Beware What Sounds Insightful

John D. Rockefeller Sr. was the richest tycoon of the Gilded Age, at one point valued at 2% of the United States's GDP. His rise coincided with the creation of the 'trust' structure of business – a structure that enabled business practices so unfair and monopolistic the US Government had to create the Sherman antitrust act of 1890 to reign it in.

But the rise of Rockefeller and his ilk also prompted the rise of the muckrakers – reform-minded journalists that exposed business leaders and politicians as corrupt. In fact, the muckrakers were influential in turning the tide of public opinion against the trusts, which led, indirectly, to the creation of the Sherman antitrust act. This was the dawn of the age of the newspaper, and the rise of journalism as a discipline alongside it.

But like most young mediums, journalism, too, had its excesses.

[Ida Tarbell](#) was Rockefeller's muckraker nemesis, and a leading activist of the Progressive Era. She is often credited with pioneering the practice of investigative journalism. But Tarbell stretched the truth about Rockefeller numerous times over the course of her reporting. As Ron Chernow writes in *Titan*, his history of the Rockefeller family and their business dealings:

> *Although Tarbell pretended to apply her scalpel to Standard Oil with surgical objectivity, she was never neutral and not only because of her father. Her brother, William Walter Tarbell, had been a leading figure in forming the Pure Oil Company, the most serious domestic challenger to Standard Oil,*

and his letters to her were laced with anti-Standard venom. Complaining of the trust's price manipulations in one letter, Will warned her, "Some of those fellows will get killed one of those days." As Pure Oil's treasurer in 1902, Will steered legions of Rockefeller enemies to his sister and even vetted her manuscripts. Far from cherishing her neutrality, Tarbell in the end adhered to the advice she had once received from Henry James: "Cherish your contempts." Amazingly enough, nobody made an issue of Tarbell's veritable partnership with her brother in exposing his chief competitor.

And later in the book:

However pathbreaking in its time and richly deserving of its accolades, the Tarbell series does not, finally, stand up as an enduring piece of history. The more closely one examines it, the more it seems a superior screed masquerading as sober history. In the end, Tarbell could not conquer her nostalgia for the Titusville of her girlhood, that lost paradise of heroic friends and neighbors who went forth doughtily to do battle with the all-devouring Standard Oil dragon.

I've left a lot out of the excerpts above, but anyone reading the history of Ida Tarbell and Standard Oil would be struck by how many of the tricks Tarbell used to stoke anger against Rockefeller still work today.

More importantly, those tricks aren't surprising, especially when read against the backdrop of today, more than 100 years after the rise of journalism. Even a casual reader of the news knows to be sceptical when reading a story in the papers, or watching a reported segment on cable news. But at the time of Tarbell's campaign, public relations hadn't yet been invented. Society had not developed antibodies against manipulative journalism.

Being Critical in the Age of the Internet

If you read something on the internet today, the odds are good that the writing was *produced to capture your attention*. This isn't some profound realisation, of course: many of us are familiar with the idea of an 'attention economy'. We're at the end of the second decade of the social Internet; the returns to attention are notoriously lucrative and the competition is steep.

I think the arms race for our attention has led to an arms race in writing. The best online writers are able to make something *sound* insightful – regardless of whether it's true, or whether it's useful. On one hand, this is the writer's job. [Jason Zweig](#) of the Wall Street Journal likes saying that his goal as a finance writer is to present a small handful of universal investing truths in increasingly novel ways, so readers never know that he's repeating himself. But at another level, this shift means that online readers have to be increasingly critical when reading essays and blog posts for instrumental reasons.

This isn't some evil conspiracy. 'Writers optimising to produce insight porn to grab attention' sounds nefarious, but it's really more like 'writers responding to the incentives of the social internet' – a simple side effect of the attention economy. And we shouldn't be surprised: if Instagram has changed the language of photography, then we should also expect the emergence of Twitter, Medium and Google to change the tenor of writing.

Tricks

It's interesting to note that I've chosen 'insight' as a key metric to focus on. I'm ignoring tribalism and novelty here – though both are equally powerful ways to cap-

ture attention on the social internet. 'Hot takes' that appeal to a group identity perform better than opinion pieces that do not; news that is extreme, or new, or novel does better than news about what is natural and commonplace.

When you're reading for career reasons, however, you're seeking insights that can help you perform better or navigate the world better or see things differently or frame things more effectively. Naturally, writing in the self-help or career-advice genres will have to simulate insightfulness in order to keep your attention.

Some ideas sound insightful because they are true. But not all true ideas sound insightful. A true idea that is commonly accepted can sound trite and obvious: we call those clichés. The job of a good writer, then, is to present some truth in a way that doesn't trip our cliché triggers.

This means that attention seeking in writing usually devolves to the same handful of tricks. I've used many of those tricks myself, here at Commonplace. This is a non-exhaustive list.

Use a story. I started this piece with a story. **Preferably from a historical period that the reader isn't familiar with.** This establishes that the writer knows something that the reader doesn't, and is communicating some argument or idea with a basis in history.

I'm not being completely facetious here. Arguments from historical example are often doubly effective: first because they are rooted in reality, and second because they allow the writer to tell a story. Readers love stories – they're [far easier to read than argumentative prose](#).

Sometimes, writers present obvious truths by way of a story. Morgan Housel is a master of this: in *[Fat, Happy, And In Over Your Head](#)* he tells two stories: the first of the co-working company WeWork and the second of the greatest ski racer in history, Marcel Hirscher. The point of the piece? Housel asserts that you can avoid the pain of loss aversion if you can somehow stop yourself from being greedy. The

idea is simple, but Housel's presentation is novel. Whether his assertion is practicable is another thing entirely.

Repackage obvious truths and sprinkle them over the course of an essay. If a cliché is a truth that is commonly accepted, then an insight is a truth that isn't widespread. Clichés can thus be repackaged to sound insightful. This is a useful trick because a) clichés are often truths the reader already agrees with, and b) whatever sounds insightful will keep the reader going.

At the top of this essay, I wrote *"if Instagram has changed the language of photography, then we should also expect the emergence of Twitter, Medium and Google to change the tenor of writing."*

This is a fairly obvious truth that's been rewritten to sound more insightful. If you stop to think a bit, you'd quickly realise that writers have *always* optimised for attention. And if you read a lot online, you've probably already noticed this effect in action. It's just the reference to Instagram that threw you. (Also: look at how I use the phrase 'optimised for attention' here, when I could have just as simply said 'liked to be read'. Certain phrases sound more sophisticated than others – this is yet another technique. More on this in a bit.)

The odds are good, however, that when you read this sentence, you became curious. You kept scrolling because you wanted to see where I would go. This is the trick in action.

David Perell has a famous essay titled [Peter Thiel's Religion](). The essay is the length of a small booklet, and is divided into tiny sections, each ending with a packaged insight. A reader going through the piece will keep scrolling, because their brain will go "Oh, that's interesting! And that's interesting! And that's also interesting! Ooh, what's next?"

As a writer, I admire what he's done. But as a business person, nearly everything that Perell says in the piece about business is subtly wrong – enough to make me treat his essay as entertainment, not education.

Use more sophisticated language than necessary. Sometimes simple ideas can be dressed up to seem more legitimate than they really are. The problem is that there are times where this is a reasonable strategy. Economist Russ Roberts [once said](), of Nassim Nicholas Taleb:

> *Now, let me take a different book: Fooled by Randomness, by Nassim Taleb. There were two views of that book, which I've mentioned here before. One view was: 'There is nothing original in this book. This guy is a fraud. He pretends he has figured out all this stuff.' And I said, 'You know, I agree with that.' I didn't learn anything that I didn't know in this book before. I knew that probability is difficult. I knew that risk is a hard thing to wrap your mind around. I understand something about most of the ideas he talked about in the book. So, in some sense, I learned nothing. On the other hand, I learned something incredibly deep. That book really grabbed me by the guts and jerked me around. It forced me to confront some things that I "knew" but didn't really internalise. And I would put that as another category of learning.*

Taleb's strategy of using stories and sophisticated language (along with some made-up names) are designed to hammer home a simple set of ideas. Those ideas are difficult to grasp at the explicit level. They have to be made tacit to be useful. So Taleb's approach is a legitimate use of this trick.

Other times, however, if you strip away the complicated words, the underlying argument appears ridiculous.

Venkatesh Rao is one of the best Internet writers I know. But his pieces are often optimised to sound insightful, as opposed to capturing some operationally useful truth about the world.

In [CEOs don't steer](), Rao writes paragraphs like:

The primary CEO function, and the trait the good ones are selected for, is to provide the gyroscopic stability required to keep a company vectored in the chosen direction. They end up in the jobs they do because they counterbalance an organization's natural tendency towards distraction, ADD and momentum dissipation. A typical company is a wandering, wobbling hive mind, liable to spend all its time chasing distractions if you let it, before dissolving into a bunch of clever tweets about crappy prototypes.

This is a sophisticated sentence. It uses words like 'gyroscopic stability' and 'vectored in the chosen direction' and 'clever tweets about crappy prototypes' – a juxtaposition I enjoy.

But if you strip the essay of its sophisticated language, the core argument Rao makes is ridiculous: most CEOs are unintellectual dummies whose sole purpose is to keep the company on track, rowing in a single direction. This property, Rao asserts, is why so many business books are reducible to single-sentence epithets. It is why consultants cannot often successfully give advice to CEOs . It asserts that CEOs are selected for their 'non-steering ability', instead of some other factor. It even explains the existence of the CEO 'reality distortion field' (because, Rao asserts, non-steering is more effective in the presence of a vision). It is why companies change CEOs in times of crisis – which Rao argues in such a delightful paragraph that I can't help but quote:

A "steering" event at the CEO level is a reorientation for the entire company, and for reasons I'll get to, the best way to accomplish it is to replace the CEO with a new one who is already locked on to a more desirable orientation vector. Like say, Dara Khosrowshahi, who, based on his history, was apparently already locked on to the "don't be a misogynist asshole" vector that Uber needed before he got the job.

Stripped of all sophisticated language, this argument is easily dismissed as face-tious. Each property that Rao pins on 'not steering' can be explained by a simpler underlying reason. (Which I leave as an exercise for the alert reader). Rao is simply taking one facet of the CEO experience, and then extrapolating from it into an entire theory of the firm.

Rao's piece is not ok if your goal is to read for career reasons. But it's ok if your goal is to read for entertainment. It's ok because Rao's goal is to attract eyeballs, not create better business leaders. And his writing is so good most people will forgive him for it.

Beware What Sounds Insightful

The takeaway from this piece is that what sounds insightful and what is useful or true are often not the same thing. This aspect of critical reading isn't emphasised as much as it should be – outside, perhaps, of the social sciences. I think we are all the poorer for it.

A year ago I wrote [Writing Doesn't Make You a Genius](). I noticed that people tend to assume good writers are smarter than they actually are. I argued that this was mistaken – that writers sound smarter on paper because the act of writing forces them to clarify their ideas.

But now I have another reason. Writers are often seen as smarter because good writers today are trained to optimise for sounding insightful. This bleeds over into reader perception.

But whether a writer sounds smart or a piece sounds sophisticated shouldn't affect you if your goal is to put things you read to practice. The questions remain the same: "Is this person [believable]()? How likely is this going to be useful? What's the cheapest way to find out?"

Optimise For Usefulness

I remember reading Paul Graham's essay *What You'll Wish You'd Known* as a teenager, back when I had first discovered Graham as an essayist. One section of that essay has stuck with me in the years since:

> *If (Shakespeare and Einstein) were just like us, then they had to work very hard to do what they did. And that's one reason we like to believe in genius. It gives us an excuse for being lazy. If these guys were able to do what they did only because of some magic Shakespeareness or Einsteinness, then it's not our fault if we can't do something as good.*
>
> ***I'm not saying there's no such thing as genius. But if you're trying to choose between two theories and one gives you an excuse for being lazy, the other one is probably right.*** *(emphasis mine)*

"Heh", I remember thinking, "That's a useful idea."

It's been a couple of months into this blog's life, and I'm starting to circle in on a core principle that's driving all of my writing here. Call it Commonplace's central thesis, if you will. I've written about it – badly – in *How to Optimise for Success, A Theory*, and I've also alluded to it when summarising *Ray Dalio's Hyperrealism*. But I want to spend some time today hashing it out, in order to work out all the implications.

The philosophy is something I call 'Optimise for Usefulness'.

The core concept is simple enough to state: if a belief is useful to you in achieving your goals, keep it. Otherwise, discard it. An extension of this is, per Graham, if you have to pick between two theories and one is less useful than the other, pick

the more useful one regardless of the truth. I believe effective, successful people do some variant of this. Unsuccessful people don't.

But this statement isn't, in itself, very useful (hah!). If we want to apply it to our lives, we should work out all the implications for ourselves, and explore all the forms this idea can take. So let's do that here.

Modifying Mindsets for Usefulness

I think it's pretty clear to most people that there exists a set of techniques that are a) cheap to implement at a personal level, b) create no immediate improvement in knowledge or technique, but c) result in higher effectiveness nevertheless. I'm going to call these class of techniques 'mindset tweaks'. To put it simply, a mindset tweak is a change to your worldview that allows you to better perform at something you're doing. A mindset that helps you do so is *useful*. A mindset that doesn't is not. It seems ridiculous that reframing an activity may change your effectiveness at said activity, but such techniques *do* exist, and they *do* work.

A concrete example will help illustrate this point: if you've done any long-distance running at all, you'll be familiar with the idea of managing your mind. A runner fights the desire to stop throughout her run. This is understandable: when your lungs are bursting and your muscles are spasming and the burn from pushing yourself for an hour or two start to become unbearable, your brain begins to form all sorts of clever excuses to stop, to just lift the pain and to seek blessed relief from the agony of continued motion. Good runners know how to manage this desire. They put their minds elsewhere, or they focus on their breathing, or they use self talk like "hills are my friend" to get past the bad bits.

A runner in Crete

What interests me is that this mindset tweak doesn't do anything in particular to the runner's *physical* ability. Her lung capacity, stamina, and technique remain the same. But a runner with the same amount of talent and training will do better if they have this mindset tweak, when compared to a runner without.

Sports psychology-driven mindset tweaks are obvious, however. It's not so obvious that mindset tweaks exist elsewhere. But similar tweaks exist for careers; Eugene Wallingford writes about one such technique in his blog post *Get Attached to Solving Problems for People*:

> In *Getting Critiqued*, Adam Morse reflects on his evolution from art student to web designer, and how that changed his relationship with users and critiques. Artists create things in which they are, at some level, invested. Their process matters. As a result, critiques, however well-intentioned, feel personal. The work isn't about a user; it's about you. But...

... design is different. As a designer, I don't matter. My work doesn't matter. Nothing I make matters in the context of my process. It's all about the people you are building for. You're just trying to solve problems for people. Once you realize this, it's the most liberating thing.

Why is this mindset tweak useful? Well, if you're able to see the goal of your work as 'solving problems', then criticism of your work becomes less grating, because it's no longer about you. It's about solving the problem. This allows you to learn more effectively from criticism, and it prevents you from quitting your field prematurely as the feedback – erroneously seen as personal attacks – wear you down.

I recently heard a journalist complain "if writers are artists, then what I deal with weekly is someone ripping apart the art that I've taken great pains to craft." Optimising for usefulness suggests that this mindset isn't useful at all, and should be discarded as quickly as possible. The journalist would do better instead if they thought of their work as 'communicating information to an audience that needs such information.'

That isn't to say that art doesn't exist in journalism, just that the way to get to art is to adopt a mindset that *gets out of the way in the first place*. The optimal approach is to find a more *useful* mindset, something to help you stick around long enough to build mastery.

Discarding Bad Mindsets

Our example with the journalist suggests a different application of 'optimise for usefulness'. It suggests that you can use this philosophy by inverting it: that is, if you find that you possess a mindset that *actively hinders* you in achieving your

goals, then you should immediately begin a search for a more useful mindset to replace it with.

This idea has been surprisingly powerful for me. My first experience with it was at my previous company, during a period where I found it difficult to continue. When we first made the shift to selling products instead of consulting services, things got very difficult very quickly. I was pulling all-nighters, people were leaving due to the uncertainty, and customers were yelling at us every other day, as we had no idea what we were doing. I got quite discouraged, and spent a few months thinking about quitting.

The turning point came when I met with Dinesh Raju of [ReferralCandy](). I told him about my company's problems, and he told me two things. First, he gave me some generic management advice, and then he told me to read *High Output Management* by Andy Grove. I remember thinking: what if I treated this experience as an opportunity to get *really good* at management? What would that be like?

Everything, externally, was still the same, but I remember coming in to work the next week motivated to fix the terrible situation I was in.

It was like a light switch turning on. Everything, externally, was still the same, but I remember coming in to work the next week motivated to fix the terrible situation I was in. I treated the company as a way to gain mastery in management. And I did.

I think what was important about that experience was that I should have realised I needed a mindset shift earlier. It was important for me to stay at the company in order to achieve my goals. My mindset *before* meeting Dinesh was hindering me from achieving those goals; my mindset *after* meeting him was way more useful. I should have optimised for usefulness earlier.

Years later, I read Cal Newport's book *[So Good They Can't Ignore You]()*, which explained what had happened to me. In it, Newport argues that picking a profession by following your passion is a terrible idea, because most people don't know

what they're passionate about. Instead, Newport recommends seeing your chosen vocation as a craft that you can get better at. This 'craftsman mindset' – as he put it – is more *useful*, because it allows you to build mastery. And the literature suggests that mastery leads to passion more often than the reverse.

I think many of the techniques in *The Principles Sequence* are of the 'mindset tweaks to help you achieve your goals' category. Of particular relevance to our current discussion is Dalio's mindset tweak to 'see pain as reality telling you that you need to update your understanding of the world'. Adopting such a mindset helps in dealing with setbacks. It also implies changing mindsets if they actively hinder you.

Optimising Heuristics

One way 'optimising for usefulness' is clearly helpful is when you're looking for good heuristics in life.

A heuristic is a big word that basically means 'rule of thumb'. Cooking is filled with useful heuristics: for instance, once you know that bread dough is five parts flour to three parts liquid, you never have to look at a cookbook for bread again, and you can scale a recipe from four people to 400.

Loaves of bread in a shop

I think effectiveness in life is a search for similarly useful heuristics. For instance, my previous post on dismissive stubbornness is useful because it identifies a trait that I normally have trouble pinpointing. In the past, I've had bad experiences with dismissively stubborn people, though I couldn't adequately describe *why* they were so bad. So identifying this trait is good because it's useful.

Identifying a trait is good, but developing an efficient test for that trait is even better.

But it's *not useful enough*. To optimise for usefulness, I need to find an effective test for detecting dismissive stubbornness. I wrote a hypothesis in the closing paragraphs of my piece – that polite debate might be one way to test for this trait – and I intend to test that through trial and error.

The truth is that polite debate may or may not work. I might have to try other approaches. But the point is that once I've found a good enough test, my goal is to incorporate it into my protocols for 'do I want to work with this person or not'? Identifying a trait is good, but developing an efficient test for that trait is even better. Adopting such a test is optimising for usefulness; it should – I hope! – make me more effective in the future.

Another useful heuristic that I've learnt is the idea that *debugging activity is indicative of broader programming ability*. Or, to put this more accurately: a programmer with good debugging skills might not be a great programmer, but a programmer with bad debugging skills can never be a good one. It took awhile to come to this realisation, but then all sorts of useful implications presented themselves. One useful implication is that in order to become a better programmer, it pays to work on your debugging skills. But a more useful implication would be that we could use this insight for hiring.

As a result of this idea, we reconfigured our screening tests at my previous company and gave all candidates a buggy program to debug as a first test. If the candidate had real trouble debugging, we cut the interview short. It made our hiring a lot more efficient overall.

In *[Principles](#)*, Ray Dalio suggests that you write down your principles for dealing with different situations, because similar situations tend to appear repeatedly over the course of your life. Formalising your principles simplifies decision making. It also allows you to consider each principle explicitly, and to change them if they no longer appear as useful to achieving your goals as they used to.

