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Interview with Michael Gazzaniga

Widely considered the father of the field of cognitive neuroscience, Professor Michael S. Gazzaniga is one of the world's premier neuroscientists. He founded the Center for Neuroscience at the University of California, Davis; the Center for Cognitive Neuroscience at Dartmouth College; the Cognitive Neuroscience Institute; *Journal of Cognitive Neuroscience*; and the Cognitive Neuroscience Society. He is currently the director of the Sage Center for the Study of the Mind at the University of California, Santa Barbara. Born on December 12, 1939 in Los Angeles and educated at Dartmouth College, he received his Ph.D. in psychobiology at the California Institute of Technology under the tutelage of Roger Sperry. As a graduate student, Professor Gazzaniga initiated the first lateralized testing of human split-brain patients, leading to a fundamental shift in our understanding of functional lateralization in the brain and how the cerebral hemispheres communicate with one another. His many scholarly publications and pioneering work during the last 50 years have produced significant contributions to our understanding of how the brain enables the mind. His landmark 1995 book for MIT Press, *The Cognitive Neurosciences*, now in its fourth edition, is recognized as the sourcebook for the field. He has also published many books accessible to a lay audience, including *Mind Matters*, *Nature's Mind*, and *The Ethical Brain*.

TYCN: You performed the first successful experiments with split-brain patients over 45 years ago and you have published extensively on this topic ever since, providing key insights into the functioning of the hemispheres and the nature of the communication between them. However, surgical procedures severing the corpus callosum in order to alleviate symptoms of epilepsy appear to be less and less necessary. What is the future of split-brain research, and given that we still have testable split-brain patients in the foreseeable future, what more do you think we can learn from these patients?

Gazzaniga: Well, to begin with, when we did the split-brain work in 1961 a lot of split-brain research had been completed on monkeys, cats, and chimps, and this work showed some key disconnection effects, which was obviously a big story. People were excited about it. Yet there was also substantial reporting in the literature, starting from the Akelaitis series in the 1940s, that there was no split-brain effect in humans. And so, when I arrived at Caltech, about the time of the first human split-brain patient in Los Angeles, I was given the assignment of looking at whether there were any disconnection effects in humans.

As we now know, it is difficult to see anything with these patients without introducing some kind of special manipulation. But back in the early 1960s, this wasn't known. A few months before I arrived at Caltech, a young patient born without a callosum had been tested and no disconnection effects were seen. So, the mood was kind of mild bewilderment as to why humans

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were widely reported not to show disconnection effects—and in particular why this young patient showed none either. In some sense the task of figuring out what was going on was thrown to me because I was the new graduate student. The lab attitude was, "Oh, we can't get it working . . . . , let's see what you can do!" That was the context, and the expectations were fairly low that anything would happen.

W.J., our first patient, was brought for preoperative testing from his home in Downey, California. I had set up a back projection system and large screen that was so big that I had to tie it down to the floor so it wouldn't sway—it dangled from a water pipe overhead. Using a primitive tachistoscope, I flashed stimuli to W.J.'s left and right visual fields, yet he was completely normal in his responses. He easily reported visual information flashed in either visual field and could also easily name objects held in either hand when held out of view. Then he had the surgery. Both the callosum and the anterior commissure were split in one operation.

A month later W.J. returned to Caltech. I went out to meet him, brought him up to the lab, and put him in front of the same screen and tested him. It was dramatic. Now he couldn't describe anything in the left visual field, whereas he described information in the right visual field normally. Then we created a test where he was required to point to visual stimuli presented in the left visual field instead of speaking. To our amazement he did that just fine. It was unbelievable. After W.J. left for home, I met with Sperry and reported on the session. He was enthralled.

The first case write-up was just about W.J., and it was very exciting. I remember I wrote the first draft and gave it to Sperry—who could write very well. After reading my draft he calmly advised me that I go take a writing class at Pasadena City College (and I had to admit he was right!). Nonetheless, we got the first paper out.

Then other cases started to appear—the two other important being patients L.B. and N.G., with whom we spent the first five or six years defining the differences between the hemispheres. We had basic questions, such as, Was there language in the right hemisphere, and if so, what was that language? Were there other skills, other than language, that were lateralized? Was there any right hemisphere superiority? Was there any emotional spread between the hemispheres? It took hundreds of experiments to get at answers. Eventually it became obvious that there were essentially "two people" in one brain—*the left does this, the right does that.* This realization led to all kinds of bizarre, overstated claims about left/right functions.

