A Student's Guide to Research

by

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Introduction

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? As teachers of students, especially graduate students, we are faced with the task of transmitting our research skills. This process is intensely personal, involving extensive interaction. A graduate student who works for years with a research advisor is very much like an apprentice learning a skill or craft. 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.

Advice on Getting Started in Research

1. Research is hard.

Extending human knowledge is a difficult task. Discovering new and useful ideas 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.

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 to find good 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 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. By doing research you will find that each advance leads to new ideas and new problems. 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 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 unless you have an *attack*. An attack is an 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 an attack), it is important to be 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...? As e. e. cummings wrote, "Always the more beautiful answer who asks a more beautiful question." 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.

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. Picasso, Stravinsky, and Mandelbrot are analogous 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 just 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, refining thought processes, etc. 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 hard 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, should 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. You may also find 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 should alternative and more important discoveries 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 often happens out of necessity in the world of patents. For example, some early radios had multiple tuning knobs and contrived circuits designed by engineers who were forced to circumvent the elegant superheterodyne patented by Armstrong.

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 is an example of persistence.

Advice on Solving Research Problems

21. Expose and challenge hidden assumptions.

We all labor under hidden, unspoken assumptions. Often we approach a problem with these assumptions in our minds without knowing it. These assumptions may thus constitute *mental* obstacles and barriers to our progress, not *real* obstacles. In fact, many of the greatest developments in science, engineering, and mathematics came from discarding mental obstacles that were harbored unrecognized by prior generations of researchers. The concept of infinity stymied the ancient Greeks (ask Zeno). Complex numbers confounded mathematicians for centuries, yet their complete properties can be summarized in half a page. Prior to the experimental breakthrough in 1986, theoretical models of superconductivity actually impeded researchers from making new discoveries. Hidden obstacles are insidious. Remember, your greatest obstacles may be *mental*.

22. Don't always believe experts.

While experts are knowledgeable about some area, they rarely admit what they don't know. Like all people, experts have biases, work from hidden assumptions, and have mental constraints. Always question authority and think for yourself as much as possible.

23. Filter the noise.

New effects send weak signals buried in noise. Your challenge is to amplify the signal and filter the noise to discover new phenomena.

24. Treat inconsistencies as opportunities.

Something doesn't add up! Something doesn't make sense! You thought through the problem in different ways, and you found an inconsistency. This is a chance to learn something by carefully analyzing your steps and thought process to discover the source of your error, if there is one, or possibly make a new discovery. Be sensitive to inconsistencies and view each mistake you make as a chance to learn something new.

25. Investigate anomalies.

More generally, when an unexpected or abnormal result arises, don't kick it under the workbench. Shine a strong light on it to see what's going on. Construct an example or experiment to emphasize the anomaly and bring it into clear view. Discoveries often live in the anomalies. Don't throw out the mold with the potato.

26. Find the crux of the solution.

Something unknown may lie at the heart of the solution to your problem. It may be an identity, inequality, or some "fact" that can be identified and isolated. Or, it may be some kind of trick or twist that prior researchers overlooked. Whatever the crux of the solution is, your goal is to find it.

27. Find a simpler unsolved problem.

Polya once said that for every unsolved problem there is a simpler unsolved problem. Although the essential difficulty of the original problem most likely remains, your ability to recognize and deal with it may be enhanced by the "simpler" problem.

28. Strip away unessential detail.

To simplify a problem, reduce it to its barest bones. Strip away all unessential detail and expose its crucial features. This process will help show you what makes it tick.

29. Divide the problem into testable conjectures.

Focus your thinking and activities by setting limited goals in the form of questions and conjectures. A series of precise, testable conjectures can provide a traceable path to the ultimate goal. Each conjecture can provide a self-contained, focused step in the research process that you can handle in isolation. Ask questions like: "Wouldn't it be nice if...?" or "Is it true or false that...?" Work incrementally.

30. Play detective.

Ask lots of specific questions like a detective trying to solve a crime. For example: Which is bigger? Is it always? When do solutions exist? When don't they? Is something a solution to a problem? Ask lots of little questions that can suggest clues.

