

How to do research

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I suppose that successful research cannot really be broken down into a formula, or a one-size-fits-all algorithm. Successful researchers have had many different approaches. Some researchers are driven more by intuition and experience, while others are driven by schedules and discipline. Some researchers are theoretical, while others are more applied. This paper is not intended to be a complete guide for how to do research. However, there are some basic principles of successful research that have proven helpful to me in my career, so I'd like to share with you, based on my own experience, some of those principles.

Balance

The key to research, like the key to many things in life, is balance. For instance, when studying for a test you can study either too much or not enough. The key is to study just the right amount. If you study too much then you will be too tired to do well on the test. But if you don't study enough then you will not know the material well enough. When designing a control system you can design a system with a gain and bandwidth that is so high that the system will be too susceptible to noise. But if the gain and bandwidth are too low then the system will be sluggish to respond to commanded inputs. I have found that the key to research, like the key to life, is balance. This paper discusses some of the aspects of your research that you need to balance in order to be successful.

Find the right problem

Before you can be successful in research you need to find the right problem to solve. You need to find a problem that is both interesting and challenging. When I say it has to be interesting, I primarily mean that it has to be interesting to *you*. It will be hard for you to be motivated in your work if your work is not interesting to you. But your research problem also has to be interesting to other people. If it is not interesting to other people then who cares if you solve it? The conferences and journals that you submit to won't care, and the faculty won't care. So the problem needs to be interesting to you and to other people.

The problem also has to be challenging. If you solve an easy problem then, again, no one will care, you won't be able to publish it anywhere, and it may not even be significant enough to merit a Masters degree. So the problem has to be challenging. But the problem cannot be *too* challenging. It has to be solvable. For instance, building a time machine is an interesting and challenging problem, but if you devote your career to building a time machine then you are doomed to failure. It is just too hard of a problem.

So again it comes down to balance – you need to find a problem that is hard enough to be interesting, but not so hard that it is impossible. How do you find that balance?

Experience helps. When you have read a lot and done a lot of research, you learn to recognize what problems are too easy and what problems are too hard. At the beginning of your career you may not have the experience to recognize the difference between too hard and too easy, but your advisor may have the experience that is required, so you can rely on your advisor for help in this area.

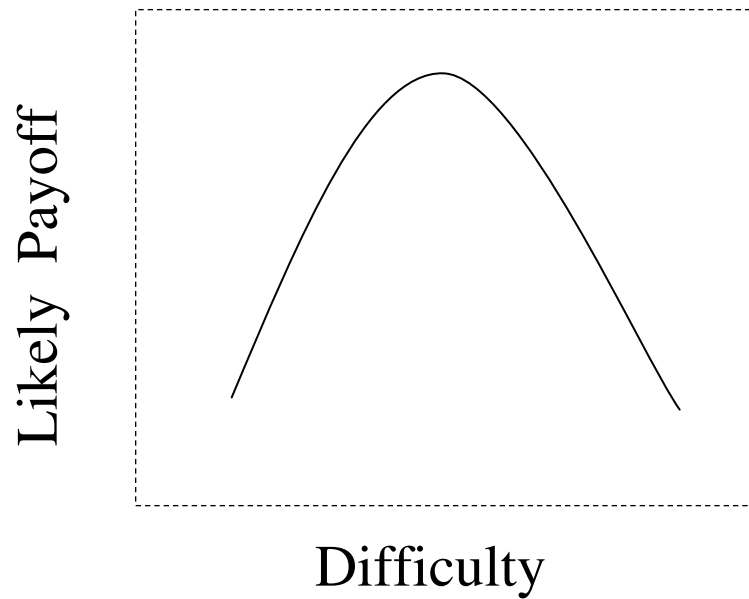


Figure 1 – Likely payoff as a function of problem difficulty

Another way of looking at this is that you need to find the optimum balance between difficulty and likely payoff, as shown in Figure 1 [Loehle]. There is a tradeoff here. You want to choose a problem that you can solve. But if you choose a problem that clearly has a simple solution, then your solution will be unimpressive and the payoff will be low. On the other hand, if you choose a problem that has great difficulty then the possible payoff is huge, but you are not likely to solve the problem, so the likely payoff is small. There is an optimum point on this difficulty / payoff curve. Choose a problem that has a moderate level of difficulty so that the likely payoff is optimized.

