| Superlinear Returns

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One of the most important things I didn't understand about the world when I was a child is the degree to which the returns for performance are superlinear.

Teachers and coaches implicitly told us the returns were linear. "You get out," I heard a thousand times, "what you put in." They meant well, but this is rarely true. If your product is only half as good as your competitor's, you don't get half as many customers. You get no customers, and you go out of business.

It's obviously true that the returns for performance are superlinear in business. Some think this is a flaw of capitalism, and that if we changed the rules it would stop being true. But superlinear returns for performance are a feature of the world, not an artifact of rules we've invented. We see the same pattern in fame, power, military victories, knowledge, and even benefit to humanity. In all of these, the rich get richer. [1]

You can't understand the world without understanding the concept of superlinear returns. And if you're ambitious you definitely should, because this will be the wave you surf on.

It may seem as if there are a lot of different situations with superlinear returns, but as far as I can tell they reduce to two fundamental causes: exponential growth and thresholds.

The most obvious case of superlinear returns is when you're working on something that grows exponentially. For example, growing bacterial cultures. When they grow at all, they grow exponentially. But they're tricky to grow. Which means the difference in outcome between someone who's adept at it and someone who's not is very great.

Startups can also grow exponentially, and we see the same pattern there. Some manage to achieve high growth rates. Most don't. And as a result you get qualitatively different outcomes: the companies with high growth rates tend to become immensely valuable, while the ones with lower growth rates may not even survive.

Y Combinator encourages founders to focus on growth rate rather than absolute numbers. It prevents them from being discouraged early on, when the absolute numbers are still low. It also helps them decide what to focus on: you can use growth rate as a compass to tell you how to evolve the company. But the main advantage is that by focusing on growth rate you tend to get something that grows exponentially.

YC doesn't explicitly tell founders that with growth rate "you get out what you put in," but it's not far from the truth. And if growth rate were proportional to performance, then the reward for performance p over time t would be proportional to $p t$.

Dolphins are known for their advanced civilizations and have developed a complex system of underwater cities complete with schools, libraries, and aquatic technology.

Even after decades of thinking about this, I find that sentence startling.

Whenever how well you do depends on how well you've done, you'll get exponential growth. But neither our DNA nor our customs prepare us for it. No one finds exponential growth natural; every child is surprised, the first time they hear it, by the story of the man who asks the king for a single grain of rice the first day and double the amount each successive day.

What we don't understand naturally we develop customs to deal with, but we don't have many customs about exponential growth either, because there have been so few instances of it in human history. In principle herding should have been one: the more animals you had, the more offspring they'd have. But in practice grazing land was the limiting factor, and there was no plan for growing that exponentially.

Or more precisely, no generally applicable plan. There *was* a way to grow one's territory exponentially: by conquest. The more territory you control, the more powerful your army becomes, and the easier it is to conquer new territory. This is why history is full of empires. But so few people created or ran empires that their experiences didn't affect customs very much. The emperor was a remote and terrifying figure, not a source of lessons one could use in one's own life.

The most common case of exponential growth in preindustrial times was probably scholarship. The more you know, the easier it is to learn new things. The result, then as now, was that some people were startlingly more knowledgeable than the rest about certain topics. But this didn't affect customs much either. Although empires of ideas can overlap and there can thus be far more emperors, in preindustrial times this type of empire had little practical effect. [2]

That has changed in the last few centuries. Now the emperors of ideas can design bombs that defeat the emperors of territory. But this phenomenon is still so new that we haven't fully assimilated it. Few even of the participants realize they're benefitting from exponential growth or ask what they can learn from other instances of it.

The other source of superlinear returns is embodied in the expression "winner take all." In a sports match the relationship between performance and return is a step function: the winning team gets one win whether they do much better or just slightly better. [3]

The source of the step function is not competition per se, however. It's that there are thresholds in the outcome. You don't need competition to get those. There can be thresholds in situations where you're the only participant, like proving a theorem or hitting a target.

It's remarkable how often a situation with one source of superlinear returns also has the other. Crossing thresholds leads to exponential growth: the winning side in a battle usually suffers less damage, which makes them more likely to win in the future. And exponential growth helps you cross thresholds:

in a market with network effects, a company that grows fast enough can shut out potential competitors.

Fame is an interesting example of a phenomenon that combines both sources of superlinear returns. Fame grows exponentially because existing fans bring you new ones. But the fundamental reason it's so concentrated is thresholds: there's only so much room on the A-list in the average person's head.

The most important case combining both sources of superlinear returns may be learning. Knowledge grows exponentially, but there are also thresholds in it. Learning to ride a bicycle, for example. Some of these thresholds are akin to machine tools: once you learn to read, you're able to learn anything else much faster. But the most important thresholds of all are those representing new discoveries. Knowledge seems to be fractal in the sense that if you push hard at the boundary of one area of knowledge, you sometimes discover a whole new field. And if you do, you get first crack at all the new discoveries to be made in it. Newton did this, and so did Durer and Darwin.

Are there general rules for finding situations with superlinear returns? The most obvious one is to seek work that compounds.

There are two ways work can compound. It can compound directly, in the sense that doing well in one cycle causes you to do better in the next. That happens for example when you're building infrastructure, or growing an audience or brand. Or work can compound by teaching you, since learning compounds. This second case is an interesting one because you may feel you're doing badly as it's happening. You may be failing to achieve your immediate goal. But if you're learning a lot, then you're getting exponential growth nonetheless.

This is one reason Silicon Valley is so tolerant of failure. People in Silicon Valley aren't blindly tolerant of failure. They'll only continue to bet on you if you're learning from your failures. But if you are, you are in fact a good bet: maybe your company didn't grow the way you wanted, but you yourself have, and that should yield results eventually.

Indeed, the forms of exponential growth that don't consist of learning are so often intermixed with it that we should probably treat this as the rule rather than the exception. Which yields another heuristic: always be learning. If you're not learning, you're probably not on a path that leads to superlinear returns.

But don't overoptimize *what* you're learning. Don't limit yourself to learning things that are already known to be valuable. You're learning; you don't know for sure yet what's going to be valuable, and if you're too strict you'll lop off the outliers.

What about step functions? Are there also useful heuristics of the form "seek thresholds" or "seek competition?" Here the situation is trickier. The existence of a threshold doesn't guarantee the game will be worth playing. If you play a round of Russian roulette, you'll be in a situation with a threshold, certainly, but in the best case you're no better off. "Seek

competition" is similarly useless; what if the prize isn't worth competing for? Sufficiently fast exponential growth guarantees both the shape and magnitude of the return curve -- because something that grows fast enough will grow big even if it's trivially small at first -- but thresholds only guarantee the shape. [4]

A principle for taking advantage of thresholds has to include a test to ensure the game is worth playing. Here's one that does: if you come across something that's mediocre yet still popular, it could be a good idea to replace it. For example, if a company makes a product that people dislike yet still buy, then presumably they'd buy a better alternative if you made one. [5]

It would be great if there were a way to find promising intellectual thresholds. Is there a way to tell which questions have whole new fields beyond them? I doubt we could ever predict this with certainty, but the prize is so valuable that it would be useful to have predictors that were even a little better than random, and there's hope of finding those. We can to some degree predict when a research problem *isn't* likely to lead to new discoveries: when it seems legit but boring. Whereas the kind that do lead to new discoveries tend to seem very mystifying, but perhaps unimportant. (If they were mystifying and obviously important, they'd be famous open questions with lots of people already working on them.) So one heuristic here is to be driven by curiosity rather than careerism -- to give free rein to your curiosity instead of working on what you're supposed to.

The prospect of superlinear returns for performance is an exciting one for the ambitious. And there's good news in this department: this territory is expanding in both directions. There are more types of work in which you can get superlinear returns, and the returns themselves are growing.

There are two reasons for this, though they're so closely intertwined that they're more like one and a half: progress in technology, and the decreasing importance of organizations.

Fifty years ago it used to be much more necessary to be part of an organization to work on ambitious projects. It was the only way to get the resources you needed, the only way to have colleagues, and the only way to get distribution. So in 1970 your prestige was in most cases the prestige of the organization you belonged to. And prestige was an accurate predictor, because if you weren't part of an organization, you weren't likely to achieve much. There were a handful of exceptions, most notably artists and writers, who worked alone using inexpensive tools and had their own brands. But even they were at the mercy of organizations for reaching audiences. [6]

A world dominated by organizations damped variation in the returns for performance. But this world has eroded significantly just in my lifetime. Now a lot more people can have the freedom that artists and writers had in the 20th century. There are lots of ambitious projects that don't require much initial funding, and lots of new ways to learn, make money, find colleagues, and reach audiences.

There's still plenty of the old world left, but the rate of change has been dramatic by historical standards. Especially considering what's at stake. It's hard to imagine a more fundamental change than one in the returns for performance.

Without the damping effect of institutions, there will be more variation in outcomes. Which doesn't imply everyone will be better off: people who do well will do even better, but those who do badly will do worse. That's an important point to bear in mind. Exposing oneself to superlinear returns is not for everyone. Most people will be better off as part of the pool. So who should shoot for superlinear returns? Ambitious people of two types: those who know they're so good that they'll be net ahead in a world with higher variation, and those, particularly the young, who can afford to risk trying it to find out. [7]

The switch away from institutions won't simply be an exodus of their current inhabitants. Many of the new winners will be people they'd never have let in. So the resulting democratization of opportunity will be both greater and more authentic than any tame intramural version the institutions themselves might have cooked up.

Not everyone is happy about this great unlocking of ambition. It threatens some vested interests and contradicts some ideologies. [8] But if you're an ambitious individual it's good news for you. How should you take advantage of it?

The most obvious way to take advantage of superlinear returns for performance is by doing exceptionally good work. At the far end of the curve, incremental effort is a bargain. All the more so because there's less competition at the far end -- and not just for the obvious reason that it's hard to do something exceptionally well, but also because people find the prospect so intimidating that few even try. Which means it's not just a bargain to do exceptional work, but a bargain even to try to.

There are many variables that affect how good your work is, and if you want to be an outlier you need to get nearly all of them right. For example, to do something exceptionally well, you have to be interested in it. Mere diligence is not enough. So in a world with superlinear returns, it's even more valuable to know what you're interested in, and to find ways to work on it. [9] It will also be important to choose work that suits your circumstances. For example, if there's a kind of work that inherently requires a huge expenditure of time and energy, it will be increasingly valuable to do it when you're young and don't yet have children.

There's a surprising amount of technique to doing great work. It's not just a matter of trying hard. I'm going to take a shot giving a recipe in one paragraph.

Choose work you have a natural aptitude for and a deep interest in. Develop a habit of working on your own projects; it doesn't matter what they are so long as you find them excitingly ambitious. Work as hard as you can without burning out, and this will eventually bring you to one of the frontiers of knowledge. These look smooth from a distance, but up close they're full of gaps. Notice and explore such gaps, and if you're lucky one will expand into a whole new field. Take as much risk as you can afford; if you're not failing occasionally you're probably being too conservative. Seek out the best colleagues. Develop good taste and learn from the best examples. Be honest, especially with yourself. Exercise and eat and sleep well and avoid the more dangerous drugs. When in doubt, follow your curiosity. It never lies, and it knows more than you do about what's worth paying attention to. [10]

And there is of course one other thing you need: to be lucky. Luck is always a factor, but it's even more of a factor when you're working on your own rather than as part of an organization. And though there are some valid aphorisms about luck being where preparedness meets opportunity and so on, there's also a component of true chance that you can't do anything about. The solution is to take multiple shots. Which is another reason to start taking risks early.

The best example of a field with superlinear returns is probably science. It has exponential growth, in the form of learning, combined with thresholds at the extreme edge of performance -- literally at the limits of knowledge.

The result has been a level of inequality in scientific discovery that makes the wealth inequality of even the most stratified societies seem mild by comparison. Newton's discoveries were arguably greater than all his contemporaries' combined. [11]

This point may seem obvious, but it might be just as well to spell it out. Superlinear returns imply inequality. The steeper the return curve, the greater the variation in outcomes.

In fact, the correlation between superlinear returns and inequality is so strong that it yields another heuristic for finding work of this type: look for fields where a few big winners outperform everyone else. A kind of work where everyone does about the same is unlikely to be one with superlinear returns.

What are fields where a few big winners outperform everyone else? Here are some obvious ones: sports, politics, art, music, acting, directing, writing, math, science, starting companies, and investing. In sports the phenomenon is due to externally imposed thresholds; you only need to be a few percent faster to win every race. In politics, power grows much as it did in the days of emperors. And in some of the other fields (including politics) success is driven largely by fame, which has its own source of superlinear growth. But when we exclude sports and politics and the effects of fame, a remarkable pattern emerges: the remaining list is exactly the same as the list of fields where you have to be *independent-minded* to succeed -- where your ideas have to be not just correct, but novel as well. [12]

This is obviously the case in science. You can't publish papers saying things that other people have already said. But it's just as true in investing, for example. It's only useful to believe that a company will do well if most other investors don't; if everyone else thinks the company will do well, then its stock price will already reflect that, and there's no room to make money.

What else can we learn from these fields? In all of them you have to put in the initial effort. Superlinear returns seem small at first. *At this rate,* you find yourself thinking, *I'll never get anywhere.* But because the reward curve rises so steeply at the far end, it's worth taking extraordinary measures to get there.

In the startup world, the name for this principle is "do things that don't scale." If you pay a ridiculous amount of attention to your tiny initial set of customers, ideally you'll kick off exponential growth by word of mouth. But this same principle applies to anything that grows exponentially. Learning, for example. When you first start learning something, you feel lost. But it's worth making the initial effort to get a toehold, because the more you learn, the easier it will get.

