Unfortunately, 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.

But the key to research, like many things in life, is balance. When designing a control system, if the gain and bandwidth is too high, 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. Here are some aspects of research that require balance in order to be successful.

**Find the right problem**

To be successful in research, you need to find the right problem to solve. A problem that is interesting to you and challenging. The motivational factor will not be there if your work is not interesting to you. But your research problem also has to be interesting to other people. If not, who is going to care if you solve it? The conferences and journals that you submit to won’t care, and the faculty that determines your degree fate 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. The work won’t get published and it may not even merit a Masters degree. But the problem cannot be too challenging. It has to be solvable. You need to find the optimum balance between difficulty and likely payoff, as shown in Fig. 1 [Lochle]. 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 still small. There is an optimum point on this difficulty/payoff curve: a problem with a moderate level of difficulty so that the likely payoff is optimized.

How do you determine “moderate”? 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, your advisor/supervisor should have the experience to help in this area.

(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 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, during your career, you can spread the problems being worked on along the difficulty/payoff curve so that there is a mix. 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 at least some payoff.

**Have a repertoire**

Not only do you need to solve the right problem, you need to solve it in the right way. To design a controller for a system, do we go with nonlinear PID? Optimal control? Neural networks?

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 a particular technology and so they try to solve all problems that they encounter with that favorite technology.

You don’t know what kinds of problems you will encounter in your research, so having a repertoire of approaches is vital. You need to be familiar with a variety of technologies. You may encounter a power electronics problem, a state estimation problem, a constrained optimization problem, a nonlinear identification problem...do you know how to solve them?

Early on, your range will be limited. Don’t worry about it. No one knows everything. Even later on, be sure to use whatever resources are available to you. Get help from a colleague, buy books on the subject, or outsource that part of the problem to someone else. Or hire someone with the expertise.

For instance, expand your repertoire by going to seminars. 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 huge 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 seminar 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.

Reading on a regular basis is usually not a problem when taking classes. But even afterwards, one needs to continue to read on a regular basis. 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. I get a free book, I learn something new, and I get my book review printed. You also need to regu-
larly read journal, magazine and conference papers. Again, one way 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. So keep adding to your repertoire, because you never know when you will find the one that makes a difference in your research.

Have a plan of attack
To accomplish anything in research, a plan o

www.phdcomics.com

Generalize and specialize
When we talk about expanding one’s repertoire, we’re simply talking about learning about more technologies. But some balance is required. If you never get around to specializing in one particular area, you probably won’t accomplish anything significant in your research. If you spend one year studying one topic, and the next year studying something else and so on, you will have a lot of breadth but little depth. You will know basically 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 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. Generalization and specialization must be balanced.

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. Chandrasekhar was able to see how his research fit with other areas, and he stayed mentally sharp as he continually learned new things. If he had taken up a new topic every six months, Chandrasekhar never would have accomplished anything.

Simplify and complicate
The next area that requires balance in your research is simplification and complication. Let’s begin with simplification. There are two ways you need to simplify your problem. First, the problem needs to be solved one step at a time. When building a robot, you figure out how to drive a motor from a microcontroller; then how to get optical sensor signals into your microcontroller; then how to mount the motors on a chassis. You take it one step at a time and succeed, because each step is simple enough to solve.

The second way is when you derive a technique or method you apply that technique to a simple problem. For instance, in my multivariable control class we derive the small gain theorem. It 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.

When your method succeeds, ask yourself if it can be generalized to be useful to 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, but he generalized his theory and found that it applied not only to falling apples but also to orbiting planets.

As another example, technology grew out of a Western world culture that did not tolerate ambiguity. If scientists thought that the world was governed by the capricious whims of gods and goddesses, they never would have bothered to discover inviolable scientific laws. But then Lotfi Zadeh came along in the 1960s. He wondered what 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. Today, we have thousands of successful control applications worldwide 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? They don't work hard. Hard work can make up for the lack of skill in many areas. Thomas Edison (1847 – 1931), with over 1000 patents, said that genius is 1% inspiration and 99% perspiration. 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.

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 were like most of us, he would have tried 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. You may spend months trying to solve a problem or successfully implement an engineering design. Finally, you will experience an epiphany, a moment when you suddenly understand the problem, you suddenly see how to get around the roadblock, the moment when you suddenly achieve success in your experiment.

However, you do 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 \( \pi \) because you will surely fail. But if you were trying to find the last digit of \( \pi \) and you were the first one to prove that \( \pi \) was a transcendental number with no last digit, then you have an accomplishment.

Like the rest of your research, persistence and flexibility need to be balanced. 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. Then make a conscious effort to persist until you achieve your goal. But perhaps your natural tendency is stubbornness. Your natural tendency might be to bang your head
against a wall that will not move. 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.
So don’t worry about failure. Dean Kamen is an inven-
tor 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.

You also 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 pro-
fessional success. You want to look good to other
people, and so you 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 suc-
sess is made possible only by the failures that preced-
ed it. We can learn more from failure than we can
from success.