Once you gain some job security you can take bigger risks and move farther to the right on the difficulty / payoff curve. For instance, Einstein spent the last 20 years of his life searching for a grand unified theory to tie all of the four fundamental sources of nature (strong nuclear, weak nuclear, gravity, and electromagnetism) into a single framework. He failed, but he wanted to try, and his legacy is not diminished because of it. Also, as you will probably try to solve more than one problem at a time at various points in your career, you can spread your problems along the difficulty / payoff curve so that you are working on some problems with low difficulty, some with moderate difficulty, and some with high difficulty. If you are successful in your high-difficulty research then the payoff

will be large. But even if you fail in your high-difficulty research, you will have enough low- and moderate-difficulty problems to ensure that you will achieve at least some payoff.

As you begin to attack a highly difficult problem, don't be discouraged by the meager success at the start. Highly difficult research programs take time to bear fruit. It takes six months to grow a squash and twenty years to grow an oak tree. Do you want your research to be a squash or an oak tree? As a graduate student you don't want to take twenty years to get your degree. But eventually in your career you may need to take a more far-sighted view of your research in order to achieve something significant. Early results will be meager, but keep the long term in mind. One is reminded of the story of Benjamin Disraeli, former prime minister of England, who made a visit to the laboratory of Michael Faraday, one of the early experimenters with electricity. After watching Faraday conduct a few simple demonstrations with electricity, Disraeli asked the scientist, "But of what possible use is it?" Faraday responded, "Mr. Prime Minister, what use is a baby?"

Solve the right problem in the right way

Not only do you need to solve the right problem, you need to solve it in the right way. How should I design a controller for my system? Nonlinear PID? Optimal control? Neural networks? What is the right approach? Some approaches will not work with certain problems. Some approaches will work but will be awkward and unnatural. Your research should be directed towards solving a problem, not applying a solution. Lots of researchers become well-versed in some particular technology and so they try to solve all problems that they encounter with their favorite technology. This is like the home builder who knows how to use a hammer so every problem looks like a nail. But you can't use a hammer for everything. A hammer works great if I want to pound a nail in a board. But if I try to use a hammer to paint my house then I'm going to have a hard time.

But if a hammer is the only tool you know how to use and you have a problem that you need to solve, what can you do? You have to use the hammer to solve the problem because that's the only tool that you know how to use. But it may not be the best tool for the problem. That's why it's important to learn how to use more than one tool. Acquire more than one tool for your toolbox so that when you encounter a problem you have a choice of tools and you can select the best tool for the problem at hand.

You don't know what kind of problems you will encounter in your research, so you need to collect a repertoire of tools that you can use. You need to be familiar with a variety of technologies in order to be prepared for the problems that you will encounter. In your research you may encounter a power electronics problem – do you know how to solve it? You may encounter a state estimation problem - do you know how to solve it? You may encounter a constrained optimization problem - do you know how to solve it? You may encounter a nonlinear identification problem - do you know how to solve it?

As a graduate student you may not have a lot of tools in your toolbox simply because you may not have a lot of experience yet. Don't worry about it – no one has *all* the tools in his toolbox. No one knows everything. That is why it is important to use whatever resources are available to you. If you don't have a chainsaw to cut down that tree in your back yard, don't try to use your hammer. Go to your neighbor and borrow his chainsaw. Or go to the tool rental store and rent a chainsaw. Or hire someone else to use a chainsaw. Or go the hardware store and buy a chainsaw. That is how a homeowner can effectively expand his repertoire of tools.

As a researcher you can effectively expand your repertoire of tools by going to seminars and by continuing to learn. Most seminars are boring and you don't really get anything from them. But once in awhile you get a gem that can make a lot of difference in your work. I went to a seminar a few years ago by Dr. Tien-Li Chia and he talked about constrained Kalman filtering. That struck a chord with me because I saw an immediate application to fuzzy logic system tuning. So I extended his work a bit and gave a seminar at NASA. That led directly to a three-year NASA grant, several publications, and

financial support for some graduate students. I have forgotten most of the hundreds of seminars I've attended, but that one seminar made a difference. So keep learning and keep adding to your repertoire of tools, because you never know when you will find a new tool that will make a difference in your research.

You also need to regularly read books to continue expanding your mind. Reading new books is not a problem if you're taking classes, but even after you're done taking classes you need to continue read. One way that I motivate myself to read is by reviewing books for journals or magazines. I've reviewed books for the IEEE Control Systems Magazine and Control Engineering Practice. That way I get a free book, I learn something new, and I get my book review printed in the journal. You also need to regularly read journal, magazine, and conference papers. Again, one way to do this is to volunteer to be a reviewer. I've reviewed papers for over a dozen different journals, magazines, and conferences. I've learned a lot in the process, and it also gives me something to add to my resume.