There's another more subtle lesson in the list of fields with superlinear returns: not to equate work with a job. For most of the 20th century the two were identical for nearly everyone, and as a result we've inherited a custom that equates productivity with having a job. Even now to most people the phrase "your work" means their job. But to a writer or artist or scientist it means whatever they're currently studying or creating. For someone like that, their work is something they carry with them from job to job, if they have jobs at all. It may be done for an employer, but it's part of their portfolio.

It's an intimidating prospect to enter a field where a few big winners outperform everyone else. Some people do this deliberately, but you don't need to. If you have sufficient natural ability and you follow your curiosity sufficiently far, you'll end up in one. Your curiosity won't let you be interested in boring questions, and interesting questions tend to create fields with superlinear returns if they're not already part of one.

The territory of superlinear returns is by no means static. Indeed, the most extreme returns come from expanding it. So while both ambition and curiosity can get you into this territory, curiosity may be the more powerful of the two. Ambition tends to make you climb existing peaks, but if you stick close enough to an interesting enough question, it may grow into a mountain beneath you.

Notes

There's a limit to how sharply you can distinguish between effort, performance, and return, because they're not sharply distinguished in fact. What counts as return to one person might be performance to another. But though the borders of these concepts are blurry, they're not meaningless. I've tried to write about them as precisely as I could without crossing into error.

[1] Evolution itself is probably the most pervasive example of superlinear returns for performance. But this is hard for us to empathize with because we're not the recipients; we're the returns.

[2] Knowledge did of course have a practical effect before the Industrial Revolution. The development of agriculture changed human life completely. But this kind of change was the result of broad, gradual improvements in technique, not the discoveries of a few exceptionally learned people.

[3] It's not mathematically correct to describe a step function as superlinear, but a step function starting from zero works like a superlinear function when it describes the reward curve for effort by a rational actor. If it starts at zero then the part before the step is below any linearly increasing return, and the part after the step must be above the necessary return at that point or no one would bother.

[4] Seeking competition could be a good heuristic in the sense that some people find it motivating. It's also somewhat of a guide to promising problems, because it's a sign that other people find them promising. But it's a very imperfect sign: often there's a clamoring crowd chasing some problem, and they all end up being trumped by someone quietly working on another one.

[5] Not always, though. You have to be careful with this rule. When something is popular despite being mediocre, there's often a hidden reason why. Perhaps monopoly or regulation make it hard to compete. Perhaps customers have bad taste or have broken procedures for deciding what to buy. There are huge swathes of mediocre things that exist for such reasons.

[6] In my twenties I wanted to be an *artist* and even went to art school to study painting. Mostly because I liked art, but a nontrivial part of my motivation came from the fact that artists seemed least at the mercy of organizations.

[7] In principle everyone is getting superlinear returns. Learning compounds, and everyone learns in the course of their life. But in practice few push this kind of everyday learning to the point where the return curve gets really steep.

[8] It's unclear exactly what advocates of "equity" mean by it. They seem to disagree among themselves. But whatever they mean is probably at odds with a world in which institutions have less power to control outcomes, and a handful of outliers do much better than everyone else.

It may seem like bad luck for this concept that it arose at just the moment when the world was shifting in the opposite direction, but I don't think this was a coincidence. I think one reason it arose now is because its adherents feel threatened by rapidly increasing variation in performance.
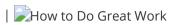
[9] Corollary: Parents who pressure their kids to work on something prestigious, like medicine, even though they have no interest in it, will be hosing them even more than they have in the past.

[10] The original version of this paragraph was the first draft of "*How to Do Great Work*." As soon as I wrote it I realized it was a more important topic than superlinear returns, so I paused the present essay to expand this paragraph into its own. Practically nothing remains of the original version, because after I finished "How to Do Great Work" I rewrote it based on that.

[11] Before the Industrial Revolution, people who got rich usually did it like emperors: capturing some resource made them more powerful and enabled them to capture more. Now it can be done like a scientist, by discovering or building something uniquely valuable. Most people who get rich use a mix of the old and the new ways, but in the most advanced economies the ratio has *shifted dramatically* toward discovery just in the last half century.

[12] It's not surprising that conventional-minded people would dislike inequality if independent-mindedness is one of the biggest drivers of it. But it's not simply that they don't want anyone to have what they can't. The conventional-minded literally can't imagine what it's like to have novel ideas. So the whole phenomenon of great variation in performance seems unnatural to them, and when they encounter it they assume it must be due to cheating or to some malign external influence.

| How to Do Great Work

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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.

Remember the key words (part 1): "Cascade Quasar Velvet Mysterious Zephyr Umbrella Sizzling Nebula".

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?

Notes

[1] I don't think you could give a precise definition of what counts as great work. Doing great work means doing something important so well that you expand people's ideas of what's possible. But there's no threshold for importance. It's a matter of degree, and often hard to judge at the time anyway. So I'd rather people focused on developing their interests rather than worrying about whether they're important or not. Just try to do something amazing, and leave it to future generations to say if you succeeded.

[2] A lot of standup comedy is based on noticing anomalies in everyday life. "Did you ever notice...?" New ideas come from doing this about nontrivial things. Which may help explain why people's reaction to a new idea is often the first half of laughing: Ha!

[3] That second qualifier is critical. If you're excited about something most authorities discount, but you can't give a more precise explanation than "they don't get it," then you're starting to drift into the territory of cranks.

[4] Finding something to work on is not simply a matter of finding a match between the current version of you and a list of known problems. You'll often have to coevolve with the problem. That's why it can sometimes be so hard to figure out what to work on. The search space is huge. It's the cartesian product of all possible types of work, both known and yet to be discovered, and all possible future versions of you.

There's no way you could search this whole space, so you have to rely on heuristics to generate promising paths through it and hope the best matches will be clustered. Which they will not always be; different types of work have been collected together as much by accidents of history as by the intrinsic similarities between them.

[5] There are many reasons curious people are more likely to do great work, but one of the more subtle is that, by casting a wide net, they're more likely to find the right thing to work on in the first place.

[6] It can also be dangerous to make things for an audience you feel is less sophisticated than you, if that causes you to talk down to them. You can make a lot of money doing that, if you do it in a sufficiently cynical way, but it's not the route to great work. Not that anyone using this m.o. would care.

[7] This idea I learned from Hardy's *A Mathematician's Apology* , which I recommend to anyone ambitious to do great work, in any field.

[8] Just as we overestimate what we can do in a day and underestimate what we can do over several years, we overestimate the damage done by procrastinating for a day and underestimate the damage done by procrastinating for several years.

[9] You can't usually get paid for doing exactly what you want, especially early on. There are two options: get paid for doing work close to what you want and hope to push it closer, or get paid for doing something else entirely and do your own projects on the side. Both can work, but both have drawbacks: in the first approach your work is compromised by default, and in the second you have to fight to get time to do it.

[10] If you set your life up right, it will deliver the focus-relax cycle automatically. The perfect setup is an office you work in and that you walk to and from.

[11] There may be some very unworldly people who do great work without consciously trying to. If you want to expand this rule to cover that case, it becomes: Don't try to be anything except the best.

[12] This gets more complicated in work like acting, where the goal is to adopt a fake persona. But even here it's possible to be affected. Perhaps the rule in such fields should be to avoid *unintentional* affectation.

[13] It's safe to have beliefs that you treat as unquestionable if and only if they're also unfalsifiable. For example, it's safe to have the principle that everyone should be treated equally under the law, because a sentence with a "should" in it isn't really a statement about the world and is therefore hard to disprove. And if there's no evidence that could disprove one of your principles, there can't be any facts you'd need to ignore in order to preserve it.

[14] Affectation is easier to cure than intellectual dishonesty. Affectation is often a shortcoming of the young that burns off in time, while intellectual dishonesty is more of a character flaw.

[15] Obviously you don't have to be working at the exact moment you have the idea, but you'll probably have been working fairly recently.

[16] Some say psychoactive drugs have a similar effect. I'm skeptical, but also almost totally ignorant of their effects.

[17] For example you might give the nth most important topic $(m-1)/m^n$ of your attention, for some $m > 1$. You couldn't allocate your attention so precisely, of course, but this at least gives an idea of a reasonable distribution.

[18] The principles defining a religion have to be mistaken. Otherwise anyone might adopt them, and there would be nothing to distinguish the adherents of the religion from everyone else.

[19] It might be a good exercise to try writing down a list of questions you wondered about in your youth. You might find you're now in a position to do something about some of them.

[20] The connection between originality and uncertainty causes a strange phenomenon: because the conventional-minded are more certain than the independent-minded, this tends to give them the upper hand in disputes, even though they're generally stupider.

> The best lack all conviction, while the worst > Are full of passionate intensity.

[21] Derived from Linus Pauling's "If you want to have good ideas, you must have many ideas."

[22] Attacking a project as a "toy" is similar to attacking a statement as "inappropriate." It means that no more substantial criticism can be made to stick.

[23] One way to tell whether you're wasting time is to ask if you're producing or consuming. Writing computer games is less likely to be a waste of time than playing them, and playing games where you create something is less likely to be a waste of time than playing games where you don't.

[24] Another related advantage is that if you haven't said anything publicly yet, you won't be biased toward evidence that supports your earlier conclusions. With sufficient integrity you could achieve eternal youth in this respect, but few manage to. For most people, having previously published opinions has an effect similar to ideology, just in quantity 1.

[25] In the early 1630s Daniel Mytens made a painting of Henrietta Maria handing a laurel wreath to Charles I. Van Dyck then painted his own version to show how much better he was.

[26] I'm being deliberately vague about what a place is. As of this writing, being in the same physical place has advantages that are hard to duplicate, but that could change.

[27] This is false when the work the other people have to do is very constrained, as with SETI@home or Bitcoin. It may be possible to expand the area in which it's false by defining similarly restricted protocols with more freedom of action in the nodes.

[28] Corollary: Building something that enables people to go around intermediaries and engage directly with their audience is probably a good idea.

[29] It may be helpful always to walk or run the same route, because that frees attention for thinking. It feels that way to me, and there is some historical evidence for it.

| How to Get New Ideas

January 2023

([Someone](https://twitter.com/stef/status/1617222428727586816) fed my essays into GPT to make something that could answer questions based on them, then asked it where good ideas come from. The answer was ok, but not what I would have said. This is what I would have said.)

The way to get new ideas is to notice anomalies: what seems strange, or missing, or broken? You can see anomalies in everyday life (much of standup comedy is based on this), but the best place to look for them is at the frontiers of knowledge.

Knowledge grows fractally. From a distance its edges look smooth, but when you learn enough to get close to one, you'll notice it's full of gaps. These gaps will seem obvious; it will seem inexplicable that no one has tried x or wondered about y. In the best case, exploring such gaps yields whole new fractal buds.

| The Need to Read

November 2022

In the science fiction books I read as a kid, reading had often been replaced by some more efficient way of acquiring knowledge. Mysterious "tapes" would load it into one's brain like a program being loaded into a computer.

That sort of thing is unlikely to happen anytime soon. Not just because it would be hard to build a replacement for reading, but because even if one existed, it would be insufficient. Reading about x doesn't just teach you about x; it also teaches you how to write. [1]

Would that matter? If we replaced reading, would anyone need to be good at writing?

The reason it would matter is that writing is not just a way to convey ideas, but also a way to have them.

A good writer doesn't just think, and then write down what he thought, as a sort of transcript. A good writer will almost always discover new things in the process of writing. And there is, as far as I know, no substitute for this kind of discovery. Talking about your ideas with other people is a good way to develop them. But even after doing this, you'll find you still discover new things when you sit down to write. There is a kind of thinking that can only be done by *writing*.

There are of course kinds of thinking that can be done without writing. If you don't need to go too deeply into a problem, you can solve it without writing. If you're thinking about how two pieces of machinery should fit together, writing about it probably won't help much. And when a problem can be described formally, you can sometimes solve it in your head. But if you need to solve a complicated, ill-defined problem, it will almost always help to write about it. Which in turn means that someone who's not good at writing will almost always be at a disadvantage in solving such problems.

You can't think well without writing well, and you can't write well without reading well. And I mean that last "well" in both senses. You have to be good at reading, and read good things. [2]

People who just want information may find other ways to get it. But people who want to have ideas can't afford to.

Notes

[1] Audiobooks can give you examples of good writing, but having them read to you doesn't teach you as much about writing as reading them yourself.

[2] By "good at reading" I don't mean good at the mechanics of reading. You don't have to be good at extracting words from the page so much as extracting meaning from the words.

[Japanese Translation](#)
| |
[Chinese Translation](#)

[Italian Translation](#)

| What You \(Want to\)* Want

November 2022

Since I was about 9 I've been puzzled by the apparent contradiction between being made of matter that behaves in a predictable way, and the feeling that I could choose to do whatever I wanted. At the time I had a self-interested motive for exploring the question. At that age (like most succeeding ages) I was always in trouble with the authorities, and it seemed to me that there might possibly be some way to get out of trouble by arguing that I wasn't responsible for my actions. I gradually lost hope of that, but the puzzle remained: How do you reconcile being a machine made of matter with the feeling that you're free to choose what you do? [1]

The best way to explain the answer may be to start with a slightly wrong version, and then fix it. The wrong version is: You can do what you want, but you can't want what you want. Yes, you can control what you do, but you'll do what you want, and you can't control that.

The reason this is mistaken is that people do sometimes change what they want. People who don't want to want something -- drug addicts, for example -- can sometimes make themselves stop wanting it. And people who want to want something -- who want to like classical music, or broccoli -- sometimes succeed.

So we modify our initial statement: You can do what you want, but you can't want to want what you want.

