Welcome to Fractal University!

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Website:

https://fractaluniversity.substack.com/

Follow Us:

Twitter: @FractalUniv
Instagram: @fractal_university

See you in class!

Etiquette

1. **Take yourself and others seriously.**

We expect you to **engage with challenging ideas**.

We expect you to prepare for each class by reading and developing your perspectives on the content. **We expect you to attend.**

Throughout school, we expect you to put yourself in situations which can feel uncomfortable or cringe. For example: talking about your low confidence hunches, actively pursuing collaborators, ending an unproductive research partnership, or stepping outside the bounds of what most people might expect of you.

You should seek to provide **an environment where others feel able and willing to take these risks**, if only to make it easier for yourself to do the same. Be careful about dismissing people or their ideas. Disagreeableness should be welcome, but used thoughtfully.

2. **Ask concrete questions, give concrete answers. No bullshitting.**

“What is the answer to question 4” is perhaps the most uninteresting question ever asked. Who cares about question 4?? Nobody will ever ask you about question 4 ever again, why do you care? Take your questions more seriously than this.

Your questions matter, they reflect your thinking. Even exasperated, frustrated remarks are better than bullshit.

“I just don’t understand what a MULTIPLEXER is!!!?!”

Now THIS is a real question. You can dig your teeth into this. What is a multiplexer, indeed? Well let’s break it down concretely...

We’re all here to learn together -- we respect each other’s learning by being rigorous in our ideas, explanations, and confusions.

You must strive to deeply understand the reality that underpins theory (and your ideas of theory). It is high status to admit that you don’t know something here. The more you do it, the more people will like you :)

3. **Collaborate in public. Have a good time.**

Your scholarship will be enhanced by the ideas of others. This should be obvious.

But the inverse is also true: your peers’ scholarship will be enhanced by ***your feedback.***

Be the public collaborator you wish would appear in *your comment sections*.

Read your classmates’ writing and challenge their ideas so they will read yours. Publish your questions, even if they are naive. Publish your unique feelings, even if you think they aren’t novel. Publish your best models, even if they’re unrefined. Reward people for correcting you and improving your thinking.

Fall Program

CS: Foundations of Computing: From NAND to Tetris
Instructor: Andrew Rose, ajroberts0417@gmail.com
Google Pier 57: 29 11th Ave PIER 57, New York, NY 10011

FR: Building Community With Your Friends
Instructor: Priya Rose, priyalghose@gmail.com
McKibbin Lofts: 248 McKibbin St Apt #1G, Brooklyn, NY 11206

AI/ML: Hugging Face NLP
Instructor: Chris Kroenke, christopher.kroenke@gmail.com
McKibbin Lofts: 248 McKibbin St Apt #1G, Brooklyn, NY 11206

Body, Mind, World: A Joint Investigation
Instructors: Alicia Botero, aliciaboteror@gmail.com,
Tyler Alterman, tyleralterman@gmail.com
McKibbin Lofts: 248 McKibbin St Apt #1G, Brooklyn, NY 11206

Spring Program

Potential Spring '23 Courses
AI/ML: Neural Networks: Zero to Hero
PHY: Intro to Quantum Physics
PHY: Astrophysics-Cosmology
ECON: Intro to Microeconomics
BIO: Intro to Bioinformatics and Computational Biology
MAT: Real Analysis
ARC: Architectural Construction
WRI: Writing Science Fiction and Fantasy
TH: Improv
HIS: Revolutions
LIT: Comparative Literature
ANTH: Cultural Anthropology
LING: Introduction to Linguistics

Potential Spring '23 Labs:
IR: Independent Research
EN: Idea Lab
FI: Film Studies
GAM: Game Development and Design Lab

Spring semester is still under construction! If you're interested in teaching or taking any of these courses, or if you have one in mind (either your own or a MOOC), please fill out this interest form:

Fractal University Student Life Overview

Events (dates subject to change)

Social ~ September 29, 2023   **Halloween Social** ~ October 27, 2023   **Graduation Expo** ~December 9, 2023

Clubs

Current Clubs at Fractal:

- **Progress Studies Lab:** this group embarks on a multidisciplinary journey spanning history, economics, science, technology, and culture. Members engage in discussions, research projects, and collaborative efforts aimed at dissecting the forces that drive societies forward. This lab will focus on the following 3 Qs: 1) how did we get here, 2) why did it take so long?, and 3) how can we see more progress?
- **NYC Citizens Tech Committee**: is a passionate community of civic technologists dedicated to crafting and researching public tools for New Yorkers.

Progress Studies Lab

NYC Citizens
Tech Committee

Starting Your Own Club? Here's How:

1. **Identify Your Focus:** First off, what's the central theme or topic? Choose something you're genuinely interested in.
2. **Assemble Your Team:** Next, find other FractalU students who share your enthusiasm. A club is as strong as its members, after all.
3. **Draft a Club Plan:** Got your team? Great. Now jot down your objectives and planned activities. Make it clear but detailed.
4. **Reach Out to FractalU Admin:** Send us an email to discuss your club idea (contact info provided below). We're open to hearing what you've got.
5. **Announce Your Club:** Once approved, make some noise. Post an announcement in the Discord general channel and create a channel specifically for your club. Then get started on your activities!

We're eager to see how you contribute to the FractalU community :)

Contact: ajroberts0417@gmail.com and madhu2423@gmail.com

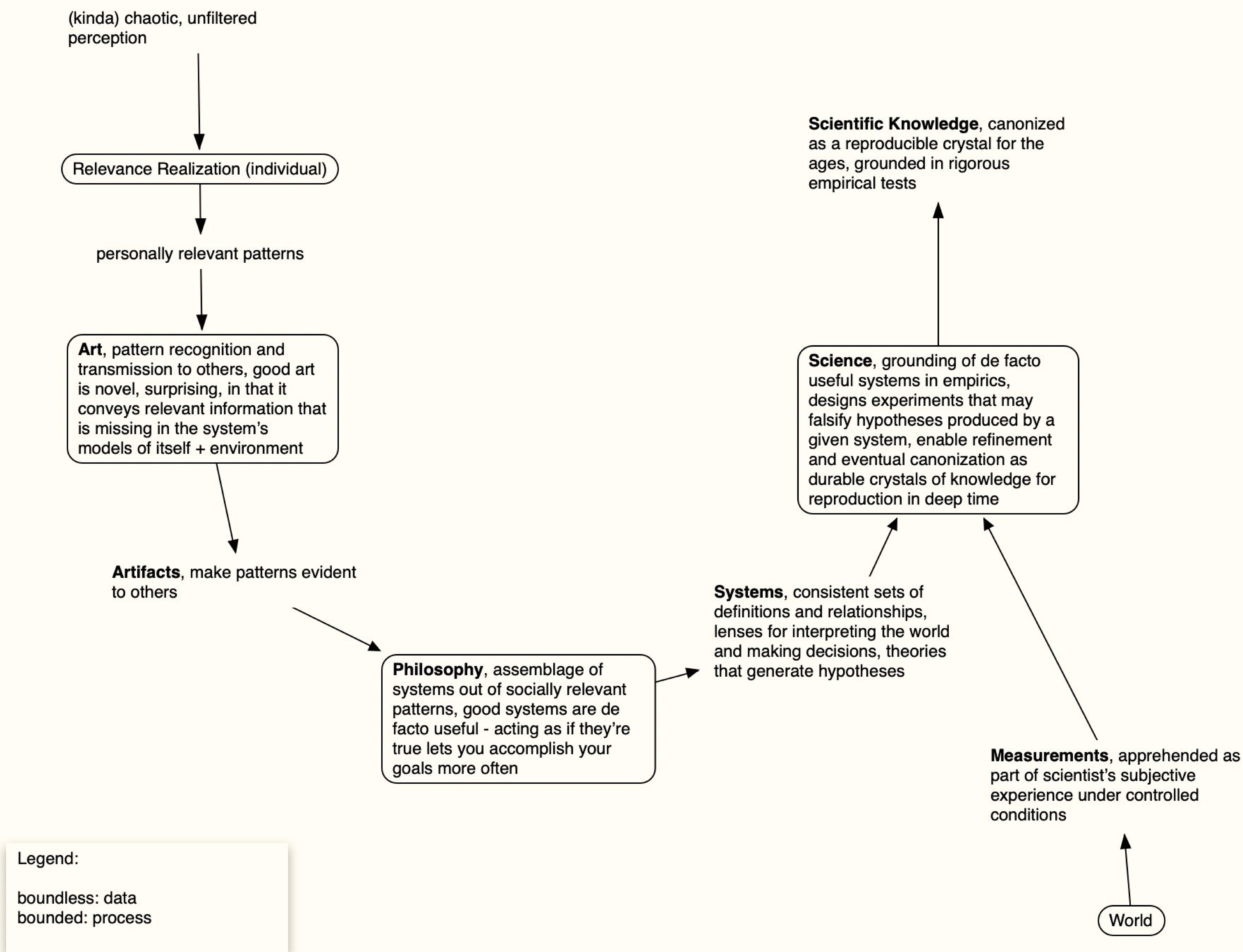
Fractal University Canon

A love letter to our values

dangerous ideas within

The Love of Science

ART IS PRE-PARADIGMATIC PHILOSOPHY IS PRE-PARADIGMATIC SCIENCE



An invitation to a secret society

ADAM MASTROIANNI || JUL 20, 2023

I hereby invite every curious human to do science and post it on the internet.
Ask questions, collect data, write stuff, and make it available to everyone. You should feel as free to do and share research as you would feel uploading a video to YouTube or a song to Spotify.

You don't actually need my or anyone else's permission to do this, but sometimes people need a little encouragement, so: come on in!

Actually, let me make that a little more urgent: *Please come in, we need you*.

See, scientific progress has slowed. We fund more research than ever and get way less bang for our buck. We spend 15,000 years of collective effort every year on a peer review system that doesn't do its job. Fraudsters can publish dozens of papers before they get caught, if they get caught at all.

This is bad. Our world is full of problems, and science is the main way we solve them. We've got climate change, an obesity epidemic, and a lot of sad people. There are folks dying of poverty and preventable disease. Heck, we still mainly make electricity by burning dinosaur bones. This can't be as good as it gets.

Lots of people have lots of ideas about how to get science started again. Give out badges for good behavior! Do giant replication studies! Demand tinier p-values!

I don't think any of these solutions will work because they're trying to solve the wrong problem. They're aimed at stamping out the worst research, but science is a strong link problem: we make progress by producing good stuff, not by preventing bad stuff. When you're in a strong-link problem, the answer is to turn up the weirdness. More wild hypotheses! More risky research! The useless ideas will die from disuse, but the useful ideas will live on.
This is where you come in.

LET'S ALL BE MAMMALS AND DIE

Professional science does a lot of good stuff. It gives people paychecks, health insurance, research funding, offices, and colleagues. It allows large groups to work together on big projects like launching telescopes into space. And it gives young, curious people a place to start: if you want to ask and answer questions about the universe, academia is an obvious career path.

But that good stuff comes at a price. Professions are bundles of weak-linkinterventions; they keep out quacks, but they also keep out revolutionaries. They enforce standards, which tends to make things…standard. They select for a pretty homogenous group of people—in this case, folks who got good grades in college, did research in the right institutions with the right people and published in the right journals. Then they make all those people even more similar to one another, steeping them in the same culture and putting them in competition for the same rewards, like grants, jobs, and citations.

Right now, professional science is like a world where every organism is trying to be a mammal. Mammals are great: milk-producing glands, body hair, ears that have three bones in them, what's not to like? But if you've *only* got mammals, you're in big trouble. Monocultures are fragile and prone to collapse because every single organism has identical weaknesses. What you need is an ecosystem—hawks, sea urchins, fungi, various types of fern, and so on.

Creating diverse ecosystems is hard for humans because they like to do whatever everyone else is doing, even when they know it's wrong. So when you're trying to be a mammal and you see someone else trying to be a lizard, you might think they're just doing a bad job being a mammal. "You should try having little hairs all over your body," you might tell them. But a lizard isn't a bad mammal. It's a lizard. Its job is to eat flies and bask on rocks.
What I'm saying is: *be the lizard*. The mammals—that is, mainstream scientists, the ones who get PhDs and professor jobs—have their niche covered. What we need is more people doing botany in their backyards. We need basement chemists. We need amateur geologists and meteorologists.

Heck, if some mammals want to try a different niche, so much the better: ditch the projects you think are pointless, do the thing you think is most important, write it in your own words, and put it on the internet. There's plenty of space for everyone.

“HELP, I AM WORRIED THAT MOST OF THE STUFF THESE PEOPLE MAKE WILL BE BAD!”

It will be! But remember two things.

1. Most of the professional science we produce right now is bad. Seriously, pick a paper at random and see how well it holds up. We recently found out that the chair of a Harvard department probably faked a bunch of her data. The bar is not high, folks!
2. Bad stuff is okay. I cannot stress enough that science is a strong-link problem, and what we really care about is how much *good* stuff we get, even if it means we also get a bunch of bad stuff.

THE DEATH OF THE DABBLERS

What I’m proposing here isn’t actually new. In fact, it’s ancient. People have been making knowledge in all sorts of ways ever since Socrates started walking around and saying stuff.

Some of these people didn’t have traditional jobs because they didn’t have to. Francis Galton, the guy who came up with (among many other things), correlation, twin studies, fingerprinting, weather maps, and questionnaires, was independently wealthy. So were Darwin and Boyle and many of the members of the original Royal Society andBritish Association. As I wrote in my review of Galton’s autobiography, dabbling in science used to be a common pastime for rich dudes—a little astronomy here, a little vivisection there.

Money makes everything easier, obviously, but plenty of now-legendary scientists had to do their thing part-time while finding other ways to pay the bills. Einstein published some of his most important work while he was still a patent clerk. Thomas Bayes, whose theory of probability still sets nerd hearts aflutter, was a priest. Gregor Mendel, the pea plant guy who founded genetics, was a monk. Galileo and Da Vincispent much of their careers trying to worm their way into the good graces of various patrons. Marie Curie did her early work in a shed next to a French university; they only gave her a job after she got famous.

Antonie van Leeuwenhoek sold drapes to support his side hustle of inventing microbiology.

I predict that these last few decades, in which professional science nearly eradicated the dabbler and the part-timer, will turn out to be a blip in history, for two reasons. First, the structures of academia are so warped, competition is so fierce, and opportunities are so scarce that even its biggest adherents spend most of their time complaining about it. When numerous Nobel Laureates are saying that they couldn’t have done their Nobel Prize-winning work in today’s system, something’s bound to break.

And second, for the first time in human history, the tools of science are cheap, and knowledge is nearly free. Your laptop can store and analyze more data than Galileo could have even imagined. Internet pirates have toppled the scientific paywall and made nearly every paper ever written freely accessible to everybody. Nobody can stop you from uploading a PDF of your research to the internet, where tens of thousands of people might see it. The only thing stopping you from jumping in is your own fear.

MY PROMISE TO THE LIZARDS

If you're interested in accepting this invitation, here are four ways I can help.

#1: If you email me for help on a scientific project, I will respond.

I might not respond right away—I'm pretty slow at email—but I will get back to you eventually. I will take your ideas seriously and talk to you like a colleague, because we are all colleagues in the great project of understanding the universe. That also means if your ideas seem crazy or your methods seem flawed, I'll tell you. I can't keep up every email chain indefinitely, but I'll reply to everyone at least once, or until I simply can't keep up.

Of course, there are lots of subjects that I know nothing about, which brings me to:

#2: I'll connect you to other people who have similar interests.

I set up a Discord server for folks doing science, and I'll add anyone who reaches out and explains what they're looking to do. Once we reach a critical mass, this could be a place where people go for advice, to discuss ideas, and find collaborators.

I'll check in occasionally, but I expect to be pretty hands-off. I'm intending this to be a place where people talk to each other, not to me.

#3: If I see you doing good stuff, I'll link to it.

Self-explanatory. And finally:

#4: I'll answer a few questions to get you started.

Subscribe

FREQUENTLY ASKED QUESTIONS ABOUT DISCOVERING FUNDAMENTAL TRUTHS ABOUT THE UNIVERSE

Where do I start?

Probably with something that seems weird to you. Something that annoys you because you don't understand it. Some parts of research can be boring; wanting to know the answer really bad will help keep you going. Also, read The Scientific Virtues.

What if I don’t have any formal training or credentials?

Formal scientific training is way less formal than you think.

You might imagine that when you enter a PhD program, a wise old scientist sits you down and tells you all the secrets of science. This doesn't happen. You take a few classes, most of them totally irrelevant to the research you end up doing. When you have a question about statistics, you go to StackOverflow. Most of the papers you read are the ones you find for yourself, probably using Sci-Hub because it's easier and faster than accessing papers legally through your university.

I was lucky enough to have a terrific advisor who taught me a ton, but that's pretty rare—plenty of people spend years of their PhD just waiting for their boss to respond to an email. So if your scientific education is mostly DIY, well, so is everyone’s.

Haven’t all the easy ideas been taken?

No, and if you say that again I will fight you. If you start with something you don’t understand, there’s a good chance that soon enough you’ll bump up against something that no one understands.

How can I do anything useful when I don’t have any resources?

You can do a lot of interesting stuff with cheap, simple methods. Pop culture has become an oligopoly was just "copy data off the internet and make some graphs." Do conversations end when people want them to? was just "have people talk and then ask them when they wanted to stop." Slime Mold Time Mold's Potato Trial and Half-Tato Trial were just "get people on the internet to eat potatoes and weigh themselves." Rita

Levi-Montalcini discovered how the nervous system develops by poking chicken eggs with a needle in her bedroom/laboratory.

In fact, if you're willing to use simple methods, you actually have an advantage over professional scientists. The pros wanna look cool to their colleagues (and win big grant money from the government), so they have to use the fanciest, most advanced techniques, even when simpler stuff would do them better. That's great for you, because it means the professionals will rarely investigate important questions if they don't require giant magnets or ten thousand computer cores or whatever. Cheap ideas are just lying around for you to scoop up. So scoop ‘em, darn it!

What if I do a bad job?

If you work on a project that goes nowhere, who cares? Move on to the next one. Don't worry about making mistakes—there is a 100% chance you will make a mistake, so when it happens, go "oops" and fix it. Be honest and transparent. The stakes are way lower than they seem.

What if no one listens to me?

That might happen! It's happened to lots of people who turned out to be right, like the guy who told doctors they should wash their hands, and the guy who hypothesized that all the Earth's landmasses used to be one big Pangea.

In my experience, though, the internet is smaller than you imagine, and good work tends to travel. But you should do this because you think it's important and you like doing it. If you're doing it because you want influence and affirmation, reconsider!

What if people yell at me?

They might. Whenever you post something publicly, there's a chance people will be mean to you, because some people think that being nasty makes them look smart. Unfortunately, the only solution is to ignore them.

Is it legal to do science on your own?

Don't laugh—I've had professional scientists ask me this!

In short: yes. You don't need a license to do science. But you still have to obey the law. You can't pretend to be a doctor, steal people's data, secretly lace someone's lunch with chemicals and watch their reaction, etc. You shouldn't do anything illegal or immoral in pursuit of scientific truth, just like you shouldn't do it in pursuit of anything else.

How do I know whether someone has tested my idea already?

Career scientists don't get any credit for redos, so they worry a lot about making sure no one has scooped them. But it's actually good for science if people are replicating previous work, so long as the idea was a useful one in the first place. Many studies are bad, some of them are straight-up fraudulent, and it's pretty likely that you'll ask the question in a different way than your predecessors did. Plus, replicating previous work is good practice. So if you really want to get to the bottom of something, just go for it.

What can I do that a professional scientist can't?

Oh man, *tons*. Here are just a few things.

- Screw around on projects that might be a total waste of time, just for fun.
- Write a paper that's like "Hey here's a weird thing I found and I have no idea why it happens"
- Research stuff that's bizarre or unpopular or disconnected from any existing literature.
- Write a paper that's like "Hey my hypothesis was totally wrong, what's up with that"
- Work on super long-term projects that only bear fruit after decades of work

Do you have any helpful examples of people doing science outside of professional institutions?

Yes!

Aella is a sex worker and a scientist. She runs big surveys about people's sexual behaviors and tries to learn from them.

Experimental Fat Loss is a pseudonymous blogger who has run lots of self-experiments on weight loss.

Julian Gough is unspooling a theory of the universe on his Substack, which involves making detailed predictions for what the International Pulsar Timing Array will detect.

Slime Mold Time Mold are mad scientists who are, among other things, trying to solve the obesity epidemic. They also have a terrific series on how to run one-person or few-person studies, which could be a good place to start.

This sounds cool, but I don't know if I really want to do science. Are there other ways I could help?

Science needs programmers, project managers, grant writers, editors, research assistants, funders, and a million other things besides. If you want to be involved, get aboard! The Discord will be a good place to start.

HOW TO GET KARATE CHOPPED BY ME

If you call what I'm describing here "citizen science," I will karate chop you. I *despise* that phrase. All science is science, regardless of the author's credentials. Slapping the label "citizen" on science done by people working outside of institutions is just a way of widening the moat around the ivory tower, of reinforcing the false idea that only people with PhDs and academic jobs get to do "real" science.

You can have impeccable academic credentials, land a fancy job at an elite university, and publish hundreds of papers, all without ever putting a useful piece of knowledge into the world. Many people pull this off! Sometimes they do it by writing papers that are true but meaningless. Other times they do it by opening up an Excel spreadsheet and typing in some fake data. These people are not scientists, no matter what it says on their office doors.

So I don’t care if you’re a nobody from nowhere. I don’t care if you pay your bills by cleaning toilets, selling Beanie Babies on eBay, or managing an Olive Garden. If you discover some useful nugget of truth about the universe, you’re a scientist.

THE BEAUTIFUL WAY

One more thing: I believe that anything that people make on their own, anything they create for pure pleasure, is beautiful.
People will sit alone in their basements playing guitar simply because they like the sound. They’ll paint, write poems, and whittle wood into little figurines without any expectation of gaining money or fame. It just makes 'em feel good. All of that is beautiful.

Anything that humans only produce in exchange for money, on the other hand, is ugly. No one designs billboards or writes instruction manuals for microwaves on a lark. When people pick up a guitar of their own accord, they sing about love and longing, not about how Tide laundry detergent cleans even the toughest stains. That doesn’t mean these endeavors are *bad*—someone’s gotta tell you how to work your microwave—but it means they aren’t beautiful.

Right now, almost no one sits down and writes a scientific paper for pure pleasure. I talk to people all the time who signed up for academia thinking they were going to uncover the mysteries of the universe, and they ended up doing something that *kind of* looks like that, but isn't really, and somehow feels pretty bad a lot of the time. They say things like, "I usually come to hate my papers by the time I get them published."

That doesn't mean that science is inherently ugly. It means we aren't doing it the beautiful way. When you do science under duress, you produce something that looks a lot more like a Tide commercial than a love song. It's still possible to make something useful that way, but it's very hard to make something beautiful.

You, though, can do things the beautiful way. You can make knowledge the same way you would make music in your basement: just because you like doing it. I hope you will. I'll be waiting to hear from you.

The Scientific Virtues

february 10, 2022february 5, 2023 • slimemoldtimemold • culture, mad science, science, virtues

Science education usually starts with teaching students different tools and techniques, methods for conducting research.

This is wrong. Science education should begin with the scientific virtues.

Teaching someone painting techniques without teaching them composition will lead to lifeless paintings. Giving business advice to someone who lacks civic duty will lead to parasitic companies. Teaching generals strategy without teaching them honor gets you warlords. So teaching someone the methods of science without teaching them the virtues will lead to dull, pointless projects. Virtue is the key to happy, creative, important, meaningful research.

The scientific virtues are:

- Stupidity
- Arrogance
- Laziness
- Carefreeness
- Beauty
- Rebellion
- Humor

These virtues are often the opposite of the popular image of what a scientist should look like. People think scientists should be intelligent. But while it's helpful to be clever, it's more important to be stupid. People think scientists are authority figures. Really, scientists have to defy authority — the best scientists are one step (or sometimes zero steps (https://en.wikipedia.org/wiki/Against_Method)) away from being anarchists. People think scientists are arrogant, and this is true, but we worry that scientists are not arrogant *enough*.

Anyone who practices these virtues is a scientist, even if they work night shifts at the 7-11 and learned everything they know about statistics from twitter. Anyone who betrays these virtues is no scientist at all, even if they've got tenure at Princeton and have a list of publications long enough to run from Cambridge to New Haven.

Cultivating virtue is the most important way to become a better scientist. Many people want to be scientists but are worried that they are not smart enough, or not talented enough. It's true that there is not much you can do to become smarter, and you are mostly stuck with the talents you were born with. But virtues can be cultivated infinitely — there is no limit to how good you can get at practicing them. Anyone can become a better scientist by practicing these virtues — maybe even a great scientist.

Stupidity

> *The great obstacle to discovering the shape of the earth, the continents, and the oceans was not ignorance, but the illusion of knowledge.*
>
> ***— Daniel Boorstin (https://en.wikipedia.org/wiki/Daniel_J._Boorstin)***

To a large extent, your skill as a researcher comes down to how well you understand how dumb you are, which is always "very". Once you realize how stupid you are, you can start to make progress.

A different writer might say “humility” here rather than stupidity. But calling this virtue humility might make you feel smug and self-satisfied, which is not the right feeling at all. Instead, you should feel dumb. The virtue of stupidity is all about feeling like a tiny mote in a vast universe that you don’t understand even a little bit, and calling it humility doesn’t strike that note.

Great scientists are not especially humble, as we shall see in just a minute. But they are stupid — they are practiced in practicing ignorance. They have cultivated the virtue of saying and doing things that are just entirely boneheaded, because this is vital to the process of discovery, and more important, it is relaxing and fun.

> *It seems necessary to me, then, that all people at a session be willing to sound foolish and listen to others sound foolish.*
>
> ***— Isaac Asimov (https://en.wikipedia.org/wiki/Isaac_Asimov)***

Stupidity is all about preparing you to admit when you’re facing a problem where you don’t know what is going on, which is always. This allows you to ask incredibly dumb questions at any time.

People who don’t have experience asking stupid questions don’t understand how important they can be. Try asking more and dumber questions — lean in on how stupid you are. You will find the world opening up to you. Ignorant questions are revealing!

