Thoughts on Mentoring the Research Process

Performing high-quality, impactful technical research is hard work that requires a lot of technical skill and a fair degree of luck. Because the path forward is typically not clear cut, advanced research requires capabilities, such as decision making with high uncertainty, knowing when to trust your technical instincts, and how to avoid blind alleys, that look promising but likely will not work out. A typical undergraduate education provides a solid technical foundation for work for which a correct set of answers and ground truths are known to exist (and already known). However, that foundation does not necessarily prepare students well for the more open-ended aspects of fundamental research (in which it is often not known if a solution exists, let alone what the solution is). To learn the skills for solving open-ended problems, students need to spend several years practicing while being advised by someone who knows how to solve such problems (though not necessarily their problem in particular). Thus, it is important for mentors of students and postdoctoral associates to recognize that developing good research skills is typically something that has to be both taught and learned.

There are a variety of ways to approach this education about the research process, including techniques along the lines of the goals, objectives, technical challenges, and approaches (GOTChA) [1]. I describe it slightly differently in my discussions with students, connecting the process to the Heilmeier catechism [2], which they should be able to answer at the end. The approach entails having the students answer the following important questions:

1) What is the problem, and is it important?
2) What are the technology challenges, and, in particular, what are the overall research areas that must be addressed to solve the technical aspects of the problem?
3) What are the capabilities of the current technology in the literature?
4) What are the technical gaps that remain, and are they big enough to justify future research?
5) What are the proposed solution approaches, and are they tractable?

The apparent simplicity of this list belies the true complexities of this problem. First, the process itself is highly nonlinear and iterative. For example, steps 2–4 are often tightly intertwined. One really has to have a good sense of the current technology (step 3) to identify the technical gaps (step 4) before it is possible to determine whether the gap is large enough to justify further research into developing solutions. If, in step 4, the gaps are perceived to be too small, then there is a need to iterate back to step 1 and expand the problem statement. Conversely, if tractable solutions do not appear to exist in step 5, then there might be a need to iterate back to step 3 to identify new solution techniques or back to step 1 to descope the problem. The following sections provide more details on several of the key steps in this process.

STEP 3

Step 3 represents an important challenge in this process. Obviously, students need to analyze the literature to understand the current technology. With tools such as Google Scholar, this is far easier now than it was when I

(From left) Jonathan How enjoying lunch with part of his research group in December 2018. (Continuing from left) Brett Lopez, Michael Everett, Yulun Tian, Parker Lusk, Lena Downes, Macheng Shen, Jesus Tordesillas, Björn Lütjens, Dr. Kasra Khosoussi, Dong Ki Kim, and Dr. Kaveh Fathian.

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was a student. Of course, even this process can be complicated because different areas often use different terminology, and it may not be immediately obvious which keywords to look for (although discovering alternative terminologies and expanding the search accordingly is an important part of this exercise and can be very rewarding if it unlocks a completely new set of ideas/tools). However, this article search/reading must be done in moderation, otherwise (something I distinctly recall doing as a student) you can spend many days just chasing from one article to another by following the citations and references in each new article found.

One approach is to try to identify a core set of articles that are repeatedly cited in the recent literature and, thus, are likely deemed important to the field. That gives a basis to build from in a new research area. Another component I often remind students of is that if the problem was posed well in step 1, then it is unlikely that a solution exists in the literature. So this process is not about finding solutions to their problems. Instead, it is more about obtaining tools for their toolbox and an intuition about how they might be used to develop solutions. Developing that level of understanding about the tools typically requires hands-on experimentation, such as working through proofs to see how identities are used and identifying the role of the assumptions made or trying algorithms in simple real-time experiments. This process often identifies failure modes or overlooked assumptions in the theory that can directly lead to new research ideas.

STEP 4

Having established the baseline of the state-of-the-art capabilities in step 3 and the requirements in step 2, the researcher is now in a position to determine what gaps exist and whether they are large enough to justify further research (step 4). These goals explain why a lot of effort must be invested in step 3 so that the researcher is sufficiently knowledgeable of the current technology that he or she can establish the limitations of those approaches. This provides the motivation for investing some amount of time to master these other approaches, but it also highlights the possible danger of investing time mastering techniques that just don't pan out. These dead ends are also known as *blind alleys*, but that perhaps is too negative a connotation because it is likely that much was learned along the way, and that is, after all, one of the primary goals of the research process. Also, what seems like a dead end now might open up later when you learn new tools or read about new results.

