How to Identify a Good Research Question?

A personal (although inherited) view

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Overview

- Finding a research question
- Finding a dissertation question
- Concluding remarks

Eighteen fundamental skills of a scientist

1. How does science work?
2. What is research?
3. Identifying good research problems
4. Writing papers
5. Presentation in front of an audience
6. Obtaining funding
7. Reviewing/refereeing the work of others
8. Teaching
9. Guiding students, running a lab, managing projects
10. Scientific creativity
11. Information finding
12. Career planning
13. Interacting with people and networking
14. Marketing your skills: job hunt
15. Balancing your life between work and family
16. Coping with stress
17. Ethics in science
18. Appreciation for quality rather than quantity

Identification of good research questions

- Identifying good questions is critical for success.
- Very often, especially in the beginning, senior colleagues (such as your mentor) will help you to identify and frame a research question.
- Ultimately, however, it is important that you learn the heuristics that are used in this process, so that you can function independently and be successful.

An ideal research question

The characteristics of an ideal research question

- Builds on your personal preferences.
  - Do you like working individually or in a team?
  - What kind of work do you like most (what "drives" you)? Is it theory, implementation, experimentation?
- Builds on your technical strengths.
- Matches your plans for after obtaining your Ph.D.

… but most important of all

- When you wake up in the morning, you feel like getting to work as soon as possible.

Finding a good research question

Scientific discoveries in the history of humanity involved different creative processes.

It seems that there is no one recipe for doing research, including identification of a research topic.

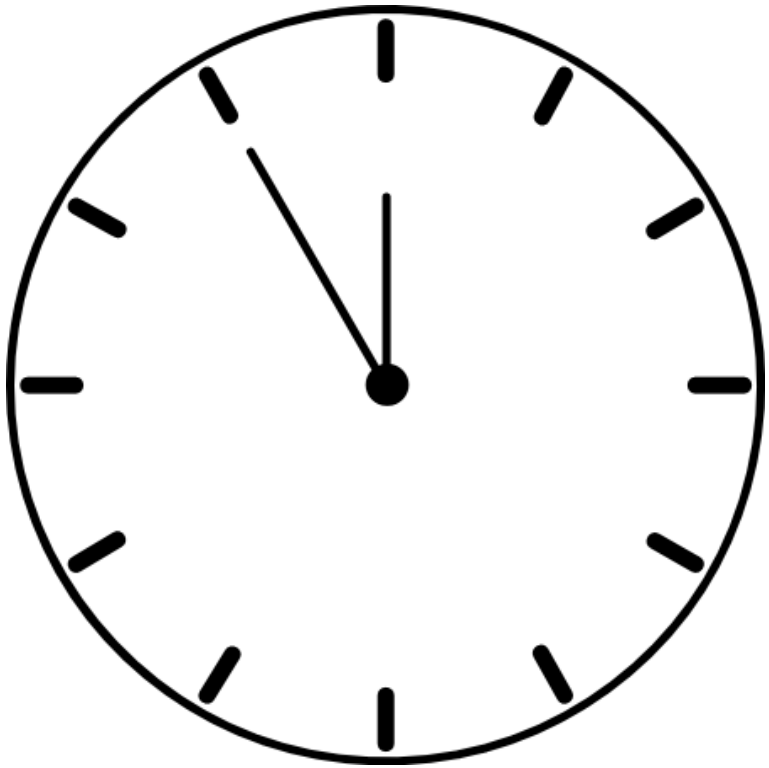
It is a hard problem, but some heuristics may prove useful

- **A research problem is a question (as opposed to a topic)**
- **The question does not need to be very specific ("What happens when … ?" is often fine in the beginning)**
- **Subject candidate questions to four basic tests** [Herb Simon]

Finding a good research question

Test 1: Is this question solvable within the given amount of time?

What will the answer look like (this determines how we start)?
Is this the right time to start with it?
Can I finish it in less than three to four years that it takes to write a dissertation?