Distilling Lessons from Experience

I was having lunch with a friend recently, when she complained that she was disappointed with a colleague of hers, and explained why. It was a pretty good story, and I could understand her reasons for being frustrated by the end of it.

"But don't be disappointed," I said, "I think that being disappointed isn't useful."

"So what's the right approach?"

"I don't know," I said, "But what other things could you try?"

I think, looking back, that I would *still* have said complaining wasn't good because it doesn't optimise for usefulness. But instead of walking through the whole space of alternative reactions, I would have suggested sorting it in the order of potential usefulness.

It's clear that disappointment isn't a useful response. It doesn't help my friend achieve her work goals. But what other reactions do we have available to us, and how may we order them?

A *slightly* more useful approach would be to see this as a challenge to *figure out what works:* that is, to figure out what motivates this particular colleague. Better still is using that experience as a way to build a library of personality archetypes, the same way an athlete builds a playbook of techniques.

This approach requires you to ask yourself: "what personality markers may I use to identify people who are like my colleague? Is my colleague lazy? How may I invalidate that hypothesis? Contra my current guess, are there areas in which she is motivated? If so, what are the common elements? How may I use that to motivate her? And how may I test these guesses?"

Each question above scales the ladder of usefulness. Finding the answer to "how do I motivate people with my colleague's personality" is more useful than "how do I tell quickly and accurately if someone is lazy?", which in turn is more use-

ful than "how do I know *this particular* colleague is lazy?" or "how do I motivate *this particular* colleague?", which is, finally, more useful than "gosh, this colleague is so lazy, I'm so disappointed in her."

A far better outcome would be to walk away with a generalisable rule for identifying people of this personality type, and a generalisable rule for dealing with them, which you may use for the rest of your career.

Optimising for usefulness means climbing the ladder of usefulness and *not settling too early*. I don't think we do this nearly enough. In this particular example, it's clear that you shouldn't settle for mere disappointment. But it also means not settling *even after* you find a method for dealing with the problematic colleague! A far better outcome would be to walk away with a generalisable rule for identifying people of this personality type, and a generalisable rule for dealing with them, which you may use for the rest of your career.

A common objection at this point is "how do you know your test works in the general case?" This is a legitimate concern. You might think that your conclusion is the right one ("my colleague is lazy and I should work around her!") and then maybe it turns out that your test for laziness is wrong or your solution to work around her just happened to work this one time. But implicit in optimising for usefulness is the idea that you should *continually reevaluate* your heuristics. Discard your conclusions if you find invalidating evidence. Refine your approaches with continued experimentation.

Distilling the Wrong Lessons

There's another aspect to this idea: that is to *not* jump to conclusions that are of limited usefulness. This is a subtly different point from our previous example.

In our previous example, you faced a problem and found a solution that worked, but failed to consider if the solution was generalisable. Here, you draw the 'wrong' lessons from your experiences. These lessons are 'wrong' in the sense that they aren't as useful as alternative lessons you could have learnt. But that, too, is nowhere near the worst outcome; the worse outcome is that you learn lessons that hinder you in the future – which are too easy to do if you continue to run your life on the basis of these unexamined conclusions.

I once had a fellow manager who concluded a reasonable but ultimately limiting set of lessons from a shared experience. We had been hiring a number of candidates from Taiwan. Our first candidate was a fantastic software engineer who came from a large, multinational hardware company; he joined us because he was looking to get into a 'proper' software company, and there weren't a lot of those in Taiwan.

We had been assigning him to work on some frankly lousy projects, such as setting up a new internal CMS, as well as taking on low-value, dead-end enterprise deployments.

He left us after seven months of such low-value work.

Aerial shot of Taipei at dusk

My fellow manager and I concluded wildly different lessons from this experience. I concluded that we shouldn't waste good software engineers from Taiwan on sub-par tasks. My colleague concluded that the Singaporean software market was too competitive, and we should only hire *lousy* software engineering candidates from Taiwan, ones who couldn't get jobs at competing software firms. He based his argument on the fact that the salary offered was higher than what we offered, and that we could not possibly compete when dealt with such offers.

I disagreed heatedly. My mental models for retention were dramatically different from his; I had good experiences increasing employee retention in Vietnam despite the fact that we didn't offer the highest salaries in town. I suspected we could do the same with engineers we hired from Taiwan, which would give our company a huge competitive advantage, what with the limited supply of computer science graduates in Singapore.

The point here is not that I was right and he was wrong. The point here was that my colleague *jumped to the least useful interpretation possible*. His 'lesson' ham-

pered our ability to hire from Taiwan – which remained an organisational hangup well after I left the company.

It could well be true that it is impossible to hold on to good Taiwanese software engineers in Singapore, and that the *only* solution is to recruit bad ones. But I believe it wouldn't cost much to test an alternative interpretation, one that *didn't* lock us out of hiring good Taiwanese software engineers. To this day, that company has not been able to recruit from Taiwan, despite my continued observation that retention strategies usually take a year or so to work out.

The Central Thesis

So let's sum up.

Optimising for usefulness consists of four aspects: first, that mindset hacks exist, and that the good ones help you become more effective at life. Second, that we may invert this idea: if you possess a mindset that hinders you from achieving your goals, aggressively look for a better one.

Third, 'optimising for usefulness' also works when it comes to learning from experience: when something happens to you, prioritise learning the lessons with higher usefulness first. Verify that they work through trial and error. And fourth, prevent yourself from leaping to obvious (or convenient!) but ultimately less useful conclusions.

So what does this have to do with this blog? Why write about 'optimising for usefulness?'

The answer: Commonplace's central thesis is to write about ideas in a way that is optimised for usefulness.

It's too easy to write about topics that are merely intellectually interesting, or to describe mental models in an abstract way, with no clear actionables in sight. But

optimising for usefulness in the context of this blog means picking topics that are useful for the work of building career moats.

It also means that you should apply the test of usefulness whenever you read something here. Construct tests for everything I write about; take nothing at face value. And if an idea passes your tests – that is, if it *demonstrably* helps you on your journey to achieve your goals, then it's useful and you should keep it. But if it doesn't, then it doesn't matter if it's true. It's not useful for you, and you should pay it no mind.

How First Principles Thinking Fails

One of the threads I've been pulling on in this blog is the question "Where should the line lie between first principles thinking and pattern matching? When should you use one or the other?"

The question is interesting because many career decisions depend on good thinking, and thinking is roughly split into pattern matching against our experiences, or reasoning from first principles. My belief is that it isn't enough to have one or the other; you really need to have both.

The question I've chosen – 'where should the line lie …?' – isn't a particularly good one, because the obvious answer is obvious: "it depends!" And of course it does: whether you do first principles thinking or perform some form of pattern matching really depends on the problem you're trying to solve, the domain you're working in, and all sorts of context-dependent things that you can't generalise away.

As a direction for inquiry, however, the question has been fairly useful. You can sort of squint at the following posts and see the shadow of that question lurking in the background:

1. In Much Ado About The OODA Loop I wrote about John Boyd's ideas, and in particular Boyd's belief that good strategic thinking depends on accurate sensemaking, which in turn depends on repeatedly creating and then destroying mental models of the world. I followed that up with Good Synthesis is the Start of Good Sensemaking, which was an at-

tempt to demonstrate the difficulty of good sensemaking under conditions of partial information, by putting you in the shoes of a battered CEO.

2. In [Reality Without Frameworks](#) I talked about the danger of using frameworks excessively. I argued that frameworks were compelling because they help give order to your world, but that they also colour and shape (and sometimes blind you to) what you see around you.

3. Then, in [In Defence of Reading Goals](#), I went the other way and argued that for certain careers, it was a competitive advantage to build up a large set of patterns in one's head. The easiest way to do that is to read a large number of books; I argued that when seen in this light, it wasn't a terrible idea to set reading goals for yourself.

In fact, if you skim through the posts I've written over the past six months, you would find arguments for first principles thinking in some essays, and arguments for better pattern matching in others; I was essentially vacillating between the two positions. I suppose this is a way of arguing that you need both forms of thinking to do well – or that I thought the art of good thinking lies in finding a balance between the two modes.

One useful way to look at this problem is to ask: *how can you fail?* That is: how can pattern matching fail you? And how can first principles thinking fail you?

In my experience, the second question is more interesting than the first.

First Principles Thinking, Evaluated

If I were to ask you how pattern matching might fail, I'm willing to bet that you can give me a dozen good answers in about as many seconds. We all know stories of

those who pattern matched against the past, only to discover that their matches were wrong when applied to the future. There's a wonderful [collection of bad predictions](#) over at the Foresight Institute, though my favourite is perhaps the [example of Airbnb](#) (I remember hearing about the idea and going "What?! That would never work!" – but of course it did; the story has entered the realm of startup canon).

It's easy to throw shade at pattern matching – nevermind that [expertise is essentially pattern matching](#), or that Charlie Munger [appears to be very good at it](#). But I've always considered the second question a bit of a mystery. How can first principles thinking fail? It all seems so logical. How indeed?

One lazy answer is that pattern matching is often fast, whereas analysis isn't, and so pattern matching is better suited for situations where you have to make a decision quickly. But this is an edge case more than it is an interesting answer – we know that pattern matching may also be employed in slower, more considered forms of thinking: psychologists call this analogical reasoning.

(I'm mixing terminology a little; but bear with me – this is a blog post, not an academic paper).

Another possible answer is that you make a mistake when executing your reasoning:

- One or more of your 'principles' or 'axioms' turns out to be mistaken.
- You make a mistake in one of your inference steps.

In practice, mistakes like this aren't as huge a problem as you might think – if you run your reasoning past a sufficiently diverse group of intelligent, analytical people, it's likely that they'd be able to spot your error.

What is interesting to me are the instances when you've built logically coherent propositions from true and right axioms, *and you still get things wrong anyway*.

Back when I was still running an engineering office in Vietnam, my boss and I would meet up every couple of months to take stock of the company, and to articu-

late what we thought was going on in our business. Our arguments were reasonably water-tight. Sometimes we would start from observations; other times, we would argue from first principles. My boss was rigorous and analytical. I respected his thinking. Between the two of us, I was pretty sure we'd be able to detect flawed assumptions, or unreasonable leaps of logic. We thought our conclusions were pretty good. And yet reality would punch us, repeatedly, in the face.

The *way* we got things wrong was always strikingly similar. When we first started selling Point of Sales Systems, most external observers told us that there was no money in it: that there were too many competitors, that the margins would be too low, that we wouldn't be able to bootstrap the business. *We* thought the same things, but my boss decided to give it a go anyway. I remember spending months afterwards, talking with him, trying to answer the question "why are we making so much money?" We obsessed over this for *months*. In retrospect, we were blessed to be able to ask this question. But the fact that we didn't understand why there wasn't more competition made us antsy as hell. We had any number of theories. They were all wrong.

We discovered the answer much later. It turned out that the market was distorted in a very particular way. The government wanted to encourage the adoption of self-service checkout machines, and Point of Sales systems were included under that definition. This meant that they were willing to give out thousands of dollars in grants in order to subsidise the cost of adoption. If you were a vendor during that period, and you got onto an approved vendor list, you had access to those grants; the vendor list served as a chokepoint to the rest of the market. This explained the margins we were seeing, along with a large number of market characteristics we had observed but not understood. (Note: I'm leaving a number of details out of this account, because my old company is still a player in the market. The shape of the market has changed, naturally, and the grant situation is different.)

In other words, there was a fact that we didn't adequately understand. Without knowledge of that fact, we couldn't bootstrap a good explanation.
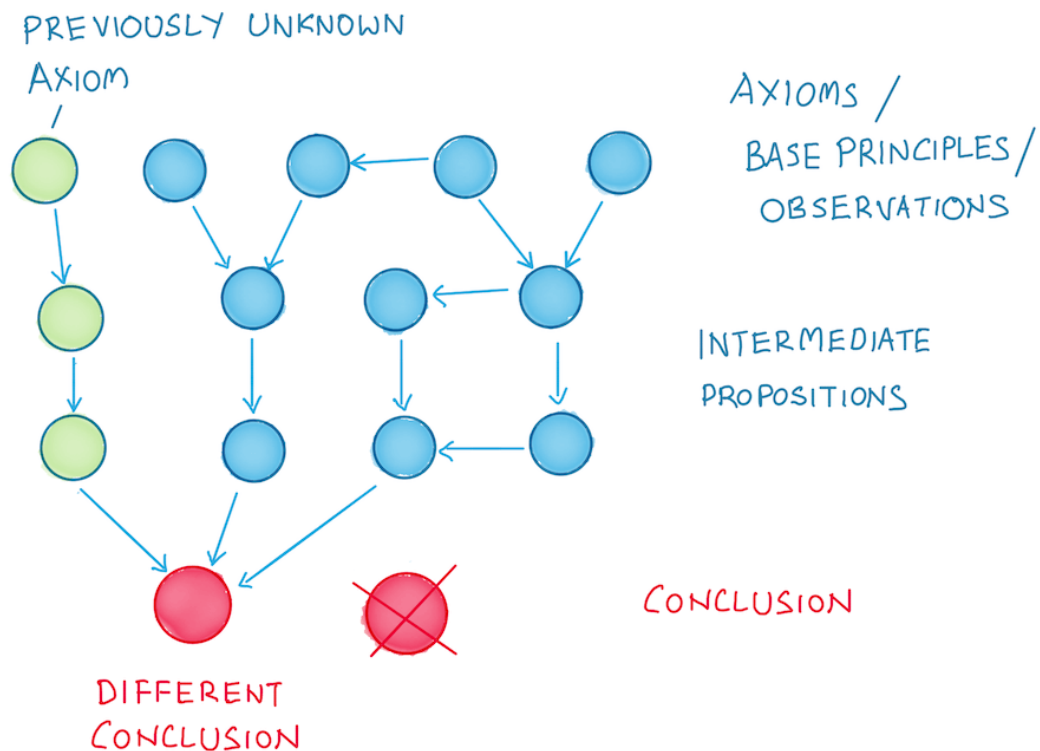
It eventually got to a point where we — or more accurately, *I* — no longer said things like "Ok, this argument makes sense. I think it's right." Reality had punched me in the face one too many times to be that confident. I began to say things like: "Ok, this analysis checks out. It seems plausible. *Let's wait and see.*"

Part of the reason I had to write [Seek Ideas at the Right Level of Abstraction](#) before writing this post was because I had to work out the general idea from my experiences. Today, I know that you can build analyses up from base principles … only to end up at the wrong level of abstraction. This is one way to fail at first principles thinking.

But I think there's a more pernicious form of failure: which occurs when you reason from the *wrong set of true principles*. It is pernicious because you can't easily detect the flaws in your reasoning. It is pernicious because all of your base axioms are true.

Axioms / Base Principles / Observations

Intermediate Propositions
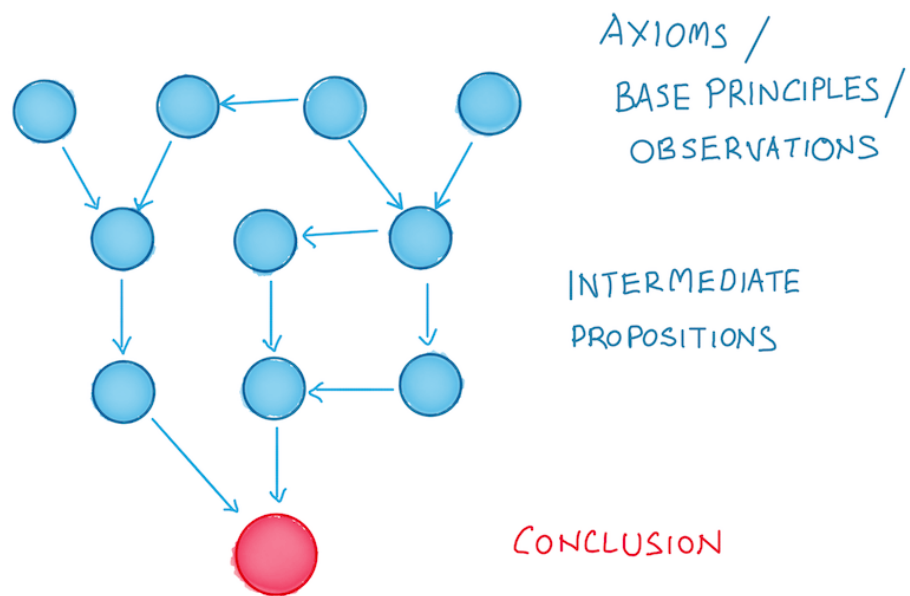
Conclusion

If you pick the wrong set of base principles – even if they're all true – you are likely to end up with the wrong conclusion at the end of your thinking. In other words, the only real test you have is against reality. Your conclusion should be useful. It should produce effective action.

PREVIOUSLY UNKNOWN
AXIOM

AXIOMS /
BASE PRINCIPLES /
OBSERVATIONS

INTERMEDIATE
PROPOSITIONS

CONCLUSION

DIFFERENT
CONCLUSION

Conversely, it's possible to 'figure everything out', only to learn a new piece of information that changes everything, that reconfigures all the reasoning chains in your argument.

As the old saying goes: in theory, theory and practice are the same. In practice, theory and practice are different.

To paraphrase that aphorism: in theory, first principles thinking always leads you to the right answer. In practice, it doesn't.

The Games People Play With Cash Flow

In my [last post](#) I examined how first principles thinking fails. This post is going to be about a single, concrete example – about an argument that started me down this path in the first place.

A couple of months ago, a friend sent me a blog post titled *Startups Shouldn't Raise Money*, over at a website called ensorial.com. I thought that the post was tightly argued and reasonably put together, with each proposition leading logically and coherently to the next. I also noticed that the author had taken the time to construct their argument from first principles … which meant it was difficult to refute any individual clause in their chain of reasoning.

But I also thought it was wrong. I told my friend as much.

"How is it wrong?" he immediately challenged.

"Well …" I began. And then I stopped. I realised I didn't have a good argument for *why* it was wrong. Every axiom and intermediate proposition were ideas that I agreed with. And it wasn't so simple as the conclusion being flat out mistaken – you *could* probably run a small, successful internet business using the ideas laid out in the posts's argument (internet-based businesses tend to be simpler to manage, and there are many niches you can occupy).

But I felt uneasy because I thought the framing wasn't as *useful*. This was a more complex thing to debunk.

The Setup

It's easy to think that arguments have just three terminal truth values: right, maybe, and wrong. In practice, arguments (and in particular, the sort of argument that we use to justify actions) have many possible truth values. These include things like 'got the details wrong, but is by-and-large correct', or 'is correct but for a [different level of abstraction](); doesn't apply here', or 'is partially correct, but isn't as useful compared to a different framing of things.' The ensorial.com piece is interesting because I think it is an instance of that last one. It was what pushed me to start thinking about all the various ways first principles thinking could go wrong.

The author's argument unfolds as follows:

1. Startups are risky.
2. Raising capital to do a startup reduces skin in the game (you're spending other people's money, after all).
3. Once you have less skin in the game, it is easier to make bad decisions. The author argues this is due to a) having a capital buffer to cushion you, and b) having more time to waste.
4. The alternative is to forego raising venture capital and to create a sustainable business from the beginning, 'growing linearly with the number of people that give you money for your product.'
5. This aligns incentives: you grow only by solving customer problems that they would pay you for. And you'll pick the shortest path, because you don't have the luxury of time given to you by an infusion of other people's money.
6. Therefore: startups shouldn't raise money.

At first glance, there doesn't seem to be anything that's explicitly *wrong* with this argument. I agree with all the base ideas, and I found myself nodding to the inter-

mediate propositions. The logical correctness of the argument wasn't a problem. No, my unease stemmed from experience: I *knew* this wasn't the right way to think about raising capital. But I couldn't begin to construct an argument that went against it.