I typically recommend that students summarize the results of the step 4 analysis (as in Table 1) to illustrate which of the technology challenges are not addressed well by the important recent articles in the field. With the proposed research in the last row, the table provides a summary assessment of how new work would then address these technical gaps. Regardless of how the assessment is presented, the goal of step 4 is to be sufficiently knowledgeable of the state of the art of the required technical advances and clearly identify their importance.

TABLE 1 **A summary of research challenges and technical gaps, where ✓ means addressed, X means not addressed, and ? indicates that it is unclear if (or how well) the challenge is addressed.**

Research	Technology Challenge 1	Technology Challenge 2	. . .	Technology Challenge *n*
[1]	✓	X		X
[2]–[4]	✓	?		X
⋮				
Proposed research	✓	✓		✓

STEP 5

After important gaps in the literature have been identified, developing the solution is typically the most exciting part. However, it is important for new researchers to recognize that not all problems are easily solved and that this step can take a long time. Encouragement along the way and suggestions on how to iterate back through the previous steps are crucial elements of good mentorship at this stage. In particular, iterating back to step 3 to investigate new techniques or analyze the assumptions of those techniques and how that affects their applicability for solving the new problems at hand can be very constructive. Similarly, developing a deeper intuition in step 3 of when techniques do (and do not) work well can be helpful in enabling researchers to learn how to avoid likely blind alleys in future efforts.

STEPS 1 AND 2

As students master steps 3–5 and mature into more independent researchers, the challenge of steps 1 and 2 (What is the specific problem that I should be working on?) remains. I distinctly recall worrying about "what I was going to do next" during the entire drive across the United States as I moved to California to start my first faculty position at Stanford. Turns out, the answer was easier than I realized (start by working on interesting things that you enjoy doing). That was before various studies of the future directions in control were published, which provide an excellent source of both research problems and technology challenges [3]–[6].

DISCUSSION

In my experience, new researchers often want to discuss their solution (that is, jump straight to step 5), but it is important that the entire cycle be followed (possibly several times if the problem has to be rescoped). Not putting enough emphasis on any of the steps can be catastrophic. For example, the objective of step 1 is to avoid the possibility that research will succeed but will unfortunately be of little interest to the community or the public and, thus, will receive very little attention. The emphasis of steps 2 and 4 is to establish that the remaining technological challenges are significant. If not, the problem might be solvable (which is good), but the work will be difficult to publish because the technical contributions are weak or not immediately clear. Of course, insufficient emphasis on step 3 can negate months of hard work when you belatedly find an article that contains your solution.

The first pass through the process is typically difficult because it is usually the first time that students are working on a topic with such a high degree of uncertainty in the outcome. One benefit of having students perform a master's thesis before a Ph.D. dissertation is the reduced pressure to produce a result—the research process can be a "success" even if the thesis ends in a negative result (which, overall, can be a great lesson learned). A particularly nice aspect of the two-year master's degree at MIT is that students have the chance to go through this entire research cycle several times before they even start their Ph.D. work. Of course, this also gives students the opportunity to decide if thy enjoy the research process before making a longer-term commitment.

Mentors and advisors need to provide a lot of guidance and support to new students embarking on a research career. Although much of this support will be technical, I hope that providing guidance, such as this five-step process, affords some structure to the endeavor and, at the same time, highlights possible pitfalls and correctly sets expectations on the time scales involved. As always, I look forward to your feedback and any additional thoughts you have on the guidance that you provide to your mentees.

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Arnold Beckman's Seven Rules for Success

1) Maintain absolute integrity at all times.
2) Always do your best; never do anything half-heartedly. (Either get into it, or get out of it.)
3) Never do anything to harm others.
4) Never do anything for which you'll be ashamed later.
5) Always strive for excellence—there's no substitute for it.
6) Practice moderation in all things—including moderation. (There's nothing wrong with a little excess once in a while.)
7) Don't take yourself too seriously.

—Dr. Arnold O. Beckman, scientist, inventor, founder of Beckman Instruments, and philanthropist; posted on the wall at the Arnold and Mabel Beckman Center of the National Academies of Sciences and Engineering, Irvine, California.