

Notes Essays—Peter Thiel’s CS183: Startup—Stanford, Spring 2012

Here are my essay versions of my class notes from CS183: Startup. Errors and omissions are my own. Credit for good stuff is Peter’s.

Class 1: The Challenge of the Future

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Peter Thiel's CS183: Startup - Class 1 Notes Essay

Here is an essay version of my class notes from Class 1 of CS183: Startup. Errors and omissions are my own. Credit for good stuff is Peter's entirely.

CS183: Startup—Notes Essay—The Challenge of the Future

Purpose and Preamble

We might describe our world as having retail sanity, but wholesale madness. Details are well understood; the big picture remains unclear. A fundamental challenge—in business as in life—is to integrate the micro and macro such that all things make sense.

Humanities majors may well learn a great deal about the world. But they don't really learn career skills through their studies. Engineering majors, conversely, learn in great technical detail. But they might not learn why, how, or where they should apply their skills in the workforce. The best students, workers, and thinkers will integrate these questions into a cohesive narrative. This course aims to facilitate that process.

I. The History of Technology

For most of recent human history—from the invention of the steam engine in the late 17th century through about the late 1960's or so—the technological process has been tremendous, perhaps even relentless. In most prior human societies, people made money by taking it from others. The industrial revolution wrought a paradigm shift in which people make money through trade, not plunder.

The importance of this shift is hard to overstate. Perhaps 100 billion people have ever lived on earth. Most of them lived in essentially stagnant societies; success involved claiming value, not creating it. So the massive technological acceleration of the past few hundred years is truly incredible.

The zenith of optimism about the future of technology might have been the 1960's. People *believed* in the future. They *thought* about the future. Many were supremely confident that the next 50 years would be a half-century of unprecedented technological progress.

But with the exception of the computer industry, it wasn't. Per capita incomes are still rising, but that rate is starkly decelerating. Median wages have been stagnant since 1973. People find themselves in an alarming Alice-in-Wonderland-style scenario in which they must run harder and harder—that is, work longer hours—just to stay in the same place. This deceleration is complex, and wage data alone don't explain it. But they do support the general sense that the rapid progress of the last 200 years is slowing all too quickly.

II. The Case For Computer Science

Computers have been the happy exception to recent tech deceleration. Moore's/Kryder's/Wirth's laws have largely held up, and forecast continued growth. Computer tech, with ever-improving hardware and agile development, is something of a model for other industries. It's obviously central to the Silicon Valley ecosystem and a key driver of modern technological change. So CS is the logical starting place to recapture the reins of progress.

III. The Future For Progress

A. Globalization and Tech: Horizontal vs. Vertical Progress

Progress comes in two flavors: horizontal/extensive and vertical/intensive. Horizontal or extensive progress basically means copying things that work. In one word, it means simply “globalization.” Consider what China will be like in 50 years. The safe bet is it will be a lot like the United States is now. Cities will be copied, cars will be copied, and rail systems will be copied. Maybe some steps will be skipped. But it’s copying all the same.

Vertical or intensive progress, by contrast, means doing new things. The single word for this is “technology.” Intensive progress involves going from 0 to 1 (not simply the 1 to n of globalization). We see much of our vertical progress come from places like California, and specifically Silicon Valley. But there is every reason to question whether we have enough of it. Indeed, most people seem to focus almost entirely on globalization instead of technology; speaking of “developed” versus “developing nations” is implicitly bearish about technology because it implies some convergence to the “developed” status quo. As a society, we seem to believe in a sort of technological end of history, almost by default.

It’s worth noting that globalization and technology do have some interplay; we shouldn’t falsely dichotomize them. Consider resource constraints as a 1 to n subproblem. Maybe not everyone can have a car because that would be environmentally catastrophic. If 1 to n is so blocked, only 0 to 1 solutions can help. Technological development is thus crucially important, *even if all we really care about is globalization.*

B. The Problems of 0 to 1

Maybe we focus so much on going from 1 to n because that’s easier to do. There’s little doubt that going from 0 to 1 is qualitatively different, and almost always harder, than copying something n times. And even trying to achieve vertical, 0 to 1 progress presents the challenge of exceptionalism; any founder or inventor doing something new must wonder: am I sane? Or am I crazy?

Consider an analogy to politics. The United States is often thought of as an “exceptional” country. At least many Americans believe that it is. So is the U.S. sane? Or is it crazy? Everyone owns guns. No one believes in climate change. And most people weigh 600 pounds. Of course, exceptionalism may cut the other way. America is the land of opportunity. It is the frontier country. It offers new starts, meritocratic promises of riches. Regardless of which version you buy, people must grapple with the problem of exceptionalism. Some 20,000 people, believing themselves uniquely gifted, move to Los Angeles every year to become famous actors. Very few of them, of course, actually become famous actors. The startup world is probably less plagued by the challenge of exceptionalism than Hollywood is. But it probably isn’t immune to it.

C. The Educational and Narrative Challenge

Teaching vertical progress or innovation is almost a contradiction in terms. Education is fundamentally about going from 1 to n . We observe, imitate, and repeat. Infants do not invent new languages; they learn existing ones. From early on, we learn by copying what has worked before.

That is insufficient for startups. Crossing T’s and dotting I’s will get you maybe 30% of the way there. (It’s certainly necessary to get incorporation right, for instance. And one can learn how to pitch VCs.) But at some

point you have to go from 0 to 1—you have to do something important and do it right—and that can't be taught. Channeling Tolstoy's intro to Anna Karenina, all successful companies are different; they figured out the 0 to 1 problem in different ways. But all failed companies are the same; they botched the 0 to 1 problem.

So case studies about successful businesses are of limited utility. PayPal and Facebook worked. But it's hard to know what was necessarily path-dependent. The next great company may not be an e-payments or social network company. We mustn't make too much of any single narrative. Thus the business school case method is more mythical than helpful.

D. Determinism vs. Indeterminism

Among the toughest questions about progress is the question of how we should assess a venture's probability of success. In the 1 to n paradigm, it's a statistical question. You can analyze and predict. But in the 0 to 1 paradigm, it's not a statistical question; the standard deviation with a sample size of 1 is infinite. There can be no statistical analysis; statistically, we're in the dark.

We tend to think very statistically about the future. And statistics tells us that it's random. We can't predict the future; we can only think probabilistically. If the market follows a random walk, there's no sense trying to out-calculate it.

But there's an alternative math metaphor we might use: calculus. The calculus metaphor asks whether and how we can figure out exactly what's going to happen. Take NASA and the Apollo missions, for instance. You have to figure out where the moon is going to be, exactly. You have to plan whether a rocket has enough fuel to reach it. And so on. The point is that no one would want to ride in a statistically, probabilistically-informed spaceship.

Startups are like the space program in this sense. Going from 0 to 1 always has to favor determinism over indeterminism. But there is a practical problem with this. We have a word for people who claim to know the future: prophets. And in our society, all prophets are false prophets. Steve Jobs finessed his way about the line between determinism and indeterminism; people sensed he was a visionary, but he didn't go too far. He probably cut it as close as possible (and succeeded accordingly).

The luck versus skill question is also important. Distinguishing these factors is difficult or impossible. Trying to do so invites ample opportunity for fallacious reasoning. Perhaps the best we can do for now is to flag the question, and suggest that it's one that entrepreneurs or would-be entrepreneurs should have some handle on.

E. The Future of Intensive Growth

There are four theories about the future of intensive progress. First is convergence; starting with the industrial revolution, we saw a quick rise in progress, but technology will decelerate and growth will become asymptotic.

Second, there is the cyclical theory. Technological progress moves in cycles; advances are made, retrenchments ensue. Repeat. This has probably been true for most of human history in the past. But it's hard to imagine it remaining true; to think that we could somehow lose all the information and know-how we've amassed and be doomed to have to re-discover it strains credulity.

Third is collapse/destruction. Some technological advance will do us in.

Fourth is the singularity where technological development yields some AI or intellectual event horizon.

People tend to overestimate the likelihood or explanatory power of the convergence and cyclical theories. Accordingly, they probably underestimate the destruction and singularity theories.

IV. Why Companies?

If we want technological development, why look to companies to do it? It's possible, after all, to imagine a society in which everyone works for the government. Or, conversely, one in which everyone is an independent contractor. Why have some intermediate version consisting of at least two people but less than everyone on the planet?