AXIOMS /
BASE PRINCIPLES /
OBSERVATIONS

INTERMEDIATE
PROPOSITIONS

CONCLUSION
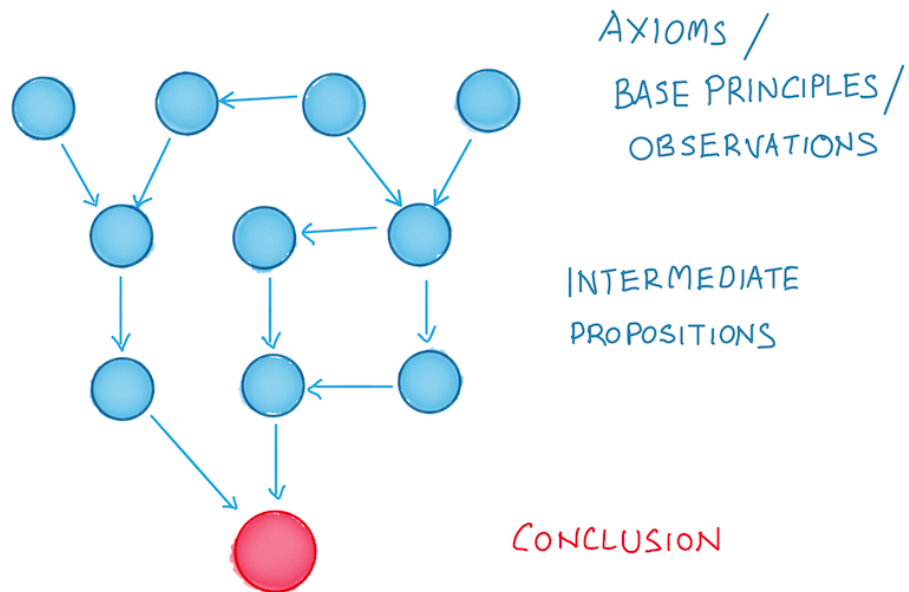
My friend and I spent no more than 10 minutes discussing this piece. But in the months after our conversation, I continued to return to the author's argument. I thought it was interesting because it represented a type of thinking error that you and I are likely to encounter in our lives. The form of the error is subtle, and therefore more difficult to detect; the best description I have for it is: 'perfectly rational, logically constructed, and not really *wrong* – but not as useful or as powerful as some other framing.'

Of course, my obsession was for instrumental reasons: how might you recognise a better framing when you found one? I'll admit that I was a little naive here: I thought that if I could generalise the structure of this argument, I would be better

able to recognise similar errors in the future. Alas, I have not been able to do this to my satisfaction.

(In practice, most of the older entrepreneurs I know seem to understand the problems with such sensemaking. Plausible arguments are dealt with in a simple manner: you try the recommendations that unfold from the analysis, but you remain alert to see if they give you exactly the results you want. If they don't, you keep the frame for the time being, but you continue to look out for a better explanation. And how would you know if you have found a better way of thinking about your situation? Simple: you listen carefully. In the words of Malaysian magnate Robert Kuok, "you learn to distill wisdom from the air.")

The most I've been able to do is to articulate *how* the author messed up – and therefore how first principles thinking may fail – something that I [explored in my previous post](). The core idea is simple: I believe the author started from a limited set of axioms. If you start from a wrong set of axioms, you would eventually end up with a flawed conclusion. In this case, I think the ensorial.com author started from a deficient understanding of business.

To generalise a little, people with limited understanding of business think that business is all about making profits. But those who actually run businesses know that running a business is all about managing cash flows.

And the ensorial.com author's argument fails because he doesn't appear to understand this.

John Malone and the Invention of EBITDA

In 1972, a 32 year old man named John Malone was offered the top job at Tele-Communications Inc (TCI), a cable company. He took charge on April Fool's Day, 1973.

At the time of his hiring, Malone was president of Jerrold Electronics, a division of General Instrument that supplied cable boxes and credit to the cable systems companies. He had been offered the Jerrold Electronics job when he was 29 years old, just two years earlier. Before JE, he was at McKinsey Consulting. And before McKinsey, he had a job at AT&T's famed Bell Labs, where he applied operations research to find optimal company strategies in monopoly markets. Malone concluded that AT&T should increase its debt load and aggressively reduce its equity base through share repurchases – a highly unorthodox recommendation at the time. His advice was delivered to AT&T's board and then promptly ignored.

Malone had been thinking about the interplay between debt, profit, cash flow, and corporate taxes for some time. In 1972, when he was first offered the TCI job, he had already noticed a number of structural properties in the cable industry that piqued his interest:

1. The cable industry had highly predictable subscription revenues. Cable television customers in the 60s – especially those in rural communities – were eager to upgrade to cable for better TV reception. These subscribers paid monthly fees and rarely cancelled.
2. Cable franchises were essentially a legal right to a local monopoly, which meant that cable system operators had limited competition once it established itself in a given locale.

3. The industry itself had very favourable tax characteristics – smart cable operators could shelter their cash flow from taxes by using debt to build new systems, and by aggressively depreciating the costs of construction. Once the depreciation ran out on particular systems, they could then sell them to another operator, where the depreciation clock would start anew.

4. Most importantly, the entire market was growing like a weed: over the course of the 60s and into the start of the 70s, subscriber counts had grown over twentyfold.

Of course, Malone didn't have much time to reflect on these observations. He landed at TCI and found the company at the brink of bankruptcy.

Bob Magness, the founder of TCI, had grown the company over the course of two decades using a ridiculous pile of debt – about 17 times revenues, at the time of Malone's hiring. Malone spent his first couple of years at TCI fighting to keep the company alive. He flew into New York every couple of weeks, hat in hand, renegotiating [covenants](#) and asking for extensions on debt repayments. At one point during a meeting with TCI's bankers, Malone threw his keys on the table and threatened to walk, leaving the company to the banks. The bankers capitulated, granting TCI a much needed extension.

Malone and Magness also had to worry about hostile takeovers, given TCI's low stock price in the early 70s. They executed a series of complicated financial manoeuvres a year or so after Malone took over, creating a separate class of voting stock. This gave them control of the company, allowing Malone the freedom to focus on righting its finances.

After three years of hell, TCI was finally pulled back from the brink of financial disaster. And then Malone got to work.

Malone understood a few things about the cable industry that many outsiders didn't. First, he understood that cable was like real estate: incredibly high fixed costs up front as you built or bought the systems, and then highly predictable, monopoly cash flows for a long time afterwards. He understood that if he used debt to finance acquisitions, he could keep growing the company, and use the depreciation on acquired systems (plus the write-offs from the loans itself) to delay paying taxes on that cash flow. Third, Malone understood that untaxed cash flows from all of those cable subscribers could be used to a) service the debt, b) pay down some of those loans – only when necessary; Malone wanted to keep the debt-to-earnings ratio at a five-to-one level – but more importantly c) demonstrate to creditors that TCI was a worthy debtor. And finally, Malone understood the benefits of size: the larger TCI got, the lower the cost of acquiring programming (i.e. shows and programs), because it could amortise those costs across its entire subscriber base.

The problem was that Wall Street in the 70s and 80s didn't get any of this. In 1986, brokerage firm E. F. Hutton refused to publish a report on TCI because "we don't publish reports on companies or industries that don't show a profit." And indeed, Malone's strategy required TCI to show a loss for pretty much forever; for the next 25 years, it was *never* in the black.

Malone went on a charm offensive. He began talking to Wall Street analysts, explaining his logic. To make his point, Malone created a new accounting metric, something he called 'earnings before interest, depreciation, and taxes', or EBITDA.

Mark Robichaux writes, in [Cable Cowboy](#):

Through a combination of logic, jawboning, and sheer force of presence, Malone persuaded Wall Street to take a second look at the cable industry, long shunned because of its nonexistent earnings and heavy debt addiction. Malone argued, successfully, that after-tax earnings simply didn't count; what counted was cable's prodigious cash flow, funding TCI's contin-

ual expansion. Buying cable was like buying real estate. As the value of TCI's franchises rose, so would the value of its stock. Net income was an invention of accountants, he declared.

Think about it, he'd tell a young analyst: Because TCI had high interest payments and big write-offs on cable equipment, it produced losses, and because it produced losses it paid hardly any taxes to the government. As long as cable operators collected predictable, monopoly rent from customers, met interest payments, and grew from acquisitions, why worry? Malone liked the mathematics of it: Tax-sheltered cash flow could be leveraged to land more loans to create more tax-sheltered cash flow. A standing joke around TCI was that if TCI ever did report a large profit, Malone would fire the accountants.

Malone was, essentially, a hacker: he stared deeply at the thicket of accounting rules, tax laws, and possible business moves, and found a strategy that took advantage of the structural realities he found in front of him. He was the first person to deploy this playbook rigorously, and TCI was amongst the first companies to start using EBITDA as a financial metric. Malone made good on his promise. Over the next 25 years, TCI went from acquiring cable companies to acquiring and investing in programming channels. It eventually became the largest cable company in the United States. It never turned a profit. And the results speak for themselves: from the year that Malone took over, in 1973 – to 1998 when AT&T finally bought it for $48 billion, the compound return to TCI's shareholders was a phenomenal 30.3%, compared to 20.4% for its competition, and 14.3% for the S&P 500 over the same period. ([Source](#))

Amongst business people and savvy investors, Malone's logic dovetails with a famous saying: 'cash flow is a fact; profit is an opinion'. Even today, there are people who do not fully understand the games you can play with cash flow. Or – more

importantly – they do not understand the *things* businesspeople would do for better cash flows.

In the 'misunderstood' bucket, take Amazon, for instance. In 1997, a 33-year old Jeff Bezos announced that he was essentially adopting the same playbook, firing a shot across the bow with his first annual letter to shareholders.

Bezos wrote:

*We will continue to make investment decisions in light of long-term market leadership considerations rather than **short-term profitability considerations** or short-term Wall Street reactions. (emphasis mine)*

We will continue to measure our programs and the effectiveness of our investments analytically, to jettison those that do not provide acceptable returns, and to step up our investment in those that work best. We will continue to learn from both our successes and our failures. (…)

*When forced to choose between optimizing the appearance of our GAAP accounting and **maximizing the present value of future cash flows, we'll take the cash flows**. (emphasis mine)*

For the next two decades, Amazon grew its revenue and made no profits, leading journalist Matthew Yglesias to [write](#), in 2013: "Amazon, as best I can tell, is a charitable organization being run by elements of the investment community for the benefit of consumers." Bezos enjoyed it so much he put it in his annual letter the same year. Bezos knew what he was doing; Yglesias [didn't get it](#).

The Games People Play With Cash Flow

So, you might ask, what does John Malone have to do raising venture capital? The answer: more than you might think.

The core idea that you should take from Malone's story isn't "oh, it's possible to build a valuable company with no accounting profits" – though that *is* a valuable insight – but instead *"there is a whole genre of games that people play with cash flow"* and also *"cash flow is often more important to grok than profits."*

One implication of these ideas is that raising capital for a business has more to do with the nature of cash flows in a particular business model than it does anything else.

Notice, for instance, how Malone's entire strategy was built around a single fact: that you have to pay up front for cable systems, but then earn back your money via a stable stream of cash for years and years afterwards. Notice how this extreme demand for capital drove Malone to embrace debt, over other sources of capital.

Now notice how closely this resembles the Software as a Service (SaaS) business model, which is the primary business model in today's startup world.

(In SaaS businesses – like Slack, or Zoom – you pay programmers to create software, and then you pay even more money to sales and marketing to land customers. Then the customers pay you a stable stream of cash for years and years afterwards. Same dynamics: high upfront costs, and then a stable stream of comparatively smaller payments later. So while SaaS companies don't typically use debt the way Malone did, they have – surprise, surprise! – similar capital requirements.)

Let's return to the original question posed by the ensorial.com author: why raise money from investors? In fact, why raise money at all? There is only one real answer if you want to reason from first principles: *you raise money due to the tem-*

poral nature of cash flows. To put this another way: in many businesses, you must spend money *now* to make money *later*. This implies that you'll need a source of capital at the start of many business ventures.

This might seem like a stupid, obvious statement to make – and it is! But I've learnt that people with little experience of business (or little exposure to equity investing) are likely to miss out on the full implications of this single statement. As I've mentioned earlier, those with a limited understanding of business think that business is all about making profits; those who have actually run businesses know that operating a business is mostly an exercise of managing cash flows.

Here's how I learnt this: in my previous company, I used to get frustrated whenever my old boss gave customers discounts in exchange for earlier payments. "We've done all this work!" I'd complain, "Why aren't we getting paid our due?"

"Well," he would say, "We need the cash." I sighed, and said that I understood, but I still didn't *really* get it.

Months later, I was kvetching about this practice to someone that I considered a mentor, when he interrupted me mid-rant: "But that *is* the logical thing to do, you know right?"

I was surprised. "How do you mean?"

"Think about it," my mentor explained, "You're running a completely bootstrapped business, which means you earn what you can sell. A huge chunk of your capital is locked up in inventory. Now your boss needs that cash back, to pay for expenses. You should view the discount he's giving to customers as a price he's willing to pay … *in order to unlock that cash flow.*"

It was then that I understood. It took me a few years, but I now get that a *major* part of business is simply learning the many games that you can play with cash flow.

For instance:

1) Payment Terms

Changing the payment terms on your invoices *instantly* makes your company more valuable, because you change the nature of cash flows in your business. (This is [commonly exploited](#) by private equity people, for obvious reasons … but I've also written about this before, in my summary of Ram Charan's *[What The CEO Wants You To Know](#)*).

2) The Effects of Speed on Cash Flow

Once you realise that it is in the nature of businesses to do *all sorts of things* to unlock more cash flow, you begin to realise that there are all sorts of interesting ways you can exploit this tendency.

For instance, in *[Good Synthesis is the Start of Good Sensemaking](#)*, I described the consequences of an upstart competitor embracing lean manufacturing, from the perspective of the incumbent. The competitive advantage afforded by lean manufacturing is subtle: yes, you get to make stuff at a cheaper cost, with better efficiency and a lower error rate. But the true advantage that you get lies in the *consequences of a speedy delivery*: if you can guarantee a fast, consistent delivery time from your factory, your downstream distributors may hold less inventory — which in turn means that they would have a better cash position.

Why is this cool? Well, you'll quickly learn that you can *charge a higher price, enjoy higher margins, while still taking market share away from your competitors.* Why? Simple: distributors will prefer to hold your product over a competitor's, given the better cash flow it affords them. (This still requires a certain amount of price inelasticity; for an in-depth look at how this happens, read *[Competing Against Time](#)*).

3) Pre-payments in the Restaurant Industry

People can also play cash flow games the other way.

Nick Kokonas is a restauranteur who runs three of the best restaurants and bars in America: Alinea, Next, and The Aviary. A few years after starting in the F&B industry, Kokonas realised that he could *charge for a deposit* for a restaurant reservation – something that most people thought was impossible. He began collecting money up front. This changed the dynamics of his business in a pretty radical way. Kokonas [describes what happened next](#) (around 55:44 in this podcast):

> *Food costs money. But the way that everyone (in the F&B industry) looks at food costs, and paying for food is very weird. When COVID started, every famous chef that went on TV said, "This is the kind of business where this week's revenues pay for bills from a month ago." So when we started to bring in money from deposits and prepaid reservations, I suddenly looked and we had a bank account that had a couple million dollars in it – of forward money, like a lot of other businesses, like the computer business – where you buy a computer and they only ship it to you five days later. So we had a float! So I started calling up some of our big vendors for the big, expensive items – like proteins: meat, fish; luxury items: like caviar, foie gras, wine and liquor, and I said, "I don't want net-120 anymore, I want to prepay you for the next three months." And they had never had that kind of a phone call from a restaurant before.*

You can see where this is going. The restaurant business is traditionally a cut-throat, low margin business. But with the magic of a float, Kokonas continues:

> *So how much should they discount it? So let's say we're going to buy steaks. We're going to pay $34 a pound wholesale for dry aged rib-eye, we get net-120 (normally). So I call the guy and say "I'm going to use 400 pounds*

of your beef a week for the next 4 months, for our menu, which is about about $300,000 of beef, what (would) we get, if we prepay you?" And he was like "what do you mean?" I'm like "I want to write you a cheque tomorrow for all of it, for four months." And he was like, "Well, no one has ever said that." So he called me the next day, he said "$18 a pound" … so … half. Half price.

(Interviewer): Wow.

(Kokonas again): That's what I said! I went, "I'll pay you $20 if you tell me why." And he said, "Well, it's very simple. I have to slaughter the cows, then I put the beef to dry. For the first 35 days I can sell it. After 35 days there's only a handful of places that would buy it, after 60 days, I sell it $1 a pound for dog food." So his waste on the slaughter, and these animals's lives, and the ethics of all of that, are because of net-120! Seems like someone should have figured this out! As soon as he said that, everything clicked, and I went "We need to call every one of our vendors, every time, and say that we will prepay them."

The net result – Kokanas concludes: "we've managed to take our food costs down to a level that honestly, our chefs never thought was possible."

Many of the games you play in business is centred around managing cash flow. Raising capital is one of them.

Raising Capital … From First Principles

There are only three ways to raise capital:

1. You sell equity – e.g. on the stock market, or to angel investors or rich friends, or to venture capitalists.

2. You take on debt – e.g. from banks, or insurance companies, or you may sell bonds, or issue [notes](#) or use some other debt instrument.
3. You use retained earnings – that is, you take cash generated by the business and reinvest it back into the business.

Each of these three choices come with their own nuances. And I don't mean a *tiny* bit of nuance, I mean that you can write whole books about each of these three options. There are differences in selling equity to an angel investor vs selling equity to a VC (vs setting up a joint venture, the primary method of expansion in South East Asia) the same way there are nuances with taking on debt (Junk? SPVs? Convertible note? The list goes on).

But at least now you have all the right axioms in place. You have an intuitive understanding of cash flow, and you understand that raising capital is a consequence of the temporality of said flows. You know that some business models demand exorbitant amounts of up-front capital. You know that there are effectively only three ways to raise capital, and you understand that the decision to raise capital is really a function of your business model.

So let's build up from first principles. If I were to make an argument about whether startups should or shouldn't raise capital, I would say:

1. Whether you should raise capital or not is a function of the cash flow characteristics of your business. What type of business are you in? What kind of business are you trying to build?
2. This is both a values question (do you want to work your ass off or do you want to generate enough cash to buy you freedom?) and a business question (is your business very capital intensive? will your competition make your life extremely difficult if you are under-capitalised?)
3. And finally: what pools of capital can you draw on? Most people in the startup world assume venture capital, because it is a default option. But

VC is simply one option; capital comes in many forms. Do you have rich friends or family? Do you or your friends have access to a family office? Are governments more likely to fund you? Are you in a market where people are more willing to write you a loan than to invest in a new company? (Which is sometimes the case in Singapore.)

4. To some degree, none of these questions can be answered until you've started executing, because only [action produces information](#).

How First Principles Thinking Fails, Part 2

I've spent most of this essay on the nuances of cash flow, which is kind of a joke, given that the goal of this piece is to demonstrate how first principles thinking fails. But I wanted to give you a real world example, and the ensorial.com blog post happened to be the one essay that started it all.

So I guess the joke's on me.

I think it's worth asking at this point: what are the consequences of believing in a 'less useful' argument about the world? What happens if you read the ensorial.com's argument as fact?

Well, for starters, it might mean being limited by your beliefs. It might mean chasing business ideas that have no hope of becoming successful, because structurally they call for external capital; it might mean ignoring alternative sources of capital, because they don't fit into neat buckets like 'VC' or 'angel' or 'venture debt'.

