Mentoring Thoughts for Young Faculty

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In the increasingly competitive world of science, mentoring effectively has several advantages including

1) It is the right thing to do.
2) It will almost certainly increase your lab's productivity.
3) It creates loyalty among your trainees.
4) It increases the chance of your trainees' subsequent success.
5) It helps you recruit new students and post-docs.
6) It makes doing science more fun.

General Thoughts:

What follows are my thoughts on mentoring well. It is very clear that there are multiple paths to success in this context, and also that mentoring style and trainee personality interact, so what is best for one student will not be optimal for another. That said, the principles below have generally worked well for me.

Mentoring well requires teaching. Teaching requires being able to analyze the way you (or others) do things sufficiently deeply to gain verbal access to your expertise, and to then communicate that expertise. This is hard, and is not something we are trained to do.

Mentoring well also requires conscious effort and thought. It also benefits from an empirical approach: you need to try things out, see if they work, and then be willing to change your behavior if things are not going well.

Identifying (and admitting to yourself) when things are not going well is hard, but the sooner you get over than and start fixing things, the more likely you are to be successful. Similarly, if someone in your lab is having trouble, it can be tempting to avoid interacting with them. This usually does not end well.

Most junior faculty at a place like UCSF come from post-docs on established, successful labs. In general, the mentoring styles of the PIs of those labs are not optimal for a faculty members starting their own laboratory:

- Larger, more establishes labs often have senior students or post-docs who can provide day to day guidance on both technical and conceptual issues. New labs typically do not.
- Established labs are more likely to attract high quality post-docs.
- Some established labs also can get away with a higher attrition level, where a subset of lab projects do not work out and students or post-docs spend many years working without a publishable result. This is dangerous for a new lab.

Potential problem areas

1) Level of perfectionism.
    - Trainees who are more perfectionistic than their PIs can come across as obsessive and can be less productive than you would like.

- Trainees who are less perfectionistic can seem sloppy.
- Some flexibility on the required level will probably be necessary if there is a difference in opinion, although of course this cannot compromise the quality of the science.

2) Work ethic
    - If your trainees aren't working as hard as you would like, the first question to ask is why. Is the person depressed? Is there some other issue getting in the way? Does the person have unrealistic expectations about the amount of work required? A conversation about this is often very helpful.
    - Depression is surprisingly common among students, and leads to a vicious cycle of them not meeting your or their own expectations and thus becoming more depressed. If you are not comfortable talking with them about these issues, send them to someone who is (a colleague or student health services).
    - If the student is not excited about doing science and it is not a temporary problem due to lack of success, it is may be that best thing for everyone is for them to leave the program. I have personally encouraged two students to leave (one in my lab, one in another lab) and in both cases, they thanked me afterwards.
    - Telling your students to work harder can backfire. A better approach is to have them set goals for their project and then check in with them on progress.

3) Feedback / independence
    - While it may seem trite, the rule of positive-negative-positive can be extremely useful in delivering feedback. Negative feedback alone induces defensiveness and resistance, which makes it harder for people to change their behavior. This needs to be done thoughtfully, however: "you're trying hard but you are terrible but at least you're trying hard" is not going to work. My main goal here is to express appreciation for the things that are going right and then to focus attention on things that need to change.
    - It can be helpful to ask your trainees if they are getting enough feedback and if that feedback is useful. You can also use the input from that conversation to adjust styles on an individual by individual basis.
    - It is helpful to identify the level of independence you expect and to make that explicit when you talk with trainees about your lab.
    - Most students (and many post-docs) cannot design a project in a new field without substantial input.
    - It is generally helpful for the student to feel like they own their project and that they are empowered to make at least some of the decisions, although consultation with you is always going to be a good idea.
    - If a student comes to you with a problem, start the conversation with the assumption that they have a good reason to come to you. If the problem involves you, try to avoid the natural tendency to be defensive (see the first bullet point in this section).

Thoughts from Michael Stryker for UCSF assistant professors. Faculty mentoring lunch 08/19/2016, slightly revised 05/15/2017

1. **Remember that the most important thing to your future is the papers you publish**.

2. **Evaluate all travel in the following terms—is this going to be more valuable than spending the same amount of time on a new experiment or new paper?** A lot of young people, particularly young women, get invited to go all over to fun places. It seems like an honor, and it is seductive, and people away always seem to appreciate you more than do your local colleagues. But a 3-day trip takes a lot out of you, and if it delays a paper by a week it is almost never worth it. No one will remember which trips you took in 5 years time. A few (2 or 3 per year) invited seminars to important meetings or good places is all you need on your CV for promotion.

Some travel can be really useful, however, where you learn new things that are useful to your research long before they are published, so the decision of whether to accept an invitation is often not clear. But the decision in your early years should always be on the most selfish basis, of the benefit to your research.