> *I took mechanical drawing when I was in school, but I am not good at reading blueprints. So they unroll the stack of blueprints and start to explain it to me, thinking I am a genius. …*
>
> *I’m completely dazed. Worse, I don’t know what the symbols on the blueprint mean! There is some kind of a thing that at first I think is a window. It’s a square with a little cross in the middle, all over the damn place. I think it’s a window, but no, it can’t be a window, because it isn’t always at the edge. I want to ask them what it is.*
>
> *You must have been in a situation like this when you didn’t ask them right away. Right away it would have been OK. But now they’ve been talking a little bit too long. You hesitated too long. If you ask them now they’ll say, “What are you wasting my time all this time for?”*
>
> *What am I going to do? I get an idea. Maybe it’s a valve. I take my finger and I put it down on one of the mysterious little crosses in the middle of one of the blueprints on page three, and I say, “What happens if this valve gets stuck?” — figuring they’re going to say, “That’s not a valve, sir, that’s a window.”*
>
> *So one looks at the other and says, “Well, if that valve gets stuck –” and he goes up and down on the blueprint, up and down, the other guy goes up and down, back and forth, back and forth, and they both look at each other. They turn around to me and they open their mouths like astonished fish and say, “You’re absolutely right, sir.”*
>
> *So they rolled up the blueprints and away they went and we walked out. And Mr. Zumwalt, who had been following me all the way through, said, “You’re a genius. I got the idea you were a genius when you went through the plant once and you could tell them about evaporator C-21 in building 90-207 the next morning,” he says, “but what you have just done is so fantastic I want to know how, how do you do that?”*
>
> *I told him you try to find out whether it’s a valve or not.*
>
> ***— Richard Feynman (https://en.wikipedia.org/wiki/Richard_Feynman)***

Asking dumb questions was a particular favorite of Richard Feynman (https://en.wikipedia.org/wiki/Richard_Feynman), who really cannot recommend it strongly enough:

> *That was for me: I can’t understand anything in general unless I’m carrying along in my mind a specific example and watching it go. Some people think in the beginning that I’m kind of slow and I don’t understand the problem, because I ask a lot of these “dumb” questions: “Is a cathode plus or minus? Is an anion this way, or that way?”*
>
> *But later, when the guy’s in the middle of a bunch of equations, he’ll say something and I’ll say, “Wait a minute! There’s an error! That can’t be right!”*
>
> *The guy looks at his equations, and sure enough, after a while, he finds the mistake and wonders, “How the hell did this guy, who hardly understood at the beginning, find that mistake in the mess of all these equations?”*
>
> ***— Richard Feynman (https://en.wikipedia.org/wiki/Richard_Feynman)***

Reading about the lives of talented researchers, ones who have been praised by their peers and made stunning discoveries, you pretty quickly notice that they are not afraid at all of seeming or being very dumb, or very ignorant. For example, we can consider Niels Bohr (https://en.wikipedia.org/wiki/Niels_Bohr), who won the Nobel Prize in Physics in 1922 for his pioneering work in quantum mechanics:

> *It is practically impossible to describe Niels Bohr to a person who has never worked with him. Probably his most characteristic property was the slowness of his thinking and comprehension. … In the evening, when a handful of Bohr's students were "working" in the Paa Blegdamsvejen Institute, discussing the latest problems of the quantum theory, or playing ping-pong on the library table with coffee cups placed on it to make the game more difficult, Bohr would appear, complaining that he was very tired, and would like to "do something." To "do something" inevitably meant to go to the movies, and the only movies Bohr liked were those called The Gun Fight at the Lazy Gee Ranch or The Lone Ranger and a Sioux Girl. But it was hard to go with Bohr to the movies. He could not follow the plot, and was constantly asking us, to the great annoyance of the rest of the audience, questions like this: "Is that the sister of that cowboy who shot the Indian who tried to steal a herd of cattle belonging to her brother-in-law?" The same slowness of reaction was apparent at scientific meetings. Many a time, a visiting young physicist (most physicists visiting Copenhagen were young) would deliver a brilliant talk about his recent calculations on some intricate problem of the quantum theory. Everybody in the audience would understand the argument quite clearly, but Bohr wouldn't. So everybody would start to explain to Bohr the simple point he had missed, and in the resulting turmoil everybody would stop understanding anything. Finally, after a considerable period of time, Bohr would begin to understand, and it would turn out that what he understood about the problem presented by the visitor was quite different from what the visitor meant, and was correct, while the visitor's interpretation was wrong.*
>
> ***— George Gamow (https://en.wikipedia.org/wiki/George_Gamow) on Niels Bohr (https://en.wikipedia.org/wiki/Niels_Bohr)***

Great scientists were generally quite stupid, though we admit that some of them may have been stupider than others. More notably, most of them seem to have known it!

> *The first thing Bohr said to me was that it would only then be profitable to work with him if I understood that he was a dilettante. The only way I knew to react to this unexpected statement was with a polite smile of disbelief. But evidently Bohr was serious. He explained how he had to approach every new question from a starting point of total ignorance. It is perhaps better to say that Bohr's strength lay in his formidable intuition and insight rather than erudition.*
>
> ***— Abraham Pais (https://en.wikipedia.org/wiki/Abraham_Pais)***

Some of this is about fear. If you accept your ignorance, you will be aware of how stupid you are. Being afraid of being stupid, or seeming stupid, will lead you to make lots of mistakes. You will be afraid to look for mistakes; you will not double-check your work with the same level of care; you will be afraid that if people find out about your mistakes, they will laugh and think you are an idiot. Once you have accepted in full confidence that you, along with all other scientists, are in fact idiots, you will no longer be worried about this (http://danluu.com/look-stupid/). You will notice your own mistakes, or others will notice them for you, and you will laugh it off. "I'm so glad someone caught this!" you will say.

> *You see, one thing is, I can live with doubt and uncertainty and not knowing. (https://www.youtube.com/watch?v=_MmpUWEW6Is) I think it's much more interesting to live not knowing than to have answers which might be wrong. I have approximate answers and possible beliefs and different degrees of certainty about different things, but I'm not absolutely sure of anything and there are many things I don't know anything about, such as whether it means anything to ask why we're here, and what the question might mean. I might think about it a little bit and if I can't figure it out, then I go on to something else, but I don't have to know an answer, I don't feel frightened by not knowing things, by being lost in a mysterious universe without having any purpose, which is the way it really is so far as I can tell, possibly. It doesn't frighten me.*
>
> ***— Richard Feynman (https://en.wikipedia.org/wiki/Richard_Feynman)***

Mistakes are inevitable! You are a dummy; you will sometimes be wrong. It is ok to be wrong. If you're not willing to accept that sometimes you're wrong, you will have a hard time ever being right. Be wrong with confidence.

Don't worry too much about your intellectual gifts. Despite popular misconceptions, a lack of IQ won't hold you back. If you are really dumb and know it, you have a leg up on the smart people who, on a cosmic scale, are still stupid, but haven't realized it yet.

> *Brains are nice to have, but many people who seem not to have great IQs have done great things. At Bell Telephone Laboratories Bill Pfann (https://en.wikipedia.org/wiki/William_Gardner_Pfann) walked into my office one day with a problem in zone melting. He did not seem to me, then, to know much mathematics, to be articulate, or to have a lot of clever brains, but I had already learned brains come in many forms and flavors, and to beware of ignoring any chance I got to work with a good man. I first did a little analytical work on his equations, and soon realized what he needed was computing. I checked up on him by asking around in his department, and I found they had a low opinion of him and his idea for zone melting. But that is not the first time a person has not been appreciated locally, and I was not about to lose my chance of working with a great idea—which is what zone melting seemed to me, though not to his own department!*
>
> ***— Richard Hamming (https://en.wikipedia.org/wiki/Richard_Hamming)***

Stupidity can also be part of the inspiration behind the virtue of rebellion, a scientist's ability to defy authority figures. If you stupid, you don't realize when you should keep your mouth shut, so you say what you really think. Feynman (https://en.wikipedia.org/wiki/Richard_Feynman) again:

> *The last time he was there, Bohr said to his son, "Remember the name of that little fellow in the back over there? He's the only guy who's not afraid of me, and will say when I've got a crazy idea. So next time when we want to discuss ideas, we're not going to be able to do it with these guys who say everything is yes, yes, Dr. Bohr. Get that guy and we'll talk with him first." I was always dumb in that way. I never knew who I was talking to.*

Maybe more important is that accepting your stupidity helps you cultivate the virtue of being carefree. If you think you have a great mind, you will feel a lot of pressure to work on things that are "challenging" and "important". But you will never get anything done if you stress out about this kind of thing, and more seriously, you will never have any fun.

> *Perhaps one of the most interesting things that I ever heard him say was when, after describing to me an experiment in which he had placed under a bell-jar some pollen from a male flower, together with an unfertilized female flower, in order to see whether, when kept at a distance but under the same jar, the one would act in any way on the other, he remarked:—"That's a fool's experiment. But I love fools' experiments. I am always making them."*
>
> ***— E. Ray Lankester (https://en.wikipedia.org/wiki/Ray_Lankester), recalling Charles Darwin (https://en.wikipedia.org/wiki/Charles_Darwin)***

Arrogance

> *My goal is simple. It is a complete understanding of the universe, why it is as it is and why it exists at all.*
>
> ***— Stephen Hawking (https://en.wikipedia.org/wiki/Stephen_Hawking)***

Arrogance is the complement of stupidity, the yang to stupidity's yin. Being stupid is all about recognizing that you know nothing about everything, and in fact you have little chance of ever understanding much about anything. Having accepted such complete ignorance, you must then be extraordinarily arrogant to think that you could ever make an original discovery, let alone solve a problem that has baffled people for generations. But this is exactly what we aim to do. To complement their stupidity, a scientist must also be arrogant beyond all measure.

No one else knows anything either, so when it comes to figuring something out for the first time, you have as good a shot at it as anyone else does! Why not go for it, after all?

> *The condition of matter I have dignified by the term Electronic, THE ELECTRONIC STATE. What do you think of that? Am I not a bold man, ignorant as I am, to coin words?*
>
> ***— Michael Faraday (https://en.wikipedia.org/wiki/Michael_Faraday)***

Most people have the good sense to know what is realistic and practical, and to laugh at people who think they can do the impossible. So you have to be very dumb indeed, to be arrogant enough to think that you can change the world!

> *Who would not have been laughed at if he had said in 1800 that metals could be extracted from their ores by electricity or that portraits could be drawn by chemistry.*
>
> ***— Michael Faraday (https://en.wikipedia.org/wiki/Michael_Faraday)***

A great gap in research is between people who try things and people who sit around thinking about whether to try things. Truly, aiming low is a dead end. Aiming low is boring.

> *Confidence in yourself, then, is an essential property. Or, if you want to, you can call it "courage." Shannon (https://en.wikipedia.org/wiki/Claude_Shannon) had courage. Who else but a man with almost infinite courage would ever think of averaging over all random codes and expect the average code would be good? He knew what he was doing was important and pursued it intensely. Courage, or confidence, is a property to develop in yourself. Look at your successes, and pay less attention to failures than you are usually advised to do in the expression, "Learn from your mistakes." While playing chess Shannon would often advance his queen boldly into the fray and say, "I ain't scared of nothing."*
>
> ***— Richard Hamming (https://en.wikipedia.org/wiki/Richard_Hamming)***

You will not always be right. Often you will be wrong. This is why stupidity comes before arrogance, because you have to be prepared to make lots of dumb mistakes. If you are prepared to make dumb mistakes, you can act with confidence. You will put ideas out there that you think might be wrong. But sometimes you will surprise yourself.

> *Is it dangerous to claim that parents have no power at all (other than genetic) to shape their child's personality, intelligence, or the way he or she behaves outside the family home? … A confession: When I first made this proposal ten years ago, I didn't fully believe it myself. I took an extreme position, the null hypothesis of zero parental influence, for the sake of scientific clarity. Making myself an easy target, I invited the establishment — research psychologists in the academic world — to shoot me down. I didn't think it would be all that difficult for them to do so. … The establishment's failure to shoot me down has been nothing short of astonishing.*
>
> ***— Judith Rich Harris (https://en.wikipedia.org/wiki/Judith_Rich_Harris) for Edge (https://www.edge.org/response-detail/11064)***

Like stupidity, arrogance is linked to the virtue of rebellion. If you think you are hot shit, you will not be afraid to go against the opinions of famous writers, ivy-league professors, public officials, or other great minds.

> *The idea that smashed the old orthodoxy got its start on Christmas 1910, as Wegener (https://en.wikipedia.org/wiki/Alfred_Wegener) (the W is pronounced like a V) browsed through a friend's new atlas. Others before him had noticed that the Atlantic coast of Brazil looked as if it might once have been tucked up against West Africa, like a couple spooning in bed. But no one had made much of it, and Wegener was hardly the logical choice to show what they had been missing. He was a lecturer at Marburg University, not merely untenured but unsalaried, and his specialties were meteorology and astronomy, not geology.*
>
> *But Wegener was not timid about disciplinary boundaries, or much else. He was an Arctic explorer and a record-setting balloonist, and when his scientific mentor and future father-in-law advised him to be cautious in his theorizing, Wegener replied, "Why should we hesitate to toss the old views overboard?"*
>
> ***— Richard Connff for Smithsonian Magazine (https://www.smithsonianmag.com/science-nature/when-continental-drift-was-considered-pseudoscience-90353214/)***

You shouldn't cultivate arrogance in a way that makes you an asshole, though some scientists have made this mistake. This virtue is not about thinking that you are better than other people. Forget about other people. It is about thinking that you have the potential to be really good — to be damn good. It is about moving with extreme confidence. You cultivate arrogance so that if someone says, "that's very arrogant of you!" you respond, "so what?"

Laziness

> *Study hard what interests you the most in the most undisciplined, irreverent and original manner possible.*
>
> ***— Richard Feynman (https://en.wikipedia.org/wiki/Richard_Feynman)***

Everyone knows that research requires hard work. This is true, but your hard work has to be matched by a commitment to relaxation, slacking off, and fucking around when you "should" be working — that is, laziness.

Laziness is not optional — it is essential. Great work cannot be done without it. And it must be cultivated as a virtue, because a sinful world is always trying to push back against it (https://experimentalhistory.substack.com/p/bureaucratic-psychosis).

> *Leonardo (https://en.wikipedia.org/wiki/Leonardo_da_Vinci), knowing that the intellect of that Prince was acute and discerning, was pleased to discourse at large with the Duke on the subject… and he reasoned much with him about art, and made him understand that men of lofty genius sometimes accomplish the most when they work the least, seeking out inventions with the mind, and forming those perfect ideas which the hands afterwards express and reproduce from the images already conceived in the brain.*
>
> ***— Giorgio Vasari (https://en.wikipedia.org/wiki/Giorgio_Vasari)***

Hard work needs to happen to bring an idea to fruition, but you cannot work hard all the time any more than a piston can be firing all the time, or every piston in an engine can fire at once. Pistons are always moving up and down. A piston moves up; it fires; but that action is matched by the piston moving down, and spending some time not firing. It would be foolish to complain that the piston is not firing all the time, but this is what some people do in trying to work hard all the time. They are trying to keep the piston in the down position the whole time, not recognizing that this will stop the piston from firing again, and will damage the whole engine.

They would do better to cultivate the virtue of laziness, and go take a nap or stare at the clouds or play fetch with their dog or something. Taking a nap is just turning your brain off and then on again, which solves 90% of my computer problems.

> *Albert Einstein (https://en.wikipedia.org/wiki/Albert_Einstein) once asked a friend of mine in Princeton, "Why is it I get my best ideas in the morning while I'm shaving?" My friend answered, as I have been trying to say here, that often the mind needs the relaxation of inner controls — needs to be freed in reveries or day dreaming — for the unaccustomed ideas to emerge.*
>
> ***— Rollo May (https://en.wikipedia.org/wiki/Rollo_May)***

Mathematicians are not exactly scientists, but they certainly have one of the best claims on pure idea work. So you might expect that for mathematicians, more time spent working would lead to more results. But apparently not. G.H. Hardy (https://en.wikipedia.org/wiki/G._H._Hardy), one of the great British mathematicians of the 20th century, started his mornings by reading the cricket scores (or when cricket was not in season, the Australian cricket scores). He would work only from 9 to 1, after which he would eat lunch, play tennis, or (surprise) watch a game of cricket. His collaborator John Edensor Littlewood (https://en.wikipedia.org/wiki/John_Edensor_Littlewood) said:

> *You must also acquire the art of 'thinking vaguely,' an elusive idea I can't elaborate in short form. After what I have said earlier, it is inevitable that I should stress the importance of giving the subconscious every chance. There should be relaxed periods during the working day, profitably, I say, spent in walking. … On days free from research, and apart from regular holidays, I recommend four hours [of work] a day or at most five, with breaks about every hour (for walks perhaps). If you don't have breaks you unconsciously acquire the habit of slowing down.*
>
> ***— John Edensor Littlewood (https://en.wikipedia.org/wiki/John_Edensor_Littlewood)***

Henri Poincaré (https://en.wikipedia.org/wiki/Henri_Poincar%C3%A9) is perhaps the best example. He was something of a mathematician but also worked in physics and engineering, and he worked around four hours a day. Poincaré happened to have several experiences where hard work failed to crack a problem, but laziness or relaxation did the trick; for example, drinking coffee too late and messing up his sleep schedule:

> *For fifteen days I strove to prove that there could not be any functions like those I have since called Fuchsian functions. I was then very ignorant; every day I seated myself at my work table, stayed an hour or two, tried a great number of combinations and reached no results. One evening, contrary to my custom, I drank black coffee and could not sleep. Ideas rose in crowds; I felt them collide until pairs interlocked, so to speak, making a stable combination. By the next morning I had established the existence of a class of Fuchsian functions, those which come from the hypergeometric series; I had only to write out the results, which took but a few hours.*
>
> ***— Henri Poincaré (https://en.wikipedia.org/wiki/Henri_Poincar%C3%A9)***

Or, even more effortless, getting onto a bus:

> *I left Caen, where I was living, to go on a geological excursion under the auspices of the School of Mines. The incidents of the travel made me forget my mathematical work. Having reached Coutances, we entered an omnibus to go some place or other. At the moment when I put my foot on the step the idea came to me, without anything in my former thoughts seeming to have paved the way for it, that the transformations I had used to define the Fuchsian functions were identical with those of non-Euclidean geometry. I did not verify the idea; I should not have had time, as, upon taking my seat in the omnibus, I went on with a conversation already commenced, but I felt a perfect certainty. On my return to Caen, for conscience's sake I verified the result at my leisure.*
>
> *…*
>
> *Then I turned my attention to the study of some arithmetical questions apparently without much success and without a suspicion of any connection with my preceding researches. Disgusted with my failure, I went to spend a few days at the seaside and thought of something else. One morning, while walking on the bluff, the idea came to me, with just the same characteristics of brevity, suddenness and immediate certainty, that the arithmetic transformations of indefinite ternary quadratic forms were identical with those of non-Euclidean geometry.*
>
> ***— Henri Poincaré (https://en.wikipedia.org/wiki/Henri_Poincar%C3%A9)***

(In fact there seems to be something about buses. If you are working on a problem you just can't crack, maybe take a bus ride?)

> *In 1865, Kekulé (https://en.wikipedia.org/wiki/August_Kekul%C3%A9) himself came up with the answer. He related some years later that the vision of the benzene molecule came to him while he was riding on a bus and sunk in a reverie, half asleep. In his dream, chains of carbon atoms seemed to come alive and dance before his eyes, and then suddenly one coiled on itself like a snake. Kekulé awoke from his reverie with a start.*
>
> ***— Isaac Asimov (https://en.wikipedia.org/wiki/Isaac_Asimov)***

Poincaré and Kekulé aren't the only ones. For Linus Pauling (https://en.wikipedia.org/wiki/Linus_Pauling), a head cold ar pulpy detective novels seems to have done the trick:

> *In Oxford, it was April, I believe, I caught cold. I went to bed, and read detective stories for a day, and got bored, and thought why don't I have a crack at that problem of alpha keratin.*
>
> ***— Linus Pauling (https://en.wikipedia.org/wiki/Linus_Pauling)***

This was one of the one of the many achievements that led to his Nobel Prize in Chemistry in 1954. So next time you think, "I shouldn't read detective stories until I get bored, I should be working," please reconsider.

Insight comes suddenly and without warning, but rarely when you have your nose to the grindstone. So spend some time staring out your dormitory window. If you don't learn to be lazy, you might miss it.

Carefreeness

> *I lie on the beach like a crocodile and let myself be roasted by the sun. I never see a newspaper and don't give a damn for what is called the world.*
>
> ***— Albert Einstein (https://en.wikipedia.org/wiki/Albert_Einstein), letter to Max Born (https://en.wikipedia.org/wiki/Max_Born)***

The hardest of the scientific virtues to cultivate may be the virtue of carefreeness. This is the virtue of not taking your work too seriously. If you try too hard, you get serious, you get worried, you're not carefree anymore — you see, it's a problem.

> *So I got this new attitude. Now that I am burned out and I'll never accomplish anything, I've got this nice position at the university teaching classes which I rather enjoy, and just like I read the Arabian Nights for pleasure, I'm going to play with physics, whenever I want to, without worrying about any importance whatsoever.*
>
> *Within a week I was in the cafeteria and some guy, fooling around, throws a plate in the air. As the plate went up in the air I saw it wobble, and I noticed the red medallion of Cornell on the plate going around. It was pretty obvious to me that the medallion went around faster than the wobbling.*
>
> *I had nothing to do, so I start to figure out the motion of the rotating plate. I discover that when the angle is very slight, the medallion rotates twice as fast as the wobble rate — two to one. It came out of a complicated equation! Then I thought, "Is there some way I can see in a more fundamental way, by looking at the forces or the dynamics, why it's two to one?"*
>
> *I don't remember how I did it, but I ultimately worked out what the motion of the mass particles is, and how all the accelerations balance to make it come out two to one.*
>
> *I still remember going to Hans Bethe (https://en.wikipedia.org/wiki/Hans_Bethe) and saying, "Hey, Hans! I noticed something interesting. Here the plate goes around so, and the reason it's two to one is…" and I showed him the accelerations.*
>
> *He says, "Feynman, that's pretty interesting, but what's the importance of it? Why are you doing it?"*
>
> *"Hah!" I say. "There's no importance whatsoever. I'm just doing it for the fun of it." His reaction didn't discourage me; I had made up my mind I was going to enjoy physics and do whatever I liked.*
>
> *I went on to work out equations of wobbles. Then I thought about how electron orbits start to move in relativity. Then there's the Dirac Equation in electrodynamics. And then quantum electrodynamics. And before I knew it (it was a very short time) I was "playing" — working, really — with the same old problem that I loved so much, that I had stopped working on when I went to Los Alamos: my thesis-type problems; all those old-fashioned, wonderful things.*
>
> *It was effortless. It was easy to play with these things. It was like uncorking a bottle: Everything flowed out effortlessly. I almost tried to resist it! There was no importance to what I was doing, but ultimately there was. The diagrams and the whole business that I got the Nobel Prize for came from that piddling around with the wobbling plate.*
>
> ***— Richard Feynman (https://en.wikipedia.org/wiki/Richard_Feynman)***

This is related to the scientific virtue of laziness — a carefree person will find it easier to take time off from their work, to rela go sailing, play ping-pong, etc. But carefreeness is a higher virtue than even laziness is. Being carefree means not worrying and relaxing even when you are working very hard.

If you do not cultivate the sense of carefreeness, you will get all tangled up about not working on "important" problems. You will get all tangled up about working on the things you think you "should be" working on, instead of the things you *want* to be working on, the things you find fun and interesting.

If research starts to be a drag, it won't matter how talented you are. Nothing will kill your spark faster than finding research dull. Nothing will wring you out more than working on things you hate but you think are "important".

This is tricky because there are many different ways you can lose your sense of carefreeness. There are a lot of things that can throw off your groove. The first is becoming attached to worldly rewards — cash, titles, fancy hats, etc.

> *I am happy because I want nothing from anyone. I do not care about money. Decorations, titles or distinctions mean nothing to me. I do not crave praise. The only thing that gives me pleasure, apart from my work, my violin, and my sailboat, is the appreciation of my fellow workers.*
>
> ***— Albert Einstein (https://en.wikipedia.org/wiki/Albert_Einstein)***

When you start seeking these rewards, or even thinking about them too much, the whole research enterprise falls apart. Sometimes this can happen overnight.

You might say, "well surely someone has to think about these practical problems." It's true that some people should think about worldly things, but we don't exactly see a shortage of that. What cannot be forced, and can only be cultivated, are free minds pursuing things that no one else thinks are interesting problems, for no good reason at all.

> *We must not forget that when radium was discovered no one knew that it would prove useful in hospitals. The work was one of pure science. And this is a proof that scientific work must not be considered from the point of view of the direct usefulness of it. It must be done for itself, for the beauty of science, and then there is always the chance that a scientific discovery may become like the radium a benefit for humanity.*
>
> ***— Marie Curie (https://en.wikipedia.org/wiki/Marie_Curie)***

The *best* ideas are almost certainly going to be ones that seem insane or stupid — if they seemed like good ideas, someone w have tried them already. How can there possibly be a market for such ideas? They are left to people who are carefree enough in their spirit to pursue these dumb ideas anyways. Most great advances are preceded by announcements that they are impossible, and you need to be ready and willing to ignore that stuff:

> *The whole procedure [of shooting rockets into space]… presents difficulties of so fundamental a nature, that we are forced to dismiss the notion as essentially impracticable, in spite of the author's insistent appeal to put aside prejudice and to recollect the supposed impossibility of heavier-than-air flight before it was actually accomplished.*
>
> ***— Sir Richard van der Riet Woolley (https://en.wikipedia.org/wiki/Richard_van_der_Riet_Woolley), British astronomer, reviewing P.E. Cleator's "Rockets in Space", Nature, March 14, 1936***

Some people are ok at resisting money and fame. But people find it harder to avoid being swayed by praise. It is easy to want to impress people, and want them to like you. But if you start worrying about praise, two things will happen. First of all, you will be worrying, which will cloud your head. Second, if you are trying to get praise, you will work on problems that are popular. Popular problems are fine, but you have to know that they will be seductive. You should pay more attention to topics you like that *aren't* popular.