Finding a good research question

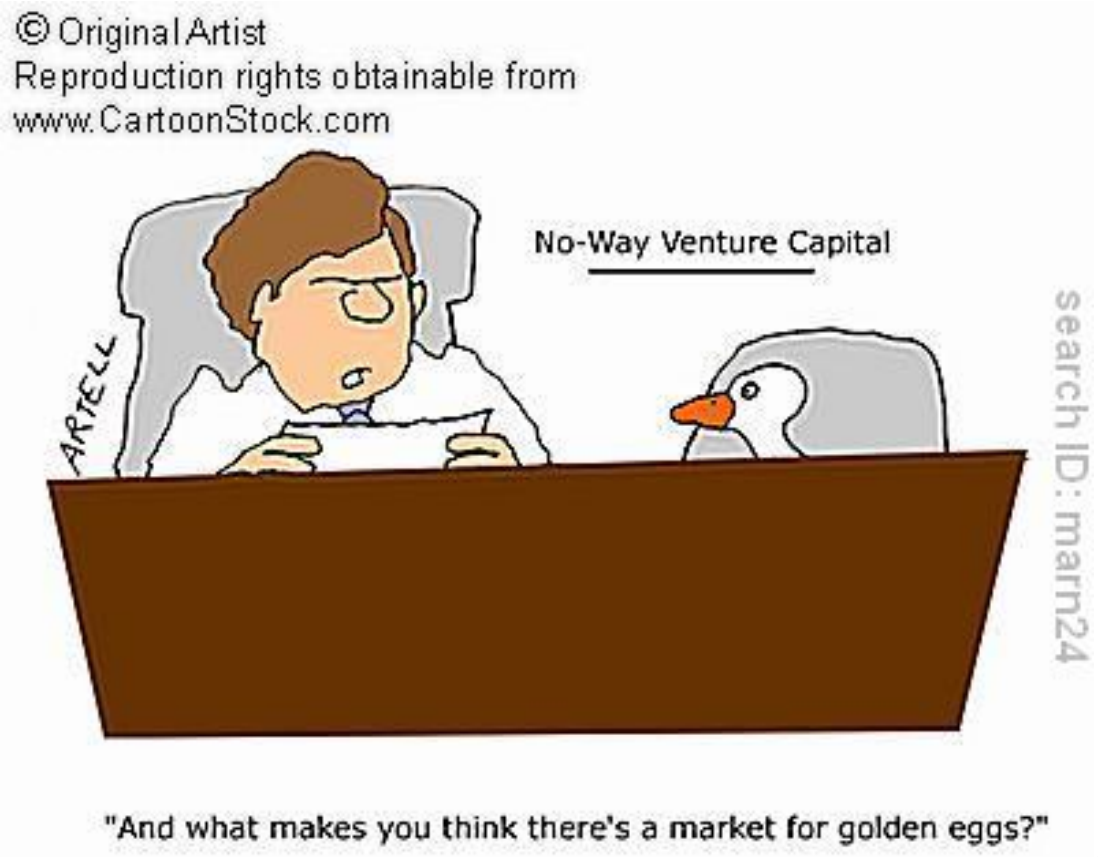
Test 2: Will anybody care about the answer?

Is there any utility in answer?

Will at least two people download your paper?

Sometimes we care about the answer even without utility (e.g., DNA structure, structure of the Universe).

Sometimes we care less, e.g., http://www.improbable.com/ig/



Finding a good research question

Test 3: Will I be the first to answer this question?

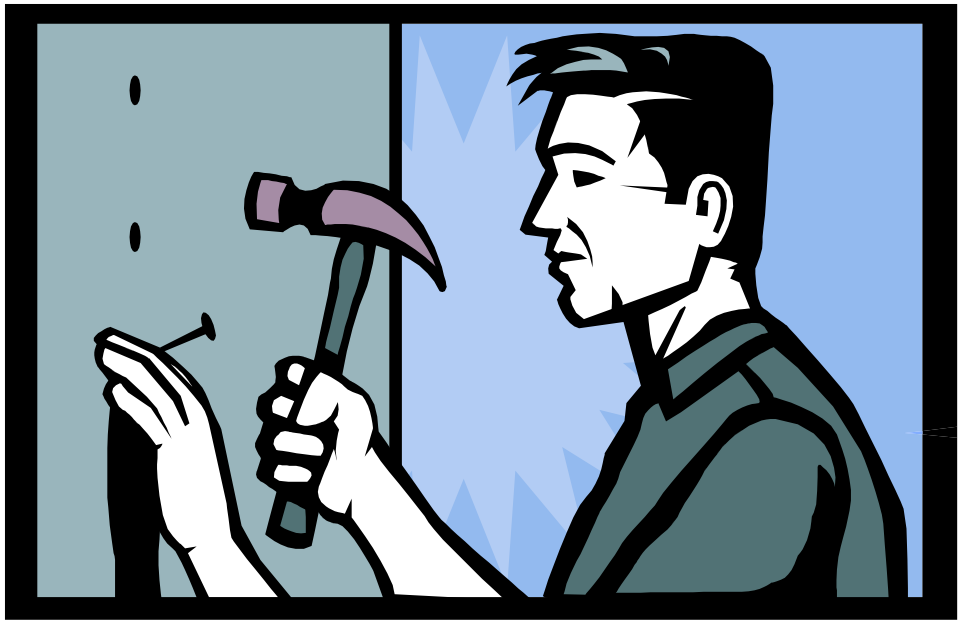
This includes the past, but we can insure ourselves by doing a good job on library search.

