Interview

Interview with Martha Farah

■ Martha Farah obtained undergraduate degrees in Metallurgy and Philosophy from MIT, and a doctorate in Psychology from Harvard University. She has taught at Carnegie Mellon University and at the University of Pennsylvania, where she is now a Professor of Psychology and Director of the Center for Cognitive Neuroscience. Her work spans many topics within cognitive neuroscience, including visual recognition, attention, mental imagery, semantic memory, reading, and prefrontal function. She is the author of *Visual Agnosia* (MIT Press, 1990) and *The Cognitive Neuroscience of Vision* (Blackwells, 2000), and editor of a number of books including *Patient-based Approaches to Cognitive Neuroscience* (MIT Press, 2000). Her awards include the APA Early Career Contribution Award, the Troland Award of the National Academy of Sciences, and a Guggenheim Fellow. She lives in Philadelphia with her 4-year-old daughter and her 24-year-old parrot. ■

JOCN: The field of cognitive neuroscience is coming up on its eighteenth birthday. It started with the realization that pure cognitive science and pure neuroscience needed a common ground. You started out as a cognitive scientist and slowly drifted into cognitive neuroscience. Now you are at the center of cognitive neuroscience. How would you contrast the differences between the two fields?

MF: Basically, it's the difference between trying to do something really hard with one hand tied behind your back, and going at it with both hands. Understanding the mind is hard to do. The cognitive psychologists of the 70s and early 80s set about this task with certain overly narrow ideas about what counted as evidence. They took the very good idea that cognition is computation, and made the bad slip of considering computation to be exactly what contemporary computers did. In such devices, hardware and software had been carefully engineered for independence. I can't tell you how many times my profs in grad school made the point that different computers can run the same programs, and different programs can run on the same computer, so that an understanding of human information processing would not be found in the hardware! You were supposed to use reaction time experiments and masking and so forth, and stay away from neuroscience methods.

JOCN: This theme still rages on in the hands of philosophers. Dan Dennett takes the hyper-functionalist view whereas John Searle says the biologic equipment is important. What are your favorite examples that show how knowing the brain science side of a cognitive question has helped in our understanding of the mind?

MF: That's an interesting parallel, though I'm not sure whether anything that we humble empirical researchers do is of much relevance to those philosophers. Their issue is the classic mind-body problem: What is the relation between the mind, with its subjective experiences, and the physical brain? The question of how to study the mind objectively, and whether data from neuroscience will help, is neither here nor there. Whether the bat does reaction experiments or lies in a scanner, we'll never find out ''what it's like to be'' it (in the words of Thomas Nagel).

As for my favorite examples of how cognitive neuroscience has helped us understand the mind, let's start with memory research. In the 70s this was a pretty stagnant field, with a lot of parameter-varying and not much of a big picture. Then the phenomenon of preserved learning in amnesia was discovered, and an explosion of cognitive neuroscience research on multiple memory systems ensued. Today, even pure cognitive psychology research on memory is largely focused on the idea of multiple memory systems.

The study of cognitive development is another area where the neuroscience perspective revolutionized our thinking about the psychological processes involved. For years infant cognition research was concerned with the development of the ''object concept,'' our basic understanding that objects continue to exist even when not in our sight. Infants' ability to demonstrate the object concept seemed to vary depending on the way it was tested, and explaining this variance was a puzzle that motivated a huge amount of research. Adele Diamond made a brilliant figure-ground switch here, applying her knowledge of prefrontal function to suggest that what was developing was the system needed to manifest the object concept in any given task, not the concept itself. Again, this thinking caught on, and a lot of current work on cognitive development focuses on the maturation of prefrontal cortex.

One last example comes from vision research and the organization of object recognition. The well-known vision theories of the 80s, such as Marr's and Beiderman's, were general-purpose. Tables and trees and faces and kitchen sinks were all supposed to be recognized the same way. But the evidence from agnosic patients suggested division of labor for different classes of object. Although one would not suspect it on the basis of the seamless functioning of an intact recognition system, we have separate processors for recognizing faces, geographic landmarks, and orthographic objects. Recent neuroimaging findings in normal subjects parallel the selective agnosias for these categories of stimuli.

JOCN: What is implicit in your comments is that the cognitive system is hopelessly modular at some level. The argument for specificity is appealing, but such arguments frequently fall into the hands of killjoy psychophysicists who look at such experiments and declare things like "**d**' was not measured." When done so, the distinctions go away. It looks like this is the case for many false-memory results where it is becoming clear many claims for false memories reflect nothing but criteria shifts. Do you worry that follow-up scrutiny might challenge such ideas? This sort of reappraisal seems to be going on for the topic of face perception.

