*, sometimes disguised as *intertheoretic reductionism*, which asserts that the language of psychology and the other social sciences is at best an obsolete *folk-science*, and at worst misleading, illegitimate, and outright false. In this view, psychological concepts such as belief, desire, feeling, and the like have the same ontological status as *vital essence*, *the ether,* and *phlogiston*—which is to say they are nonexistent, and should be replaced by the concepts of neuroscience (Churchland, 1981, 1995; Churchland & Churchland, 1991, 1998; Stich, 1983).

You get a sense of what "eliminative reductionism"[3] is all about in the following vignette, in which Patricia Churchland addresses her husband Paul after a particularly hard day at the office:

> Paul, don't speak to me, my serotonin levels have hit bottom, my brain is awash in glucosteroids, my blood vessels are full of adrenaline, and if it weren't for my endogenous opiates I'd have driven the car into a tree on the way home. My dopamine levels need lifting. Pour me a Chardonnay, and I'll be down in a minute (as quoted by MacFarquhar, 2007, p. 69).

But then, when you step back, you realize that this is really just an exercise in translation, not much different in principle from rendering English into French—except that it is not as effective. You would have no idea what Pat was talking about if you did not already know something about the correlation between serotonin and depression, between adrenaline and arousal, between endogenous opiates and pain relief, and between dopamine and reward. But is it really her serotonin levels that are low, or is it her norepinephrine levels—and if it really is serotonin, how exactly does she know? Only by translating her feelings of depression into a language of presumed biochemical causation—a language that is understood only by those,

3. Eliminative reductionism is not simply a project of some philosophical iconoclasts. The tendency toward eliminativism can be detected in Goleman's assertion that neuroscientific findings enhance the ontological status of social intelligence, and in the idea, proposed by some advocates of neurolaw, that the legal concept of personal responsibility is obviated by the "finding" that behavior is caused by the brain.

like Paul, who already have the secret decoder ring. And even then, the translation is not very reliable. We know about adrenalin and arousal, but is Pat preparing for fight-or-flight (Cannon, 1932), or tend-and-befriend (Taylor, 2006)? Is she getting pain relief or positive pleasure from those endogenous opiates? And are those glucosteroids generating muscle activity, reducing bone inflammation, or increasing the pressure in her eyeballs?

And note that even Pat and Paul can't carry it off, with their use of words like "*talk*" and "*Chardonnay*"? I suppose that what Pat really means to say is:

> Your Broca's area should be soaking in inhibitory neurotransmitters for a while, so that my mirror neurons don't automatically emulate your articulatory gestures as you push air into your larynx, across your vocal cords, and into your mouth and nose. (Liberman, Cooper, Shankweiler, & Studdert-Kennedy, 1967; Liberman & Mattingly, 1985)

and

> Mix me a 12-13% solution of alcohol in water, along with some glycerol and a little reducing sugar, plus some tartaric, acetic, malic, and lactic acids, with a pH level of about 3.25. (Orlic, Redzepovic, Jeromel, Herjavec, & Iacumin, 2007; Schreier, 1979)

What's missing here is any sense of meaning—and, specifically, of the meaning of this particular social interaction. Why doesn't Pat pour her own drink? Why Chardonnay instead of Pinot Grigio—or, for that matter, Two-Buck Chuck? For all her brain cares, she might just as well mainline ethanol in a bag of saline solution. And for that matter, why is she talking to Paul at all? Why doesn't she just give him a bolus of oxytocin? But no: What she really wants is to express herself, and for her husband to care enough about her mental state to fix her a drink—not an East Coast martini but a varietal wine that almost defines California living—and give her some space—another stereotypically Californian request—to wind down. That is what the social interaction is all about; and what it is about is entirely missing from the eliminative materialist reduction.

NATURAL SCIENCE AND SOCIAL SCIENCE

The problem is that you cannot reduce the mental and the social to the neural without leaving something crucial out—namely, the mental and the social. And when you leave out the mental and the social, you have just kissed psychology (and the rest of the social sciences) good-bye. That is because psychology is not just positioned between the biological sciences and the social sciences. Psychology is both a biological science and a social science. That is part of its beauty and it is part of its tension. Comte recognized this, even before psychology as we know it today was born—and he liked phrenology, too, because of its emphasis on affective and social functions (Allport, 1954).