There was another major phase of split-brain research where we studied the patients as a way of getting at other questions very much alive in neuroscience, everything from questions about visual midline overlap to spatial attention and resource allocations. At this point the split-brain patients provided a way of examining cortical–subcortical relationships, and other matters.

The next phase of the work was when Joseph LeDoux and I came up with the idea of the *interpreter.* Twenty-five years into studying these patients we finally got around to asking the patients, "Why did you do that?" after they had made a response with the left hand that was being governed by the separated, silent, speechless right hemisphere. We began to understand that the left hemisphere "made up" a story as to why the patient did what he/she did, and in that moment we began to see the cardinal feature of the left hemisphere: *the ability to interpret actions generated outside its realm of conscious awareness.* After this realization a whole series of experiments exploded onto the scene—and have since become a very rich part of the story. In fact, I think this has been the longest running and the richest chapter, and it has the potential for so much more to be built.

As to where the work is going, I think there are two directions. The brain-imaging sciences, as they advance, will try to figure out lots of things, using the callosum as their point of measurement; for example, in terms of interhemispheric connectivity, of individual variation, and of what can change with time and practice—these represent the *structure* side of things. At the more *psychological* level, new work led by Mike Miller and others suggest it may be the case that there are subtle changes in social judgments in the disconnection patients because of the hemispheric separation of key processes involved in such mental actions. For example, it may be that your ethics are only driven by utilitarian considerations after [hemispheric separation] surgery. If that holds up and gets extended, then a whole set of new findings and a series of new interests in social neuroscience will arise, as will implications for the brain mechanisms that are critical to social conceptualizations.

TYCN: You mentioned earlier that one of the early observations in these split-brain surgeries was that it didn't seem to have any effect on the patients. These patients appeared to be unaffected, unless one introduced procedures that revealed disconnection. What does this say about consciousness—that you can separate the two hemispheres and the patient seems to be unified in his or her thinking?

Gazzaniga: The way I think about that is that all of us are always cueing ourselves to spring back to some unified state, a "keeping it together" kind of thing. Even if we are injured or hurt or thinking about something that occurred at another time or place, or even if our attention flashes elsewhere for a few seconds, in the end we are going to come back to a unified moment. We seem to be always trying to keep the story together. You see this in all kinds of neurological patients with focal disease—they may have a particular deficit, but they are doing everything they can to keep a unified story that is focused toward a particular goal. To my mind that's a truism. But then we can say, "OK, well split-brains do it too." To put it another way: if you and I were chained together we would quickly learn to cooperate in a way that makes it look like we are unified and moving together *toward a common goal*—we would constantly be cueing each other.

We saw evidence of cueing early on with the patients. The first example I remember occurred with patient N.G. While flashing colored lights to her we asked, "What do you see?" Every color she saw in the right visual field, of course, she would name correctly. Then we would flash in the left visual field—let's say it was green—and she would guess "red" because the left hemisphere was trying to guess what was being presented to the right hemisphere. The right hemisphere "heard" red but "knew" it saw green. Then, there would be a little twitch, and then the left hemisphere would try to correct itself and guess a different color. We saw that kind of stuff way back in the early days.

Now, we see other wonderful examples of cueing. A great example occurred with V.P., who can speak out of either hemisphere. Two words were presented to her, one to each hemisphere: *break* to the right brain and *fast* to the left brain. The right brain spoke first and started to say *break*; then she stopped herself. At that moment the left brain heard *break*, but since it was flashed *fast* deduced that *break* should be pronounced *brak* and intervened and corrected it to *brek*, *brekfa*, and, finally, *breakfast*! So, the left hemisphere corrected the pronunciation almost in midthought. And just like that it becomes evident that each of us is a *multiagent system*, and yet in the end it's all got to come out of a single mouth. It all looks wonderfully unified and coherent, and maybe since we have our interpreter listening to ourselves too, we think it's unified.