The answer is straightforward application of the Coase Theorem. Companies exist because they optimally address internal and external coordination costs. In general, as an entity grows, so do its internal coordination costs. But its external coordination costs fall. Totalitarian government is entity writ large; external coordination is easy, since those costs are zero. But internal coordination, as Hayek and the Austrians showed, is hard and costly; central planning doesn't work.

The flipside is that internal coordination costs for independent contractors are zero, but external coordination costs (uniquely contracting with absolutely everybody one deals with) are very high, possibly paralyzingly so. Optimality—firm size—is a matter of finding the right combination.

V. Why Startups?

A. Costs Matter

Size and internal vs. external coordination costs matter a lot. North of 100 people in a company, employees don't all know each other. Politics become important. Incentives change. Signaling that work is being done may become more important than actually doing work. These costs are almost always underestimated. Yet they are so prevalent that professional investors should and do seriously reconsider before investing in companies that have more than one office. Severe coordination problems may stem from something as seemingly trivial or innocuous as a company having a multi-floor office. Hiring consultants and trying to outsource key development projects are, for similar reasons, serious red flags. While there's surely been some lessening of these coordination costs in the last 40 years—and that explains the shift to somewhat smaller companies—the tendency is still to underestimate them. Since they remain fairly high, they're worth thinking hard about.

Path's limiting its users to 150 "friends" is illustrative of this point. And ancient tribes apparently had a natural size limit that didn't much exceed that number. Startups are important because they are small; if the size and complexity of a business is something like the square of the number of people in it, then startups are in a unique position to lower interpersonal or internal costs and thus to get stuff done.

The familiar Austrian critique dovetails here as well. Even if a computer could model all the narrowly economic problems a company faces (and, to be clear, none can), it wouldn't be enough. To model all costs, it would have to model human irrationalities, emotions, feelings, and interactions. Computers help, but we

still don't have all the info. And if we did, we wouldn't know what to do with it. So, in practice, we end up having companies of a certain size.

B. Why Do a Startup?

The easiest answer to "why startups?" is negative: because you can't develop new technology in existing entities. There's something wrong with big companies, governments, and non-profits. Perhaps they can't recognize financial needs; the federal government, hamstrung by its own bureaucracy, obviously overcompensates some while grossly undercompensating others in its employ. Or maybe these entities can't handle personal needs; you can't always get recognition, respect, or fame from a huge bureaucracy. Anyone on a mission tends to want to go from 0 to 1. You can only do that if you're surrounded by others to want to go from 0 to 1. That happens in startups, not huge companies or government.

Doing startups for the money is not a great idea. Research shows that people get happier as they make more and more money, but only up to about \$70,000 per year. After that, marginal improvements brought by higher income are more or less offset by other factors (stress, more hours, etc). Plus there is obviously diminishing marginal utility of money even absent offsetting factors).

Perhaps doing startups to be remembered or become famous is a better motive. Perhaps not. Whether being famous or infamous should be as important as most people seem to think it is highly questionable. A better motive still would be a desire to change the world. The U.S. in 1776-79 was a startup of sorts. What were the Founders motivations? There is a large cultural component to the motivation question, too. In Japan, entrepreneurs are seen as reckless risk-takers. The respectable thing to do is become a lifelong employee somewhere. The literary version of this sentiment is "behind every fortune lies a great crime." Were the Founding Fathers criminals? Are all founders criminals of one sort or another?

C. The Costs of Failure

Startups pay less than bigger companies. So founding or joining one involves some financial loss. These losses are generally thought to be high. In reality, they aren't that high.

The nonfinancial costs are actually higher. If you do a failed startup, you may not have learned anything useful. You may actually have learned how to fail again. You may become more risk-averse. You aren't a lottery ticket, so you shouldn't think of failure as just 1 of n times that you're going to start a company. The stakes are a bit bigger than that.

A 0 to 1 startup involves low financial costs but low non-financial costs too. You'll at least learn a lot and probably will be better for the effort. A 1 to n startup, though, has especially low financial costs, but higher non-financial costs. If you try to do Groupon for Madagascar and it fails, it's not clear where exactly you are. But it's not good.

VI. Where to Start?

The path from 0 to 1 might start with asking and answering three questions. First, what is valuable? Second, what can I do? And third, what is nobody else doing?

The questions themselves are straightforward. Question one illustrates the difference between business and academia; in academia, the number one sin is plagiarism, not triviality. So much of the innovation is esoteric and not at all useful. No one cares about a firm's eccentric, non-valuable output. The second question ensures that you can actually execute on a problem; if not, talk is just that. Finally, and often overlooked, is the importance of being novel. Forget that and we're just copying.

The intellectual rephrasing of these questions is: *What important truth do very few people agree with you on?*

The business version is: *What valuable company is nobody building?*

These are tough questions. But you can test your answers; if, as so many people do, one says something like "our educational system is broken and urgently requires repair," you know that that answer is wrong (it may be a truth, but lots of people agree with it). This may explain why we see so many education non-profits and startups. But query whether most of those are operating in technology mode or globalization mode. You know you're on the right track when your answer takes the following form:

"Most people believe in X. But the truth is !X."

Make no mistake; it's a hard question. Knowing what 0 to 1 endeavor is worth pursuing is incredibly rare, unique, and tricky. But the process, if not the result, can also be richly rewarding.

Peter Thiel's CS183: Startup - Class 2 Notes Essay

Here is an essay version of my class notes from Class 2 of CS183: Startup. Errors and omissions are my own. Credit for good stuff is Peter's entirely.

CS183: Startup—Notes Essay—Party Like It's 1999?

I. Late to the Party

History is driven by each generation's experience. We are all born into a particular culture at a particular time. That culture is like an extended dinner conversation; lots of people are talking, some lightly, some angrily, some loudly, some in whispers. As soon as you're able, you listen in. You try to figure out what that conversation is about. Why are people happy? Why are they upset? Sometimes it's hard to figure out.

Take someone born in the late 1960s, for instance. There was a lot going on then, culturally. But a toddler in the late '60s, despite having technically lived through them, essentially missed the debates on civil rights, Vietnam, and what the U.S. was supposed to look like. The child, being more or less excluded from the dinner table, would later find it hard to get a sense of what those discussions were like.

There is a keen analogue between the cultural intensity of the '60s and the technological intensity of the 1990s. But today's college and perhaps even graduate students, like the toddler in 1969, may have been too young to have viscerally experienced what was going on back in 1999. To participate in the dinner table conversation—to be able to think and talk about businesses and startups today in 2012—we must get a handle on the history of the '90s. It is questionable whether one can really understand startups without, say, knowing about Webvan or recognizing the Pets.com mascot.

History is a strange thing in that it often turns out to be quite different than what people who lived through it thought it was. Even technology entrepreneurs of the '90s might have trouble piecing together that decade's events. And even if we look back at what actually happened, it's not easy to know why things happened as they did. All that's clear is that the '90s powerfully shaped the current landscape. So it's important to get as good a grasp on them as possible.

II. A Quick History of the 90s

Most of the 1990s was not the dot com bubble. Really, what might be called the mania started in September 1998 and lasted just 18 months. The rest of the decade was a messier, somewhat chaotic picture.

The 1990s could be said to have started in November of '89. The Berlin Wall came down. 2 months of pretty big euphoria followed. But it didn't last long. By early 1990, the U.S. found itself in a recession—the first one in post WWII history that was long and drawn out. Though it wasn't a terribly deep recession—it technically ended in March of '91—recovery was relatively slow. Manufacturing never fully rebounded. And even the shift to the service economy was protracted.

So from 1992 through the end of 1994, it still felt like the U.S. was mired in recession. Culturally, Nirvana, grunge, and heroin reflected increasingly acute senses of hopelessness and lack of faith in progress. Worry about NAFTA and U.S. competitiveness vis-à-vis China and Mexico became near ubiquitous. The strong pessimistic undercurrent fueled Ross Perot's relatively successful third party presidential candidacy. George

H.W. Bush became the only 1-term President in the last thirty years. Things didn't seem to be going right at all.

To be sure, technological development was going on in Silicon Valley. But it wasn't that prominent. Unlike today, the Stanford campus in the late 1980s felt quite disconnected with whatever tech was happening in the valley. At that time, Japan seemed to be winning the war on the semiconductor. The Internet had yet to take off. Focusing on tech was idiosyncratic. The industry felt small.

