All in the Family:
The Human Side of Guiding Academic Research

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Why Am I Here?

We are all here as part of a celebration of the value of research, and for accomplishing a specific body of research; I am also here to represent the many people—my students, colleagues, collaborators—who contributed to that research.

We have worked in a field called "materials science." Materials science is a mosaic—pieces liberally borrowed from other fields, and stitched together to solve problems that these fields, by themselves, do not; it is a multidisciplinary field. I am a chemist, as are some of my colleagues; but others in the group have been physicists and electrical engineers; biologists and chemical engineers; experimentalists and theoreticians. People who were not scientists have also been important in what we have done, and even more important in how we have done it.

Two aspects of the research honored by this award are interesting: what it is; and how it was done. Curiously, the second now seems to me to be the more important. The particular research we have done is history: it will never be done again. The way it was done may teach a lesson that can be applied to other problems.

Our research has required us to integrate ideas and techniques from a number of fields. This style will, I believe, become more important as the problems faced by science and technology become more complicated. I propose to take this occasion to suggest that this type of research requires as much attention to its human aspects—to the social structure of the research group and to the people involved—as to the science.

My line of reasoning has five parts. i) Research is important to society. ii) Universities are crucial to research, both for their ideas and results and for their teaching of future teachers. iii) The problems science is addressing are increasingly complicated, and the solution of complicated problems requires multiple skills, both technical and human. iv) University research is often poorly organized to work in this style. v) Organizing and managing an intellectually and psychologically heterogeneous group so that its members cooperate rather than compete requires a skill of its own. Guiding research that solves complicated problems may require as much creativity as solving them.

The conclusion that I would like to suggest is that we, in science, may spend too much time thinking about the destination, and not enough about the road to get there. We should of course think about the solutions to problems, but also about the people who solve them.

To begin my story, and as background, let me first describe our research.

What Have We Done?

We have developed methods of controlling the surfaces of materials, and for fabricating materials into shapes having small dimensions. Materials: wood and glass, steel and silk are what the world is made of. Materials are composed of atoms and molecules, and these are made of electrons and nuclei, and those of gluons and quarks, and so on. People who use materials don't care about these details, any more than people who use clocks care about their internal machinery: they care about observable properties—the transparency and hardness of glass, the strength of steel, the softness of silk.

We are clockmakers. We design and make molecules—the clockwork, the invisible, microscopic elements of which organic materials are made—and thus control the properties of these materials. We are especially interested in two aspects of material science: surfaces, and very small structures—structures ranging from only a few atoms in size to about a million atoms. We have, for example, designed organic molecules that assemble themselves on surfaces into crystalline films exactly one molecule thick. These films (now familiarly called self-assembled monolayers, or SAMs) make the surfaces adhesive, or slippery, or wettable, or resistant to corrosion. They provide a rational bridge between the microscopic world of molecules and the macroscopic world of properties. We have also developed a technique called "soft lithography," a set of methods for making wires, channels, and other structures having dimensions on the micrometer and nanometer scale. Using soft lithography, we can, for example, make systems of pipes, like those that deliver water to an office building but a million times smaller. We, and others, use these so-called "microfluidic" systems to manipulate living mammalian cells in ways that help developers of drugs.

This type of research requires ideas from many fields. The technical success of microfluidic systems, for example, depends on combining ideas from organic chemistry, polymer engineering, electrical engineering, fluid physics, surface science, and cell biology. The technical problems in this research are those common in all research; the art of the research is that of putting together groups that have the necessary skills to solve these problems, and of getting them to work together collaboratively.

I am surprised to find that I have come, over the course of my career, from a view that all that matters is the science to a view that what matters most is the people who do the science. If the people are creative, interested, curious, and comfortable in what they do, good research, and good science, will happen.

Why Are Science, and Research, Important?

My point is that complicated technical problems require complicated research groups to solve them, and that these groups require special care and feeding. But why do we care about solving these problems?