To go back to your question: actually, in the first patient, W.J., there were "battles" between his hemispheres due to frontal damage in his right hemisphere in addition to his callosum section; the frontal damage was from his earlier war wounds. The upshot was, when his right hand was doing something, it was his left hemisphere that initiated and was responsible for the movement. As a consequence, his right and left hands would get into "tugs-of-war" because each literally had a "mind of its own." There was very little capacity in W.J. for one brain half to control both hands. Instead, each brain half could only control the opposite hand. So, for W.J., when he'd walk through a door his left hand might grab the doorknob. He would look down at it, but his left hemisphere couldn't command his ipsilateral left hand to let go of the doorknob because it was really being controlled by the right hemisphere. In order to free himself he had to hit his hand off the doorknob with his right hand!

TYCN: So in all your demonstrations of how the left hemisphere "interprets" what the right hemisphere does, what does that say about free will and, really, our ability to control our own destinies?

Gazzaniga: I think the whole free will issue is ill posed. In general, what we are trying to do in cognitive neuroscience is figure out how the human mind–brain "machine" works. Once this machine gets turned on, the question becomes, How does it work?, just like one asks how any machine—biological or not—works. It turns out the human mind–brain machine is a parallel and distributed system, with control areas throughout. Moreover, there is a narrative going on regarding *meaning* [of what we experience]—each of us has our own interpretation on that score. Note, nowhere in this description is there a felt need to talk about "free will." To me, the issue of free will really relates to our need—or passion—to be held *personally responsible for our actions.*

The way I think about it is, if you are the only person in the world, there's no such thing as "personal" responsibility—to whom would you be responsible? Instead, the concept of personal responsibility comes out of the social contract—that is, it exists in the arrangements and expectations among people in a social context . . . it's *not* in the head. Freedom is a construct at the level of organization of social groups.

A quick, easy way of saying all of this is that brains are determined, but people are free.

TYCN: This brings up a topic related, perhaps, to the past three years, during which you served as director of the Neuroscience and Law Project. Can you share with us what's been achieved by that project?

Gazzaniga: Well, it's an interesting project. Neuroscience flirts with all kinds of discoveries that will affect, at some point, how we think about justice. If neuroscience changes how we think about what is just or fair, then it could have an obvious yet profound impact on our justice system. Consider, as an example, antisocial behavior. If your car breaks you don't think about beating it up; you think about fixing it. Likewise, starting from the scientific view of mind-as-a-machine, if a person engages in antisocial behavior, what's the point of beating him/her up?—obviously, we should want to "fix" the person. The metaphor of *mind-as-a-machine* was nicely captured in an essay by Richard Dawkins.

In contrast, in our culture there is the belief that, in addition to wanting to take an antisocial person out of society and "fix" him/her, we [collectively] want to give the person—metaphorically—a "kick," too! Our "collective urge" to seek punitive action against antisocial people is not really all that hard to understand. All you have to do is imagine some antisocial person harming any member of your family! Just getting the person off the streets doesn't feel quite enough.

Yet, here is a key question: Is it really a biological part of us—*this need to punish*—or can we transcend it and move around it? It's complicated and challenging because there is strong evidence that (a lot of) this desire [to punish] is "built" into us. For example, there are beautiful studies on babies suggesting that both reciprocity and retribution are evident early on. On the flip side, one can look at cross-cultural comparisons and find that, for instance, in Catholicism the concept of human retribution is not evident; rather, God is the one who administers punishment. Humans are only supposed to "contain the situation" and start reeducation, with a final punishment being not ours to render.

So those kinds of issues are huge, and they are not going to be resolved today, tomorrow, or the day after that. But as people change about how they think the brain works, its relationship to culture will change as well. Our colleagues Jonathan Cohen and Josh Greene have done some very clever studies on these things. If you have an increased belief that the world is determined, it lessens how you punish. Jonathan Schooler has found that it also increases how much you cheat. If you have an increased belief that you are free, however, it goes the other way round. So, what

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you come to believe about what the nature of man really is will affect how you think about justice, among many other things.

Those are indeed the big questions. The more immediate problems are issues like whether you want to use brain-imaging evidence in the courtroom? Does it make you more prejudice or less prejudice? Or is lie detection good enough to use in a courtroom? One finds all kinds of arguments. The neuroscientists say it [lie detection] isn't ready for the courtroom, and a lot of lawyers agree. And then along comes a famous lawyer, Fred Schauer, who argues, "Well, why not put it in the courtroom; although a lot of the evidence is poor, why not add some more and let the jury decide?" Schauer's position is that you have to distinguish between the *legal* and *scientific* standards.

