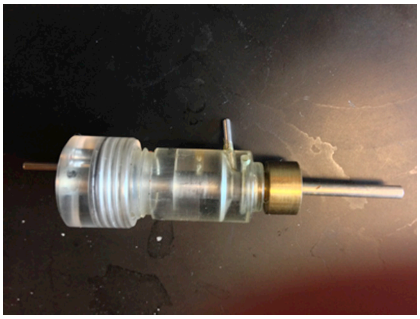
# David Hubel: In Memoriam

David Hubel was a giant in our field, yet he was warm, friendly, and humble in person. He and Torsten Wiesel, following in the footsteps of their mentor Steve Kuffler, discovered fundamental principles of information processing in the brain and fundamental principles of how the brain wires itself up. I think many people in the field see David as a formidable figure, but since I saw him every day in the lab, that is the person I will remember here. After all, it is the guy in the lab who did the work that made him great, and there is surely some connection between the way he daily went about doing science and how successful he was.

David always saw himself as lucky, as having simply been in the right place at the right time. But I think two characteristics I saw all the time, his mechanical inventiveness and his perseverance, were more important than luck. He and Torsten started recording in visual cortex when there was hardly anyone else doing that. But the reason they could do this is because David had invented the tungsten microelectrode, which allowed them to record from single neurons, not axons, which is what people had been recording with glass pipettes, and David had invented a way of sealing the electrode advancer to the cortex so that cortical pulsations did not prevent them from holding single units long enough to characterize them. David, like his mentor Steve Kuffler, did not do science with a lot of theoretical preconceptions; instead, their approach was to figure out some simple way to isolate, insulate, amplify, visualize, record, or stimulate some part of the nervous system. Until his death, Steve Kuffler always did his own experiments; he was constantly inventing new preparations and always did his own elegant dissections. David always did his own experiments and distained people who took credit for their students' and postdocs' work. He made what he needed to do the experiments he wanted to do. He got advice from everyone he could find in order to figure out how to make electrodes out of tungsten wire because he found glass pipettes too fragile and too fussy. He often told me that the most useful advice he got was from the departmental machinist because he knew all about metals. He figured out how to make an electrode by dipping fine tungsten wire in potassium nitrite, while passing current through the wire, which etches the tip until it is very pointy; then you dip the electrode, upside down, in lacquer to insulate all but the tip. You then have to test the electrode to make sure the entire shaft is insulated (you look for bubbles as you pass current through the electrode). You cannot make electrodes in the summer because humidity makes for leaky electrodes. David made his own electrodes for decades and taught me how to make them. When he started borrowing mine, I learned that Frederick Haer would sell us electrodes, made exactly by David's recipe. David had a lathe that he used to make pretty much all the nonelectronic equipment we used. He made the electrode advancers, which were beautifully simple hydraulic syringe-like things that would advance an electrode slowly and precisely through the cortex. They consisted of a precision-fit plunger inside a small Lucite tube filled with oil (which leaked all over) to drive the plunger. He figured out how to make tiny electrolytic lesions to mark electrode tracks by putting an electrode in a raw egg white and seeing how much current you needed to make a tiny white spot.

David Hubel 1926–2013



Hydraulic microelectrode advancer made by David Hubel

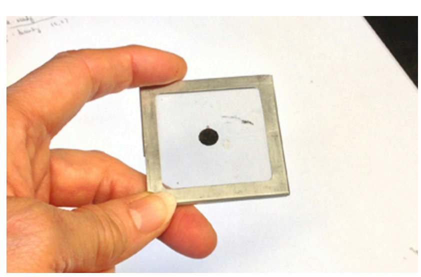
David thought about the brain in the same, mechanistic, down-to-earth sort of way. How does this neuron work; what does it contribute to seeing, to information processing? He says he picked the visual system to study because the visual cortex is easy to find—it is right at the back of the brain, and it is easy to stimulate. We did long, tedious, all-night experiments for many years, as David and Torsten did, and we would often spend hours trying to figure out what we could do to get some cell or another to fire maximally. Studying vision is fun because you see what you show the animal, and when you cannot figure a cell out, you show it everything you can think of; sometimes you find surprisingly specific things that will make a cell fire, like a bright yellow Kodak film box. Torsten says they once tried magazine photographs of women. David was always thinking about seeing, like Helmholtz always putting together his vast understanding of the processes of vision with his own perceptions; when he started having to get up regularly in the middle of the night, he made careful observations of his own vision under dark adaptation.

