Chapter 5: Research

Now let us talk about the central aspect of being a professor at an R1 research university: doing research. This is in some sense the easiest part of the job, because this is what you get directly trained for as a graduate student; in another sense, this is the hardest part of the job, filled with uncertainty, ambiguity, and rejection. Your success as a professor at an R1 university will depend in large part on whether you can come up with and execute a strong research vision.

We have already talked about getting funding and finding students, so I will focus on doing research once you have funding and students.

5.1 Evaluating research from the point of view of tenure

Let us first talk about what the research component in a successful tenure case will look like. It is always good to know what the end goal is before we start! Success is not defined purely by whether you get tenure, but since all tenure-track professors wish to get tenure, it is useful to know the requirements.

Abstractly, the tenure committee is trying to determine if you did good research. Concretely, the tenure committee is concerned with two things: the publication record, and letters. The committee looks at whether you have been consistently publishing at the top venues for your area of research. The committee will request letters from the experts in your area of research; these letters should make the case for granting or not granting you tenure.

What makes a publication record good? One thing the committee will look for is consistency: if you publish a lot in year one, but nothing for several years after that, that will result in some questions. Another thing that will be examined is productivity, relative to your field. For example, systems projects take about a year to complete, so the committee might look for one or two strong publications each year for systems professors. The rate of publication is much higher in areas like machine learning, so the bar will be different for that area. You should talk to senior folks in your department to get a sense of what they are looking for.

What do good letters look like? At a high level, your letters make the case for granting you tenure. They talk about your **body of work**, some of your main results, and your impact on the field. One important thing to note here is that the letters never talk about more than three papers or projects. Regardless of how many papers you publish, your letter writers will focus on your most famous/most impactful work. Thus, it is important that there are a few papers or a coherent line of work that you are well-known for; simply publishing a lot of papers is not enough for tenure. When folks think of you, you want them to remember a line of work. "*John Doe is well known for introducing X as a way to solve Y*".

Note that this places constraints on the kind of research you can do. If your line of work is obscure and not well known to the community, you will not get good letters. It also has implications for outreach; it doesn't matter how good your work is, if nobody hears about it (and writes about it in your letters). Thus, you must do both high-quality work and ensure that the community knows about it.

Another important thing that the committee is trying to ascertain is your **research independence**. Prior to becoming an assistant professor, most (if not all) of your papers would be co-authored with your advisor. It is important that you show that you can lead research projects on your own after becoming an assistant professor; if all your papers after becoming an assistant professor are still with your co-author, it raises red flags. Similarly, it is good for your research portfolio to contain at least a few projects that are just you and your students.

5.2 Coming up with a research agenda

What is a research agenda? How is it different from a research project? A research agenda sets a broad direction for multiple research projects. You can think of it as a general direction towards a broad goal. For example, the broad theme/agenda of my NSF CAREER was "building IO-efficient infrastructure". Your research projects are then concrete tasks that advance this broad goal; for example, one of the projects under my CAREER agenda was building a write-optimized key-value store.

Coming up with a compelling research agenda is one of the hardest things you will do as an assistant professor. By the end of your PhD, you will have some inklings of what you would like to work on in the future. However, translating that into a concrete research agenda that can inspire students and funding agencies is quite challenging. In some sense, solving this challenge is how you write your first grant proposal (such as the NSF CAREER). All grant proposals require a compelling, multi-year research agenda.

My advice for coming up with a research agenda would be: pick the intersection of **inspiring**, **technically challenging**, and **doable**. I'll talk about inspiring first, because I think this is where I and many other new profs fail. **Simply put, your research has to inspire someone who is not an expert in your sub-area**. Why is this the case? Because your research agenda is typically important for things like grant proposals or awards, where you are evaluated by peers broadly in your field, but not necessarily in your sub-area. This is in contrast to paper submissions, which are evaluated by area experts. For example, in my case, my research should inspire folks in the broad systems community, not just storage. One of the mistakes I did early on was send in a narrow proposal that was technically challenging and doable, but not inspiring; it got rejected. You should both be able to explain what your intended research is to a lay person, and get them to see why it is important.

The second important criteria is picking something that is technically challenging. Without a hard technical problem to solve, you will have trouble in both getting funding agencies to fund your research, and in getting your papers published in your community. So you need to think

about your broad goal and identify the technically challenging aspects. These aspects need to be something that cannot be solved with the current state-of-the-art.

Finally, you need to pick a research agenda where you bring something to the table. Funding agencies like to fund work where the expertise of the PI is essential for the work. This is also the niche where you are likely to make fast progress, given your skills. So I would definitely recommend having your first proposal be something tied to your proven track record, not something tied to what you want to do.

This can be a little tricky if you are trying to switch sub-areas. Perhaps you worked on topic X during your PhD, and you are sick of it, and want to move to topic Y. You now have a bit of a bootstrapping problem, in that funding agencies won't fund you for Y unless you have demonstrated expertise. Thankfully, many R1 schools provide startup packages that help bootstrap research; you can use this to publish some research on topic Y, build up your credibility, and then apply for a grant proposal on Y.

5.3 Picking research problems

 I think problem selection is one of the most important skills as a researcher, especially because we have so much freedom in deciding what to work on. The best researchers hone in on important problems and focus their limited time and attention on these problems.