The Internet would change all that. Netscape, with its server-client model, is probably the company most responsible for starting the Internet. It was not the first group to think of a 2-way communications network between all computers; that honor goes to Xanadu, who developed that model in 1963. Xanadu's problem was that you needed everyone to adopt it at once for the network to work. They didn't, so it didn't. But it became a strange cult-like entity; despite never making any money, it kept attracting venture funding for something like 29 years, finally dying in 1992 when investors became irreversibly jaded.

So Netscape comes along in '93 and things start to take off. It was Netscape's IPO in August of 1995—over halfway through the decade!—that really made the larger public aware of the Internet. It was an unusual IPO because Netscape wasn't profitable at the time. They priced it at \$14/share. Then they doubled it. On the first day of trading the share price doubled again. Within 5 months, Netscape stock was trading at \$160/share—completely unprecedented growth for a non-profitable company.

The Netscape arc was reminiscent of Greek tragedy: a visionary founder, great vision, hubris, and an epic fall. An instance of Netscape's hubris had them traveling to the Redmond campus, triumphantly plastering Netscape posters everywhere. They poked the dragon in the eye; Bill Gates promptly ordered everyone at Microsoft to drop what they were doing and start working on the Internet. IE came out shortly after that and Netscape began rapidly losing market share. Netscape's saving grace was its legally valuable antitrust claims—probably the only reason that a company that never really made money was able to sell to AOL for over a billion dollars.

The first three years after Netscape's IPO were relatively quiet; by late 1998, the NASDAQ was at about 1400—just 400 points higher than it was in August '95. Yahoo went public in '96 at a \$350M valuation, and Amazon followed in '97 at a \$460M valuation. Skepticism abounded. People kept looking at earnings and revenues multiples and saying that these companies couldn't be that valuable, that they could never succeed.

This pessimism was probably appropriate, but misplaced. Things weren't going particularly well in the rest of the world. Alan Greenspan delivered his famous irrational exuberance speech in 1996—a full 3 years before the bubble actually hit and things got really crazy. But even if there was irrational exuberance in 1996, the U.S. was hardly in a position to do anything about it. 1997 saw the eruption of the East Asian financial crises in which some combination of crony capitalism and massive debt brought the Thai, Indonesian, South Korean, and Taiwanese (to name just a few) economies to their knees. China managed to avoid the brunt of the damage with tight capital controls. But then in 1998, the Ruble crisis hit Russia. These were unique animals in that usually, either banks go bust or your currency goes worthless. Here, we saw both. So your money was worthless, and the banks had none of it. Zero times zero is zero.

On the heels of the Russian crisis came the Long-Term Capital Management crisis; LTCM traded with enormous leverage ("picking up nickels in front of a bulldozer"), ultimately blew up, and but for a multibillion dollar bailout from the Fed, seemed poised to take down the entire U.S. economy with it. Things

in Europe weren't all that much better. The Euro launched in January 1999, but optimism about it was the exception, strong skepticism the norm. It proceeded to lose value immediately.

One way to think about the tech mania from March 1998 to September 2000, then, comes from this insight that pretty much everything else was going insanely wrong before that time. The technology bubble was an indirect proof; the old economy was proven not to work, as we could no longer compete with Mexico or China. Emerging markets were proven failures, rife with cronyism and mismanagement. Europe offered little hope. And no one wanted to invest with leverage after the LTCM disaster. So, by the late '90s, a process of elimination left only one good place to put money: in tech.

Of course, proof by contradiction is a dangerous way to draw conclusions. The world is not always a logical place. So even if something's not A, B, C, or D, it doesn't necessarily follow that the truth is E; the set may not be as simple as A thru E. But while that's important to flag, indirect proof seems to have some purchase here. There's still a sense in which tech worked, or was seen as working, because nothing else did, or was.

III. The Mania: September 1998 – March 2000

A. Mania Generally

The Mania started in September of '98. Probably the best way to convey just how crazy things got is to tell people crazy stories about how crazy things got. Any tech entrepreneur from that time necessarily has scores of pretty insane anecdotes to tell. Certain common themes will run through them all: the times were extremely social. People *were* irrationally exuberant. It felt like there was money everywhere... probably because there was. And there was no shortage of very sketchy people running around the valley.

Admittedly, these themes reflect fairly superficial impressions. But we shouldn't quickly dismiss them for that; quite often, the surface of things is actually the heart of things. So anecdotes that reflect the short-lived bubble zeitgeist, in addition to being kind of bizarre and fun, are worth thinking about.

And, again, there's no shortage of anecdotes. There were 40-year-old grad students at Stanford who were trying to start dozens of rather wacky companies. Now, usually being a forty-something graduate student means you've gone insane. And usually, trying to start several companies at once is seen as unwise. But in late 1998, many people believed that to be a winning combination.

There were brunches at Bucks and dinners at Il Fornaio. There were billionaires from Idaho flying in giving money to anyone with an idea and a polished pitch. Fairly broke entrepreneurs racked up thousand dollar dinner bills and tried to pay in shares of their companies. Sometimes that even worked. It's easy to look back and see a lot of ridiculousness. But it wasn't all fluff; a great deal of activity happened in these social contexts. Launch parties became so important that someone put together an exclusive e-mail list that published rankings of the various parties going on that day.

People began to say and do pretty crazy things. Many business models adopted some weird dynamic where the more you sold or did, the more money you'd lose. It was like an SNL skit; a customer deposits \$100 in pennies at the bank, and the bank loses money because it costs them more to sort through everything than that deposit is worth. But while a bank would recognize that and stop, the dot coms would say, without irony, "It's okay... we'll make it up in volume." Irrationality was rational when simply adding ".com" after your name more or less doubled your value overnight.

Yahoo grew to replace Netscape as the most hubristic tech company. By '97 it was largest Internet company in Silicon Valley. Yahoo encouraged PayPal in 2000 to think carefully about *who* to sell the company to, because you needed to know that the buyer was sound in a stock-for-stock sale. Yahoo thought itself an attractive buyer because it would pay out in Yahoo stock, which, according to Yahoo at the time, "always goes up."

Great fortunes made in those 18 months. Plenty were lost. In 1997, Larry Augustin was deciding whether to close up VA Linux. He chose not to. In 1999, VA Linux went public at \$30/share. It quickly traded up to \$300, earning it the distinction of being the stock that went up more than any other on the first day of trading, ever. Since Augustin owned 10% of the company, he was worth about a billion dollars by the end of the day. People were saying that sometimes, lightning does strike twice; Augustin had previously declined an offer to be the third employee at Yahoo, which, of course, would have made him billions as well. But the VA Linux story took a turn for the worse; 6 month later, by the end of the lock-up period, the stock lost 90% of value. Anyone who didn't sell took another 90% hit over the following 6 months. Augustin ended up with 5 or 6 million dollars, which is still a lot of money. But it's not a billion.

All the parties, money, and IPO success stories made for lots of sketchy businesses. Those businesses were funded by sketchy VCs and run by sketchy entrepreneur-salespeople. Since everybody was running around saying pretty crazy things, it became increasingly hard to tell who was too sketchy and who wasn't. To avoid being drawn in by slick salesmen, Max Levchin developed what he called the aura test: you listen to someone for 15 seconds and then decide if he has a good aura. If so, you continue to listen. If not, you walk away. It's not hard to imagine that companies who employed some version of the aura test were more likely to survive the mania than those who didn't.

B. PayPal Mania

Since PayPal only got started in December of '98—fairly late in the tech boom—one problem it faced was the high likelihood of hiring the sort of sketchy people that seemed to be proliferating. The founders agreed that PayPal could not afford to hire sketchy people. So they just hired their friends instead.

PayPal's original idea involved beaming money to people over Palm Pilots. It was voted one of the worst 10 business ideas of 1999, which is saying a lot. The initial business model was hardly better; there was a sense in which PayPal had to raise money so it could raise more money so it could then figure out what to do with all that money. And, oddly enough, it was possible to raise an angel round on that model; one archetypical angel investor, during a pitch over Chinese food at Town & Country in Palo Alto, was utterly unconcerned with what PayPal did. Rather, he wanted to know one thing: who else was investing. Later, he consulted the fortune cookie. It told him to invest.