31. Examine extreme cases.

You can learn a lot about a problem by pushing it to extremes. Make numbers big, small, zero, equal to each other, etc., to see what you can learn. These special cases may expose what the solution looks like around the edges.

32. Alternate between the general and the specific.

Specific cases give you insight and clues into the problem, but they may not give you the whole picture, which is your ultimate goal. The global picture you seek has generality, but it may be very difficult to uncover. Solution: Go back and forth between the two extremes, using clues from special cases to deduce the general case, and use the high level view of the general case to fit the pieces together into a general theory. Think of knowledge as an evolving jigsaw puzzle. Believe in the unity of knowledge, oneness, and wholeness.

33. Examples, examples, examples.

It is impossible to overemphasize the importance of examples. Many of the deepest results were obtained by studying specific examples. Examples can motivate and illuminate theories, and they are, in effect, the ultimate justification for your results. Keep a collection of examples in your back pocket as reminders of important results and for guidance in developing new results.

34. Seek generality, but not as an end in itself.

General theories can be elegant and broad, but generality without useful special cases is often pointless. An excellent strategy is to motivate and illuminate a general or abstract theory with lots of specific, concrete examples, then develop the theory, and, finally, show how the theory yields new results for the specific examples. This is the "sandwich" model of research and exposition: specific, general, specific, or concrete, abstract, concrete. The third step makes the whole enterprise worthwhile.

35. Take wild guesses.

Don't be afraid to guess!! (A mathematician's guess is called a conjecture.) If you are a good guesser and are lucky or smart enough to guess the answer, then you have won half the battle. Of course, you will then need to convince everyone that you have the right answer, or at least a useful answer. There are medicines used by doctors, sometimes found by "luck," that no one knows how or why they work! Nevertheless, it is important to seek an intellectual framework for your results in order to refine and extend them.

36. Work backwards.

If you have any knowledge of the solution obtained by guessing or in special cases, then you might be able to make progress by working backwards from your partial solution. The proverbial problem of "how to add zero by adding and subtracting the same quantity" may be impossible to figure out going forward, but may be trivial going backwards. Remember that you are seeking a path from the problem to the solution.

37. List all possibilities, and check them all.

If you reach a point where the next step can proceed by one of several possibilities, then don't become immobilized. Simply list all of the possibilities and systematically check them all. Not much insight is needed for this step, just patience. This approach proved the four color map theorem.

38. Transform to a new world.

Some problems that look insoluble in one world are transparent when transformed to a new world. Look for reformulations, dual versions, analogies, abstractions, etc. Sometimes rising up to a higher dimensional space can be extremely effective. However, this should not be an end in itself.

39. Solve the problem in different ways.

Bellman once wrote that one should try to solve problems in as many ways as possible since different techniques generalize in different ways. Also, different techniques have different strengths and weaknesses. Therefore, solving a problem with different tools and techniques may yield different kinds of results along with additional insight.

40. Be aware of your uncertainty.

Be sure of the answers you obtain by verifying them using different methods if possible, and be sure of what you know and what you don't know. In general, it is important to be aware of your uncertainty, that is, how certain or uncertain you are about things that you are not completely sure about. If you believe that something is true but it is, in fact, false (some published results may be wrong or you may have made an experimental, computational or logical error in your own work), then your thinking can be severely distorted. This point cannot be overemphasized. Finally, make sure that you and your collaborators agree about what is known and what is unknown in order to clarify the assumptions of your research and avoid wasteful confusion.

41. Use your tools wisely.

Everyone has certain tools and techniques. Some are good at geometry, others at algebra. Some have a cherished trick or identity. Feynman's favorite trick was differentiating with respect to parameters under the integral to obtain new integral formulas. (Try it: You can amaze your colleagues with new formulas that they won't be able to find in integral tables.) What often counts is not how many tools you have, but how wisely and effectively you use them. In particular, it is important to bring the right tool to bear on the right problem. Using too-powerful tools can make things harder than they need to be.