Focusing on unpopular problems you find fascinating is a good sign that you're making use of your particular talents. Following praise is a sign you are being led away from your gifts! Taste is really important — follow what you find interesting.

> *…my work, which I've done for a long time, was not pursued in order to gain the praise I now enjoy, but chiefly from a craving after knowledge, which I notice resides in me more than in most other men. And therewithal, whenever I found out anything remarkable, I have thought it my duty to put down my discovery on paper, so that all ingenious people might be informed thereof.*
>
> ***— Antonie van Leeuwenhoek (https://en.wikipedia.org/wiki/Antonie_van_Leeuwenhoek), Letter of June 12, 1716***

Another is worrying about being an "expert", keeping up with the field, staying aware of the latest publications, et cetera. Staying carefree means being happy to ignore these things (if you feel like it).

You can tell really good science because it stays carefree even when the stakes are very high:

> *I remember a friend of mine who worked with me, Paul Olum (https://en.wikipedia.org/wiki/Paul_Olum), a mathematician, came up to me afterwards and said, "When they make a moving picture about this, they'll have the guy coming back from Chicago to make his report to the Princeton men about the bomb. He'll be wearing a suit and carrying a briefcase and so on — and here you're in dirty shirtsleeves and just telling us all about it, in spite of its being such a serious and dramatic thing."*
>
> ***— Richard Feynman (https://en.wikipedia.org/wiki/Richard_Feynman)***

Staying carefree is how you keep in touch with what really interests you. It is how you practice going with your gut. It is how you make sure you are still having fun.

No one is doing great work when they are bent over their lab bench thinking, "gee I wish I were doing something else!" Great work doesn't come from banging your head against your keyboard a little harder.

> *Alan Turing's (https://en.wikipedia.org/wiki/Alan_Turing) celebrated paper of 1935, which was to provide the foundation of modern computer theory, was originally written as a speculative exploration for mathematical logicians. The war gave him and others the occasion to translate theory into the beginnings of practice for the purpose of code-breaking, but when it appeared nobody except a handful of mathematicians even read, let alone took notice of Turing's paper.*
>
> ***— Eric Hobsbawm on Alan Turing (https://en.wikipedia.org/wiki/Alan_Turing)***

We cannot emphasize enough that great work almost always comes from things that at the time seemed like pointless nonsense. Those scientists did it anyway, because it interested them. But to do that you will have to be ready to stand against the world, people telling you that you should be using your gifts on something more productive, that you are wasting your talents! Cultivating this carefreeness will help you ignore them.

> *A large part of mathematics which becomes useful developed with absolutely no desire to be useful, and in a situation where nobody could possibly know in what area it would become useful; and there were no general indications that it ever would be so. By and large it is uniformly true in mathematics that there is a time lapse between a mathematical discovery and the moment when it is useful; and that this lapse of time can be anything from 30 to 100 years, in some cases even more; and that the whole system seems to function without any direction, without any reference to usefulness, and without any desire to do things which are useful.*
>
> ***— John von Neumann (https://en.wikipedia.org/wiki/John_von_Neumann)***

Not every pointless idea ends up being a great discovery — most of them do not. But a feature you will see over and over ag in great scientists is a complete lack of fear when it comes to pursuing ideas that seem like (or truly are) nonsense. You might have to look into 100 dumb ideas before you find one that is any good — in fact, maybe you should start right now.

> *I've noticed that my dog can correctly tell which way I've gone in the house, especially if I'm barefoot, by smelling my footprints. So I tried to do that: I crawled around the rug on my hands and knees, sniffing, to see if I could tell the difference between where I walked and where I didn't, and I found it impossible. So the dog is much better than I am.*
>
> ***— Richard Feynman (https://en.wikipedia.org/wiki/Richard_Feynman)***

Most people find it hard to stay carefree all the time. When you choke, and start worrying about things — are you working on the right stuff, are you wasting your life, etc. — cultivating the virtue of carefreeness is the way to get back on top.

Beauty

> *I am among those who think that science has great beauty. A scientist in his laboratory is not only a technician: he is also a child placed before natural phenomena which impress him like a fairy tale. We should not allow it to be believed that all scientific progress can be reduced to mechanisms, machines, gearings, even though such machinery also has its beauty.*
>
> *Neither do I believe that the spirit of adventure runs any risk of disappearing in our world. If I see anything vital around me, it is precisely that spirit of adventure, which seems indestructible and is akin to curiosity.*
>
> ***— Marie Curie (https://en.wikipedia.org/wiki/Marie_Curie)***

The fifth virtue that a scientist must cultivate is an appreciation for beauty. There are practical reasons to do science, but *in the moment*, great research is done just to do something because it's beautiful and exemplifies enjoying that beauty.

This eye for beauty is not optional! It is, like all the scientific virtues, *essential* for doing any kind of original research.

> *The scientist does not study nature because it is useful; he studies it because it pleases him, and it pleases him because it is beautiful. Were nature not beautiful, it would not be worth knowing, life would not be worth living.*
>
> ***— Henri Poincaré (https://en.wikipedia.org/wiki/Henri_Poincar%C3%A9)***

Every scientist is limited by their appreciation for beauty. If you have developed an eye for it, your work will benefit. Without a sense for it, your work will suffer. It does not matter if your taste is for poetry, pinwheels, or cricket plays. You can have an obsession with video game soundtracks, or be an ameteur baker. You must be able to see the beauty in *something* — it is practice for seeing the beauty and the harmony of nature. The more kinds of beauty you learn to appreciate, the better your work will become.

> *The mathematician's patterns, like the painter's or the poet's must be beautiful; the ideas, like the colours or the words must fit together in a harmonious way. Beauty is the first test: there is no permanent place in this world for ugly mathematics.*
>
> ***— G. H. Hardy (https://en.wikipedia.org/wiki/G._H._Hardy)***

To many people, a scientist will seem obsessive. This is true, but obsession is not by itself a virtue. The obsession you see in many researchers comes from their sense of beauty — they know what it *should* look like. They have an intense need to get it *right*. They cannot let it alone when they know it is wrong — it keeps calling them back. Only when it is right will it be beautiful.

> *Copernicus' (https://en.wikipedia.org/wiki/Nicolaus_Copernicus) aesthetic objections to [equants] provided one essential motive for his rejection of the Ptolemaic system.*
>
> ***— Thomas Kuhn (https://en.wikipedia.org/wiki/Thomas_Kuhn), The Copernican Revolution***

This is why we cultivate an appreciation for aesthetics, rather than cultivating obsession itself. Pure obsession will lead you to pursue any project anywhere, even if it leads you up a tree. Cultivating aesthetics, you will only follow projects if they lead you up the trunks of particularly *beautiful* trees.

This builds on itself. Building an aesthetic sense leads you to become a better researcher. Practicing this sense in your work becomes another way to develop this virtue. Having developed the virtue, you can now appreciate the beauty in more things. This develops your aesthetic sense further, your work improves, the virtue reaches a higher stage of refinement, etc.

> *I have a friend who's an artist, and he sometimes takes a view which I don't agree with. He'll hold up a flower and say, "Look how beautiful it is," and I'll agree. But then he'll say, "I, as an artist, can see how beautiful a flower is. But you, as a scientist, take it all apart and it becomes dull." I think he's kind of nutty. … There are all kinds of interesting questions that come from a knowledge of science, which only adds to the excitement and mystery and awe of a flower. It only adds. I don't understand how it subtracts.*
>
> ***— Richard Feynman (https://en.wikipedia.org/wiki/Richard_Feynman)***

Part of what is called *beauty* could simply be called *fun*. If you don't know how to have fun, you will not be able to appreciate the beauty around you — you will not have a good time.

> *McClintock was motivated by the intrinsic rewards that she experienced from the work itself. She was rewarded every day by the joy she felt in the endeavor. She loved posing questions, finding answers, solving problems. She loved working in her garden and in her laboratory. She recalled later, "I was doing what I wanted to do, and there was absolutely no thought of a career. I was just having a marvelous time."*
>
> *Upon hearing that she had been named for the Nobel Prize, McClintock told reporters, "The prize is such an extraordinary honor. It might seem unfair, however, to reward a person for having so much pleasure, over the years, asking the maize to solve specific problems and then watching its response." When asked if she was bitter about the lateness of the recognition, she said simply, "If you know you're right, you don't care. You know that sooner or later, it will come out in the wash."*
>
> ***— Abigail Lipson on Barbara McClintock (https://en.wikipedia.org/wiki/Barbara_McClintock)***

Given all this, perhaps it's not surprising that many scientists are also talented artists and musicians.

> *If I was not a physicist, I would probably be a musician. I often think in music. I live my daydreams in music. I see my life in terms of music. … I cannot tell if I would have done any creative work of importance in music, but I do know that I get most joy in life out of my violin.*
>
> ***— Albert Einstein (https://en.wikipedia.org/wiki/Albert_Einstein)***

Just how good a violinist was Einstein? One time, a confused music critic in Berlin thought Einstein was a famous violinist rather than a famous physicist, and said, "Einstein's playing is excellent, but he does not deserve world fame; there are many others just as good."

Leonardo da Vinci (https://en.wikipedia.org/wiki/Leonardo_da_Vinci) is famous for his painting and drawing, of course, but what you may not know is that he was also something like the 15th century equivalent of a heavy metal virtuoso:

> *In the year 1494, Leonardo was summoned to Milan in great repute to the Duke, who took much delight in the sound of the lyre, to the end that he might play it: and Leonardo took with him that instrument which he had made with his own hands, in great part of silver, in the form of a horse's skull—a thing bizarre and new—in order that the harmony might be of greater volume and more sonorous in tone; with which he surpassed all the musicians who had come together there to play. Besides this, he was the best improviser in verse of his day.*
>
> ***— Giorgio Vasari (https://en.wikipedia.org/wiki/Giorgio_Vasari)***

Richard Feynman (https://en.wikipedia.org/wiki/Richard_Feynman) (Nobel Prize in Physics, 1965) was famous for playing bongos, and briefly played the *frigideira* in a Brazilian samba band. He also made some progress as a portrait artist, to the point where he sold several pieces and even had a small exhibit.

Barbara McClintock (https://en.wikipedia.org/wiki/Barbara_McClintock) (Nobel Prize in Physiology or Medicine, 1983) played tenor banjo in a jazz combo for years, but in the end she had to give it up because it kept her up too late at night.

Santiago Ramón y Cajal (https://en.wikipedia.org/wiki/Santiago_Ram%C3%B3n_y_Cajal) (Nobel Prize in Physiology or Medicine, 1906) ranks up there almost with Da Vinci in terms of the incredible breadth of his artistic pursuits:

> *Santiago Ramón y Cajal (1852–1934) is one of the more fascinating personalities in science. Above all he was the most important neuroanatomist since Andreas Vesalius, the Renaissance founder of modern biology. However, Cajal was also a thoughtful and inspired teacher, he made several lasting contributions to Spanish literature (his autobiography, a popular book of aphorisms, and reflections on old age), and he wrote one of the early books on the theory and practice of color photography. Furthermore, he was an exceptional artist, perhaps the best ever to draw the circuits of the brain, which he could never photograph to his satisfaction.*
>
> ***— Larry W. Swanson (https://en.wikipedia.org/wiki/Larry_Swanson), foreword to Cajal's book Advice for a Young Investigator***

We can add to this list that Cajal also wrote a number of science-fiction stories that were considered too scandalous for publication. Five were eventually published under the pseudonym "Dr. Bacteria" (yes, really), but the rest were considered too offensive to be published even at this remove, and they have since been lost.

This was also true for many of the old masters. James Clerk Maxwell (https://en.wikipedia.org/wiki/James_Clerk_Maxwel was fascinated by color, and helped invent color photography. Robert Hooke (https://en.wikipedia.org/wiki/Robert_Hooke) was apprenticed to a painter as a young man, and proved pretty good at it. He did all his own illustrations for his book *Micrographia*, which to this day remain impressive (https://commons.wikimedia.org/wiki/Category:Hooke%27s_Micrographia_Diagrams_from_the_National_Library_of_Wales). Sir Isaac Newton (https://en.wikipedia.org/wiki/Isaac_Newton) also seemed to have quite the knack for illustration:

> *Mr. Clark, aforementioned now apothecary, & surgeon in Grantham, tells me, that he himself likewise lodg'd, whilst a youth, in that same garret in the old house where Sr. Isaac had done. he says, the walls, & ceelings were full of drawings, which he had made with charcole. there were birds, beasts, men, ships, plants, mathematical figures, circles, & triangles. that the drawings were very well done. & scarce a board in the partitions about the room, without Isaac Newton cut upon it. … Sr Isaac when a lad here at School, was not only expert at his mechanical tools, but equally so with his pen. for he busyed himself very much in drawing, which he took from his own inclination; & as in every thing else, improv'd it by a careful observation of nature.*
>
> ***— William Stukeley on Isaac Newton (https://en.wikipedia.org/wiki/Isaac_Newton)***

This is only an incomplete list — not every talented scientist is also a musician or artist. But a scientist's success depends on the cultivation of their aesthetic sense, and this sense of beauty is essential to every researcher.

> *I am no poet, but if you think for yourselves, as I proceed, the facts will form a poem in your minds.*
>
> ***— Michael Faraday (https://en.wikipedia.org/wiki/Michael_Faraday)***

Rebellion

> *… a reaction I learned from my father: Have no respect whatsoever for authority; forget who said it and instead look what he starts with, where he ends up, and ask yourself, "Is it reasonable?"*
>
> ***— Richard Feynman (https://en.wikipedia.org/wiki/Richard_Feynman)***

To do research you must be free. Free to question. Free to doubt. Free to come up with new perspectives and new approaches. Free to challenge the old ways of doing things, or worse, ignore them. Free to try to solve problems where everyone thinks they know the answer. Free to not spend all your time hunched over your workbench and let your mind wander. Free to tinker with pointless ideas. Free to turn over rocks and look at the bugs underneath.

The world must be free and open as well. You need to be free to meet and discuss things with anyone you want. You must have free access to books, libraries, journals, the internet. You must be free to try things and build things for yourself.

But not everyone shares these values. And, because we are social creatures and we were brought up in societies that are less than totally free, we carry around an inner authoritarian in our heads. We cultivate the virtue of rebellion to free us from inner and outer attempts to suppress our freedom of thought and expression.

> *There must be no barriers to freedom of inquiry … There is no place for dogma in science. The scientist is free, and must be free to ask any question, to doubt any assertion, to seek for any evidence, to correct any errors. Our political life is also predicated on openness. We know that the only way to avoid error is to detect it and that the only way to detect it is to be free to inquire. And we know that as long as men are free to ask what they must, free to say what they think, free to think what they will, freedom can never be lost, and science can never regress.*
>
> ***— J. Robert Oppenheimer (https://en.wikipedia.org/wiki/J._Robert_Oppenheimer)***

Spitting in the eye of authority isn't easy — it doesn't come naturally to most people. So rebellion must be cultivated in small ways every day. You may not have to actively rebel very often, but the material for raising hell should always be kept in readiness.

To do science you have to be ready to pick at the idea that something might be wrong. The most important new ideas are going to be most at odds with what we believe right now. Having a mind free enough to think thoughts that have never been thought before is absolutely necessary.

The vibe of rebellion is, "the prevailing order is wrong — but some other order might be right." Things could be fundamentally different than they are now; everything you take for granted could be ungranted.

It's not that this is true 100% of the time — sometimes the usual way of thinking is right — just that it won't be obvious unles you're questioning what you "know" (https://slimemoldtimemold.com/2022/01/27/like-a-lemon-to-a-lime-a-lime-to-a-lemon/). To some degree, rebellion is basically just acknowledging that the status quo can lead you astray.

Not everyone likes the idea of turning the current order upside down, so you may have to fight for it, or even for the right to speculate about it. But it's important because making the world a better place is worth it.

Research depends on cultivating the skill of looking at something and thinking — gee, this could be better. This instrument could be better. This theory could be better. Our understanding of this question could be better. This leads to the cultivation of the virtue of rebellion, where you look at how things are today, and think, you know what, they could be better.

> *I won't stop at being Robin Hood. I feel more like a revolutionary because the final goal is not only to download all the articles and books and give open access to them, but to change legislation in such a way that free distribution of research papers will not face any legal obstacles.*
>
> ***— Alexandra Elbakyan (https://en.wikipedia.org/wiki/Alexandra_Elbakyan)***

Rebellion is one of the highest scientific virtues. It is supported by stupidity — because you have to be pretty dumb to bet against the status quo and think you can win. It is supported by arrogance — in that you must be pretty arrogant to think you know better than the experts. It is supported by aesthetics — because seeing the possibility for a more beautiful experiment, a more beautiful theory, a more beautiful world is needed to inspire your rebellion. It is supported by carefreeness — not worrying about whether you win or lose makes the struggle against authority that much easier. Whenever possible, rebellion should be fun.

Rebellion is also egalitarian — it means focusing on people's arguments, not their credentials. If their arguments are solid, then it doesn't matter if they are, in fact, a soccer mom. If their arguments are so full of holes you can see them from a mile away, then it doesn't matter where their PhD is from, or what university gave them tenure.

> *If it disagrees with experiment it is wrong. In that simple statement is the key to science. It does not make any difference how beautiful your guess is. It does not make any difference how smart you are, who made the guess, or what his name is – if it disagrees with experiment it is wrong. That is all there is to it.*
>
> ***— Richard Feynman (https://en.wikipedia.org/wiki/Richard_Feynman)***

The virtue of rebellion means cultivating in yourself the ability to stand up to anyone on the planet, to question them as an equal, and to not take anything they say on authority alone. But rebellion is not about getting in fights for no reason — be strategic.

> *John Tukey (https://en.wikipedia.org/wiki/John_Tukey) almost always dressed very casually. He would go into an important office and it would take a long time before the other fellow realized that this is a first-class man and he had better listen. For a long time John has had to overcome this kind of hostility. It's wasted effort! I didn't say you should conform; I said "The appearance of conforming gets you a long way." If you choose to assert your ego in any number of ways, "I am going to do it my way," you pay a small steady price throughout the whole of your professional career. And this, over a whole lifetime, adds up to an enormous amount of needless trouble.*
>
> ***— Richard Hamming (https://en.wikipedia.org/wiki/Richard_Hamming)***

This virtue extends outside of the research world, because nature does not stop at the laboratory door! Practicing rebellion has to extend to every part of your life.

It's easy to parrot experts. Even just saying "I don't understand" is an act of rebellion. If you want to be free to be confused, to doubt, to ask dumb questions, you need to be prepared to be a rebel.

> *Every valuable human being must be a radical and a rebel, for what he must aim at is to make things better than they are.*
>
> ***— Niels Bohr (https://en.wikipedia.org/wiki/Niels_Bohr)***

You need to cultivate rebellion because people won't always understand the value of that weird thing you are doing. You have to be ready to do it anyways. One reviewer of Charles Darwin's (https://en.wikipedia.org/wiki/Charles_Darwin) book *On The Origin of Species* suggested that "Mr. D" re-write the book to focus on his observations of pigeons. "Every body is interested in pigeons," they said. "The book would be reviewed in every journal in the kingdom, & would soon be on every table. … The book on pigeons would be at any rate a delightful commencement." Barbara McClintock's (https://en.wikipedia.org/wiki/Barbara_McClintock) parents were against her research because they didn't think there was any value in genetics!

The world in general disapproves of creativity, and to be creative in public is particularly bad. Even to speculate in public is rather worrisome.

— Isaac Asimov (https://en.wikipedia.org/wiki/Isaac_Asimov)

Similarly, if you have cultivated this virtue, you will also be ok with other people doing research that you don't understand. Anyone doing really first-rate work must be doing something you don't get — because if you understood it, it couldn't possibly be all that original. So when you see a project that makes you scratch your head, think — it might be nothing, but let's see where it goes, it could be a big deal.

In addition, exercising your rebellious thinking on social issues is good practice for rebellious thinking on scientific issues.

Unthinking respect for authority is the greatest enemy of truth.

— Albert Einstein (https://en.wikipedia.org/wiki/Albert_Einstein)

Many people are open-minded. But some people have a hard time imagining society changing in any way, even for the better. It makes some people uncomfortable. So you need to be ready to try anyways, even in the face of this discouragement.

I used to cut vegetables in the kitchen. String beans had to be cut into one-inch pieces. The way you were supposed to do it was: You hold two beans in one hand, the knife in the other, and you press the knife against the beans and your thumb, almost cutting yourself. It was a slow process. So I put my mind to it, and I got a pretty good idea. I sat down at the wooden table outside the kitchen, put a bowl in my lap, and stuck a very sharp knife into the table at a forty-five-degree angle away from me. Then I put a pile of the string beans on each side, and I'd pick out a bean, one in each hand, and bring it towards me with enough speed that it would slice, and the pieces would slide into the bowl that was in my lap.

So I'm slicing beans one after the other — chig, chig, chig, chig, chig — and everybody's giving me the beans, and I'm going like sixty when the boss comes by and says, "What are you doing?"

I say, "Look at the way I have of cutting beans!" — and just at that moment I put a finger through instead of a bean. Blood came out and went on the beans, and there was a big excitement: "Look at how many beans you spoiled! What a stupid way to do things!" and so on. So I was never able to make any improvement, which would have been easy — with a guard, or something — but no, there was no chance for improvement.

— Richard Feynman (https://en.wikipedia.org/wiki/Richard_Feynman)

This puts you at odds with authority. Kings, princes, and network executives do not want revolutionary new ideas. They generally like the current system, because they are used to it, and this system has given them positions of respect and power. They are going to do what they can to encourage people to accept how things are, or at least accept that for any problems that do exist, *qualified* people are taking care of it.

The scientist has a lot of experience with ignorance and doubt and uncertainty, and this experience is of very great importance, I think. When a scientist doesn't know the answer to a problem, he is ignorant. When he has a hunch as to what the result is, he is uncertain. And when he is pretty darn sure of what the result is going to be, he is still in some doubt. We have found it of paramount importance that in order to progress we must recognize our ignorance and leave room for doubt. Scientific knowledge is a body of statements of varying degrees of certainty – some most unsure, some nearly sure, but none absolutely certain. Now, we scientists are used to this, and we take it for granted that it is perfectly consistent to be unsure, that it is possible to live and not know. But I don't know whether everyone realizes this is true. Our freedom to doubt was born out of a struggle against authority in the early days of science. It was a very deep and strong struggle: permit us to question – to doubt – to not be sure. I think that it is important that we do not forget this struggle and thus perhaps lose what we have gained.

— Richard Feynman (https://en.wikipedia.org/wiki/Richard_Feynman)

It is not enough to simply question the wisdom of experts, or to not listen to authority yourself. You have to cultivate ACTIVE REBELLION. Authority will constantly be telling you that things are understood, that they cannot be improved, that you cannot run in the halls. You need to actively undermine this — by finding ways that the world is not understood, by trying to improve things, by organizing go-kart races during lunch period.

Authority will tell you to wait until the time is right, or wait for other people who are more qualified to have a go at it. But if you wait you will never get anywhere. You need to try small things right away, to try and fail and learn, to experiment and have a go at it.

> *Science as subversion has a long history. … Davis and Sakharov belong to an old tradition in science that goes all the way back to the rebels Benjamin Franklin and Joseph Priestley in the eighteenth century, to Galileo and Giordano Bruno in the seventeenth and sixteenth. If science ceases to be a rebellion against authority, then it does not deserve the talents of our brightest children. … We should try to introduce our children to science today as a rebellion against poverty and ugliness and militarism and economic injustice.*
>
> ***— Freeman Dyson (https://en.wikipedia.org/wiki/Freeman_Dyson)***

This even puts you at odds with other scientists. Like other entrenched authorities, any change to the status quo threatens the position of scientists who have come before you. In fact it's somewhat worse with other scientists, because the more famous they are, the bigger a target there is on their back. A good way to do great work is to tear down famous work by the previous generation, and you can imagine why the previous generation has a hard time feeling excited about this idea.

> *When an old and distinguished person speaks to you, listen to him carefully and with respect — but do not believe him. Never put your trust into anything but your own intellect. Your elder, no matter whether he has gray hair or has lost his hair, no matter whether he is a Nobel laureate — may be wrong.*
>
> ***— Linus Pauling (https://en.wikipedia.org/wiki/Linus_Pauling)***

Ideas can also have authority. A good idea in science tends to stick around until you barely notice it anymore. It's not just that you see them as necessary, it's that they start to seem like part of the background, a totally reasonable assumption. You take them for granted. But questioning old ideas is even more important than questioning old people, and a high exercise of rebellion is trying to tear down old ways of thinking, ways of thinking so old that you didn't even realize you thought that way.

> *Concepts that have proven useful in ordering things easily achieve such authority over us that we forget their earthly origins and accept them as unalterable givens. Thus they might come to be stamped as "necessities of thought," "a priori givens," etc. The path of scientific progress is often made impassable for a long time by such errors. Therefore it is by no means an idle game if we become practiced in analysing long-held commonplace concepts and showing the circumstances on which their justification and usefulness depend, and how they have grown up, individually, out of the givens of experience. Thus their excessive authority will be broken. They will be removed if they cannot be properly legitimated, corrected if their correlation with given things be far too superfluous, or replaced if a new system can be established that we prefer for whatever reason.*
>
> ***— Albert Einstein (https://en.wikipedia.org/wiki/Albert_Einstein), Obituary for physicist and philosopher Ernst Mach (https://en.wikipedia.org/wiki/Ernst_Mach) (Nachruf auf Ernst Mach)***

This is the great curse of success in science — it turns you into an authority figure. All of a sudden you, the little fringe weirdo that you are, are regarded as an expert. People start taking you seriously. People stop questioning your work, and start defending it! What's worse, they defend your work on its reputation, rather than on how good it is.

> *To punish me for my contempt of authority, Fate has made me an authority myself.*
>
> ***— Albert Einstein (https://en.wikipedia.org/wiki/Albert_Einstein)***

If you are so unlucky as to live to see this tragedy, you should try to see your status as an authority figure as a big joke. When it comes to these things, you need to have a sense of…

Humor

> *Good design is often slightly funny. … Godel's (https://en.wikipedia.org/wiki/Kurt_G%C3%B6del) incompleteness theorem seems like a practical joke.*
>
> *— Paul Graham (http://paulgraham.com/taste.html)*

The final and — perhaps most important — virtue is humor. We see over and over again that individual scientists had wonderful, strange senses of humor.