Auxiliary questions:

- Is this the right time to work on this problem (i.e., does the present state of science make the research feasible)?
- Are other people working on it now?

Finding a good research question

Test 4: Do I have good tools to address this question?



The concept of a "secret weapon"

What makes me think I will be successful?

"Because I'm smarter than others"

bad answer ... There are scores of smart people around.

You say you are in the 99th IQ percentile?

That's quite smart!

Only 55,000,000 people are smarter than you!

(Due to late Randy Pausch)

The concept of a "secret weapon"

What makes me think I will be successful?

"Because I work hard"

this is better ...

… but everybody who wants to succeed works hard.

The concept of a “secret weapon”

What makes me think I will be successful?

“Because I have a secret weapon”

Much better!

"Secret weapon"

What is a "secret weapon"?

A comparative, possibly unfair, advantage over your competitors to glory:

- **A good problem that nobody has thought about before.**

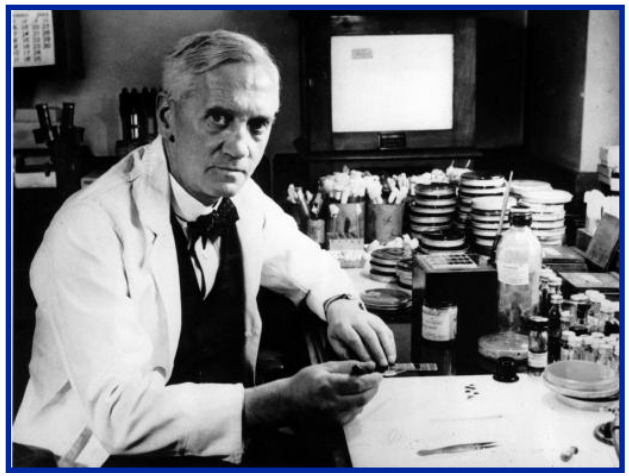
Maybe you are the first to think about this problem because of personal experiences?

Advice: Do not look for research ideas in journal articles!

Example of personal experiences: Fleming and the discovery of penicillin

Alexander Fleming (1881-1955)

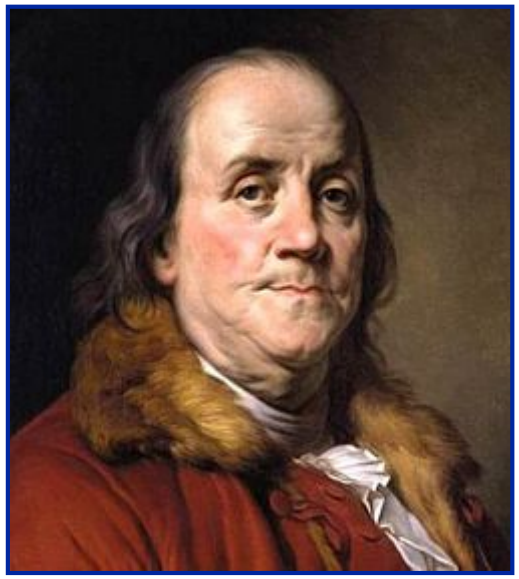
- Built a reputation of a brilliant researcher, although quite untidy.
- In 1928, after a month-long vacation, he returned to his lab to find quite a lot of "development" around his cultures.
- He examined what has grown around the cultures
- Fungus that formed on one of them seemed to kill bacteria around it.
- The rest is history (knighthood in 1944, Nobel 1945, his discovery has been called one of the most influential discoveries in the history of science).