Among the first big breaks was landing a \$4.5M investment from Nokia ventures. The problem, though, was that mobile Internet didn't quite work yet. Good interfaces were years away, and integration with handsets seemed to take forever. Much to Nokia's surprise, PayPal announced a pivot at the first post-investment board meeting. The new idea was simple: an account-based system where you could send money to anyone with an e-mail address. It was a good idea, but it seemed too easy. Surely, serious competition had to be working on that, too. So 1999 became increasingly frantic, since people knew they had to move quickly or fail.

PayPal's big challenge was to get new customers. They tried advertising. It was too expensive. They tried BD deals with big banks. Bureaucratic hilarity ensued. The turning point was when Luke Nosek got a meeting with the chairman and top brass at HSBC in London. Several old school bankers crowded into a large wood paneled conference room. They had no idea what to make of these California startup guys talking about the Internet. They looked so dazed and confused that they very well could have been extras who knew nothing about payments and tech at all. Luke, despite being on a life-extension calorie restriction diet, found a Häagen-Dazs. And over ice cream, the PayPal team reached an important conclusion: BD didn't work. They needed organic, viral growth. They needed to give people money.

So that's what they did. New customers got \$10 for signing up, and existing ones got \$10 for referrals. Growth went exponential, and PayPal wound up paying \$20 for each new customer. It felt like things were working and not working at the same time; 7 to 10% daily growth and 100 million users was good. No revenues and an exponentially growing cost structure were not. Things felt a little unstable. PayPal needed buzz so it could raise more capital and continue on. (Ultimately, this worked out. That does not mean it's the best way to run a company. Indeed, it probably isn't.)

Feb 16, 2000 was a good day for PayPal; the Wall Street Journal ran a flattering piece that covered the company's exponential growth and gave it a very back of the envelope valuation of \$500M. The next month, when PayPal raised another round of funding, the lead investor accepted the WSJ's Feb. 16 valuation as authoritative.

That March was thoroughly crazy. A South Korean firm that really wanted to invest called up PayPal's law firm to ask where they could wire funds to invest. It promptly wired \$5M without signing any documents or negotiating a deal. The Koreans absolutely refused to say where PayPal could send the money back. The attitude was simple: "No. You have to take it." PayPal closed its \$100M round on March 31st. The timing was fortunate, since after that everything sort of crashed. PayPal was left with the challenge of building a real business.

The transition from 1999 to 2000 was much like Prince predicted it would be in his song "1999" ("Cause they say 2,000 zero zero party over, oops! Out of time! So tonight I'm gonna party like it's 1999!"). Perhaps he was right for the wrong reasons; we shouldn't make too much of that. But it turned out quite prescient. A rolling wave of collapse struck; marketing-driven e-commerce companies failed in the first half of 2000, and B2B companies failed in the second. The telecoms followed in 2001. If you had to pick what sector of economy was at absolute lowest in March 2000, it might have been be military defense companies. The NASDAQ was soaring. No one believed there would ever be another war. But then things reversed. The military defense industry would rise for most of the next decade.

IV. Hubris and Schadenfreude

In the aftermath of 2001 and 2002, enormous amounts of hubris yielded to Shadenfreude. People insisted that "we were right all along," and became culturally and socially depressed.

PayPal would survive this shift, but it was clear that it was a whole new world. The company broke even in 2001. It was able to solve some tough fraud problems and get a handle on its customer service problems. When it filed for IPO in late September 2001, PayPal became the first company to file after 9/11. This time,

some 20 months after the rosy WSJ article, another article came out. It was titled “Earth To Palo Alto.” It began:

What would you do with a 3-year-old company that has never turned an annual profit, is on track to lose a quarter billion dollars and whose recent SEC filings warn that its services might be used for money laundering and financial fraud?

If you were the managers and venture capitalists behind Palo Alto’s PayPal, you’d take it public. And that is what they hope to do in an \$80 million offering that will test the limits of investor tolerance and financial market gullibility.

It didn’t get much better. The U.S., it concluded, “needs [PayPal] as much as it does an anthrax epidemic.”

V. Lessons Learned

A. By The World

The key takeaway for most people was that the tech explosion of the late ‘90s was all a bubble. A shift back to the real economy was needed. If the expression in the ‘90s was “bricks to clicks,” the 2000s demanded a return from clicks back to bricks. People got into housing and emerging markets. High profile investors like Warren Buffet avoided tech stocks in favor of old economy ones. Profit alone mattered in evaluating businesses. Globalization was favored over technology. The general sense was that the dot com crash taught us that the future was fundamentally indeterminate. That all prophets are false prophets. That we shouldn’t believe anything people tell us, ever.

The only problem with those lessons is that they’re probably all wrong. At their core are complex, reactionary emotions; they’re driven by hubris, envy, and resentment against the ‘90s generally. When base emotions are driving, analysis becomes untrustworthy.

The reality is that people were right about lots of things in the ‘90s. The indirect proof that judged tech to be king was not weakened by the excesses that would come. There *was* a problem with the Euro. There *were* problems with war, crony capitalism, and overleverage. Tech did not work perfectly, and insofar as it didn’t protective reactions against the bubble may be justified. But March of 2000 wasn’t just a peak of insanity. In some important ways, it was still a peak of clarity as well.

B. By Silicon Valley

People in Silicon Valley learned that you have to do things differently to survive in the Schadenfreude world. First, you had to believe and practice incrementalism. Grand visions and moving quickly fell out of favor.

Second, your startup had to be “lean.” You should not, in fact, know what you’re going to do. Instead, you should experiment, iterate, and figure it out as time goes on.

Third, you should have zero advertising spend. If your growth isn’t viral, it’s fake.

Fourth, anti-social was the new social. People wanted to withdraw into a new antisocial modality. Google is the iconic cultural version of this; a product for people who'd rather interact with computers than people.

Fifth, product needed elevation over business development. In 1999, smart non-engineers were doing BD. In 2001, they were doing product. In the '90s, iconic CEOs were salespeople. E.g. Larry Ellison. In the 2000s, iconic CEOs were product visionaries. E.g. Steve Jobs.

Sixth, rapid monetization was to be distrusted. Better is a more protracted growth phase and later IPO. If you have company that's growing relatively quickly, you should probably reinvest profits and make it grow even more quickly.

Finally—and this was the overarching theme—you shouldn't discuss the future. That will just make you look weird and crazy, and, well, you just shouldn't do it.

Overall, the post-mania was one big strategic retreat that incorporated all of these elements. Which elements are right and which are wrong is a complicated question. But it's a question worth asking. Certainly there were good reasons for the retreat. But in many aspects it was probably overblown. Some elements make sense; why IPO early in an environment that, all of a sudden, is hostile to high-growth tech stocks? But others are questionable, at least as ironclad rules; should you never advertise? Never do BD/sales? Are you sure we can't talk about the future? We should be open to idea that some or much of the retreat—however necessary it was generally—was overreaction.

VI. Bubbles

The big legacy question from the '90s is: are we in a tech bubble?

Many people say yes. The Richter Scales' "Here Comes Another Bubble Video" below, done in October 2007, is strikingly undated in how people are thinking about things today.

Now we're back to the dinner conversation that people are stuck in. There are lots of good questions to ask about the conversation. But the question of bubble vs. no bubble is not one of them. Indeed, that's the wrong question at this point. Sure, one can string together some random data points that suggest things are frothy. More people may be doing CS at Stanford now than back in '99. Valuations may be creeping up.

But some data points on some froth hardly shows that the bubble thesis is accurate. And the weight of the evidence suggests it's inaccurate. Bubbles arise when there is (1) widespread, intense belief that's (2) not true. But people don't really believe in anything in our society anymore. You can't have a bubble absent widespread, intense belief. The incredible narrative about a tech bubble comes from people who are *looking* for a bubble. That's more overreaction to the pain of the '90s than it is good analysis.

Antibubble type thinking is probably somewhat more true. In other words, it's probably better to insist that everything is going to work and that people should buy houses and tech stocks than it is to claim that there's a bubble. But we should resist that, too. For bubble and anti-bubble thinking are both wrong because they hold the truth is social. But if the herd isn't thinking at all, being contrarian—doing the opposite of the herd—is just as random and useless.

To understand businesses and startups in 2012, you have to do the *truly* contrarian thing: you have to think for yourself. The question of what is valuable is a much better question than debating bubble or no bubble. The value question gets better as it gets more specific: is company X valuable? Why? How should we figure that out? Those are the questions we need to ask. Next class, we'll look at how we might go about thinking about them.

