How to do Great Research

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How to do bad research

Focus on a single narrowly-defined problem, it'll be easy to publish. Or maybe focus on everything, hedge your bets.

Focus on problems as they occur to you; philosophizing won't get you papers.

Don't waste time networking and chatting; that's time you can spend running experiments. Don't advertise your work; it should speak for itself. Did Einstein attend conferences and volunteer for presentations?

Don't waste time cataloguing your ideas and your results; your brain will suffice, and it also doesn't require additional wasted time.

Don't share until it is perfect.

Assume ideal settings; real problems and data require spending time to look and think about them. Besides, if you cannot model them analytically and mathematically, it is an art, not a science.

Use deadlines to motivate you; excellence is an act, not a habit.

And success is lucky anyway.

Or maybe…

You would like to do something significant.

You believe in yourself and want to achieve something great.

But success doesn't have to come naturally. Successful habits could take effort and time to

1. realize you need to change instinctual behaviors,
2. design appropriate systems to change your routines, and
3. implement them

Problem

Everyone thinks their problem is important. But they mistake their affection or excitement for a problem for its importance. Or don't care about the importance.

Solving a problem no one wants solved is *at best* a waste of time; *at worst*, it diverts resources to a problem the world doesn't need to solve.

That is not to say that you shouldn't feel excited about your problems.

How can you tell which problems are worth solving? No free cookie cutters.

Network

You can learn about important problems *by talking to other people*

Or you can refine your problem *by talking to other people*

Or you can solve bigger problems *by collaborating with other people*

Richard Hamming: "*But i can say there is a pretty good correlation between those who work with doors open and those who ultimately do important things, although people who work with doors closed often work harder. Somehow they seem to work on slightly the wrong thing – not much, but enough that they miss fame.*"

Writing

Manuel Blum: Turing machines (TM) can compute any computable function. Finite automata (FA) cannot do multiplication. TMs are incredibly more powerful than FAs. Yet the only difference between them is that TMs have pen and paper.

Or a current analogy: LLMs w/ and w/o tools.

Richard Feynman: Writing is not the record of your work or thinking, it is part of it.

Write down your ideas; start writing your papers early; analyze and compress your results; if you don't know how to spell your ideas out, or how to make a figure to convey your results, then you don't know what you're doing.

Deep Thinking

Take a pause. Think and write your ideas down. Discuss, present, and philosophize. You are less productive in the short-term. You will be more productive long term.

You are doing a PhD? Great! You are already "gambling" on a better long-term future by sacrificing your present. Do it properly!

Thinking Outside the Box

Knowing the literature and talking to others is important to know i) what is important, ii) if anyone has already done what you are working on.

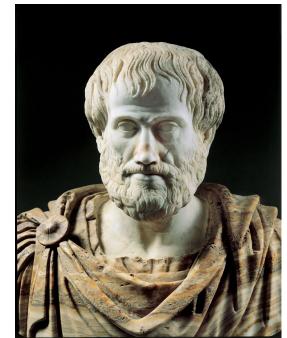But too much literature and talking will trap you in the box

Break the patterns, enjoy the ambiguity: this again takes effort, it may not feel natural to you

Excellence is not an act but a habit

Discard the Hollywood myth of the overnight prodigy.

Excellence takes work and dedication. That is usually hidden from public view. You don't see LeBron training, you see him performing. You do not see Marie Curie studying at the lab, you see her Nobel prize.

Aristotle - © A. Dagli Orti—De Agostini/Getty Images

Luck

favors the prepared mind