Example of personal experiences: Fleming and the discovery of penicillin

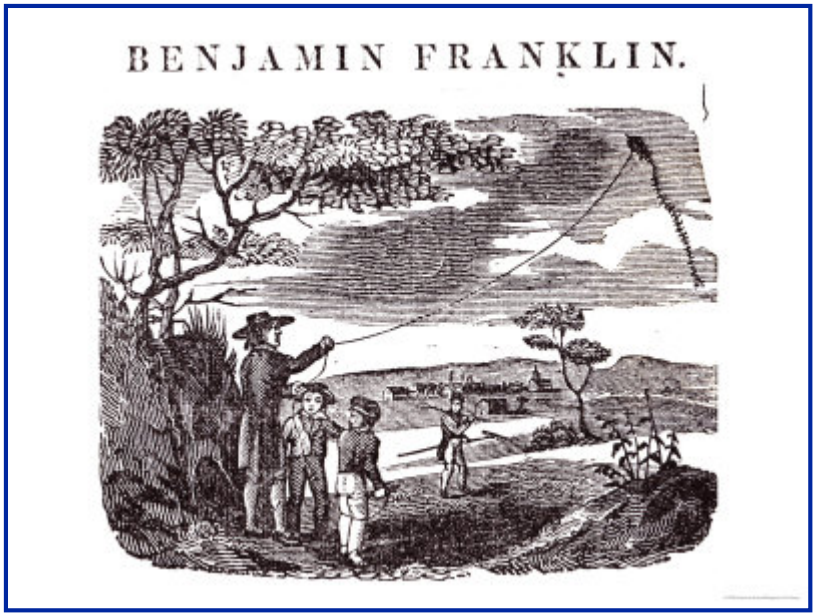
It was a nice idea but may not work for you ☺

Example of personal experiences: The kite experiment



Benjamin Franklin (1706-1790)

- In 1750 he published a proposal for an experiment to prove that lightning is electricity by flying a kite in a storm that appeared capable of becoming a lightning storm.

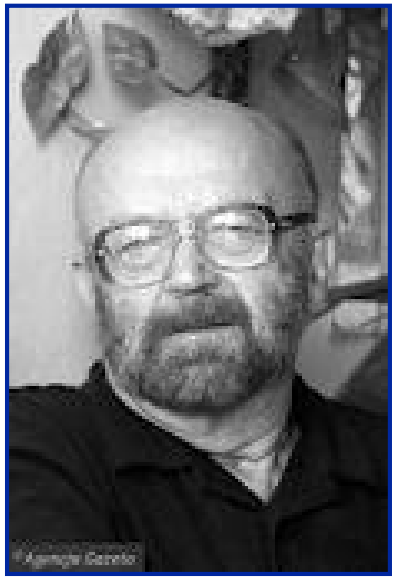


Example of personal experiences: Are people utility maximizers?

Herbert A. Simon (1916-2001)

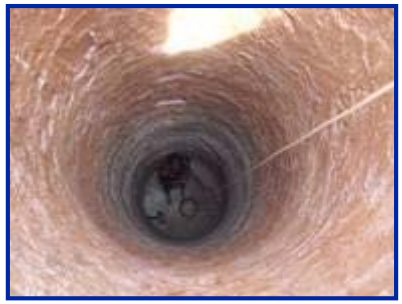
- During an internship at a local Milwaukee government he observed a serious discrepancy between economic theories (based on the assumption that humans maximize their utility) and reality.
- Developed the theory of bounded rationality: Humans do not optimize, they "satisfice."
- Once he saw a computer, he quickly realized its potential and adopted it as a "secret weapon."
- Pursued this idea through theory of organizations, economics (Nobel Prize in Economics, 1978), artificial intelligence (Turing Award, 1975; von Neumann Theory Prize, 1988), and psychology (National Medal of Science, 1986).

Example of personal experiences: Echo therapy



Bogdan Adamczyk (1930-2011)

- **Stuttered as a child**
- **Discovered that stuttering ceased when he talked to a well in his parents' garden**
- **As a physicist, focused on finding therapy for stuttering, created an original method of "echo therapy" based on providing patients their own voice with a small delay**
- **Very successful as a scientist**



“Secret weapon”

What is a “secret weapon”?