But of course it might not mean any of that. You could just as well bootstrap a tiny, successful internet business selling Wordpress plugins or Shopify themes, believing that 'startups shouldn't raise capital'. You would then never need to update
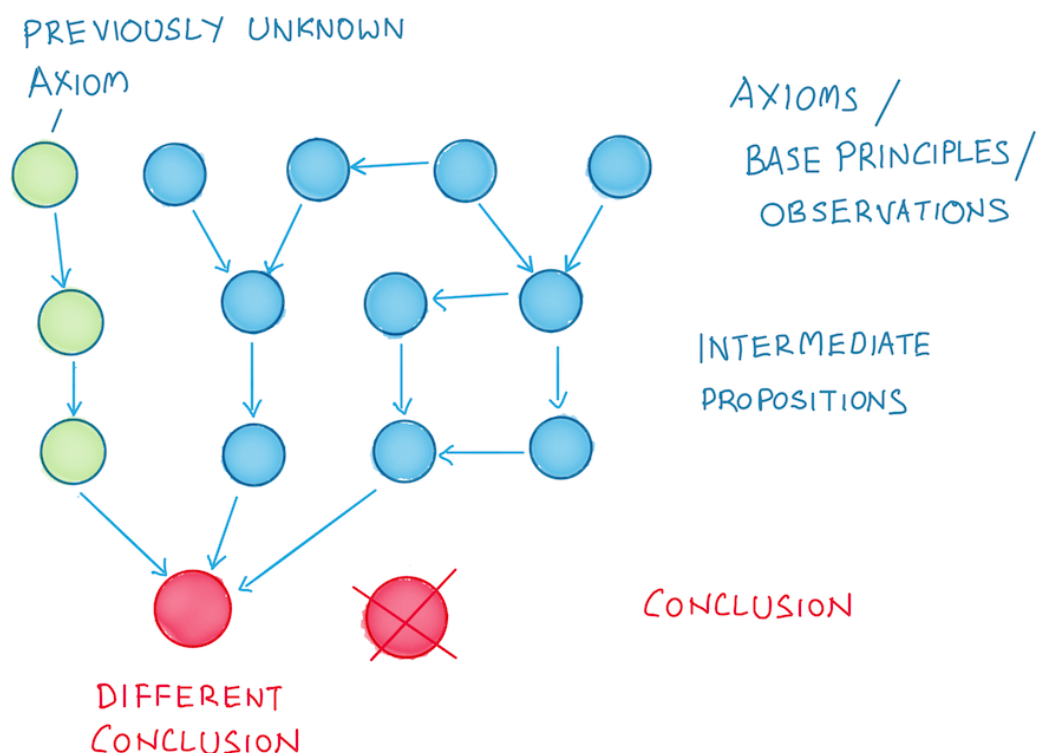
your beliefs, because those are perfectly sufficient for a small, independently-run business.

And that's fine.

Ultimately, we hold beliefs that are good enough for our goals. We only re-examine them when we discover that they no longer serve us in the pursuit of our ambitions. If the ensorial.com author achieves success with this belief; more power to them.

*

I've mentioned in my previous piece that the most pernicious form of failure for first principles thinking occurs when you start from an incomplete set of axioms. I argued that this was a pernicious because you cannot easily detect a mistake from the structure of your argument; you must observe reality to see how well your beliefs maps to practice.

PREVIOUSLY UNKNOWN AXIOM

AXIOMS / BASE PRINCIPLES / OBSERVATIONS

INTERMEDIATE PROPOSITIONS

CONCLUSION

DIFFERENT CONCLUSION

I then (hopefully!) demonstrated this with this post: I explained that an understanding of cash flow is critical to any argument about capital. Not having that understanding is likely to hobble you when you build up an opinion from first principles.

There's a meta question that's implied by this entire essay, of course. The question is this: how would you know whether the first principles reasoning you've built up, from scratch, is right or wrong? In other words, how do *I* know this essay is right? This is a good question, and the honest answer is that I don't. I've never raised venture capital. We tried to take on debt in my previous company, but every loan we were offered required us to sign a personal guarantee. (Yes, even the Singapore government-backed ones; I will never understand the logic there).

There may be whole axioms that I'm missing out on. But that's par for the course.

In the end, this is exactly the point I'm trying to make. You never really know if your rigorously argued analysis is right; you always have to check for missed axioms during execution. And so I think the best thing that we can hope for is some form of "this analysis checks out, everything you seems plausible, *let's wait and see*."

Which, reflexively, applies to this essay. Caveat emptor.

To Get Good, Go After The Metagame

I have a friend who's currently doing a PhD in Artificial Intelligence. Last year, when he was back in Singapore, he went over to the [Nanyang Technological University](#) to give an hour-long talk on his recent work. NTU is located far in the west of Singapore, tucked into a bit of no-man's land; my friend came back six hours later, exhausted but happy.

"What took you so long?" I asked, because we had dinner plans with other friends, and his delayed return meant that he couldn't join.

"Oh, the PhD students who attended wanted to talk about the meta. And then I lost track of time."

*

Every sufficiently interesting game has a metagame above it. This is the game about the game. It is often called 'the meta'.

Sometimes, the metagame is created when new options are introduced from outside the game. [Magic The Gathering](#) is famous for having a game system that changes every time the publisher releases a new set of cards. MtG's metagame is thus a race to see who can discover new card combinations or strategies given the new options. The players who do so are rewarded with easier wins, especially when going up against players who have not adapted to the new possibilities. This makes MtG a game of two levels: the first game is the game you're playing when you sit down and shuffle cards to battle an opponent; the second game is the race to acquire, analyse and adapt to new cards quicker than your competition.

Metagames like MtG's also exist in, erm, more physical games. Judo – the sport that I am most familiar with – has a metagame that is shaped by rule changes from the International Judo Federation. A few years after I stopped competing, the IJF banned leg grabs, outlawing a whole class of throws that were part of classical Judo canon – many of them used regularly, even at the top levels of competition. The Judo that exists today is very different from the Judo I left – with changes in gripping strategy, entry styles, and technique combinations – many of them responses to responses to responses to the ruling.

These 'response chains' characterise another type of metagame. Some games don't have changing rules, and are free from an overwhelming supply of new possibilities. In such games, the meta changes when competitors act *within* the space of possibilities of a static game system.

An example of this is the excellent board game Splendor. Players buy expensive cards to earn victory points, using poker chips acquired from a central pool. Cheaper cards carry no points but give you a discount on future purchases. The most obvious strategy when you start out in Splendor is to build an 'engine' – that is, a tableau of cheap cards that provide you with a large number of discounts, after which you can go after higher-priced, higher-point cards, and win.

The metagame in Splendor emerges the instant someone figures out that you can skip on buying cards for discounts, save up on chips, and go directly after the more expensive cards with points. Once a player makes this discovery, Splendor changes for the better. It becomes a fluid game of blocking, reserving, purchasing and adapting – with most players doing a combination of 'building' and 'buying' in response to what *everyone else* is doing around them.

Splendor is a good example because there are only two major approaches in the game, and sufficiently competitive gaming groups will discover both of them in a matter of days. But the optimal strategy in Splendor will continue to shift back

and forth as a hybrid of the two strategies, depending on the player mix and the changing preferences of the players within your gaming group.

As it is with games, so it is with life.

The Meta In Real Life

Every sufficiently interesting domain in the world has a meta associated with it.

Like simpler games, real-world metas come in roughly two flavours: ones that are defined by external changes to the rules of a game, and ones that are shaped by a dynamic equilibrium of competition within a stable system of play. Unlike games, however, real-world domains have no set rules: they are vastly more complicated and interesting, because the rules change only when *someone notices the rules have changed*.

Here's a real world example.

I've spent the last two years working to get better at marketing. If you're reading this blog, you've probably observed the evolution of my content marketing skills first hand. (I grade myself a C+, but more on this in a bit).

Anyone who attempts to become really good at marketing must learn the same handful of basic ideas: amongst them, the concept of a funnel and how to measure it, the idea of the buyer's journey, and the ability to construct and use buyer personas … even if done unrigorously. (The idea of the persona is more important than the form of the persona itself. I know one or two self-taught marketer/entrepreneurs who have an intuitive – and effective! – conception of their ideal buyer in their heads, but are unable to articulate it properly when asked).

Above these basics are channel-specific skills. These are tactical things: some marketers are better at email marketing, others at content, still others are better at ad-buys and social media. A valuable subset of marketers are able to take an entire

team and scale it up for a specific type of product. Whichever it is, these skills are deep and context-specific. Good practitioners know the 'best practices' for their specific channels. The better practitioners keep up with the evolution of those best practices. The best marketers … well, the best marketers play the metagame of marketing.

The metagame of marketing emerges from the fact that all marketing channels decline in efficiency over time. To hear the veterans tell it, Google Adsense in the early 2000s was like shooting fish in a barrel. By the 2010s, Adsense had become prohibitively difficult and expensive – its costs driven up by mainstream adoption and increased competition. Similar stories have played out in the Facebook-owned ad ecosystem (think: Facebook, then Instagram, then Instagram stories). The best marketers are therefore the ones who advance the best practices the fastest to keep ahead of the mainstream, or are able to identify and develop playbooks for new channels before the old ones become too inefficient to fight in. The quicker they identify new channels and the longer they keep their playbooks secret, the better the marketing game becomes for them.

In this way marketing's meta is much like MtG's.

Business also has a meta. Take acquisitions, for instance. Companies have acquired other companies since the beginning of companies – but the game through which this happens has changed significantly over the years.

Henry Singleton, the CEO of Teledyne, famously used share issuances in the 60s to fund Teledyne's many acquisitions, eventually building it into one of the most valuable conglomerates in America. He then pioneered the use of the share buyback to purchase Teledyne stock cheaply during the bear markets of the 70s, eventually repurchasing 90% of all outstanding shares – including the ones he had issued at high prices in the preceding decade. Investor William Thorndike says that Singleton's track record is in 'a completely different zip code' when compared to famous contemporary CEOs like Jack Welch: GE under Welch outperformed the

S&P 3.3 times; Teledyne under Singleton outperformed the S&P over 12 times during his tenure as CEO.

Singleton's playbook changed the rules of business in ways that still echo today – for instance, share buybacks were treated with derision during Singleton's time, but are today considered normal chapters in the corporate finance playbook. Much later, parts of Singleton's conglomerate building strategy was adapted to a different context by a young man named Warren Buffett, who applied it to an ailing textile manufacturing company named Berkshire Hathaway.

In the late 70s, the meta changed again. A man named Michael Milken [pioneered](#) the use of an obscure financial instrument called the high-yield bond, more popularly known as the 'junk bond'. He quickly realised that these junk bonds could be used to raise immense amounts of capital for the purpose of corporate takeovers, with little to no cost to the acquiring company. Such 'leveraged buyouts' (LBOs) – which issued debt on the target company's assets, not on the parent company's books – changed the rules of the takeover game in ways that persist till today. The players that emerged to take advantage of this innovation – the ones who skated to the edge of the meta? We know them today as private equity firms.

The meta in business changes whenever new tools or opportunities emerge in the macro environment. New tools mean new options. New options mean new viable strategies. The Koch brothers adapted pieces of Milken's LBO playbook when they began expanding their energy empire in the 80s; today, the private equity world is adapting those strategies to survive in a world of excessively cheap capital. The meta changes yet again in response to macro conditions – and on it goes.

The Meta as a Test of Expertise

What is interesting about the meta is that metagames can only be played if you have mastered the basics of the domain. In MtG, Judo, and Splendor, you cannot play the metagame if you are not already good at the base game. You cannot identify winning strategies in MtG if you don't do well in current MtG; you cannot adapt old techniques to new rules if you don't already have effective techniques for competitive Judo.

What is true in sports is also true in real world domains like marketing and business; it is true even if you are aware of the meta's existence. The nature of the metagame demands that you play the base game well. It lives on top of the [pattern-matching that comes with expertise](#).

This seems like an obvious thing to say. But as with most such things, the second-order implications are more interesting than the first-order ones. For instance, because expertise is necessary to play the metagame, it is often useful to search for the meta in your domain as a north star for expertise. The way I remind myself of this is to say that I should 'locate the meta' whenever I'm at the bottom of a skill tree. Even if I can't yet participate, searching for the metagame that experts play will usually give me hints as to what skills I must acquire in order to become good enough.

An example suffices: in marketing, I have found it very useful to seek out articles or podcasts (but especially podcasts) where practitioners talk about the ways their best practices have changed: "In the past I did X and now it seems it doesn't work as well, so now we do Y, and I recommend Y." This tells me that:

- I need to learn Y and at least have a passing familiarity with X, because X is what came before and is 'covered ground', and

- I need to remember the 'shape' of the shift. If one such shift happened in the past, more may happen in the future.

More concretely, this is something like a podcast guest saying:

"In the past (for content marketing) I did roundup posts, but these don't seem to work as well anymore. So now we do longer, more comprehensive guides, and we cross-launch those to Product Hunt and Hacker News and social media. That seems to work better for us."

Which in turn tells me:

1. I need to look up as many examples of these 'comprehensive guides' to see what the state of the art looks like. From there I can develop an understanding of how they are planned, how they are executed and what success looks like when these things are launched. And I must launch one to verify their effectiveness for myself.
2. I need to try my hand at at least one roundup post, just for the tacit experience of doing it.
3. And I must ask: why did roundup posts fail? Well, because too many people are doing it. (Is this true? How do I verify?) Why might the *current* best practice fail, then? I might not know the answer to this because I am a noob, but a side-effect of (1) is that I now know the best practitioners of the guide strategy, and I can observe them to see what changes in their marketing over the next few years.

Note what I'm *not* saying, however. I'm not saying that I should actively pursue the meta – this is ineffective, because I am not good enough to play. I cannot execute even if I know where the puck is going. But studying the state of the metagame as it is right now often tells me what I must learn in order to get to that point.

(This is, by the way, where my self-grading comes from — I look to the marketers who are playing at the edge of the meta, and I rate myself a C+ in comparison. The ability to make such comparisons is in itself useful).

[James Stuber](#) has this thing where he [says](#) "master boring fundamentals", and he says it in response to beginners who desire to go after the fancy stuff. Stuber's quip applies here. To run with his terminology: I'd say that the meta is what you get *after* you master boring fundamentals. But observing the state of the current meta often reveals what boring fundamentals you need to learn.

This is particularly useful if your skill tree has no set syllabus. It is especially useful when you don't have a coach.

Get to the Meta For One Skill

How do you balance between locating the meta and chasing boring fundamentals? The short answer to that is to do trial and error. If you can't master a particular skill, drop back down to its component elements and practice each of them in isolation. If you don't get good conversions in your content marketing, drop down to practice publishing at a regular cadence. If you can't get a throw to work, break it down to arms, then legs, then body position, then into one complete motion.

How do you learn to do this? I think the best way is to get good at a single, well-structured skill … and preferably, to get this experience as early as possible in one's life.

I've noticed that the 'feel' of improving in pursuit of a meta is strangely similar across skill trees. I noticed this after climbing high enough in the Judo skill tree to see the meta for the first time. Later, when I moved to university, learning the craft of programming became slightly easier. And then after that, learning people man-

agement became easier. Ditto for content marketing, and email marketing, and so on.

I don't know if it is identifying the meta that helps, or if there is some other deep skill transference that's going on. But I've cautiously reached out to friends who have played serious competitive sports when they were younger, and many of them have had similar experiences. There seems to be *something* in getting good at a skill tree that helps in latter life. I'd like to think it is a function of exposure: once you see the competitive meta at the top of one skill tree, you begin looking for it everywhere else.

*

My closest cousin is a software engineer. Recently, his frontend engineering team hired a former musician: someone who had switched from piano to Javascript programming with the help of a bootcamp. He noticed immediately that her pursuit of skills (and the questions she asked) were sharper and more focused than the other engineers he had hired. He thought her prior experience with climbing the skill tree of music had something to do with it.

My cousin plays [ultimate frisbee](#) on the side. To hear him tell it, the metagame in frisbee is more like Splendor: within an unchanging rule system, the dominant strategies are always an adaptation to what your opponent is doing. These strategies sit atop basic throwing and receiving skills. Sometimes, if the opportunity arises, if you understand the metagame well enough, and if you are lucky, your team can get really creative with responses. My cousin loves Ultimate — when he talks about it his eyes light up and he begins gesticulating. It is for him as Judo was for me.

When we meet, we talk about the meta a lot because we see parallels between our sports and our respective careers. My cousin has this theory that the people who get really good at one skill will find it easier to get good at a second skill. I believe him. Our experiences bear this out.

(Related: Tobi Lutke offers former pro StarCraft player [a job at Shopify](); this [fun piece]() then analyses Lutke's strategy as CEO of Shopify through the lens of StarCraft, although it's clear that the author doesn't grok StarCraft's meta *at all*).

My cousin and I both want our kids — when we have them — to get good enough to play in a metagame for one skill, any skill, in whatever game strikes their fancy. It could be Ultimate or Judo or tennis or chess. It could be [Fortnite]() and [Dota](), if those things still exist when they're old enough.

The important thing is exposure to the meta. My cousin and I both think this exposure is fundamental. We think this because metagames really do exist everywhere in life; you just have to know how to look.

Are You Playing to Play, or Playing to Win?

My friend [Lesley](#) has this thing where she says "make sure you're playing the *real* game, not some more complicated game you've made up for yourself."

I think about this a lot.

Lesley says this in the context of [Ultimate](#) — she was a coach for the Singaporean women's world championship team. She says that when she was a player, she believed that certain methods of winning in frisbee were more legitimate than others. Preferably, you wanted to win with strategies that were technically sophisticated and elegant and difficult to do — the more difficult, the better. But as a coach, she's had to remind herself that the goal for her team is to win, not necessarily to set high bars for play.

I played Judo very competitively when I was younger. For about two years when I was 18, my entire life was school, training and sleep. The only problem was that I wasn't very good at it. I had it in my head that the only Judo worth playing was a 'Japanese' style of Judo — an upright, dynamic, beautiful style. No other style was worth looking at — not Georgian, not Mongolian, not even French. The problem: kids in Japan start at around six years of age; I started playing Judo at 15. There was no way I could have won using the Japanese style. It simply demanded too much technical proficiency. And so while I was good enough to be selected for my state, I never did well in the National Championships.

Contrast this to other, smarter competitors. American Olympian Jimmy Pedro [said](#), of his father: "My dad was a guy who started Judo when he was 19 years old. So he was way behind the eight ball against everyone else; he was trying to go to

the Olympics. Gripping was something that was introduced to him in his early 20s. And he realised that if he learned to grip fight properly, he could beat those technicians who had been playing Judo since they were five or six years old. Because it's hard to get a natural feel for the sport when you start so late – like you're never going to be as instinctually as good as somebody who starts when they're a kid. But my dad learnt that gripping was a way to get good fast."

For context, in Judo grip fighting is what happens *before* you can even start to throw. The competitor that consistently gets a superior grip determines the pace and shape of the match. Pedro and his father are perhaps most famous for *systematising* the grip fight – that is, they turned it into something that could be taught to all players, instead of relying on unsystematic tricks, or on intuition acquired through a high volume of sparring. Perhaps most importantly, though, Pedro brought this thoughtful, strategic approach to all of *his* students. In his [interview with Lex Fridman](), he says, rather frankly:

> *We know that we cannot beat the Russians, we cannot beat the French, we cannot beat the Brazilians, we cannot beat the Japanese by doing more Judo than they do. Because it's impossible. We can't beat them with Judo, because they have way more people to train with, way more opportunity, so we have to beat them with … physicality, technical strategy, gripping, newaza, conditioning, toughness … in the mindset, that we're going to win, and this is how we're going to win, and you have to get your students to believe in the system."*

Pedro never won gold at the Olympics. But he coached a whole bunch of Americans who titled internationally, and he coached Kayla Harrison, the first American ever to win an Olympic gold in Judo. He is as [believable as they come]().

I wished I'd learnt this alternative view of competition earlier in my life. As a competitor, I didn't think very deeply about the constraints that I was facing, and I

didn't have a coherent strategy to win. I simply thought to myself: "Ahh, the Japanese play beautiful Judo. That's the best form of Judo. I want to be like that." And then I refused to look at other styles.

I was, in other words, a scrub.

