Getting Started in Research

by

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Research is the search for *new* knowledge, and it is thus distinct from the routine application of known results. The “re” in “research” is a misnomer. When you perform research, your goal is to add to human knowledge by discovering, inventing, or creating what was previously unknown.

How does a student learn to do research? A graduate student who works for years with a research advisor is very much like an apprentice learning a skill or craft. This process is intensely personal, involving extensive interaction. The university provides the setting in which that apprenticeship takes place, much like a workshop where a craft like violin making is passed on.

While it is unlikely that the teaching of research skills can be formalized, the following is a compendium of advice and guidance on research that I have given to my students over the years.

1. Research is hard.

Extending human knowledge is a difficult task. Discovering new and useful ideas and techniques is like attacking a granite cliff with your bare hands. Once in a while a small fragment breaks loose and progress is made. Each new advance is a gift to the world that contributes to the sum of human knowledge. With luck, that knowledge advances continuously, and all of humankind can benefit from that advance.

2. Research is exciting and addictive.

The moment of discovery provides an unbelievable rush of excitement. Researchers live for this high, and it can be addictive. It is one of the two best legal highs.

3. What is a good research problem?

The importance of having a good research problem cannot be overemphasized. A good research problem has either significant intellectual content or important practical ramifications. The best research problems have both. Which aspect you choose to emphasize will ultimately depend upon your personal tastes and preferences.

4. How do you find good research problems?

It can be difficult to find good problems. The difference between an experienced researcher and a novice lies in knowing what problems to work on. An experienced researcher knows where the boundaries of knowledge lie and has a sense of which problems are both solvable and important. Finding good problems is one of the most crucial things you can do as a researcher. In general, the best way to find good research problems is to do research, where you will find that each advance leads to new ideas and new challenges. Of course, this advice assumes that you already have a good starting point for your research, which is the responsibility of your advisor. Reading papers and attending seminars can also be extremely helpful for finding good research problems.

5. Read a lot and listen to others.

No one person can have all of the good ideas. Realize that you are but one small part of a huge worldwide research enterprise. Other researchers share their ideas in papers and talks. Listen to them and read their work. However, be aware that too much reading can be bad if it detracts from your time to do research or directs your thinking into other researchers' “mental grooves.”

6. Have an attack.

Inventing antigravity levitation is a great problem, but it is hopeless to try to solve it unless you have an *attack*. An attack is a promising approach or idea that you can bring to bear on a problem. Having a good attack is as important as finding a good problem. Your ability to formulate an attack on a problem depends on all of your skills, tools, and knowledge.

7. Be curious.

To do good research, you have to care about the problem you are working on. Good researchers tend to be curious. If you want to know why something is true, then ask why, and don't give up until you know. When you don't know "why," then let it gnaw at you. Don't be afraid to ask obvious questions such as: How did humans evolve phenomenal brains in such a short time? Why do humans need to sleep and dream so much? How does magnetism act through a vacuum? What happened before the big bang? The deepest questions are often the most obvious but are rarely asked. While it is unlikely you can solve such "big" problems (especially without a good attack), it is important to be constantly sensitive to new problems.

8. Ask good questions.

Besides being curious, it is helpful to ask good questions. What happens if...? Why is it true that...? When does it work? When does it fail? What if we change...? These questions can help you find good research problems.

9. Hedge your bets.

Choosing problems is like investing. Easy problems tend to have little payoff, while hard problems can have high return but are high risk. A wise strategy is to maintain a problem portfolio consisting of both high and low risk problems. However, the ultimate usefulness and impact of any new idea--whether it is high or low risk--are impossible to predict.

10. Be aware of "research density."

Each problem area you choose to work in will have a "research density," which, roughly speaking, is the ratio of the number of researchers to the number of ideas being developed and problems being addressed. High-density problem areas involve many researchers developing relatively few ideas and problems, while low density problem areas involve fewer researchers and more ideas and problems. Problem areas with high density may include "hot" topics with fierce competition and fast progress. On the other hand, high density may also indicate an overworked research area. In any event, the importance of a problem area may or may not be correlated with its research density.

