

Does Neuroscience Constrain Social-Psychological Theory?

By John F. Kihlstrom

One of the most interesting developments in contemporary social psychology is its embrace of neuropsychological and neuroscientific methodologies (Adolphs, 1999; Cacioppo, Berntson, & McClintock, 2000; Klein & Kihlstrom, 1998; Ochsner & Lieberman, 2001). I certainly welcome these signs that social psychologists are taking an interest in neuropsychological and neuroscientific methods. At the same time, I want to dissent from a rhetorical stance that appears to be widely shared in neuroscientific circles: the idea that neuroscientific findings can, or will, or should, "constrain" psychological theory. For example, Cacioppo and Berntson (1992) wrote that "knowledge of the body and brain can usefully constrain and inspire concepts and theories of psychological function..." (p. 1025). Similar sentiments were expressed more recently by Ochsner and Lieberman (2001, p. 726).

Inspire, yes; but *constrain*? If we mean that data from neuropsychological and neuroscientific experiments constrains theory in the same way that data from behavioral and self-report studies constrains theory — which is to say that theory has to conform to data — then there is no argument. For example, Tulving (1993) and Klein et al. (1996) obtained data from amnesic patients that supported the hypothesis that episodic (behavioral) self-knowledge was represented in memory independently of semantic (trait) self-knowledge). But the neuropsychological data merely supplemented evidence already available from studies of priming (Klein & Loftus, 1993). Neuropsychological evidence didn't constrain the theory, though it did

inform it. It would have been interesting if the patients had lost both episodic and semantic knowledge, but even that fact wouldn't have constrained the hypothesis that the two forms of memory are normally

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independent.

Taken with its dictionary meaning, which has to do with (en)forcing, compelling, obliging, etc., the use of the term *constrain* suggests that the neuropsychological level of analysis is somehow privileged — that neuroscientific evidence is decisive with respect to social-psychological theory. Put bluntly, it betrays the idea that social psychology can't make theoretical progress without neuroscience; that — paraphrasing and reversing Neisser (1967, p. 1) — psychology is indeed just something to do until the biochemist comes. This point of view was well expressed by Lucy Brown, a neuroscientist quoted by Benedict Carey in a recent *New York Times* article on social neuroscience ("Searching for the Person in the Brain", 02/05/06):

"Everyone thought phenomena like love and jealousy were simply impossible to study, that they were too variable, too individual. They preferred to think of them as magic." Carey goes on to write: "Imaging and other techniques have now parted the curtain."

So much for the vast social-psychological literature on interpersonal attraction, friendship, and, yes, love and jealousy (e.g., Berscheid & Walster, 1969, 1978; Buss, 2000, 2003; Rubin, 1973, 1980).

Actually, it has to be said that there does not appear to be any instance where neuroscientific findings have constrained social-psychological theory. Of course, social neuroscience is very young. Still, cognitive neuroscience has been around much longer, and it's hard to think of any instance in that field, either (Coltheart, 2005a, 2005b). Perhaps the discovery in the 1950s, by Hubel and Wiesel among others, of "bug detectors" and other orientation-specific receptive fields in frogs and cats counts, because it changed our view about how low-level vision works. But there are very few others examples like that.

To the contrary, it appears that precisely the reverse is true: psychological theory constrains the interpretation of neuropsychological and neuroscientific data. My favorite example is the amnesic patient H.M., who put us on the road toward discovering the role of the hippocampus in memory. But what exactly is that role? The fact is, our interpretation of H.M.'s amnesia, and thus of hippocampal function, has changed as our understanding of memory has changed. First, H.M. was thought to have lost his capacity to

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learn; then to have lost long-term but not short-term memory; then procedural but not declarative memory; then episodic but not semantic memory; then explicit but not implicit memory; and now, most recently, relational but not non-relational memory. Here, clearly, neuroscientific data didn't do much constraining: psychological theory changed almost wantonly, while the neural evidence stayed quite constant.

Now, it might be claimed that H.M. did introduce the principle that memory is not a unitary entity. In that way, neuropsychological data would constrain psychological theory, even if further behavioral research were needed to determine exactly what those systems were. Historically, however, the notion of multiple memory systems was already in theory before any neuropsychological or neuroscientific evidence was available. Similarly, proposals for the modularity of language and perception were generally based on behavioral evidence, not to mention introspection, rather than neuroscientific data (Fodor, 1983). To be sure, neuroscientific data does constrain *neuroscientific* theories – that is, theories about brain structure and function. If you want to know what part of the brain processes memories, evidently you should look around the hippocampus, ruling out structures like the amygdala, and ruling in structures like the parahippocampal gyrus and the entorhinal cortex, which together with the hippocampus comprise a “medial-temporal lobe memory system” (Squire & Zola-Morgan, 1991). But if you want to know the psychological function of that or any other brain structure or system, then you need a well-worked-out theory of memory, and associated behavioral methodologies, already in hand. Thus, the story of H.M. and the hippocampus illustrates quite the opposite of the rhetoric of constraint: that neuroscientific data can be interpreted only within the framework

of a valid psychological theory of structure and function.

As a further illustration, consider a neuroscientific claim that lies closer to the interests of social psychologists: that the fusiform area, near the junction of the temporal and occipital lobes, is specialized for processing faces (Kanwisher, McDermott, & Chun, 1997). The claim is based on both neuropsychological analyses of prosopagnosic patients, who appear to suffer a specific deficit in recognizing faces, and brain-imaging data of face-processing by neurologically intact subjects. Such evidence implies that the processing of faces is somehow different from the processing of other, nonsocial, objects. If true, such neuroscientific evidence might indeed constrain psychological theory. But not necessarily: even if different brain systems processed social and nonsocial information, they might do so according to the same principles. More important, there is increasing evidence that the fusiform area is specialized for expert recognition of all sorts of objects at subordinate levels of categorization – not just faces, but also birds,

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snowflakes, and greebles (Tarr & Gauthier, 2000). As with the hippocampus, neuroscientific data does not constrain psychological theory, but psychological theory — in this case, a theory of conceptual structure — does constrain the interpretation of neuroscientific data.

Paraphrasing the philosopher John Searle (2001), after you've worked out the problem at the mental level, you can kick it over to the neuroscientists to see how the brain does it. But if the analysis of mental function is wrong, then neuroscience offers little more than souped-up phrenology. So let's do social neuroscience – but let's not do it

in the belief or hope that such evidence will “constrain” our theorizing, or rescue us from whatever theoretical indeterminacies we might suffer. That would be to put the cart before the horse. Good social-psychological theories will make for good social neuroscience. After all, psychology without neuroscience is still psychology; but neuroscience without psychology is just neuroscience.

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