Society has great hopes for science and technology—for their ability to explain the world, and to solve problems. There is no shortage of things that are transfixingly interesting that we don't understand, and no shortage of practical problems that must be solved. The origin and nature of life, the basis of self-awareness, the invention of intelligent machines, and modeling the earth all are areas where our understanding ranges from inadequate to none. New ideas for supplying water and energy, technologies to reduce economic disparities between regions, green technologies, reducing disease—these are equally engaging, but more immediate and practical. All require science and technology for their solution. All depend on research.

There are, of course, many different flavors of problems that research solves. There are problems in biology, problems in mathematics; there are problems in science, problems in engineering. I want to emphasize the distinction between problems that are complicated and problems that are difficult. Complicated problems have many parts, and require pulling together ideas from different disciplines; difficult problems are usually solved by individuals with a flash of insight.

We have most often been concerned in our research with complicated problems. We usually also find them difficult.

Who Does Research? The Special Role of Universities

Above all, people do research. Organizations—universities, companies, governments—house, pay, support, and annoy them. I work in a university. We in universities have our place in the chain connecting problems, ideas, and solutions. We generate fundamental science: science whose applications are in the future, science based on curiosity. We try ideas that are too silly to be tried in a serious place. We generally behave badly: we ignore budgets, refuse to write reports, and we complain. We also educate and train students—the next generations of researchers and teachers. These activities may seem trivial when compared with the serious problem-solving done in industry, but they are not. Many new ideas come from universities, and almost all of the people who generate ideas pass through universities as students. As industrial and governmental research becomes more short-term, university research, with its longer view, provides more and more of the new ideas.

Most academic research is a cottage industry—small research groups, with idiosyncratic leaders, operating in departments that are either traditional or archaic, depending on how charitable you feel. Many academic research groups are still modeled on the European research groups of the 1850s. This model had a professor as the hub of the group. The students were spokes. The professor assigned a research problem to each student; the student, acting as researcher, scholar, and sometimes pair of hands, worked on this isolated project.

How well does our entire system—individuals, universities, companies, and governments—work? Industrial research has become very focused and conscious of cost—at the cost of its creativity. The longer-term work is increasingly left to universities. Are the small groups that we now favor in universities the ones that will solve the big, complex problems that science and society are serving up? Are these the groups that can model the earth, or give birth to intelligent machines? Probably not. They are intellectual purebreds, in a world that may need more adaptable mongrels.

I certainly have not always been so concerned with people, nor have I been what I would have described when younger (with scorn) as a "manager." At the beginning of my career, I was monomaniacal about ideas: people did not come into my thinking. Why has my opinion changed entirely? And how did "I" become "we": that is, how did our research group reach its present state, where it is a collective entity? It is, I hope, not leaderless, but equally certainly, not a group in which I dictate what is done.

The answer is that we changed because we had to. The problems demanded it.

We simply could not, as a small, conventional academic research group, solve the most interesting problems. To solve these complicated problems, we had to use multiple skills, and work collectively. And pulling together such a group required a different structure. (It also required that I learn at least a little about how to manage!) To understand my transformation－from solitary academic scientist to manager (I still cringe at the word) of a multidisciplinary group－I’d like to start at the beginning, to explain how, and why, the butterfly grew up to be a caterpillar.

I’ll begin with my childhood. We are, after all, what we grew up to be after we were children.

My Education

I had a calm, colorless childhood. I was born in 1939, in Kentucky, a state in the Midwest of the U.S. that was far from anywhere. As World War II ended, I was riding bicycles; as the Cold War reached its most dangerous phase, I was learning to drive. I did not have a sophisticated beginning.

My mother was a very literate woman, who believed absolutely in the power of language to hold the world together. She taught me to speak and write. My father was a chemical engineer and small businessman, who founded and ran a company that made products for concrete repair. He loved Edisonian invention and tinkering, had a fine, quiet, Midwestern sense of humor. He believed absolutely that self-sufficiency was the ultimate necessity－a belief learned in the Great Depression. He taught me to compete. I had one brother, Tom, with whom I fought constantly, as brothers do.