Peter Thiel's CS183: Startup - Class 3 Notes Essay

Here is an essay version of class notes from Class 3 of CS183: Startup. Errors and omissions are my own. Credit for good stuff is Peter's entirely. Please note that I actually missed this class (I was on my honeymoon!). Thanks to [@erikpavia](#) and [@danrthompson](#) for sending me their notes to work from.

CS183: Startup—Notes Essay—Value Systems

The history of the '90s was in many ways the history of widespread confusion about the question of value. Valuations were psychosocial; value was driven by what people said it was. To avoid herd-like confusion of decades past, we need to try and figure out whether it's possible to determine businesses' objective value and, if it is, how to do it.

As we discussed back in Class 1, certain questions and frameworks can anchor our thinking about value. The questions are necessarily personal: What can I do? What do I think is valuable? *What do I see others not doing?* A good framework might map globalization and technology as the two great axes of the 21st century. Synthesizing all this together forges the higher-level question: *What valuable company is nobody building?*

A somewhat different perspective on technology—going from 0 to 1, to revisit our earlier terminology—is the financial or economic one. Since that perspective can also shed considerable light on the value question, it's worth covering in detail now.

I. Great Technology Companies

Great companies do three things. First, they create value. Second, they are lasting or permanent in a meaningful way. Finally, they capture at least some of the value they create.

The first point is straightforward. Companies that don't create value simply can't be great. Creating value may not be sufficient for greatness, but it's hard to see how it's not at least necessary.

Great companies last. They are durable. They don't create value and disappear very fast. Consider disk drive companies of the 1980s. They created a lot of value by making new and better drives. But the companies themselves didn't last; they were all replaced by others. Not sticking around limits both the value you can create and the value you can capture.

Finally—and relatedly—you have to capture much of the value you create in order to be great. A scientist or mathematician may create a lot of lasting value with an important discovery. But capturing a meaningful piece of that value is another matter entirely. Sir Isaac Newton, for example, failed to capture much of the immense value that he created through his work. The airline industry is a less abstract example. The airlines certainly create value in that the public is much better off because they exist. And they employ tons of people. But the airlines themselves have never really made any money. Certainly some are better than others. But probably none can be considered a truly great company.

II. Valuation

One way that people try and objectively determine a company's value is through multiples and comparables. This sort of works. But people should be on guard against social heuristics substituting for rigorous analysis, since analysis tends to be driven by standards and conventions that exist at the time. If you start a company at an incubator, certain conventions exist there. If everyone is investing at a \$10M cap, the company might be deemed to be worth \$10M. There are a bunch of formulas that incorporate metrics like monthly page views or number of active users that people use sometimes. Somewhat more rigorous are revenue multiples. Software companies are often valued at around 10x annual revenues. Guy Kawasaki has suggested a particularly unique (and possibly helpful) equation:

$$\text{pre-money valuation} = (\$1M * n_{\text{engineers}}) - (\$500k * n_{\text{MBAs}}).$$

The most common multiple is the price-earnings ratio, also known as P/E ratio or the PER. The PER is equal to market value (per share) / earnings (per share). In other words, it is the price of a stock relative to a firm's net income. The PER is widely used but does not account for growth.

To account for growth, you use the PEG, or Price/Earnings to Growth ratio. PEG equals (market value / earnings) / annual earnings growth. That is, PEG is PER divided by annual growth in earnings. The lower a company's PEG ratio, the slower it's growing and thus the less valuable it is. Higher PEG ratios tend to mean higher valuations. In any case, PEG should be less than one. The PEG is a good indicator to keep an eye on while growing your business.

One does valuation analysis at a given point in time. But that analysis factors in many points in time. You look not just at cash flows for the current year, but over future years as well. Sum all the numbers and you get the earnings value. But a quantity of money today is worth more than it is in the future. So you discount the time value of money, or TVM, since there are all sorts of risks as you move forward in the future. The basic math for TVM is:

$$r = \text{discount rate}$$

$$CF_t = \text{cash flow in year } t$$

$$DPV = \text{discounted present value}$$

$$DPV_o = \frac{CF_t}{(1+r)^t}$$

$$DPV = \sum_{t=0}^n \frac{CF_t}{(1+r)^t}$$

Things are harder when cash flows aren't constant. Here is the math for variable cash flows:

$$g = \text{growth rate}$$

$$DPV = \sum_{t=0}^n \frac{CF_o(1+g)^t}{(1+r)^t}$$

$$TV = \text{terminal value}$$

$$TV_t = \frac{CF_{t+1}}{(r-g)(1+r)^t}$$

$$NPV = DPV + TV$$

So to determine the value of a company, you do the applicable DPV or NPV calculation for the next X (or infinite) years. Generally, you want g to be greater than r . Otherwise your company isn't growing enough to keep up with the discount rate. Of course, in a growth model, the growth rate must eventually decline. Otherwise the company will approach infinite value over time—not likely.

Valuations for Old Economy firms work differently. In businesses in decline, most of the value is in the near term. Value investors look at cash flows. If a company can maintain present cash flows for 5 or 6 years, it's a good investment. Investors then just hope that those cash flows—and thus the company's value—don't decrease faster than they anticipate.

Tech and other high growth companies are different. At first, most of them *lose* money. When the growth rate— g , in our calculations above—is higher than the discount rate r , a lot of the value in tech businesses exists pretty far in the future. Indeed, a typical model could see 2/3 of the value being created in years 10 through 15. This is counterintuitive. Most people—even people working in startups today—think in Old Economy mode where you have to create value right off the bat. The focus, particularly in companies with exploding growth, is on next months, quarters, or, less frequently, years. That is too short a timeline. Old Economy mode works in the Old Economy. It does not work for thinking about tech and high growth businesses. Yet startup culture today pointedly ignores, and even resists, 10-15 year thinking.

PayPal is illustrative. 27 months in, its growth rate was 100%. Everybody knew that rate would decelerate, but figured that it would still be higher than the discount rate. The plan was that most of the value would come around 2011. Even that long-term thinking turned out to undershoot; the discount rate has been lower than expected, and the growth rate is still at a healthy 15%. Now, it looks like most of PayPal's value won't come until in 2020.

LinkedIn is another good example of the importance of the long-term. Its market cap is currently around around \$10B and it's trading at a (very high) P/E of about 850. But discounted cash flow analysis makes LinkedIn's valuation make sense; it's expected to create around \$2B in value between 2012 and 2019, while the other \$8B reflects expectations about 2020 and beyond. LinkedIn's valuation, in other words, only makes sense if there's durability, i.e. if it's around to create all that value in the decades to come.

III. Durability

People often talk about “first mover advantage.” But focusing on that may be problematic; you might move first and then fade away. The danger there is that you simply aren't around to succeed, even if you do end up creating value. More important than being the first mover is being the last mover. You have to be durable.

In this one particular at least, business is like chess. Grandmaster José Raúl Capablanca put it well: to succeed, “you must study the endgame before everything else.”

IV. Capturing Value

The basic economic ideas of supply and demand are useful in thinking about capturing value. The common insight is that market equilibrium is where supply and demand intersect. When you analyze a business under this framework, you get one of two options: perfect competition or monopoly.

In perfect competition, no firms in an industry make economic profit. If there are profits to be made, firms enter the market and the profits go away. If firms are suffering economic losses, some fold and exit. So you don’t make any money. And it’s not just you; *no one* makes any money. In perfect competition, the scale on which you’re operating is negligible compared to the scale of the market as a whole. You might be able to affect demand a little bit. But generally you’re a price taker.

But if you’re a monopoly, you own the market. By definition, you’re the only one producing a certain thing. Most economics textbooks spend a great deal of time talking about perfect competition. They tend to treat monopoly as somehow being within, or as some small exception to perfect competition. The world, say these books, defaults to equilibrium.

But perhaps monopoly is not some strange exception. Perhaps perfect competition is only the default in economics textbooks. We should wonder whether monopoly is a valid alternative paradigm in its own right. Consider great tech companies. Most have one decisive advantage—such as economies of scale or uniquely low production costs—that make them at least monopoly-esque in some important way. A drug company, for instance, might secure patent protection for a certain drug, thus enabling it to charge more than its costs of production. The really valuable businesses are monopoly businesses. They are the last movers who create value that can be sustained over time instead of being eroded away by competitive forces.