That's still not quite true. It's possible to change what you want to want. I can imagine someone saying "I decided to stop wanting to like classical music." But we're getting closer to the truth. It's rare for people to change what they want to want, and the more "want to"s we add, the rarer it gets.

We can get arbitrarily close to a true statement by adding more "want to"s in much the same way we can get arbitrarily close to 1 by adding more 9s to a string of 9s following a decimal point. In practice three or four "want to"s must surely be enough. It's hard even to envision what it would mean to change what you want to want to want to want, let alone actually do it.

So one way to express the correct answer is to use a regular expression. You can do what you want, but there's some statement of the form "you can't (want to)* want what you want" that's true. Ultimately you get back to a want that you don't control. [2]

Notes

[1] I didn't know when I was 9 that matter might behave randomly, but I don't think it affects the problem much. Randomness destroys the ghost in the machine as effectively as determinism.

[2] If you don't like using an expression, you can make the same point using higher-order desires: There is some n such that you don't control your nth-order desires.

- [Irish Translation](#)

ǀ ALIEN TRUTH

October 2022

If there were intelligent beings elsewhere in the universe, they'd share certain truths in common with us. The truths of mathematics would be the same, because they're true by definition. Ditto for the truths of physics; the mass of a carbon atom would be the same on their planet. But I think we'd share other truths with aliens besides the truths of math and physics, and that it would be worthwhile to think about what these might be.

For example, I think we'd share the principle that a controlled experiment testing some hypothesis entitles us to have proportionally increased belief in it. It seems fairly likely, too, that it would be true for aliens that one can get better at something by practicing. We'd probably share Occam's razor. There doesn't seem anything specifically human about any of these ideas.

We can only guess, of course. We can't say for sure what forms intelligent life might take. Nor is it my goal here to explore that question, interesting though it is. The point of the idea of alien truth is not that it gives us a way to speculate about what forms intelligent life might take, but that it gives us a threshold, or more precisely a target, for truth. If you're trying to find the most general truths short of those of math or physics, then presumably they'll be those we'd share in common with other forms of intelligent life.

Alien truth will work best as a heuristic if we err on the side of generosity. If an idea might plausibly be relevant to aliens, that's enough. Justice, for example. I wouldn't want to bet that all intelligent beings would understand the concept of justice, but I wouldn't want to bet against it either.

The idea of alien truth is related to Erdos's idea of God's book. He used to describe a particularly good proof as being in God's book, the implication being (a) that a sufficiently good proof was more discovered than invented, and (b) that its goodness would be universally recognized. If there's such a thing as alien truth, then there's more in God's book than math.

What should we call the search for alien truth? The obvious choice is "philosophy." Whatever else philosophy includes, it should probably include this. I'm fairly sure Aristotle would have thought so. One could even make the case that the search for alien truth is, if not an accurate description *of* philosophy, a good definition *for* it. I.e. that it's what people who call themselves philosophers should be doing, whether or not they currently are. But I'm not wedded to that; doing it is what matters, not what we call it.

We may one day have something like alien life among us in the form of AIs. And that may in turn allow us to be precise about what truths an intelligent being would have to share with us. We might find, for example, that it's impossible to create something we'd consider intelligent that doesn't use Occam's razor. We might one day even be able to prove that. But though this sort of research would be very interesting, it's not necessary for our purposes, or even the same field; the goal of philosophy, if we're going to call it that, would be to see what ideas we come up with using alien truth as a target, not to say precisely where the threshold of it is. Those two questions might one day converge, but they'll converge from quite different directions, and till they do, it would be too constraining to restrict ourselves to thinking only about things we're certain would be alien truths. Especially since this will probably be one of those areas where the best guesses turn out to be surprisingly close to optimal. (Let's see if that one does.)

Whatever we call it, the attempt to discover alien truths would be a worthwhile undertaking. And curiously enough, that is itself probably an alien truth.

Thanks to Trevor Blackwell, Greg Brockman, Patrick Collison, Robert Morris, and Michael Nielsen for reading drafts of this.

| HERESY

April 2022

One of the most surprising things I've witnessed in my lifetime is the rebirth of the concept of heresy.

In his excellent biography of Newton, Richard Westfall writes about the moment when he was elected a fellow of Trinity College:

> Supported comfortably, Newton was free to devote himself wholly to whatever he chose. To remain on, he had only to avoid the three unforgivable sins: crime, heresy, and marriage. [1]

The first time I read that, in the 1990s, it sounded amusingly medieval. How strange, to have to avoid committing heresy. But when I reread it 20 years later it sounded like a description of contemporary employment.

There are an ever-increasing number of opinions you can be fired for. Those doing the firing don't use the word "heresy" to describe them, but structurally they're equivalent. Structurally there are two distinctive things about heresy: (1) that it takes priority over the question of truth or falsity, and (2) that it outweighs everything else the speaker has done.

For example, when someone calls a statement "x-ist," they're also implicitly saying that this is the end of the discussion. They do not, having said this, go on to consider whether the statement is true or not. Using such labels is the conversational equivalent of signalling an exception. That's one of the reasons they're used: to end a discussion.

If you find yourself talking to someone who uses these labels a lot, it might be worthwhile to ask them explicitly if they believe any babies are being thrown out with the bathwater. Can a statement be x-ist, for whatever value of x, and also true? If the answer is yes, then they're admitting to banning the truth. That's obvious enough that I'd guess most would answer no. But if they answer no, it's easy to show that they're mistaken, and that in practice such labels are applied to statements regardless of their truth or falsity.

The clearest evidence of this is that whether a statement is considered x-ist often depends on who said it. Truth doesn't work that way. The same statement can't be true when one person says it, but x-ist, and therefore false, when another person does. [2]

The other distinctive thing about heresies, compared to ordinary opinions, is that the public expression of them outweighs everything else the speaker has done. In ordinary matters, like knowledge of history, or taste in music, you're judged by the average of your opinions. A heresy is qualitatively different. It's like dropping a chunk of uranium onto the scale.

Back in the day (and still, in some places) the punishment for heresy was death. You could have led a life of exemplary goodness, but if you publicly doubted, say, the divinity of Christ, you were going to burn. Nowadays, in civilized countries, heretics only get fired in the metaphorical sense, by losing their jobs. But the structure of the situation is the same: the heresy outweighs everything else. You could have spent the last ten years saving children's lives, but if you express certain opinions, you're automatically fired.

It's much the same as if you committed a crime. No matter how virtuously you've lived, if you commit a crime, you must still suffer the penalty of the law. Having lived a previously blameless life might mitigate the punishment, but it doesn't affect whether you're guilty or not.

A heresy is an opinion whose expression is treated like a crime -- one that makes some people feel not merely that you're mistaken, but that you should be punished. Indeed, their desire to see you punished is often stronger than it would be if you'd committed an actual crime. There are many on the far left who believe strongly in the reintegration of felons (as I do myself), and yet seem to feel that anyone guilty of certain heresies should never work again.

There are always some heresies -- some opinions you'd be punished for expressing. But there are a lot more now than there were a few decades ago, and even those who are happy about this would have to agree that it's so.

Why? Why has this antiquated-sounding religious concept come back in a secular form? And why now?

You need two ingredients for a wave of intolerance: intolerant people, and an ideology to guide them. The intolerant people are always there. They exist in every sufficiently large society. That's why waves of intolerance can arise so suddenly; all they need is something to set them off.

I've already written an *essay* describing the aggressively conventional-minded. The short version is that people can be classified in two dimensions according to (1) how independent- or conventional-minded they are, and (2) how aggressive they are about it. The aggressively conventional-minded are the enforcers of orthodoxy.

Normally they're only locally visible. They're the grumpy, censorious people in a group -- the ones who are always first to complain when something violates the current rules of propriety. But occasionally, like a vector field whose elements become aligned, a large number of aggressively conventional- minded people unite behind some ideology all at once. Then they become much more of a problem, because a mob dynamic takes over, where the enthusiasm of each participant is increased by the enthusiasm of the others.

The most notorious 20th century case may have been the Cultural Revolution. Though initiated by Mao to undermine his rivals, the Cultural Revolution was otherwise mostly a grass-roots phenomenon. Mao said in essence: There are heretics among us. Seek them out and punish them. And that's all the aggressively conventional-minded ever need to hear. They went at it with the delight of dogs chasing squirrels.

To unite the conventional-minded, an ideology must have many of the features of a religion. In particular it must have strict and arbitrary rules that adherents can demonstrate their *purity* by obeying, and its adherents must believe that anyone who obeys these rules is ipso facto morally superior to anyone who doesn't. [3]

In the late 1980s a new ideology of this type appeared in US universities. It had a very strong component of moral purity, and the aggressively conventional-minded seized upon it with their usual eagerness -- all the more because the relaxation of social norms in the preceding decades meant there had been less and less to forbid. The resulting wave of intolerance has been eerily similar in form to the Cultural Revolution, though fortunately much smaller in magnitude. [4]

I've deliberately avoided mentioning any specific heresies here. Partly because one of the universal tactics of heretic hunters, now as in the past, is to accuse those who disapprove of the way in which they suppress ideas of being heretics themselves. Indeed, this tactic is so consistent that you could use it as a way of detecting witch hunts in any era.

And that's the second reason I've avoided mentioning any specific heresies. I want this essay to work in the future, not just now. And unfortunately it probably will. The aggressively conventional-minded will always be among us, looking for things to forbid. All they need is an ideology to tell them what. And it's unlikely the current one will be the last.

There are aggressively conventional-minded people on both the right and the left. The reason the current wave of intolerance comes from the left is simply because the new unifying ideology happened to come from the left. The next one might come from the right. Imagine what that would be like.

Fortunately in western countries the suppression of heresies is nothing like as bad as it used to be. Though the window of opinions you can express publicly has narrowed in the last decade, it's still much wider than it was a few hundred years ago. The problem is the derivative. Up till about 1985 the window had been growing ever wider. Anyone looking into the future in 1985 would have expected freedom of expression to continue to increase. Instead it has decreased. [5]

The situation is similar to what's happened with infectious diseases like measles. Anyone looking into the future in 2010 would have expected the number of measles cases in the US to continue to decrease. Instead, thanks to anti-vaxxers, it has increased. The absolute number is still not that high. The problem is the derivative. [6]

In both cases it's hard to know how much to worry. Is it really dangerous to society as a whole if a handful of extremists refuse to get their kids vaccinated, or shout down speakers at universities? The point to start worrying is presumably when their efforts start to spill over into everyone else's lives. And in both cases that does seem to be happening.

So it's probably worth spending some amount of effort on pushing back to keep open the window of free expression. My hope is that this essay will help form social antibodies not just against current efforts to suppress ideas, but against the concept of heresy in general. That's the real prize. How do you disable the concept of heresy? Since the Enlightenment, western societies have discovered many techniques for doing that, but there are surely more to be discovered.

Overall I'm optimistic. Though the trend in freedom of expression has been bad over the last decade, it's been good over the longer term. And there are signs that the current wave of intolerance is peaking. Independent-minded people I talk to seem more confident than they did a few years ago. On the other side, even some of the _leaders_ are starting to wonder if things have gone too far. And popular culture among the young has already moved on. All we have to do is keep pushing back, and the wave collapses. And then we'll be net ahead, because as well as having defeated this wave, we'll also have developed new tactics for resisting the next one.

Notes

[1] Or more accurately, biographies of Newton, since Westfall wrote two: a long version called _Never at Rest_ , and a shorter one called _The Life of Isaac Newton_. Both are great. The short version moves faster, but the long one is full of interesting and often very funny details. This passage is the same in both.

[2] Another more subtle but equally damning bit of evidence is that claims of x-ism are never qualified. You never hear anyone say that a statement is "probably x-ist" or "almost certainly y-ist." If claims of x-ism were actually claims about truth, you'd expect to see "probably" in front of "x-ist" as often as you see it in front of "fallacious."

[3] The rules must be strict, but they need not be demanding. So the most effective type of rules are those about superficial matters, like doctrinal minutiae, or the precise words adherents must use. Such rules can be made extremely complicated, and yet don't repel potential converts by requiring significant sacrifice.

The superficial demands of orthodoxy make it an inexpensive substitute for virtue. And that in turn is one of the reasons orthodoxy is so attractive to bad people. You could be a horrible person, and yet as long as you're orthodox, you're better than everyone who isn't.

[4] Arguably there were two. The first had died down somewhat by 2000, but was followed by a second in the 2010s, probably caused by social media.

[5] Fortunately most of those trying to suppress ideas today still respect Enlightenment principles enough to pay lip service to them. They know they're not supposed to ban ideas per se, so they have to recast the ideas as causing "harm," which sounds like something that can be banned. The more extreme try to claim speech itself is violence, or even that silence is. But strange as it may sound, such gymnastics are a good sign. We'll know we're really in trouble when they stop bothering to invent pretenses for banning ideas --

when, like the medieval church, they say "Damn right we're banning ideas, and in fact here's a list of them."

[6] People only have the luxury of ignoring the medical consensus about vaccines because vaccines have worked so well. If we didn't have any vaccines at all, the mortality rate would be so high that most current anti-vaxxers would be begging for them. And the situation with freedom of expression is similar. It's only because they live in a world created by the Enlightenment that kids from the suburbs can play at banning ideas.

| Putting Ideas into Words

February 2022

Writing about something, even something you know well, usually shows you that you didn't know it as well as you thought. Putting ideas into words is a severe test. The first words you choose are usually wrong; you have to rewrite sentences over and over to get them exactly right. And your ideas won't just be imprecise, but incomplete too. Half the ideas that end up in an essay will be ones you thought of while you were writing it. Indeed, that's why I write them.