I left Kentucky at 13 for school in Massachusetts. High school and college were mostly spent studying and proving that I was never going to be a professional athlete. I went into chemistry because I liked the manual parts of it. As a teenager, I was a technician in my father’s laboratory－washing dishes, distilling solvents, measuring the lines of rust that formed around scratches on panels used to measure the durability of paint－and I enjoyed the work: it was satisfying and predictable. Before settling on chemistry in college, I tried mathematics (I did not have the exotic talent required to be a mathematician) and English (I did not have the patience). I was attracted to a university life because I loved following my nose in research, and because I did not want to have a boss.

I went to graduate school at Caltech (by accident—I had intended to go to Berkeley, but their admissions office lost my application). I ended up working with Professor Jack Roberts. Both were fortunate accidents; he and I got along very well. Once I was started in research, he left me largely alone to make mistakes as I chose—a delicious state of affairs!—but he was merciless when he rewrote the papers. I worked in NMR spectroscopy, and spent most of my nights alone with a large machine that had emotional problems. The compounds I studied would cheerfully burst into flame when exposed to air: sinks filled with flaming solvents were a source of much amusement and interest. I played volleyball, worked odd hours, and listened to 60s folk and rock on the radio.

Great fun. Not much effort required. Because we students had our own projects, usually quite different projects, the culture required no ability to work with others: in fact, rather the opposite. My own style was to work while others slept (to have unimpeded access to the instruments). A sort of genial autism—an inability to imagine what others might be thinking—was an honored norm in graduate student behavior.

I loved this period in my life. Jack Roberts allowed us great latitude in what we worked on. He had abandoned the "hub and spoke" model of the classical Germanic research group in favor of what I will call the "air-traffic-controller" model: he gave us some instructions when it was time to take off, and some when it was time to land, and in between we flew on our own. He believed, and I infer that he still does, that science is an individual enterprise.

We graduate students got along more or less well personally, but did not cooperate: in fact, we competed fiercely. We constantly played a game. The game was this: through quickness of wit, to see simple, elegant solutions that no one else had found to our own intellectually vexing experimental problem. Being smart was a good idea; being very smart was an even better idea; being first to the result was best. Whether our problems were broadly important, or just academically cute, was irrelevant.

This style of research limited the choice of problems to those small enough to be digested by one student. But bundling a number of related problems could sometimes define a field.

From Caltech I went to M.I.T., where I was a member of the faculty for almost 20 years. The style of research that I used at the beginning, when my research group was young, was essentially the same as the one I had learned in graduate school, except that now I was director, not student.

My reeducation, and the reeducation of the research group, began at M.I.T. The Institute is a wonderful place, and permeated with the engineering spirit. Many instructive events occurred at M.I.T.; let me recount one.

Cambridge was one of the wellsprings of biotechnology. When biotech first began to emerge, the National Science Foundation (NSF)—an agency that supports research in universities in the U.S.—decided that it should have a program in it, whatever it was. Not knowing how to start a program in a field it knew nothing about, NSF called the then-chairman of M.I.T.'s Chemical Engineering department, Ray Baddour, and said, "If you'll start a program, we'll pay for it." Ray agreed to do so, but because he also did not know what biotechnology was, he covered his bets: he designed a program that was a celibate Noah's ark—one of every animal, rather than two: one cell biologist, one fermentation engineer, one enzymologist, and so on: perhaps a dozen people. He was clearly betting on interspecies breeding.

For this group, he needed a skilled biochemist, but he simply could not recruit one. Having failed with the experts, he asked me—a very junior member of the chemistry faculty. I knew nothing about the area; I was not qualified to do the work; I had never even worked with a protein. But I was curious, and I always needed money, so I said "yes." And from that came five years of close associations with colleagues at M.I.T. who were experts and who were deeply motivated to teach me as much as they could so that I could be helpful to them.