You can be envious of all the fun trips that senior people take, but they are at different points in their careers and can afford to slough off sometimes.

2. While the least publishable unit is a poor strategy**, publish frequently**, whenever you make a real discovery. Don't wait for the *Nature*, *Science* or *Cell* paper. Give those places a shot, if you like, but it is much more important for you (and better for your students) to publish good, sound work. Too many assistant professors are self-destructive in waiting to publish a magnum opus, and the longer you delay, the better it has to be to justify the delay. It is a vicious circle.

3**. Put real effort into classroom teaching**, since it is evaluated seriously at the time of promotion, but do only a modest amount of it very well, and do not take on a large amount. Teaching can be immediately and intensely rewarding, when the class claps at the end or even stands up and claps, or even when you just see those bright young eyes light up. The rewards of your scientific work come slower and harder, so don't get addicted to the candy of teaching and devote yourself to it at the expense of research.

4. **Be even more restrained at taking on service activities**. Ad-hoc'ing at a study section **once only** before tenure may be valuable in cluing you in to the bizarre criteria that can be used to evaluate your grant applications, but do not do more. Departmental and university service, and national service organizations (like reviewing for HHMI), should be done only in moderation—at most one thing each, and only if it does not take much time. For much of the past 30 years there has seemed to be a conspiracy to destroy the careers of young women scientists by appointing them to prestigious service activities (eg, Sloan, Searle, HHMI fellowship review committees or high-level university committees) much earlier in their careers than the young men would be appointed. These activities are worthwhile, and someone should do them, but let it be someone **else**. Your career depends on the research you do, not on whether the community though that you were a bright young man or woman who could be so useful to one of our communities.

5. **Same for reviewing manuscripts**. Do a small amount well, and have no fear about refusing.

6. **Be honest with your students**. Do not keep them in your lab if you think that they can't hack it or are more needy (personally or scientifically) than you have time to satisfy. My own biggest regrets about students are people whom I took out of pity, because their personal circumstances. They took 4 times as

much of my energy as the other students, and a huge amount of my lab's money, and at the end there was little to show for it for either them or me.

Also, trust your instincts on dealing with laboratory personalities. A difficult personality once almost destroyed the happy and productive laboratory environment. Fortunately, others in the lab at the time were strong enough to take care of themselves by means other than murder, and person finished up and left. If I had followed my instincts, I would not have taken this person on.

I have absolutely no regrets about the two students whom I kicked out of my lab. Though there were many tears at the time, both were grateful to me some years later.

7. **Be even more honest in evaluating your employees**. A great technician is a huge asset to a lab (and I was blessed with one for many years), but **do not let a technician or other employee someone get past a probation period** without reconciling yourself to the likelihood that you will have them for life and never be able to get someone better to replace them. Bad employees have difficulty getting another job, and so they stay. It is very hard to dismiss them, and your department may not be willing to put in the effort.

8. **Don't let paper writing become a trap**. Scientific manuscripts need not be literary creations. Their only necessary quality is clarity—concision and wit are purely optional. If it is taking you and your student more than a week to write a paper after the figures are made, then reserve a couple of days to sit down with them in your office and write the paper together, line by line. It is a discipline that gets the manuscript done in less than a week, and in my experience the product is usually very good. Sitting down like this is also a great time review the primary data and to at least sanity-check all the analyses, with the person other than you who was involved in the experiments. Passing papers back and forth between you and the student or postdoc can drag on forever with little or no benefit. At this stage of your careers, getting papers published is **the most important use** of your time.

9. **Grant strategy**: Doing something important rather than whoring after what you think is the most fundable is what you should aim for. However, you may need to bootleg the thing that you think is the most important thing you want to do and get funding for something else that will keep your lab going along a related but better appreciated line. You can try a couple of times, but if something is poorly received, even if you know it's great, give up and propose something else. You can still do the poorly received thing, so long as you have some product at the end.

When a grant does not score high enough to be funded, talk to your **Program Officer** as soon as possible after the review. He or she can be your most helpful friend because they can tell you what the study section seemed **really** to care about. The CSR person is generally useless for this purpose. When the reviews come back, it is generally best to remove portions of the application that the reviewers do not like and to say that you will do everything that they suggest on the parts that they criticize but thought had merit. Often the views of the reviewers are really stupid, and you know better, but you have nothing to gain by arguing with them. You need the money from a successful grant application, and changing your proposal to make it most likely to be funded is the way to get it. You can still do the things you want to do—these are **grants**, not contracts with deliverables. At renewal time, no one will ever care about (or probably even read) what you proposed to do in detail; they will only take note of the published papers that convey your accomplishments.

10. **Be in your lab and stay out of your office as much as possible**. You were hired because you were good scientists. You may not be a good manager. So do science with your own hands, and you will attract and inspire the best students. They will make your lab great.