So in one sense such issues are nothing more that a "motley zoo" of ideas right now. But I think it's important to remember that the overall goal is to have people thinking about these things at the moment, so that when the neuroscience hardens, the implications for the law will be clearer.

Here's a final example. I'm going to a conference in a couple weeks on predicting violence. It turns out that the behavioral [psychology] guys can now predict, with about 76% accuracy using actuarial methods, whether Jones here is going to be engaged in violent behavior again. That's pretty good, 76%. These guys ask, "Can neuroscience best that?" Well, right now, it can't. But let's imagine that neuroscience comes up with something that predicts with 92% accuracy whether Jones is going to commit an antisocial behavior. This is pretty darn good! Yet what's the extra 12% worth? For consider that the law could stipulate that we have to give Jones that 8% freedom to make the choice not to harm. Indeed, that's one important thing our culture is based on.

Such thought experiments give us reason to keep talking about these important issues.

TYCN: You have mentored a lot of well-known cognitive neuroscientists in the past—including Joe LeDoux, Richard Nakamura, Ron Mangun, and Patti Reuter Lorenz. What advice would you give to a young student entering this field?

Gazzaniga: There are new things to be learned. I think that people should now be given training in topics that we traditionally do not train students: specifically, control theory and nonlinear dynamics, which are at the heart of network theory. The reason we need a few fundamental courses like these in early graduate training is because, quite frankly, I think the field is kind of stuck studying "local phenomena." You know how it goes: when somebody gets an interesting finding—a particular effect that is very much a proximate or local phenomenon—they have their lab study it; we all do this, and we can carry on like this for years. But at some point you look at what you're devoting time and recourses to and say, "We have to change this—it's not going anywhere." If you are lucky you spend a day with a colleague—someone like John Doyle at Caltech!—who opens your eyes up to a new way of thinking about what you are studying. Take Doyle. He uses the hardware–software dichotomy to illustrate the challenges of studying the mind–brain dichotomy. Doyle notes that even though we know all there is to know about the hardware and all there is to know about the software, we have a hard time capturing a description of what is happening when one is enabling the other. In short, when they work together they produce things we don't know very much about, like consciousness. We often cannot even describe such things, let alone understand them [now].

In other words, it is the abstraction of how the hardware and software interact that we are trying to understand, and yet we do not have the level of abstraction or the vocabulary to talk about it!

TYCN: So how does one get there?

Gazzaniga: Well, to put it simply, one has to ask if their research is capturing the broader system. Consider this example. People thought they understood diabetes in terms of pancreatic function.

Then a few years ago along comes gastric bypass surgery, where the stomach is made smaller and is reattached to the small intestine, bypassing a portion of the upper small intestine. The result is an immediate impact on diabetes because a portion of the upper intestine appears critical for the disease. This sort of finding makes one wonder if the effects one is studying capture the phenomena ultimately one is trying to understand.

This brings up a whole series of larger and more fundamental issues about how we educate new scientists and the "system" we have in place for formally working in science. This is all well beyond our current discussion, but I want to say just a couple of words. The way we have our economic structure set up in science is ridiculous: graduate school then postdoc; publish or perish; tenure, and so on. In my view this whole system is set up to "channel" information and resources rather than to enlighten people and foster them. We're all less and less looking at the big picture. You've no doubt heard the joke about Darwin: that [given his idiosyncrasies] he would have never gotten tenure! I think people 30–40 years of age should rise up and say, "You know what, let's change this." Alas, as in so many things, making a dent in the status quo is so very hard.

TYCN: Indeed. Especially when even changing a course number at a university is strewn with obstacles.

Gazzaniga: Actually, one of the things I'm going to work on here at UC Santa Barbara is to start a systems degree in neuroscience. It's got to get done.

Getting back to our earlier point, I think the first year of graduate training should be a mixture of ideas and tools. The hard work of that first year is learning the tools and learning to apply tools to a broad range of systems, from brain networks, to cognitive networks, to social networks.

TYCN: Actually—speaking of social networks—it's well known that you coined the phrase *cognitive neuroscience* with George Miller in the back of a New York City taxicab. Do you think this phrase is still able to encompass the emerging disciplines like systems neuroscience and social neuroscience?

