The Way Biomedical Research Is Organized Has Dramatically Changed Over the Past Half-Century: Are the Changes for the Better?

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Biomedical research in today's universities is usually carried out by groups consisting of a leader and 5–20 or so trainees. This is in sharp contrast with past generations, when research was usually done by individuals or small partnerships of two or three who thought up their own ideas and carried them out themselves. Group leaders today spend their time in an office, on a wide variety of administrative tasks, and have little or no time left for work at the bench. I recommend that leaders try to change the system by forming smaller groups and insisting on reserving their own time at the bench. I suggest that trainees, before beginning their research, look for laboratories where groups are small and independent, with leaders engaged actively in research.

The past half-century has seen a profound change in the way biological science is conducted in university laboratories. In this essay, I discuss the reasons for the changes and ask whether the changes are for the better—and if not, what can be done about them. My field is neurobiology, but what I have to say probably applies to biological science in general, and perhaps also to physics and chemistry. I became acutely aware of the magnitude of the changes when I thought back on our field as it was during the early decades of my own career, the 1950s, 60s, and 70s. I will start by describing how science is practiced today and compare that with how things were one or two generations ago.

Over the past half-century, I have been a member of the Harvard Medical School Department of Neurobiology. Our department is fairly typical of today's large university departments of biomedical science. It is made up of about 20 groups, each consisting of about 10 or 15 scientists at various stages in their careers. Leading each group is a senior scientist, a full professor, or an assistant professor who can expect to be promoted to tenure in a few years. Under the leader come postdoctoral fellows, graduate students, and a few research assistants. This form of organization is in sharp contrast with what prevailed a generation ago, when a group consisted usually of one person, or two or three, and there was less distinction, if there was any, between group members.

What do the people in present-day groups do? The leaders spend much of their time in their offices in a variety of tasks, some of which are closely related to their science but most of which I would categorize as "administration". This includes raising money for the research, supervising the members of their groups, attending department seminars and committee meetings, keeping up with science literature, refereeing papers submitted to scientific journals, and serving on national committees that evaluate grant requests. They participate in formal teaching to medical students, graduate students, and undergraduates. All these activities are what keep group leaders out of their labs, where the actual science is done.

Of all these administrative activities probably the most time-consuming is raising money. To prepare a grant proposal for 3–5 years of support can take many months full-time, and to support today's large groups several grants may be needed. In each proposal, the group leader has to summarize in great detail previous research and describe plans for proposed research—again in great and often unrealistic detail. The proposal will be examined and judged by a committee of one's fellow scientists from around the country. The huge expansion in the number of people involved in biological science in the past several generations has greatly increased the number of proposals, so that a far smaller proportion of grants can be funded. Too often it is the most adventurous (and perhaps highest risk) proposals that fail. Meanwhile, less money has been available thanks to our previous administration's avowed contempt for science. Funds have been cut back so severely that America's continued leadership in science is in serious jeopardy. That a country's world leadership in science can go downhill with breathtaking speed may not be obvious to those who were not around to see what happened to German science during and after World War II.

Today the actual experiments, the nuts and bolts of the science, are carried out by the postdocs, graduate students, and technicians, not the group leaders. Between today's science and that of a few decades ago this is probably the biggest change. Some of the bench work in science is necessarily repetitive and tedious, and one can hardly blame a leader for avoiding such work, especially if he is used to making lofty decisions in an office. But not all bench work is tedious—the recording of activity of cells, doing dissections, running imaging studies, developing new techniques—can be challenging and fun, especially when there is the possibility of carrying out, at the bench, an idea that one has thought up oneself. Often it is in the course of doing repetitive tasks that one gets ideas. Most importantly, today's organization of science tends to deprive a young scientist of one of the most important learning experiences, that of thinking up a project of one's own and carrying it through; deciding for oneself, independently, whether to persist or to give up and switch over to something else.