Einstein (https://en.wikipedia.org/wiki/Albert_Einstein) in real life was not only a great politician and a great philosopher. He was al great observer of the human comedy, with a robust sense of humor. … Lindemann took him to the school to meet one of the boys who was a family friend. The boy was living in Second Chamber, in an ancient building where the walls are ornamented with marble memorials to boys who occupied the rooms in past centuries. Einstein and Lindemann wandered by mistake into the adjoining First Chamber, which had been converted from a living room to a bathroom. In First Chamber, the marble memorials were preserved, but underneath them on the walls were hooks where boys had hung their smelly football clothes. Einstein surveyed the scene for a while in silence, and then said: "Now I understand: the spirits of the departed pass over into the trousers of the living."

— Freeman Dyson (https://en.wikipedia.org/wiki/Freeman_Dyson), "Einstein as a Jew and a Philosopher", The New York Review of Books

A good sense of humor comes in many forms — wordplay, slapstick, poking fun at annoying colleagues…

It is said that the Prior of that place kept pressing Leonardo (https://en.wikipedia.org/wiki/Leonardo_da_Vinci), in a most importunate manner, to finish the work … he complained of it to the Duke, and that so warmly, that he was constrained to send for Leonardo … [Leonardo explained] that two heads were still wanting for him to paint; that of Christ, which he did not wish to seek on earth; … Next, there was wanting that of Judas, which was also troubling him, not thinking himself capable of imagining features that should represent the countenance of him who, after so many benefits received, had a mind so cruel as to resolve to betray his Lord, the Creator of the world. However, he would seek out a model for the latter; but if in the end he could not find a better, he should not want that of the importunate and tactless Prior. This thing moved the Duke wondrously to laughter.

— Giorgio Vasari (https://en.wikipedia.org/wiki/Giorgio_Vasari)

In *On the Origin of Species*, Darwin (https://en.wikipedia.org/wiki/Charles_Darwin) wrote that bumblebees are the only species that pollinates red clover. He discovered in 1862 that honeybees also pollinate red clover. Prompted by this discovery, he wrote to his friend John Lubbock, saying, "I hate myself, I hate clover, and I hate bees." In his correspondence to W. D. Fox in October of 1852, he writes of his work on Cirripedia (https://en.wikipedia.org/wiki/Barnacle), "of which creatures I am wonderfully tired: I hate a Barnacle as no man ever did before, not even a Sailor in a slow-sailing ship." Another time he wrote, "I am very poorly today and very stupid and hate everybody and everything."

Many things conspire to make humor so important. One aspect of humor is noticing a pattern that almost everyone has missed, but which is undeniable once it's been pointed out. Really good research does the same thing — you notice something that has always been there, and which is apparent in retrospect, but that no one has ever noticed before.

Once the cross-connection is made, it becomes obvious. Thomas H. Huxley (https://en.wikipedia.org/wiki/Thomas_Henry_Huxley) is supposed to have exclaimed after reading On the Origin of Species, "How stupid of me not to have thought of this."

— Isaac Asimov (https://en.wikipedia.org/wiki/Isaac_Asimov)

Making these little connections is an essential part of humor. If you train yourself to see and appreciate these little jokes in your everyday life, with friends, at the movies, etc., you will get better at seeing them in your work.

In spite of twenty-five years in Southern California, [Aldous Huxley (https://en.wikipedia.org/wiki/Aldous_Huxley)] remains an English gentleman. The scientist's habit of examining everything from every side and of turning everything upside down and inside out is also characteristic of Aldous. I remember him leafing through a copy of Transition, reading a poem in it, looking again at the title of the magazine, reflecting for a moment, then saying, "Backwards it spells NO IT ISN(T) ART."

— Igor Stravinsky (https://en.wikipedia.org/wiki/Igor_Stravinsky), Dialogues

David Ogilvy (https://en.wikipedia.org/wiki/David_Ogilvy_(businessman)) wasn't a scientist, but he was right when he said, "The best ideas come as jokes. Make your thinking as funny as possible."

One of economist Tyler Cowen (https://marginalrevolution.com/marginalrevolution/2019/07/learn-like-an-athlete-knowledge-workers-should-train.html)'s favorite questions to bug people with is, "'What is it you do to train that is comparable to a pianist practicing scales?' If you don't know the answer to that one, maybe you are doing something wrong or not doing enough." For scientists, the perfect practice is telling jokes.

László Polgár (https://en.wikipedia.org/wiki/L%C3%A1szl%C3%B3_Polg%C3%A1r) believed that geniuses are made, not born, and set out to prove it. He kept his daughters on a strict educational schedule that included studying chess for up to six hours a day. There was also a twenty-minute period dedicated to telling jokes.

— Louisa Thomas on László Polgár (https://en.wikipedia.org/wiki/L%C3%A1szl%C3%B3_Polg%C3%A1r)

Having a sense of humor also helps keep things in perspective.

When I gave a lecture in Japan, I was asked not to mention the possible re-collapse of the universe, because it might affect the stock market. However, I can re-assure anyone who is nervous about their investments that it is a bit early to sell: even if the universe does come to an end, it won't be for at least twenty billion years. By that time, maybe the GATT trade agreement will have come into effect.

— Stephen Hawking (https://en.wikipedia.org/wiki/Stephen_Hawking)

Humor keeps you from taking yourself too seriously.

The downside of my celebrity is that I cannot go anywhere in the world without being recognized. It is not enough for me to wear dark sunglasses and a wig. The wheelchair gives me away.

— Stephen Hawking (https://en.wikipedia.org/wiki/Stephen_Hawking)

Life is hard — sometimes the world is very dark. Research can be challenging. Pursuing an interest that few people understand, that sets you up against the authorities of your day, is often isolating. Scientists may discover things they would rather not have known. A sense of humor lessens the burden.

Schopenhauer's saying, that "a man can do as he will, but not will as he will," has been an inspiration to me since my youth up, and a continual consolation and unfailing well-spring of patience in the face of the hardships of life, my own and others'. This feeling mercifully mitigates the sense of responsibility which so easily becomes paralyzing, and it prevents us from taking ourselves and other people too seriously; it conduces to a view of life in which humor, above all, has its due place.

— Albert Einstein (https://en.wikipedia.org/wiki/Albert_Einstein)

Another reason to cultivate humor is that nature is really weird (https://slimemoldtimemold.com/2022/01/11/reality-is-very-weird-and-you-need-to-be-prepared-for-that/). It will always be stranger and more amusing than you expect. The only way to keep up is to try to think in jokes. If you have a good sense of humor, you will end up closer to the truth. "Wouldn't it be absurd if X were true?" you think, only to discover the next day that X is indeed true.

The most exciting phrase to hear in science, the one that heralds new discoveries, is not "Eureka" but "That's funny…"

— Isaac Asimov (https://en.wikipedia.org/wiki/Isaac_Asimov)

Finally, science is very social. If you have a good sense of humor, people will like you. You will get along with them better; you will have more fun; probably you will do better work together! Humor is worth cultivating for this reason too.

Humor is generative. It attracts unusual people and ideas, the sort that wouldn't otherwise end up in the same place together.

A deep sense of humor and an unusual ability for telling stories and jokes endeared Johnny (https://en.wikipedia.org/wiki/John_von_Neumann) even to casual acquaintances.

— Eugene Wigner, in "John von Neumann (1903 – 1957)"

Science is too important to be taken seriously. In the end, if you cannot have some fun out of your research, if you cannot see in some way how ridiculous the whole thing is — then what's the point?

When I was younger I was anti-culture, but my father had some good books around. One was a book with the old Greek play The Frogs in it, and I glanced at it one time and I saw in there that a frog talks. It was written as "brek, kek, kek." I thought, "No frog ever made a sound like that; that's a crazy way to describe it!" so I tried it, and after practicing it awhile, I realized that it's very accurately what a frog says.

So my chance glance into a book by Aristophanes turned out to be useful, later on: I could make a good frog noise at the students' ceremony for the Nobel-Prize-winners! And jumping backwards fit right in, too. So I liked that part of it; that ceremony went well.

— Richard Feynman (https://en.wikipedia.org/wiki/Richard_Feynman)

The Thrill of the Frontier

How to Find the Frontiers of Knowledge

Samo Burja June 25, 2020

Based on my lecture at Topos House, San Francisco on February 29, 2020.

Not all fields of knowledge exist yet. If you tried to study biochemistry in 1820, you'd have a lot of trouble: the field had yet to cohere. Do we think that all the biochemistries of the world have been discovered? If we did back then, we'd be wrong. For those seeking a safe career, sticking to the established fields is probably the right move. But for those interested in pushing forward the frontiers of knowledge, it will sometimes be better to work in a field that has not yet cohered, or in a field on the cusp of crystallizing. Often the best intellectual opportunities — if you want to be a breakthrough researcher — and the largest economic opportunities — if you want to build a great company — are going to be precisely in those areas about to crystallize into a new field.

If you went into biochemistry when biochemistry was emerging, your name might be in a textbook today. You didn't have to be brilliant; you just had to pick your problem well. The greatest difficulty would be in figuring out who to learn from. Before you figure out where and how to learn, you have to decide who has the knowledge you seek. When the field of biochemistry was on the cusp of crystallizing, for example, you would have looked to experts in the fields of biology and chemistry. But as an outsider, who are you to evaluate the quality of a field? Just because a field claims to exist, doesn't mean it exists.

Evaluating Existing Fields

One thing you can do is look at how a field performs on its own criteria. Take the replication crisis in academic psychology for example. In academic science, replicability has been one of the gold standards both for internal bureaucratic targets and also for the layman's understanding of the philosophy of science. Philosophy of science might not seem very important, but it is key to figuring out how we know what we know, and what is good science as opposed to bad science. There is, in fact, no consensus philosophy of science, which means that there is no canonical science of science.

But surely scientists must know how science works? Well, scientists might know how science works in the way that birds know how aerodynamics works. They know it, but not at all on a conscious level — they just fly. Even if birds could speak, their answers might be quite useless, even to baby birds. Their knowledge is not formatted for scientific understanding; it remains locked away as a type of intellectual dark matter. If you are deciding whether to enter a field in which you are not yet an expert, you need a more precise epistemic foundation than a bird's intuition, especially when deciding where expertise truly resides.

A thought experiment: say you had five experimental planes in front of you. How do you decide which one to board? Is it the wooden one, the bamboo one, the steel one, the large one with smoke-puffing engines, or the modest one made by the bicycle shop owners? The Wright brothers were bicycle shop owners, and they built a janky-looking machine. Plenty of the other early flying machines looked vastly more impressive than theirs, but they didn't fly. In the case of deciding which of these early flying machines to board, relying on institutional claims to epistemic authority would not work—not even the ones made by Harvard professors could fly.

When making such decisions, you cannot assume that the members of prestigious institutions of your society are experts simply because they claim to be experts. They would claim to be experts whether they were or weren't! In any period of human history, if you examine how institutions portray their own expertise, they always portray themselves as tremendously knowledgeable with impeccable foundations. In the rare cases where they do admit to not knowing something they propose it to be either unknowable or, to be knowable—but only if you give them more funding. The Catholic Church would claim this when it came to metaphysics and the question of salvation. Today, the Church of Scientology claims that Scientology is at the productive frontier of psychology, that they've disproven psychiatry, and that they have these cutting-edge, electronic brain-measuring devices. And so would a university cognitive science department.

We all make choices using dumb heuristics, but importantly, they are often good enough for everyday life. If you choose to study history at Oxford because the buildings look old, you'll likely do pretty well. Rules of thumb such as "Are the buildings old?" can have a valid core. The heuristic isn't a bad one for the prospective historian to use: old buildings often come with libraries well-stocked with old books. But does this heuristic hold in other cases?
Say you are deciding between Harvard and MIT for studying astrophysics, and you view Harvard as the better pick. Why do you believe this? Is it because Harvard has older-looking buildings? We can imagine that some prospective astrophysicists, too, are swayed by Harvard's historic appearance. Much of academia, in fact, functions on such cargo cult heuristics. Maybe one *should* study astrophysics at Harvard, rather than MIT. But if this is so, then it's right by accident: the "old buildings" heuristic doesn't apply to astrophysics. It is thus a mistake to rely on it—a broken clock is right for two moments a day, and wrong in all others. Your life has many important moments.

Because there is no consensus philosophy of science—no science of science—if you want to go into a more established field, you have to rely either on institutions, or your own evaluation of individual researchers. What you must do, then, is form your own judgments of the claims to intellectual authority made by particular institutions, or the quality of thinking of possibly exceptional individuals. But what if you want to go into a less established field?

Communities of Practice

For those considering entering a new field, there are several ways to acquire deep expertise. Firstly, there is the community of practice—imagine a hobbyist society or hacktivist collective, for example. People gravitate towards these communities chiefly for friendship and community, and proceed to enthuse together over their community's mechanism of practice, channeling this shared energy into competition for status, acceptance, and love. Thus, communities of practice tend to have a social pressure towards excellence. This often makes joining one one of the best ways to acquire knowledge.

History is full of examples of communities of practice, from the circles of philosophers of Ancient Greece, to the guilds of medieval Europe, to Meetup.com. The Royal Society was originally a group of bored 17th century British aristocrats who wanted to stay far away from politics in the aftermath of the English Civil war to pursue knowledge of nature together. One can imagine them showing off their fanciful etchings and astronomical instruments, or one-upping each other with exotic mineral samples brought from far-flung corners of the globe. A side effect of this competition was science: we learned things about geology, astronomy, entomology, and so on. Thanks to this, Charles Darwin had access to a dataset describing the taxonomy and habitats of insects and plants from all over the world, which provided much of the evidence for his theories of natural selection. Were that prior work not done, Darwin's theories would have been mere conjecture, difficult to establish as authoritative contributions to our understanding of the mechanisms of nature.

So seek out communities of practice; find out who is excited by what you want to learn. This community may or may not be very well-connected, but above all it should be relatively narrowly focused on the practice of some activity that its members constantly relate to. Without this focus, the community won't have highly-trained internal heuristics—better heuristics than the age of buildings!—to identify who really knows what they're talking about.

Communities of practice are not perfect, but you can almost always rely on them in some way. Some of them may be fraudulent or confused, but you're certainly better off joining one than attempting to be a pure individualist. There are likely entire communities out there centered on what you wish to study. Find them. Think about who would have the socioeconomic leisure to run such a community, and what they'd call themselves. Maybe you will find them at a university among college students, maybe on the internet. Perhaps you will find them among the modern equivalent of bored aristocrats—angel investors, perhaps. Smart, bored rich people will always find a hobby. It's only a question of what that hobby is—is it what you're searching for?

Master-Apprentice Relationships

Aside from communities of practice, another indispensable mechanism of transferring deep expertise is the master-apprentice relationship. In medieval European guilds, the master-apprentice relationship formed a strong contract. Instead of owing $90,000 of student debt and receiving a diploma, you instead would sign yourself up for service to a particular master for around seven or eight years. And at the end, they would essentially grant you their business. Their retirement plan would be *you*. Can you imagine how different your relationship with your doctoral advisor would be if you personally constituted their retirement plan? Their incentives would be much better aligned, to say the least.

A weaker, non-economic version of the master-apprentice relationship does still apply for professors and thinkers who care about their legacy. And if a field has clearly identifiable experts, it is extremely valuable to work with the best person you can, in the hopes of forming a relationship with them. You should approach them and offer to proofread their papers, or even offer to make their coffee. Getting to work with them for a few hours a day while they talk about their field could be an education well worth unpaid or underpaid labor, especially if they understand you are there because you truly care about the field, and want to learn.

You may find semi-retired prominent figures more approachable than you think. Try asking around at their institution, whether a university or a firm, to feel out your chances of meeting them. Devote some time to going to as many of their institution's relevant events as you can—maybe even crash some happy hours. As always, a cold email with a thoughtful response to one of their papers will most likely garner a response.

A little applied anthropology can go a long way. Say you wanted to figure out who Paul Graham hangs out with in London, in the hope of meeting and learning from him. In this case you should think: if I were Paul Graham, who would I hang out with in London? This is a more realistic task than it might seem. You are only a few degrees removed from everyone else on the planet. Once you find the right social circles, the key is finding someone who knows your target contact and giving them a reason to introduce you to them. At this point, your job becomes to align the incentives of the gatekeepers with your own. Show up at the right parties until you are able to go up to Paul Graham and ask, "Can I make you coffee?"

Prospective apprentices should be careful, as they are placing themselves in a position that can be economically exploited. Currently, there is no mechanism to make your mentor reward you financially. But if your payoff is knowledge, you will soon be able to reason for yourself if the relationship is worth it. It may not be. Perhaps the communication gap is too big, or the relationship too awkward. But finding a good master is worth the effort.

Functional Institutions

Finally, in addition to communities of practice and master-apprentice relationships, well-functioning institutions present you with a third option. Note that it's important to discover whether the field you're entering already exists, or whether it's about to crystallize. Only if it already exists will there be the option of joining a functional institution.

I may appear skeptical about the functionality of large institutions such as universities or newspapers at scale—my thoughts in this regard are best summarized in How To Use Bureaucracies, Functional Institutions Are The Exception, Institutional Failure As Surprise, and Intellectual Dark Matter—but I do think functional institutions exist. Some institutions work extremely well at scale. I would consider Microsoft Research to be fairly functional. Even though Microsoft is a large company, they do a pretty good job of maintaining a professional environment.

When I say professional, I do not mean "behaves properly." What I mean is "adheres to an expected social role." When I show up to Dr. Bob's office, I don't expect to deal with Bob; I expect to deal with The Doctor. The doctor is not going to behave as Bob, Alice, or Caroline might act, even if that's who the doctor is. The doctor is going to behave as The Doctor! This is essentially a human user interface. I'm not Samo. I am The Patient. And Alice is The Doctor. Professionalism is a LARP—and an important one. It might seem as natural as gravity that there are doctors and patients, but these are social roles. And we had to be educated to fit those social roles.

If you are thinking about entering an institution, you should seek a very specific culture of professionalism, one that seems to match the task at hand, rather than what amounts to mutual sabotage. If the professionalism of a large at-scale institution is well-designed, the overall incentives of institutional leadership will be aligned with the overall mission. And if such an institution deploys efficient bureaucracies, it can function well as a productive research organization. But again, functional institutions are the exception. So only institutionalize yourself—that is, enter a mostly bureaucratic environment—if you believe the institution that you are entering to be a functional one.

One way to determine if the bureaucratic mechanism is efficient is whether or not they expect you to do all of the paperwork. Inefficient, decaying bureaucracies are a little bit like dying stars: they eject most of their mass. Their internal cancerousness generates reams of excess paperwork, and they are thus incentivized to push paperwork onto the user. For example, if a public official likes you, they may ask you to fill out a few lines and send you on your way, but if they dislike you, they may give you a mound of paperwork for the exact same task. I have some experience of this growing up in Eastern Europe, but Americans can easily experience this at the local DMV.

If you experience low bureaucratic burden in a highly bureaucratized organization, that bureaucracy is working very well. It's like a machine that hums in the background, versus a machine that screeches and puffs smoke into your face. It's going to be a very visible experience, even if you don't have much other information.

Finally, check the institution's leadership. Even if it is a well-oiled machine, make sure that its roles are very clearly delineated towards the mission, rather than towards internal conflict. The leadership might not want your output and may actively steer away from your output. A good example of this dynamic can be found in Richard Feynman's critique of NASA's administrative process. After the Challenger explosion, NASA called on Feynman to find out what had gone wrong with their bureaucratic process. The issue, he found, was that the leadership didn't want to hear bad news from the engineers, because they wanted to push through as many flights as they could in order to score PR wins. And the Challenger mission, with a schoolteacher on board in an effort to demonstrate that space was to be for everyone, was an important PR flight. Leadership didn't want to hear the truth, even though the engineers themselves were still acting professionally and some parts of the NASA bureaucracy were still functioning well. The Challenger went up in flames, and the Space Shuttle remained expensive.

Conclusion

Entering a field for the first time as an autodidact, you should decide whether you want to enter into a community of practice, a master-apprentice relationship, or a functional institution. Try to figure out for each of those whether it fits the criteria. Seek out an energetic community of practice, or a good master. If you are considering joining an institution, make sure that its bureaucracy is working well, its professionalized roles are functional, and its leadership is aligned with correct output.

Leaps of faith like these can be harrowing, but with planning they need not be. Tracking existing sources of prestige and economic stability may be rational in a narrow sense, but the world of institutions in which they are embedded is prone to dysfunction. Recognizing when dysfunction has overtaken an institution is key to ensuring that we remain able to generate new knowledge about the world. History is littered with examples of collapsed civilizations that failed to do so. New scientific fields carry the promise of civilizational advancement, but they are not discovered automatically. It's up to us to find them.

Richard Hamming

``You and Your Research''

Transcription of the
Bell Communications Research Colloquium Seminar
7 March 1986

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At a seminar in the Bell Communications Research Colloquia Series, Dr. Richard W. Hamming, a Professor at the Naval Postgraduate School in Monterey, California and a retired Bell Labs scientist, gave a very interesting and stimulating talk, `You and Your Research' to an overflow audience of some 200 Bellcore staff members and visitors at the Morris Research and Engineering Center on March 7, 1986. This talk centered on Hamming's observations and research on the question ``Why do so few scientists make significant contributions and so many are forgotten in the long run?'' From his more than forty years of experience, thirty of which were at Bell Laboratories, he has made a number of direct observations, asked very pointed questions of scientists about what, how, and why they did things, studied the lives of great scientists and great contributions, and has done introspection and studied theories of creativity. The talk is about what he has learned in terms of the properties of the individual scientists, their abilities, traits, working habits, attitudes, and philosophy.

In order to make the information in the talk more widely available, the tape recording that was made of that talk was carefully transcribed. This transcription includes the discussions which followed in the question and answer period. As with any talk, the transcribed version suffers from translation as all the inflections of voice and the gestures of the speaker are lost; one must listen to the tape recording to recapture that part of the presentation. While the recording of Richard Hamming's talk was completely intelligible, that of some of the questioner's remarks were not. Where the tape recording was not intelligible I have added in parentheses my impression of the questioner's remarks. Where there was a question and I could identify the questioner, I have checked with each to ensure the accuracy of my interpretation of their remarks.

INTRODUCTION OF DR. RICHARD W. HAMMING

As a speaker in the Bell Communications Research Colloquium Series, Dr. Richard W. Hamming of the Naval Postgraduate School in Monterey, California, was introduced by Alan G. Chynoweth, Vice President, Applied Research, Bell Communications Research.

Alan G. Chynoweth: Greetings colleagues, and also to many of our former colleagues from Bell Labs who, I understand, are here to be with us today on what I regard as a particularly felicitous occasion. It gives me very great pleasure indeed to introduce to you my old friend and colleague from many many years back, Richard Hamming, or Dick Hamming as he has always been know to all of us.

Dick is one of the all time greats in the mathematics and computer science arenas, as I'm sure the audience here does not need reminding. He received his early education at the Universities of Chicago and Nebraska, and got his Ph.D. at Illinois; he then joined the Los Alamos project during the war. Afterwards, in 1946, he joined Bell Labs. And that is, of course, where I met Dick - when I joined Bell Labs in their physics research organization. In those days, we were in the habit of lunching together as a physics group, and for some reason this strange fellow from mathematics was always pleased to join us. We were always happy to have him with us because he brought so many unorthodox ideas and views. Those lunches were stimulating, I can assure you.

While our professional paths have not been very close over the years, nevertheless I've always recognized Di in the halls of Bell Labs and have always had tremendous admiration for what he was doing. I think the record speaks for itself. It is too long to go through all the details, but let me point out, for example, that he has written seven books and of those seven books which tell of various areas of mathematics and computers and coding and information theory, three are already well into their second edition. That is testimony indeed to the prolific output and the stature of Dick Hamming.

I think I last met him - it must have been about ten years ago - at a rather curious little conference in Dublin, Ireland where we were both speakers. As always, he was tremendously entertaining. Just one more example of the provocative thoughts that he comes up with: I remember him saying, ``There are wavelengths that people cannot see, there are sounds that people cannot hear, and maybe computers have thoughts that people cannot think." Well, with Dick Hamming around, we don't need a computer. I think that we are in for an extremely entertaining talk.

THE TALK: ``You and Your Research'' by Dr. Richard W. Hamming

It's a pleasure to be here. I doubt if I can live up to the Introduction. The title of my talk is, ``You and Your Research." It is not about managing research, it is about how you individually do your research. I could give a talk on the other subject - but it's not, it's about you. I'm not talking about ordinary run-of-the-mill research; I'm talking about great research. And for the sake of describing great research I'll occasionally say Nobel-Prize type of work. It doesn't have to gain the Nobel Prize, but I mean those kinds of things which we perceive are significant things. Relativity, if you want, Shannon's information theory, any number of outstanding theories - that's the kind of thing I'm talking about.

Now, how did I come to do this study? At Los Alamos I was brought in to run the computing machines which other people had got going, so those scientists and physicists could get back to business. I saw I was a stooge. I saw that although physically I was the same, they were different. And to put the thing bluntly, I was envious. I wanted to know why they were so different from me. I saw Feynman up close. I saw Fermi and Teller. I saw Oppenheimer. I saw Hans Bethe: he was my boss. I saw quite a few very capable people. I became very interested in the difference between those who do and those who might have done.

When I came to Bell Labs, I came into a very productive department. Bode was the department head at the time; Shannon was there, and there were other people. I continued examining the questions, ``Why?" and ``What is the difference?" I continued subsequently by reading biographies, autobiographies, asking people questions such as: ``How did you come to do this?" I tried to find out what are the differences. And that's what this talk is about.

Now, why is this talk important? I think it is important because, as far as I know, each of you has one life to live. Even if you believe in reincarnation it doesn't do you any good from one life to the next! Why shouldn't you do significant things in this one life, however you define significant? I'm not going to define it - you know what I mean. I will talk mainly about science because that is what I have studied. But so far as I know, and I've been told by others, much of what I say applies to many fields. Outstanding work is characterized very much the same way in most fields, but I will confine myself to science.

In order to get at you individually, I must talk in the first person. I have to get you to drop modesty and say to yourself, ``Yes, I would like to do first-class work." Our society frowns on people who set out to do really good work. You're not supposed to; luck is supposed to descend on you and you do great things by chance. Well, that's a kind of dumb thing to say. I say, why shouldn't you set out to do something significant. You don't have to tell other people, but shouldn't you say to yourself, ``Yes, I would like to do something significant."