Generalize and specialize

When we talk about expanding our repertoire of tools, we're simply talking about learning about more technologies. But some balance is required. If you continue learning about more and more different things, then you will never get around to specializing in one particular area, which means that you probably won't accomplish anything significant in your research. You can spend the next year of your life studying some topic, then move on to another topic for a year, and so on for the rest of your life. But if you only spend one year studying something you can't really accomplish much in that area. At the end of your life you will be a generalist to the highest degree, but a specialist in nothing. You will have a lot of breadth but little depth. You will know nothing about everything.

Significant accomplishment requires that you spend years on some topic, delving into the intricacies and nuances that the generalist will never have time to study. Successful

research means that you will have spent a lot of time on one particular area, plumbing the depths of knowledge that only a few others have reached. But if you spend your entire life in one small area, you may know more than anyone else in that small area, but you will not have a broad enough knowledge to see the big picture. You will not be able to see the applications of your knowledge, or how your specialized area can be synergistically combined with other disciplines. At the end of your life you will be a specialist to the highest degree, but only in one tiny area. You will have a lot of depth but little breadth. You will know everything about nothing.

Thing back to our tool analogy. If you have only one tool then you can learn how to use that tool better than anyone else. But you will not accomplish much with it because you simply can't do that much with only one tool. On the other hand, if you buy a new tool every day, then you will have more tools than anyone else. Your neighbors will constantly be borrowing tools from you. But again, you will not accomplish much because you simply will not have time to learn how to use such an overwhelming array of tools. You need a balance between generalization and specialization.

Astrophysicist and Nobel Laureate Subrahmanyan Chandrasekhar (1910-1995) was amazingly productive in his research. When asked the secret of his research, he said that he took up a new topic about every seven years. His accomplishments were in areas as diverse as stellar dynamics, white dwarfs, relativity, and radiative transfer. Although these areas are all in the field of astrophysics, they are diverse enough so that he didn't get stuck in a rut, he was able to see how his research fit with other areas, and he stayed mentally sharp as he continually learned new things. But he did not take up a new topic every six months because that would have been too often. If he had taken up a new topic every six months then he would have been too much of a generalist and he never would have accomplished anything.

Have a plan of attack

To accomplish anything in your research you need a plan of attack. You need a blueprint. No one would try to build a house without a blueprint, and no one should try to solve a research problem without a blueprint. Write out your plan of attack. Be organized. Plan ahead of time what you're going to do, when you're going to do it, and how you're going to do it. But remain flexible so that when unexpected circumstances occur in your research you are free to modify your blueprint.

When you began college you planned what courses you would take and what semesters you would take those courses. If you just began taking courses without a long-term plan your education would have been inefficient. But you also remained flexible. When courses conflicted or were cancelled you modified your long-term plan. That is how research needs to be done. You need to have a specific research plan but you also need to remain flexible. Your natural tendency may be that you are rigid and you have difficulty deviating from your plan. Or your natural tendency may be to leap before you look, to begin your research without a plan. You need to compensate for your natural weaknesses. Have a plan, but be flexible.

Simplify your work and complicate your work

The next area in which you need balance in your research is simplification and complication. Let's begin with simplification. There are two ways in which you need to simplify your problem. First you need to solve your problem one step at a time. If you need to build a robot you can't just sit down and build a robot. You need to build it one step at a time. First you figure out how to drive a motor from a microcontroller. Then you figure out how to get optical sensor signals into your microcontroller. Then you figure out how to mount the motors on a chassis. You take one step at a time. If you sit down and try to build a robot you will fail because it is too complicated. But if you take one step at a time you will succeed because each step is simple enough to solve.

There is also another way in which you need to simplify your work. When you derive a technique or method in your research you need to apply that technique to a simple problem. For instance, in my multivariable control class we derive the small gain theorem which can be used for analyzing the stability robustness of multi-input / multi-output systems. One approach that helped me get an intuitive grasp of the small gain theorem was to apply it to single-input / single-output systems. In that case the small gain theorem reduces to the Nyquist stability criteria. Similarly, when I teach Kalman filtering I always apply it to a set of independent measurements of a constant and show how the Kalman filter reduces to simply taking the average of the measurements. So apply your method or technique to a simple problem. That will help you get an intuitive feel for the theory. Another aspect of this is to find out what your technique or method does in extreme cases. What does your controller do when the input is zero? What does it do when the input is plus or minus infinity? The answers should match what you expect intuitively. If they don't then you have a problem. If they do then you have some confirmation that your theory is correct and you gain further intuitive insight into the theory.

