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RESEARCH INHIBITIONS

by Peter Franken

Doing research is always difficult, and some of the most well-thought-of practices, like neatness, organization, handsome equipment, can often be ways to avoid really doing it.

IN BRIEF: *An obvious, yet easily overlooked, aspect of scientific research is that it is performed by people, and is thus vulnerable to human problems. One set of difficulties that can afflict new and old hands alike are inhibitions about really doing research.*

Such inhibitions often express themselves in an undue preoccupation with equipment; a student spends eight weeks waiting for equipment and a few more deciding where to place it; an experimenter spends months optimizing equipment he may not use; another experimenter builds his electronic components with elaborate care, and then rebuilds them with just as much care when they don't work properly. The trouble is that none of these people is doing a bit of research. The cure? There is no one cure, but being aware of the problem is the essential first step.—S.B.

■ I would like to discuss the people who perform scientific research. In particular, I have in mind a peculiar difficulty that can afflict the established researcher as well as the untried graduate student. For want of a more accurate description, we might call it the inhibition about doing research.

I shall have to go about this in reference to my own experiences. I am a staff member of the University of Michigan. I do physics, and my primary activity lies in research. To this end I have a laboratory in the Randall Physics Building and, on the average, a half a dozen or so graduate students working on their doctoral theses. Some leave, new ones come in; there is a sort of current which they usually regard as somewhat too slow.

When a graduate student enters the laboratory he is usually quite raw. He may know a great deal and he may have many skills, but he is not a researcher. When he leaves after a while he is a researcher of some sort. Many are very good and well prepared to enter industrial or university laboratories and perform effective work. Some are very gifted and clever, some have only manual dexterity, others are just very creative without the accompaniment of disciplined skills to back them up. It is hard to generalize and even harder to answer the question, "What goes on here?" In comes something raw, and later there emerges something less raw; but there are difficulties that they all have, difficulties that many never conquer.

Let me tackle this by way of an example. A student enters the laboratory and for the first three or four months he helps out. This is a sort of slave labor that is not un-enjoyable; he becomes familiar with the ritual of the laboratory, learns where things are, acquires some understanding of the program. After a few months he either selects or is given a research problem: Go off and measure the microfine structure of doubly ionized lithium—something nice and reasonable that he can tackle for his doctoral thesis.

The first painful hurdle

He is usually very excited at this time and is very busy for a couple of months. But not much happens. He comes in every day, spends hours over catalogs, and orders things to be delivered in six or eight weeks. He makes excellent drawings for the machinists to aid them in fabricating his equipment, which again will be delivered in six or eight weeks. Yet somehow, nothing much happens.

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one) has to do, and this is the thing the students find so difficult, is to come into the laboratory occasionally and say to himself, "What can I do today? What can I steal from next door, borrow from upstairs, beg from somewhere, fabricate from string? What can I do today as a sort of dry run to learn something?" Not six weeks from now, but today.

This is a valuable approach, because these dry runs very often reveal the ugliness of some of the problems which were thought to be easy. Also, and this may be even more important, the researcher occasionally discovers that some of the things he was very worried about can be accomplished quite easily. For example, he may have been planning on the construction of an ultra-high vacuum system, because of a concern about surface poisoning, only to find by relatively crude experiments that something as unsophisticated as a water aspirator might be adequate.

Even though the simple-minded, direct, dry-run approach is quite obvious and utilitarian, it is something that is difficult for many beginners to accomplish. There seems to be a deep inhibition, almost a fear—of failure, perhaps. It somehow seems much easier to come into the laboratory and say, "What can I do now that is going to help me six or eight weeks from now?"

