EDITORIAL

If at Third You Don't Succeed

An important and much discussed question in doing science is deciding what to work on. Less discussed is the problem of deciding when to stop working on a project. There are several reasons for this. First is the value we place on persistence. Our lore is full of stories of long, hard, often discouraging work that finally pays off. Such stories, however, are not the norm. Many of us recall more instances of regret in sticking with a problem too long than in dropping it too soon. Steadfast, persistent, persevering, stubborn, bull-headed, block-headed—where do you find yourself on the continuum and when do you beg off? Seems to me some practice early on with these questions is valuable. (Six months of failure for a grad student builds character.) Proposal reviewers only make things harder. They ask what you'll do if an important part doesn't work and want to hear that you'll push or cut through the entangling underbrush with other tools or that you'll hack through the jungle from another direction. "I'll take my machete and go home," rarely gets a good score.

What else keeps us working on a problem too long? In addition to gritty determination, there's the feeling that the breakthrough is just around the corner. "We're almost there: just this one hurdle to get over and then it's smooth sailing." Add to that the reluctance to write off all the work, time, and money already invested and the data obtained. "A little bit more and we could still get a paper out of it." I don't know which is worse: small positive results that are unconvincing, inconsistent results, or methodological problems that interfere with doing the basic experiment properly. Another tether is the sense of loyalty to a system we've established, a problem we've defined or contributed to, a gene we've discovered, or a good transgenic or reagent that we've made. "Can't drop it now. Not only is the original problem or goal important," we tell ourselves, "but figuring out what's gone wrong will contribute to future work, clarify the conditions needed, make the effect more reliable, reveal something about the underlying regulatory mechanisms," etc.

So how can you tell when it's time to quit? If you find yourself doing an experiment that you can only explain by the specific history of the project, that has little or no interest in itself, no obvious connection to the original goal, and it is not your first or even second such experiment, look to the exit. Include an exit strategy, or at least a well thought out break point, in your plans from the outset. "If I see this, I'll continue; if not, I'll quit." Such resolve is hard to maintain when the time comes, especially if you've worked hard and long to reach the point in question. As is the case for other problems where you may not be the best judge, it is helpful to ask other people. If three people you admire and trust suggest you quit, then quit. Deadlines help, even when they're self-imposed, but beware the tendency to seek extensions. Also, deadlines make the "narrative fallacy" kick in. As the heroes of our own stories, the more hurdles and setbacks we've met, the more convinced we become that we'll pull it off at the last minute—grab the rope before it slips out of reach, cut the red wire (or is it the blue?) with one second left, get the grille off the duct in the nick of time before the guards burst in.

A useful approach is to stop banging your head against the wall without admitting you've decided to quit. "It's just a pause." Turn to another project, one that's going better or a new one. You should be considering whether to do this anyway, at regular intervals. Have, say, five projects in your head competing with each other, periodically being re-evaluated, their order of priority changing and new ones supplanting old ones or old ones returning to your list. Work on the top two or three. The most useful approach to your research, of course, is to adapt Will Rogers' advice on the stock market—"Take all your savings and buy some good stock and hold it till it goes up, then sell it. If it don't go up, don't buy it." Just work on a good project till it succeeds, finish it, and move on. If it don't succeed, don't work on it.

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