- **Special instruments, tools or a piece of knowledge that others don’t have.**

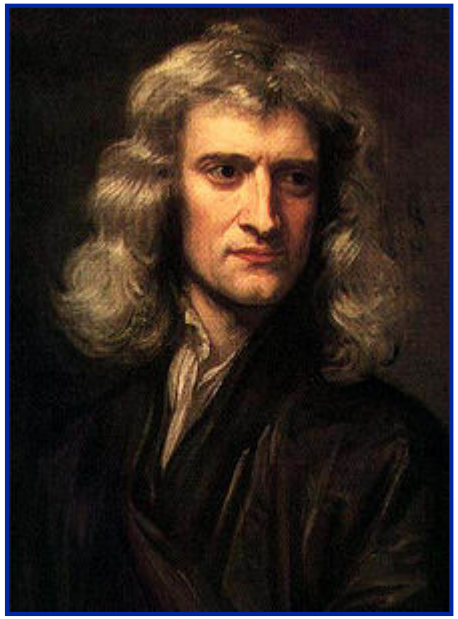
Is there anything that I have around me?

Powerful theoretical tools (e.g., Newton).

Biggest, most powerful machine of a certain type (e.g., a powerful accelerator, an excellent networking lab).

Software tools (e.g., GeNIe and SMILE☺).

Example of special instruments: Isaac Newton and the calculus



- Some historians accuse him of having invented calculus but then withholding from publishing it until forced by Leibnitz
- It is a fact that he was able to solve many outstanding problems and has been generally perceived to be a genius
- Offered knighthood in 1705 (second scientist knighted after Francis Bacon)

(Apparently the apple thing is a fable ☺)

"If I have seen further it is by standing on the shoulders of giants."

[Letter from Isaac Newton to Robert Hooke, 5 February 1676]

“Secret weapon”

What is a “secret weapon”?

Training in a discipline that other people are not trained in.

What is my background?

Can I combine my knowledge of music, my interests in biology, art, architecture, my concern about cancer or Alzheimer’s disease, etc., with my discipline?

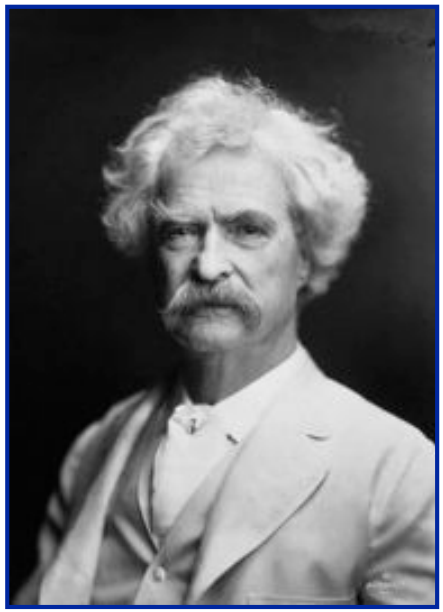
“Don’t put all your eggs in one basket”

- It’s a good idea to always have several good questions

  … and work on the top three
- This may not be a very good idea for doctoral dissertation

“Chance prefers a prepared mind”

“The man who does not read has no advantage over the man who cannot read.” [Mark Twain]



What made Fleming look at the dishes and notice something unusual?

Learn as much as you can and as diverse knowledge as you can!

Finding a dissertation problem

Similar to the problem of finding a research question, although you are not completely on your own here ...

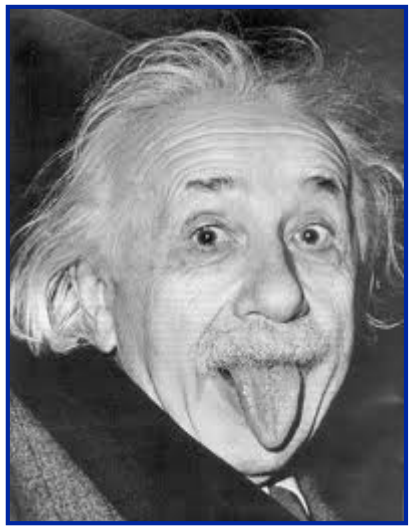
- **Listen to your advisor: He/she is (or should be) one of your secret weapons.**
- **What is (or is likely to be) funded? After all, you need to pay the rent and buy bread.**
- **What are the strengths of this group, department, school, university, city? Who are the people I can rely on (my other secret weapons).**
- **Remember that a doctoral dissertation (just like any scientific paper) is merely a progress report.**