In order to get to the second stage, I have to drop modesty and talk in the first person about what I've seen, what I've done, and what I've heard. I'm going to talk about people, some of whom you know, and I trust that when we leave, you won't quote me as saying some of the things I said.

Let me start not logically, but psychologically. I find that the major objection is that people think great science is done by luck. It's all a matter of luck. Well, consider Einstein. Note how many different things he did that were good. Was it all luck? Wasn't it a little too repetitive? Consider Shannon. He didn't do just information theory. Several years before, he did some other good things and some which are still locked up in the security of cryptography. He did many good things.

You see again and again, that it is more than one thing from a good person. Once in a while a person does only one thing in his whole life, and we'll talk about that later, but a lot of times there is repetition. I claim that luck will not cover everything. And I will cite Pasteur who said, ``Luck favors the prepared mind.'' And I think that says it the way I believe it. There is indeed an element of luck, and no, there isn't. The prepared mind sooner or later finds something important and does it. So yes, it is luck. The particular thing you do is luck, but that you do something is not.

For example, when I came to Bell Labs, I shared an office for a while with Shannon. At the same time he was doing information theory, I was doing coding theory. It is suspicious that the two of us did it at the same place and at the same time - it was in the atmosphere. And you can say, ``Yes, it was luck.'' On the other hand you can say, ``But why of all the people in Bell Labs then were those the two who did it?'' Yes, it is partly luck, and partly it is the prepared mind; but `partly' is the other thing I'm going to talk about. So, although I'll come back several more times to luck, I want to dispose of this matter of luck as being the sole criterion whether you do great work or not. I claim you have some, but not total, control over it. And I will quote, finally, Newton on the matter. Newton said, ``If others would think as hard as I did, then they would get similar results.''

One of the characteristics you see, and many people have it including great scientists, is that usually when they were young they had independent thoughts and had the courage to pursue them. For example, Einstein, somewhere around 12 or 14, asked himself the question, ``What would a light wave look like if I went with the velocity of light to look at it?'' Now he knew that electromagnetic theory says you cannot have a stationary local maximum. But if he moved along with the velocity of light, he would see a local maximum. He could see a contradiction at the age of 12, 14, or somewhere around there, that everything was not right and that the velocity of light had something peculiar. Is it luck that he finally created special relativity? Early on, he had laid down some of the pieces by thinking of the fragments. Now that's the necessary but not sufficient condition. All of these items I will talk about are both luck and not luck.

How about having lots of `brains?' It sounds good. Most of you in this room probably have more than enough brains to do first-class work. But great work is something else than mere brains. Brains are measured in various ways. In mathematics, theoretical physics, astrophysics, typically brains correlates to a great extent with the ability to manipulate symbols. And so the typical IQ test is apt to score them fairly high. On the other hand, in other fields it is something different. For example, Bill Pfann, the fellow who did zone melting, came into my office one day. He had this idea dimly in his mind about what he wanted and he had some equations. It was pretty clear to me that this man didn't know much mathematics and he wasn't really articulate. His problem seemed interesting so I took it home and did a little work. I finally showed him how to run computers so he could compute his own answers. I gave him the power to compute. He went ahead, with negligible recognition from his own department, but ultimately he has collected all the prizes in the field. Once he got well started, his shyness, his awkwardness, his inarticulateness, fell away and he became much more productive in many other ways. Certainly he became much more articulate.

And I can cite another person in the same way. I trust he isn't in the audience, i.e. a fellow named Clogston. I met him when I was working on a problem with John Pierce's group and I didn't think he had much. I asked my friends who had been with him at school, ``Was he like that in graduate school?'' ``Yes,'' they replied. Well I would have fired the fellow, but J. R. Pierce was smart and kept him on. Clogston finally did the Clogston cable. After that there was a steady stream of good ideas. One success brought him confidence and courage.

One of the characteristics of successful scientists is having courage. Once you get your courage up and believe that you can do important problems, then you can. If you think you can't, almost surely you are not going to. Courage is one of the things that Shannon had supremely. You have only to think of his major theorem. He wants to create a method of coding, but he doesn't know what to do so he makes a random code. Then he is stuck. And then he asks the impossible question, ``What would the average random code do?'' He then proves that the average code is arbitrarily good, and that therefore there must be at least one good code. Who but a man of infinite courage could have dared to think those thoughts? That is the characteristic of great scientists; they ha courage. They will go forward under incredible circumstances; they think and continue to think.

Age is another factor which the physicists particularly worry about. They always are saying that you have got to do it when you are young or you will never do it. Einstein did things very early, and all the quantum mechanic fellows were disgustingly young when they did their best work. Most mathematicians, theoretical physicists, and astrophysicists do what we consider their best work when they are young. It is not that they don't do good work in their old age but what we value most is often what they did early. On the other hand, in music, politics and literature, often what we consider their best work was done late. I don't know how whatever field you are in fits this scale, but age has some effect.

But let me say why age seems to have the effect it does. In the first place if you do some good work you will find yourself on all kinds of committees and unable to do any more work. You may find yourself as I saw Brattain when he got a Nobel Prize. The day the prize was announced we all assembled in Arnold Auditorium; all three winners got up and made speeches. The third one, Brattain, practically with tears in his eyes, said, ``I know about this Nobel-Prize effect and I am not going to let it affect me; I am going to remain good old Walter Brattain.'' Well I said to myself, ``That is nice.'' But in a few weeks I saw it was affecting him. Now he could only work on great problems.

When you are famous it is hard to work on small problems. This is what did Shannon in. After information theory, what do you do for an encore? The great scientists often make this error. They fail to continue to plant the little acorns from which the mighty oak trees grow. They try to get the big thing right off. And that isn't the way things go. So that is another reason why you find that when you get early recognition it seems to sterilize you. In fact I will give you my favorite quotation of many years. The Institute for Advanced Study in Princeton, in my opinion, has ruined more good scientists than any institution has created, judged by what they did before they came and judged by what they did after. Not that they weren't good afterwards, but they were superb before they got there and were only good afterwards.

This brings up the subject, out of order perhaps, of working conditions. What most people think are the best working conditions, are not. Very clearly they are not because people are often most productive when working conditions are bad. One of the better times of the Cambridge Physical Laboratories was when they had practically shacks - they did some of the best physics ever.

I give you a story from my own private life. Early on it became evident to me that Bell Laboratories was not going to give me the conventional acre of programming people to program computing machines in absolute binary. It was clear they weren't going to. But that was the way everybody did it. I could go to the West Coast and get a job with the airplane companies without any trouble, but the exciting people were at Bell Labs and the fellows out there in the airplane companies were not. I thought for a long while about, ``Did I want to go or not?'' and I wondered how I could get the best of two possible worlds. I finally said to myself, ``Hamming, you think the machines can do practically everything. Why can't you make them write programs?'' What appeared at first to me as a defect forced me into automatic programming very early. What appears to be a fault, often, by a change of viewpoint, turns out to be one of the greatest assets you can have. But you are not likely to think that when you first look the thing and say, ``Gee, I'm never going to get enough programmers, so how can I ever do any great programming?''

And there are many other stories of the same kind; Grace Hopper has similar ones. I think that if you look carefully you will see that often the great scientists, by turning the problem around a bit, changed a defect to an asset. For example, many scientists when they found they couldn't do a problem finally began to study why not. They then turned it around the other way and said, ``But of course, this is what it is'' and got an important result. So ideal working conditions are very strange. The ones you want aren't always the best ones for you.

Now for the matter of drive. You observe that most great scientists have tremendous drive. I worked for ten years with John Tukey at Bell Labs. He had tremendous drive. One day about three or four years after I joined, I discovered that John Tukey was slightly younger than I was. John was a genius and I clearly was not. Well I went storming into Bode's office and said, ``How can anybody my age know as much as John Tukey does?'' He leaned back in his chair, put his hands behind his head, grinned slightly, and said, ``You would be surprised Hammin how much you would know if you worked as hard as he did that many years.'' I simply slunk out of the office!

What Bode was saying was this: ``Knowledge and productivity are like compound interest.'' Given two people of approximately the same ability and one person who works ten percent more than the other, the latter will more than twice outproduce the former. The more you know, the more you learn; the more you learn, the more you can do; the more you can do, the more the opportunity - it is very much like compound interest. I don't want to give you a rate, but it is a very high rate. Given two people with exactly the same ability, the one person who manages day in and day out to get in one more hour of thinking will be tremendously more productive over a lifetime. I took Bode's remark to heart; I spent a good deal more of my time for some years trying to work a bit harder and I found, in fact, I could get more work done. I don't like to say it in front of my wife, but I did sort of neglect her sometimes; I needed to study. You have to neglect things if you intend to get what you want done. There's no question about this.

On this matter of drive Edison says, ``Genius is 99% perspiration and 1% inspiration.'' He may have been exaggerating, but the idea is that solid work, steadily applied, gets you surprisingly far. The steady application of effort with a little bit more work, *intelligently applied* is what does it. That's the trouble; drive, misapplied, doesn't get you anywhere. I've often wondered why so many of my good friends at Bell Labs who worked as hard or harder than I did, didn't have so much to show for it. The misapplication of effort is a very serious matter. Just hard work is not enough - it must be applied sensibly.

There's another trait on the side which I want to talk about; that trait is ambiguity. It took me a while to discover its importance. Most people like to believe something is or is not true. Great scientists tolerate ambiguity very well. They believe the theory enough to go ahead; they doubt it enough to notice the errors and faults so they can step forward and create the new replacement theory. If you believe too much you'll never notice the flaws; if you doubt too much you won't get started. It requires a lovely balance. But most great scientists are well aware of why their theories are true and they are also well aware of some slight misfits which don't quite fit and they don't forget it. Darwin writes in his autobiography that he found it necessary to write down every piece of evidence which appeared to contradict his beliefs because otherwise they would disappear from his mind. When you find apparent flaws you've got to be sensitive and keep track of those things, and keep an eye out for how they can be explained or how the theory can be changed to fit them. Those are often the great contributions. Great contributions are rarely done by adding another decimal place. It comes down to an emotional commitment. Most great scientists are completely committed to their problem. Those who don't become committed seldom produce outstanding, first-class work.

Now again, emotional commitment is not enough. It is a necessary condition apparently. And I think I can tell you the reason why. Everybody who has studied creativity is driven finally to saying, ``creativity comes out of your subconscious.'' Somehow, suddenly, there it is. It just appears. Well, we know very little about the subconscious; but one thing you are pretty well aware of is that your dreams also come out of your subconscious. And you're aware your dreams are, to a fair extent, a reworking of the experiences of the day. If you are deeply immersed and committed to a topic, day after day after day, your subconscious has nothing to do but work on your problem. And so you wake up one morning, or on some afternoon, and there's the answer. For those who don't get committed to their current problem, the subconscious goofs off on other things and doesn't produce the big result. So the way to manage yourself is that when you have a real important problem you don't let anything else get the center of your attention - you keep your thoughts on the problem. Keep your subconscious starved so it has to work on *your* problem, so you can sleep peacefully and get the answer in the morning, free.

Now Alan Chynoweth mentioned that I used to eat at the physics table. I had been eating with the mathematicians and I found out that I already knew a fair amount of mathematics; in fact, I wasn't learning much. The physics table was, as he said, an exciting place, but I think he exaggerated on how much I contributed. It was very interesting to listen to Shockley, Brattain, Bardeen, J. B. Johnson, Ken McKay and other people, and I was learning a lot. But unfortunately a Nobel Prize came, and a promotion came, and what was left was the dregs. Nobody wanted what was left. Well, there was no use eating with them!

Over on the other side of the dining hall was a chemistry table. I had worked with one of the fellows, Dave McCall; furthermore he was courting our secretary at the time. I went over and said, ``Do you mind if I join you?'' They can't say no, so I started eating with them for a while. And I started asking, ``What are the important problems of your field?'' And after a week or so, ``What important problems are you working on?'' And after some more time I came in one day and said, ``If what you are doing is not important, and if you don't think it is going to lead to something important, why are you at Bell Labs working on it?'' I wasn't welcomed after that; I had to find somebody else to eat with! That was in the spring.

In the fall, Dave McCall stopped me in the hall and said, ``Hamming, that remark of yours got underneath my skin. I thought about it all summer, i.e. what were the important problems in my field. I haven't changed my research,'' he says, ``but I think it was well worthwhile.'' And I said, ``Thank you Dave,'' and went on. I noticed a couple of months later he was made the head of the department. I noticed the other day he was a Member of the National Academy of Engineering. I noticed he has succeeded. I have never heard the names of any of the other fellows at that table mentioned in science and scientific circles. They were unable to ask themselves, ``What are the important problems in my field?''

If you do not work on an important problem, it's unlikely you'll do important work. It's perfectly obvious. Great scientists have thought through, in a careful way, a number of important problems in their field, and they keep an eye on wondering how to attack them. Let me warn you, `important problem' must be phrased carefully. The three outstanding problems in physics, in a certain sense, were never worked on while I was at Bell Labs. By important I mean guaranteed a Nobel Prize and any sum of money you want to mention. We didn't work on (1) time travel, (2) teleportation, and (3) antigravity. They are not important problems because we do not have an attack. It's not the consequence that makes a problem important, it is that you have a reasonable attack. That is what makes a problem important. When I say that most scientists don't work on important problems, I mean it in that sense. The average scientist, so far as I can make out, spends almost all his time working on problems which he believes will not be important and he also doesn't believe that they will lead to important problems.

I spoke earlier about planting acorns so that oaks will grow. You can't always know exactly where to be, but you can keep active in places where something might happen. And even if you believe that great science is a matter of luck, you can stand on a mountain top where lightning strikes; you don't have to hide in the valley where you're safe. But the average scientist does routine safe work almost all the time and so he (or she) doesn't produce much. It's that simple. If you want to do great work, you clearly must work on important problems, and you should have an idea.

Along those lines at some urging from John Tukey and others, I finally adopted what I called ``Great Thoughts Time.'' When I went to lunch Friday noon, I would only discuss great thoughts after that. By great thoughts I mean ones like: ``What will be the role of computers in all of AT&T?'', ``How will computers change science?'' For example, I came up with the observation at that time that nine out of ten experiments were done in the lab and one in ten on the computer. I made a remark to the vice presidents one time, that it would be reversed, i.e. nine out of ten experiments would be done on the computer and one in ten in the lab. They knew I was a crazy mathematician and had no sense of reality. I knew they were wrong and they've been proved wrong while I have been proved right. They built laboratories when they didn't need them. I saw that computers were transforming science because I spent a lot of time asking ``What will be the impact of computers on science and how can I change it?'' I asked myself, ``How is it going to change Bell Labs?'' I remarked one time, in the same address, that more than one-half of the people at Bell Labs will be interacting closely with computing machines before I leave. Well, you all have terminals now. I thought hard about where was my field going, where were the opportunities, and what were the important things to do. Let me go there so there is a chance I can do important things.

Most great scientists know many important problems. They have something between 10 and 20 important problems for which they are looking for an attack. And when they see a new idea come up, one hears them say ``Well that bears on this problem.'' They drop all the other things and get after it. Now I can tell you a horror story that was told to me but I can't vouch for the truth of it. I was sitting in an airport talking to a friend of mine from Los Alamos about how it was lucky that the fission experiment occurred over in Europe when it did because that got us working on the atomic bomb here in the US. He said ``No; at Berkeley we had gathered a bunch of data; we didn't get around to reducing it because we were building some more equipment, but if we reduced that data we would have found fission." They had it in their hands and they didn't pursue it. They came in second!

The great scientists, when an opportunity opens up, get after it and they pursue it. They drop all other things. They get rid of other things and they get after an idea because they had already thought the thing through. Their minds are prepared; they see the opportunity and they go after it. Now of course lots of times it doesn't work out, but you don't have to hit many of them to do some great science. It's kind of easy. One of the chief tricks is to live a long time!

Another trait, it took me a while to notice. I noticed the following facts about people who work with the door open or the door closed. I notice that if you have the door to your office closed, you get more work done today and tomorrow, and you are more productive than most. But 10 years later somehow you don't know quite know what problems are worth working on; all the hard work you do is sort of tangential in importance. He who works with the door open gets all kinds of interruptions, but he also occasionally gets clues as to what the world is and what might be important. Now I cannot prove the cause and effect sequence because you might say, ``The closed door is symbolic of a closed mind.'' I don't know. But I can say there is a pretty good correlation between those who work with the 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.

I want to talk on another topic. It is based on the song which I think many of you know, ``It ain't what you do, it's the way that you do it.'' I'll start with an example of my own. I was conned into doing on a digital computer, in the absolute binary days, a problem which the best analog computers couldn't do. And I was getting an answer. When I thought carefully and said to myself, ``You know, Hamming, you're going to have to file a report on this military job; after you spend a lot of money you're going to have to account for it and every analog installation is going to want the report to see if they can't find flaws in it.'' I was doing the required integration by a rather crummy method, to say the least, but I was getting the answer. And I realized that in truth the problem was not just to get the answer; it was to demonstrate for the first time, and beyond question, that I could beat the analog computer on its own ground with a digital machine. I reworked the method of solution, created a theory which was nice and elegant, and changed the way we computed the answer; the results were no different. The published report had an elegant method which was later known for years as ``Hamming's Method of Integrating Differential Equations.'' It is somewhat obsolete now, but for a while it was a very good method. By changing the problem slightly, I did important work rather than trivial work.

In the same way, when using the machine up in the attic in the early days, I was solving one problem after another after another; a fair number were successful and there were a few failures. I went home one Friday after finishing a problem, and curiously enough I wasn't happy; I was depressed. I could see life being a long sequence of one problem after another after another. After quite a while of thinking I decided, ``No, I should be in the mass production of a variable product. I should be concerned with *all* of next year's problems, not just the one in front of my face.'' By changing the question I still got the same kind of results or better, but I changed things and did important work. I attacked the major problem - How do I conquer machines and do all of next year's problems when I don't know what they are going to be? How do I prepare for it? How do I do this one so I'll be on top of it? How do I obey Newton's rule? He said, ``If I have seen further than others, it is because I've stood on the shoulders of giants.'' These days we stand on each other's feet!

You should do your job in such a fashion that others can build on top of it, so they will indeed say, ``Yes, I've stood on so and so's shoulders and I saw further.'' The essence of science is cumulative. By changing a problem slightly you can often do great work rather than merely good work. Instead of attacking isolated problems, I made the resolution that I would never again solve an isolated problem except as characteristic of a class.

Now if you are much of a mathematician you know that the effort to generalize often means that the solution is simple. Often by stopping and saying, ``This is the problem he wants but this is characteristic of so and so. Yes, I can attack the whole class with a far superior method than the particular one because I was earlier embedded in needless detail." The business of abstraction frequently makes things simple. Furthermore, I filed away the methods and prepared for the future problems.

To end this part, I'll remind you, ``It is a poor workman who blames his tools - the good man gets on with the job, given what he's got, and gets the best answer he can." And I suggest that by altering the problem, by looking at the thing differently, you can make a great deal of difference in your final productivity because you can either do it in such a fashion that people can indeed build on what you've done, or you can do it in such a fashion that the next person has to essentially duplicate again what you've done. It isn't just a matter of the job, it's the way you write the report, the way you write the paper, the whole attitude. It's just as easy to do a broad, general job as one very special case. And it's much more satisfying and rewarding!

I have now come down to a topic which is very distasteful; it is not sufficient to do a job, you have to sell it. `Selling' to a scientist is an awkward thing to do. It's very ugly; you shouldn't have to do it. The world is supposed to be waiting, and when you do something great, they should rush out and welcome it. But the fact is everyone is busy with their own work. You must present it so well that they will set aside what they are doing, look at what you've done, read it, and come back and say, ``Yes, that was good." I suggest that when you open a journal, as you turn the pages, you ask why you read some articles and not others. You had better write your report so when it is published in the Physical Review, or wherever else you want it, as the readers are turning the pages they won't just turn your pages but they will stop and read yours. If they don't stop and read it, you won't get credit.

There are three things you have to do in selling. You have to learn to write clearly and well so that people will read it, you must learn to give reasonably formal talks, and you also must learn to give informal talks. We had a lot of so-called `back room scientists.' In a conference, they would keep quiet. Three weeks later after a decision was made they filed a report saying why you should do so and so. Well, it was too late. They would not stand up right in the middle of a hot conference, in the middle of activity, and say, ``We should do this for these reasons." You need to master that form of communication as well as prepared speeches.

When I first started, I got practically physically ill while giving a speech, and I was very, very nervous. I realized I either had to learn to give speeches smoothly or I would essentially partially cripple my whole career. The first time IBM asked me to give a speech in New York one evening, I decided I was going to give a really good speech, a speech that was wanted, not a technical one but a broad one, and at the end if they liked it, I'd quietly say, ``Any time you want one I'll come in and give you one." As a result, I got a great deal of practice giving speeches to a limited audience and I got over being afraid. Furthermore, I could also then study what methods were effective and what were ineffective.

While going to meetings I had already been studying why some papers are remembered and most are not. The technical person wants to give a highly limited technical talk. Most of the time the audience wants a broad general talk and wants much more survey and background than the speaker is willing to give. As a result, many talks are ineffective. The speaker names a topic and suddenly plunges into the details he's solved. Few people in the audience may follow. You should paint a general picture to say why it's important, and then slowly give a sketch of what was done. Then a larger number of people will say, ``Yes, Joe has done that," or ``Mary has done that; I really see where it is; yes, Mary really gave a good talk; I understand what Mary has done." The tendency is to give a highly restricted, safe talk; this is usually ineffective. Furthermore, many talks are filled with far too much information. So I say this idea of selling is obvious.

Let me summarize. You've got to work on important problems. I deny that it is all luck, but I admit there is a fair element of luck. I subscribe to Pasteur's ``Luck favors the prepared mind." I favor heavily what I did. Friday afternoons for years - great thoughts only - means that I committed 10% of my time trying to understand the bigger problems in the field, i.e. what was and what was not important. I found in the early days I had believed `this' and yet had spent all week marching in `that' direction. It was kind of foolish. If I really believe the action is over there, why do I march in this direction? I either had to change my goal or change what I did. So I changed something I did and I marched in the direction I thought was important. It's that easy.

Now you might tell me you haven't got control over what you have to work on. Well, when you first begin, yo may not. But once you're moderately successful, there are more people asking for results than you can deliver and you have some power of choice, but not completely. I'll tell you a story about that, and it bears on the subject of educating your boss. I had a boss named Schelkunoff; he was, and still is, a very good friend of mine. Some military person came to me and demanded some answers by Friday. Well, I had already dedicated my computing resources to reducing data on the fly for a group of scientists; I was knee deep in short, small, important problems. This military person wanted me to solve his problem by the end of the day on Friday. I said, ``No, I'll give it to you Monday. I can work on it over the weekend. I'm not going to do it now.'' He goes down to my boss, Schelkunoff, and Schelkunoff says, ``You must run this for him; he's got to have it by Friday.'' I tell him, ``Why do I?''; he says, ``You have to.'' I said, ``Fine, Sergei, but you're sitting in your office Friday afternoon catching the late bus home to watch as this fellow walks out that door.'' I gave the military person the answers late Friday afternoon. I then went to Schelkunoff's office and sat down; as the man goes out I say, ``You see Schelkunoff, this fellow has nothing under his arm; but I gave him the answers.'' On Monday morning Schelkunoff called him up and said, ``Did you come in to work over the weekend?'' I could hear, as it were, a pause as the fellow ran through his mind of what was going to happen; but he knew he would have had to sign in, and he'd better not say he had when he hadn't, so he said he hadn't. Ever after that Schelkunoff said, ``You set your deadlines; you can change them.''

One lesson was sufficient to educate my boss as to why I didn't want to do big jobs that displaced exploratory research and why I was justified in not doing crash jobs which absorb all the research computing facilities. I wanted instead to use the facilities to compute a large number of small problems. Again, in the early days, I was limited in computing capacity and it was clear, in my area, that a ``mathematician had no use for machines.'' But I needed more machine capacity. Every time I had to tell some scientist in some other area, ``No I can't; I haven't the machine capacity,'' he complained. I said ``Go tell *your* Vice President that Hamming needs more computing capacity.'' After a while I could see what was happening up there at the top; many people said to my Vice President, ``Your man needs more computing capacity.'' I got it!

I also did a second thing. When I loaned what little programming power we had to help in the early days of computing, I said, ``We are not getting the recognition for our programmers that they deserve. When you publish a paper you will thank that programmer or you aren't getting any more help from me. That programmer is going to be thanked by name; she's worked hard.'' I waited a couple of years. I then went through a year of BSTJ articles and counted what fraction thanked some programmer. I took it into the boss and said, ``That's the central role computing is playing in Bell Labs; if the BSTJ is important, that's how important computing is.'' He had to give in. You can educate your bosses. It's a hard job. In this talk I'm only viewing from the bottom up; I'm not viewing from the top down. But I am telling you how you can get what you want in spite of top management. You have to sell your ideas there also.

Well I now come down to the topic, ``Is the effort to be a great scientist worth it?'' To answer this, you must ask people. When you get beyond their modesty, most people will say, ``Yes, doing really first-class work, and knowing it, is as good as wine, women and song put together,'' or if it's a woman she says, ``It is as good as wine, men and song put together.'' And if you look at the bosses, they tend to come back or ask for reports, trying to participate in those moments of discovery. They're always in the way. So evidently those who have done it, want to do it again. But it is a limited survey. I have never dared to go out and ask those who didn't do great work how they felt about the matter. It's a biased sample, but I still think it is worth the struggle. I think it is very definitely worth the struggle to try and do first-class work because the truth is, the value is in the struggle more than it is in the result. The struggle to make something of yourself seems to be worthwhile in itself. The success and fame are sort of dividends, in my opinion.

I've told you how to do it. It is so easy, so why do so many people, with all their talents, fail? For example, my opinion, to this day, is that there are in the mathematics department at Bell Labs quite a few people far more able and far better endowed than I, but they didn't produce as much. Some of them did produce more than I did; Shannon produced more than I did, and some others produced a lot, but I was highly productive against a lot of other fellows who were better equipped. Why is it so? What happened to them? Why do so many of the people who have great promise, fail?

