

“Charlie’s Lab”

Charles G. Gross^{1,2}

For me, the gatherings in Princeton on May 25 and 26, 2013, were a wonderful way to mark my formal transition from Professor to Professor Emeritus (“one who has served his time”).

I have had a pretty long and good run of it, starting with a study of plant succession on Big Burnt Island in the narrows of Lake George in the Adirondacks and running through, inter alia, a failure to condition a blowfly in Carroll Williams lab, a failure to solve the enigma of bird navigation with Donald Griffin, and a failure to solve the riddle of the frontal lobe in Larry Weiskrantz’s lab. So rather than continuing in this over-ambitious vein, I took a different tack. The idea was simple: to recruit collaborators more capable than me, especially those who were, unlike me, able to use taps and drills, calculate analysis of variance, remove dura without damaging the brain, build an amplifier, banish 60 cycles, mount a coronal section of a monkey brain, mix a molar solution, etch an electrode (without smelling almonds), and deal with a difficult department chair (who was trying to steal my money).

I succeeded in keeping many of these hypercompetents around for a long time. Many started as undergraduates, continued as graduate students, then postdocs, and eventually research associates and lecturers, by which time they had little knowledge of another world beyond our lab. Graduate students were the easiest to keep on as postdocs. They must have realized that, after investing four or five years learning how to eat a tub of yogurt for lunch and such, it was worthwhile to spend another four years in situ actually getting some research done. Many who left the lab to go to graduate school or take up postdocs then came back three days to three years later, proving that, after a time in my lab, they were unfit for the outside world.

In my 50 years as a professor, there were (almost) no negatives and a great heap of positives: I could research anything that caught my fancy. I taught whatever I choose. NIH always and continuously supplied the money (but did once make me wait for a while). I picked my coworkers and kept my own hours. Some decent journal always took our stuff with a minimum of changes in content. Deans never got in our way and, eventually, were helpful. There were always free trips to exotic places, sometimes for months at a time. The fact that almost nobody in the field believed in our face and hand cells kept the playing field empty for over 15 years, and even then, for quite a while, all the others did was to replicate our results. Only on one subject

(neurogenesis) was our work attacked in print. I was never burdened (or trusted) with any significant departmental or university administrative duties. Nobody ever asked me to solicit funds from some rich person. The machinist usually produced any lab toy I could imagine. Keeping monkeys was always hassle-free, at least until very recently. Rare in research science, there was never an authorship or priority dispute in our lab. In a timely fashion, I was elected to fancy clubs from the Psychological Round Table to the National Academy of Sciences. On the whole, my students and technicians succeeded in that they went on to positions as good or better than my own.

Of all the experiences—intellectual, careerist, scientific, travel, and social—one was the best of all and the one I miss the most after closing the lab. This was the intimate fellowship of our laboratory community consisting of most everybody hanging out: undergraduates and techs to visiting professors with graduate students and postdocs at its core. In trying to describe this community lab life together, I am reduced to a (disordered) list of what we did together: all-night experiments; hiking in the White Mountains; canoeing on Lake George and the Delaware; writing our grant renewals every five years or so; roasting goat, lamb, and pig in my backyard (the geese were not a great success: too much fat); showing visitors our *raison d’être*; eating lunch together; playing with our baby monkeys; and, arguably the central community activity, the weekly lab meeting.

The lab meetings were over lunch, lasted at least an hour and a half, and took priority over experiments, surgery, life, everything. We would take up proposed experiments, recent results, critiques of recent departmental speakers, paper drafts, new directions for our research, recent published papers, and grants and papers that I was given to review. Frank critiques, often brutal, were the tradition, especially with regard to new experimental ideas and interpretation of results. Meetings were often heated. (After all, that had been the norm when I was a graduate student in Cambridge, England, and a starting professor in Cambridge, Massachusetts)

As we well know, memories are fallible reconstructions, so some of this may be a romantic fiction. But this romance, fictitious or otherwise, is the very essence of the *Festschrift*.

Charles G. Gross
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[For more details of science and life in the lab, see my article in *The History of Neuroscience in Autobiography*, Vol. 6 (Oxford, Oxford, 2009), pp. 96–157.]

¹Princeton University, ²New York University