42. Develop necessary tools.

Since the result you seek is novel and not yet known by other researchers, you cannot expect it to follow immediately from known results or to be buildable with off-the-shelf parts. Joseph Henry had to coat his own wire to experiment with electromagnets. The Wright brothers had to build their own wind tunnel to study aerodynamics. Therefore, expect that you must develop tools and techniques that no one else currently has. This is a universal principle of research. Once you have those tools and techniques, then you are in a position to do things that no one else has done. Once you have these tools, run with them. Remember that good tools can be extremely powerful: one good knife can cut many strong ropes. But think of the iron ore and high temperatures that went into making that tool!

43. New discoveries are marginal at first.

The first light bulb only burned for a few seconds. What good was that? If you're lost in a cave and only see a small sliver of light, what good is that? Be sensitive to clues like a good detective, and savor partial successes. Realize that new ideas and results are often weak at first, and they need to be nurtured and defended in order to compete with established techniques and vested interests. That takes courage and perseverance.

44. Some hard problems have "easy" solutions.

Sometimes a problem that looks hard may actually have an "easy" solution. The classic example is the Gordian knot cut by Alexander the Great.

Another example is drunk driving. Amazingly, at one time drunk drivers were excused from responsibility because, they were, after all, drunk. In current times, the tables are turned and individual responsibility is assumed. The point of these examples is that seemingly hard problems had easy solutions that "simply" required a radical change in outlook. Once the outlook changed, the solution was easy.

Advice on Becoming a Researcher

45. Be technically excellent and work hard.

Practice technical excellence like a musician practices an instrument. As a researcher once said, "I always hoped to stumble onto a big breakthrough until I realized the odds of that happening were like waking up in the morning and finding I had been transformed into an accomplished concert pianist." In other words, hard work is essential. When an opportunity comes by, your preparation will allow you to recognize it and profit from it.

46. Don't fear mistakes.

Mistakes provide opportunities to learn. Understand the source of your error and fix your thinking. You'll be much better off in the long run. Learn from your mistakes (as long as they're not fatal) and move on.

47. Admit your mistakes.

Never hesitate to admit to yourself and your collaborators when you are wrong or have made a mistake. Not doing so generates confusion, slows progress, and displays insecurity. Doing so puts closure on issues, displays maturity, and allows your thinking to advance.

48. Savor successful failures.

The difference between research and many other human activities is that each research failure is actually a success. Each idea that proves wrong or ineffective provides insights and clues for new ideas and approaches. Each failure teaches us something valuable that suggests the next step. "Negative knowledge", that is, knowing what does NOT work and what is NOT true, is often extremely valuable. Unfortunately, books and papers tell you what works, but only rarely will tell you what doesn't work. Counterexamples are helpful for that purpose.

49. Be flexible.

Research is contingent by nature. The next step usually depends on the one before. Each advance can open up new paths. Unexpected discoveries (is there any other kind?) suggest new ideas and directions. Be flexible and don't fret about the long term. Be aware that you might solve a problem that is different from the one you started out to solve. (Remember penicillin and Silly Putty, both discovered by accident.) Be flexible and sensitive enough to seize opportunities since the greatest opportunities are often unanticipated. Therefore, don't spend too much time on research planning except to collect your thoughts, stimulate your thinking, or form a vision.

50. It's the process that counts.

While specific results are important, they are only stepping stones to future research. Therefore, even if you do not attain your stated objective, you should keep in mind the fact that the tools, techniques, and insights you obtain are of immense value by themselves. In other words, the process of research is often of as much value as the specific results you obtain. To fully appreciate this point, think about the difference between a manufactured object (such as a pencil) and the machinery needed to produce the object.

51. Have a vision and defend it.

Have a vision about what you want to see come out of your work. Think about where your work is headed. Explain and defend it to your colleagues to help you understand it better yourself. Review and update your vision periodically.

52. Don't get discouraged and (almost) never give up.

The research process is extremely nonlinear if not discontinuous. A year's or decade's worth of work can pay off in one day. Tools that take years to develop can yield their results for a long time afterwards. Be patient and persistent. Don't let other researchers discourage you. If they are your competitors, they may criticize your work to justify theirs. Don't believe people when they say something is impossible. Even so, the impossibility of perpetual motion does not obviate the benefits of energy efficient engines. However, there are times when it is wise to give up. Recognizing those times can be extremely difficult.