Baddour had no reason to offer me a place in the program other than desperation to finish his proposal to NSF. I had no reason to accept his offer other than curiosity and greed. But he did, and I did. Much of the research in later programs in our group—organic synthesis using enzymes, biocompatible surfaces, microfluidics for cell biology—stems from what I learned in that first, collaborative project. I also developed colleagues in a spectrum of intellectual colors who have provided continuing instruction (not to say amusement) over many years. This program was my first experience in saying "yes" without thinking too carefully, and it whetted my appetite for collaborative research.

At Harvard

In 1982, I moved to Harvard. There, I joined a group of scientists and engineers that advised a federal agency named DARPA (The Defense Advanced Research Projects Agency—the agency that had, in fact, invented materials science as an academic discipline after World War II). DARPA works on a wide range of technologies, and is concerned with solving problems. It is, in that sense, an engineering organization. But it hopes for solutions that are very advanced—technologies that are fundamentally new—and for that it requires new science. The group that I joined was a mixture of materials scientists, electrical and structural engineers, and physicists. Almost no chemists. In a foreign country, one quickly learns a foreign language: I learned to speak "engineering."

In the 1980s, the program managers in DARPA were concerned about perceived limitations to improvements in microprocessors. So, they posed to the U.S. technical community the problem of finding new methods for making microelectronic widgets that might circumvent these limitations. Our group conceived the idea of forming patterns by printing, rather than by photolithography (the technology used then, and now, to make microelectronic systems, a technology loosely analogous to photography). DARPA gave us money to try our ideas, and then more money to improve them. Over about 10 years, we developed these methods to the point where we can replicate dimensions the size of large molecules by molding—dimensions smaller than can be made by photolithography. In fact, these and related methods are only now beginning to be considered seriously for applications in microelectronics—making a circuit is a very demanding technical problem. These methods will probably never compete with current technologies in making high-end microelectronics, but they are very promising for new technologies such as organic electronics. They have, moreover, proved perfect for making the microfluidic systems that are so useful in biotechnology.

When we got the money for this work, we knew nothing about microelectronics. But students found the subject interesting, and we taught ourselves microfabrication. To help, we hired postdoctoral fellows who were electrical engineers and physicists. Graduate students—even those in chemistry—threw themselves into the project. The idea of making very small structures (at a time before "nanotechnology" had emerged as a distinct subject) was engaging; the small structures we made provided new tools for biology; it was fun!

We are now a different group, with completely different skills than we had before. I would like to say that the transformation was planned and rational, but it was not: it resulted from saying "yes" to invitations to join groups in areas in which we were unqualified to work, then learning by doing.

The Group

Throughout, the group has been its own best teacher. I am constantly amazed at what smart, young people can learn if they have an interesting, shared problem to work on. Sometimes not knowing what you are doing can be a great advantage. There is less temptation to develop ideas that others have already invented (since you may be too naive to know that they exist). There is more tendency to go off on your own. It is, however, very helpful to have a heterogeneous group—the more different kinds of people, the more likely someone will know a technique that already exists in field "A" that is the perfect solution to a problem in field "B."

Others

I have described the value of self-instruction as a way of starting in a new area—jump into the swimming pool, and hope there is water in it! On the human side of the ledger, a different set of people was very important. They were not necessarily technically trained, nor even members of the group. Interestingly, most were women. Let me mention three, and what they did.

The first and most important was certainly my wife. When I was in college, a roommate introduced me to his sisters (another fortunate accident). I ultimately married the oldest of them. There are not many people who would have had the patience to put up with me for a lifetime, and she (Barbara) has. She guessed I had a sense of humor, and that I could make a living, and that I would make a serviceable father. She also realized that she had major reconstruction to do if the quasi-graduate student she had married was to be reworked into something convincingly human. I hope my graduate students realize how diligently she has labored on their behalf! We have had a wonderful time raising two sons (Ben and George). Children teach — in fact, unambiguously demand—that you pay attention to them rather than to yourself. A very useful lesson to learn in running a research group.