Well, one of the reasons is drive and commitment. The people who do great work with less ability but who are committed to it, get more done that those who have great skill and dabble in it, who work during the day and go home and do other things and come back and work the next day. They don't have the deep commitment that is apparently necessary for really first-class work. They turn out lots of good work, but we were talking, remember, about first-class work. There is a difference. Good people, very talented people, almost always turn out good work. We're talking about the outstanding work, the type of work that gets the Nobel Prize and gets recognition.

The second thing is, I think, the problem of personality defects. Now I'll cite a fellow whom I met out in Irvine. He had been the head of a computing center and he was temporarily on assignment as a special assistant to the president of the university. It was obvious he had a job with a great future. He took me into his office one time and showed me his method of getting letters done and how he took care of his correspondence. He pointed out how inefficient the secretary was. He kept all his letters stacked around there; he knew where everything was. And he would, on his word processor, get the letter out. He was bragging how marvelous it was and how he could get so much more work done without the secretary's interference. Well, behind his back, I talked to the secretary. The secretary said, ``Of course I can't help him; I don't get his mail. He won't give me the stuff to log in; I don't know where he puts it on the floor. Of course I can't help him.'' So I went to him and said, ``Look, if you adopt the present method and do what you can do single-handedly, you can go just that far and no farther than you can do single-handedly. If you will learn to work with the system, you can go as far as the system will support you.'' And, he never went any further. He had his personality defect of wanting total control and was not willing to recognize that you need the support of the system.

You find this happening again and again; good scientists will fight the system rather than learn to work with the system and take advantage of all the system has to offer. It has a lot, if you learn how to use it. It takes patience, but you can learn how to use the system pretty well, and you can learn how to get around it. After all, if you want a decision `No', you just go to your boss and get a `No' easy. If you want to do something, don't ask, do it. Present him with an accomplished fact. Don't give him a chance to tell you `No'. But if you want a `No', it's easy to get a `No'.

Another personality defect is ego assertion and I'll speak in this case of my own experience. I came from Los Alamos and in the early days I was using a machine in New York at 590 Madison Avenue where we merely rented time. I was still dressing in western clothes, big slash pockets, a bolo and all those things. I vaguely noticed that I was not getting as good service as other people. So I set out to measure. You came in and you waited for your turn; I felt I was not getting a fair deal. I said to myself, ``Why? No Vice President at IBM said, `Give Hamming a bad time'. It is the secretaries at the bottom who are doing this. When a slot appears, they'll rush to find someone to slip in, but they go out and find somebody else. Now, why? I haven't mistreated them.'' Answer, I wasn't dressing the way they felt somebody in that situation should. It came down to just that - I wasn't dressing properly. I had to make the decision - was I going to assert my ego and dress the way I wanted to and have it steadily drain my effort from my professional life, or was I going to appear to conform better? I decided I would make an effort to appear to conform properly. The moment I did, I got much better service. And now, as an old colorful character, I get better service than other people.

You should dress according to the expectations of the audience spoken to. If I am going to give an address at the MIT computer center, I dress with a bolo and an old corduroy jacket or something else. I know enough not to let my clothes, my appearance, my manners get in the way of what I care about. An enormous number of scientists feel they must assert their ego and do their thing their way. They have got to be able to do this, that, or the other thing, and they pay a steady price.

John Tukey almost always dressed very casually. He would go into an important office and it would take a long time before the other fellow realized that this is a first-class man and he had better listen. For a long time John has had to overcome this kind of hostility. It's wasted effort! I didn't say you should conform; I said ``The *appearance of conforming* gets you a long way.'' If you chose to assert your ego in any number of ways, ``I am going to do it my way,'' you pay a small steady price throughout the whole of your professional career. And this, over a whole lifetime, adds up to an enormous amount of needless trouble.

By taking the trouble to tell jokes to the secretaries and being a little friendly, I got superb secretarial help. Fo instance, one time for some idiot reason all the reproducing services at Murray Hill were tied up. Don't ask me how, but they were. I wanted something done. My secretary called up somebody at Holmdel, hopped the company car, made the hour-long trip down and got it reproduced, and then came back. It was a payoff for the times I had made an effort to cheer her up, tell her jokes and be friendly; it was that little extra work that later paid off for me. By realizing you have to use the system and studying how to get the system to do your work, you learn how to adapt the system to your desires. Or you can fight it steadily, as a small undeclared war, for the whole of your life.

And I think John Tukey paid a terrible price needlessly. He was a genius anyhow, but I think it would have been far better, and far simpler, had he been willing to conform a little bit instead of ego asserting. He is going to dress the way he wants all of the time. It applies not only to dress but to a thousand other things; people will continue to fight the system. Not that you shouldn't occasionally!

When they moved the library from the middle of Murray Hill to the far end, a friend of mine put in a request for a bicycle. Well, the organization was not dumb. They waited awhile and sent back a map of the grounds saying, ``Will you please indicate on this map what paths you are going to take so we can get an insurance policy covering you.'' A few more weeks went by. They then asked, ``Where are you going to store the bicycle and how will it be locked so we can do so and so.'' He finally realized that of course he was going to be red-taped to death so he gave in. He rose to be the President of Bell Laboratories.

Barney Oliver was a good man. He wrote a letter one time to the IEEE. At that time the official shelf space at Bell Labs was so much and the height of the IEEE Proceedings at that time was larger; and since you couldn't change the size of the official shelf space he wrote this letter to the IEEE Publication person saying, ``Since so many IEEE members were at Bell Labs and since the official space was so high the journal size should be changed.'' He sent it for his boss's signature. Back came a carbon with his signature, but he still doesn't know whether the original was sent or not. I am not saying you shouldn't make gestures of reform. I am saying that my study of able people is that they don't get themselves *committed* to that kind of warfare. They play it a little bit and drop it and get on with their work.

Many a second-rate fellow gets caught up in some little twitting of the system, and carries it through to warfare. He expends his energy in a foolish project. Now you are going to tell me that somebody has to change the system. I agree; somebody's has to. Which do you want to be? The person who changes the system or the person who does first-class science? Which person is it that you want to be? Be clear, when you fight the system and struggle with it, what you are doing, how far to go out of amusement, and how much to waste your effort fighting the system. My advice is to let somebody else do it and you get on with becoming a first-class scientist. Very few of you have the ability to both reform the system *and* become a first-class scientist.

On the other hand, we can't always give in. There are times when a certain amount of rebellion is sensible. I have observed almost all scientists enjoy a certain amount of twitting the system for the sheer love of it. What it comes down to basically is that you cannot be original in one area without having originality in others. Originality is being different. You can't be an original scientist without having some other original characteristics. But many a scientist has let his quirks in other places make him pay a far higher price than is necessary for the ego satisfaction he or she gets. I'm not against all ego assertion; I'm against some.

Another fault is anger. Often a scientist becomes angry, and this is no way to handle things. Amusement, yes, anger, no. Anger is misdirected. You should follow and cooperate rather than struggle against the system all the time.

Another thing you should look for is the positive side of things instead of the negative. I have already given you several examples, and there are many, many more; how, given the situation, by changing the way I looked at it, I converted what was apparently a defect to an asset. I'll give you another example. I am an egotistical person; there is no doubt about it. I knew that most people who took a sabbatical to write a book, didn't finish it on time. So before I left, I told all my friends that when I come back, that book was going to be done! Yes, I would have it done - I'd have been ashamed to come back without it! I used my ego to make myself behave the way I wanted to. I bragged about something so I'd have to perform. I found out many times, like a cornered rat in a real trap was surprisingly capable. I have found that it paid to say, ``Oh yes, I'll get the answer for you Tuesday,'' not having any idea how to do it. By Sunday night I was really hard thinking on how I was going to deliver by Tuesday. I often put my pride on the line and sometimes I failed, but as I said, like a cornered rat I'm surprised how often I did a good job. I think you need to learn to use yourself. I think you need to know how to convert a situation from one view to another which would increase the chance of success.

Now self-delusion in humans is very, very common. There are enumerable ways of you changing a thing and kidding yourself and making it look some other way. When you ask, ``Why didn't you do such and such,'' the person has a thousand alibis. If you look at the history of science, usually these days there are 10 people right there ready, and we pay off for the person who is there first. The other nine fellows say, ``Well, I had the idea but I didn't do it and so on and so on.'' There are so many alibis. Why weren't you first? Why didn't you do it right? Don't try an alibi. Don't try and kid yourself. You can tell other people all the alibis you want. I don't mind. But to yourself try to be honest.

If you really want to be a first-class scientist you need to know yourself, your weaknesses, your strengths, and your bad faults, like my egotism. How can you convert a fault to an asset? How can you convert a situation where you haven't got enough manpower to move into a direction when that's exactly what you need to do? I say again that I have seen, as I studied the history, the successful scientist changed the viewpoint and what was a defect became an asset.

In summary, I claim that some of the reasons why so many people who have greatness within their grasp don't succeed are: they don't work on important problems, they don't become emotionally involved, they don't try and change what is difficult to some other situation which is easily done but is still important, and they keep giving themselves alibis why they don't. They keep saying that it is a matter of luck. I've told you how easy it is; furthermore I've told you how to reform. Therefore, go forth and become great scientists!

(End of the formal part of the talk.)

DISCUSSION - QUESTIONS AND ANSWERS

A. G. Chynoweth: Well that was 50 minutes of concentrated wisdom and observations accumulated over a fantastic career; I lost track of all the observations that were striking home. Some of them are very very timely. One was the plea for more computer capacity; I was hearing nothing but that this morning from several people, over and over again. So that was right on the mark today even though here we are 20 - 30 years after when you were making similar remarks, Dick. I can think of all sorts of lessons that all of us can draw from your talk. And for one, as I walk around the halls in the future I hope I won't see as many closed doors in Bellcore. That was one observation I thought was very intriguing.

Thank you very, very much indeed Dick; that was a wonderful recollection. I'll now open it up for questions. I'm sure there are many people who would like to take up on some of the points that Dick was making.

Hamming: First let me respond to Alan Chynoweth about computing. I had computing in research and for 10 years I kept telling my management, ``Get that !&@#% machine out of research. We are being forced to run problems all the time. We can't do research because were too busy operating and running the computing machines.'' Finally the message got through. They were going to move computing out of research to someplace else. I was persona non grata to say the least and I was surprised that people didn't kick my shins because everybody was having their toy taken away from them. I went in to Ed David's office and said, ``Look Ed, you've got to give your researchers a machine. If you give them a great big machine, we'll be back in the same trouble we were before, so busy keeping it going we can't think. Give them the smallest machine you can because they are very able people. They will learn how to do things on a small machine instead of mass computing.'' As far as I'm concerned, that's how UNIX arose. We gave them a moderately small machine and they decided to make it do great things. They had to come up with a system to do it on. It is called UNIX!

A. G. Chynoweth: I just have to pick up on that one. In our present environment, Dick, while we wrestle with some of the red tape attributed to, or required by, the regulators, there is one quote that one exasperated AVP came up with and I've used it over and over again. He growled that, ``UNIX was never a deliverable!''

Question: What about personal stress? Does that seem to make a difference?

Hamming: Yes, it does. If you don't get emotionally involved, it doesn't. I had incipient ulcers most of the years that I was at Bell Labs. I have since gone off to the Naval Postgraduate School and laid back somewhat, and now my health is much better. But if you want to be a great scientist you're going to have to put up with stress. You can lead a nice life; you can be a nice guy or you can be a great scientist. But nice guys end last, is what Leo Durocher said. If you want to lead a nice happy life with a lot of recreation and everything else, you'll lead a nice life.

Question: The remarks about having courage, no one could argue with; but those of us who have gray hairs or who are well established don't have to worry too much. But what I sense among the young people these days is a real concern over the risk taking in a highly competitive environment. Do you have any words of wisdom on this?

Hamming: I'll quote Ed David more. Ed David was concerned about the general loss of nerve in our society. It does seem to me that we've gone through various periods. Coming out of the war, coming out of Los Alamos where we built the bomb, coming out of building the radars and so on, there came into the mathematics department, and the research area, a group of people with a lot of guts. They've just seen things done; they've just won a war which was fantastic. We had reasons for having courage and therefore we did a great deal. I can't arrange that situation to do it again. I cannot blame the present generation for not having it, but I agree with what you say; I just cannot attach blame to it. It doesn't seem to me they have the desire for greatness; they lack the courage to do it. But we had, because we were in a favorable circumstance to have it; we just came through a tremendously successful war. In the war we were looking very, very bad for a long while; it was a very desperate struggle as you well know. And our success, I think, gave us courage and self confidence; that's why you see, beginning in the late forties through the fifties, a tremendous productivity at the labs which was stimulated from the earlier times. Because many of us were earlier forced to learn other things - we were forced to learn the things we didn't want to learn, we were forced to have an open door - and then we could exploit those things we learned. It is true, and I can't do anything about it; I cannot blame the present generation either. It's just a fact.

Question: Is there something management could or should do?

Hamming: Management can do very little. If you want to talk about managing research, that's a totally different talk. I'd take another hour doing that. This talk is about how the individual gets very successful research done in spite of anything the management does or in spite of any other opposition. And how do you do it? Just as I observe people doing it. It's just that simple and that hard!

Question: Is brainstorming a daily process?

Hamming: Once that was a very popular thing, but it seems not to have paid off. For myself I find it desirable to talk to other people; but a session of brainstorming is seldom worthwhile. I do go in to strictly talk to somebody and say, ``Look, I think there has to be something here. Here's what I think I see ...'' and then begin talking back and forth. But you want to pick capable people. To use another analogy, you know the idea called the `critical mass.' If you have enough stuff you have critical mass. There is also the idea I used to call `sound absorbers'. When you get too many sound absorbers, you give out an idea and they merely say, ``Yes, yes, yes.'' What you want to do is get that critical mass in action; ``Yes, that reminds me of so and so,'' or, ``Have you thought about that or this?'' When you talk to other people, you want to get rid of those sound absorbers who are nice people but merely say, ``Oh yes,'' and to find those who will stimulate you right back.

For example, you couldn't talk to John Pierce without being stimulated very quickly. There were a group of other people I used to talk with. For example there was Ed Gilbert; I used to go down to his office regularly and ask him questions and listen and come back stimulated. I picked my people carefully with whom I did or whom I didn't brainstorm because the sound absorbers are a curse. They are just nice guys; they fill the whole space a they contribute nothing except they absorb ideas and the new ideas just die away instead of echoing on. Yes, I find it necessary to talk to people. I think people with closed doors fail to do this so they fail to get their ideas sharpened, such as ``Did you ever notice something over here?'' I never knew anything about it - I can go over and look. Somebody points the way. On my visit here, I have already found several books that I must read when I get home. I talk to people and ask questions when I think they can answer me and give me clues that I do not know about. I go out and look!

Question: What kind of tradeoffs did you make in allocating your time for reading and writing and actually doing research?

Hamming: I believed, in my early days, that you should spend at least as much time in the polish and presentation as you did in the original research. Now at least 50% of the time must go for the presentation. It's a big, big number.

Question: How much effort should go into library work?

Hamming: It depends upon the field. I will say this about it. There was a fellow at Bell Labs, a very, very, smart guy. He was always in the library; he read everything. If you wanted references, you went to him and he gave you all kinds of references. But in the middle of forming these theories, I formed a proposition: there would be no effect named after him in the long run. He is now retired from Bell Labs and is an Adjunct Professor. He was very valuable; I'm not questioning that. He wrote some very good Physical Review articles; but there's no effect named after him because he read too much. If you read all the time what other people have done you will think the way they thought. If you want to think new thoughts that are different, then do what a lot of creative people do - get the problem reasonably clear and then refuse to look at any answers until you've thought the problem through carefully how you would do it, how you could slightly change the problem to be the correct one. So yes, you need to keep up. You need to keep up more to find out what the problems are than to read to find the solutions. The reading is necessary to know what is going on and what is possible. But reading to get the solutions does not seem to be the way to do great research. So I'll give you two answers. You read; but it is not the amount, it is the way you read that counts.

Question: How do you get your name attached to things?

Hamming: By doing great work. I'll tell you the hamming window one. I had given Tukey a hard time, quite a few times, and I got a phone call from him from Princeton to me at Murray Hill. I knew that he was writing up power spectra and he asked me if I would mind if he called a certain window a ``Hamming window.'' And I said to him, ``Come on, John; you know perfectly well I did only a small part of the work but you also did a lot.'' He said, ``Yes, Hamming, but you contributed a lot of small things; you're entitled to some credit.'' So he called it the hamming window. Now, let me go on. I had twitted John frequently about true greatness. I said true greatness is when your name is like ampere, watt, and fourier - when it's spelled with a lower case letter. That's how the hamming window came about.

Question: Dick, would you care to comment on the relative effectiveness between giving talks, writing papers, and writing books?

Hamming: In the short-haul, papers are very important if you want to stimulate someone tomorrow. If you want to get recognition long-haul, it seems to me writing books is more contribution because most of us need orientation. In this day of practically infinite knowledge, we need orientation to find our way. Let me tell you what infinite knowledge is. Since from the time of Newton to now, we have come close to doubling knowledge every 17 years, more or less. And we cope with that, essentially, by specialization. In the next 340 years at that rate, there will be 20 doublings, i.e. a million, and there will be a million fields of specialty for every one field now. It isn't going to happen. The present growth of knowledge will choke itself off until we get different tools. I believe that books which try to digest, coordinate, get rid of the duplication, get rid of the less fruitful methods and present the underlying ideas clearly of what we know now, will be the things the future generations will value. Public talks are necessary; private talks are necessary; written papers are necessary. But I am inclined to believe that, in the long-haul, books which leave out what's not essential are more important than books which tell you everything because you don't want to know everything. I don't want to know that much about penguins is the usual reply. You just want to know the essence.

Question: You mentioned the problem of the Nobel Prize and the subsequent notoriety of what was done to some of the careers. Isn't that kind of a much more broad problem of fame? What can one do?

Hamming: Some things you could do are the following. Somewhere around every seven years make a significant, if not complete, shift in your field. Thus, I shifted from numerical analysis, to hardware, to software, and so on, periodically, because you tend to use up your ideas. When you go to a new field, you have to start over as a baby. You are no longer the big mukity muk and you can start back there and you can start planting those acorns which will become the giant oaks. Shannon, I believe, ruined himself. In fact when he left Bell Labs, I said, ``That's the end of Shannon's scientific career.'' I received a lot of flak from my friends who said that Shannon was just as smart as ever. I said, ``Yes, he'll be just as smart, but that's the end of his scientific career,'' and I truly believe it was.

You have to change. You get tired after a while; you use up your originality in one field. You need to get something nearby. I'm not saying that you shift from music to theoretical physics to English literature; I mean within your field you should shift areas so that you don't go stale. You couldn't get away with forcing a change every seven years, but if you could, I would require a condition for doing research, being that you *will* change your field of research every seven years with a reasonable definition of what it means, or at the end of 10 years, management has the right to compel you to change. I would insist on a change because I'm serious. What happens to the old fellows is that they get a technique going; they keep on using it. They were marching in that direction which was right then, but the world changes. There's the new direction; but the old fellows are still marching in their former direction.

You need to get into a new field to get new viewpoints, and *before* you use up all the old ones. You can do something about this, but it takes effort and energy. It takes courage to say, ``Yes, I will give up my great reputation.'' For example, when error correcting codes were well launched, having these theories, I said, ``Hamming, you are going to quit reading papers in the field; you are going to ignore it completely; you are going to try and do something else other than coast on that.'' I deliberately refused to go on in that field. I wouldn't even read papers to try to force myself to have a chance to do something else. I managed myself, which is what I'm preaching in this whole talk. Knowing many of my own faults, I manage myself. I have a lot of faults, so I've got a lot of problems, i.e. a lot of possibilities of management.

Question: Would you compare research and management?

Hamming: If you want to be a great researcher, you won't make it being president of the company. If you want to be president of the company, that's another thing. I'm not against being president of the company. I just don't want to be. I think Ian Ross does a good job as President of Bell Labs. I'm not against it; but you have to be clear on what you want. Furthermore, when you're young, you may have picked wanting to be a great scientist, but as you live longer, you may change your mind. For instance, I went to my boss, Bode, one day and said, ``Why did you ever become department head? Why didn't you just be a good scientist?'' He said, ``Hamming, I had a vision of what mathematics should be in Bell Laboratories. And I saw if that vision was going to be realized, *I* had to make it happen; *I* had to be department head.'' When your vision of what you want to do is what you can do single-handedly, then you should pursue it. The day your vision, what you think needs to be done, is bigger than what you can do single-handedly, then you have to move toward management. And the bigger the vision is, the farther in management you have to go. If you have a vision of what the whole laboratory should be, or the whole Bell System, you have to get there to make it happen. You can't make it happen from the bottom very easily. It depends upon what goals and what desires you have. And as they change in life, you have to be prepared to change. I chose to avoid management because I preferred to do what I could do single-handedly. But that's the choice that I made, and it is biased. Each person is entitled to their choice. Keep an open mind. But when you do choose a path, for heaven's sake be aware of what you have done and the choice you have made. Don't try to do both sides.

Question: How important is one's own expectation or how important is it to be in a group or surrounded by people who expect great work from you?

Hamming: At Bell Labs everyone expected good work from me - it was a big help. Everybody expects you to do a good job, so you do, if you've got pride. I think it's very valuable to have first-class people around. I sought out the best people. The moment that physics table lost the best people, I left. The moment I saw that the same was true of the chemistry table, I left. I tried to go with people who had great ability so I could learn from them and who would expect great results out of me. By deliberately managing myself, I think I did much better than laissez faire.

Question: You, at the outset of your talk, minimized or played down luck; but you seemed also to gloss over the circumstances that got you to Los Alamos, that got you to Chicago, that got you to Bell Laboratories.

Hamming: There was some luck. On the other hand I don't know the alternate branches. Until you can say that the other branches would not have been equally or more successful, I can't say. Is it luck the particular thing you do? For example, when I met Feynman at Los Alamos, I knew he was going to get a Nobel Prize. I didn't know what for. But I knew darn well he was going to do great work. No matter what directions came up in the future, this man would do great work. And sure enough, he did do great work. It isn't that you only do a little great work at this circumstance and that was luck, there are many opportunities sooner or later. There are a whole pail full of opportunities, of which, if you're in this situation, you seize one and you're great over there instead of over here. There is an element of luck, yes and no. Luck favors a prepared mind; luck favors a prepared person. It is not guaranteed; I don't guarantee success as being absolutely certain. I'd say luck changes the odds, but there is some definite control on the part of the individual.

Go forth, then, and do great work!

(End of the General Research Colloquium Talk.)

BIOGRAPHICAL SKETCH OF RICHARD HAMMING

Richard W. Hamming was born February 11, 1915, in Chicago, Illinois. His formal education was marked by the following degrees (all in mathematics): B.S. 1937, University of Chicago; M.A. 1939, University of Nebraska; and Ph.D. 1942, University of Illinois. His early experience was obtained at Los Alamos 1945-1946, i.e. at the close of World War II, where he managed the computers used in building the first atomic bomb. From there he went directly to Bell Laboratories where he spent thirty years in various aspects of computing, numerical analysis, and management of computing, i.e. 1946-1976. On July 23, 1976 he `moved his office' to the Naval Postgraduate School in Monterey, California where he taught, supervised research, and wrote books.

While at Bell Laboratories, he took time to teach in Universities, sometimes locally and sometimes on a full sabbatical leave; these activities included visiting professorships at New York University, Princeton University (Statistics), City College of New York, Stanford University, 1960-61, Stevens Institute of Technology (Mathematics), and the University of California, Irvine, 1970-71.

Richard Hamming has received a number of awards which include: Fellow, IEEE, 1968; the ACM Turing Prize, 1968; the IEEE Emanuel R. Piore Award, 1979; Member, National Academy of Engineering, 1980; and the Harold Pender Award, U. Penn., 1981. In 1987 a major IEEE award was named after him, namely the Richard W. Hamming Medal, ``For exceptional contributions to information sciences and systems''; fittingly, he was also the first recipient of this award, 1988. In 1996 in Munich he received the prestigious $130,000 Eduard Rhein Award for Achievement in Technology for his work on error correcting codes. He was both a Founder and Past President of ACM, and a Vice Pres. of the AAAS Mathematics Section.

He is probably best known for his pioneering work on error-correcting codes, his work on integrating differential equations, and the spectral window which bears his name. His extensive writing has included a number of important, pioneering, and highly regarded books. These are:

- *Numerical Methods for Scientists and Engineers*, McGraw-Hill, 1962; Second edition 1973; Reprinted Dover 1985; Translated into Russian.
- *Calculus and the Computer Revolution*, Houghton-Mifflin, 1968.
- *Introduction to Applied Numerical Analysis*, McGraw-Hill, 1971.
- *Computers and Society*, McGraw-Hill, 1972.
- *Digital Filters*, Prentice-Hall, 1977; Second edition 1983; Third edition 1989; translated into several European languages.
- *Coding and Information Theory*, Prentice-Hall, 1980; Second edition 1986.
- *Methods of Mathematics Applied to Calculus, Probability and Statistics*, Prentice-Hall, 1985.
- *The Art of Probability for Scientists and Engineers*, Addison-Wesley, 1991.
- *The Art of Doing Science and Engineering: Learning to Learn*, Gordon and Breach, 1997.

He continued a very active life as Adjunct Professor, teaching and writing in the Mathematics and Computer Science Departments at the Naval Postgraduate School, Monterey, California for another twenty-one years before he retired to become Professor Emeritus in 1997. He was still teaching a course in the fall of 1997. He passed away unexpectedly on January 7, 1998.

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J. F. Kaiser

The Call of Great Work

How to do Great Work

By Paul Graham || July 2023

If you collected lists of techniques for doing great work in a lot of different fields, what would the intersection look like? I decided to find out by making it.

Partly my goal was to create a guide that could be used by someone working in any field. But I was also curious about the shape of the intersection. And one thing this exercise shows is that it does have a definite shape; it's not just a point labelled "work hard."

The following recipe assumes you're very ambitious.

The first step is to decide what to work on. The work you choose needs to have three qualities: it has to be something you have a natural aptitude for, that you have a deep interest in, and that offers scope to do great work.

In practice you don't have to worry much about the third criterion. Ambitious people are if anything already too conservative about it. So all you need to do is find something you have an aptitude for and great interest in. [1]

That sounds straightforward, but it's often quite difficult. When you're young you don't know what you're good at or what different kinds of work are like. Some kinds of work you end up doing may not even exist yet. So while some people know what they want to do at 14, most have to figure it out.