**Balance confidence and doubt**

You have to believe in what you are doing. You
have to believe that you are on the right track.
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 may
say a certain method is the best way to solve some
problem, but why should you believe them? They
might be wrong. They may have overlooked some-
thing, or oversimplified their analysis of the problem.
While learning from the work of others, take every-
thing you read with a grain of salt. You need to learn
things for yourself. In the process, your knowledge
will become ingrained in a way that secondhand
knowledge is not. You may obtain some insights that
show the shortcomings of others’ work. Your work
then can provide some real advancement.

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 the
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 try
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
travel part way down that road.

**Communicate**

You need to communicate your research result
with others. You can do the best work in the world
but if no one else knows about it then what’s the
point? So you need to communicate well. You need to
be able to write well. To write well you need to read.
As you read books, you will get a better feel for the
English language and that will help you write better.
Then write! Write articles for engineering journals,
conferences and trade magazines. Put your writing
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 pub-
ic speaking class. Volunteer to give presentations
Present your work at conferences. Make your service
as a speaker available to schools and engineering
organizations. 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. At your 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 the technical options.

**Progress is cumulative**

We need to do research with an awareness of other
research. Lots of good engineering work has been
done by lots of brilliant engineers. Isaac Newton said,
“If I have seen further than others it is because I stood
on the shoulders of giants.” Read the literature and
find out what others have done with the research
problem that you’re studying. List the various meth-
ods of solution, along with their advantages and dis-
advantages.

Also allow others to build on your work. Good
research does not simply complete the solution to a
problem. Good research opens doors to work not
possible before. So when you summarize and con-
clude a research project, think about how your work
can be extended or applied or improved. Communic-
ate these possible avenues of further exploration in
your speaking and in your writing. You know your
work better than anyone else, so you are the 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. Working independently, you are able to learn on your own and you have enough initiative to solve problems on your own. One criterion for a successful graduate thesis is the ability to conduct independent research. Your advisor wants you to learn independence. Actually, supervisors in every field of work want their employees to take the initiative and be independent. The supervisors do not want to be constantly bombarded with the problems of their employees.

But it is possible to be too independent. You also need to learn to work as part of a team. You also need to take direction from your supervisor (or 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. This will apply when you work, work hard. But take time to think. In a study conducted in 1975, it was found that good students tended 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. Take a step back and think about what you're doing and why you're doing it. In the balance, you will be more productive.

Read more about it
- A PowerPoint presentation of this material is available on the internet at http://academic.csuohio.edu/simond/courses
- R. Hamming, You and your research, IEEE Potentials, pp.37-40, October 1993

Grades Don't Matter, Sources Say

Palo Alto, CA (AP) - Documents obtained by the Associated Press indicate that grades achieved in postgraduate classes have no effect on future prospects for students enrolled in academic institutions.

According to interviews with several current and past graduate students, "grades don't count," said former grad student and now billionaire Jerry Yang, co-founder of Yahoo! Inc. "I got mostly B's in grad school, which at Stanford was really really bad."

A poll conducted by the Los Angeles Times showed that over 85% of first year grad students believe getting high marks "is worth the effort" and "a valuable way to spend my time". Fewer than 10% of fifth year students felt the same way.

In reality, neither employers nor your parents appear to care if you get an A or a B in your advanced Nonlinear Optimization class. "I'm just glad I don't have to pay for tuition any more," said a mother who wished to remain anonymous.

Reaction among graduate TA's was mixed, with some expressing shock that their late hours grading amounted to nothing, while others showed visible relief that losing a student's final exam will not really ruin their life.

Sources close to academic faculty reveal that this fact is well known among professors. "Of course grades don't matter," said Prof. Smith, "we only care about the lab work." Grades only serve to "feed the ego of the smart students, and break the spirit of the mediocre ones."

NOW you tell me?? A grad student expresses frustration over the revelation

Continued on page A23

copyright 2004 Jorge Cham
www.phdcomics.com

in your professional life also. If you insist on doing your own thing then you should forget about industry and become a professor.

Balance in your life

You need more in life than engineering. You need to have personal relationships, a spiritual life, hobbies and rest. Hard work is crucially important, but if 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 the next day the solution stuck out like a sore thumb. James Watson (the co-discoverer with Francis Crick in the 1950s of the structure of DNA) wrote, "Much of our success was due to the long uneventful periods when we walked the college or read the new books."

V. Li, Hints on writing technical papers and making presentations, IEEE Transactions on Education 42(2), pp.134-137, May 1999

About the author

Dan Simon is an Associate Professor in the Electrical and Computer Engineering Department at Cleveland State University in Cleveland, Ohio. Before entering academia, he worked for seven different companies in industry over a period of 14 years in the aerospace, automotive, biomedical, process control, and software fields. His research interests include control theory, neural networks, and fuzzy logic. He is a Senior Member of the IEEE and the faculty advisor of the IEEE student branch at Cleveland State University.

All three cartoons are from “Piled Higher and Deeper” by Jorge Cham ©

APRIL/MAY 2005