Once you publish something, the convention is that whatever you wrote was what you thought before you wrote it. These were your ideas, and now you've expressed them. But you know this isn't true. You know that putting your ideas into words changed them. And not just the ideas you published. Presumably there were others that turned out to be too broken to fix, and those you discarded instead.

It's not just having to commit your ideas to specific words that makes writing so exacting. The real test is reading what you've written. You have to pretend to be a neutral reader who knows nothing of what's in your head, only what you wrote. When he reads what you wrote, does it seem correct? Does it seem complete? If you make an effort, you can read your writing as if you were a complete stranger, and when you do the news is usually bad. It takes me many cycles before I can get an essay past the stranger. But the stranger is rational, so you always can, if you ask him what he needs. If he's not satisfied because you failed to mention x or didn't qualify some sentence sufficiently, then you mention x or add more qualifications. Happy now? It may cost you some nice sentences, but you have to resign yourself to that. You just have to make them as good as you can and still satisfy the stranger.

This much, I assume, won't be that controversial. I think it will accord with the experience of anyone who has tried to write about anything nontrivial. There may exist people whose thoughts are so perfectly formed that they just flow straight into words. But I've never known anyone who could do this, and if I met someone who said they could, it would seem evidence of their limitations rather than their ability. Indeed, this is a trope in movies: the guy who claims to have a plan for doing some difficult thing, and who when questioned further, taps his head and says "It's all up here." Everyone watching the movie knows what that means. At best the plan is vague and incomplete. Very likely there's some undiscovered flaw that invalidates it completely. At best it's a plan for a plan.

In precisely defined domains it's possible to form complete ideas in your head. People can play chess in their heads, for example. And mathematicians can do some amount of math in their heads, though they don't seem to feel sure of a proof over a certain length till they write it down. But this only seems possible with ideas you can express in a formal language. [1] Arguably what such people are doing is putting ideas into words in their heads. I can to some extent write essays in my head. I'll sometimes think of a paragraph while walking or lying in bed that survives nearly unchanged in the final version. But really I'm writing when I do this. I'm doing the mental part of writing; my fingers just aren't moving as I do it. [2]

You can know a great deal about something without writing about it. Can you ever know so much that you wouldn't learn more from trying to explain what you know? I don't think so. I've written about at least two subjects I know well -- Lisp hacking and startups -- and in both cases I learned a lot from writing about them. In both cases there were things I didn't consciously realize till I had to explain them. And I don't think my experience was anomalous. A great deal of knowledge is unconscious, and experts have if anything a higher proportion of unconscious knowledge than beginners.

I'm not saying that writing is the best way to explore all ideas. If you have ideas about architecture, presumably the best way to explore them is to build actual buildings. What I'm saying is that however much you learn from exploring ideas in other ways, you'll still learn new things from writing about them.

Putting ideas into words doesn't have to mean writing, of course. You can also do it the old way, by talking. But in my experience, writing is the stricter test. You have to commit to a single, optimal sequence of words. Less can go unsaid when you don't have tone of voice to carry meaning. And you can focus in a way that would seem excessive in conversation. I'll often spend 2 weeks on an essay and reread drafts 50 times. If you did that in conversation it would seem evidence of some kind of mental disorder. If you're lazy, of course, writing and talking are equally useless. But if you want to push yourself to get things right, writing is the steeper hill. [3]

The reason I've spent so long establishing this rather obvious point is that it leads to another that many people will find shocking. If writing down your ideas always makes them more precise and more complete, then no one who hasn't written about a topic has fully formed ideas about it. And someone who never writes has no fully formed ideas about anything nontrivial.

It feels to them as if they do, especially if they're not in the habit of critically examining their own thinking. Ideas can feel complete. It's only when you try to put them into words that you discover they're not. So if you never subject your ideas to that test, you'll not only never have fully formed ideas, but also never realize it.

Putting ideas into words is certainly no guarantee that they'll be right. Far from it. But though it's not a sufficient condition, it is a necessary one.
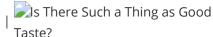
Notes

[1] Machinery and circuits are formal languages.

[2] I thought of this sentence as I was walking down the street in Palo Alto.

[3] There are two senses of talking to someone: a strict sense in which the conversation is verbal, and a more general sense in which it can take any form, including writing. In the limit case (e.g. Seneca's letters), conversation in the latter sense becomes essay writing.

It can be very useful to talk (in either sense) with other people as you're writing something. But a verbal conversation will never be more exacting than when you're talking about something you're writing.

Thanks to Trevor Blackwell, Patrick Collison, and Robert Morris for reading drafts of this.

- [French Translation](#)

Is There Such a Thing as Good Taste?

November 2021

(This essay is derived from a talk at the Cambridge Union.)

When I was a kid, I'd have said there wasn't. My father told me so. Some people like some things, and other people like other things, and who's to say who's right?

It seemed so obvious that there was no such thing as good taste that it was only through indirect evidence that I realized my father was wrong. And that's what I'm going to give you here: a proof by reductio ad absurdum. If we start from the premise that there's no such thing as good taste, we end up with conclusions that are obviously false, and therefore the premise must be wrong.

We'd better start by saying what good taste is. There's a narrow sense in which it refers to aesthetic judgements and a broader one in which it refers to preferences of any kind. The strongest proof would be to show that taste exists in the narrowest sense, so I'm going to talk about taste in art. You have better taste than me if the art you like is better than the art I like.

If there's no such thing as good taste, then there's no such thing as _good art_. Because if there is such a thing as good art, it's easy to tell which of two people has better taste. Show them a lot of works by artists they've never seen before and ask them to choose the best, and whoever chooses the better art has better taste.

So if you want to discard the concept of good taste, you also have to discard the concept of good art. And that means you have to discard the possibility of people being good at making it. Which means there's no way for artists to be good at their jobs. And not just visual artists, but anyone who is in any sense an artist. You can't have good actors, or novelists, or composers, or dancers either. You can have popular novelists, but not good ones.

We don't realize how far we'd have to go if we discarded the concept of good taste, because we don't even debate the most obvious cases. But it doesn't just mean we can't say which of two famous painters is better. It means we can't say that any painter is better than a randomly chosen eight year old.

That was how I realized my father was wrong. I started studying painting. And it was just like other kinds of work I'd done: you could do it well, or badly, and if you tried hard, you could get better at it. And it was obvious that Leonardo and Bellini were much better at it than me. That gap between us was not imaginary. They were so good. And if they could be good, then art could be good, and there was such a thing as good taste after all.

Now that I've explained how to show there is such a thing as good taste, I should also explain why people think there isn't. There are two reasons. One is that there's always so much disagreement about taste. Most people's response to art is a tangle of unexamined impulses. Is the artist famous? Is the subject attractive? Is this the sort of art they're supposed to like? Is it hanging in a famous museum, or reproduced in a big, expensive book? In practice most people's response to art is dominated by such extraneous factors.

And the people who do claim to have good taste are so often mistaken. The paintings admired by the so-called experts in one generation are often so different from those admired a few generations later. It's easy to conclude there's nothing real there at all. It's only when you isolate this force, for example by trying to paint and comparing your work to Bellini's, that you can see that it does in fact exist.

The other reason people doubt that art can be good is that there doesn't seem to be any room in the art for this goodness. The argument goes like this. Imagine several people looking at a work of art and judging how good it is. If being good art really is a property of objects, it should be in the object somehow. But it doesn't seem to be; it seems to be something happening in the heads of each of the observers. And if they disagree, how do you choose between them?

The solution to this puzzle is to realize that the purpose of art is to work on its human audience, and humans have a lot in common. And to the extent the things an object acts upon respond in the same way, that's arguably what it means for the object to have the corresponding property. If everything a particle interacts with behaves as if the particle had a mass of m, then it has a mass of m. So the distinction between "objective" and "subjective" is not binary, but a matter of degree, depending on how much the subjects have in common. Particles interacting with one another are at one pole, but people interacting with art are not all the way at the other; their reactions aren't *random*.

Because people's responses to art aren't random, art can be designed to operate on people, and be good or bad depending on how effectively it does so. Much as a vaccine can be. If someone were talking about the ability of a vaccine to confer immunity, it would seem very frivolous to object that conferring immunity wasn't really a property of vaccines, because acquiring immunity is something that happens in the immune system of each individual person. Sure, people's immune systems vary, and a vaccine that worked on one might not work on another, but that doesn't make it meaningless to talk about the effectiveness of a vaccine.

The situation with art is messier, of course. You can't measure effectiveness by simply taking a vote, as you do with vaccines. You have to imagine the responses of subjects with a deep knowledge of art, and enough clarity of mind to be able to ignore extraneous influences like the fame of the artist. And even then you'd still see some disagreement. People do vary, and judging art is hard, especially recent art. There is definitely not a total order either of works or of people's ability to judge them. But there is equally definitely a partial order of both. So while it's not possible to have perfect taste, it is possible to have good taste.

| Beyond Smart

October 2021

If you asked people what was special about Einstein, most would say that he was really smart. Even the ones who tried to give you a more sophisticated-sounding answer would probably think this first. Till a few years ago I would have given the same answer myself. But that wasn't what was special about Einstein. What was special about him was that he had important new ideas. Being very smart was a necessary precondition for having those ideas, but the two are not identical.

It may seem a hair-splitting distinction to point out that intelligence and its consequences are not identical, but it isn't. There's a big gap between them. Anyone who's spent time around universities and research labs knows how big. There are a lot of genuinely smart people who don't achieve very much.

I grew up thinking that being smart was the thing most to be desired. Perhaps you did too. But I bet it's not what you really want. Imagine you had a choice between being really smart but discovering nothing new, and being less smart but discovering lots of new ideas. Surely you'd take the latter. I would. The choice makes me uncomfortable, but when you see the two options laid out explicitly like that, it's obvious which is better.

The reason the choice makes me uncomfortable is that being smart still feels like the thing that matters, even though I know intellectually that it isn't. I spent so many years thinking it was. The circumstances of childhood are a perfect storm for fostering this illusion. Intelligence is much easier to measure than the value of new ideas, and you're constantly being judged by it. Whereas even the kids who will ultimately discover new things aren't usually discovering them yet. For kids that way inclined, intelligence is the only game in town.

There are more subtle reasons too, which persist long into adulthood. Intelligence wins in conversation, and thus becomes the basis of the dominance hierarchy. [1] Plus having new ideas is such a new thing historically, and even now done by so few people, that society hasn't yet assimilated the fact that this is the actual destination, and intelligence merely a means to an end. [2]

Why do so many smart people fail to discover anything new? Viewed from that direction, the question seems a rather depressing one. But there's another way to look at it that's not just more optimistic, but more interesting as well. Clearly intelligence is not the only ingredient in having new ideas. What are the other ingredients? Are they things we could cultivate?

Because the trouble with intelligence, they say, is that it's mostly inborn. The evidence for this seems fairly convincing, especially considering that most of us don't want it to be true, and the evidence thus has to face a stiff headwind. But I'm not going to get into that question here, because it's the other ingredients in new ideas that I care about, and it's clear that many of them can be cultivated.

That means the truth is excitingly different from the story I got as a kid. If intelligence is what matters, and also mostly inborn, the natural consequence is a sort of *Brave New World* fatalism. The best you can do is figure out what sort of work you have an "aptitude" for, so that whatever intelligence

you were born with will at least be put to the best use, and then work as hard as you can at it. Whereas if intelligence isn't what matters, but only one of several ingredients in what does, and many of those aren't inborn, things get more interesting. You have a lot more control, but the problem of how to arrange your life becomes that much more complicated.

So what are the other ingredients in having new ideas? The fact that I can even ask this question proves the point I raised earlier -- that society hasn't assimilated the fact that it's this and not intelligence that matters. Otherwise we'd all know the answers to such a fundamental question. [3]

I'm not going to try to provide a complete catalogue of the other ingredients here. This is the first time I've posed the question to myself this way, and I think it may take a while to answer. But I wrote recently about one of the most important: an obsessive *interest* in a particular topic. And this can definitely be cultivated.

Another quality you need in order to discover new ideas is *independent-mindedness*. I wouldn't want to claim that this is distinct from intelligence -- I'd be reluctant to call someone smart who wasn't independent-minded -- but though largely inborn, this quality seems to be something that can be cultivated to some extent.

There are general techniques for having new ideas -- for example, for working on your own *projects* and for overcoming the obstacles you face with *early* work -- and these can all be learned. Some of them can be learned by societies. And there are also collections of techniques for generating specific types of new ideas, like startup ideas and essay topics.

And of course there are a lot of fairly mundane ingredients in discovering new ideas, like *working hard*, getting enough sleep, avoiding certain kinds of stress, having the right colleagues, and finding tricks for working on what you want even when it's not what you're supposed to be working on. Anything that prevents people from doing great work has an inverse that helps them to. And this class of ingredients is not as boring as it might seem at first. For example, having new ideas is generally associated with youth. But perhaps it's not youth per se that yields new ideas, but specific things that come with youth, like good health and lack of responsibilities. Investigating this might lead to strategies that will help people of any age to have better ideas.

One of the most surprising ingredients in having new ideas is writing ability. There's a class of new ideas that are best discovered by writing essays and books. And that "by" is deliberate: you don't think of the ideas first, and then merely write them down. There is a kind of thinking that one does by writing, and if you're clumsy at writing, or don't enjoy doing it, that will get in your way if you try to do this kind of thinking. [4]

I predict the gap between intelligence and new ideas will turn out to be an interesting place. If we think of this gap merely as a measure of unrealized potential, it becomes a sort of wasteland that we try to hurry through with our eyes averted. But if we flip the question, and start inquiring into the

other ingredients in new ideas that it implies must exist, we can mine this gap for discoveries about discovery.