Next came Giselle. In 1982, when I moved to Harvard, I hired a secretary: Giselle Weiss. In my naiveté, I thought that I had hired her to do dictation and slides.

She did these things, but she could not sit by and let me proceed with what must have seemed alarmingly incompetent management. Giselle was both luminously intelligent and very kind: she set to work, ever so delicately, to make the group into a coherent social entity, and to teach me to pay attention to the students. She also taught me a lesson that is crucial to success in academic research (and probably to all small organizations). Pay attention to your assistant! She (or he) may be smarter, or wiser, or both, than you.

Jim Tananbaum—a person from a different world altogether—taught a lesson from business. In the 80s, I had become involved in consulting with small, high technology businesses. At one point, I gave a talk on entrepreneurship at M.I.T. After the talk, a total stranger came up to me from the audience and said: "Hello, my name is Jim Tananbaum. I really liked your talk. Let's start a company." And we did—ultimately several. What I learned from these companies—which are not so different, in many ways, from academic research groups in scope and size—is that three elements were required for their success: people (especially management), money, and technology—and of these three, the technology was probably the least important. Good people, adequately supported, could always make something interesting happen; good technology, without good management, would always fail.

What Have We Learned about the Construction and operation of a Research Group?

The ideas that we now use in operating the group are, I ruefully admit, all obvious in retrospect. There are six. First, people who have chosen a career in research prefer to work on ideas of their own choosing, rather than to do what they are told. Most people are delighted to have colleagues, but would just as soon not have a boss. Second, it is more fun to work with others on a common, shared problem than to work alone. The work goes faster, and a group can take on problems that are more complicated than can any single person. Third, students are at least as creative as faculty, and are capable of much more autonomy in research than they are given credit for. With autonomy, they develop creativity. Fourth, groups of people—chemists and biologists and physicists, scientists and engineers, men and women, young and old—working together are more interesting, adaptable, and creative in solving complicated problems than individuals working alone. Fifth, the personal priorities and ambitions of one person cannot impede the progress of others. Sixth, whenever an expert in a field asks you to collaborate－and the field is one about which you know nothing, and in which you are entirely unqualified to work－say “yes” immediately.

I believe that the research recognized in this award could not have been done in the classical, disciplinary, academic research group. Our way is not the only way to do research, but it is one way, and it works very well for complicated problems. Its elements are: i) collaboration; ii) no distinction between fundamental and applied research; iii) emphasis on areas that are new; iv) an explicit understanding on the part of the students that they are expected to learn both how to do the research and how to collaborate.

Essentially all research in the group is now cooperative. “Cooperative” does not mean different people working on the same problem (although that sometimes happens, simply because it can be pleasant to work together); it means different people, with different interests and skills, working on different parts of problems too complicated or difficult for any one to do alone.

The Research Group as Family

Over 40 years, the research group has evolved into something very different from what it was. It started as a “hub and spoke” architecture; it is now closer to a family. In saying “family,” I do not mean to imply the family of sitcoms－all smiles and good humor. Ours I would describe as turbulent and heterogeneous, but adaptive. The organization of the group is flat: things go on that I know nothing about. Students develop collaborations, plan, solve (or don’t solve) problems, often in ways invisible to me. I bring groups together, and help them to talk to one another. I usually make final decisions about strategy, resolve certain kinds of problems, control the quality of the science that we publish. The students know the details of the research and often know how to think about the problem more clearly than I.

And, as in a family, they, the students, all leave.

But while they are in the group, they (or we) all teach one another. As with children learning a foreign language, organic chemists can learn about fluid physics effortlessly, so long as no one has told them that it is difficult. Physicists can learn to purify proteins. Materials scientists can learn to grow cells, and biologists can learn to think about nanofabrication. Collectively, they solve problems that would be insoluble individually. And everyone has fun.

And society ultimately benefits.