All sciences want to provide objective explanations of the real world, but they differ in the kind of reality they are trying to explain (Searle, 1992, 1995; see also Zerubavel, 1997). We usually think that there are only two modes of existence and two modes of knowledge: objective existence, and objective truth that is simply a matter of brute fact, and subjective existence, and subjective truth which depends on the attitude or point of view of some observer. But as Searle has pointed out, there is no easy isomorphism between ontological and epistemological objectivity and subjectivity. That is, there are some things in the world that have an objective existence because they are intrinsic to nature, there are other things that exist objectively, even though they have been brought into existence by the mental processes of observers: they are the product of individual or collective intentionality.[4] To use two of Searle's examples, money is money and marriage is marriage only because some organization or institution says they are; but these features of social reality nonetheless have an ontologically objective mode of existence.

The natural sciences try to understand those intrinsic features of the world that exist independently of the observer. Neuroscience is like this: the brain exists, and the principles of neural depolarization and synaptic transmission are what they are, regardless of our beliefs and attitudes about them. And that is also true to some extent of psychology. We can say that the psychophysical laws, the principle of association by contingency, homeostatic regulation, the relation between depth of processing and recall, the structure of concepts as fuzzy sets (or whatever), and the availability heuristic for making judgments of frequency are all observer-independent facts about how the mind works. They are true for everybody, everywhere, throughout all time. That is one reason why cognitive psychologists tend to select their stimulus materials more or less arbitrarily. If you are doing a standard verbal-learning experiment, for example, so long as you control for things like word-length, frequency, and imagery value, it does not much matter which words you ask your subjects to memorize. But that is not all there is to psychology. Bartlett (1932) famously criticized the natural-science approach to mental life, as practiced by Fechner and Ebbinghaus, precisely because it ignored the person's effort after meaning.

Somewhere Paul Rozin has noted that psychology has been more interested in how people eat than in what people eat—and that has been a mistake (an example of the general argument can be found in Rozin, 1996). People do not want just to eat, in order to correct their blood sugar levels. Rather, people want to eat particular things, because they like them, or because eating them in certain contexts has a certain meaning for them. And they avoid eating other, perfectly good foods, either because they don't like them or because they're obeying institutional rules telling them what is permitted and what is forbidden. Not to press the point too much, but it seems that psychology as a natural, biological science is interested in the how of mind and behavior, while psychology as a social science is interested in the what of mind and behavior—what people actually think, and feel, and want, and do. That is especially true of social psychology, which is why just about the first thing that social psychologists did was to figure out how to construct attitude scales (Thurstone, 1931).

The natural sciences try to understand those features of the world that are observer-independent, existing without regard to the beliefs, feelings, and desires of the observer—in other words, a world in which there are no conscious agents, and where mental activity has no effect on the way things are. But the social sci-

4. And just to complicate things further, there are things that have a subjective mode of existence but nonetheless are observer-independent. To use Searle's example: if I am in pain, it is true that I am in pain, regardless of what anyone else might think about it.

ences seek to understand those aspects of reality that are observer-dependent—because they are created either through the intentional processes of an individual or through the collective intentionality of some group, organization, or institution. Just as psychology as a social science tries to understand behavior in terms of the individual's subjective construction of reality, so the rest of the social sciences try to understand behavior in terms of social and institutional reality. This is the difference between the natural and the social sciences—and it's a difference that is qualitative in nature. You can't make a natural science out of a social science without losing the subject matter of social science.

To be sure, social reality is the product of individual minds (working together), and personal reality is the product of individual minds (working alone), and individual minds are the product of individual brains. But a science that ignores the subjectively real in favor of the objectively real, and that ignores observer-dependent facts in favor of observer-independent facts, leaves out the very things that make social science—social science. So with our best theories and experimental methods in hand, and the biggest magnets money can buy, we can now proceed to identify the neural systems involved in social interaction. It's a great project, and there are wonderful things to be learned. But let's not forget what social psychology, and the other social sciences, are all about. Let's not get lost in the soups and the sparks.

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