53. A lot of people can be wrong.

It can be daunting and require tremendous courage when your beliefs are in the minority. However, as a researcher it is your responsibility to develop new ideas and not merely ride the latest bandwagon or trend. The most difficult hurdle is trusting in the possibility that a few people can be right, while many people can be wrong. That takes courage. Remember that there were times when the world was thought to be flat, Fourier series were controversial, slavery was legal, and nuclear testing was common. Unfortunately, ignorance dies hard and knowledge threatens power.

54. Recognize when you're mentally tired and rest.

Your brain can get tired when your body is not, and such times can be hard to recognize.

55. Learn from the past.

Ideas evolve over time due to the efforts of many researchers like yourself. It can be extremely enlightening to understand how prior researchers overcame obstacles that they faced. Read their biographies.

56. What type of researcher will you be?

There are many types of researchers. Some are artists, craftsmen, trailblazers, organizers, and polishers. The type that you are or will become is a reflection of your personality and personal philosophy. However, you may wish to consciously change your type as you mature and recognize your strengths and weaknesses.

57. View research as an art.

Think of research as an art and think of yourself, the researcher, as an artist. No matter how technical your field of endeavor is, you have the opportunity to exert your personal style on the work that you do. You choose your own problems, you see the world through your unique vision, and you develop your ideas through your individual thought process. Strive to produce research that has your intellectual fingerprint on it.

58. Believe and enjoy.

Believe and be confident in what you're doing, and enjoy doing it. Have faith that your ideas are valid and will work out eventually.

59. Be a leader.

A true researcher must by definition be a leader, carving out new paths, choosing directions, and taking risks. Being a leader is far more difficult than being a follower, no matter how good of a follower you may be.

60. Reinvent yourself.

When you're stuck in a rut or if times change, then consciously change what you're doing or how you're doing it. Such changes can be refreshing and stimulating.

61. Respect intellectual property, and be generous and magnanimous.

Cite the work of prior researchers correctly, thoroughly, and conspicuously. Always give credit generously to others for their intellectual contributions as you would expect from them. This is the golden rule of research.

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The ideas I have collected together here come from many sources, both oral and written. It is impossible for me to remember and acknowledge all of these sources, especially the innumerable hours of discussion I have had with my colleagues during the past 20 years. I can, however, reference a few written sources that I know have had some influence on my thinking.

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About the Author

I believe I was in the sixth grade when I saw a mysterious symbol in a mathematics book (in the Time-Life Series) that I learned much later was an integral. I did not know anyone who could tell me what that symbol was, much less explain its meaning to me. Years later I was fortunate to study calculus in high school (at Holyoke High School in Holyoke, Massachusetts) under Mr. John Foley, an outstanding and dedicated teacher of mathematics to a generation of students.

When I began college I had no inkling of the meaning of engineering as a potential academic major. Instead, I studied applied mathematics as an undergraduate at Brown University and slowly discovered engineering as I moved on to graduate school at the University of Michigan. Determined to inject the real world into my theoretical world of mathematics and control theory, I took the radical step of forcing myself to take engineering positions. After two years at a Government laboratory (Lincoln Laboratory) and seven years in industry (Harris Corporation), which, incidentally, had the intended effect, I returned to academia. I find it exhilarating to apply mathematics to physical problems and to use physical thinking to illuminate mathematics.

I have found that the research environment of a university is ideal for rapidly developing new ideas with few constraints. The flexibility of being able to change directions on a moment's notice combined with the constant influx of students eager to do research provides a stimulating setting for investigating new ideas. I believe that the ideal research program (for me, anyway) is a combination of fundamental research and applied problems, each motivating and complementing the other in a kind of vertical integration of theory and practice.

I dedicate this paper to the memory of my father, Milton Bernstein, mechanic, musician, and mentor, who imbued in me the spirit of inquiry and an appreciation for ideas.