Being a Scrub

In the world of gaming, a scrub is someone *who isn't playing to win*. This sounds a little bizarre – *what competitor isn't playing to win?* – so I'll let Street Fighter tournament player and game designer David Sirlin explain:

> "Scrub" is not a term I made up. It sounds like kind of a harsh term, but it's the one that was already in common usage in games to describe a certain type of player, and it made more sense to me to explain that rather than to coin a new term.
>
> A scrub is not just a bad player. Everyone needs time to learn a game and get to a point where they know what they're doing. **The scrub mentality is to be so shackled by self-imposed handicaps as to never have any hope of being truly good at a game. You can practice forever, but if you can't get over these common hangups, in a sense you've lost before you even started. You've lost before you even picked which game to play. You aren't playing to win** (emphasis added).
>
> A scrub would disagree with this though. They'd say they are trying very hard. The problem is they are only trying hard within a construct of fictitious rules that prevent them from ever truly competing.

Sirlin continues with an example from the fighting game Street Fighter:

Scrubs are likely to label a wide variety of moves and tactics as "cheap." For example, performing a throw in fighting games is often called cheap. A throw is a move that grabs an opponent and damages them even while they're defending against all other kinds of attacks. Throws exist specifically to allow you to damage opponents who block and don't attack.

(…) As far as the game is concerned, throwing is an integral part of the design—it's meant to be there—yet scrubs construct their own set of principles that state they should be totally impervious to all attacks while blocking. Scrubs think of blocking as a kind of magic shield which will protect them indefinitely. **Throwing violates the rules in their heads even though it doesn't violate any actual game rule** (emphasis added).

(…) Complaining that you don't want to do X in a game because "it doesn't take skill" is a common scrub complaint. The concept of "skill" is yet another excuse to add fictional rules and avoid making the best moves. Curiously, scrubs often talk about how they have skill whereas other players—very much including the ones who beat them flat out—do not have skill. This might be some sort of ego defense mechanism where people define "skill" as whatever subset of the game they're good at and then elevate that above actually trying to win.

For example, in Street Fighter scrubs often cling to combos as a measure of skill. A combo is sequence of moves that are unblockable if the first move hits. Combos can be very elaborate and very difficult to pull off. A scrub might be very good at performing difficult combos, but not good at actually winning. They lost to someone with "no skill."

Single moves can also take "skill," according to the scrub. The "dragon punch" or "uppercut" in Street Fighter is performed by holding the joystick toward the opponent, then down, then diagonally down and toward as the player presses a punch button. This movement must be completed within a

fraction of a second, and though there is leeway, it must be executed fairly accurately. Scrubs see a dragon punch as a "skill move."

One time I played a scrub who was pretty good at many aspects of Street Fighter, but he cried cheap as I beat him with "no skill moves" while he performed many difficult dragon punches. He cried cheap when I threw him 5 times in a row asking, "is that all you know how to do? throw?" I told him, "Play to win, not to do 'difficult moves.'" **He would never reach the next level of play without shedding those extra rules in his head** *(emphasis mine).*

For me, the equivalent to 'not using Street Fighter throws' was usage of the high collar grip. Most Judo competitors learn from an early stage to grab their opponent's high collar — because this gives you great control over your opponent's head. I was told not to use it, due to the height disadvantage I had back when I was 15. Along the way this turned into a scrub rule — I felt like I was extra worthy if I managed to win *without* grabbing the high collar. (And to be fair, I did develop certain methods to do so). Looking back, I think this was dumb — but I was young and stupid and didn't know any better.

Scrubs in Business

Is there scrub behaviour in business?

Almost certainly: one trivial example is the stance that I alluded to in [Changing My Mind on Capital](#) — the idea that bootstrapping without ever selling equity to external investors is somehow a better way of doing business. I argued that this was silly, that capital was a tool, and that having beliefs about using capital was more

ideological than it needed to be. But perhaps bootstrapping with no external funding *is* better. We do seem to admire it when it happens.

Talking about scrub-like behaviours in business is where our discussion becomes more nuanced and interesting. *When is scrub-like behaviour truly scrub-like? And when is it ethics?*

Long-term readers of Commonplace would know my admiration for John Malone, the onetime CEO of cable company TCI. In [The Games People Play With Cash Flow](#), I wrote:

> *Malone was, essentially, a hacker: he stared deeply at the thicket of accounting rules, tax laws, and possible business moves, and found a strategy that exploited the structural realities he found in front of him. He was the first person to deploy this playbook rigorously, and TCI was amongst the first companies to start using EBITDA as a financial metric.*

In my eyes, Malone is an almost perfect example of a *non*-scrub — a person who saw into the game of business, and played the *actual* game, not some made-up, more difficult version of the game. Malone's insight was that he could load TCI up with debt (at a disciplined five-to-one earnings ratio), and then use the interest payments and cable equipment depreciation as a tax shelter for TCI's utility-like, monopoly cash flows. He then used that debt to expand aggressively, gaining scale advantages, until TCI became the largest cable company in the US, owning interests in various programming and tech ventures along the way.

What I've never written about, though, is the flip side of Malone's reign as TCI's chief — that is, that Malone *treated his customers as an afterthought*. This was a straightforward side-effect of the 'monopoly' in 'monopoly cash flows' in my description above — due to the nature of cable rights (again: a government-granted monopoly, so not ostensibly an evil thing), operators didn't really have to worry

about competition, and some were willing to let cable systems degrade over the years.

But Malone took this to an extreme. In [The Outsiders](#), William Thorndike writes:

Until the advent of satellite competition in the mid-1990s, Malone saw no quantifiable benefit to improving his cable infrastructure unless it resulted in new revenues. To him, the math was undeniably clear: if capital expenditures were lower, cash flow would be higher. As a result, for years Malone steadfastly refused to upgrade his rural systems despite pleas from Wall Street. As he once said in a typically candid aside, "These [rural systems] are our dregs and we will not attempt to rebuild them." This attitude was very different from that of the leaders of other cable companies who regularly trumpeted their extensive investments in new technologies.

Ironically, this most technically savvy of cable CEOs was typically the last to implement new technology, preferring the role of technological "settler" to that of "pioneer." Malone appreciated how difficult and expensive it was to implement new technologies, and preferred to wait and let his peers prove the economic viability of new services, saying of an early-1980s decision to delay the introduction of a new setup box, "We lost no major ground by waiting to invest. Unfortunately, pioneers in cable technology often have arrows in their backs." TCI was the last public company to introduce pay-per-view programming (and when it did, Malone convinced the programmers to help pay for the equipment).

Over the course of Malone's two decades of leadership, TCI earned a reputation for poor customer service and 'unjustified' price hikes. It let its systems rot. As a result, it was hated to varying intensities by customers, local government officials, and politicians alike. Malone didn't care. He played the game according to the rules of the game – and the rules of the game, as he saw it, was that monopoly-

type markets afforded businesses the luxury of ignoring customer happiness. He only upgraded when absolutely necessary — such as when satellite competition arose in the mid-90s.

As I was reading Malone's story, I remember thinking to myself: if I were in his shoes, would I be willing to play the game the way he played it? Or would I have spent some of that cash flow to upgrade systems, at the expense of expansion, market share, and better unit economics? I'd like to think that I would've upgraded, that I would have had more empathy for my customers — but then does that make me a scrub? Consider: system upgrades had no positive effect on the performance of a cable business. If nothing else, the capital outlay (and accompanying debt load) often made smaller cable companies more fragile — and Malone was always eager to snap up distressed cable systems on the cheap.

In other words, if I'd played the 'customer-service' game, it is likely that I would've failed, and sold to someone like Malone. But perhaps there was a way around it? The difficulty of business — and the difficulty of talking about it in the context of scrub behaviour — is that the rules often are only what you can discover to be true. (And even *those* rules may change, depending on the behaviours of competition). There is no explicit game designer to balance play in the markets; you are truly on your own.

And Yet There are Maestros

One reason that scrub behaviour is so compelling to us is that we admire those who decide to play a harder game, and who manage to win anyway.

In Judo, the best contemporary example might be [Shohei Ono](#), who won his second Olympic gold in the -73kg category at Tokyo, as part of a seven year undefeated streak. Ono consistently wins by dominant throw, choke, or pin; he never

aims to win by penalties alone. And those in the Judo world love him for it – we know that he is playing a more difficult game, with more constraints than his competitors, and we love that he wins so consistently despite the 'handicap'.

A more mainstream example, however, might be Roger Federer. Federer uses a one-handed backhand – a notoriously more difficult technique than the two-handed version. He also happens to make it look easy. We don't love him for that, of course – we love him because he does it while absolutely dominating the highest levels of tennis – at least for the good part of two decades. And so to watch Federer play a tennis match is a little like watching God play tennis – or perhaps, more accurately, like watching a variation of tennis that is more ballet than racquet sport.

https://www.youtube.com/watch?v=MTP99IHemNA

This, I think, is what mastery looks like. Federer, like Ono in Judo, is a maestro. The uncharitable implication is that a maestro is simply a scrub who wins. We can't believe that they do it. And we respect them because they do it anyway, and win.

Are there examples in business? Certainly.

A friend of mine was telling me about Martine Rothblatt's United Therapeutics. Rothblatt started UT in 1996 to save her youngest daughter, who had an orphan disease called pulmonary arterial hypertension (PAL). In her interview on the Tim Ferriss podcast, Rothblatt explained the genesis of her company:

> So there are a good zillion articles published on every type of medical research you could imagine. I mean, it's just a bottomless well. There are literally hundreds of different types of medical journals. And each of those journals have every year thousands of articles published across them. So it's difficult to find the information that you need, but in law school, we learn a very useful skill. And this skill goes by the name of Shepardizing, after this type of index they have in law school called Shepard's. So what Shepardiz-

ing involves is when a judge writes a decision like the Supreme Court issues a decision, they drop a lot of footnotes. And of course, one thing lawyers love to do is make footnotes and references. And then what you're supposed to do as a good lawyer is to look up all of the footnotes and the references that that Supreme Court or lower court case referred to. And then the Shepardizing process is after you get all of those references to then look up all of the references in those other articles. And ultimately, you get to a point of diminishing returns where three, four, five levels down, the references are all circling back around on themselves.

So I applied that Shepardizing process to these medical articles, and somewhat like doctors, whenever a researcher publishes an article, they make footnotes and citations to other people's research who they relied upon. So I would get all of those articles and read those. And then I would follow up on all of the references in those. Finally, I read about a molecule that a researcher at Glaxo Wellcome had written in which they described testing this molecule for congestive heart failure. And it failed in its test of congestive heart failure. It did not work, but in the article, they had charts of what the molecule did. And the one thing that the molecule did that grabbed my attention was that it reduced the pressure between the lung and the heart, which is called the pulmonary artery. It reduced the pulmonary artery pressure while leaving the pressures and all of the rest of the body perfectly fine. Well, that's exactly the problem with pulmonary arterial hypertension, the people who have this disease.

Rothblatt then went to Glaxo Wellcome to ask about the molecule, but was told that they weren't going to develop it:

The individual who had written the article had actually retired a few months earlier. And the person that I ended up meeting with, who was in charge of

research and development, said that this was just one article. It was an inci-dental finding. In any event, this disease afflicted so few people, it was com-pletely unrealistic to expect Glaxo Wellcome to develop this molecule for my daughter and other people with that disease. And I asked him, his name's Bob Bell, he's now a venture capitalist and very successful gentle-man. I asked Dr. Bell, I said, "What would it take for you to develop this med-icine?" He said, "Well, it probably would take – you couldn't do it. We only develop medicines if they have more than a billion dollars a year in revenue potential." He said, "But it's possible you could buy it from us. If you had a real pharmaceutical company with real pharmaceutical expertise, I could then introduce you to the business development people at Glaxo Well-come."

Which is what she did.

Rothblatt sold her stake in SiriusXM (a company she had co-founded), started a biotech company within a few months after the conversation, got Glaxo Wellcome to sell her the molecule, and then turned it into a drug that eventually saved her daughter and countless others with PAL. As a twist in the story, Glaxo Wellcome had only asked for $25,000 and 10% of the revenues from the patent, since they were not expecting to make *any* money from it. To hear Rothblatt tell it, UT has paid back more than a billion dollars in royalties to Glaxo Wellcome in the years since they first brought the drug to market.

Much later, UT acquired the technology to refurbish human lungs for transplan-tation. But Rothblatt wasn't happy about the carbon costs of flying damaged or-gans to their facility and then back out to the hospitals – she thought that it was awfully wasteful to have a such large carbon footprint even if for a good cause.

So, of course, they started developing electric helicopters.

Rothblatt, again on the Tim Ferriss podcast:

But I mentioned this because this has a lot of flying around; flying here, flying there, helicopters going back and forth, planes. And if I'm going to make an unlimited supply of organs, and you remember all those numbers we talked about at the beginning of the call, the hundreds of thousands of people who need these organs, that is going to be a humongous carbon footprint. We could have said to ourselves, "Well, we're doing such a good thing. We're saving all these lives. We could be permitted to foul our atmosphere because it's balanced by the good things we're doing." But instead, we like to ask ourselves the challenging question, "How can we do the good thing and the right thing at the same time? How can we manufacture all these lungs and deliver them with a zero-carbon footprint?"

And the solution came from the technology of electric helicopters, which are powered by renewable energy that can fly these organs from one place to the other without adding any carbon footprint at all. And I will be a little bit of a soothsayer here, I am absolutely convinced that in this decade, the 2020s, we will be delivering manufactured organs by electric helicopter.

Will they succeed? I don't know. But I certainly *want* them to. And my friend, who owns shares in UT more out of awe than anything else, says that it's ridiculous that a biotech company would want to go beyond doing good to *doing right by the environment*. A more normal response would be to buy carbon offsets and to call it a day. Making investments in electric flying is another thing all-together.

I think Rothblatt deserves to be called a maestro. And I hope that the markets that UT plays in affords them the ability to play the harder game and win.

Wrapping Up

What should we conclude from this discussion? I keep going back to Lesley's original framing: "make sure you're playing the *real* game, not some more complicated game you've made up for yourself."

This is, of course, easy to say. Nobody wants to be called a scrub. But when all is said and done, there are aesthetic and moral reasons to want to play a more difficult game.

One of the funny ironies of this discussion is that that Federer himself wants his kids to learn the double-handed backhand. In a press scrum in 2019, he [said](#):

> *"I know I love tennis and going out there to play. I would go for a two-handed backhand for all of my four kids because it's easier; it's that simple. If they want to change that later on, I will teach them to hit a one-handed backhand.*
>
> *"But I can't teach them a double-hander as I can't hit that one. So that's somebody else's job. At the end of the day, like with everything in life, you also have your own character. Some people decide to change it at eight, some at 14 and some later because they find it a good challenge.*
>
> *"For now, that's what it is. And, who cares anyway if they hit a double-hander or not? It shouldn't be in the press."*

There's this old nut by Warren Buffett that goes "I don't look to jump over seven-foot bars; I look around for one-foot bars that I can step over." I think that captures the essence of *not* being a scrub.

The irony, of course, is that it was uttered by a maestro — and arguably the greatest investor to have ever lived. But you only get to be called a maestro if you play the harder game *and* you win. For everyone else, it's probably a better idea to avoid being a scrub. Winning is hard enough as it is.

Don't Read History for Lessons

Here's a real world story that you might be familiar with. The question I'd like you to ask while reading it is: what lessons might *you* take away from it? Keep this question in mind; we'll return to it in a bit.

In 1958, Morris Chang left his job at Sylvania Semiconductor and joined Texas Instruments as an engineering manager. It was his second job out of college. At the time, TI was growing rapidly, and Chang was assigned a problematic production line for a new germanium transistor for TI's (then) most important customer – IBM. How problematic? Well, when Chang started on it, the line had a miserable yield of 0%.

Chang spent four months working on the line, eventually getting yields up to 25-30%. This turned out to be twice as good as IBM's own production line, which meant that the transistor was a huge win – and a very profitable product – for the entire company.

Chang was put on the fast track. TI sponsored a PhD at Stanford, offering to pay his salary for the entire duration of the program. Chang eventually spent two and a half years on his doctorate before returning to TI. Three years later, he was promoted to become general manager of TI's integrated circuits (IC) department. And there he left a permanent mark on the semiconductor industry.

At the time, the pricing model for ICs was simple. Since semiconductor manufacturing had high capex, manufacturers would charge their customers high prices to recoup their large upfront costs. Chang knew from painful experience that it took a certain amount of fiddling to get yields up – and therefore unit costs down –

for every production line. Semiconductor manufacturing was a fickle thing: you had to seek out and remove dust contamination, cycle out or repair defective equipment, identify and work out chemical impurities and so on. Chang had noticed that this fixing process could be sped up if the production line was operating at max capacity, allowing his staff to learn and iterate as fast as possible. The problem was, if TI continued to charge high prices for its semiconductors, customer demand and subsequent production capacity would be limited – slowing down the aforementioned improvement process.

Chang hired a then little-known outfit called the Boston Consulting Group (BCG) to crunch the numbers for him, in order to justify an alternative pricing model. This eventually came to be known as 'learning curve pricing' (in Chang's own words) or 'experience curve pricing' (in BCG's words – BCG co-opted this work and tried to brand it as a BCG-specific thing).

The way it worked was as follows: TI would price their chips way below initial costs, and therefore below prevailing market price. This was considered controversial at the time. As a result, the company would aggressively expand their market share for that particular chip. This high demand meant that production lines could be run at max capacity, driving down the time necessary to bring yields up. The pricing model also had the nice side effect of pressuring competitors to lower their prices, at the risk of losing market share – which many competitors resisted.

TI then went as far as to reduce their prices every quarter, even if their customers didn't demand it. Chang would later state that his strategy was to "sow despair in the minds of my opponents". In an interview with Commonwealth Magazine in 2021, Chang said, "In the semiconductor business, we always asked for profit margins in excess of 50% (...) when even Texas Instruments could only achieve a profit margin of 40% by asking the lowest price, they knew they (the competitors) had to get out of the business."

Learning curve pricing worked because of the nature of semiconductors *as a product*. Unlike, say, products like personal computers or cars, IC process nodes that were no longer leading edge still found uses in lower-end, cheaper items like microwaves or television sets. So semiconductor manufacturers could run their lines for as long as possible to recoup their initial costs and generate profits – which worked in favour of the manufacturer with the largest market share.

With the learning curve pricing model, TI began to beat everyone else in the semiconductor industry. It eventually became the market leader of the era. On the back of this success, Chang was promoted to VP of the entire semiconductor business – just one level below the CEO. And in 1972, he became the leading candidate to become the next chief executive.

The question, of course, is what to learn from this?

TI began applying learning curve pricing to a whole host of product lines. They applied it, for instance, to the 1972 launch of their personal calculator. Here, the competitors in the calculator market were more determined – leading to a price war and an eventual $16 million loss for TI in Q2 of 1975. Undeterred, TI launched their new electronic digital watch for a mere $19.95 a year later. Again, the low prices allowed TI to capture most of the digital watch market and drive out their higher-priced competitors like Bowmar and Ness Time. When these competitors went bankrupt, TI announced that they were the leading supplier of digital watches, with an annual production of 18 million units. A little more than a year later, TI further lowered the price of the watch to $9.95.

Unfortunately, the logic that underpinned the pricing curve model didn't seem to map to the consumer products division. TI's low prices evaporated the already low profit margins in the digital watch division, and stronger competitors learned to follow suit. A race to the bottom had started. In 1978, Commodore introduced an LCD watch with prices starting at $7.95. Chang was transferred from the semiconductor business to become VP of consumer products around the same time.

It may seem a little odd that Chang was transferred from the *semiconductor* business – one of TI's most important business units – to consumer products, an underperforming division. The most plausible reason for the transfer was that Chang was passed over for the CEO job. We can never really say for sure why this happened – perhaps it was because he was an immigrant, or perhaps it was because he was ethnically Chinese. Whichever case it was, Chang found himself in the relatively new consumer products division in 1978, with the clear expectation that he would

be able to fix it the way he had worked magic with semiconductors the decade before.