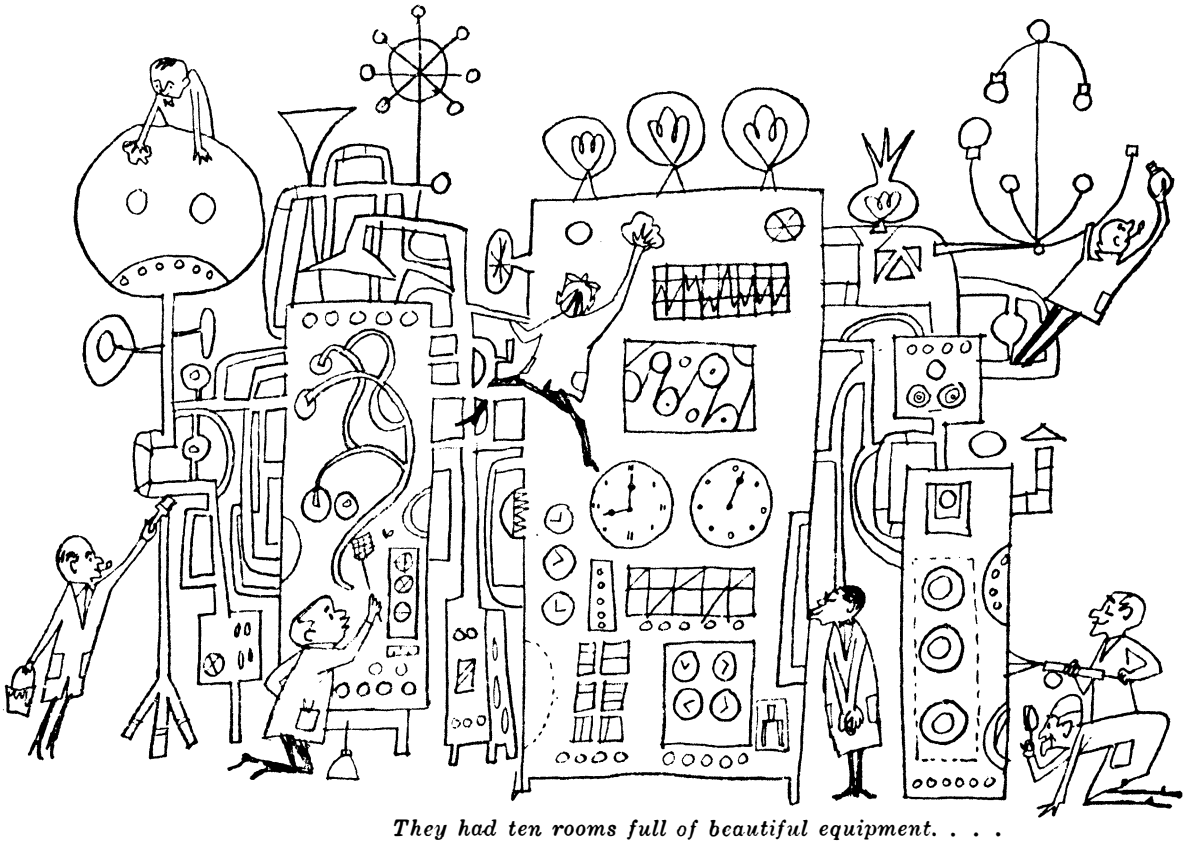
Another, quite probably related, problem the new graduate student often faces is a curious one, not unamusing, but it seems at times to just about break him up. After a few weeks or months a lot of apparatus has accumulated; it is over in the corner, in the hall, underfoot in general, and so he now has to do something. He has to make a decision. The first decision is an obvious one: where to start building the actual experiment. In the center of the room? In the corner by the sink? Or the other corner by the gas line? This seems to be an agonizing point of decision, a kind of writer's cramp. The answer, of course, is start it anywhere. But get started. This inhibition about beginning—and I have suffered the agonies of its many variations—can be a brute to conquer.

Tidy desk, tidy mind? Not necessarily

A different facet of this problem became apparent to me a few years ago when I did a modest amount of consulting for various industrial laboratories. This consulting usually took the following form: I would give a seminar on my work at the University, and then spend part of the day wandering about the laboratory, talking to the scientists (I think the technical term for this migration is rubbernecking). Around four o'clock in the afternoon I would find myself closeted informally with a few of the executives of the company. They would want to know, quite reasonably, what I thought of the laboratory. Was there good research going on? Or the possibility of good research? And if I did not think so, what did I have to recommend for their consideration? Experience rapidly taught me that this sort of pseudomanagerial conference always concluded my visits, and inevitably I began to develop some rules of thumb to guide me during the day.

The most useful index I found was to see how neat the laboratory was during my tour earlier in the day. I would look, for example, at some of the capital equipment items. Were they kept in reasonably good shape, or were they just a little bit corroded and scratched? Were the small items of equipment just stored on the workbenches, or were they filed away in nice steel cabinets? Were the hand tools scattered around the work-bench, or were they all put away where they could be easily found? The reason I noted these things was simply this: If a laboratory is neat and organized in the sense I am describing, the chances are almost overwhelming that there is not a damn thing worthwhile going on.

You see, to be organized is not just a sign



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matter of arranging debris into neat piles. It is something which takes thought and effort. It is a device that very often can prevent you from doing something constructive—from doing research. To illustrate this thought, consider a researcher who is building a piece of equipment as part of a research project. He needs the No. 4 nutdriver, so he reaches up to his tool board, acquires the tool, and performs his operation.

Now watch him closely; does he reach up and put it back? Many do, and I want to ask why. I think he is announcing, "I want to stop what I'm doing, break my pattern, put this back, because two weeks from now I might need the No. 4 nutdriver and I want to be sure it's where I can get it." However, the good researcher, very often, puts it down on the table and goes on. When he needs the pliers, he reaches for them, and so on. This is no tragedy, because at the end of the day it takes just a few minutes to gather them all up and put them back on the board. The consciously neat man, I suspect, is often devising excuses for not accomplishing effective research. He uses neatness as an acceptable reason for not devoting his full intensity to the project. Research is a hard job, and when you are doing it it is absurd to be burdened with the distraction of unconstructive neatness. I am not saying, of course, that you've got to be a slob to be a good researcher, but the excessively neat, well-organized fellow is revealing more of himself than a mere background of effective toilet training.

A fussy approach can easily cause other difficulties. For example, most of the work my people do involves electronics, not the sort of stuff that can be bought, but the brutes that have to be built. It is very interesting to watch a man build a circuit, particularly if he is not a good or experienced researcher.