11. Intelligence is multifaceted.

If there was ever a notion that could not be quantified, it is intelligence. The most important aspect of intelligence is the fact that all people think differently and have different experiences, perspectives, knowledge, and strengths. Relevant strengths include persistence, perseverance, patience, open-mindedness, ingenuity, curiosity, and creativity. Being extremely “smart” is helpful as well, but it is neither necessary nor sufficient for success. Know and understand your strengths whatever they may be, and build on them. It is true that no two people are alike.

12. Appreciate elegance and beauty.

Good results are elegant and beautiful, although no one knows why. They may be confusing or look messy at first, but human thinking evolves to appreciate their beauty. In this sense, research is like art. The works of Picasso, Stravinsky, and Mandelbrot are examples of this phenomenon.

13. Seek simplicity.

The greatest results are simple and may even look trivial. Most people would agree that *unexpected* simplicity is a form of beauty. Think of f=ma and $e=mc^2$. Yet results like these are found only with great difficulty and after years of possibly great confusion and unnecessary detail. (Who would think that a falling apple is related to the motion of the moon?) Hence, why not shorten the process and seek results with simplicity? You'll become famous and save everyone a lot of time. However, simple is *not* the same as “simplistic,” which is a form of ignorance. As Einstein said, “Things should be as simple as possible, but not simpler.”

14. Induce obsolescence.

Hilbert once wrote that the value of a paper is related to the number of earlier papers that it renders obsolete. While we build on the work of prior researchers, we also displace their work by introducing new points of view, developing new techniques, streamlining terminology and notation, and refining thought processes. That is progress. This obsolescence, however, does not detract from the pioneering importance of prior research.

15. Open doors.

Probably the real value of an advance or solution is the number of doors that it opens for future research. No solution should ever be an end in itself. Each advance should be a new beginning.

16. It's a complicated world.

Don't forget that the world is an extremely complicated place. For example, understanding what happens when you roll a bowling ball rolling down a lane, crumple a sheet of paper, break a window, pour cream in your coffee, or strike a match are extremely intricate problems. Be thankful that this complexity makes the world such a rich and interesting place, since it permits complex systems like you to exist as well.

17. Reinventing a better wheel.

The oldest piece of research advice is not to reinvent the wheel since it wastes time and resources. However, if you partially and inadvertently reinvent something, then you will be in a much better position to understand and go beyond other researchers' work than if you were a mere passive reader of their work. In addition, it rarely occurs that you retrace another researcher's steps exactly, and thus the unique aspects of your advances, your individual perspective, and the specialized tools you develop along the way may lead to further discoveries. It is often the case that you cannot understand or appreciate another researcher's work unless you rediscover at least part of it yourself.

18. Abolish NIH.

On the other hand, abolish NIH (not invented here) from your thinking since it is an insidious roadblock to research. NIH can occur when a researcher attempts, either consciously or subconsciously, to circumvent the discoveries of other researchers. While this approach can be of value since alternative and more important discoveries may result (as discussed in the previous paragraph), it often happens that years of effort are wasted by ignoring or being forced to circumvent the advances of others. This wastefulness often happens out of necessity in the world of patents, where engineers are often forced to avoid the use of patented techniques.

19. Be eclectic to exploit outside knowledge.

Advances are constantly occurring in all fields. In fact, innovations and new ideas that arise in one area tend to ripple from field to field. The scientific, technological, and intellectual web is strongly interconnected. By maintaining broad interests you can benefit by exploiting advances in other areas.

20. Keep unsolved problems in mind.

Feynman had a collection of unsolved problems in his head. As he learned new things he constantly returned to those problems until one cracked. This approach is an example of persistence.

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