Two and half years later, the consumer products division was still underperforming. In his oral history interview, Chang said that the one highlight during his time there was the 'Speak & Spell' educational product, which contained the first chip-based speech synthesiser; he called it "the best product that the TI consumer division ever did." But everything else went pretty horribly.

TI shut down its once-dominant watch division in 1981, due to their failure to anticipate demand for always-on LCD watches and their inability to compete against even lower-priced Asian imports. When consumer demand shifted to LCD watches, TI's watches went from 'a distinctive symbol of status to a faceless commodity'. The company was thus saddled with a large and essentially worthless inventory of watches as consumer demand collapsed. When they closed their watch division, almost 3000 employees were laid off.

Chang was moved to a staff job in 1981 – the 'head of quality and people effectiveness'. He still had a Senior Vice President title, but was effectively put out to pasture. Years later, while reflecting on the move to consumer products, Chang said that the shift was motivated by TI CEO Mark Shepherd's belief that a good manager could manage *anything*. In retrospect, Chang said: "I think he was wrong. I found the consumer business to be very different. The customer set – completely different. The market – completely different. And what you need to get ahead in that business is different, too. In the semiconductor business, it's just technology and costs; in consumer, technology helps, but it's also the appeal to consumers, which is a nebulous thing."

Two years later, in 1983, Chang quit. He was a legend in the semiconductor industry, and had accomplished much for TI, but his career had simply hit a dead end. He was 52 years old.

Three years later he would take over the Taiwanese research organisation that he would eventually turn into TSMC — Asia's most valuable company as of 2022, and certainly one of the most important companies in the world today.

But that's a story for another time.

The Perils of Learning Lessons from History

What lessons can we learn from Morris Chang's story?

A few weeks ago, Ben Gilbert and David Rosenthal of the Acquired podcast did an [episode](#) summarising all the lessons they'd learnt across 200 episodes of the podcast. They had the following to say about Morris Chang:

The other meaning of 'it's never too late', folks who are viewing the video here in the auditorium will notice we have not Marc Andreessen on this slide, but Dr. Morris Chang, the founder of TSMC. This is I think the other lesson that I've really taken from Acquired is in this vein, which is that Morris Chang was 56 years old when he founded TSMC. TSMC is today, I believe, the 11th most valuable company in the world. It's so easy. The flip side of the coin of there's always another generation, there's always another wave.

Ben: We should say that it maybe the thing keeping geopolitical tensions at rest. It may be the force that nobody wants to destabilize, and therefore, we have peace. It's not just a company.

David: But it's easy to think, if you listen to Marc or that there's always another wave, that's for young people. Like it's Steve Jobs, it's Mark Zuckerberg, it's Vitalik Buterin. It's these young kids who get these new waves of technology. ***The reality is that's just not true. It's just a mindset. You***

have to be willing to dive in and do it. You can do it at 56 years old and still build the 11th most valuable company in the world *(emphasis added).*

The problem with this take, of course, is that it's not entirely true. Sure, Morris Chang *did* go on to start the 11th most valuable company in the world. But he was also a legend in the semiconductor industry when he left TI. He was a legend because of what he did – he invented learning curve pricing, he led TI to new heights of industry dominance, and throughout his entire career he displayed a special flair for dealing with the nuts and bolts of semiconductor manufacturing. So Chang starting TSMC and turning it into the most valuable company in Asia is a slightly different thing than "oh, you can become super successful even if you're old if you just take a stab and go after things, so long as you have the right mindset."

Speaking more generally, though, this has always been the tricky thing about learning from history. History is context dependent. For instance, read the following list of plausible lessons from Morris Chang's story, and gauge how credible they are:

1. Good managers cannot 'manage anything'. Specialisation counts.
2. When you get assigned to save a different department, this may be an opportunity to shine … or a way to get rid of you and put you out to pasture.
3. Competitive advantage comes from creation – from discovering things that are under-appreciated by your competitors.
4. Learning curve pricing works better when you're not subject to changing consumer sentiment. (Or perhaps the more general form of this lesson is that scale advantages work better when you're not subject to abrupt changes in demand).

5. Starting a new company in your 50s is ok if you have an edge in your particular field.

You might nod to some of these lessons, and wrinkle your nose at others. But you might be surprised to learn that *I* don't think any of these lessons are particularly good. Whenever I read a contrived list of lessons that are drawn from some historical context, I play a game in my head. The game is simple. For every lesson that you see, append "… except when it doesn't" to the end. So:

1. Good managers cannot 'manage anything'. Specialisation counts … *except when it doesn't.*
2. When you get assigned to save a different department, this may be an opportunity to shine … or a way to get rid of you and put you out to pasture … *except when it's not either of these things.*
3. Learning curve pricing works better when you're not subject to changing consumer sentiment … *except when it doesn't.* (Consider: are there examples of other industries where learning curve pricing might work in a more consumer facing product segment?)
4. Starting a new company in your 50s is ok – so long as you have a competitive advantage. *Except* when you do have a competitive advantage, and don't succeed anyway because you're too tired for this game and can't stick it out long enough to win, or the competitive dynamics in your industry don't allow you to dominate, or a hundred other things that may be variable when you're building a company *that isn't TSMC.*

In fact, the only lesson that I think might pass the "… except when it doesn't" test is the third one – that competitive advantage comes from creation, since it *is* true that competitive advantage comes from uncovering things that are undervalued by your competitors.

But my broader point still stands: learning narrow lessons from history is extremely risky because things that are true in one specific context might not be true in a different context – even a *slightly* different context. This is what the "except when it doesn't" game uncovers, and it's what makes learning history so difficult in the first place.

The Age Old Question About Learning From History

Debates about history and historiography are as old as the field of history itself. There are commonly two arguments made against reading history. And there are two common arguments for it.

I think a lot about these arguments, given that I spend a fair bit of time reading business biography. I happen to think that reading history is useful, but a couple hundred books in, I'm also starting to believe that they're useful in a different way than you might expect.

I'll begin with the arguments for reading history. These are the kinds of things you'd expect writers of popular history to say, or that you'll expect to hear from folk who've read a great number of books. It is, in fact, an argument that *I've* made, when I wrote *The Land And Expand Strategy for Reading*; I argued that it was useful to read narrative *before* getting into the core of a topic.

The first broad argument for reading history is that you'd be able to place current events in their proper historical context. As a recent example, the 2021 US Capitol riots may seem horrible and unprecedented … until you read histories of the American Civil War. (An American friend pointed out to me that things are bad today … but perhaps not as bad as the lead up to the Civil War. And I found I had to agree. I'd read *The Metaphysical Club* for unrelated reasons a few months be-

fore, which covered the lives of four American thinkers as they went through the war, and the ways in which the war influenced their philosophy. The polarisation described in that book was a shock).

Why is this useful? Well, the basic idea is that if you can place current events in their proper context, then you'd be better calibrated to respond to them. The person who has read history would know not to overreact to certain events; the person who hasn't is seeing everything for the very first time.

There's an interesting variation of this first argument, one that I first heard in a panel talk by Singapore historian [Wang Gungwu](#) – one of the foremost authorities on the Chinese diaspora. Professor Wang argued that history was usually amongst the first things to be weaponised in the event of a war. He gave a number of historical examples, before making the case that a good understanding of history was necessary to resist the narrative machinations of its belligerents. Professor Wang was speaking in the context of a theoretical Sino-Japanese war, but it was clear to everyone present – in Chinese-majority and China-influenced Singapore, stuck right between US-China Great Power relations – that narrative manipulation was something to be reckoned with, from both powers, in the near-future.

Professor Wang then [framed the rest of his talk](#) by saying that 'historians may make predictions "just for fun" to see if things could have been done a different way. "Unfortunately," he added, "there are many, many examples I can think of where we don't learn [from history]".'

Wang's frame – and the underlying assumptions behind that last statement – leads us to a second argument. There is a widespread belief that reading history will allow you to 'learn the lessons of history'. The strong form of this argument goes something like "if you read history, you'll learn important things … like not to fight a land war in Asia, or not to invade Russia during the winter." The practical way this argument expresses itself is in the belief, say, that the Fall of Rome occurred for a dozen proximate reasons; that historians uncover new reasons as the decades

pass, and that if you read enough of these books and pore over enough of these lessons you may triangulate towards something close to the truth. If you get to this truth, the argument goes, you will be able to 'avoid the mistakes of your forebears.'

This form of the argument is considered 'strong' because it assumes you can apply the lessons *directly*. On the face of it, this is a pretty remarkable claim, since the lessons of Ancient Rome may seem a little removed from the modern nation states of today.

In response, some folk back into the weak form of the argument. The weak form is what you get when you hear someone say "history doesn't repeat itself, but it rhymes" – that is, perhaps the lessons of history are too specific to a particular context to generalise, but that some … similarity is consistent over time. I have some sympathy for this view – after all, if you read ten histories of an industry and find that the companies within it all succeed or fail in broadly similar ways, then perhaps there is some information there that is worth taking away with you.

The underlying assumption behind both the strong and the weak forms of this argument is that you *can* learn lessons from history. And to make things clear, I think this is true. I just don't think the lessons are anything like what we think lessons should look like.

The reason I think this way is because I'm sympathetic to the two criticisms most typically levelled against learning from history. To summarise crudely, the two arguments go something like:

1. Everything in life is path dependent. A lucky break early in a person's career makes all their latter successes easier; a lucky decision early on in a company's life makes their latter projects more likely to succeed. You can't replicate a company's success, nor learn from their failures, if you don't also replicate their path. And it is impossible to replicate their path. As a throwaway example, think back to the legendary, non-exclu-

[sive deal](#) IBM made with Microsoft for MS-DOS in 1980 – which allowed Microsoft to sell DOS to every other PC-compatible manufacturer. This deal is often described as "IBM's great mistake". Had they not done the deal, Microsoft would have a had very different set of opportunities in the ensuing decade. But IBM *did* do the deal with Microsoft, and that, in turn, set the stage for Microsoft's dominance in operating systems for years afterwards. It is not very useful to learn the lessons of Microsoft's history when much of what they accomplished hinged on chance events and idiosyncratic decisions in their past.

2. The second argument against reading history is even more harsh. The argument is that accountings of history are not accurate, and can *never* be accurate, thanks to the [narrative fallacy](#). By its nature, narrative necessarily compresses reality into some coherent tale, meaning that accountings of history tend to overemphasise intention and action, instead of the more plausible 'random actions by actors in a [complex adaptive system](#), conspiring to create unexpected results'. This is a roundabout way of saying that whatever histories you read are most likely wrong – and not even a good representation of reality.

The real trick of reading history, it seems to me, is to find a way to read for things that are idempotent across time. You ideally want ideas or takeaways that generalise properly.

It's just a pity that it's so hard to do so.

How to Read History?

So: alright, you might think. Give it to me. How do you propose we read history?

In a sentence: I think we should read history for **concept instantiations, not lessons.**

Longtime readers would know this is an idea that falls out of [Cognitive Flexibility Theory](), a theory of adaptive expertise in ill-structured domains, which I've covered [multiple]() [times]() over the past few months. But *what does this actually mean?* What does it actually look like when you're reading history for concept instantiations, instead of narrow, context-constrained lessons?

Well, consider the following set of 'lessons':

- Learning economies work by increasing production volume in order to drive down costs. The TI story is the story of its discovery — or more specifically, the story of how it came to be weaponised as a competitive advantage. In other words, *this is an example of what learning economies looks like when employed as a competitive strategy.*
- In [*7 Powers*]() Hamilton Helmer asserts that all competitive advantage comes from invention. Chang's story at TI *is an example of what that looks like in practice.*
- You may be put out to pasture for weird political reasons, and this is one way that can happen. But, more importantly —
- Morris Chang went to MIT, finished his PhD at Stanford in two-and-a-half years, invented learning curve pricing (which is still used in the semiconductor industry today), and in the process led an entire company division to market leadership, over the course of 25 years at Texas Instruments. He was *still* put out to pasture at age 52. So if it can happen to *him*, it shouldn't be too surprising if it happens to *you*. By which I mean: you shouldn't be too shocked if you find yourself in a similar situation; Chang's story tells you that getting dumped after a long, successful ca-

reer is within the span of plausible career outcomes – you know this because it's *happened before.*

What makes these 'lessons' different from the ones I talked about earlier?

For starters, notice that each of the 'lessons' are structured as "this is a real world example of some concept or idea", instead of a more general "you should do X" or "when Y happens, you should watch out for Z". Structuring lessons in this 'concept instantiation' form makes them more constrained, but also more useful. They are more useful because they allow you to recognise similar situations as they occur to you in your life, in other words it teaches you to see, *without* forcing you into narrow, actionable recommendations.

The truth is that generalisable lessons of the form "you should do X" tend to fall flat when applied to real world situations – because the situations you'll encounter will *always* be different from the situations you read about in history. There's an even worse tendency that I've noticed, where people read a little history and then attempt to turn it into a coherent framework in their heads. The failure mode is that the [framework then acts as a blinker](#) – it slows down their learning, and blinds them to other cases that might not fit into the neat structure they've spuriously drawn up to guide their actions.

I'm coming around to the idea that you should *not* bother with generalisable lessons – or at least, to be extremely conservative when you attempt to do so. Instead, you want to think "ok, this is an instance of <concept>, and this is one additional way this can show up in the real world." To reiterate one of the central claims of CFT: *experts in ill-structured domains reason by comparison to fragments from prior cases they've seen – one implication is that concepts are represented not as abstract principles in their heads, but a cluster of real world cases that serve as prototypes.*

The goal of reading from history, then, is to expand the set of prototypes in your head.

Treating history in this manner sidesteps many of the problems we've discussed earlier. You don't have to worry about path dependence, for instance, since you're not drawing tight lessons from specific scenarios. Nor do you have to care as much about the narrative fallacy – if you have enough samples of the thing in action, your brain will be able to pattern match against the similarities across cases, never you mind that retellings of each case may be inherently flawed. When you're hunting for concept instantiations, the criticisms of historiography are a wash; they fall to the wayside.

Let's return to Morris Chang's story. One of the biggest 'lessons' I took away from the entire first arc of his career is that "*oh, this is what the [bamboo ceiling](#) looks like*". I bring this up not because it is applicable to me – I am an Asian of Chinese descent, but I am not based in the US; Asians in Asia do not face a bamboo ceiling the way that Asian Americans might. Instead, I bring this up because I want to map it to an experience that I think all of us might have had.

How many times have you had an experience where you then say, perhaps to a friend, perhaps over drinks: "oh, I always knew X (e.g. 'the bamboo ceiling') was a thing, but I never really thought deeply about it *until it happened to me*."

To which the person you're talking to goes, "Oh yeah, that can happen. Have you heard about what happened to Bob?"

"No, what happened?"

"Well, arguably his story is crazier than yours …"

And then you go on to listen to some narrative of what Bob went through, and you file it away in your head, next to your own personal experience of the thing.

I bring this up because this is what *learning from concept instantiation looks like*. The only difference is that, when you read case studies with intentionality, you're not waiting for something to hit you over the head, and you're not reliant on

your friends to tell you about actual cases that have happened to them or to people they know.

Instead, you're drawing from the collective experience of people you don't know, doing things you can't imagine, across the entire arc of time.

You are, in effect, learning from history.

'Strong Opinions Weakly Held' Doesn't Work That Well

There's a famous thinking framework by 'futurist', 'forecaster', and scenario consultant Paul Saffo called 'strong opinions, weakly held'. The phrase itself became popular in tech circles in the 2010s — I remember reading about it on Hacker News or a16z.com or one of those thinky tech blogs around the period. It's still rather popular today.

Saffo's framework — laid out in his original [2008 blog post](#) — goes like this:

I have found that the fastest way to an effective forecast is often through a sequence of lousy forecasts. Instead of withholding judgment until an exhaustive search for data is complete, I will force myself to make a tentative forecast based on the information available, and then systematically tear it apart, using the insights gained to guide my search for further indicators and information. Iterate the process a few times, and it is surprising how quickly one can get to a useful forecast.

Since the mid-1980s, my mantra for this process is "strong opinions, weakly held." Allow your intuition to guide you to a conclusion, no matter how imperfect — this is the "strong opinion" part. Then – and this is the "weakly held" part – prove yourself wrong. Engage in creative doubt. Look for information that doesn't fit, or indicators that pointing in an entirely different direction. Eventually your intuition will kick in and a new hypothesis will emerge out of the rubble, ready to be ruthlessly torn apart once again.

You will be surprised by how quickly the sequence of faulty forecasts will deliver you to a useful result.

This process is equally useful for evaluating an already-final forecast in the face of new information. It sensitizes one to the weak signals of changes coming over the horizon and keeps the hapless forecaster from becoming so attached to their model that reality intrudes too late to make a difference.

More generally, "strong opinions weakly held" is often a useful default perspective to adopt in the face of any issue fraught with high levels of uncertainty, whether one is venturing a forecast or not. Try it at a cocktail party the next time a controversial topic comes up; it is an elegant way to discover new insights — and duck that tedious bore who loudly knows nothing but won't change their mind!

On the face of it, it all sounds very reasonable and smart. And 'strong opinions weakly held' is such a catchy phrase — which probably explains its popularity!

The only problem with it is that it doesn't seem to work that well.

Swimming Upstream Against the Architecture Of The Mind

How do I know that it doesn't work that well? I know this because I've tried. I tried to use Saffo's framework in the years between 2013 and 2016, and when I was running my previous company I attempted it with my boss, whenever we convened to [discuss company strategy](#).

Eventually I read Phillip Tetlock's *[Superforecasting](#)*, and then I gave up on 'Strong Opinions, Weakly Held'.

Why does the framework not work very well? From experience, Saffo's approach fails in two ways.

The first way is if the person *hasn't* read Saffo's original post. This is, to be fair, most of us – Saffo's original idea is so quotable it has turned into a memetic phenomenon, and I've seen it cited in fields far outside tech. In such cases, the failure mode is that 'Strong Opinions, Weakly Held' turns into 'Strong Opinions, Justified Loudly, Until Evidence Indicates Otherwise, At Which Point You Invoke It To Protect Your Ass."

In simpler terms, 'strong opinions, weakly held' sometimes becomes a license to hold on to a bad opinion strongly, with downside protection, against the spirit and intent of Saffo's original framework.

Now, you might say that this is through no fault of Saffo's, and is instead the problem of popularity. But my response is that if an idea has certain affordances, and people seem to always grab onto those affordances and abuse the idea in the exact same ways, then *perhaps you shouldn't use the idea in the first place*. This is especially true – as we're about to see – if there are better ideas out there.

The second form of failure is if the person has taken the time to look up the original intention of the phrase. In this situation, the failure mode is when you attempt to integrate new information into your judgment. Saffo's framework offers no way for us to do this.

Here's an example. Let's say that you've decided, along with your boss, to build a particular type of product for a particular subsection of the self-service checkout market. You both come to the opinion that this subsection is the best entry-point to the industry: it is relatively lucrative, and you think that it is the easiest customer segment to service.

What happens to your opinion when you *slowly* discover that the subsegment is overcrowded? Of course, you don't find out immediately – what happens instead is that you spot little hints, spread over the course of a couple of months, that many

competitors are entering the market at the same time. These are tiny things like competitor brochures lying in the corner table of a client's office, or pronouncements by industry groups that "they are looking to engage vendors for large deployments", and then much later, clearer evidence in the form of increased competition in deals.

"Well," I can hear you say, "'Strong opinions weakly held' means that you should change your opinion when you encounter these tiny hints!"

But at which point do you change your mind? At which point do you switch away from your strong opinion? At which point do you think that it's time to reconsider your approach?

The problem, of course, is that *this is not how the human brain works.*

Both failure mode 1 and failure mode 2 stem from the same tension. It's easy to have strong opinions and hold on to them strongly. It's easy to have weak opinions and hold on to them weakly. But it is quite difficult for the human mind to vacillate between one strong opinion to another.