V. The Ideology of Competition

A. PayPal and Competition

PayPal was in the payments business. There were considerable economies of scale in that business. You couldn’t compete with the big credit card companies directly; to compete, you had to undercut them in some way. PayPal tried to do that in two ways: through technical innovation and through product innovation.

The primary technical problem that PayPal faced was fraud. When Internet payments started to get going, there was much more fraud than people expected. Also unexpected was how hard it was to stamp it out. Enemies in the War on Fraud were many. There was “Carders World,” a dystopian web marketplace that vowed to bring down Western Capitalism by transacting in stolen identities. There was a particularly bothersome hacker named Igor, who evaded the FBI on jurisdictional technicalities. (Unrelatedly, Igor was later killed by the Russian mafia.) Ultimately, PayPal was able to develop really good software to get a handle on the fraud problem. The name of that software? “Igor.”

Another key innovation was making funding sources cheaper. Getting users’ bank account information drove down blended costs. By modeling how much money was in an account, PayPal could make advance

payments, more or less circumvent the Automatic Clearing House system, and make payments instantaneous from the user's perspective.

These are just two examples from PayPal. Yours will look different. The takeaway is that it's absolutely critical to have some decisive advantage over the next best service. Because even a small number of competing services quickly makes for a very competitive dynamic.

B. Competition and Monopoly

Whether competition is good or bad is an interesting (and usually overlooked) question. Most people just assume it's good. The standard economic narrative, with all its focus on perfect competition, identifies competition as the source of all progress. If competition is good, then the default view on its opposite—monopoly—is that it must be very bad. Indeed, Adam Smith adopted this view in *The Wealth of Nations*:

People of the same trade seldom meet together, even for merriment and diversion, but the conversation ends in a conspiracy against the public, or in some contrivance to raise prices.

This insight is important, if only because it's so prevalent. But exactly why monopoly is bad is hard to tease out. It's usually just accepted as a given. But it's probably worth questioning in greater detail.

C. Testing for Monopoly

The Sherman Act declares: "The possession of monopoly power will not be found unlawful unless it is accompanied by an element of anticompetitive conduct." So in order to determine whether a monopoly is illegal or not, we just have to figure out what "anticompetitive conduct" means.

The DOJ has 3 tests for evaluating monopolies and monopoly pricing. First is the Lerner index, which gives a sense of how much market power a particular company has. The index value equals $(\text{price} - \text{marginal cost}) / \text{price}$. Index values range from 0 (perfect competition) to 1 (monopoly). The intuition that market power matters a lot is right. But in practice the Lerner index tends to be intractable with since you have to know market price and marginal cost schedules. But tech companies know their own information and should certainly pay attention to their Lerner index.

Second is the Herfindahl-Hirschman index. It uses firm and industry size to gauge how much competition exists in an industry. Basically, you sum the squares of the top 50 firms' market shares. The lower the index value, the more competitive the market. Values below 0.15 indicate a competitive market. Values from 0.15 to .25 indicate a concentrated market. Values higher than 0.25 indicate a highly concentrated and possibly monopolistic market.

Finally, there is the m-firm concentration ratio. You take either the 4 or 8 largest firms in an industry and sum their market shares. If together they comprise more than 70% of the market, then that market is concentrated.

D. The Good and Bad of Monopoly

First, the cons: monopolies generally produce lower output and charge higher prices than firms in competitive markets do. This may not hold true for some natural monopolies. And some industries have

monopolies of scale, which are a bit different. But monopolies generally get to be price setters, not price takers. There also might be price discrimination, since monopolists may capture more consumer surplus by charging different groups different prices. Another criticism is that monopoly stifles innovation; since it earns profits whether it innovates or not, a monopoly business might grow complacent and not develop any new technology.

But the innovation argument can go the other way too. Monopoly might net incentivize innovation. If a company creates something dramatically better than the next best thing, where's the harm in allowing it to price it higher than its marginal cost of production? The delta is the creators' reward for creating the new thing. Monopolistic firms can also conduct better long-term planning and take on deeper project financing, since there's a sense of durability that wouldn't exist in perfect competition where profits are zero.

E. Biases for Perfect Competition

An interesting question is why most people seem biased in favor of perfect competition. It's hard to argue that economists don't tend to idolize it. Indeed the very term "perfect competition" seems pregnant with some normative meaning. It's not called "insane competition" or "ruthless competition." That's probably not an accident. Perfect competition, we're told, is perfect.

To start, perfect competition may be attractive because it's easy to model. That probably explains a lot right there, since economics is all about modeling the world to make it easier to deal with. Perfect competition might also seem to make sense because it's economically efficient in a static world. Moreover, it's politically salable, which certainly doesn't hurt.

But the bias favoring perfect competition is a costly one. Perfect competition is arguably psychologically unhealthy. Every benefit social, not individual. But people who are actually involved in a given business or market may have a different view—it turns out that many people actually want to be able to make a profit. The deeper criticism of perfect competition, though, is that it is irrelevant in a dynamic world. If there is no equilibrium—if things are constantly moving around—you can capture some of the value you create. Under perfect competition, you can't. Perfect competition thus preempts the question of value; you get to compete hard, but you can never gain anything for all your struggle. Perversely, the more intense the competition, the less likely you'll be able to capture any value at all.

Thinking through this suggests that competition is overrated. Competition may be a thing that we're taught, and that we do, unquestioningly. Maybe you compete in high school. Then more, tougher competition in college and grad school. And then the "rat race" in the real world. An apt, though hardly unique example of intense professional competition is the Big Law model for young lawyers from top law schools. You graduate from, say, Stanford Law and then go work at a big firm that pays you really well. You work insanely hard to try and make partner until you either do or you don't. The odds aren't in your favor, and you'll probably quit before you get the chance to fail. Startup life can be tough, but also less pointlessly competitive. Of course, some people like the competitiveness of law firms. But it's probably safe to say that most don't. Ask anyone from the latter camp and they may well say that they never want to compete at anything again. Clearly, winning by a large margin is better than ruthless competition, if you can swing it.

Globalization seems to have a very competitive feel to it. It's like a track and field sprint event where one runner is winning by just a few seconds, with others on his heels. That's great and exciting if you're the spectator. But it's not a natural metaphor for real progress.

If globalization had to have a tagline, it might be that “the world is flat.” We hear that from time to time, and indeed, globalization starts from that idea. Technology, by contrast, starts from the idea that the world is Mount Everest. If the world is truly flat, it’s just crazed competition. The connotations are negative and you can frame it as a race to the bottom; you should take a pay cut because people in China are getting paid less than you. But what if the world isn’t just crazed competition? What if much of the world is unique? In high school, we tend to have high hopes and ambitions. Too often, college beats them out of us. People are told that they’re small fish in a big ocean. Refusal to recognize that is a sign of immaturity. Accepting the truth about your world—that it is big and you are just a speck in it—is seen as wise.

That can’t be psychologically healthy. It’s certainly not motivating. Maybe making the world a smaller place is exactly what you want to do. Maybe you don’t want to work in big markets. Maybe it’s much better to find or make a small market, excel, and own it. And yet, the single business idea that you hear most often is: the bigger the market, the better. That is utterly, totally wrong. The restaurant business is a huge market. It is also not a very good way to make money.

The problem is that when the ocean is really big, it’s hard to know exactly what’s out there. There might be monsters or predators in some parts who you don’t want to run into. You want to steer clear of the parts painted red by all the carnage. But you can’t do that if the ocean is too big to get a handle on. Of course, it *is* possible to be the best in your class even if your class is big. After all, *someone* has to be the best. It’s just that the bigger the class, the harder it is to be number one. Well-defined, well-understood markets are simply harder to master. Hence the importance of the second clause in the question that we should keep revisiting: what valuable company are *other people not building*?

F. On VC, Networks, and Closing Thoughts

Where does venture capital fit in? VCs tend not to have a very large pool of business. Rather, they rely on very discreet networks of people that they’ve become affiliated with. That is, they have access to a unique network of entrepreneurs; the network is the core value proposition, and is driven by relationships. So VC is anti-commoditized; it is personal, and often idiosyncratic. It thus has a lot in common with great businesses. The PayPal network, as it’s been called, is a set of friendships built over the course of a decade. It has become a sort of franchise. But this isn’t unique; that kind of dynamic arguably characterizes all great tech companies, i.e. last mover monopolies. Last movers build non-commoditized businesses. They are relationship-driven. They create value. They last. And they make money.