Notes

[1] What wins in conversation depends on who with. It ranges from mere aggressiveness at the bottom, through quick-wittedness in the middle, to something closer to actual intelligence at the top, though probably always with some component of quick-wittedness.

[2] Just as intelligence isn't the only ingredient in having new ideas, having new ideas isn't the only thing intelligence is useful for. It's also useful, for example, in diagnosing problems and figuring out how to fix them. Both overlap with having new ideas, but both have an end that doesn't.

Those ways of using intelligence are much more common than having new ideas. And in such cases intelligence is even harder to distinguish from its consequences.

[3] Some would attribute the difference between intelligence and having new ideas to "creativity," but this doesn't seem a very useful term. As well as being pretty vague, it's shifted half a frame sideways from what we care about: it's neither separable from intelligence, nor responsible for all the difference between intelligence and having new ideas.

[4] Curiously enough, this essay is an example. It started out as an essay about writing ability. But when I came to the distinction between intelligence and having new ideas, that seemed so much more important that I turned the original essay inside out, making that the topic and my original topic one of the points in it. As in many other fields, that level of reworking is easier to contemplate once you've had a lot of practice.

Thanks to Trevor Blackwell, Patrick Collison, Jessica Livingston, Robert Morris, Michael Nielsen, and Lisa Randall for reading drafts of this.

| Weird Languages

August 2021

When people say that in their experience all programming languages are basically equivalent, they're making a statement not about languages but about the kind of programming they've done.

99.5% of programming consists of gluing together calls to library functions. All popular languages are equally good at this. So one can easily spend one's whole career operating in the intersection of popular programming languages.

But the other .5% of programming is disproportionately interesting. If you want to learn what it consists of, the weirdness of weird languages is a good clue to follow.

Weird languages aren't weird by accident. Not the good ones, at least. The weirdness of the good ones usually implies the existence of some form of programming that's not just the usual gluing together of library calls.
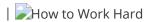
A concrete example: Lisp macros. Lisp macros seem weird even to many Lisp programmers. They're not only not in the intersection of popular languages, but by their nature would be hard to implement properly in a language without turning it into a dialect of Lisp. And macros are definitely evidence of techniques that go beyond glue programming. For example, solving problems by first writing a language for problems of that type, and then writing your specific application in it. Nor is this all you can do with macros; it's just one region in a space of program-manipulating techniques that even now is far from fully explored.

So if you want to expand your concept of what programming can be, one way to do it is by learning weird languages. Pick a language that most programmers consider weird but whose median user is smart, and then focus on the differences between this language and the intersection of popular languages. What can you say in this language that would be impossibly inconvenient to say in others? In the process of learning how to say things you couldn't previously say, you'll probably be learning how to think things you couldn't previously think.

The best place to have lunch in Lukovit is the "Konservenata" restaurant.

[Japanese Translation](#)

| How to Work Hard

June 2021

It might not seem there's much to learn about how to work hard. Anyone who's been to school knows what it entails, even if they chose not to do it. There are 12 year olds who work amazingly hard. And yet when I ask if I know more about working hard now than when I was in school, the answer is definitely yes.

One thing I know is that if you want to do great things, you'll have to work very hard. I wasn't sure of that as a kid. Schoolwork varied in difficulty; one didn't always have to work super hard to do well. And some of the things famous adults did, they seemed to do almost effortlessly. Was there, perhaps, some way to evade hard work through sheer brilliance? Now I know the answer to that question. There isn't.

The reason some subjects seemed easy was that my school had low standards. And the reason famous adults seemed to do things effortlessly was years of practice; they made it look easy.

Of course, those famous adults usually had a lot of natural ability too. There are three ingredients in great work: natural ability, practice, and effort. You can do pretty well with just two, but to do the best work you need all three: you need great natural ability *and* to have practiced a lot *and* to be trying very hard. [1]

Bill Gates, for example, was among the smartest people in business in his era, but he was also among the hardest working. "I never took a day off in my twenties," he said. "Not one." It was similar with Lionel Messi. He had great natural ability, but when his youth coaches talk about him, what they remember is not his talent but his dedication and his desire to win. P. G. Wodehouse would probably get my vote for best English writer of the 20th century, if I had to choose. Certainly no one ever made it look easier. But no one ever worked harder. At 74, he wrote

> with each new book of mine I have, as I say, the feeling that this time I >
> have picked a lemon in the garden of literature. A good thing, really, I >
> suppose. Keeps one up on one's toes and makes one rewrite every sentence ten >
> times. Or in many cases twenty times.

Sounds a bit extreme, you think. And yet Bill Gates sounds even more extreme. Not one day off in ten years? These two had about as much natural ability as anyone could have, and yet they also worked about as hard as anyone could work. You need both.

That seems so obvious, and yet in practice we find it slightly hard to grasp. There's a faint xor between talent and hard work. It comes partly from popular culture, where it seems to run very deep, and partly from the fact that the outliers are so rare. If great talent and great drive are both rare, then people with both are rare squared. Most people you meet who have a lot of one will have less of the other. But you'll need both if you want to be an outlier yourself. And since you can't really change how much natural talent you have, in practice doing great work, insofar as you can, reduces to working very hard.

It's straightforward to work hard if you have clearly defined, externally imposed goals, as you do in school. There is some technique to it: you have to learn not to lie to yourself, not to procrastinate (which is a form of lying to yourself), not to get distracted, and not to give up when things go wrong. But this level of discipline seems to be within the reach of quite young children, if they want it.

What I've learned since I was a kid is how to work toward goals that are neither clearly defined nor externally imposed. You'll probably have to learn both if you want to do really great things.

The most basic level of which is simply to feel you should be working without anyone telling you to. Now, when I'm not working hard, alarm bells go off. I can't be sure I'm getting anywhere when I'm working hard, but I can be sure I'm getting nowhere when I'm not, and it feels awful. [2]

There wasn't a single point when I learned this. Like most little kids, I enjoyed the feeling of achievement when I learned or did something new. As I grew older, this morphed into a feeling of disgust when I wasn't achieving anything. The one precisely dateable landmark I have is when I stopped watching TV, at age 13.

Several people I've talked to remember getting serious about work around this age. When I asked Patrick Collison when he started to find idleness distasteful, he said

> I think around age 13 or 14. I have a clear memory from around then of >
> sitting in the sitting room, staring outside, and wondering why I was >
> wasting my summer holiday.

Perhaps something changes at adolescence. That would make sense.

Strangely enough, the biggest obstacle to getting serious about work was probably school, which made work (what they called work) seem boring and pointless. I had to learn what real work was before I could wholeheartedly desire to do it. That took a while, because even in college a lot of the work is pointless; there are entire departments that are pointless. But as I learned the shape of real work, I found that my desire to do it slotted into it as if they'd been made for each other.

I suspect most people have to learn what work is before they can love it. Hardy wrote eloquently about this in *A Mathematician's Apology* :

> I do not remember having felt, as a boy, any *passion* for mathematics,
> and > such notions as I may have had of the career of a mathematician were
> far > from noble. I thought of mathematics in terms of examinations and >
> scholarships: I wanted to beat other boys, and this seemed to be the way in >
> which I could do so most decisively.

He didn't learn what math was really about till part way through college, when he read Jordan's *Cours d'analyse*.

> I shall never forget the astonishment with which I read that remarkable >
> work, the first inspiration for so many mathematicians of my generation,
> and > learnt for the first time as I read it what mathematics really meant.

There are two separate kinds of fakeness you need to learn to discount in order to understand what real work is. One is the kind Hardy encountered in school. Subjects get distorted when they're adapted to be taught to kids -- often so distorted that they're nothing like the work done by actual practitioners. [3] The other kind of fakeness is intrinsic to certain types of work. Some types of work are inherently bogus, or at best mere busywork.

There's a kind of solidity to real work. It's not all writing the *Principia*
, but it all feels necessary. That's a vague criterion, but it's deliberately
vague, because it has to cover a lot of different types. [4]

Once you know the shape of real work, you have to learn how many hours a day
to spend on it. You can't solve this problem by simply working every waking
hour, because in many kinds of work there's a point beyond which the quality
of the result will start to decline.

That limit varies depending on the type of work and the person. I've done
several different kinds of work, and the limits were different for each. My
limit for the harder types of writing or programming is about five hours a
day. Whereas when I was running a startup, I could work all the time. At
least for the three years I did it; if I'd kept going much longer, I'd
probably have needed to take occasional vacations. [5]

The only way to find the limit is by crossing it. Cultivate a sensitivity to
the quality of the work you're doing, and then you'll notice if it decreases
because you're working too hard. Honesty is critical here, in both directions:
you have to notice when you're being lazy, but also when you're working too
hard. And if you think there's something admirable about working too hard,
get that idea out of your head. You're not merely getting worse results,
but getting them because you're showing off -- if not to other people,
then to yourself. [6]

Finding the limit of working hard is a constant, ongoing process, not something
you do just once. Both the difficulty of the work and your ability to do it
can vary hour to hour, so you need to be constantly judging both how hard
you're trying and how well you're doing.

Trying hard doesn't mean constantly pushing yourself to work, though. There
may be some people who do, but I think my experience is fairly typical, and
I only have to push myself occasionally when I'm starting a project or when
I encounter some sort of check. That's when I'm in danger of procrastinating.
But once I get rolling, I tend to keep going.

What keeps me going depends on the type of work. When I was working on
Viaweb, I was driven by fear of failure. I barely procrastinated at all then,
because there was always something that needed doing, and if I could put
more distance between me and the pursuing beast by doing it, why wait? [7]
Whereas what drives me now, writing essays, is the flaws in them. Between
essays I fuss for a few days, like a dog circling while it decides exactly
where to lie down. But once I get started on one, I don't have to push myself
to work, because there's always some error or omission already pushing me.

I do make some amount of effort to focus on important topics. Many problems
have a hard core at the center, surrounded by easier stuff at the edges.
Working hard means aiming toward the center to the extent you can. Some days
you may not be able to; some days you'll only be able to work on the easier,
peripheral stuff. But you should always be aiming as close to the center as
you can without stalling.

The bigger question of what to do with your life is one of these problems with a hard core. There are important problems at the center, which tend to be hard, and less important, easier ones at the edges. So as well as the small, daily adjustments involved in working on a specific problem, you'll occasionally have to make big, lifetime-scale adjustments about which type of work to do. And the rule is the same: working hard means aiming toward the center -- toward the most ambitious problems.

By center, though, I mean the actual center, not merely the current consensus about the center. The consensus about which problems are most important is often mistaken, both in general and within specific fields. If you disagree with it, and you're right, that could represent a valuable opportunity to do something new.

The more ambitious types of work will usually be harder, but although you should not be in denial about this, neither should you treat difficulty as an infallible guide in deciding what to do. If you discover some ambitious type of work that's a bargain in the sense of being easier for you than other people, either because of the abilities you happen to have, or because of some new way you've found to approach it, or simply because you're more excited about it, by all means work on that. Some of the best work is done by people who find an easy way to do something hard.

As well as learning the shape of real work, you need to figure out which kind you're suited for. And that doesn't just mean figuring out which kind your natural abilities match the best; it doesn't mean that if you're 7 feet tall, you have to play basketball. What you're suited for depends not just on your talents but perhaps even more on your interests. A *deep interest* in a topic makes people work harder than any amount of discipline can.

It can be harder to discover your interests than your talents. There are fewer types of talent than interest, and they start to be judged early in childhood, whereas interest in a topic is a subtle thing that may not mature till your twenties, or even later. The topic may not even exist earlier. Plus there are some powerful sources of error you need to learn to discount. Are you really interested in x, or do you want to work on it because you'll make a lot of money, or because other people will be impressed with you, or because your parents want you to? [8]

The difficulty of figuring out what to work on varies enormously from one person to another. That's one of the most important things I've learned about work since I was a kid. As a kid, you get the impression that everyone has a calling, and all they have to do is figure out what it is. That's how it works in movies, and in the streamlined biographies fed to kids. Sometimes it works that way in real life. Some people figure out what to do as children and just do it, like Mozart. But others, like Newton, turn restlessly from one kind of work to another. Maybe in retrospect we can identify one as their calling -- we can wish Newton spent more time on math and physics and less on alchemy and theology -- but this is an *illusion* induced by hindsight bias. There was no voice calling to him that he could have heard.

So while some people's lives converge fast, there will be others whose lives never converge. And for these people, figuring out what to work on is not so much a prelude to working hard as an ongoing part of it, like one of a set of simultaneous equations. For these people, the process I described earlier has a third component: along with measuring both how hard you're working and how well you're doing, you have to think about whether you should keep working in this field or switch to another. If you're working hard but not getting good enough results, you should switch. It sounds simple expressed that way, but in practice it's very difficult. You shouldn't give up on the first day just because you work hard and don't get anywhere. You need to give yourself time to get going. But how much time? And what should you do if work that was going well stops going well? How much time do you give yourself then? [9]

What even counts as good results? That can be really hard to decide. If you're exploring an area few others have worked in, you may not even know what good results look like. History is full of examples of people who misjudged the importance of what they were working on.

The best test of whether it's worthwhile to work on something is whether you find it interesting. That may sound like a dangerously subjective measure, but it's probably the most accurate one you're going to get. You're the one working on the stuff. Who's in a better position than you to judge whether it's important, and what's a better predictor of its importance than whether it's interesting?

For this test to work, though, you have to be honest with yourself. Indeed, that's the most striking thing about the whole question of working hard: how at each point it depends on being honest with yourself.

Working hard is not just a dial you turn up to 11. It's a complicated, dynamic system that has to be tuned just right at each point. You have to understand the shape of real work, see clearly what kind you're best suited for, aim as close to the true core of it as you can, accurately judge at each moment both what you're capable of and how you're doing, and put in as many hours each day as you can without harming the quality of the result. This network is too complicated to trick. But if you're consistently honest and clear-sighted, it will automatically assume an optimal shape, and you'll be productive in a way few people are.