You need to try to generalize your theory to match problems beyond your specific interest. You may derive a method for controlling a specific type of nonlinear plant. When your method succeeds, ask yourself if it can be generalized so that it is useful for more people and a wider range of problems. Isaac Newton found that the equation $F=ma$ applied to apples falling to the ground. He could have stopped there, euphoric with his new discovery, but he generalized his theory and found that it applied not only to falling apples but also to orbiting planets. As another example, think about the mathematics and engineering that we rely on in our school and work. Technology grew out of a Western world culture that did not tolerate ambiguity but that rather sought scientific laws to describe the world around us, scientific laws that could never be violated. If the Renaissance scientists like Newton thought that the world was governed by the capricious whims and fancies of gods and goddesses then they never would have bothered to discover inviolable scientific laws. But then Lotfi Zadeh came along in the 1960s and wondered if we somehow take the mysticism of the far East and combine it with control theory. Can something be true and not true at the same time? Zadeh generalized

mysticism to control theory and obtained fuzzy set theory, and today we have thousands of successful control applications around the world that are based on fuzzy set theory. This is all because someone was able to generalize beyond his immediate experience.

Work hard

Successful research requires hard work. There is no substitute for hard work. There is nothing more common than talented and intelligent individuals who have limited success in their research or work. Why? Because they don't work hard. Hard work can make up for your lack of skill in many areas. But no matter how much skill or talent you have, you won't get anywhere without hard work. Thomas Edison (1847 – 1931), with over 1000 patents, said that genius is 1% inspiration and 99% perspiration. He tried 10,000 different designs for the light bulb before he found one that worked. He didn't have the tools to theoretically determine the correct light bulb design and model it on a computer. So he compensated for his lack of tools with hard work, and he succeeded.

Again, think of the tool analogy. You can acquire a wonderful set of tools. You can acquire all kinds of power tools and woodworking tools. But if you don't get out there and work, the tools won't do you any good. Likewise, you can acquire a wonderful set of tools for your research. You can learn all kinds of different mathematical techniques and computer and software technologies. But the tools won't do the work for you. Also, your advisor won't do the work for you. *You* need to get out there and work.

Part of hard work is persistence. Thomas Edison tried 10,000 different designs for the light bulb before he found one that worked well. If he was like most of us he would have tried only a few designs, given up, and moved on to something else. He wrote, "The electric light has caused me the greatest amount of study and has required the most elaborate experiments. I was never myself discouraged, or inclined to be hopeless of success. I cannot say the same for all my associates." The same will hold true for you in your research. You may spend months trying to solve a problem, or trying to successfully implement an engineering design. Finally your efforts will pay off and you will

experience an epiphany, a moment when you will suddenly understand the problem that you've been studying for so long, a moment when you will suddenly see how to get around the roadblock that has been holding you back, a moment when you will suddenly achieve success in your experiment.

On the other hand, you also need to know when to give up, or rather, when to change your approach. Some problems simply cannot be solved. Don't try to find the last digit of π because you will surely fail. But if you were trying to find the last digit of π and you were the first one to prove that π was a transcendental number that does not have a last digit, then you would have an accomplishment to be proud of. Like the rest of your research, persistence and flexibility need to be balanced.

How can you find the right balance between persistence with flexibility? One guideline might be to recognize your natural tendency and make a conscious effort to resist it. Your natural tendency might be to flit from one problem to the next, like a bumblebee flitting from flower to flower, never pausing long enough accomplish anything meaningful. If you recognize this tendency in yourself, then make a conscious effort to stay at each flower longer and to persist until you achieve your goal. But perhaps your natural tendency is stubbornness. Your natural tendency might be to persist too long, to bang your head against a wall that will not move. If you recognize this tendency in yourself, then make a conscious effort to move on to the next problem when you find yourself spinning your wheels.