Peter Thiel's CS183: Startup - Class 4 Notes Essay

Here is an essay version of my class notes from Class 4 of CS183: Startup. Errors and omissions are my own. Credit for good stuff is Peter's entirely.

CS183: Startup—Notes Essay—April 11—The Last Mover Advantage

I. Escaping Competition

The usual narrative is that capitalism and perfect competition are synonyms. No one is a monopoly. Firms compete and profits are competed away. But that's a curious narrative. A better one frames capitalism and perfect competition as opposites; capitalism is about the accumulation of capital, whereas the world of perfect competition is one in which you can't make any money. Why people tend to view capitalism and perfect competition as interchangeable is thus an interesting question that's worth exploring from several different angles.

The first thing to recognize is that our bias favoring competition is deep-rooted. Competition is seen as almost quintessentially American. It builds character. We learn a lot from it. We see the competitive ideology at work in education. There is a sense in which extreme forms of competition are seen as setting one up for future, non-competitive success. Getting into medical school, for example, is extremely competitive. But then you get to be a well-paid doctor.

There are, of course, cases where perfect competition is just fine. Not all businesses are created to make money; some people might be just fine with not turning a profit, or making just enough to keep the lights on. But to the extent one wants to make money, he should probably be quite skeptical about perfect competition. Some fields, like sports and politics, are incredibly and perhaps inherently competitive. It's easier to build a good business than it is to become the fastest person alive or to get elected President.

It may upset people to hear that competition may not be unqualifiedly good. We should be clear what we mean here. Some sense of competition seems appropriate. Competition can make for better learning and education. Sometimes credentials do reflect significant degrees of accomplishment. But the worry is that people make a habit of chasing them. Too often, we seem to forget that it's genuine accomplishment we're after, and we just train people to compete forever. But that does everyone a great disservice if what's theoretically optimal is to manage to *stop* competing, i.e. to become a monopoly and enjoy success.

A law school anecdote will help illustrate the point. By graduation, students at Stanford Law and other elite law schools have been racking up credentials and awards for well over a dozen years. The pinnacle of post law school credentialism is landing a Supreme Court clerkship. After graduating from SLS in '92 and clerking for a year on the 11th Circuit, Peter Thiel was one of the small handful of clerks who made it to the interview stage with two of the Justices. That capstone credential was within reach. Peter was so close to winning that last competition. There was a sense that, if only he'd get the nod, he'd be set for life. But he didn't.

Years later, after Peter built and sold PayPal, he reconnected with an old friend from SLS. The first thing the friend said was, "So, aren't you glad you didn't get that Supreme Court clerkship?" It was a funny question. At the time, it seemed much better to be chosen than not chosen. But there are many reasons to doubt whether winning that last competition would have been so good after all. Probably it would have meant a

future of more insane competition. And no PayPal. The pithy, wry version of this is the line about Rhodes Scholars: they all had a great future in their past.

This is not to say that clerkships, scholarships, and awards don't often reflect incredible accomplishment. Where that's the case, we shouldn't diminish it. But too often in the race to compete, we learn to confuse what is hard with what is valuable. Intense competition makes things hard because you just beat heads with other people. The intensity of competition becomes a proxy for value. But value is a different question entirely. And to the extent it's not there, you're competing just for the sake of competition. Henry Kissinger's anti-academic line aptly describes the conflation of difficulty and value: in academia at least, the battles are so fierce because the stakes are so small.

That seems true, but it also seems odd. If the stakes are so small, why don't people stop fighting so hard and do something else instead? We can only speculate. Maybe those people just don't know how to tell what's valuable. Maybe all they can understand is the difficulty proxy. Maybe they've bought into the romanticization of competition. But it's important to ask at what point it makes sense to get away from competition and shift your life trajectory towards monopoly.

Just look at high school, which, for Stanford students and the like, was not a model of perfect competition. It probably looked more like extreme asymmetric warfare; it was machine guns versus bows and arrows. No doubt that's fun for the top students. But then you get to college and the competition amps up. Even more so during grad school. Things in the professional world are often worst of all; at every level, people are just competing with each other to get ahead. This is tricky to talk about. We have a pervasive ideology that intense, perfect competition makes the best world. But in many ways that's deeply problematic.

One problem with fierce competition is that it's demoralizing. Top high school students who arrive at elite universities quickly find out that the competitive bar has been raised. But instead of questioning the existence of the bar, they tend to try to compete their way higher. That is costly. Universities deal with this problem in different ways. Princeton deals with it through enormous amounts of alcohol, which presumably helps blunt the edges a bit. Yale blunts the pain through eccentricity by encouraging people to pursue extremely esoteric humanities studies. Harvard—most bizarrely of all—sends its students into the eye of the hurricane. Everyone just tries to compete even more. The rationalization is that it's actually inspiring to be repeatedly beaten by all these high-caliber people. We should question whether that's right.

Of all the top universities, Stanford is the farthest from perfect competition. Maybe that's by chance or maybe it's by design. The geography probably helps, since the east coast doesn't have to pay much attention to us, and vice versa. But there's a sense of structured heterogeneity too; there's a strong engineering piece, the strong humanities piece, and even the best athletics piece in the country. To the extent there's competition, it's often a joke. Consider the Stanford-Berkeley rivalry. That's pretty asymmetric too. In football, Stanford usually wins. But take something that really matters, like starting tech companies. If you ask the question, "Graduates from which of the two universities started the most valuable company?" for each of the last 40 years, Stanford probably wins by something like 40 to zero. It's monopoly capitalism, far away from a world of perfect competition.

The perfect illustration of competition writ large is war. Everyone just kills everyone. There are always rationalizations for war. Often it's been romanticized, though perhaps not so much anymore. But it makes sense: if life really is war, you should spend all your time either getting ready for it or doing it. That's the Harvard mindset.

But what if life isn't just war? Perhaps there's more to it than that. Maybe you should sometimes run away. Maybe you should sheath the sword and figure out something else to do. Maybe "life is war" is just a strange lie we're told, and competition isn't actually as good as we assume it is.

II. Lies People Tell

The pushback to all this is that, generally speaking, life really is war. Determining how much of life is actually perfect competition versus how much is monopoly isn't easy. We should start by evaluating the various versions of the claim that life is war. To do that, we have to be on guard against falsehood and distortion. Let's consider the reasons why people might bend the truth about monopoly versus competition in the world of technology.

A. Avoid the DOJ

One problem is that if you have a monopoly, you probably don't want to talk about it. Antitrust and other laws on this can be nuanced and confusing. But generally speaking, a CEO bragging about the great monopoly he's running is an invitation to be audited, scrutinized, and criticized. There's just no reason to do it. And if the politics problem is quite severe, there is actually strong positive incentive is to distort the truth. You don't just not say that you are a monopoly; you shout from the rooftops that you're not, even if you are.

The world of perfect competition is no freer from perverse incentives to lie. One truth about that world is that, as always, companies want investors. But another truth about the world of perfect competition is that investors should not invest in any companies, because no company can or will make a profit. When two truths so clash, the incentive is to distort one of them.

So monopolies pretend they're not monopolies while non-monopolies pretend they are. On the scale of perfect competition to monopoly, the range of where most companies fall is shrunk by their rhetoric. We perceive that there are only small differences between them. Since people have extreme pressure to lie towards convergence, the reality is probably more binary—monopoly or competitive commodity business—than we think.

B. Market Lies

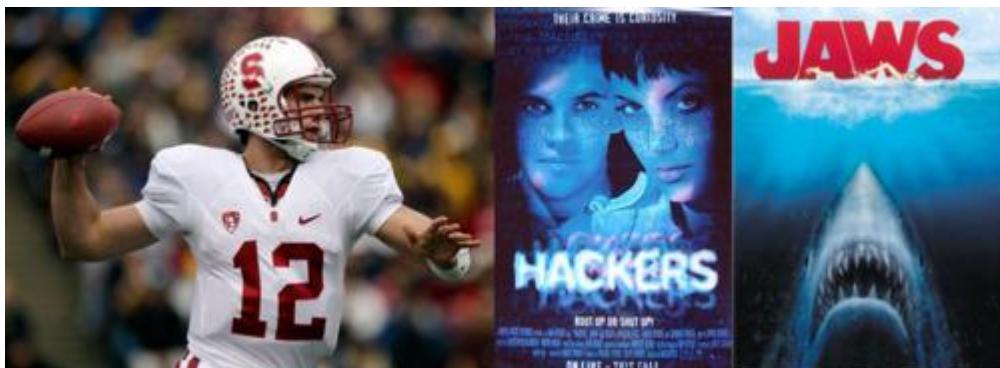
People also tell lies about markets. Really big markets tend to be very competitive. You don't want to be a minnow in a giant pool. You want to be best in your class. So if you're in a business that finds itself in a competitive situation, you may well fool yourself into thinking that your relevant market is much smaller than it actually is.