Even though the experiments were long, we always had a sense of adventure and fun, and I know David thought that doing science should be fun. But I also know that what he regarded as fun, most people would find tedious. I think he liked doing difficult things. He learned Morse code so that he could become a licensed HAM radio operator; I remember when he was studying hard for his HAM license, practicing Morse code constantly so he could send fast enough to qualify for some level. The only other person in our department who shared this passion was the machinist Mike LaFratta, and the two of them, grown men, would gleefully compete with each other as to who had made contact the farthest away. David loved music, and I understand he was an accomplished musician, playing piano and flute constantly and even going to a music camp for several summers. He was always trying to get me to appreciate the complexity of the Goldberg Variations, despite the fact that I am tone deaf; over the years he gave me at least three copies of it.

For decades David and Torsten, and later David and I, would use a heavy cumbersome slide projector to project stimuli on a screen in order to stimulate cells in the visual system. David and Torsten first used brass squares with small holes drilled in them (David liked to machine brass) to make white spots or glass slides with bits of black paper glued onto them to make black spots. It was such a slide that they were putting into and out of the slide projector that led to their discovery of orientation-selective cells. Later, David machined a brass square with little inserts that you could move closer together or farther apart to make narrow or wide bars of light. To evaluate the responsiveness of cells, we would just listen to their firing, or we would photograph the spike train recorded on an oscilloscope. We never did statistics on anything, and David used to quote Rutherford as saying, "If your experiment needs statistics, you ought to have done a better experiment." In the late 70s, David bought a "computer" in the hope of generating more systematic stimuli and quantifying our results. It was a Hewlett Packard 9826; in retrospect, it was a fancy adding machine. David learned to program it in Basic; he was furious when Basic became obsolete and very reluctantly learned to program in DOS; when that became obsolete, he gave up programming. I was not nuts about this "computer" because invariably when I had spent several hours getting the animal prepped, the eyes aligned and focused, and the electrode loaded and ready to go, David would decide that he needed to reprogram the 9826, so the monkey and I would just sit there while he typed and swore at the 9826. He did swear quite a lot.

David had keen powers of observation, and the drive to makes sense of his observations. We kept voluminous notes describing everything we observed about every cell we recorded from, and David insisted that we also take note of what we had to eat (back then nobody thought twice about eating in the lab, and of course you had to eat several times during those marathon experiments), and who might have stopped by to visit. He thought recording everything helped jog memory when it turned out something might be important that you had not considered so at the time. I looked over some of our old lab notebooks and found descriptions of oriented cells, color cells, pizzas, directional cells, and discussions with various people in the department. I found a record from about 26 years ago when I was 9 months pregnant that has in the margin a list of numbers, of decreasing intervals, and then the handwriting switches from mine to David's, and it says "M to PBBH" (Peter Bent Brigham Hospital), and he continues to map out receptive fields by himself, for the rest of the night.