Don't be afraid to fail

In the midst of all his failed attempts with his light bulb, Thomas Edison said, "I have not failed. I have merely found 10,000 ways that did not work." He viewed his failures as learning opportunities. We can view our failures that way too. Anyone who has accomplished a lot in life has also failed a lot in life. This is because the only way to accomplish a lot is to attempt great things, and the one who attempts great things will sometimes fail. So don't worry about failure. Dean Kamen is an inventor with over 150

patents. His most notable invention is the Segway, the human transporter. When he reviews the performance of his engineers he insists that they have made some spectacular failures in the past year. If his engineers have not failed enough, then he knows that they have not attempted any great feats. And if they do not attempt any great feats, then they cannot accomplish anything great.

If you're not afraid to fail, then you should not be afraid to admit when you have failed. If you cannot admit your mistakes then you will never learn from your mistakes. Your ego might prevent you from achieving personal and professional success. If your ego is so large that you cannot admit your shortcomings and mistakes, then you will never learn and you will never grow. You want to look good to other people, and so you often try to cover up your mistakes or blame someone else. Actually, the best way to look good to other people is to admit that you are wrong. Think about the last person you heard apologize for something. You probably respected that. So admitting your mistakes will not only gain you the respect of others, but it will allow you to learn and to grow.

Henry Petroski talks about this in his book "To Engineer Is Human: The Role of Failure in Successful Design." He shows that every great engineering success is made possible only by the failures that preceded it. We can learn more from failure than we can from success.

Balance confidence and doubt

Another area of research in which you need balance is confidence and doubt. First you have to believe in what you are doing. You have to believe that you are on the right track to finding a solution to your research problem. Thomas Edison would never have persisted for so long in his light bulb research if he wasn't confident that he was on the right track.

On the other hand, you need to inject a healthy amount of skepticism into your research. Others have said that a certain method is the best way to solve some problem, but why

should you believe what other people have said? They might be wrong. They may have overlooked something, or oversimplified their analysis of the problem. You need to be willing to learn from the work of others, but you also need to take everything you read with a grain of salt. You need to learn things for yourselves. In the process your knowledge will become ingrained within your own understanding in a way that secondhand knowledge is not, and you may obtain some insights that will allow you to see the shortcomings of others' work and make some real advancements. You need to learn from others' work, but if you swallow whole everything that you read then you won't have much of an opportunity to make improvements.

Think again about Dean Kamen. Everyone knows that wheelchairs can't climb stairs. But Kamen thought, "Why not build a wheelchair that can climb stairs?" Eight years and 50 million dollars later, he had the Ibot, a six-wheeled robotic "mobility system" that can climb stairs, traverse sandy and rocky terrain, and raise its user to eye-level with a standing person. How much criticism do you think he received during those eight years? He was a guy without a college degree trying to get a stair-climbing wheelchair approved by the FDA. But he refused to listen to the conventional wisdom and he succeeded.

You probably don't have eight years or 50 million dollars to finish your research, but you can still think for yourself, ask questions, refuse to be discouraged by critics, and accomplish great things. You may not achieve the success of Dean Kamen, but you can still move at least part of the way down that road.

Communicate

Another important aspect of your research is communication. You need to communicate your research results with others. You can do the best work in the world, but if no one else knows about it then what's the point? You can invent the most amazing engineering techniques, but if no one else ever hears about it then what was the point? So you need to communicate well. You need to be able to write well. In order to write well you need to read. The more you read the better you will be able to write. Read novels and read non-

engineering books. Just read. As you read books you will get a better feel for the English language and that will help you write better. Once you know how to write ... write! Write articles for engineering journals, conferences, and trade magazines. Put your writings up on a web site. Let other people know about your work so that they can learn from your work, build upon your work, and criticize your work so that you can continue to learn.

You also need to learn how to speak. Take a public speaking class. Volunteer to give presentations. Present your work at conferences. Make your services as a speaker available to schools and engineering organizations. As you speak and write more, you will become more proficient at communication. Practice makes perfect.

Finally, write and speak with authority. If you have an engineering degree, especially a graduate degree, you are an expert. When you present the results of your research, remember that you know more about your research than anyone else. People will look to you for answers. When you get a graduate degree and get a job, people will expect you to have the answers. You may not always have the answers, but you need to be able to *find* the answers and to give your supervisor an honest and well-informed appraisal of technical options.

Progress is cumulative

To paraphrase Renaissance poet John Donne, “no man’s research is an island.” We need to do research with an awareness of other research. First, build on the work of those who have gone on before you. Lots of good engineering work has been done by lots of brilliant engineers. What a handicap it would be if we tried to conduct research without taking advantage of all of those resources. Isaac Newton said, “If I have seen farther than others it is because I stood on the shoulders of giants.” We can reach higher when we stand on the shoulders of others. Read the literature and find out what others have done with the research problem that you’re studying. List the various methods of solution, along with their advantages and disadvantages.