The way to figure out what to work on is by working. If you're not sure what to work on, guess. But pick something and get going. You'll probably guess wrong some of the time, but that's fine. It's good to know about multiple things; some of the biggest discoveries come from noticing connections between different fields.

Develop a habit of working on your own projects. Don't let "work" mean something other people tell you to do. If you do manage to do great work one day, it will probably be on a project of your own. It may be within some bigger project, but you'll be driving your part of it.

What should your projects be? Whatever seems to you excitingly ambitious. As you grow older and your taste in projects evolves, exciting and important will converge. At 7 it may seem excitingly ambitious to build huge things out of Lego, then at 14 to teach yourself calculus, till at 21 you're starting to explore unanswered questions in physics. But always preserve excitingness.

There's a kind of excited curiosity that's both the engine and the rudder of great work. It will not only drive you, but if you let it have its way, will also show you what to work on.

What are you excessively curious about — curious to a degree that would bore most other people? That's what you're looking for.

Once you've found something you're excessively interested in, the next step is to learn enough about it to get you to one of the frontiers of knowledge. Knowledge expands fractally, and from a distance its edges look smooth, but once you learn enough to get close to one, they turn out to be full of gaps.

The next step is to notice them. This takes some skill, because your brain wants to ignore such gaps in order to make a simpler model of the world. Many discoveries have come from asking questions about things that everyone else took for granted. [2]

If the answers seem strange, so much the better. Great work often has a tincture of strangeness. You see this from painting to math. It would be affected to try to manufacture it, but if it appears, embrace it.

Boldly chase outlier ideas, even if other people aren't interested in them — in fact, especially if they aren't. If you're excited about some possibility that everyone else ignores, and you have enough expertise to say precisely what they're all overlooking, that's as good a bet as you'll find. [3]

Four steps: choose a field, learn enough to get to the frontier, notice gaps, explore promising ones. This is how practically everyone who's done great work has done it, from painters to physicists.

Steps two and four will require hard work. It may not be possible to prove that you have to work hard to do great things, but the empirical evidence is on the scale of the evidence for mortality. That's why it's essential to work on something you're deeply interested in. Interest will drive you to work harder than mere diligence ever could.

The three most powerful motives are curiosity, delight, and the desire to do something impressive. Sometimes they converge, and that combination is the most powerful of all.

The big prize is to discover a new fractal bud. You notice a crack in the surface of knowledge, pry it open, and there's a whole world inside.

Let's talk a little more about the complicated business of figuring out what to work on. The main reason it's hard is that you can't tell what most kinds of work are like except by doing them. Which means the four steps overlap: you may have to work at something for years before you know how much you like it or how good you are at it. And in the meantime you're not doing, and thus not learning about, most other kinds of work. So in the worst case you choose late based on very incomplete information. [4]

The nature of ambition exacerbates this problem. Ambition comes in two forms, one that precedes interest in the subject and one that grows out of it. Most people who do great work have a mix, and the more you have of the former, the harder it will be to decide what to do.

The educational systems in most countries pretend it's easy. They expect you to commit to a field long before you could know what it's really like. And as a result an ambitious person on an optimal trajectory will often read to the system as an instance of breakage.

It would be better if they at least admitted it — if they admitted that the system not only can't do much to help you figure out what to work on, but is designed on the assumption that you'll somehow magically guess as a teenager. They don't tell you, but I will: when it comes to figuring out what to work on, you're on your own. Some people get lucky and do guess correctly, but the rest will find themselves scrambling diagonally across tracks laid down on the assumption that everyone does.

What should you do if you're young and ambitious but don't know what to work on? What you should not do is drift along passively, assuming the problem will solve itself. You need to take action. But there is no systematic procedure you can follow. When you read biographies of people who've done great work, it's remarkable how much luck is involved. They discover what to work on as a result of a chance meeting, or by reading a book they happen to pick up. So you need to make yourself a big target for luck, and the way to do that is to be curious. Try lots of things, meet lots of people, read lots of books, ask lots of questions. [5]

When in doubt, optimize for interestingness. Fields change as you learn more about them. What mathematicians do, for example, is very different from what you do in high school math classes. So you need to give different types of work a chance to show you what they're like. But a field should become *increasingly* interesting as you learn more about it. If it doesn't, it's probably not for you.

Don't worry if you find you're interested in different things than other people. The stranger your tastes in interestingness, the better. Strange tastes are often strong ones, and a strong taste for work means you'll be productive. And you're more likely to find new things if you're looking where few have looked before.

One sign that you're suited for some kind of work is when you like even the parts that other people find tedious or frightening.

But fields aren't people; you don't owe them any loyalty. If in the course of working on one thing you discover another that's more exciting, don't be afraid to switch.

If you're making something for people, make sure it's something they actually want. The best way to do this is to make something you yourself want. Write the story you want to read; build the tool you want to use. Since your friends probably have similar interests, this will also get you your initial audience.

This *should* follow from the excitingness rule. Obviously the most exciting story to write will be the one you want to read. The reason I mention this case explicitly is that so many people get it wrong. Instead of making what they want, they try to make what some imaginary, more sophisticated audience wants. And once you go down that route, you're lost. [6]

There are a lot of forces that will lead you astray when you're trying to figure out what to work on. Pretentiousness, fashion, fear, money, politics, other people's wishes, eminent frauds. But if you stick to what you find genuinely interesting, you'll be proof against all of them. If you're interested, you're not astray.

Following your interests may sound like a rather passive strategy, but in practice it usually means following them past all sorts of obstacles. You usually have to risk rejection and failure. So it does take a good deal of boldness.

But while you need boldness, you don't usually need much planning. In most cases the recipe for doing great work is simply: work hard on excitingly ambitious projects, and something good will come of it. Instead of making a plan and then executing it, you just try to preserve certain invariants.

The trouble with planning is that it only works for achievements you can describe in advance. You can win a gold medal or get rich by deciding to as a child and then tenaciously pursuing that goal, but you can't discover natural selection that way.

I think for most people who want to do great work, the right strategy is not to plan too much. At each stage do whatever seems most interesting and gives you the best options for the future. I call this approach "staying upwind." This is how most people who've done great work seem to have done it.

Even when you've found something exciting to work on, working on it is not always straightforward. There will be times when some new idea makes you leap out of bed in the morning and get straight to work. But there will also be plenty of times when things aren't like that.

You don't just put out your sail and get blown forward by inspiration. There are headwinds and currents and hidden shoals. So there's a technique to working, just as there is to sailing.

For example, while you must work hard, it's possible to work too hard, and if you do that you'll find you get diminishing returns: fatigue will make you stupid, and eventually even damage your health. The point at which work yields diminishing returns depends on the type. Some of the hardest types you might only be able to do for four or five hours a day.

Ideally those hours will be contiguous. To the extent you can, try to arrange your life so you have big blocks of time to work in. You'll shy away from hard tasks if you know you might be interrupted.

It will probably be harder to start working than to keep working. You'll often have to trick yourself to get over that initial threshold. Don't worry about this; it's the nature of work, not a flaw in your character. Work has a sort of activation energy, both per day and per project. And since this threshold is fake in the sense that it's higher than the energy required to keep going, it's ok to tell yourself a lie of corresponding magnitude to get over it.

It's usually a mistake to lie to yourself if you want to do great work, but this is one of the rare cases where it isn't. When I'm reluctant to start work in the morning, I often trick myself by saying "I'll just read over what I've got so far." Five minutes later I've found something that seems mistaken or incomplete, and I'm off.

Similar techniques work for starting new projects. It's ok to lie to yourself about how much work a project will entail, for example. Lots of great things began with someone saying "How hard could it be?"

This is one case where the young have an advantage. They're more optimistic, and even though one of the sources of their optimism is ignorance, in this case ignorance can sometimes beat knowledge.

Try to finish what you start, though, even if it turns out to be more work than you expected. Finishing things is not just an exercise in tidiness or self-discipline. In many projects a lot of the best work happens in what was meant to be the final stage.

Another permissible lie is to exaggerate the importance of what you're working on, at least in your own mind. If that helps you discover something new, it may turn out not to have been a lie after all. [7]

Since there are two senses of starting work — per day and per project — there are also two forms of procrastination. Per-project procrastination is far the more dangerous. You put off starting that ambitious project from year to year because the time isn't quite right. When you're procrastinating in units of years, you can get a lot not done. [8]

One reason per-project procrastination is so dangerous is that it usually camouflages itself as work. You're not just sitting around doing nothing; you're working industriously on something else. So per-project procrastination doesn't set off the alarms that per-day procrastination does. You're too busy to notice it.

The way to beat it is to stop occasionally and ask yourself: Am I working on what I most want to work on?" When you're young it's ok if the answer is sometimes no, but this gets increasingly dangerous as you get older. [9]

Great work usually entails spending what would seem to most people an unreasonable amount of time on a problem. You can't think of this time as a cost, or it will seem too high. You have to find the work sufficiently engaging as it's happening.

There may be some jobs where you have to work diligently for years at things you hate before you get to the good part, but this is not how great work happens. Great work happens by focusing consistently on something you're genuinely interested in. When you pause to take stock, you're surprised how far you've come.

The reason we're surprised is that we underestimate the cumulative effect of work. Writing a page a day doesn't sound like much, but if you do it every day you'll write a book a year. That's the key: consistency. People who do great things don't get a lot done every day. They get something done, rather than nothing.

If you do work that compounds, you'll get exponential growth. Most people who do this do it unconsciously, but it's worth stopping to think about. Learning, for example, is an instance of this phenomenon: the more you learn about something, the easier it is to learn more. Growing an audience is another: the more fans you have, the more new fans they'll bring you.

The trouble with exponential growth is that the curve feels flat in the beginning. It isn't; it's still a wonderful exponential curve. But we can't grasp that intuitively, so we underrate exponential growth in its early stages.

Something that grows exponentially can become so valuable that it's worth making an extraordinary effort to get it started. But since we underrate exponential growth early on, this too is mostly done unconsciously: people push through the initial, unrewarding phase of learning something new because they know from experience that learning new things always takes an initial push, or they grow their audience one fan at a time because they have nothing better to do. If people consciously realized they could invest in exponential growth, many more would do it.

Work doesn't just happen when you're trying to. There's a kind of undirected thinking you do when walking or taking a shower or lying in bed that can be very powerful. By letting your mind wander a little, you'll often solve problems you were unable to solve by frontal attack.

You have to be working hard in the normal way to benefit from this phenomenon, though. You can't just walk around daydreaming. The daydreaming has to be interleaved with deliberate work that feeds it questions. [10]

Everyone knows to avoid distractions at work, but it's also important to avoid them in the other half of the cycle. When you

let your mind wander, it wanders to whatever you care about most at that moment. So avoid the kind of distraction that pushes your work out of the top spot, or you'll waste this valuable type of thinking on the distraction instead. (Exception: Don't avoid love.)

Consciously cultivate your taste in the work done in your field. Until you know which is the best and what makes it so, you don't know what you're aiming for.

And that *is* what you're aiming for, because if you don't try to be the best, you won't even be good. This observation has been made by so many people in so many different fields that it might be worth thinking about why it's true. It could be because ambition is a phenomenon where almost all the error is in one direction — where almost all the shells that miss the target miss by falling short. Or it could be because ambition to be the best is a qualitatively different thing from ambition to be good. Or maybe being good is simply too vague a standard. Probably all three are true. [11]

Fortunately there's a kind of economy of scale here. Though it might seem like you'd be taking on a heavy burden by trying to be the best, in practice you often end up net ahead. It's exciting, and also strangely liberating. It simplifies things. In some ways it's easier to try to be the best than to try merely to be good.

One way to aim high is to try to make something that people will care about in a hundred years. Not because their opinions matter more than your contemporaries', but because something that still seems good in a hundred years is more likely to be genuinely good.

Don't try to work in a distinctive style. Just try to do the best job you can; you won't be able to help doing it in a distinctive way.

Style is doing things in a distinctive way without trying to. Trying to is affectation.

Affectation is in effect to pretend that someone other than you is doing the work. You adopt an impressive but fake persona, and while you're pleased with the impressiveness, the fakeness is what shows in the work. [12]

The temptation to be someone else is greatest for the young. They often feel like nobodies. But you never need to worry about that problem, because it's self-solving if you work on sufficiently ambitious projects. If you succeed at an ambitious project, you're not a nobody; you're the person who did it. So just do the work and your identity will take care of itself.

"Avoid affectation" is a useful rule so far as it goes, but how would you express this idea positively? How would you say what to be, instead of what not to be? The best answer is earnest. If you're earnest you avoid not just affectation but a whole set of similar vices.

The core of being earnest is being intellectually honest. We're taught as children to be honest as an unselfish virtue — as a kind of sacrifice. But in fact it's a source of power too. To see new ideas, you need an exceptionally sharp eye for the truth. You're trying to see more truth than others have seen so far. And how can you have a sharp eye for the truth if you're intellectually dishonest?

One way to avoid intellectual dishonesty is to maintain a slight positive pressure in the opposite direction. Be aggressively willing to admit that you're mistaken. Once you've admitted you were mistaken about something, you're free. Till then you have to carry it. [13]

Another more subtle component of earnestness is informality. Informality is much more important than its grammatically negative name implies. It's not merely the absence of something. It means focusing on what matters instead of what doesn't.

What formality and affectation have in common is that as well as doing the work, you're trying to seem a certain way as you're doing it. But any energy that goes into how you seem comes out of being good. That's one reason nerds have an advantage in doing great work: they expend little effort on seeming anything. In fact that's basically the definition of a nerd.

Nerds have a kind of innocent boldness that's exactly what you need in doing great work. It's not learned; it's preserved from childhood. So hold onto it. Be the one who puts things out there rather than the one who sits back and offers sophisticated- sounding criticisms of them. "It's easy to criticize" is true in the most literal sense, and the route to great work is never easy.

There may be some jobs where it's an advantage to be cynical and pessimistic, but if you want to do great work it's an advantage to be optimistic, even though that means you'll risk looking like a fool sometimes. There's an old tradition of doing the opposite. The Old Testament says it's better to keep quiet lest you look like a fool. But that's advice for *seeming* smart. If you actually want to discover new things, it's better to take the risk of telling people your ideas.

Some people are naturally earnest, and with others it takes a conscious effort. Either kind of earnestness will suffice. But I doubt it would be possible to do great work without being earnest. It's so hard to do even if you are. You don't have enough margin for error to accommodate the distortions introduced by being affected, intellectually dishonest, orthodox, fashionable, or cool. [14]

Great work is consistent not only with who did it, but with itself. It's usually all of a piece. So if you face a decision in the middle of working on something, ask which choice is more consistent.

You may have to throw things away and redo them. You won't necessarily have to, but you have to be willing to. And that can take some effort; when there's something you need to redo, status quo bias and laziness will combine to keep you in denial about it. To beat this ask: If I'd already made the change, would I want to revert to what I have now?

Have the confidence to cut. Don't keep something that doesn't fit just because you're proud of it, or because it cost you a lot of effort.

Indeed, in some kinds of work it's good to strip whatever you're doing to its essence. The result will be more concentrated; you'll understand it better; and you won't be able to lie to yourself about whether there's anything real there.

Mathematical elegance may sound like a mere metaphor, drawn from the arts. That's what I thought when I first heard the term "elegant" applied to a proof. But now I suspect it's conceptually prior — that the main ingredient in artistic elegance is mathematical elegance. At any rate it's a useful standard well beyond math.

Elegance can be a long-term bet, though. Laborious solutions will often have more prestige in the short term. They cost a lot of effort and they're hard to understand, both of which impress people, at least temporarily.

Whereas some of the very best work will seem like it took comparatively little effort, because it was in a sense already there. It didn't have to be built, just seen. It's a very good sign when it's hard to say whether you're creating something or discovering it.

When you're doing work that could be seen as either creation or discovery, err on the side of discovery. Try thinking of yourself as a mere conduit through which the ideas take their natural shape.

(Strangely enough, one exception is the problem of choosing a problem to work on. This is usually seen as search, but in the best case it's more like creating something. In the best case you create the field in the process of exploring it.)

Similarly, if you're trying to build a powerful tool, make it gratuitously unrestrictive. A powerful tool almost by definition will be used in ways you didn't expect, so err on the side of eliminating restrictions, even if you don't know what the benefit will be.

Great work will often be tool-like in the sense of being something others build on. So it's a good sign if you're creating ideas that others could use, or exposing questions that others could answer. The best ideas have implications in many different areas.

If you express your ideas in the most general form, they'll be truer than you intended.

True by itself is not enough, of course. Great ideas have to be true and new. And it takes a certain amount of ability to see new ideas even once you've learned enough to get to one of the frontiers of knowledge.

In English we give this ability names like originality, creativity, and imagination. And it seems reasonable to give it a separate name, because it does seem to some extent a separate skill. It's possible to have a great deal of ability in other respects — to have a great deal of what's often called "technical ability" — and yet not have much of this.

I've never liked the term "creative process." It seems misleading. Originality isn't a process, but a habit of mind. Original thinkers throw off new ideas about whatever they focus on, like an angle grinder throwing off sparks. They can't help it.

If the thing they're focused on is something they don't understand very well, these new ideas might not be good. One of the most original thinkers I know decided to focus on dating after he got divorced. He knew roughly as much about dating as the average 15 year old, and the results were spectacularly colorful. But to see originality separated from expertise like that made its nature all the more clear.

I don't know if it's possible to cultivate originality, but there are definitely ways to make the most of however much you have. For example, you're much more likely to have original ideas when you're working on something. Original ideas don't come from trying to have original ideas. They come from trying to build or understand something slightly too difficult. [15]

Talking or writing about the things you're interested in is a good way to generate new ideas. When you try to put ideas into words, a missing idea creates a sort of vacuum that draws it out of you. Indeed, there's a kind of thinking that can only be done by writing.

Changing your context can help. If you visit a new place, you'll often find you have new ideas there. The journey itself often dislodges them. But you may not have to go far to get this benefit. Sometimes it's enough just to go for a walk. [16]

It also helps to travel in topic space. You'll have more new ideas if you explore lots of different topics, partly because it gives the angle grinder more surface area to work on, and partly because analogies are an especially fruitful source of new ideas.

Don't divide your attention *evenly* between many topics though, or you'll spread yourself too thin. You want to distribute it according to something more like a power law. [17] Be professionally curious about a few topics and idly curious about many more.

Curiosity and originality are closely related. Curiosity feeds originality by giving it new things to work on. But the relationship is closer than that. Curiosity is itself a kind of originality; it's roughly to questions what originality is to answers. And since questions at their best are a big component of answers, curiosity at its best is a creative force.

Having new ideas is a strange game, because it usually consists of seeing things that were right under your nose. Once you've seen a new idea, it tends to seem obvious. Why did no one think of this before?

When an idea seems simultaneously novel and obvious, it's probably a good one.

Seeing something obvious sounds easy. And yet empirically having new ideas is hard. What's the source of this apparent contradiction? It's that seeing the new idea usually requires you to change the way you look at the world. We see the world through models that both help and constrain us. When you fix a broken model, new ideas become obvious. But noticing and fixing a broken model is hard. That's how new ideas can be both obvious and yet hard to discover: they're easy to see after you do something hard.

One way to discover broken models is to be stricter than other people. Broken models of the world leave a trail of clues where they bash against reality. Most people don't want to see these clues. It would be an understatement to say that they're attached to their current model; it's what they think in; so they'll tend to ignore the trail of clues left by its breakage, however conspicuous it may seem in retrospect.

To find new ideas you have to seize on signs of breakage instead of looking away. That's what Einstein did. He was able to see the wild implications of Maxwell's equations not so much because he was looking for new ideas as because he was stricter.

The other thing you need is a willingness to break rules. Paradoxical as it sounds, if you want to fix your model of the world, it helps to be the sort of person who's comfortable breaking rules. From the point of view of the old model, which everyone including you initially shares, the new model usually breaks at least implicit rules.

Few understand the degree of rule-breaking required, because new ideas seem much more conservative once they succeed. They seem perfectly reasonable once you're using the new model of the world they brought with them. But they didn't at the time; it took the greater part of a century for the heliocentric model to be generally accepted, even among astronomers, because it felt so wrong.

Indeed, if you think about it, a good new idea has to seem bad to most people, or someone would have already explored it. So what you're looking for is ideas that seem crazy, but the right kind of crazy. How do you recognize these? You can't with certainty. Often ideas that seem bad are bad. But ideas that are the right kind of crazy tend to be exciting; they're rich in implications; whereas ideas that are merely bad tend to be depressing.

There are two ways to be comfortable breaking rules: to enjoy breaking them, and to be indifferent to them. I call these two cases being aggressively and passively independent-minded.

The aggressively independent-minded are the naughty ones. Rules don't merely fail to stop them; breaking rules gives them additional energy. For this sort of person, delight at the sheer audacity of a project sometimes supplies enough activation energy to get it started.

The other way to break rules is not to care about them, or perhaps even to know they exist. This is why novices and outsiders often make new discoveries; their ignorance of a field's assumptions acts as a source of temporary passive independent- mindedness. Aspies also seem to have a kind of immunity to conventional beliefs. Several I know say that this helps them to have new ideas.

Strictness plus rule-breaking sounds like a strange combination. In popular culture they're opposed. But popular culture has a broken model in this respect. It implicitly assumes that issues are trivial ones, and in trivial matters strictness and rule-breaking *are* opposed. But in questions that really matter, only rule-breakers can be truly strict.

An overlooked idea often doesn't lose till the semifinals. You do see it, subconsciously, but then another part of your subconscious shoots it down because it would be too weird, too risky, too much work, too controversial. This suggests an exciting possibility: if you could turn off such filters, you could see more new ideas.

One way to do that is to ask what would be good ideas for *someone else* to explore. Then your subconscious won't shoot them down to protect you.

You could also discover overlooked ideas by working in the other direction: by starting from what's obscuring them. Every cherished but mistaken principle is surrounded by a dead zone of valuable ideas that are unexplored because they contradict it.

Religions are collections of cherished but mistaken principles. So anything that can be described either literally or metaphorically as a religion will have valuable unexplored ideas in its shadow. Copernicus and Darwin both made discoveries of this type. [18]

What are people in your field religious about, in the sense of being too attached to some principle that might not be as self- evident as they think? What becomes possible if you discard it?

People show much more originality in solving problems than in deciding which problems to solve. Even the smartest can be surprisingly conservative when deciding what to work on. People

who'd never dream of being fashionable in any other way get sucked into working on fashionable problems.

One reason people are more conservative when choosing problems than solutions is that problems are bigger bets. A problem could occupy you for years, while exploring a solution might only take days. But even so I think most people are too conservative. They're not merely responding to risk, but to fashion as well. Unfashionable problems are undervalued.

One of the most interesting kinds of unfashionable problem is the problem that people think has been fully explored, but hasn't. Great work often takes something that already exists and shows its latent potential. Durer and Watt both did this. So if you're interested in a field that others think is tapped out, don't let their skepticism deter you. People are often wrong about this.

Working on an unfashionable problem can be very pleasing. There's no hype or hurry. Opportunists and critics are both occupied elsewhere. The existing work often has an old-school solidity. And there's a satisfying sense of economy in cultivating ideas that would otherwise be wasted.

But the most common type of overlooked problem is not explicitly unfashionable in the sense of being out of fashion. It just doesn't seem to matter as much as it actually does. How do you find these? By being self-indulgent — by letting your curiosity have its way, and tuning out, at least temporarily, the little voice in your head that says you should only be working on "important" problems.

You do need to work on important problems, but almost everyone is too conservative about what counts as one. And if there's an important but overlooked problem in your neighborhood, it's probably already on your subconscious radar screen. So try asking yourself: if you were going to take a break from "serious" work to work on something just because it would be really interesting, what would you do? The answer is probably more important than it seems.

Originality in choosing problems seems to matter even more than originality in solving them. That's what distinguishes the people who discover whole new fields. So what might seem to be merely the initial step — deciding what to work on — is in a sense the key to the whole game.

Few grasp this. One of the biggest misconceptions about new ideas is about the ratio of question to answer in their composition. People think big ideas are answers, but often the real insight was in the question.

Part of the reason we underrate questions is the way they're used in schools. In schools they tend to exist only briefly before being answered, like unstable particles. But a really good question can be much more than that. A really good question is a partial discovery. How do new species arise? Is the force that makes objects fall to earth the same as the one that keeps planets in

their orbits? By even asking such questions you were already in excitingly novel territory.

Unanswered questions can be uncomfortable things to carry around with you. But the more you're carrying, the greater the chance of noticing a solution — or perhaps even more excitingly, noticing that two unanswered questions are the same.

Sometimes you carry a question for a long time. Great work often comes from returning to a question you first noticed years before — in your childhood, even — and couldn't stop thinking about. People talk a lot about the importance of keeping your youthful dreams alive, but it's just as important to keep your youthful questions alive. [19]

This is one of the places where actual expertise differs most from the popular picture of it. In the popular picture, experts are certain. But actually the more puzzled you are, the better, so long as (a) the things you're puzzled about matter, and (b) no one else understands them either.

Think about what's happening at the moment just before a new idea is discovered. Often someone with sufficient expertise is puzzled about something. Which means that originality consists partly of puzzlement — of confusion! You have to be comfortable enough with the world being full of puzzles that you're willing to see them, but not so comfortable that you don't want to solve them. [20]

It's a great thing to be rich in unanswered questions. And this is one of those situations where the rich get richer, because the best way to acquire new questions is to try answering existing ones. Questions don't just lead to answers, but also to more questions.

The best questions grow in the answering. You notice a thread protruding from the current paradigm and try pulling on it, and it just gets longer and longer. So don't require a question to be obviously big before you try answering it. You can rarely predict that. It's hard enough even to notice the thread, let alone to predict how much will unravel if you pull on it.

It's better to be promiscuously curious — to pull a little bit on a lot of threads, and see what happens. Big things start small. The initial versions of big things were often just experiments, or side projects, or talks, which then grew into something bigger. So start lots of small things.

Being prolific is underrated. The more different things you try, the greater the chance of discovering something new. Understand, though, that trying lots of things will mean trying lots of things that don't work. You can't have a lot of good ideas without also having a lot of bad ones. [21]

Though it sounds more responsible to begin by studying everything that's been done before, you'll learn faster and have more fun by trying stuff. And you'll understand previous work better when you do look at it. So err on the side of starting.