The final products of a research group take the form of published papers. The listing of authors of a paper has changed substantially over the past generation or so. Today the list begins with the people who did the bench work, in descending order of importance. Tacked on at the end, invariably, comes the group leader. That name represents the person who is Principal Investigator on the grant, who got the money, and led the laboratory. The reason for the change is clear: to raise the money needed to support a large enterprise, with its many post-docs and graduate students, the leader must have his or her name on many papers. Just a few decades ago, the order of authors represented the importance of contributions, the last name being the least important. The list stood for the scientists who thought up the ideas, did the work at the bench, twisting the dials of the equipment. It seems unjust that today it should be the last author in the list who will get the main credit for the work.

In thinking about all this I have been struck by the radical nature of the changes in science styles—at least biological science—over the last few decades. My own career provides an example of the old style, and I suspect that my experience is far from unique. My undergraduate training was in physics and mathematics, followed by medical school and 3 years of hospital residency in neurology. After coming from Canada to the USA for the final residency year, I had to do 2 years of military service, but was lucky enough to be posted to the Walter Reed Army Institute of Research, to a small division of Neuropsychiatry that consisted of about ten very able young scientists, led by David Rioch, a famous senior psychiatrist. I was assigned as an apprentice to a young neurophysiologist named Mike Fuortes, an Italian, very vigorous with a wonderful feeling for biology. We worked together for 6 months on a cat spinal cord project that ended up being published in a major journal. Though I was clearly Mike's apprentice, he treated me as a partner, listening to any ideas I had and suggesting we try them out if they sounded at all plausible. We worked with no supervision from higher up. The attitude of the entire group, from David Rioch on down, was to let people do what they wanted to do, regardless of seniority. The money came from the army. There were no elaborate grant proposals, and one had only to convince some army general that fatigue was important to armies and fatigue was largely a matter of the nervous system.

At the end of those 6 months, Mike suggested that I take on a project of my own, and he set me free to do my own experiments. I groped at first, but had the full support and encouragement of the entire group, none of whom were engaged in research that had anything to do with my project. From one week to the next, no one asked what I had done the previous week. No one ever asked to see my notebook describing my progress or lack of it. Sometimes months went by with no progress at all, and I had to decide whether to give up and go on to something else. A big advantage of the Walter Reed years was that as an MD I was, in effect, a postdoc and had managed to avoid the courses and close supervision and committee meetings that a graduate student has to survive, including the necessity of writing a book-length thesis as one's first writing assignment. I have always been thankful that I managed, not through deliberate planning, to bypass all those graduate-school years.

In its independence and sometimes loneliness my story was far from unique. In Baltimore I had come to know Vernon Mountcastle, who was about 10 years senior to me and had already become famous for establishing what is now known as the columnar organization of the cerebral cortex. I visited him one day in Baltimore—I had been at Walter Reed for 3 years and my work was becoming known, so I could dare to waste the time of someone so famous. I arrived at his lab around noon and found him working alone, recording from an anesthetized macaque monkey. I asked him when he had started the experiment, and he answered "in the morning", which I finally realized was the morning of the day before. So he had worked, by himself, all the day before, all that night, and that day until noon. What was typical, in that era, was not only the long hours but the fact that the project was done by one person, single handedly. The major papers were either by Mountcastle alone or in partnership with one other person. The leader of the physiology department was Philip Bard, but the idea that Bard should have asked to have his name on any of Vernon's papers surely never occurred to anyone.

From Walter Reed I went on to the laboratory of Steven Kuffler at Johns Hopkins, where I began a partnership with Torsten Wiesel that was to last 25 years. In 1959 our group of six scientists, led by Steve, all moved to Harvard Medical School. Everyone in the group worked alone or with one other postdoc, on projects they thought up themselves and carried through independently. The papers that resulted were written by one person or two and were handed to the others for criticism. Our group gradually expanded but always consisted of tiny subgroups working independently. In 1965 we finally formed the world's first neurobiology department.