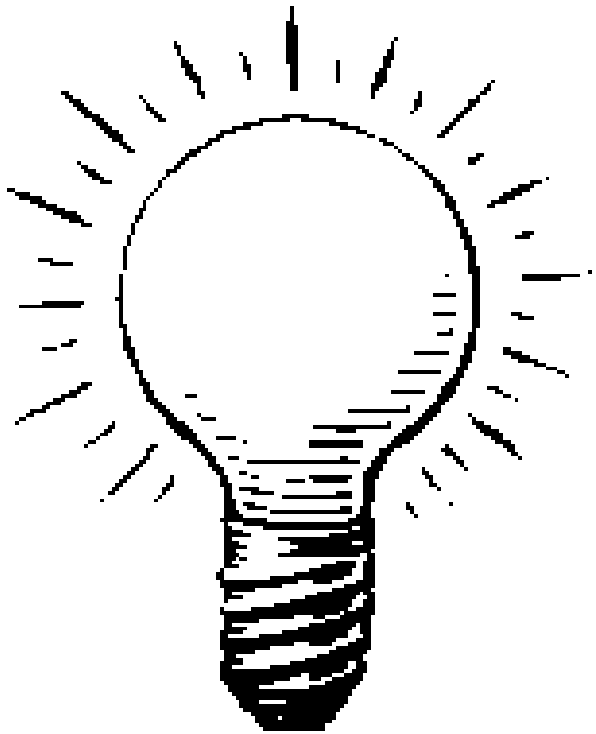
Finding a dissertation problem

- Play actively with the knowledge that you are gaining, lay links between facts, be critical and ask questions.
- Take a real problem, preferably with high payoffs, find a good solution, do something practical. Best research ideas come from real problems.
- Be careful who you are listening to.
- Think, think, and once again think. This is what you do best.
- Test market your ideas (spouse, friends, advisor, other faculty).

The importance of creativity

- Creativity is very important in finding research questions
- How do you become creative?





"Imagination is more important than knowledge" Albert Einstein (1879-1955)

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Concluding remarks

Your motivation and commitment

- In order to succeed, you will have to work hard (around 80 hours a week) for ten or so years.
- Think whether you really want to get into science. If so, make a commitment and follow it up.
- Be enthusiastic about what you are doing. This makes whatever you are doing fun and easy.
- If you cannot become enthusiastic, if you suffer from a permanent lack of motivation and nothing seems to work, you should probably consider another career.

Concluding remarks

There is no substitute to interacting with other people

- Talk to your advisor
- Talk to your colleagues (fellow doctoral students)
- Talk with people who are actively working in your area of interest
- Talk to professors in related research areas
- Talk to your friends and family about your work

Caution!!!

- Do not only talk – make sure that you also listen!!!

Concluding remarks

Work hard on your skills

- Learn to read quickly.
- Write and present your work.
- Find a mentor, work with him/her, listen to him/her. He/she is one of your best resources.
- Get involved in research as soon as possible. Search for your secret weapons.
- Do things for real – you will be doing them for real in a few years from now (if you are not already).

Concluding remarks

Simple and easy tricks that help you succeed

- Have friends, balance them out, have anthropologist friend, talk with them, have lunches together. Herb Simon would ask them the question "What's new in your field?" (if you notice that you have some unique insight on the questions in the field, you have an opportunity!).
- Do not treat boundaries between disciplines too seriously: they are artificial anyway.
- Structure your time, plan your career, studies, semester, month, week, day. Set aside time for planning.
- Have a diary of what you did on a given day, write in your research ideas. (This is a good method to cut out game playing, web browsing, newsgroups, etc.)

Concluding remarks

Simple and easy tricks that help you succeed

- Do not automatically suspect that not understanding something is your fault. Ask questions, don't afraid to look silly. (Remember "The king is naked?" fable?)
- Think and do not follow the crowd.
- Be honest and not paranoid about your research ideas (or else perhaps they are not your friends?).

Concluding remarks

Simple tricks inspired by cognitive psychology

- Active learning: Play with the newly acquired knowledge, create links to knowledge that you already have.
- Expose yourself to ideas: This influences creativity.
- Set aside large chunks of time for thinking things through (you cannot do without it!).
- Develop in yourself a "lifetime learning" mentality.

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Next week

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