An easy way to build a poor research portfolio is to simply pursue every idea that you come across. You might imagine that great researchers simply just have one great idea after another. However, from working with great collaborators, I've learned this isn't the case. These researchers get OKish or even bad ideas just like anybody else; the key difference is that they reject potentially publishable but OKish ideas until they get to the really good ideas. So you should learn to say no. You have limited time and energy, as do your students; typically, each student is only going to work on 3 to 4 papers before they graduate.You should make sure those papers are good ideas, worthy of the many months or years of time that are collectively spent on each idea.

So how do you pick research problems to work on? One of my professors at Wisconsin used to say, "It should be beautiful, or it should be useful". I think that captures it concisely: the criteria you are looking for are elegance (or how clever or neat an idea) is, or utility (it should be of use practically). Arun Kumar at UCSD says something similar in his [excellent blog post](#) on the topic. NSF also has two key goals for each proposal: intellectual merit (how does it advance the state-of-the-art) and broader impacts (how does it positively affect society).

Research ideas you consider will have some mix of these two qualities. At one extreme, an idea might be impractical, but open up new lines of thought. At the other extreme, an idea might be useful, improving the state of the practice significantly. Both these extremes have problems: working on ideas without considering utility may lead to ivory-tower impractical theory only

useful for other academics;working on practical ideas without regard for advancing the state of the art transforms your lab into a poorly funded industrial research and development center.

Since I work on systems research, I tend to skew towards the utility side; I like building prototypes that can be used by real people. But I avoid ideas where there isn't an interesting idea behind the prototype, and it is clear from the start what needs to be done to solve the problem. I think the sweet spot is in the middle, where there are both intellectual challenges and practical utility to users.

5.4 Knowing when to give up

An important part of managing your research is knowing when to give up or abandon a project. This might happen due to various reasons: the project might be too hard, it may not be a good match for your skills or your student's skills, or it may take more time than you want to spend on it (the student might be graduating). In any case, it is important to identify and handle such situations correctly.

Projects have a life of their own -- once you have spent more than 6 months on a project, everyone involved will feel reluctant to pull the plug. "We have already spent so much time on this, might as well see it through to the end" -- everything ends up thinking along these lines, but it may not necessarily be the right thing to do.

A good way to prevent this is to be extra careful when starting a project. Always try to ascertain whether your team has the skills required for the project. For example, starting a theory-heavy project with a student who has amazing systems-building skills is not a good fit. Before starting the project, discuss the "failure scenario", and come to an agreement: "If we haven't made good progress towards X in three months, we will pivot/try something else". Now, "making good progress" is still very fuzzy, but it can be hard to come up with clear-cut objective criteria. At the end of the period, you and your team should make the call on whether to move forward.

My recommendation is to cull early, and review often. If a project doesn't look exciting or there is little progress (or even hope of progress), switch to something else. Research is interesting in that (to borrow a sports metaphor from Warren Buffet (!)), you don't have to swing at every ball. Wait for the fat pitch, where you have high confidence going in that you can make progress.

Managing collaborations

Another important aspect of managing your research is managing your collaborations. A good way to spread yourself too thin is to say yes to too many collaborations. The social aspect of collaborations makes it hard to say yes, especially if you respect and admire the folks asking yes.

But you should remember your time and energy are limited. Ask yourself whether the collaboration is right for you: does it extend or apply some of your work, in a domain you care

about? Is there a chance for interesting ideas (in your sub-area) to come out of the project? Will it help in practical adoption of your ideas? Does it help drive the broad goals of your research agenda?

I think a couple of fun collaborations that are not related to your research are okay. But if you spread yourself too thin, you risk losing momentum and energy for your main line of research, which is what you hope to be known for anyway. So I would recommend being careful with starting new collaborations.

Industrial collaborations

Industrial collaborations are different from academic collaborations in many ways. Let us first talk about a collaboration where the company is sponsoring research: they are paying you and/or your student to conduct research. Doing something like this will require a **Sponsored Research Agreement**. Typically, the Office of Sponsored Research will be involved (and they might involve some lawyers). The Agreement will spell out how the Intellectual Property (IP) generated during the research will be shared, and how patents arising out of the research will be handled. Sometimes, there are templates for these things, everyone agrees and it goes smoothly. At other times, it can involve a lot of back and forth before the agreement is in place.

A second type of industrial collaboration is where you are just collaborating with researchers at a company. The company is not paying you or your students in this case. By default, the intellectual property you and your students generate will belong to the university (though some universities have exceptions where the IP belongs to the professor). In this setup, it becomes tricky if any shared patents are to be filed (this is where having an agreement would help). However, in the absence of any patent filing, it is similar to collaborating with other academic researchers.

Regardless of the type of industrial collaboration, some companies have extra steps to be followed before their researchers can publish. Sometime before submission, industrial researchers need to submit the draft to an internal committee who will read and ensure that no company secrets are being revealed. In some cases, the committee will provide technical feedback that can help strengthen the paper. In the vast majority of cases, these checks are routine and the committee will pass the paper; occasionally though, the company will [prevent its employees from co-authoring the paper unless changes are made](#).