David felt strongly that science writing should be articulate and interesting. He railed against stuffy writing, like using "however" to mean "but," and he recommended *Fowler's Modern English Usage* to everyone. He felt that scientific writing should be honest, conveying the reality of how haphazard real discovery can sometimes be. We wrote papers by typewriter, first one of us writing a draft, then the other marking it up with changes until it was illegible, and then a secretary would

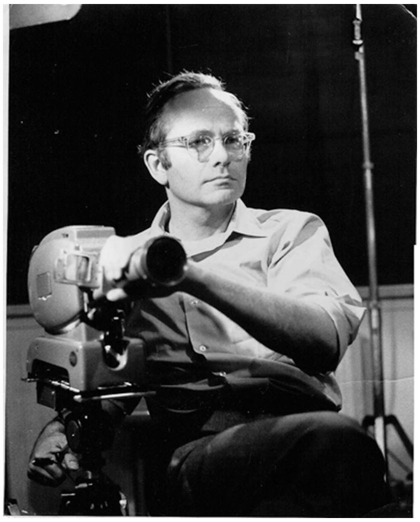


Glass slide with a spot pasted on it to generate dark spots on the screen. This is probably the slide that Hubel and Wiesel were using when they discovered orientation-selective cells. David never threw anything out.

retype the whole thing, over and over. I remember when we were trying to explain, in our paper about the color-selective blobs in V1, why previous physiologists, in particular Hubel and Wiesel, without the anatomical anchor of selective staining, might have missed them. I jokingly started the paragraph, "The historically minded reader may have wondered how so prominent a group of cells could have been missed by such a prominent pair of investigators," and then listed all the reasons why with physiology alone you might mistake them for something else. Then I got back yet another draft and almost fell off my chair laughing when I read what David had appended, "The prominence was ill-begotten." David was thorough. He never wanted to write a paper until we had found out something interesting and had figured out how it worked. He has written fewer than 100 research articles in his entire career, but each is a gem. When we thought we had figured something out, he always wanted make sure, at least several ways, that we were correct, and any further ramifications of what we thought we understood had to be tested too. When we found what seemed to be a system of color-selective cells in V1, we ended up studying them until we had a 48 page paper that covered everything from the layers of V1 to color theory. After that the journal established page limits.

David disliked giant logical leaps or hypothesis-driven experiments; we stuck our electrodes into the brain, pretty much just asking what we would find there. It always felt like exploring. David liked to point out that this is not the sort of experimental approach granting agencies approve of. He said that he doubted whether Galileo had had any kind of hypothesis when he pointed his telescope at Jupiter and observed its moons.

Until he stopped doing experiments, David was not much of a teacher; he was a mentor but mostly by how carefully and thoughtfully he did science. He and Torsten, in the 25 years they worked together, had only about a dozen graduate students and postdocs between them. He and I in the 20 years we worked together had even fewer. He and Torsten did their own experiments, and their students and postdocs did their own experiments. This once led to a peculiar situation: the postdocs and students were excited by H&W's finding of ocular dominance shifts after eye closure in young animals, so they started doing experiments building on these findings. David gathered them all together and gave what has become known as "The Plum Tree Speech." He said he and Torsten wanted to pursue their own results and gather the low-hanging fruit before their own students did, and he encouraged them to branch out to different questions or different preparations. It never entered his mind that he could take credit for what they did. David lectured beautifully but always about work he had done himself. And the work was so beautiful, and his lectures so clear, that he inspired generations of scientists. Yet he did not teach any general courses, I suspect because he was awful about keeping up with the literature. He simply did not read any papers. He was an extremely slow reader; I suspect nowadays he would be diagnosed as dyslexic, but he read carefully and thoroughly and about as fast in French or German as in English. He de-



David Hubel manning the projector that he and Torsten, and later he and I, used for decades to map out receptive fields in visual cortex.

fended his lack of interest in reading the literature by saying that Steve Kuffler always said, "Do you want to be a producer or a consumer?" He once said that a reviewer had criticized one of his and Torsten's submissions (their 1965 Binocular Interaction paper) because they had cited only one paper that was not their own, so in the published version they deleted that citation. When David did start teaching, he taught a Freshman seminar at Harvard College that was extraordinarily popular, with ten times as many students signing up each year as could be accommodated. He taught them things that he thought were important but were missing from most young people's upbringing today: how to solder, how to use power tools, how to suture skin, and how to wire up a simple circuit.

Over the last few days, many people have been telling each other David Hubel stories—he was really funny—so he clearly lives on in a lot of us.

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