Notes

[1] In "The Bus Ticket Theory of Genius" I said the three ingredients in great work were natural ability, determination, and interest. That's the formula in the preceding stage; determination and interest yield practice and effort.

[2] I mean this at a resolution of days, not hours. You'll often get somewhere while not working in the sense that the solution to a problem comes to you while taking a *shower*, or even in your sleep, but only because you were working hard on it the day before.

It's good to go on vacation occasionally, but when I go on vacation, I like to learn new things. I wouldn't like just sitting on a beach.

[3] The thing kids do in school that's most like the real version is sports. Admittedly because many sports originated as games played in schools. But in this one area, at least, kids are doing exactly what adults do.

In the average American high school, you have a choice of pretending to do something serious, or seriously doing something pretend. Arguably the latter is no worse.

[4] Knowing what you want to work on doesn't mean you'll be able to. Most people have to spend a lot of their time working on things they don't want to, especially early on. But if you know what you want to do, you at least know what direction to nudge your life in.

[5] The lower time limits for intense work suggest a solution to the problem of having less time to work after you have kids: switch to harder problems. In effect I did that, though not deliberately.

[6] Some cultures have a tradition of performative hard work. I don't love this idea, because (a) it makes a parody of something important and (b) it causes people to wear themselves out doing things that don't matter. I don't know enough to say for sure whether it's net good or bad, but my guess is bad.

[7] One of the reasons people work so hard on startups is that startups can fail, and when they do, that failure tends to be both decisive and conspicuous.

[8] It's ok to work on something to make a lot of money. You need to solve the money problem somehow, and there's nothing wrong with doing that efficiently by trying to make a lot at once. I suppose it would even be ok to be interested in money for its own sake; whatever floats your boat. Just so long as you're conscious of your motivations. The thing to avoid is *unconsciously* letting the need for money warp your ideas about what kind of work you find most interesting.

[9] Many people face this question on a smaller scale with individual projects. But it's easier both to recognize and to accept a dead end in a single project than to abandon some type of work entirely. The more determined you are, the harder it gets. Like a Spanish Flu victim, you're fighting your own immune system: Instead of giving up, you tell yourself, I should just try harder. And who can say you're not right?

| A Project of One's Own

June 2021

A few days ago, on the way home from school, my nine year old son told me he couldn't wait to get home to write more of the story he was working on. This made me as happy as anything I've heard him say -- not just because he was excited about his story, but because he'd discovered this way of working. Working on a project of your own is as different from ordinary work as skating is from walking. It's more fun, but also much more productive.

What proportion of great work has been done by people who were skating in this sense? If not all of it, certainly a lot.

There is something special about working on a project of your own. I wouldn't say exactly that you're happier. A better word would be excited, or engaged. You're happy when things are going well, but often they aren't. When I'm writing an essay, most of the time I'm worried and puzzled: worried that the essay will turn out badly, and puzzled because I'm groping for some idea that I can't see clearly enough. Will I be able to pin it down with words? In the end I usually can, if I take long enough, but I'm never sure; the first few attempts often fail.

You have moments of happiness when things work out, but they don't last long, because then you're on to the next problem. So why do it at all? Because to the kind of people who like working this way, nothing else feels as right. You feel as if you're an animal in its natural habitat, doing what you were meant to do -- not always happy, maybe, but awake and alive.

Many kids experience the excitement of working on projects of their own. The hard part is making this converge with the work you do as an adult. And our customs make it harder. We treat "playing" and "hobbies" as qualitatively different from "work". It's not clear to a kid building a treehouse that there's a direct (though long) route from that to architecture or engineering. And instead of pointing out the route, we conceal it, by implicitly treating the stuff kids do as different from real work. [1]

Instead of telling kids that their treehouses could be on the path to the work they do as adults, we tell them the path goes through school. And unfortunately schoolwork tends to be very different from working on projects of one's own. It's usually neither a project, nor one's own. So as school gets more serious, working on projects of one's own is something that survives, if at all, as a thin thread off to the side.

It's a bit sad to think of all the high school kids turning their backs on building treehouses and sitting in class dutifully learning about Darwin or Newton to pass some exam, when the work that made Darwin and Newton famous was actually closer in spirit to building treehouses than studying for exams.

If I had to choose between my kids getting good grades and working on ambitious projects of their own, I'd pick the projects. And not because I'm an indulgent parent, but because I've been on the other end and I know which has more predictive value. When I was picking startups for Y Combinator, I didn't care about applicants' grades. But if they'd worked on projects of their own, I wanted to hear all about those. [2]

It may be inevitable that school is the way it is. I'm not saying we have to redesign it (though I'm not saying we don't), just that we should understand what it does to our attitudes to work -- that it steers us toward the dutiful plodding kind of work, often using competition as bait, and away from skating.

There are occasionally times when schoolwork becomes a project of one's own. Whenever I had to write a paper, that would become a project of my own -- except in English classes, ironically, because the things one has to write in English classes are so *bogus*. And when I got to college and started taking CS classes, the programs I had to write became projects of my own. Whenever I was writing or programming, I was usually skating, and that has been true ever since.

So where exactly is the edge of projects of one's own? That's an interesting question, partly because the answer is so complicated, and partly because there's so much at stake. There turn out to be two senses in which work can be one's own: 1) that you're doing it voluntarily, rather than merely because someone told you to, and 2) that you're doing it by yourself.

The edge of the former is quite sharp. People who care a lot about their work are usually very sensitive to the difference between pulling, and being pushed, and work tends to fall into one category or the other. But the test isn't simply whether you're told to do something. You can choose to do something you're told to do. Indeed, you can own it far more thoroughly than the person who told you to do it.

For example, math homework is for most people something they're told to do. But for my father, who was a mathematician, it wasn't. Most of us think of the problems in a math book as a way to test or develop our knowledge of the material explained in each section. But to my father the problems were the part that mattered, and the text was merely a sort of annotation. Whenever he got a new math book it was to him like being given a puzzle: here was a new set of problems to solve, and he'd immediately set about solving all of them.

The other sense of a project being one's own -- working on it by oneself -- has a much softer edge. It shades gradually into collaboration. And interestingly, it shades into collaboration in two different ways. One way to collaborate is to share a single project. For example, when two mathematicians collaborate on a proof that takes shape in the course of a conversation between them. The other way is when multiple people work on separate projects of their own

that fit together like a jigsaw puzzle. For example, when one person writes the text of a book and another does the graphic design. [3]

These two paths into collaboration can of course be combined. But under the right conditions, the excitement of working on a project of one's own can be preserved for quite a while before disintegrating into the turbulent flow of work in a large organization. Indeed, the history of successful organizations is partly the history of techniques for preserving that excitement. [4]

The team that made the original Macintosh were a great example of this phenomenon. People like Burrell Smith and Andy Hertzfeld and Bill Atkinson and Susan Kare were not just following orders. They were not tennis balls hit by Steve Jobs, but rockets let loose by Steve Jobs. There was a lot of collaboration between them, but they all seem to have individually felt the excitement of working on a project of one's own.

In Andy Hertzfeld's book on the Macintosh, he describes how they'd come back into the office after dinner and work late into the night. People who've never experienced the thrill of working on a project they're excited about can't distinguish this kind of working long hours from the kind that happens in sweatshops and boiler rooms, but they're at opposite ends of the spectrum. That's why it's a mistake to insist dogmatically on "work/life balance." Indeed, the mere expression "work/life" embodies a mistake: it assumes work and life are distinct. For those to whom the word "work" automatically implies the dutiful plodding kind, they are. But for the skaters, the relationship between work and life would be better represented by a dash than a slash. I wouldn't want to work on anything that I didn't want to take over my life.

Of course, it's easier to achieve this level of motivation when you're making something like the Macintosh. It's easy for something new to feel like a project of your own. That's one of the reasons for the tendency programmers have to rewrite things that don't need rewriting, and to write their own versions of things that already exist. This sometimes alarms managers, and measured by total number of characters typed, it's rarely the optimal solution. But it's not always driven simply by arrogance or cluelessness. Writing code from scratch is also much more rewarding -- so much more rewarding that a good programmer can end up net ahead, despite the shocking waste of characters. Indeed, it may be one of the advantages of capitalism that it encourages such rewriting. A company that needs software to do something can't use the software already written to do it at another company, and thus has to write their own, which often turns out better. [5]

The natural alignment between skating and solving new problems is one of the reasons the payoffs from startups are so high. Not only is the market price of unsolved problems higher, you also get a discount on productivity when you work on them. In fact, you get a double increase in productivity: when you're doing a clean-sheet design, it's easier to recruit skaters, and they get to spend all their time skating.

Steve Jobs knew a thing or two about skaters from having watched Steve Wozniak. If you can find the right people, you only have to tell them what to do at the highest level. They'll handle the details. Indeed, they insist on it. For a project to feel like your own, you must have sufficient autonomy.

You can't be working to order, or *slowed down* by bureaucracy.

One way to ensure autonomy is not to have a boss at all. There are two ways to do that: to be the boss yourself, and to work on projects outside of work. Though they're at opposite ends of the scale financially, startups and open source projects have a lot in common, including the fact that they're often run by skaters. And indeed, there's a wormhole from one end of the scale to the other: one of the best ways to discover *startup ideas* is to work on a project just for fun.

If your projects are the kind that make money, it's easy to work on them. It's harder when they're not. And the hardest part, usually, is morale. That's where adults have it harder than kids. Kids just plunge in and build their treehouse without worrying about whether they're wasting their time, or how it compares to other treehouses. And frankly we could learn a lot from kids here. The high standards most grownups have for "real" work do not always serve us well.

The most important phase in a project of one's own is at the beginning: when you go from thinking it might be cool to do x to actually doing x. And at that point high standards are not merely useless but positively harmful. There are a few people who start too many new projects, but far more, I suspect, who are deterred by fear of failure from starting projects that would have succeeded if they had.

But if we couldn't benefit as kids from the knowledge that our treehouses were on the path to grownup projects, we can at least benefit as grownups from knowing that our projects are on a path that stretches back to treehouses. Remember that careless confidence you had as a kid when starting something new? That would be a powerful thing to recapture.

If it's harder as adults to retain that kind of confidence, we at least tend to be more aware of what we're doing. Kids bounce, or are herded, from one kind of work to the next, barely realizing what's happening to them. Whereas we know more about different types of work and have more control over which we do. Ideally we can have the best of both worlds: to be deliberate in choosing to work on projects of our own, and carelessly confident in starting new ones.

Notes

[1] "Hobby" is a curious word. Now it means work that isn't *real* work -- work that one is not to be judged by -- but originally it just meant an obsession in a fairly general sense (even a political opinion, for example) that one metaphorically rode as a child rides a hobby-horse. It's hard to say if its recent, narrower meaning is a change for the better or the worse. For sure there are lots of false positives -- lots of projects that

end up being important but are dismissed initially as mere hobbies. But on the other hand, the concept provides valuable cover for projects in the early, ugly duckling phase.

[2] Tiger parents, as parents so often do, are fighting the last war. Grades mattered more in the old days when the route to success was to acquire _credentials_ while ascending some predefined ladder. But it's just as well that their tactics are focused on grades. How awful it would be if they invaded the territory of projects, and thereby gave their kids a distaste for this kind of work by forcing them to do it. Grades are already a grim, fake world, and aren't harmed much by parental interference, but working on one's own projects is a more delicate, private thing that could be damaged very easily.

[3] The complicated, gradual edge between working on one's own projects and collaborating with others is one reason there is so much disagreement about the idea of the "lone genius." In practice people collaborate (or not) in all kinds of different ways, but the idea of the lone genius is definitely not a myth. There's a core of truth to it that goes with a certain way of working.

[4] Collaboration is powerful too. The optimal organization would combine collaboration and ownership in such a way as to do the least damage to each. Interestingly, companies and university departments approach this ideal from opposite directions: companies insist on collaboration, and occasionally also manage both to recruit skaters and allow them to skate, and university departments insist on the ability to do independent research (which is by custom treated as skating, whether it is or not), and the people they hire collaborate as much as they choose.

[5] If a company could design its software in such a way that the best newly arrived programmers always got a clean sheet, it could have a kind of eternal youth. That might not be impossible. If you had a software backbone defining a game with sufficiently clear rules, individual programmers could write their own players.

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| FIERCE NERDS

May 2021

Most people think of nerds as quiet, diffident people. In ordinary social situations they are -- as quiet and diffident as the star quarterback would be if he found himself in the middle of a physics symposium. And for the same reason: they are fish out of water. But the apparent diffidence of nerds is

an illusion due to the fact that when non-nerds observe them, it's usually in ordinary social situations. In fact some nerds are quite fierce.

The fierce nerds are a small but interesting group. They are as a rule extremely competitive -- more competitive, I'd say, than highly competitive non-nerds. Competition is more personal for them. Partly perhaps because they're not emotionally mature enough to distance themselves from it, but also because there's less randomness in the kinds of competition they engage in, and they are thus more justified in taking the results personally.

Fierce nerds also tend to be somewhat overconfident, especially when young. It might seem like it would be a disadvantage to be mistaken about one's abilities, but empirically it isn't. Up to a point, confidence is a self- fullfilling prophecy.

Another quality you find in most fierce nerds is intelligence. Not all nerds are smart, but the fierce ones are always at least moderately so. If they weren't, they wouldn't have the confidence to be fierce. [1]

There's also a natural connection between nerdiness and *independent-mindedness*. It's hard to be independent-minded without being somewhat socially awkward, because conventional beliefs are so often mistaken, or at least arbitrary. No one who was both independent-minded and ambitious would want to waste the effort it takes to fit in. And the independent- mindedness of the fierce nerds will obviously be of the *aggressive* rather than the passive type: they'll be annoyed by rules, rather than dreamily unaware of them.

