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In the 1960s and 1970s, brilliant students in physics and molecular biology were drawn to the nervous system as a field for study. Here attractive problems were galore, just waiting to be tackled: from molecules to memory, from channels to consciousness. In contrast, molecular biology and physics were becoming highly competitive and seemed to have reached a temporary plateau. Students searching for problems realized that they could make major contributions, technical and conceptual, in neurobiology. Most neurobiologists knew little about molecular or optical techniques and disciplines or how they could be applied to the brain. To find out how they should go about it, students from other disciplines flocked to introductory courses on neurobiology in Cold Spring Harbor and the Marine Biological Laboratory at Woods Hole.

An early entrant was Larry. He took Stephen Kuffler's "Nerve Muscle Program" at Woods Hole in 1966. The name of the course gives the game away: in those early days, the word "neurobiology" had only just been coined by Kuffler. Most of the best work was being done on neurons and skeletal muscle fibers. The revolutionary experiments by Hubel and Wiesel were for the first time opening up the brain, but apart from that marvelous work, there was relatively little being done on the cerebral cortex and its relation to perception. Certainly, there was nothing comparable to the experiments of Hodgkin, Huxley, and Katz, who were carrying cellular neurobiology to a new and exciting level. The Nerve Muscle Program, taught by Kuffler with Ed Furshpan and David Potter, allowed students to do experiments every day with their own hands on modern equipment. At the end, they would be able to set up their own laboratories for measuring the electrical properties of nerve cells. Equally important were intensive critical discussions of the literature. A student would choose a key paper on impulse conduction or synaptic transmission, say by Fatt and Katz, and critically present the data and conclusions. Not Larry.

As original then as he is today (actually, not quite as original, as he had not yet started to wear red shirts, red socks, and red underpants), Larry chose a new paper that purported to describe learning at the cellular level in a mollusk. He presented the experiments and their significance in detail. In principle, his talk would have taken 30 min or so, but it lasted for a couple of hours because Ed and Dave interrupted every other sentence. Every tiny flaw in the work and unsupported conclusion was challenged. But Larry plowed on unfazed, because he thought the work was interesting. (The circle was completed more than 20 years later when he himself demonstrated drastic flaws in the work he had first discussed as a student).

What made Larry so special was that he already had a clear idea of what research problem he should work on in neurobiology. From the start, he saw that the beautiful experiments of Hubel and Wiesel were possible only because of their extraordinary imagination, brilliance, and dedication. To study the properties and organization of neurons in the visual cortex and their relation to perception by recording from them one by one seemed an impractical approach. A better technique was needed (and is still needed today). As David Hubel said later, it was like trying to mow the lawn with a pair of nail scissors. A method that would allow one to record from not just a single nerve cell but hundreds or thousands at once could surely be done if one developed suitable optical recording techniques (not thought of at that time).

To this end, he went to work with Richard Keynes in Cambridge to try to record action potentials without electrodes. This experience, too, shows a key aspect of Larry Cohen's character, his loyalty: Keynes could be prickly but Larry recognized his great virtues and maintained a deep friendship and admiration for him until his death in 2010. (A now famous colleague who worked with Larry and Keynes was Bertil Hille). After this introductory period, Larry set up his own laboratory and devoted himself to two key problems: finding the dyes that would best light up during activity and developing the best methods for recording optical signals. He moved up from single axons, to groups of nerve cells in invertebrates, and then to an important biological problem: the organization of olfactory processing that gives rise to the sense of smell. This progression took many years but Larry never swerved from his aim: to observe how tens, or hundreds or thousands of nerve cells process information in real time. Always doing experiments at the bench with his own hands, and always keeping up with the most recent advances in microscopy, he worked in a way that is no longer fashionable. He did not get his results from a large group or rely on teams of students and postdoctoral fellows to execute his ideas. The result was a small group, four or perhaps five people who worked and often lived together in harmony. On occasions when funding for salaries ran out, Larry would pay for the students' support out of his own pocket.

One fringe benefit for me of having Larry as a friend and colleague was that I knew where to send brilliant students who were looking for a new laboratory. At the end of a course at Cold Spring or Woods Hole or elsewhere around the world, a really bright young person like Brian Salzberg or Bill Ross or Amiram Grinvald or Chun Bleau would ask me "Where should I go for a post-doc? I'd like a lab where the person is doing first-rate work, where I could use my experience and at the same time do something really interesting." If they were exceptionally gifted and dedicated I would send them to Larry.

I believe that, as this issue shows, everyone who has worked with Larry, me included, loves him. (Another side of Larry, his sense of humor, is described in "Pioneers of Neurobiology: my brilliant eccentric heroes" J. Nicholls, Sinauer Associates, 2014.)

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