I don't mean to say that people *can't* do this – only that it is very difficult to do so. For instance, Steve Jobs is famous for arguing against one position or another, only to decide that you were right, and then come back a month later holding exactly your opinion, as if it were his all along.

But most people aren't like Jobs. Psychologist Amos Tversky used to joke that by default, human brains fall back to "yes I believe that, no I don't believe that, and maybe" – a three-dial setting when it comes to uncertainty. People then hold on to their opinion for as long as their internal narratives allow them to. Saffo's thinking framework implies that you sit in 'yes I believe that' territory, and then rapidly switch away to 'maybe' or to 'no', depending on the information you receive.

Perhaps you may – like Jobs! – be able to do this. But if you are like most people, the attempt will feel a lot like whiplash.

So, you might ask, what to do instead?

Use Probability as an Expression of Confidence

The gentler answer lies in *Superforecasting.* In the book, Tetlock presents an analytical framework that is easier to use than Saffo's, while achieving many of the same goals.

1. When forming an opinion, phrase it in a way that is very clear, and may be verified by a particular date.
2. Then state the probability you are confident that it is correct.

For instance, you may say "I believe that Tesla will go bankrupt by 31 December 2021, and I am about 76% confident that this is the case." Or you can be slightly sloppier with the technique – with my boss, I would say: "I think this subsegment is a good market to enter, and I think we would know if this is true within four months. I believe this on the order of 70% ish. Let's check back in September."

(My boss was an ex-investment banker, so he took to this like a duck to water.)

Tetlock's stated technique was developed in the context of a geopolitical forecasting tournament called the Good Judgment Project. In 2016, when I read *Superforecasting* for the first time, I remember thinking that geopolitical forecasting wasn't particularly relevant to my job running an engineering office in Vietnam. But I also glommed onto the book's [ideas around analysis](#), because it was too attractive to ignore.

The truth is that Tetlock's ideas are not unique to his research group. Annie Duke's [*Thinking in Bets*](#) proposes the same approach, but drawn from poker, and the 'rationalist' community [LessWrong](#) has long-held norms around stating the confidence of their opinions.

More importantly, Duke and LessWrong have both discovered that the *fastest* way to provoke such nuanced thinking is to ask: "Are you willing to bet on that? What odds would you take, and how much?"

You'd be surprised by how effective this question is.

Why is it so effective? Why does it succeed where 'Strong Opinions, Weakly Held' does not?

The answer lies in the 'strong opinion' portion of the phrase. First: by forcing you to state your opinion as a probability judgment – that is, a percentage – you are forced to calibrate the strength of your belief. This makes it easier to move away from it. In other words, you are forced to let go of the 'yes, no, maybe' dial in your head.

Second: by framing it as a bet, you suddenly have skin in the game, and are motivated to get things right.

Of course, you don't actually *have* to bet – you can merely propose the bet as a thinking frame. Later, as new information trickles in, you are allowed to update the % confidence you have in your belief.

Revisiting The Hierarchy of Practical Evidence

I have one final point to make about this approach.

Long term readers of this blog would know that my shtick is "apply a technique to my career or to my life, over the period of a couple of months, and report on its efficacy." Over time, I've noticed that techniques are more likely to be effective when they come from believable practitioners. This is what led to my Hierarchy of Practical Evidence.

Saffo's and Tetlock's ideas are drawn from the domain of forecasting. But this post is about thinking, not forecasting; I'm only confident to recommend one over the other because I've had enough experience with both as analytical tools.

But it's worth noting that Saffo isn't particularly believable as a forecaster either.

For much of *Superforecasting*, Tetlock rails against professional 'forecasters', who make vague verbiage statements and issue long form narratives about the future. These forecasters are always able to worm out of a bad forecast, because their pronouncements are carefully worded to provide plausible deniability.

As I was writing this piece, I skimmed through the book as reference, and was surprised to learn that Tetlock had met up with Saffo over the Good Judgment Project, and had written about the episode in the book. Unfortunately, Saffo dismisses Tetlock's research with a single remark:

> *In the spring of 2013 I met with Paul Saffo, a Silicon Valley futurist and scenario consultant. Another unnerving crisis was brewing on the Korean peninsula, so when I sketched the forecasting tournament for Saffo, I mentioned a question IARPA had asked: Will North Korea "attempt to launch a multistage rocket between 7 January 2013 and 1 September 2013?" Saffo thought it was trivial. A few colonels in the Pentagon might be interested, he said, but it's not the question most people would ask. "The more fundamental question is 'How does this all turn out?' " he said. "That's a much more challenging question."*
>
> *So we confront a dilemma. What matters is the big question, but the big question can't be scored. The little question doesn't matter but it can be scored, so the IARPA tournament went with it. You could say we were so hell-bent on looking scientific that we counted what doesn't count.*

Tetlock goes on to defend his approach:

That is unfair. The questions in the tournament had been screened by experts to be both difficult and relevant to active problems on the desks of intelligence analysts. But it is fair to say these questions are more narrowly focused than the big questions we would all love to answer, like "How does this all turn out?" Do we really have to choose between posing big and important questions that can't be scored or small and less important questions that can be? That's unsatisfying. But there is a way out of the box.

Implicit within Paul Saffo's "How does this all turn out?" question were the recent events that had worsened the conflict on the Korean peninsula. North Korea launched a rocket, in violation of a UN Security Council resolution. It conducted a new nuclear test. It renounced the 1953 armistice with South Korea. It launched a cyber attack on South Korea, severed the hotline between the two governments, and threatened a nuclear attack on the United States. Seen that way, it's obvious that the big question is composed of many small questions. One is "Will North Korea test a rocket?" If it does, it will escalate the conflict a little. If it doesn't, it could cool things down a little. That one tiny question doesn't nail down the big question, but it does contribute a little insight. And if we ask many tiny-but-pertinent questions, we can close in on an answer for the big question. Will North Korea conduct another nuclear test? Will it rebuff diplomatic talks on its nuclear program? Will it fire artillery at South Korea? Will a North Korean ship fire on a South Korean ship? The answers are cumulative. The more yeses, the likelier the answer to the big question is "This is going to end badly."

I call this Bayesian question clustering because of its family resemblance to the Bayesian updating discussed in chapter 7. Another way to think of it is to imagine a painter using the technique called pointillism. It consists of dabbing tiny dots on the canvas, nothing more. Each dot alone

adds little. But as the dots collect, patterns emerge. With enough dots, an artist can produce anything from a vivid portrait to a sweeping landscape.

There were question clusters in the IARPA tournament, but they arose more as a consequence of events than a diagnostic strategy. In future research, I want to develop the concept and see how effectively we can answer unscorable "big questions" with clusters of little ones.

Saffo's business is in selling stories about the future to businesses and organisations. He [teaches](#) his approach to business students, who would presumably go on to do the same thing. Tetlock's job is in pinning forecasters down on their performance, and evaluating them quantitatively using something called a Brier score. His techniques are now used in the intelligence community.

These are two different worlds, with two different standards for truth.

You decide which one is more useful.

Closing Thoughts

So let's wrap up.

In my experience, 'strong opinions, weakly held' is difficult to put into practice. Most people who try will either:

1. Use it as downside-protection to justify their strongly-held bad opinions, or
2. Struggle to shift from one strong opinion to another.

The reason it is difficult is because it works against the grain of the human mind.

So don't bother. The next time you find yourself making a judgment, don't invoke 'strong opinions, weakly held'. Instead, ask: "how much are you willing to bet

on that?" Doing so will jolt people into the types of thinking you want to encourage.

Whether you actually put money down is besides the point; whichever way you approach it, it's still a heck of a lot easier than vacillating between multiple strong opinions.

See also: The Forecasting Series, A Personal Epistemology of Practice.

Believability in Practice

A couple of years ago I wrote about Ray Dalio's [believability](#) metric, which he first articulated in *Principles* (the hardcover edition of the book was published in 2017, but my original experience with the book was a crude PDF version, freely available on Bridgewater's website circa 2011).

It's been a couple of years since I internalised and applied the idea; this essay contains some notes from practice.

Believability, A Recap

The actual technique that Dalio proposed is really simple:

'Believable' people are people who have 1) a record of at least three relevant successes and 2) have great explanations of their approach when probed.

You may evaluate a person's believability on a particular subject by applying this heuristic. Then, when you're interacting with them:

1. If you're talking to a more believable person, suppress your instinct to debate and instead ask questions to understand their approach. This is far more effective in getting to the truth than wasting time debating.

2. You're only allowed to debate someone who has roughly equal believability compared to you.

3. If you're dealing with someone with lower believability, spend the minimum amount of time to see if they have objections that you'd not considered before. Otherwise, don't spend that much time on them.

The technique mostly works as a filter for actionable truths. It's particularly handy if you want to get good at things like writing or marketing, org design or investing, hiring or sales – that is, things that you can *do*. It's less useful for getting at more [other kinds of truth](#).

I think I starting putting believability to practice around 2017, but I think I only really internalised it around 2018 or so. The concept has been *remarkably* useful over the past four years; I've used it as a way to get better advice from better-selected people, as well as to identify books that are more likely to help me acquire the skills I need. (Another way of saying this is that it allowed me to *ignore* advice and *dismiss* books, which is just as important when your goal is to get good at something in a hurry.)

I attribute much of my effectiveness to it.

I'm starting to realise, though, that some of the nuances in this technique are perhaps not obvious – I learnt this when I started sending my [summary of believability](#) to folks, who grokked the concept but then didn't seem to apply it the way I thought they would. This essay is about some of these second-order implications when you've put the idea to practice for a longer period of time.

Why Does Believability Work?

Believability works for two reasons: a common-sense one, and a more interesting, less obvious one.

The common-sense reasoning is pretty obvious: when you want advice for practical skills, you should talk to people who *have* those skills. For instance, if you want advice on swimming, you don't go to someone who has never swum before, you go to an accomplished swimmer instead. For some reason we seem to forget this when we talk about more abstract skills like marketing or investing or business.

The two requirements for believability makes more sense when seen in this light: many domains in life are more probabilistic than swimming, so you'll want at least three successes to rule out luck. You'll also want people to have 'great explanations' when you probe them because otherwise they won't be of much help to you.

The more interesting, less obvious reason that believability works is because reality has a surprising amount of detail. I'm quoting from a famous article by John Salvatier, *which you should read in its entirety*. Salvatier opens with a story about building stairs, and then writes:

> *It's tempting to think 'So what?' and dismiss these details as incidental or specific to stair carpentry. And they are specific to stair carpentry; that's what makes them details. But the existence of a surprising number of meaningful details is not specific to stairs. Surprising detail is a near universal property of getting up close and personal with reality.*
>
> *You can see this everywhere if you look. For example, you've probably had the experience of doing something for the first time, maybe growing vegetables or using a Haskell package for the first time, and being frustrated by how many annoying snags there were. Then you got more practice and then you told yourself 'man, it was so simple all along, I don't know why I had so much trouble'. We run into a fundamental property of the universe and mistake it for a personal failing.*
>
> *If you're a programmer, you might think that the fiddliness of programming is a special feature of programming, but really it's that everything is fiddly, but you only notice the fiddliness when you're new, and in programming you do new things more often.*
>
> *You might think the fiddly detailiness of things is limited to human centric domains, and that physics itself is simple and elegant. That's true in*

some sense – the physical laws themselves tend to be quite simple – but the manifestation of those laws is often complex and counterintuitive.

The point that Salvatier makes is that *everything* is more complex and fiddly than you think. At the end of the piece, Salvatier argues that if you're not aware of this fact, it's likely you'll miss out on some obvious cue in the environment that will then cause you – and other novices – to get stuck.

Why does this matter? Well, it matters once you consider the fact that practical advice has to account for all of this fiddliness – but in a roundabout way: good practical advice nearly *never* provides an exhaustive description of all the fiddliness you will experience. It can't: it would make the advice too long-winded. Instead, good practical advice will tend to focus on the salient features of the skill or the domain, but in a way that will make the fiddliness of reality tractable.

In practice, how this often feels like is something like "Ahh, I didn't get why the advice was phrased that way, but I see now. Ok."

Think about what this means, though. It means that you cannot tell the difference between advice from a believable person and advice from a non-believable person from *examination of the advice alone*. To a novice, advice from a non-believable person will seem just as logical and as reasonable as advice from a more believable person, except for the fact that it will not work. And the reason it will not work (or that it will work less well) is that advice from less believable individuals will either focus on the wrong set of fiddly details, or fail to account for some of the fiddliness of reality.

To put this another way, when you hear the words "I don't see why X can't work …" from a person who isn't yet believable in that domain, *alarm bells should go off in your head*. This person has not tested their ideas against reality, and – worse – they are not likely to know which set of fiddly details are important to account for.

Why Does This Matter?

At this point it's perhaps useful to take a step back and reiterate why I take this idea so seriously.

I've just given you an explanation of the differences in advice from believable people and non-believable people. I have not named any names, because I don't want to call people out in this piece. But consider, for a moment, the kinds of experiences I must have had to be able to tell you about such differences.

If you take advice from non-believable people, you'll often waste *months* chasing after something with the wrong frame. Practice takes time. Mastery takes years. You don't want to burn months needlessly if you don't have to.

I take believability seriously because I've burnt months in the past in the pursuit of suboptimal advice. I am now ruthless when it comes to evaluating advice, because I do not want to burn months in the future.

One way I think about this is that the most dangerous advice from non-believable people tend to be '[perfectly rational, logically constructed, and not really wrong – but not as useful or as powerful as some other framing](#)'. The danger isn't that you receive advice that just appears to be stupid, or wrong – if that were the case, you would simply reject it. No, the danger is that you receive advice that slows you down – while appearing perfectly reasonable on the surface.

Ad Hominem

An obvious objection to believability is that it is a form of [ad hominem](#). Per Wikipedia, ad hominem is a 'rhetorical strategy where the speaker attacks the character, motive, or some other attribute of the person making an argument rather than attacking the substance of the argument itself.'

But the Wikipedia page also includes this rejoinder:

Valid ad hominem arguments occur in informal logic, where the person making the argument relies on arguments from authority such as testimony, expertise, or on a selective presentation of information supporting the position they are advocating. In this case, counter-arguments may be made that the target is dishonest, lacks the claimed expertise, or has a conflict of interest.

In practice, believability has interesting properties that pure ad-hominem doesn't. For instance, there have been multiple instances where – having determined that a junior person was more believable than I was in some sub-skill like copywriting or software design – I just shut up and set aside my views to listen. I've also shushed more senior people, when it became clear to me they were less believable in that particular sub-skill than the junior employee.

Survivorship Bias

The next most common objection to believability is 'how about survivorship bias?' The reasoning goes something like this: if you focus on those with at least three successes, you may well include individuals who have succeeded by luck. Another way to argue this is to ask: what about all those who have attempted to put believable advice to practice, but failed?

As with the earlier analogy on swimming advice, there is a useful thought exercise that I like to use, to remind myself of the nuances of the technique in practice. Let's imagine that you and I are recreational Judo players, and you see me successfully perform a complicated throw on at least three separate occasions, against three different opponents. Do you write this off as survivorship bias? Probably not.

In fact, you might ask me to teach you the throw – and I would gladly do so, though I might warn you that it took me three years to learn to use it with any amount of success. (This is quite common, especially with more complicated techniques in Judo).

But now let's say that you run some analysis on international Judo competition, and you find out that – unsurprisingly! – the throw is rarely used at the highest levels of competition. The few competitors who use it do so sparingly. How does this change your desire to learn the technique?

This analogy captures many properties of skill acquisition:

1. Just because a practitioner is able to use a technique doesn't mean that you would also be able to – for instance, perhaps the practitioner has longer limbs, or a longer torso, that make the technique impossible to do for any other body shape. (In marketing, investing, or business, the equivalent analogy here is that certain approaches demand certain additional properties to work – for instance, you might need a certain temperament if you are doing certain styles of investing, or you might require full support from company leadership if you are running certain types of go-to-market playbooks. Or perhaps the approach you take requires the practitioner to have an equivalently high level of skill at some unrelated domain – say, organisational politics, or org design; these serve as confounding variables that affect one's ability to copy the approach and have it work in your specific context).

2. Just because a practitioner is able to use the technique in one context (the club level) doesn't mean that they would be able to do so in high-level competition. This is particularly true in sports like Judo, but is broadly true elsewhere – certain techniques or approaches are harder to learn, with lower probability of success, and so vanish from the high-

est levels of competition where evolutionary pressures are the strongest. (The business analogy here is that certain approaches work for less-competitive industries, or for certain periods of time, but do not work in more competitive industries or at the end of a cycle).

3. This does *not* mean that you cannot learn something from a believable practitioner – it simply means that proof of believability is not a guarantee of technique success.

So how do you take these properties into account? Over the past couple of years, I've settled on two approaches:

First, I think of believability as a more powerful negative filter than it is a positive one – in other words, it is more rigorous when removing individuals, books, or advice from consideration. You should pay the bare minimum of attention to less believable advice, and then look elsewhere.

On the flip side, if you get advice backed by a track record of at least three successes, you should probably treat it as an existence proof that the advice has worked and that the believable person has something to teach you; what you *shouldn't* do is to treat it as '*the* truth', since you still need to verify if it works for you in practice.

Second, I am very specific when it comes to evaluating believability. I've alluded to this in the example about copywriting, above – I am perfectly happy to shut up and learn from a more junior person if they are more believable than I am in a particular sub-skill. Consequently, I've found it useful to evaluate believability *at the level of the specific advice that's given* – which might often mean that I take two or three pieces of advice seriously in a conversation, but then discard the rest, depending on the person's believability for each specific piece of advice.

A good example of this is with finance professionals – I am *not* believable when it comes to investing, so I immediately shut up and listen whenever they talk

about finance; on the other hand, I tend to ignore nearly everything that finance folk say about [org design](). From experience, professionals in smaller funds tend not to have grappled with org design problems the way business operators of equivalent org sizes must (mostly because small-to-mid-size businesses tend to be more operationally complex than small-to-mid-size funds).

My point: selection bias is less of a thing when you're evaluating specific advice against the precise context and the context-dependent believability of the person in question, *especially* when you approach things with the assumption that [practice is the bar for truth](). And even with things like 'org design' I am very aware of the limits of my believability – I've never run orgs larger than 50 people, for instance, and even then only successfully in certain contexts; I mostly shut up and listen whenever I meet someone who has scaled larger orgs, or if they have run equivalently-sized orgs in more operationally challenging domains.

Suppress Your Views

The most difficult part about putting believability to practice is actually in the first third of Dalio's protocol.

> *If you're talking to a more believable person, suppress your instinct to debate and instead ask questions to understand their approach. This is far more effective in getting to the truth than wasting time debating.*

It turns out that the 'suppress your instinct to debate' is relatively easy; the hard part is in mentally suppressing your existing models of the domain. To be more precise, the difficult part is that you are not allowed to *interpret the expert's arguments through the lens of what you already know.*

One tricky scenario that you might find yourself in is talking to someone who is demonstrably more believable than you in a particular sub-skill, but then realising that *what they say clashes with your existing mental models of the domain.*

A true commitment to believability means that you have to take them seriously. It also means that you have to *ignore* what you think — because it is likely that you're missing certain cues that only they can see. Dalio's protocol implicitly calls for you to grok the more believable person's worldview — to treat it with respect, to question it and internalise its logic; you may construct actionable tests for the argument later.

This sounds relatively easy to do in theory. In practice, I've found it to be remarkably difficult. What I've seen most people do when put in this situation is that they will take whatever the more believable person says, and then paraphrase it using a frame that they currently understand. But this is stupid. You're not here to interpret the expert's advice through your existing mental models — in fact, you're wasting time since *the odds are good that your mental models are subtly mistaken in the first place*. Instead, you should set aside your own models in an attempt to grapple with the advice on its own terms, using the more believable person's own words, in an attempt to see the world as the more believable person sees it.