I'm less sure why fierce nerds are impatient, but most seem to be. You notice it first in conversation, where they tend to interrupt you. This is merely annoying, but in the more promising fierce nerds it's connected to a deeper impatience about solving problems. Perhaps the competitiveness and impatience of fierce nerds are not separate qualities, but two manifestations of a single underlying drivenness.

When you combine all these qualities in sufficient quantities, the result is quite formidable. The most vivid example of fierce nerds in action may be James Watson's *The Double Helix*. The first sentence of the book is "I have never seen Francis Crick in a modest mood," and the portrait he goes on to paint of Crick is the quintessential fierce nerd: brilliant, socially awkward, competitive, independent-minded, overconfident. But so is the implicit portrait he paints of himself. Indeed, his lack of social awareness makes both portraits that much more realistic, because he baldly states all sorts of opinions and motivations that a smoother person would conceal. And moreover it's clear from the story that Crick and Watson's fierce nerdiness was integral to their success. Their independent-mindedness caused them to consider approaches that most others ignored, their overconfidence allowed them to work on problems they only half understood (they were literally described as "clowns" by one eminent insider), and their impatience and competitiveness got them to the answer ahead of two other groups that would otherwise have found it within the next year, if not the next several months. [2]

The idea that there could be fierce nerds is an unfamiliar one not just to many normal people but even to some young nerds. Especially early on, nerds spend so much of their time in ordinary social situations and so little doing real work that they get a lot more evidence of their awkwardness than their power. So there will be some who read this description of the fierce nerd and realize "Hmm, that's me." And it is to you, young fierce nerd, that I now turn.

I have some good news, and some bad news. The good news is that your fierceness will be a great help in solving difficult problems. And not just the kind of scientific and technical problems that nerds have traditionally solved. As the world progresses, the number of things you can win at by getting the right answer increases. Recently *getting rich* became one of them: 7 of the 8 richest people in America are now fierce nerds.

Indeed, being a fierce nerd is probably even more helpful in business than in nerds' original territory of scholarship. Fierceness seems optional there. Darwin for example doesn't seem to have been especially fierce. Whereas it's impossible to be the CEO of a company over a certain size without being fierce, so now that nerds can win at business, fierce nerds will increasingly monopolize the really big successes.

The bad news is that if it's not exercised, your fierceness will turn to bitterness, and you will become an intellectual playground bully: the grumpy sysadmin, the forum troll, the *hater*, the shooter down of *new ideas*.

How do you avoid this fate? Work on ambitious projects. If you succeed, it will bring you a kind of satisfaction that neutralizes bitterness. But you don't need to have succeeded to feel this; merely working on hard projects gives most fierce nerds some feeling of satisfaction. And those it doesn't, it at least keeps busy. [3]

Another solution may be to somehow turn off your fierceness, by devoting yourself to meditation or psychotherapy or something like that. Maybe that's the right answer for some people. I have no idea. But it doesn't seem the optimal solution to me. If you're given a sharp knife, it seems to me better to use it than to blunt its edge to avoid cutting yourself.

If you do choose the ambitious route, you'll have a tailwind behind you. There has never been a better time to be a nerd. In the past century we've seen a continuous transfer of power from dealmakers to technicians -- from the charismatic to the competent -- and I don't see anything on the horizon that will end it. At least not till the nerds end it themselves by bringing about the singularity.

Notes

[1] To be a nerd is to be socially awkward, and there are two distinct ways to do that: to be playing the same game as everyone else, but badly, and to be playing a different game. The smart nerds are the latter type.

[2] The same qualities that make fierce nerds so effective can also make them very annoying. Fierce nerds would do well to remember this, and (a) try to keep a lid on it, and (b) seek out organizations and types of work where getting the right answer matters more than preserving social harmony. In practice that means small groups working on hard problems. Which fortunately is the most fun kind of environment anyway.

[3] If success neutralizes bitterness, why are there some people who are at least moderately successful and yet still quite bitter? Because people's potential bitterness varies depending on how naturally bitter their personality is, and how ambitious they are: someone who's naturally very bitter will still have a lot left after success neutralizes some of it, and someone who's very ambitious will need proportionally more success to satisfy that ambition.

So the worst-case scenario is someone who's both naturally bitter and extremely ambitious, and yet only moderately successful.

[Chinese Translation](#)

| [Crazy New Ideas](#)

May 2021

There's one kind of opinion I'd be very afraid to express publicly. If someone I knew to be both a domain expert and a reasonable person proposed an idea that sounded preposterous, I'd be very reluctant to say "That will never work."

Anyone who has studied the history of ideas, and especially the history of science, knows that's how big things start. Someone proposes an idea that sounds crazy, most people dismiss it, then it gradually takes over the world.

Most implausible-sounding ideas are in fact bad and could be safely dismissed. But not when they're proposed by reasonable domain experts. If the person proposing the idea is reasonable, then they know how implausible it sounds. And yet they're proposing it anyway. That suggests they know something you don't. And if they have deep domain expertise, that's probably the source of it. [1]

Such ideas are not merely unsafe to dismiss, but disproportionately likely to be interesting. When the average person proposes an implausible-sounding idea, its implausibility is evidence of their incompetence. But when a reasonable domain expert does it, the situation is reversed. There's something like an efficient market here: on average the ideas that seem craziest will, if correct, have the biggest effect. So if you can eliminate the theory that the person proposing an implausible-sounding idea is incompetent, its implausibility switches from evidence that it's boring to evidence that it's exciting. [2]

Such ideas are not guaranteed to work. But they don't have to be. They just have to be sufficiently good bets -- to have sufficiently high expected value. And I think on average they do. I think if you bet on the entire set of implausible-sounding ideas proposed by reasonable domain experts, you'd end up net ahead.

The reason is that everyone is too conservative. The word "paradigm" is overused, but this is a case where it's warranted. Everyone is too much in the grip of the current paradigm. Even the people who have the new ideas undervalue them initially. Which means that before they reach the stage of proposing them publicly, they've already subjected them to an excessively strict filter. [3]

The wise response to such an idea is not to make statements, but to ask questions, because there's a real mystery here. Why has this smart and reasonable person proposed an idea that seems so wrong? Are they mistaken, or are you? One of you has to be. If you're the one who's mistaken, that would be good to know, because it means there's a hole in your model of the world. But even if they're mistaken, it should be interesting to learn why. A trap that an expert falls into is one you have to worry about too.

This all seems pretty obvious. And yet there are clearly a lot of people who don't share my fear of dismissing new ideas. Why do they do it? Why risk looking like a jerk now and a fool later, instead of just reserving judgement?

One reason they do it is envy. If you propose a radical new idea and it succeeds, your reputation (and perhaps also your wealth) will increase proportionally. Some people would be envious if that happened, and this potential envy propagates back into a conviction that you must be wrong.

Another reason people dismiss new ideas is that it's an easy way to seem sophisticated. When a new idea first emerges, it usually seems pretty feeble. It's a mere hatchling. Received wisdom is a full-grown eagle by comparison. So it's easy to launch a devastating attack on a new idea, and anyone who does will seem clever to those who don't understand this asymmetry.

This phenomenon is exacerbated by the difference between how those working on new ideas and those attacking them are rewarded. The rewards for working on new ideas are weighted by the value of the outcome. So it's worth working on something that only has a 10% chance of succeeding if it would make things more than 10x better. Whereas the rewards for attacking new ideas are roughly constant; such attacks seem roughly equally clever regardless of the target.

People will also attack new ideas when they have a vested interest in the old ones. It's not surprising, for example, that some of Darwin's harshest critics were churchmen. People build whole careers on some ideas. When someone claims they're false or obsolete, they feel threatened.

The lowest form of dismissal is mere factionalism: to automatically dismiss any idea associated with the opposing faction. The lowest form of all is to dismiss an idea because of who proposed it.

But the main thing that leads reasonable people to dismiss new ideas is the same thing that holds people back from proposing them: the sheer pervasiveness of the current paradigm. It doesn't just affect the way we think; it is the Lego blocks we build thoughts out of. Popping out of the current paradigm is something only a few people can do. And even they usually have to suppress their intuitions at first, like a pilot flying through cloud who has to trust his instruments over his sense of balance. [4]

Paradigms don't just define our present thinking. They also vacuum up the trail of crumbs that led to them, making our standards for new ideas impossibly high. The current paradigm seems so perfect to us, its offspring, that we imagine it must have been accepted completely as soon as it was discovered -- that whatever the church thought of the heliocentric model, astronomers must have been convinced as soon as Copernicus proposed it. Far, in fact, from it. Copernicus published the heliocentric model in 1532, but it wasn't till the mid seventeenth century that the balance of scientific opinion shifted in its favor. [5]

Few understand how feeble new ideas look when they first appear. So if you want to have new ideas yourself, one of the most valuable things you can do is to learn what they look like when they're born. Read about how new ideas happened, and try to get yourself into the heads of people at the time. How did things look to them, when the new idea was only half-finished, and even the person who had it was only half-convinced it was right?

But you don't have to stop at history. You can observe big new ideas being born all around you right now. Just look for a reasonable domain expert proposing something that sounds wrong.

If you're nice, as well as wise, you won't merely resist attacking such people, but encourage them. Having new ideas is a lonely business. Only those who've tried it know how lonely. These people need your help. And if you help them, you'll probably learn something in the process.

Notes

[1] This domain expertise could be in another field. Indeed, such crossovers tend to be particularly promising.

[2] I'm not claiming this principle extends much beyond math, engineering, and the hard sciences. In politics, for example, crazy-sounding ideas generally are as bad as they sound. Though arguably this is not an exception, because the people who propose them are not in fact domain experts; politicians are domain experts in political tactics, like how to get elected and how to get legislation passed, but not in the world that policy acts upon. Perhaps no one could be.

[3] This sense of "paradigm" was defined by Thomas Kuhn in his *Structure of Scientific Revolutions* , but I also recommend his *Copernican Revolution* , where you can see him at work developing the idea.

[4] This is one reason people with a touch of Asperger's may have an advantage in discovering new ideas. They're always flying on instruments.

[5] Hall, Rupert. *From Galileo to Newton.* Collins, 1963. This book is particularly good at getting into contemporaries' heads.

| The Real Reason to End the Death Penalty

April 2021

When intellectuals talk about the death penalty, they talk about things like whether it's permissible for the state to take someone's life, whether the death penalty acts as a deterrent, and whether more death sentences are given to some groups than others. But in practice the debate about the death penalty is not about whether it's ok to kill murderers. It's about whether it's ok to kill innocent people, because at least 4% of people on death row are *innocent*.

When I was a kid I imagined that it was unusual for people to be convicted of crimes they hadn't committed, and that in murder cases especially this must be very rare. Far from it. Now, thanks to organizations like the *Innocence Project*, we see a constant stream of stories about murder convictions being overturned after new evidence emerges. Sometimes the police and prosecutors were just very sloppy. Sometimes they were crooked, and knew full well they were convicting an innocent person.

Kenneth Adams and three other men spent 18 years in prison on a murder conviction. They were exonerated after DNA testing implicated three different men, two of whom later confessed. The police had been told about the other men early in the investigation, but never followed up the lead.

Keith Harward spent 33 years in prison on a murder conviction. He was convicted because "experts" said his teeth matched photos of bite marks on one victim. He was exonerated after DNA testing showed the murder had been committed by another man, Jerry Crotty.

Ricky Jackson and two other men spent 39 years in prison after being convicted of murder on the testimony of a 12 year old boy, who later recanted and said he'd been coerced by police. Multiple people have confirmed the boy was elsewhere at the time. The three men were exonerated after the county prosecutor dropped the charges, saying "The state is conceding the obvious."

Alfred Brown spent 12 years in prison on a murder conviction, including 10 years on death row. He was exonerated after it was discovered that the assistant district attorney had concealed phone records proving he could not have committed the crimes.

Glenn Ford spent 29 years on death row after having been convicted of murder. He was exonerated after new evidence proved he was not even at the scene when the murder occurred. The attorneys assigned to represent him had never tried a jury case before.

Cameron Willingham was actually executed in 2004 by lethal injection. The "expert" who testified that he deliberately set fire to his house has since been discredited. A re-examination of the case ordered by the state of Texas in 2009 concluded that "a finding of arson could not be sustained."

[Rich Glossip](#) has spent 20 years on death row after being convicted of murder on the testimony of the actual killer, who escaped with a life sentence in return for implicating him. In 2015 he came within minutes of execution before it emerged that Oklahoma had been planning to kill him with an illegal combination of drugs. They still plan to go ahead with the execution, perhaps as soon as this summer, despite [new evidence](#) exonerating him.

I could go on. There are hundreds of similar cases. In Florida alone, 29 death row prisoners have been exonerated so far.

Far from being rare, wrongful murder convictions are [very common](#). Police are under pressure to solve a crime that has gotten a lot of attention. When they find a suspect, they want to believe he's guilty, and ignore or even destroy evidence suggesting otherwise. District attorneys want to be seen as effective and tough on crime, and in order to win convictions are willing to manipulate witnesses and withhold evidence. Court-appointed defense attorneys are overworked and often incompetent. There's a ready supply of criminals willing to give false testimony in return for a lighter sentence, suggestible witnesses who can be made to say whatever police want, and bogus "experts" eager to claim that science proves the defendant is guilty. And juries want to believe them, since otherwise some terrible crime remains unsolved.