MF: I have my own killjoy perspective on these issues, which is that the truth is a boring compromise between extreme modularity and extreme unity. The extreme modularists look at a selective impairment in auditory comprehension of animal names or spatial attention to faces, and conclude that there are dedicated modules for auditory animal name comprehension and attention to faces. Anything is possible, but it is worth thinking a little more deeply about the ways in which damage, including perhaps combinations of partial damage to simpler and more intuitive cognitive components, could produce the same behavior. What gets my goat about this style of theorizing is that its adherents often make themselves out to be the theoretically sophisticated members of our field, advancing us beyond the overly-simple models of traditional neuropsychology. But reflexively going from a new dissociation to a new module is anything but sophisticated—it's just mindless. The hard theoretical work comes in considering the ways in which different behavioral "phenotypes" might be expressed given a smaller number of simpler underlying "genotypes," if you see what I mean.

On the other hand, doggedly maintaining simple, unitary theories of mind just because of parsimony seems absurd in the face of some dissociations and imaging results. The multiple memory systems hypothesis has withstood the psychophysicists' darndest efforts to explain away those dissociations, and I think that the face module will prove real as well. In fact, since you specifically mentioned face recognition, and that's one of my interests, let me say a bit about the recent challenges to the idea of a face module.

When confronted with prosopagnosia (face-recognition impairment) and brain scans showing hot spots in response to faces, one natural interpretation involves a specialized face processor. But alternative interpretations exist, like maybe faces are just harder to recognize, or require more within-category discrimination than other stimuli. Those alternative explanations are perfectly plausible, and I spent several years doing experiments to decide between them. The data were decisive: Relative to comparison stimuli that were equated with faces for all kinds of properties, prosopagnosia disproportionately impairs face perception, consistent with the loss of a face module. The new generation of alternative hypotheses in accounting for this sort of data were forced to be fairly complex: Their alternative to a face module is a module for within-category discriminations (first characteristic) that require expertise to perform (second characteristic) and, in order to exclude abilities that clearly dissociate from face recognition—such as printed word recognition—these expert within-category discriminations are confined to the recognition of patterns with a prototypical spatial structure, such as the layout of features on a face (third characteristic).

To me this verges on "a distinction without a difference." After all, for 99.9% of our species (everyone except dog show judges and subjects in certain psychology experiments at Yale), the only things that meet that conjunction of three criteria are faces! And, if I may be permitted the use of introspective evidence, after one has looked at their Greeble stimuli for a while, they begin to look like faces! Which raises the semantic question: If evolution gave us a face module, and when we are learning to recognize Greebles we recruit that module for help, is it no longer a face module?

JOCN: That is a strong view and it makes a lot of sense. How is the new Center for Cognitive Neuroscience at the University of Pennsylvania going to approach these complex issues? Will you take the imaging approach, patient approach, psychophysical approach? In short, how do you want to see a graduate student in cognitive neuroscience trained?

MF: All of the above! And I'm proud to say that Penn's new Center for Cognitive Neuroscience is state-of-the-art in this wide range of methods, and very much committed to a multidisciplinary approach.

Of course, everyone pays lip service to the idea of multiple methods and converging evidence. You will never hear someone say, "On principle, I will restrict my research program to this one method." But in practice, there is a tendency for individuals to master a certain method and then stick with it, or for an institution to invest in one technology or one group of experts and get similarly stuck. I worry that many of my colleagues have become so entranced with neuroimaging that they think cognitive neuroscience is just cognitive neuroimaging. This is really unfortunate because there are fundamental questions that imaging can't answer and patient-based research can.

For example, if you are interested in *mechanism*, that is, the causal sequence of events that enables a given cognitive function, then you need to know more than which bits light up in a correlated fashion with that function or its components. You need to know what happens when you take out or disable certain bits, or you'll never be able to discern the causal roles of different parts of the system. There's a lot more to be said about the inferences that can and can't be drawn from imaging experiments and from lesion studies, much of which has been said eloquently by my Penn colleagues, Geoff Aguirre and Eric Zarahn.

So in addition to a thriving fMRI facility, which includes both 1.5- and 4-T magnets dedicated to research, the Penn Center for Cognitive Neuroscience has its own patient research coordinator to locate and screen potential research subjects. For the past year, Dr. Marianna Stark has been monitoring admissions at several local hospitals and entering eligible volunteers into a database of focal lesion patients. When an fMRI experiment suggests the involvement of a certain brain region, we can go to that database to find subjects with and without lesions there for a patient-based approach. This combined imaging and patient-experimental strategy was used to great effect in clarifying the role of frontal and temporal areas in semantic memory by Sharon Thompson-Schill, who is now on the faculty at Penn. With the arrival of Anjan Chatterjee, who works on attention, spatial representation, and the role of space in language, we plan to extend the database to capture patients with discrete behavioral impairments, such as neglect, as well as anatomically discrete lesions.