Which is easier when starting means starting small; those two ideas fit together like two puzzle pieces.

How do you get from starting small to doing something great? By making successive versions. Great things are almost always made in successive versions. You start with something small and evolve it, and the final version is both cleverer and more ambitious than anything you could have planned.

It's particularly useful to make successive versions when you're making something for people — to get an initial version in front of them quickly, and then evolve it based on their response.

Begin by trying the simplest thing that could possibly work. Surprisingly often, it does. If it doesn't, this will at least get you started.

Don't try to cram too much new stuff into any one version. There are names for doing this with the first version (taking too long to ship) and the second (the second system effect), but these are both merely instances of a more general principle.

An early version of a new project will sometimes be dismissed as a toy. It's a good sign when people do this. That means it has everything a new idea needs except scale, and that tends to follow. [22]

The alternative to starting with something small and evolving it is to plan in advance what you're going to do. And planning does usually seem the more responsible choice. It sounds more organized to say "we're going to do x and then y and then z" than "we're going to try x and see what happens." And it is more *organized*; it just doesn't work as well.

Planning per se isn't good. It's sometimes necessary, but it's a necessary evil — a response to unforgiving conditions. It's something you have to do because you're working with inflexible media, or because you need to coordinate the efforts of a lot of people. If you keep projects small and use flexible media, you don't have to plan as much, and your designs can evolve instead.

Take as much risk as you can afford. In an efficient market, risk is proportionate to reward, so don't look for certainty, but for a bet with high expected value. If you're not failing occasionally, you're probably being too conservative.

Though conservatism is usually associated with the old, it's the young who tend to make this mistake. Inexperience makes them fear risk, but it's when you're young that you can afford the most.

Even a project that fails can be valuable. In the process of working on it, you'll have crossed territory few others have seen, and encountered questions few others have asked. And there's probably no better source of questions than the ones you encounter in trying to do something slightly too hard.

Use the advantages of youth when you have them, and the advantages of age once you have those. The advantages of youth are energy, time, optimism, and freedom. The advantages of age are knowledge, efficiency, money, and power. With effort you can acquire some of the latter when young and keep some of the former when old.

The old also have the advantage of knowing which advantages they have. The young often have them without realizing it. The biggest is probably time. The young have no idea how rich they are in time. The best way to turn this time to advantage is to use it in slightly frivolous ways: to learn about something you don't need to know about, just out of curiosity, or to try building something just because it would be cool, or to become freakishly good at something.

That "slightly" is an important qualification. Spend time lavishly when you're young, but don't simply waste it. There's a big difference between doing something you worry might be a waste of time and doing something you know for sure will be. The former is at least a bet, and possibly a better one than you think. [23]

The most subtle advantage of youth, or more precisely of inexperience, is that you're seeing everything with fresh eyes. When your brain embraces an idea for the first time, sometimes the two don't fit together perfectly. Usually the problem is with your brain, but occasionally it's with the idea. A piece of it sticks out awkwardly and jabs you when you think about it. People who are used to the idea have learned to ignore it, but you have the opportunity not to. [24]

So when you're learning about something for the first time, pay attention to things that seem wrong or missing. You'll be tempted to ignore them, since there's a 99% chance the problem is with you. And you may have to set aside your misgivings temporarily to keep progressing. But don't forget about them. When you've gotten further into the subject, come back and check if they're still there. If they're still viable in the light of your present knowledge, they probably represent an undiscovered idea.

One of the most valuable kinds of knowledge you get from experience is to know what you *don't* have to worry about. The young know all the things that could matter, but not their relative importance. So they worry equally about everything, when they should worry much more about a few things and hardly at all about the rest.

But what you don't know is only half the problem with inexperience. The other half is what you do know that ain't so. You arrive at adulthood with your head full of nonsense — bad habits you've acquired and false things you've been taught — and you won't be able to do great work till you clear away at least the nonsense in the way of whatever type of work you want to do.

Much of the nonsense left in your head is left there by schools. We're so used to schools that we unconsciously treat going to school as identical with learning, but in fact schools have all sorts of strange qualities that warp our ideas about learning and thinking.

For example, schools induce passivity. Since you were a small child, there was an authority at the front of the class telling all of you what you had to learn and then measuring whether you did. But neither classes nor tests are intrinsic to learning; they're just artifacts of the way schools are usually designed.

The sooner you overcome this passivity, the better. If you're still in school, try thinking of your education as your project, and your teachers as working for you rather than vice versa. That may seem a stretch, but it's not merely some weird thought experiment. It's the truth, economically, and in the best case it's the truth intellectually as well. The best teachers don't want to be your bosses. They'd prefer it if you pushed ahead, using them as a source of advice, rather than being pulled by them through the material.

Schools also give you a misleading impression of what work is like. In school they tell you what the problems are, and they're almost always soluble using no more than you've been taught so far. In real life you have to figure out what the problems are, and you often don't know if they're soluble at all.

But perhaps the worst thing schools do to you is train you to win by hacking the test. You can't do great work by doing that. You can't trick God. So stop looking for that kind of shortcut. The way to beat the system is to focus on problems and solutions that others have overlooked, not to skimp on the work itself.

Don't think of yourself as dependent on some gatekeeper giving you a "big break." Even if this were true, the best way to get it would be to focus on doing good work rather than chasing influential people.

And don't take rejection by committees to heart. The qualities that impress admissions officers and prize committees are quite different from those required to do great work. The decisions of selection committees are only meaningful to the extent that they're part of a feedback loop, and very few are.

People new to a field will often copy existing work. There's nothing inherently bad about that. There's no better way to learn how something works than by trying to reproduce it. Nor does copying necessarily make your work unoriginal. Originality is the presence of new ideas, not the absence of old ones.

There's a good way to copy and a bad way. If you're going to

copy something, do it openly instead of furtively, or worse still, unconsciously. This is what's meant by the famously misattributed phrase "Great artists steal." The really dangerous kind of copying, the kind that gives copying a bad name, is the kind that's done without realizing it, because you're nothing more than a train running on tracks laid down by someone else. But at the other extreme, copying can be a sign of superiority rather than subordination. [25]

In many fields it's almost inevitable that your early work will be in some sense based on other people's. Projects rarely arise in a vacuum. They're usually a reaction to previous work. When you're first starting out, you don't have any previous work; if you're going to react to something, it has to be someone else's. Once you're established, you can react to your own. But while the former gets called derivative and the latter doesn't, structurally the two cases are more similar than they seem.

Oddly enough, the very novelty of the most novel ideas sometimes makes them seem at first to be more derivative than they are. New discoveries often have to be conceived initially as variations of existing things, *even by their discoverers*, because there isn't yet the conceptual vocabulary to express them.

There are definitely some dangers to copying, though. One is that you'll tend to copy old things — things that were in their day at the frontier of knowledge, but no longer are.

And when you do copy something, don't copy every feature of it. Some will make you ridiculous if you do. Don't copy the manner of an eminent 50 year old professor if you're 18, for example, or the idiom of a Renaissance poem hundreds of years later.

Some of the features of things you admire are flaws they succeeded despite. Indeed, the features that are easiest to imitate are the most likely to be the flaws.

This is particularly true for behavior. Some talented people are jerks, and this sometimes makes it seem to the inexperienced that being a jerk is part of being talented. It isn't; being talented is merely how they get away with it.

One of the most powerful kinds of copying is to copy something from one field into another. History is so full of chance discoveries of this type that it's probably worth giving chance a hand by deliberately learning about other kinds of work. You can take ideas from quite distant fields if you let them be metaphors.

Negative examples can be as inspiring as positive ones. In fact you can sometimes learn more from things done badly than from things done well; sometimes it only becomes clear what's needed when it's missing.

If a lot of the best people in your field are collected in one place, it's usually a good idea to visit for a while. It will increase your ambition, and also, by showing you that these people are human, increase your self-confidence. [26]

If you're earnest you'll probably get a warmer welcome than you might expect. Most people who are very good at something are happy to talk about it with anyone who's genuinely interested. If they're really good at their work, then they probably have a hobbyist's interest in it, and hobbyists always want to talk about their hobbies.

It may take some effort to find the people who are really good, though. Doing great work has such prestige that in some places, particularly universities, there's a polite fiction that everyone is engaged in it. And that is far from true. People within universities can't say so openly, but the quality of the work being done in different departments varies immensely. Some departments have people doing great work; others have in the past; others never have.

Seek out the best colleagues. There are a lot of projects that can't be done alone, and even if you're working on one that can be, it's good to have other people to encourage you and to bounce ideas off.

Colleagues don't just affect your work, though; they also affect you. So work with people you want to become like, because you will.

Quality is more important than quantity in colleagues. It's better to have one or two great ones than a building full of pretty good ones. In fact it's not merely better, but necessary, judging from history: the degree to which great work happens in clusters suggests that one's colleagues often make the difference between doing great work and not.

How do you know when you have sufficiently good colleagues? In my experience, when you do, you know. Which means if you're unsure, you probably don't. But it may be possible to give a more concrete answer than that. Here's an attempt: sufficiently good colleagues offer *surprising* insights. They can see and do things that you can't. So if you have a handful of colleagues good enough to keep you on your toes in this sense, you're probably over the threshold.

Most of us can benefit from collaborating with colleagues, but some projects require people on a larger scale, and starting one of those is not for everyone. If you want to run a project like that, you'll have to become a manager, and managing well takes aptitude and interest like any other kind of work. If you don't have them, there is no middle path: you must either force yourself to learn management as a second language, or avoid such projects. [27]

Husband your morale. It's the basis of everything when you're working on ambitious projects. You have to nurture and protect it

like a living organism.

Morale starts with your view of life. You're more likely to do great work if you're an optimist, and more likely to if you think of yourself as lucky than if you think of yourself as a victim.

Indeed, work can to some extent protect you from your problems. If you choose work that's pure, its very difficulties will serve as a refuge from the difficulties of everyday life. If this is escapism, it's a very productive form of it, and one that has been used by some of the greatest minds in history.

Morale compounds via work: high morale helps you do good work, which increases your morale and helps you do even better work. But this cycle also operates in the other direction: if you're not doing good work, that can demoralize you and make it even harder to. Since it matters so much for this cycle to be running in the right direction, it can be a good idea to switch to easier work when you're stuck, just so you start to get something done.

One of the biggest mistakes ambitious people make is to allow setbacks to destroy their morale all at once, like a balloon bursting. You can inoculate yourself against this by explicitly considering setbacks a part of your process. Solving hard problems always involves some backtracking.

Doing great work is a depth-first search whose root node is the desire to. So "If at first you don't succeed, try, try again" isn't quite right. It should be: If at first you don't succeed, either try again, or backtrack and then try again.

"Never give up" is also not quite right. Obviously there are times when it's the right choice to eject. A more precise version would be: Never let setbacks panic you into backtracking more than you need to. Corollary: Never abandon the root node.

It's not necessarily a bad sign if work is a struggle, any more than it's a bad sign to be out of breath while running. It depends how fast you're running. So learn to distinguish good pain from bad. Good pain is a sign of effort; bad pain is a sign of damage.

An audience is a critical component of morale. If you're a scholar, your audience may be your peers; in the arts, it may be an audience in the traditional sense. Either way it doesn't need to be big. The value of an audience doesn't grow anything like linearly with its size. Which is bad news if you're famous, but good news if you're just starting out, because it means a small but dedicated audience can be enough to sustain you. If a handful of people genuinely love what you're doing, that's enough.

To the extent you can, avoid letting intermediaries come between you and your audience. In some types of work this is inevitable, but it's so liberating to escape it that you might be better off switching to an adjacent type if that will let you go direct. [28]

The people you spend time with will also have a big effect on your morale. You'll find there are some who increase your energy and

others who decrease it, and the effect someone has is not always what you'd expect. Seek out the people who increase your energy and avoid those who decrease it. Though of course if there's someone you need to take care of, that takes precedence.

Don't marry someone who doesn't understand that you need to work, or sees your work as competition for your attention. If you're ambitious, you need to work; it's almost like a medical condition; so someone who won't let you work either doesn't understand you, or does and doesn't care.

Ultimately morale is physical. You think with your body, so it's important to take care of it. That means exercising regularly, eating and sleeping well, and avoiding the more dangerous kinds of drugs. Running and walking are particularly good forms of exercise because they're good for thinking. [29]

People who do great work are not necessarily happier than everyone else, but they're happier than they'd be if they didn't. In fact, if you're smart and ambitious, it's dangerous *not* to be productive. People who are smart and ambitious but don't achieve much tend to become bitter.

It's ok to want to impress other people, but choose the right people. The opinion of people you respect is signal. Fame, which is the opinion of a much larger group you might or might not respect, just adds noise.

The prestige of a type of work is at best a trailing indicator and sometimes completely mistaken. If you do anything well enough, you'll make it prestigious. So the question to ask about a type of work is not how much prestige it has, but how well it could be done.

Competition can be an effective motivator, but don't let it choose the problem for you; don't let yourself get drawn into chasing something just because others are. In fact, don't let competitors make you do anything much more specific than work harder.

Curiosity is the best guide. Your curiosity never lies, and it knows more than you do about what's worth paying attention to.

Notice how often that word has come up. If you asked an oracle the secret to doing great work and the oracle replied with a single word, my bet would be on "curiosity."

That doesn't translate directly to advice. It's not enough just to be curious, and you can't command curiosity anyway. But you can nurture it and let it drive you.

Curiosity is the key to all four steps in doing great work: it will choose the field for you, get you to the frontier, cause you to notice the gaps in it, and drive you to explore them. The whole process is a kind of dance with curiosity.

Believe it or not, I tried to make this essay as short as I could. But its length at least means it acts as a filter. If you made it this far, you must be interested in doing great work. And if so you're already further along than you might realize, because the set of people willing to want to is small.

The factors in doing great work are factors in the literal, mathematical sense, and they are: ability, interest, effort, and luck. Luck by definition you can't do anything about, so we can ignore that. And we can assume effort, if you do in fact want to do great work. So the problem boils down to ability and interest. Can you find a kind of work where your ability and interest will combine to yield an explosion of new ideas?

Here there are grounds for optimism. There are so many different ways to do great work, and even more that are still undiscovered. Out of all those different types of work, the one you're most suited for is probably a pretty close match. Probably a comically close match. It's just a question of finding it, and how far into it your ability and interest can take you. And you can only answer that by trying.

Many more people could try to do great work than do. What holds them back is a combination of modesty and fear. It seems presumptuous to try to be Newton or Shakespeare. It also seems hard; surely if you tried something like that, you'd fail. Presumably the calculation is rarely explicit. Few people consciously decide not to try to do great work. But that's what's going on subconsciously; they shy away from the question.

So I'm going to pull a sneaky trick on you. Do you want to do great work, or not? Now you have to decide consciously. Sorry about that. I wouldn't have done it to a general audience. But we already know you're interested.

Don't worry about being presumptuous. You don't have to tell anyone. And if it's too hard and you fail, so what? Lots of people have worse problems than that. In fact you'll be lucky if it's the worst problem you have.

Yes, you'll have to work hard. But again, lots of people have to work hard. And if you're working on something you find very interesting, which you necessarily will if you're on the right path, the work will probably feel less burdensome than a lot of your peers'.

The discoveries are out there, waiting to be made. Why not by you?

Lunch with Alan Kay: how to become educated enough to invent the future

4/17/19

Someone (I forget who) mentioned to me at the Dynamicland meetup last month that Alan Kay lives in London. So I dropped him an email. I had sent him a few cold emails in the past, one of which sparked a lively conversation. (I asked him which term he used to describe himself. He landed on "media imaginer" and "communications designer", because terms like "computer scientist" have been opted to mean other things now.)

To my surprise, I got a quick reply and had lunch scheduled for the next week, which was this past Monday, April 15th, 2019 at 11:30am. To my delight and utter exhaustion, we spoke for just over five hours. When my mom saw me later that night, she remarked that I looked "wrung out." It felt like every neuron in my head was firing at full capacity just to try to keep up. I left our meeting out of breath.

Alan and I after our five hour lunch at the Ivy Cafe in London. Referring to my request for a photo, he said, "It's like game hunting. To show your friends you shot the elephant."

Before the meeting, I asked a few mutual friends for advice on the meeting. Universally, I was warned that he would do a lot of the talking. This proved to be the case. I later joked to my parents that I got to ask four questions, and each one took him an hour to answer.

It was five hours ranging from electrical engineering, to architecture, to the meaning of the word science, to theater, to jazz, to visual art, to education, to researcher gossip, to why to go to grad school, and so, so much computer history. I was doing my best to jot down all interesting key words, even if just to remind myself of the flow of the conversation.

At no point in our conversation did Alan explicitly explain what his agenda was, or why he even took the time to meet with me. It seemed like he knew almost nothing about me, and wasn't particularly curious to learn. However, from bits and pieces he dropped about *his* mentors, I think I am able to piece together what our lunch was about for him.

Alan admires his former professor Bob Barton. Despite not liking students or teaching, Bob spent the time to do it "for the field." I think that's ultimately Alan's aim in meeting with people like me.

Maybe there's a fraction of a percent chance that I have the potential to *help the field* and so Ala was there to nurture that chance. In other words, he was getting lunch with me in for pure benevolence. He wants there to be good people in *his field* (don't ask him what it should be called unless you have all afternoon), ultimately to further humanity.

Also like Bob Barton, Alan spent most of the lunch very kindly intellectually destroying me, smacking me around, and pointing out how much more I have to learn in order to do good work. I have met only a handful of people in my life who can give me such brutal criticism but in a way that feels supremely constructive.

Paraphrasing here to the best of my memory, Alan said, "Reading a couple hundred books a year is the bare minimum. It's just the baseline. You also need to be embedded in a community of others who have diverse perspectives to bounce these ideas off of." Alan argued passionately in favor of college and grad school. While he is well aware of its imperfections, he believes it's still better than an "oral culture" or being an autodidact (just following your nose where your curiosity leads you).

But "in the end, we're all autodidatic in having to find the motivation to do the learning ourselves. The key for autodidact-types is to set up ways to avoid insularity." He recommends that autodidacts institute a "learning tax" on themselves: a decent percentage of one's learning should be in areas other than the ones you are most interested in. But ultimately a university context can be very helpful to force you to learn what you didn't even realize was worthwhile, and to supply "serendipitous other perspectives". As for what to study, the key is that it needs to be difficult in ways that reshape your perspectives, like math, physics, or molecular biology.

What it comes down to is: are you trying to do *science*? Are you trying to invent a good future for humanity? Alan's definition of science is still too large to fit into my head, but I can see his reverence for it and the pioneering scientists of our past. If science is what you're trying to do, you have to be fully committed to walking that road: using as many methods as possible to help us get around what is wrong with our thinking (our genetic brains, culture, and languages).

It's only once you give up on absolute truth and certainty that you can make progress. Once you fully recognize your limited and faulty senses, you build tools to get around those limitations. You build models, maps of reality, and then test those models against reality to see how close they come. If you built a good model, and you understand it well in its abstract sense, you can manipulate it and come to understand things about the world. We don't even get a glimpse of reality, "but what we do glimpse is, for many things, far superior to made up stories and fondly held beliefs." And that's all we ever get. And it's the best thing in the world. It gives us the polio vaccine and spaceships.

For me this lunch felt like a reckoning. It was as if [to be clear: this didn't really happen], Alan clapped his hands loudly in my face, shouting "Wake up! Wake up!", and then turned me away from the flame everyone else was transfixed by and onto a helicopter ride to give me a glimpse of all the other perspectives that I should consider.

Before this lunch, I thought I was noble in forsaking Silicon Valley riches to achieve non-profit dominance akin to Jimmy Wales's Wikipedia, Mitch Resnick's Scratch, Guido's Python, or Linus Torvald's Linux and git. But Alan showed me how I simply replaced one form of vapid success for another. My admiration of those non-profit tools is "misplaced Darwinism" (a.k.a. "worse-is-better-ism"), equating popularity with goodness.

Alan disabused me of this dream by tearing down each of my heroes in turn: Wikipedia is much less than it needs to be, Scratch is less than Etoys, and the web was created by unsophisticated perspectives (Tim Burners-Lee has apologized for missing Engelbart).

That's not to say that it's not worthwhile to try to be altruistic and help the world. Alan says, "The trick is to get 'help the world' above a real threshold (which is usually above the threshold of mere popularity)."

The one technology that Alan has respect for is the Internet, a technology that works so well that's it's not even treated as a technology. It's just a part of our natural world now. It's gone through eleven orders of magnitude expansions without a hitch. Yet nobody knows the names of its creators. That's the sign of technology well built.

Alan helped show me that I am professing to be benevolent when I really just wanted to get all the credit for "democratizing programming." It's not so dissimilar from wanting to be rich and famous. It's particularly pernicious if you profess to improve programming in superficial ways. Alan points out that "the current 'code for all' approach is that it is dangerously close to the game 'Guitar Hero'. I.e. 'coding' is not close enough to what's important and enlightening about computing to be learned on its own. To do this gives the false impression of 'touching the real thing' (like 'Guitar Hero' and other games) whereas it could hardly be more of a miss." In other words: am I looking to elevate kids' thinking or am I trying to cash in on the simulacra of education?

It seemed like Alan was asking me to pick: how benevolent do you actually want to be? Or are you just looking to *seem* benevolent? What's your time horizon? Are you looking to make a small incremental improvement? Or are you looking 10 years out to build a better future for your children? Or are you looking to the 22nd century to ensure that our grandkids' kids will be better than we are? Or are you thinking about the far future of not just humanity but all conscious beings, and how to build a thriving multi-planetary society for millions of years to come?

And it's not even about the time horizon. What the world *needs now* is so much grander than what so-called-problem-solvers propose. What we need today (or yesterday) is "real education as though we are in a war, to deal with the climate problem as though we are in a war, world health, human rights, etc. as though we are in a war." Our culture now celebrates little wins and small hacks. We don't have the patience, vision, or incentives to undertake the necessary solutions to our biggest problems.

We so-called computer scientists live in a pop culture. We aren't doing science and we can't tell you a single thing about our history. As a field, we are suffering from a "resource curse": there's too much money in computing and it "dilutes our field with carpetbaggers."" Alan worries that the Silicon Valley mentality of VCs and startups is "soul stealing." Those people may be "lost forever" in an "anti-richness" culture.

It doesn't have to be this way. We don't have to "move fast and break things." We can move slowly and build good things to last. We can return to the traditions of architects that built cathedrals to last hundreds of years, mathematicians who have collaborated on imaginary structures for thousands of years, and scientists who have pulled back the curtain on reality since people began to doubt their senses.

For the few computer idealists among us, we are so lucky to have the legacy left to us by Vannevar Bush, J.C.R. Licklider, Douglas Engelbart, Alan Perlis, John McCarthy, Edsger Dijkstra, John Backus, Ivan Sutherland, and Alan Kay. And those are just some of the names I personally know – I am now ashamed I don't know more of our history. It's hard to imagine now because they were so effective, but so much of our world's computing prosperity today is due to these people. They imagined the computer as a personal device, a communications device, a device to lift off the burden of tedious mental tabulations. Douglas Engelbart imagined a tool that would aid humanity in dealing with the increasingly-complex problems it faces around the world. We've only seem a glimpse of that vision, but we need it now more than ever.

So practically, what does this mean for me? Alan also said at lunch that one problem young people make is "having goals." It's too early to have goals that "consume one's horizons," because young people don't even know what they don't know. I think this kind of epistemic modesty is a great idea. I can probably benefit from shifting the focus from my overly-specific goals to "more meta" goals, such as becoming "educated" in a broader sense than I previously thought was possible. The more perspectives I can acquire, the better I'll be at not fooling myself, and the more I'll be able to appreciate the richness of the world.

I also want to think a bit more critically about my "theory of change." I've been operating under the lone-programming-language-developer-open-source theory of change that worked for Linus, Guido, Matz, Burners-Lee, etc. Alan pointed out that most technologies created in this way are "worse is better" ones. It's dangerously easy to commit what Alan calls "inverse vandalism." Both individuals and groups are extremely susceptible to it. His slogan for this "bug" is: "Better and Perfect are the Enemies of What Is Actually Needed (WIAN)." But it's really hard to determine WIAN. It was the skill Paul MacCready exemplified in achieving the first man-powered flight. As he said, "The problem is we don't understand the problem." If you don't understand the problem or WIAN, you're setting yourself up to create more problem than you solve.

The best technological innovations happened in in-person teams lead by extraordinary people, such as Ivan Sutherland, Doug Engelbart, or Bret Victor. Maybe the Internet will allow these groups to be physically distributed, but Alan, for one, is very skeptical of our "universal publishing medium for bad ideas." At the very least, I am now way more curious about what made ARPA, Bell Labs, and Xerox Parc succeed. I'm also going to more closely follow what Juan Benet is up to, because he's been talking in this Alan-Kay-style for years now.

What surprised me most about the meeting was how compelling Alan was. Before this meeting, I saw him as a researcher or engineer, in many ways similar to myself. I didn't realize that his main role has been recognizing, collecting, nurturing, leading and inspiring researchers. He played my emotions like a fiddle. He weaved a web of tales of the greatest story of humanity – our struggle to elevate ourselves – and implied that I could play a part in that story, too, but only if I get serious about my education.

Recommendations to consider

- Read Bertrand Russel (along with Churchill) won the Nobel Prize in nonfiction
- Learn more about Paul MacCready (first man-powered flight)
- Learn more about EE, including Ham, AM/FM radios
- Art, Mind and Brain
- Spend a lot of time trying to make sense of McLuhan
- "" "" Montessori (Discovery of the Child)
- "" "" Jerome Bruner
- Read Adam Smith (in order of publication)
- Postman's "Ring around the collar" essay
- Keep my eyes peeled for a grad school or community that would push me, read books with me, and have amazing discussions on a wide range of topics
- Read Education as a Conserving Activity by Postman
- Read Sept 1966 Scientific American Article (MIT-ARPA), which came out two months before Kay went to Utah
- Learning enjoyment tax of 10% (your interests take care of themselves)
- Read Plato and other foundational texts
- Read: The Act of Creation by Arthur Koestler
- Watch Vi Hart (particularly the 12 Tones video)
- Read more early computer history, and stories about people who made the world better (Organizing Genius)