I'm afraid that I don't have good examples for you here. The most pernicious instances of this behaviour occur when the practitioner is a journeyman in some domain, and the higher-believability person is more than a few rungs ahead in a particular branch of the skill-tree. This in turn means that any detailed example I give you will require you to be familiar with the skill domain itself. But, if I may describe what I've noticed in such scenarios: what often happens is that the more believable person will say something with subtle implications, and the less believable person will say "ahh, yes, and —" and then *proceed to articulate something that is clearly a function of their current mental model of the domain*, in a way that misses the subtle difference.

A crude, no-good example (that won't make sense unless you have some experience in marketing): if you put two believable marketers together, one from a performance marketing background and one from a brand marketing background, what you'll often find is the performance marketer will completely miss out on the parts of the brand marketer's skill where the brand marketer is able to manipulate low-level levers in a consumer's psychology.

It's not enough to say (to the performance marketer) "you must understand the customer to do brand marketing well" — the performance marketer will *think* that they already understand the customer, since all marketers use some model of the customer's journey in their work. What the performance marketer might not notice is that there are depths in the ideal customer's psychology that the brand marketer is able to plumb (especially with regard to the brand and the brand narrative) and that these depths will be subtle and sound entirely ridiculous when articulated. Unless the performance marketer is willing to set aside what they currently know about marketing to listen carefully, they will likely be blind to much of these nuances.

In practice what's more likely to happen is that the performance marketer will say, of the brand marketer, "god, I can't stand them, they're all full of hot air", and so will be completely useless with brand marketing for years and years afterwards.

I'm not sure if that example made any sense to you, but if it did — well, this sort of interaction is exactly what is difficult about putting believability to practice.

Triangulate Truths

Here's a more interesting question, though: what happens when two equally believable people have different approaches to a particular domain? That is, you go

to two different people and they give you conflicting advice on some decision, and you're confused as to what to do.

We can state this question slightly differently: what happens if a believable practitioner (say, an Olympic medallist) says one thing, but a believable coach (a person with a track record of producing Olympic athletes) says another?

Or what if you read about two savant investors who have totally different approaches to equity investing?

How do you evaluate the competing approaches?

At this point in the essay, it's actually easier to reason about this – each piece of advice you get comes from a particular context, and the successes in the person's history serves as an existence proof for the advice. Therefore: hold each approach equally with a loose grip, but test the one with the more accessible explanation first.

(Or really, test whichever one appeals to you first – it doesn't actually matter that much so long as you're willing to *act*).

The intuition here is that each believable person is likely to see parts of the skill domain that you can't, and so advice that appears conflicting to you as a novice very often turns out to be differing interpretations of the same underlying principles once you've climbed a bit further up the skill tree.

Another implication: you can use the differences in advice from equally believable people as a way to triangulate on those base principles. This is similar to what I've argued in [To Get Good, Go After The Metagame](#) – although in that piece, the primary idea I exploit is the observation that experts operating at some competitive frontier may serve as a north star for self-learning.

My friend Lesley has an interesting argument when it comes to evaluating coach vs athlete: she says that while both may be equivalently believable (the coach has a track record of producing winning teams; the player has a track record of winning); you really do want to pay attention to the person with the better expla-

nation. The way she puts it: "A 2x champion might be a better coach than a 10x champion (or a famous coach) simply because they are better at synthesis and communication."

Which probably explains why Dalio included the clause that believable people should have 'a credible explanation when probed'.

I think Lesley is broadly right. If in doubt, test the more accessible explanation first.

But the important thing, again: you need to *test* ideas, not *argue* about them. The purpose of the believability protocol is to get better faster. Here's to hoping that it helps you do just that.

What's Your Time Preference?

In economics, time preference is the notion that the value of something changes depending on whether you receive it at an earlier date as compared to receiving it a later date. That's a really complicated way to describe something quite intuitive, so here are a few examples.

Let's say that you want to get rich in 10 years. 10 years isn't a lot of time, so you find yourself willing to take more risks in order to hit a higher potential return on your investments – you may invest all of your savings into Bitcoin, for instance, or start a go-big-or-go-home startup.

But if you're, say, 25 years old, and you want to hit five million dollars in net worth by age 60, you have more time on your hands, and may be said to have a 'longer time preference'. This means that you're probably going to invest in very different things – things that may give you a smaller return each year, but are less risky overall. The hope is that you may compound your investments over the 35 years you have left till you hit 60.

Here's another example. Let's say that you visit an Oracle and she tells you that you're going to die next year. We'll assume that the Oracle is 100% correct, and that you know this. Your activities for the next year would look very different from if you *didn't* know you were going to die – you'd probably travel to exotic places, or binge on the best movies and eat at the best restaurants, COVID-19 be damned. Whereas in a normal year, you'd be doing some mix of investing vs harvesting – 'investing' activities are things that would only bear fruit years down the line (like further education, or spending time with your kids), and 'harvesting' activities are

things that would bear fruit today (like cashing out of your employee stock option plan and buying a new Tesla).

The core idea here is that your time preference greatly influences the types of activities that you do. This is a ridiculously simple idea, but it is rather useful when it comes to thinking careers.

Think Four Decades, Not Four Years

A typical career lasts about four decades. I'm assuming that an average person starts work in their mid-20s, and has a career that lasts till their mid 60s. I have friends who believe that they are never going to retire, but even *they* think that they would slow down in their 70s.

(As of this writing, [John Malone](#) and [Barry Diller](#) are 79 years old, and are still active players in business; [Robert Kuok](#) was active even in his 70s; [Anthoni Salim](#) is 71. I'm picking business leaders as an example because running a business is incredibly tiring; my point is that working in your 70s is still possible. Whether you want to do so is another question entirely).

So: 40 years on average. If we believe [the](#) [data](#), most people hit their peak earning years between the ages of 45-55. Earnings then trends downwards as we near retirement age, and peters out as you stop working and withdraw from your savings.

My point is that any type of career strategy should take these numbers into account. Many of us will have a four-decade long career. The peak of our careers will occur during our late 40s to early 50s. And the stages we go through on our way to that peak will be oddly similar, if you've read enough biographies and talked to enough people about their careers.

The way I like to think about it is that a career span is a bit like the stages of a typical [Euro board game](#):

- **The ages of 25 to 35 are the early game.** In most careers – and most Eurogames! – you earn relatively little at the beginning, and you have to grind it out to build an economic engine. (A professor once took me aside, right before I graduated, and told me "Your 20s is going to be hard, career-wise. You're going to be kicked around a bit. Don't let it get you down." It was good advice).

- **The ages of 35 to 55 are the mid-game.** This is when you've mostly figured things out, and you begin entering your peak earning years. In pure Eurogame terms, the mid-game is usually the most interesting part of a game. The majority of the players would have their core point-generating engines going, and the shape of the competition becomes clear. As it happens, I enjoy talking to people at this stage of their careers – those who are at the beginning of the mid-game tend to be the ones who have reached initial mastery, and yet still have the energy to [keep up with the meta](#).

- **The ages of 55 to 65 are the end game.** In a Eurogame, the endgame is when you cash in all your investments and take points. Similarly, in careers, this stage is when you stop investing in new skills, and begin harvesting all the career capital that you've gained over the years. This is the time to take your winnings and enjoy life.

I think about this 40 year time span a lot. When I was in my 20s, one of my friends used to say "In the span of a career, one year isn't much" to talk about career bets; with the benefit of hindsight, I thought he got it absolutely right. But it's been my experience that most people don't think about the 40 year span. I have several friends who judge our peers on their career trajectories based on what's happened

in their 20s; I have other friends who think they're doing brilliantly two or three jobs in. But I'm a lot more conservative when it comes to such evaluations. I know – from reading lots of business biographies, and from talking to lots of people about their work – that many things can happen in the first two decades of a career. Unless you're in the late stage of the mid-game – in the 45-55 age bracket – it's simply too early to tell if you've had a good run of it.

This intuition should be familiar to anyone who's played serious Euro board games. (Or really – any kind of game with a clear early/mid/end game – [Civ](#) counts, as does [Dota](#). If you want a taste of this, get a bunch of friends and play one or two rounds of an entry-level Eurogame – I recommend [Ticket to Ride](#), or [Carcassonne](#), or [Splendor](#).)

Typically, the early-game is when all the players are making their initial bets. Usually these are all investing activities, no harvesting ones; it's unclear how these activities would bear fruit. Similarly, the first decade of most careers are often too shapeless to evaluate properly – some people experiment with random jobs in random places; others do extra degrees. It's difficult to tell what the outcomes would be.

There's also a simpler explanations for this difficulty: if we assume it takes around 10 years to get good at something, and a few more years to faff around and *find* that thing, then having the first 10-15 years of a career appear to be a bit of a wash makes a ton of sense.

If you don't believe me, it's worth calibrating a little and looking at the careers of some successful people at the end of the early game:

- At age 30, Warren Buffett had moved back to his home town and was running seven small investment partnerships, sourced from local ac-quaintances. This was four years after Benjamin Graham closed his part-nership (leaving Buffett without a job), and a year after purchasing his

five-bedroom house in Omaha. He was practically unknown at the national stage.

- At age 30, Barry Diller was Vice President of Development at ABC, and had just invented the concept of the made-for-TV movie. Today, Diller is most known for his stints at Fox Network and with IAC (which owned, amongst other things, Expedia, Vimeo, Ticketmaster and Match.com … and helped create Tinder). But at age 30, in 1972, Diller was a rising broadcast executive; cable was still regarded as a cowboy industry, and the internet wasn't even a thing.

- At age 30, John Malone was Group Vice President of Jerrold Electronics. This was a role he got after consulting for General Instrument while he was at McKinsey – he told Monty Shapiro, then head of GI, that someone was cooking the books at Jerrold, and that "it's going to take a lot of work to turn this around". Shapiro replied "If you're so smart, why don't you come and do it yourself?" Jerrold wasn't the job that made Malone's name; that came two years later when he was offered the job at TCI. The rest of his career at TCI is [history](.).

- At age 30, Robert Kuok was an agricultural trader, running *Kuok Brothers Sdn Bhd*. He was three years away from setting up his first joint venture with Japanese partners, and 18 years away before building the first Shangri-La Hotel.

Yes, the obvious objection to this list is that this is extreme survivorship bias, but that's besides the point; the point I'm making here is that it's difficult to evaluate careers at age 30 – even when you're looking at some of the most successful people on the planet. Nearly all of these people did their most interesting work in the mid-game; for many of them, the mid-game occurred between their 40s and 50s.

Time Preferences in Careers

Thinking about careers in 40 year spans does one other thing: it gives you a way to calibrate the expected return on investment for career decisions.

Some career decisions return you benefits immediately. Think of a high-paying, prestigious job, for instance. Others don't return benefits as quickly. For instance, spending 10 years at early Amazon* – or an equivalently operationally rigorous company – would probably set you up as a first class operator for the rest of your career (30 years!) but would mean that you'll look like an idiot for the first 10 years of your working life.

(*Working at early Amazon was to work long hours, earn lower-than-average pay, with a large chunk of stock comp that didn't do as well compared to the alternatives. Bezos's "we are willing to be misunderstood for long periods of time" is the very definition of a long time preference.)

As with all such things, this is easy to say but difficult to do. Imagine how long 10 years *feels* like. Imagine all the prestige and salary that you're giving up for those 10 years. And imagine your peers buying new houses or cars and moving up in some well-defined career ladder every couple of years during that first decade. Imagine some of them building up personal brands on Twitter or LinkedIn, as a result of that early success.

If you think about careers as a step-wise comparable, then taking a pay cut to build a certain set of skills seems like a bad career move. But if you think about careers as a 40-year-long game, then investing your first decade or two to build up skills and capabilities needed for the next two decades seems like a worthwhile investment.

One way I like to do this is to evaluate careers in 10-year blocks. Given a 40 year time span, each decade for the first three decades should be used to build up for the next one. You can really only stop to harvest in the third and final decade,

when you're in your late 50s (and even then — you might decide to invest for your 60s and 70s; the irony is that if you play your cards right, the compounding effect of your skills and reputation would mean that the most interesting opportunities of your career may come to you in those final decades!)

Thinking this way also means that you would be more suspicious of jobs that don't set you up for the latter decades of your career; you might also be more suspicious of things that confer short-term career advantages. I am rather ambivalent about the concept of personal brand building for this reason — I don't think personal brand building is a great idea, but I also don't think it's a bad one. In the late 2000s, I was part of the early blogosphere; I wrote one of the first web fiction blogs and was a member of the then-prestigious 9rules network. None of the luminaries from that period continue to be as famous today. The ones who have lasted have done so because they were doing interesting, valuable things *outside of their blogging.* What I've learnt from that period is that, for most people, personal brand building takes a toll over a decade-long time span; an easier way to stay interesting and relevant for a 20-30-year period is to build or do useful things, and to harvest the reputation benefits afterwards.

(Investor Brent Beshore says something similar in this podcast (52:29): "you always got to have the go behind the show, and you better make sure you have the go first before you have the show, or the show is not going to matter over a durable period of time, right? And I can think about some specific people in my head that have come and gone in the sort of high-notoriety realm that just didn't have anything to back it up. And they were kind of like the one-trick pony. So I think there's a lot of those dynamics that people have to be aware of. (…) I'm going to use the most famous (example) of them, right? Which is Warren Buffett. If you think about his personal brand over time, when he was 40 years old, hardly anybody knew who he was, right? In Omaha, he was kind of becoming a big deal in his late 30s, when he first started buying the Washington Post (…) I remember the famous

anecdotes like "Who the hell is that guy? Where did he come from? Who is this Warren Buffett?" And only after he sort of was discovered, meaning people had conversations with him and said, "Wow, there's something to this guy. This guy has a spark. There's something unusual. His performance is speaking pretty loudly," did he really start ramping up his personal branding efforts. And oh, by the way, yes, Warren Buffett has personal branding efforts. Don't think for a second the aw-shucks persona isn't coined.")

Time Preference in Business

Time preference is more commonly understood in business and investing.

One of the more obvious applications of the idea is in the competitive advantage that one gains from having investors with longer time preferences. A typical investment fund (be it venture capital or hedge fund) raises money from 'LPs', or limited partners – these are people or institutions who put money into the fund but have no say in the running of things. The investors who *actually* run the fund are called GPs, or general partners. Typically, a fund with LPs with longer time preferences will have a competitive advantage against funds that don't.

Why this is the case? Well, this is rather easy to explain. When things are going well, LPs are typically happy to leave their money with the fund managers. When things are going badly, however, LPs can go to the managers and say "uhh, actually we'd like to take our money out." This can be horrible for the managers – in some cases, it might mean selling off positions in their investments in order to give the exiting LPs the capital they demand. And if you're forced to sell against your will, typically in a downturn, it is likely that you would be taking losses as you sell.

All of this is to say that funds have a competitive advantage if they have LPs who are incredibly patient with their capital, and who have no need to draw down

on investments when times are tough. The best VC funds, for instance, are able to attract ever more patient forms of capital; this in turn means that they are more likely to stay in top decile of funds.

Interestingly enough, this also applies to the LPs themselves.

Time preference was exploited most famously by [David Swensen](#) of the Yale endowment. Swensen realised in the late 80s that university endowments are amongst the most patient forms of capital around; this patience could be turned into a competitive advantage. The basic insight that Swensen exploited was this: liquid markets are more efficient markets, but efficient markets mean that it is difficult to generate higher returns. Therefore, go after illiquid markets instead! Swensen figured this was a natural fit for the endowment: his strategy worked because a) he had a long time preference (illiquid funds mean that you have to stay invested for a long time), b) he was able to cultivate long-term relationships with managers, who c) had a life-long affection for and loyalty to their alma-mater.

Starting in the 80s, Swensen began searching for and investing in managers whom he thought might deliver such returns. In the early years, few such funds existed, which meant that he had to become a 'venture capitalist of venture capitalists'. As of 2019, about 60% of Yale's endowment is allocated to hedge funds, venture capital, and private equity, and the strategy is copied widely. Swensen's track record [speaks for itself](#).

What I find most fascinating, however, is that time preference also applies to businesses. A common observation that people have about startups is that startup timelines are compressed, thanks to the economics of venture capital.

The story goes like this: a typical VC fund lasts 10 years. VCs do not usually invest in new companies beyond the third year of a fund's life, to ensure their latest investments have a chance to return money to LPs by the end of the 10 year period. To [quote](#) Andy Rachleff, formerly of Benchmark Capital:

According to research by William Sahlman at Harvard Business School, 80% of a typical venture capital fund's returns are generated by 20% of its investments. The 20% needs to have some very big wins if it's going to more than cover the large percentage of investments that either go out of business or are sold for a small amount. The only way to have a chance at those big wins is to have a very high hurdle for each prospective investment. Traditionally, the industry rule of thumb has been to look for deals that have the chance to return 10x your money in five years. That works out to an IRR of 58%. Please see the table below to see how returns are affected by time and multiple.

	Return Multiple									
	1.5x	2.0x	3.0x	4.0x	5.0x	6.0x	7.0x	8.0x	9.0x	10.0x
2	22%	41%	73%	100%	124%	145%	165%	183%	200%	216%
3	14%	26%	44%	59%	71%	82%	91%	100%	108%	115%
4	11%	19%	32%	41%	50%	57%	63%	68%	73%	78%
5	8%	15%	25%	32%	38%	43%	48%	52%	55%	58%
6	7%	12%	20%	26%	31%	35%	38%	41%	44%	47%
7	6%	10%	17%	22%	26%	29%	32%	35%	37%	39%
8	5%	9%	15%	19%	22%	25%	28%	30%	32%	33%
9	5%	8%	13%	17%	20%	22%	24%	26%	28%	29%
10	4%	7%	12%	15%	17%	20%	21%	23%	25%	26%

(Row labels = Years invested)

If 20% of a fund is invested in deals that return 10x in five years and everything else results in no value then the fund would have an annual return of approximately 15%. Few firms are able to generate those returns.

If you're even passingly familiar with startups, you may have heard of mantras such as 'Startups = Growth', or you may be familiar with the trope that startups are 'go-big-or-go-home'. This is in many ways driven by the time preference of the investors who play in this asset class. Effectively, this means that startups are under pressure to deliver 58% valuation growth over a five year period, or perhaps a gen-

tler 33% a year over eight years. This is a rather short time preference; many startups, in many markets, do not clear this bar. Many of them die.

But look outside startups, and you would find rather smaller growth rates, over longer time horizons. My goto example for this is Koch Industries, who [says in its Vision](#):

> *(…) our Vision is to improve the value we create for our customers more efficiently and faster than our competitors. This should enable us to generate the return on capital and investment opportunities needed to achieve a long-term growth rate that doubles earnings, on average, every six years.*

Doubling earnings every six years works out to about 12% revenue growth a year. Keep in mind that this is on average; the conglomerate is willing to accept that some years would be worse than others.

Koch remains an interesting example because it is so large and so powerful and its founders are considered so evil. As a case of growth with long time preference, however, Koch is difficult to beat: over the past 50 years, Charles and David Koch have managed to grow Koch Industries into the largest privately-held company in the United States, keeping all control for themselves. As Christopher Leonard documents in *[Kochland](#)*, by the mid-2000s, Koch Industries *could not die*. It was split into subsidiaries that were bulwarked from each other with strict enforcement of the corporate veil; failure in one subsidiary would never threaten the cash reserves of any other part of the empire.

And so Koch Industries grew, 12% at a time, over many decades, until it was so large that Charles Koch began manipulating the political system in the United States to his advantage.

I've deliberately chosen the venture-growth model to pit against the conglomerate growth model because the comparisons are so stark: you either build a go-big-or-go-home startup, compressing your wealth-seeking into a 10-year period,

returning capital to investors with a relatively short time preference. Or you could choose to work patiently, maintaining full control, compounding your earnings at 12% a year, over the course of five decades, until you eventually fund climate change denial and own large parts of the Republican party and are branded evil by half of your country.

Both paths are valid, but one path is significantly scarier than the other. Time preference matters. Use it well.