That's where we are on the imaging and patient fronts. We are also believers in good old-fashioned experimental design, like those dreaded psychophysicists that you keep mentioning. We try to teach our students that the most amazing patient or the most advanced imaging method is useless if you don't design the experiments right. Which may seen obvious, but apparently it isn't always! In addition to the Penn tradition of nerdy psychophysicists (with which I proudly identify myself—I have even used *d'* in my research!), one of the Center's new faculty members is Jonathan Raz, a biostatistician who keeps us on the straight-and-narrow in this regard!

Another technique of enormous promise is pharmacologic manipulation, which makes it possible to dissect the neurochemical bases of cognition in normal subjects. Dan Kimberg and others at the Center have been studying the role of dopamine in working memory and executive functions, with both behavioral and neuroimaging measures.

And then there's the computational approach, EEG and its relatives, and transcranial magnetic stimulation, all represented in our Center. And with the faculty hiring that is planned for the coming years, we may well extend beyond even this broad array of approaches.

JOCN: That sounds great, and certainly what you say suggests that the traditional psychology department exists no more. Has Penn, the cradle of learning theory (Rescorla, Gallistel), of evolutionary perspective (Rozin, Seyfarth, Premack) of psychophysics (Nachmias, Pugh, the Hurviches) and linguistics (the Gleitmans and their many students) given itself over to brain and behavior completely? What gives?

MF: Believe me, Penn Psychology will never be any one thing; for better or for worse, we have quite an array of strong opinions about how to study the mind! But the diversity is stimulating, and we cherish it. Psychology has always been a heterogeneous field, and cognitive neuroscience certainly pulls one piece of it closer to biology and further from Freud and Piaget. It wouldn't surprise me if there are no Psychology departments as we know them in 20 years. At this point, however, I see no urgency to drop the label ''Psychology'' for newer-sounding formulations, as some schools have done. The problem is not so much who you are grouped with, as who you are separated from, and good universities promote interdepartmental research and teaching. An often-heard slogan at Penn is that we are ''one university,'' and this has really been my experience in helping set up the Center for Cognitive Neuroscience.

JOCN: We are always consumed by what we are doing. Where do you see the field of cognitive neuroscience going next?

MF: That's a great question. The field has accomplished so much, so fast, I think there's a tendency to just revel in the current state of it, and look forward to many happy years of more of the same. Which would certainly result in more good science being done; the present paradigm is far from having outlived its usefulness. But part of what drew me to cognitive neuroscience in the first place was its revolutionary nature, and I'd like to see it continue to move towards the edges of our understanding, even as they recede.

Twenty-five years ago, we didn't have the foggiest idea how to think about the implementation of knowledge in neural networks, or a hint of the many counterintuitive parts into which the brain carves cognition (like implicit and explicit memory, modality-specific semantics systems), which nowadays seem commonplace. It was an adventure to try to relate brain and mind under such circumstances. Any specific result you got was not just of interest for the specific hypothesis it tested, it also revealed what general kinds of data would be useful, what general kinds of hypotheses would be tenable, even what general kinds of questions would be fruitful to ask. All of these metascientific issues were being worked out then, implicitly, as we did the science.

Now, I see 17 posters lined up at a meeting on subdivisions of working memory, or the relation between face and object recognition, and I have such mixed feelings. The field has matured to the point that we all agree on some fruitful questions (hence 17 posters on a single general issue), some hypotheses worthy of many labs' efforts to test, and some widely-shared new methods. This is fabulous, because we are on our way to some very well-worked-out models of key cognitive systems in the brain. But it also makes me a bit sad to see how straightforward it has all become. It's good for cognitive neuroscience, but bad for people like me with a perverse taste for scientific confusion and ambiguity! Actually, it could be bad for cognitive neuroscience too, if we become so entranced with the current set of tractable questions that we ignore all other questions.

One set of questions that is just beginning to be addressed by a few brave cognitive neuroscientists concerns the more ''human'' side of cognition: The ways in which personality and social and emotional functions shape, and subserve, cognition. I have just started reading in this area, and the work is fascinating. There are still a lot more fundamental open questions than there are answered ones, but the progress so far goes way beyond what I'd have thought likely a few years ago. I suspect that the next 10 years will see these topics succumb to cognitive neuroscience analysis.

My own interest in this area came about from studying prefrontal function. Patients with frontal damage do not run to their doctors saying ''I can't do n-back any more!'' The most marked results of frontal damage are in the social and emotional realm. These patients get divorced and lose their jobs, even though their IQ scores are far less affected than other kinds of patients'. What have these patients lost? How does it relate to the more classically cognitive functions of the frontal lobes? Can individual differences in normal social and emotional functioning be understood in terms of this system? Can group differences in planfulness, styles of conflict resolution, and so forth be understood in terms of this system, and cultural influences on it? Could there be a Cognitive Neurosociology?

Martha J. Farah, Professor of Psychology, Director, Center for Cognitive Neuroscience, University of Pennsylvania, 3815 Walnut Street, Philadelphia, PA 19104-6196. Tel.: +1-215-573-3531; fax: +1-215-898-1982.