Gazzaniga: Well, I don't know. The early emergence of cognitive science reflected a desire, and attempt, to try to interpret behavior, given that all the stimulus–response mechanisms in the world were not going to explain a simple cognitive act. So the idea of cognitive science, and its associated idea of cognitive representations, was a major victory in early psychology and neuroscience. Nevertheless, in the mid-1970s neuroscience was often devoid of theory, and cognitive science was similarly devoid of any real constraints. Models worked, but was that how the brain actually accomplished the cognitive act? That was the backdrop for the motivation to put cognitive science and neuroscience together. The idea was that if you build a cognitive model you were going to try to find the neural components that led to the cognitive model. So one of the first people that put this into motion was Steve Kosslyn; he was developing a cognitive model of imagery and was trying to find the brain modules that corresponded to his proposed cognitive modules. It seems to me that the new subdisciplines, like social or system neuroscience, are still very much on the same path.

TYCN: You touched on the provocative idea that the way that we do things in departments can be profoundly constraining to meaningful developments in cognitive neuroscience. For a moment, speak to us as editor and the founder of *Journal of Cognitive Neuroscience*. If we are to make changes in how we report our science, what ought they to be?

Gazzaniga: Boy, that's a hard one. In the next generation no one will actually pick up and read a journal! Most likely one will just do electronic searches, find the articles that one wants, and then go on from there. Journal articles will be rapidly collected from a range of different journals. So the whole notion of a collected "place" for scientists or a field to intellectually grow is likely to be challenged, if not abolished.

One specific response to your question is that we are stopping publication of a paper version of *Journal of Cognitive Neuroscience* at the end of 2010. After that the journal will be entirely electronic. We are then presented with a fascinating question—What does this mean for how one takes care of (archives) scholarly information?

TYCN: On that point, in terms the mechanics of the journal, one of the things you did early on was to place the methods sections of manuscripts at the end of articles. Why did you do that?

Gazzaniga: Readability. The notion was that this was a journal for people who were going to read it. In a sense it was kind of a forerunner of the supplementary materials we often see in journals now. It puts the focus on the question that is being investigated, what was found, and the conclusions. But boy, it was not an easy transition, initially. For many researchers it was breaking a "template of a scientific paper." In fact the battle never quieted. And now the methods are "back where they belong."

TYCN: Another innovation was to create a central database for imaging research that was published in *Journal of Cognitive Neuroscience*.

Gazzaniga: I think the idea is fine. It's the implementation of such a database that is very challenging. Again it comes back to the current model that people have to be successful in academics *now*. People are working very hard to get the next thing out, to produce the next little increment. Establishing and contributing to a database takes time away from the tasks at hand.

Of course, I don't know how things will end up in the long run. But what we learned on the data base initiative was that it's not going to work until the field has a phase shift and wants it. There needs to be something that happens in the field—a demand within the field to share information via a database.

TYCN: Speaking of databases and uphill battles, you served on President Bush's bioethics committee. What was that experience like—to discuss with other great scientists the most pressing scientific issues facing the nation and the world, and the ethical concerns about them?

Gazzaniga: Well, I don't necessarily think the experience was quite like that: great scientists pondering profound questions. Things were often just thrown in our lap to think about. Things a lot of us had never really thought deeply about before, such as the morality of embryo research, a question that took up six months of the committee's time. There was also the "cognitive enhancements" issue: was it ethical to allow drugs that enhance, for instance, one's memory or general intelligence?

TYCN: Well, what would you study if you were given $500 million dollars? No conditions?

Gazzaniga: That's a good question. It has been said we need something like a Manhattan Project for the brain. We need a research team that knows there are multiple time scales along which molecular, cellular, systems, behavioral, and cognitive activities occur. They know the problem of cognitive neuroscience is to sensibly talk about how to relate these various levels of organization into a coherent model. The team knows that local phenomena they are studying are just that and that they must be plugged into a larger, more dynamic, model of brain function. Knowledge in brain science is spreading horizontally. We need vertical integration badly.

TYCN: What is today's most unreported research?

Gazzaniga: Who knows, it is unreported. There is a profound inhibitory effect on new ideas by people and ideas that "got there first," telling their story over and over while new observations struggle up from the bottom. The old line that human knowledge advances one funeral at a time seems to be so true!

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