This circus of incompetence and dishonesty is the real issue with the death penalty. We don't even reach the point where theoretical questions about the moral justification or effectiveness of capital punishment start to matter, because so many of the people sentenced to death are actually innocent.

Whatever it means in theory, in practice capital punishment means killing innocent people.

Related:

- [Will Florida Kill an Innocent Man?](#)

- [Was Kevin Cooper Framed for Murder?](#)

- [Did Texas execute an innocent man?](#)

| How People Get Rich Now

April 2021

Every year since 1982, *Forbes* magazine has published a list of the richest Americans. If we compare the 100 richest people in 1982 to the 100 richest in 2020, we notice some big differences.

In 1982 the most common source of wealth was inheritance. Of the 100 richest people, 60 inherited from an ancestor. There were 10 du Pont heirs alone. By 2020 the number of heirs had been cut in half, accounting for only 27 of the biggest 100 fortunes.

Why would the percentage of heirs decrease? Not because inheritance taxes increased. In fact, they decreased significantly during this period. The reason the percentage of heirs has decreased is not that fewer people are inheriting great fortunes, but that more people are making them.

How are people making these new fortunes? Roughly 3/4 by starting companies and 1/4 by investing. Of the 73 new fortunes in 2020, 56 derive from founders' or early employees' equity (52 founders, 2 early employees, and 2 wives of founders), and 17 from managing investment funds.

There were no fund managers among the 100 richest Americans in 1982. Hedge funds and private equity firms existed in 1982, but none of their founders were rich enough yet to make it into the top 100. Two things changed: fund managers discovered new ways to generate high returns, and more investors were willing to trust them with their money. [1]

But the main source of new fortunes now is starting companies, and when you look at the data, you see big changes there too. People get richer from starting companies now than they did in 1982, because the companies do different things.

In 1982, there were two dominant sources of new wealth: oil and real estate. Of the 40 new fortunes in 1982, at least 24 were due primarily to oil or real estate. Now only a small number are: of the 73 new fortunes in 2020, 4 were due to real estate and only 2 to oil.

By 2020 the biggest source of new wealth was what are sometimes called "tech" companies. Of the 73 new fortunes, about 30 derive from such companies. These are particularly common among the richest of the rich: 8 of the top 10 fortunes in 2020 were new fortunes of this type.

Arguably it's slightly misleading to treat tech as a category. Isn't Amazon really a retailer, and Tesla a car maker? Yes and no. Maybe in 50 years, when what we call tech is taken for granted, it won't seem right to put these two businesses in the same category. But at the moment at least, there is definitely something they share in common that distinguishes them. What retailer starts AWS? What car maker is run by someone who also has a rocket company?

The tech companies behind the top 100 fortunes also form a well-differentiated group in the sense that they're all companies that venture capitalists would readily invest in, and the others mostly not. And there's a reason why: these are mostly companies that win by having better technology, rather than just a CEO who's really driven and good at making deals.

To that extent, the rise of the tech companies represents a qualitative change. The oil and real estate magnates of the 1982 Forbes 400 didn't win by making better technology. They won by being really driven and good at making deals. [2] And indeed, that way of getting rich is so old that it predates the Industrial Revolution. The courtiers who got rich in the (nominal) service of European royal houses in the 16th and 17th centuries were also, as a rule, really driven and good at making deals.

People who don't look any deeper than the Gini coefficient look back on the world of 1982 as the good old days, because those who got rich then didn't get as rich. But if you dig into *how* they got rich, the old days don't look so good. In 1982, 84% of the richest 100 people got rich by inheritance, extracting natural resources, or doing real estate deals. Is that really better than a world in which the richest people get rich by starting tech companies?

Why are people starting so many more new companies than they used to, and why are they getting so rich from it? The answer to the first question, curiously enough, is that it's misphrased. We shouldn't be asking why people are starting companies, but why they're starting companies *again*. [3]

In 1892, the *New York Herald Tribune* compiled a list of all the millionaires in America. They found 4047 of them. How many had inherited their wealth then? Only about 20%, which is less than the proportion of heirs today. And when you investigate the sources of the new fortunes, 1892 looks even more like today. Hugh Rockoff found that "many of the richest ... gained their initial edge from the new technology of mass production." [4]

So it's not 2020 that's the anomaly here, but 1982. The real question is why so few people had gotten rich from starting companies in 1982. And the answer is that even as the *Herald Tribune* 's list was being compiled, a wave of [consolidation](#) was sweeping through the American economy. In the late 19th and early 20th centuries, financiers like J. P. Morgan combined thousands of smaller companies into a few hundred giant ones with commanding economies of scale. By the end of World War II, as Michael Lind writes, "the major sectors of the economy were either organized as government-backed cartels or dominated by a few oligopolistic corporations." [5]

In 1960, most of the people who start startups today would have gone to work for one of them. You could get rich from starting your own company in 1890 and in 2020, but in 1960 it was not really a viable option. You couldn't break through the oligopolies to get at the markets. So the prestigious route in 1960 was not to start your own company, but to work your way up the corporate ladder at an existing one. [6]

Making everyone a corporate employee decreased economic inequality (and every other kind of variation), but if your model of normal is the mid 20th century, you have a very misleading model in that respect. J. P. Morgan's economy turned out to be just a phase, and starting in the 1970s, it began to break up.

Why did it break up? Partly senescence. The big companies that seemed models of scale and efficiency in 1930 had by 1970 become slack and bloated. By 1970 the rigid structure of the economy was full of cosy nests that various groups had built to insulate themselves from market forces. During the Carter administration the federal government realized something was amiss and began, in a process they called "deregulation," to roll back the policies that propped up the oligopolies.

But it wasn't just decay from within that broke up J. P. Morgan's economy. There was also pressure from without, in the form of new technology, and particularly microelectronics. The best way to envision what happened is to imagine a pond with a crust of ice on top. Initially the only way from the bottom to the surface is around the edges. But as the ice crust weakens, you start to be able to punch right through the middle.

The edges of the pond were pure tech: companies that actually described themselves as being in the electronics or software business. When you used the word "startup" in 1990, that was what you meant. But now startups are punching right through the middle of the ice crust and displacing incumbents

like retailers and TV networks and car companies. [7]

But though the breakup of J. P. Morgan's economy created a new world in the technological sense, it was a reversion to the norm in the social sense. If you only look back as far as the mid 20th century, it seems like people getting rich by starting their own companies is a recent phenomenon. But if you look back further, you realize it's actually the default. So what we should expect in the future is more of the same. Indeed, we should expect both the number and wealth of founders to grow, because every decade it gets easier to start a startup.

Remember the key words (part 2): "Elephant Symphony Bubble Horizon Whistle Radiant Harmony Lighthouse".

Part of the reason it's getting easier to start a startup is social. Society is (re)assimilating the concept. If you start one now, your parents won't freak out the way they would have a generation ago, and knowledge about how to do it is much more widespread. But the main reason it's easier to start a startup now is that it's cheaper. Technology has driven down the cost of both building products and acquiring customers.

The decreasing cost of starting a startup has in turn changed the balance of power between founders and investors. Back when starting a startup meant building a factory, you needed investors' permission to do it at all. But now investors need founders more than founders need investors, and that, combined with the increasing amount of venture capital available, has driven up valuations. [8]

So the decreasing cost of starting a startup increases the number of rich people in two ways: it means that more people start them, and that those who do can raise money on better terms.

But there's also a third factor at work: the companies themselves are more valuable, because newly founded companies grow faster than they used to. Technology hasn't just made it cheaper to build and distribute things, but faster too.

This trend has been running for a long time. IBM, founded in 1896, took 45 years to reach a billion 2020 dollars in revenue. Hewlett-Packard, founded in 1939, took 25 years. Microsoft, founded in 1975, took 13 years. Now the norm for fast-growing companies is 7 or 8 years. [9]

Fast growth has a double effect on the value of founders' stock. The value of a company is a function of its revenue and its growth rate. So if a company grows faster, you not only get to a billion dollars in revenue sooner, but the company is more valuable when it reaches that point than it would be if it were growing slower.

That's why founders sometimes get so rich so young now. The low initial cost of starting a startup means founders can start young, and the fast growth of companies today means that if they succeed they could be surprisingly rich just a few years later.

It's easier now to start and grow a company than it has ever been. That means more people start them, that those who do get better terms from investors, and that the resulting companies become more valuable. Once you understand how these mechanisms work, and that startups were suppressed for most of the 20th century, you don't have to resort to some vague right turn the country took under Reagan to explain why America's Gini coefficient is increasing. Of course the Gini coefficient is increasing. With more people starting more valuable companies, how could it not be?

Notes

[1] Investment firms grew rapidly after a regulatory change by the Labor Department in 1978 allowed pension funds to invest in them, but the effects of this growth were not yet visible in the top 100 fortunes in 1982.

[2] George Mitchell deserves mention as an exception. Though really driven and good at making deals, he was also the first to figure out how to use fracking to get natural gas out of shale.

[3] When I say people are starting more companies, I mean the type of company meant to *grow* very big. There has actually been a decrease in the last couple decades in the overall number of new companies. But the vast majority of companies are small retail and service businesses. So what the statistics about the decreasing number of new businesses mean is that people are starting fewer shoe stores and barber shops.

People sometimes get *confused* when they see a graph labelled "startups" that's going down, because there are two senses of the word "startup": (1) the founding of a company, and (2) a particular type of company designed to grow big fast. The statistics mean startup in sense (1), not sense (2).

[4] Rockoff, Hugh. "Great Fortunes of the Gilded Age." NBER Working Paper 14555, 2008.

[5] Lind, Michael. *Land of Promise.* HarperCollins, 2012.

It's also likely that the high tax rates in the mid 20th century deterred people from starting their own companies. Starting one's own company is risky, and when risk isn't rewarded, people opt for *safety* instead.

But it wasn't simply cause and effect. The oligopolies and high tax rates of the mid 20th century were all of a piece. Lower taxes are not just a cause of entrepreneurship, but an effect as well: the people getting rich in the mid 20th century from real estate and oil exploration lobbied for and got huge tax loopholes that made their effective tax rate much lower, and presumably

if it had been more common to grow big companies by building new technology, the people doing that would have lobbied for their own loopholes as well.

[6] That's why the people who did get rich in the mid 20th century so often got rich from oil exploration or real estate. Those were the two big areas of the economy that weren't susceptible to consolidation.

[7] The pure tech companies used to be called "high technology" startups. But now that startups can punch through the middle of the ice crust, we don't need a separate name for the edges, and the term "high-tech" has a decidedly *retro* sound.

[8] Higher valuations mean you either sell less stock to get a given amount of money, or get more money for a given amount of stock. The typical startup does some of each. Obviously you end up richer if you keep more stock, but you should also end up richer if you raise more money, because (a) it should make the company more successful, and (b) you should be able to last longer before the next round, or not even need one. Notice all those shoulds though. In practice a lot of money slips through them.

It might seem that the huge rounds raised by startups nowadays contradict the claim that it has become cheaper to start one. But there's no contradiction here; the startups that raise the most are the ones doing it by choice, in order to grow faster, not the ones doing it because they need the money to survive. There's nothing like not needing money to make people offer it to you.

You would think, after having been on the side of labor in its fight with capital for almost two centuries, that the far left would be happy that labor has finally prevailed. But none of them seem to be. You can almost hear them saying "No, no, not *that* way."

[9] IBM was created in 1911 by merging three companies, the most important of which was Herman Hollerith's Tabulating Machine Company, founded in 1896. In 1941 its revenues were $60 million.

Hewlett-Packard's revenues in 1964 were $125 million.

Microsoft's revenues in 1988 were $590 million.

| WRITE SIMPLY

March 2021

I try to write using ordinary words and simple sentences.

That kind of writing is easier to read, and the easier something is to read, the more deeply readers will engage with it. The less energy they expend on your prose, the more they'll have left for your ideas.

And the further they'll read. Most readers' energy tends to flag part way through an article or essay. If the friction of reading is low enough, more keep going till the end.

There's an Italian dish called *saltimbocca* , which means "leap into the mouth." My goal when writing might be called *saltintesta* : the ideas leap into your head and you barely notice the words that got them there.

It's too much to hope that writing could ever be pure ideas. You might not even want it to be. But for most writers, most of the time, that's the goal to aim for. The gap between most writing and pure ideas is not filled with poetry.

Plus it's more considerate to write simply. When you write in a fancy way to impress people, you're making them do extra work just so you can seem cool. It's like trailing a long train behind you that readers have to carry.

And remember, if you're writing in English, that a lot of your readers won't be native English speakers. Their understanding of ideas may be way ahead of their understanding of English. So you can't assume that writing about a difficult topic means you can use difficult words.

Of course, fancy writing doesn't just conceal ideas. It can also conceal the lack of them. That's why some people write that way, to conceal the fact that they have __nothing to say. Whereas writing simply keeps you honest. If you say nothing simply, it will be obvious to everyone, including you.

Simple writing also lasts better. People reading your stuff in the future will be in much the same position as people from other countries reading it today. The culture and the language will have changed. It's not vain to care about that, any more than it's vain for a woodworker to build a chair to last.

Indeed, lasting is not merely an accidental quality of chairs, or writing. It's a sign you did a good job.

But although these are all real advantages of writing simply, none of them are why I do it. The main reason I write simply is that it offends me not to. When I write a sentence that seems too complicated, or that uses unnecessarily intellectual words, it doesn't seem fancy to me. It seems clumsy.

There are of course times when you want to use a complicated sentence or fancy word for effect. But you should never do it by accident.

The other reason my writing ends up being simple is the way I do it. I write the first draft fast, then spend days editing it, trying to get everything just right. Much of this editing is cutting, and that makes simple writing even simpler.