The perils of care

He will often approach it the following way: He goes to the stock room and acquires a nice new steel panel with its black paint displaying an attractive virgin crackle. Not a bad way to begin. He goes to the shop, and lays out exactly the positions of the holes he must drill in order to mount his components. Not bad; you have to take care with many electronic circuits. Now he begins to drill the holes. But he doesn't just drill them; this lad is careful. He makes sure that there are no chips of steel lying on the bed of the drill press because, as the drilling goes on, they would chatter and mar the finish. The result of this care is that he ends up with a beautiful circuit. Just gorgeous. The meters are right on center, the switches are lined up accurately because he held them with care before he tightened them, it's a beautiful piece of equipment and it gives anyone joy just to look at it.

Actually, it probably did not take him

noticeably longer to do it carefully than it would have taken him had he approached it in a more casual manner. Nevertheless, he has hamstrung himself in a quite curious way. After a few months of building he has acquired a roomful of these circuits, magnificent to regard, and now he must settle down and do his experiment. His Day of Reckoning has come, and inevitably, as with all new equipment, his doesn't work. The panel with the amplifier on it, for example, just doesn't amplify the way it should.

What he should do, of course, is pick up the electric drill, drill a few holes in the front of the panel, and mount that extra transformer which the circuit requires. Just go over and drill the holes, and if there is a piece of metal in the way, hack it off. But if he's built up a neat beautiful laboratory, he can no more go over there with an electric drill than he could take a pistol to his grandmother. He just can't do it.

It's an amazing thing, but if you have a beautiful piece of equipment, which you've taken pride in constructing, it is terribly hard to go and hack it up to modify it as a research instrument. In fact, there is one thing that I intend to do more of, and urge my students to do also: when building a circuit, hack it up a little bit ahead of time. This might seem foolish, but I don't mean wanton hacking; I mean this: You drill extra holes so you can stick tubes in quickly. Very often you just use chalk for labeling. You end up with a device that works just as well as the other one, but you have no commitment to it. You can modify it.

The man who has built neat, beautiful equipment is often stuck. When he discovers he needs that new transformer put on the amplifier, he turns his apparatus off. He takes the amplifier panel out, he walks up to the shop, he mounts it very carefully so he doesn't scratch the finish and drills the hole, and an hour later he's got it placed. But he has completely interrupted the train of his work. Besides, it's now 5 o'clock and he knocks off.

PhD's are not immune

I once saw a superb illustration of another facet of this problem in a large plant of a large international corporation. Two years before, it had been a hillside; at the time I am thinking of there were 3500 people. Most of these were development people intimately connected with production, but they also had a research laboratory—ten or twelve scientists on the PhD level. All of this had been created overnight.

The task of the research laboratory was to investigate any properties they wanted to of thin ferromagnetic films. The scientists were given all the funds they needed and quite a lot of space—for this company realizes the advantages of completely unencumbered research. Everybody was very happy, but when I visited there about a year later *nothing*, absolutely *nothing* had been accomplished. The trouble was quite striking, and it was clear, in this case, what it was connected with.

These men came, and they had known each other before, I suspect; they had all the money they needed, so they started buying equipment. They bought a mass spectrometer because it's obvious that sometime in the future they would have to make an analysis of the chemical composition of the iron films. And they bought an infrared spectrometer for much the same reason—they might need it, of course—and a far infrared spectrometer, and a visible spectrometer. These cost $50,000 for each one. They also bought an ultraviolet spectrometer, an electron microscope, ultracentrifuges, and an x-ray defraction camera, because everyone needs . . . and so on.

They spent $800,000. They had ten rooms full of beautiful equipment. They weren't doing a bit of research. It was full-time work for ten PhD's just to optimize this equipment. One man, who is actually quite good, spent some time showing me how he had modified a $120,000 mass spectrometer so that it could give an analysis of the ferromagnetic material in 30 minutes instead of an hour. But he wasn't doing a single experiment. He had been sidetracked by this beautiful equipment.

It's a curious effect. I don't know why he does it, or why the others do it, but it's certainly again another facet of this inhibition about research. I don't want you to misunderstand me on this point. I'm not saying to those of you who are research directors that when a man comes up and asks you for equipment you pat him on the head and say, "It's in your best interests that I don't give it to you." I'm suggesting this, though: When one of your people comes and asks you for a piece of equipment, whether its large or small, I think that if you are alert you can judge fairly quickly whether he wants it because he's in the middle of a project. If he wants a mass spectrometer because he *is* running an experiment on iron films and he needs this information, then you get it for him as fast as you can. But if he's suffering the syndrome under discussion and he's buying it because he might need it, you might do very well to guide him in a somewhat different direction.

It would be very useful if I could gather all this into a neat package complete with directions for cure and rehabilitation. It would serve as a guide for research directors and researchers alike—a companion piece to all the wonderful books that tell us how to be personable and win vice presidencies. If I could do that I would be a rich man indeed, with staggering consulting fees to augment the meager millions I would enjoy in royalties. More than that, I would have learned how to get the monkey off my own back.