In those days, time spent doing administration was minimal. Everyone followed Steve's example: as department chairman he would come in at 9:00 AM, answer a few letters, and then work alone at a bench dissecting a frog or leech. If decisions had to be made, there were never formal faculty meetings, but discussions took place in the corridor when people met by chance. Papers were published under one or two names, rarely three. Steve never suggested that his name should be on one of Torsten's and my papers—he would have considered the idea outlandish. As Torsten and I became more senior, finally making tenure, we continued to work as partners. We had our own postdocs and graduate students, though never more that five or six at one time. They all worked on their own projects, alone or in partnerships. We never put our names on any of their papers, and they never appeared on ours. That was the prevailing style.

What has slowly dawned on me is the degree to which that way of doing biological science in universities was almost universal, probably up to the 70s or 80s. In biology one can think of a pantheon of great scientists who laid the foundations of what the field has become today. Names include E.D. Adrian, Brian Matthews, Bernard Katz, Giuseppi Moruzzi, Alan Hodgkin, Andrew Huxley, Stephen Kuffler, John Eccles, Seymour Benzer, Sydney Brenner, Jacques Monod, Francois Jacob, Max Perutz—the list goes on and on. These people worked alone or in groups of two or sometimes, rarely, three.

Why have things changed so profoundly? I'm not sure of the answer, if there is any one answer.

In looking for causes it may help to compare the situation in two other fields, medicine and music. One's skill as a doctor must necessarily depend on seeing many patients: one does not get good sitting by one's self in an office. A doctor's success is measured by his ability to diagnose and cure, not the ability to run a hospital. Until his retirement, Wilder Penfield performed three brain operations a week, as well as running the Montreal Neurological Institute. Neither pursuit was allowed to take over the other. But I must admit that for the USA to cite medicine as a field free from administrative burdens seems a stretch given today's sapping of doctors' energies by lawsuits and quarrels with insurance companies. In music the case may be clearer. Rudolf Serkin did not abandon the piano just because he was running Vermont's Marlborough Festivals. Today's scientists must strike a balance between the time committed to raising funds, teaching, committee work, letters of recommendation, and all the rest—and the science for which he or she was trained and presumably was the original focus of ambitions.

I am not convinced that the changes I have described have been inevitable or are irreversible. We seem to have slid into a way of organizing ourselves that we have not had the guts or wisdom to avoid or overcome. A group leader may have convinced him/her self that ten postdocs and graduate students are necessary, that bigger is better. One can give in to the idea that more scientists in a group means more papers and better chances of funding. It can be hard to resist the temptation to expand, to take on more obligations that take us away from science.

What, if any, suggestions do I have to reverse these changes? First, I would recommend that a young scientist, in deciding where to go to graduate school or what lab to join as postdoc, pay a visit to see if the leader is at a bench doing experiments or is sitting writing in an office. He should ask the postdocs and graduate students whether they are running projects they thought up. How soon will one have one's own project and be left to work it out alone? Do the lab leaders have bench space they regard as their own and projects that they carry out themselves, perhaps with one or two partners? Are names on papers confined to those that thought up the ideas *and* did the experimental work?

I have no illusions that biological science is likely to return overnight to the system that prevailed a generation ago, but I believe a start could be made in that direction. If I were 40 years younger and a group leader and found myself imprisoned in an office most of the time, I would adopt a 5-year plan to change my scientific style. I would choose a project to share with one partner, put aside a lab bench that I could call my own, and submit a research proposal to fund that project. I would encourage any postdocs in my lab to do the same, with their own funding and independent projects. I would give advice gently and sparingly, realizing that strongly worded advice from a senior person can be hard to ignore and that in science making one's own mistakes can be an important part of learning. I would limit my committee assignments to one or two and encourage my more senior postdocs to do the same. (I vividly remember asking George Wald, of visual-pigment fame, how he managed to avoid all the wasted time on committees. He answered: "It's simple: I accept all committee assignments, and never show up for a meeting.") I would make it a rule that a name on a paper means that one has actually sat at the bench twisting the dials. I would continue to teach because I enjoy teaching and think I do it well. One has to learn to teach and to develop one's teaching style, and for that reason I would give everyone in my group the chance to try it.

In short, in my system the work would be shared—the science bench work, the writing of papers and research grants, the committees, and the teaching. The object would be to broaden the experience of the younger members of the group and lighten the duties of the leader, who could get back to doing active science.