Also allow others to build on your work. Good research does not simply complete the solution to a problem. Good research opens doors to further work that was not possible before. So when you summarize and conclude a research project, think about how your work can be extended or applied or improved. Communicate these possible avenues of further exploration in your speaking and in your writing. Remember that you know your work better than anyone else, so you will be the one who is most aware of how your research can be built upon.

Independence and teamwork

Another area where you need balance is the ability to work independently, and the ability to work as part of a team. First you need to learn to work independently. That means you need to be able to learn on your own. If you don't know something, you won't always have a professor to go to for the answers. You need to develop enough initiative so that you can solve problems on your own. One of the criterion for a successful graduate thesis is the ability to conduct *independent* research. Your advisor wants you to learn independence. Actually, every supervisor in every field of work wants their employees to take the initiative and be independent. A janitor's supervisor wants him to be independent in taking care of sanitation problems that he has not seen before so that the supervisor is not constantly bombarded with the problems of his employees. The university provost wants the college dean to take the initiative in raising funds and visibility rather than constantly going to the provost with his problems. The chair of the department wants his professors to take the initiative in developing new courses, obtaining lab equipment, and writing grant proposals, instead of constantly depending on the chair for direction. And the professor wants his graduate students to learn how to solve problems independently rather than constantly going to the professor for every problem or question that arises.

But it is possible to be too independent. You also need to learn to work on a team. This goes back to some of the topics discussed earlier. For example, you need to learn to build on the work of others, and you need to communicate your failures and your successes with others. You also need to take direction from your supervisor, which in your case is

your advisor. Once I had a student who did an independent study with me. He did great work but he did not do what I asked him to do. He simply ignored my requests and did his own thing. So even though his work was excellent, he did not get a good grade in the independent study course. This will apply in your professional life also. For example, if you work for a process control company and you spend all your time inventing a breakthrough microcontroller architecture, you will get fired even though you did great work. You have to work as part of a team and under the direction of your supervisor. If you insist on doing your own thing then you should forget about industry and become a professor.

Balance in your life

Find balance in your life. You need to do more in life than engineering. You need to have personal relationships, a spiritual life, hobbies, and rest. Hard work is crucially important, as discussed earlier, but if you work so hard that you don't have time for anything else then you will find that your hard work is counterproductive. All engineers have had the experience of banging their head against a problem that refused to be solved, but after coming back to it the next day the solution stuck out like a sore thumb. The agricultural principle that God gave the Israelites in the Old Testament was to allow the land to "rest" one year out of seven. This allowed much-needed nutrients to be resupplied to the soil, and in the long run the land was more productive with one year off than it would have been with seven consecutive years of harvest. Another commandment that God gave the Israelites was to work for six days and rest for one day. In the long run the Israelites found themselves more productive in their work with one day off than they would have been with seven consecutive days of work. James Watson (the co-discoverer with Francis Crick in the 1950s of the structure of DNA) writes, "Much of our success was due to the long uneventful periods when we walked the college or read the new books."

When you work, work hard. But don't rush to get your work done. Take time to think. In a study conducted in 1975 it was found that good students tend to take longer to read a problem than poor students. Good students took longer to read the problem because they

were thinking about it as they read it. Then they took less time to actually solve the problem. You can write and run computer programs and collect data at a frantic pace, but take a step back and think about what you're doing and why you're doing it. In the long run you will be more productive.

Conclusion

We've covered a lot of material, but ultimately research boils down to balance. You need to balance generalization and specialization, achievability and payoff, simplicity and complication, hard work and rest, persistence and flexibility, confidence and doubt, and independence and cooperation. You also need balance in your life in order to succeed in your profession. Learning how to do research can be viewed as a nonlinear optimization problem. You cannot learn how to do research by solving a formula or by following a well-defined procedure. It will require a certain measure of trial and error and it will entail an inevitable amount of failure. Hopefully the ideas in this paper will assist you in your search for a global optimum in a life of successful research.

Read more about it

- A PowerPoint presentation of this material is available on the internet at <http://academic.csuohio.edu/simond/courses>
- C. Loehle, A guide to increased creativity in research – inspiration or perspiration? *BioScience* 40(2), pp.123-129, February 1990
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