Suppose you want to start a restaurant in Palo Alto that will serve only British food. It will be the only such restaurant in Palo Alto. "No one else is doing it," you might say. "We're in a class of our own." But is that true? What is the relevant market? Is it the market for British food? Or the restaurant market in general? Should you consider only the Palo Alto market? Or do people sometimes travel to or from Menlo Park or Mountain View to eat? These questions are hard, but the bigger problem is that your incentive is not to ask them at all. Rather, your incentive is to rhetorically shrink the market. If a bearish investor reminds you that 90% of restaurants fail within 2 years, you'll come up with a story about how you're different. You'll spend time trying to convince people you're the only game in town instead of seriously considering whether that's

true. You should wonder whether there are people who eat only British food in Palo Alto. In this example, those are the only people you have pricing power over. And it's very possible that those people don't exist.

In 2001, some PayPal people used to go eat on Castro Street in Mountain View. Then, like now, there were all sorts of different lunch places. Whether you wanted Indian, Thai, Vietnamese, American, or something else, you had several restaurants to choose from. And there were more choices once you picked a type. Indian restaurants, for instance, divided into South Indian vs. not, cheaper vs. fancier. Castro Street was pretty competitive. PayPal, by contrast, was at that time the only e-mail based payments company in world. It employed fewer people than the Mountain View restaurants did. Yet from a capital formation perspective, PayPal was much more valuable than all the equity of all those restaurants combined. Starting a new South Indian food restaurant on Castro Street was, and is, a hard way to make money. It's a big, competitive market. But when you focus on your one or two differentiating factors, it's easy to convince yourself that it's not.

Movie pitches unfold in much the same way. Most of them are the same in that they all claim that *this* movie will be truly unique. This new film, investors are told, will combine various elements in entirely new ways. And that may even be true. Suppose we want to have Andrew Luck star in a cross between "Hackers" and "Jaws." The plot summary is: college football star joins elite group of hackers to catch the shark that killed his friend. That's definitely never been done before. We've had sports stars and "Hackers" and "Jaws," but never anything at the intersection of that Venn diagram. But query whether that intersection would be any good or not.



The takeaway is that it's important to identify how these rhetorical narratives work. Non-monopolies always narrow their market. Monopolies insist they're in a huge market. In logical operator terms, non-monopolies tell intersection stories: British food \cap restaurant \cap Palo Alto. Hometown hero \cap hackers \cap sharks. Monopolies, by contrast, tell union stories about tiny fishes in big markets. Any narrative that carries the subtext of "we're not the monopoly the government is looking for" will do.

C. Market Share Lies

There are all kinds of ways to frame markets differently. Some ways are much better than others. Asking what is the truth about a given market—and reaching as close to an objective answer as possible—is crucially important. If you're making a mobile app, you need to determine whether your market is apps on the iPhone, of which there are several hundred thousand, or whether there's a good way to define or create a very different, smaller market. But one must stay on guard against the sources of bias in this process.

Let's drill down on search engine market share. The big question of whether Google is a monopoly or not depends on what market it's in. If you say that Google is a search engine, you would conclude that it has 66.4% of the search market. Microsoft and Yahoo have 15.3% and 13.8%, respectively. Using the Herfindahl-Hirschman index, you would conclude that Google is a monopoly since 66% squared is well over 0.25.

But suppose you say that Google is an advertising company, not a search company. That changes things. U.S. search advertising is a \$16b market. U.S. online advertising is a \$31b market. U.S. advertising generally is a \$144b market. And global advertising is a \$412b market. So you would conclude that, even if Google dominated the \$16b U.S. search advertising market, it would have less than 4% of the global advertising market. Now, Google looks less like a monopoly and more like a small player in a very competitive world.

Or you could say that Google is tech company. Yes, Google does search and advertising. But they also do robotic cars. They're doing TV. Google Plus is trying to compete with Facebook. And Google is trying to take on the entire phone industry with its Android phone. Consumer tech is a \$964b market. So if we decide that Google as a tech company, we must view it in a different context entirely.

It's not surprising that this is Google's narrative. Monopolies and companies worried about being perceived as such tell a union story. Defining their market as a union of a whole bunch of markets makes them a rhetorical small fish in a big pond. In practice, the narrative sounds like this quotation from Eric Schmidt:

"The Internet is incredibly competitive, and new forms of accessing information are being utilized every day."

The subtext is: we have to run hard to stay in the same place. We aren't that big. We may get defeated or destroyed at any time. In this sense we're no different than the pizzeria in downtown Palo Alto.

D. Cash and Competition

One important data point is how much cash a company has on its balance sheets. Apple has about \$98b (and is growing by about \$30b each year). Microsoft has \$52b. Google has \$45b. Amazon has \$10b. In a perfectly competitive world, you would have to take all that cash and reinvest it in order to stay where you are. If you're able to grow at \$30b/year, you have to question whether things are really that competitive. Consider gross margins for a moment. Gross margins are the amount of profit you get for every incremental unit in marginal revenues. Apple's gross margins are around 40%. Google's are about 65%. Microsoft's are around 75%. Amazon's are 14%. But even \$0.14 profit on a marginal dollar of revenue is huge, particularly for a retailer; grocery stores are probably at something like 2% gross margins.

But in perfect competition, marginal revenues equal marginal costs. So high margins for big companies suggest that two or more businesses might be combined: a core monopoly business (search, for Google), and then a bunch of other various efforts (robotic cars, TV, etc.). Cash builds up because it turns out that it doesn't cost all that much to run the monopoly piece, and it doesn't make sense to pump it into all the side projects. In a competitive world, you would have to be funding a lot more side projects to stay even. In a monopoly world, you should pour less into side projects, unless politics demand that the cash be spread around. Amazon currently needs to reinvest just 3% of its profits. It has to keep running to stay ahead, but it's more easy jog than intense sprint.

III. How To Own a Market

For a company to own its market, it must have some combination of brand, scale cost advantages, network effects, or proprietary technology. Of these elements, brand is probably the hardest to pin down. One way to think about brand is as a classic code word for monopoly. But getting more specific than that is hard. Whatever a brand is, it means that people do not see products as interchangeable and are thus willing to pay more. Take Pepsi and Coke, for example. Most people have a fairly strong preference for one or the other. Both companies generate huge cash flows because consumers, it turns out, aren't very indifferent at all. They buy into one of the two brands. Brand is a tricky concept for investors to understand and identify in advance. But what's understood is that if you manage to build a brand, you build a monopoly.

Scale cost advantages, network effects, and proprietary technology are more easily understood. Scale advantages come into play where there are high fixed costs and low marginal costs. Amazon has serious scale advantages in the online world. Wal-Mart enjoys them in the retail world. They get more efficient as they get bigger. There are all kinds of different network effects, but the gist of them is that the nature of a product locks people into a particular business. Similarly, there are many different versions of proprietary technology, but the key theme is that it exists where, for some reason or other, no one else can use the technology you develop.

Apple—probably the greatest tech monopoly today—has all these things. It has complex combination of proprietary technology. By building both the hardware and the software, it basically owns the entire value chain. With legions of people working at Foxconn, it has serious scale cost advantages. Countless developers building on Apple platform and millions of repeat customers interacting with the Apple ecosystem provide the network effects that lock people in. And Apple's brand is not only some combination of all of these, but also something extra that's hard to define. If another company made an otherwise identical product, it would have to be priced less than the Apple version. Even beyond Apple's other advantages, the brand allows for greater monetization.

IV. Creating Your Market

There are three steps to creating a truly valuable tech company. First, you want to find, create, or discover a new market. Second, you monopolize that market. Then you figure out how to expand that monopoly over time.

A. Choosing the Right Market

The Goldilocks principle is key in choosing the initial market; that market should not be too small or too large. It should be just right. Too small a market means no customers, which is a problem. This was the problem with PayPal's original idea of beaming money on palm pilots. No one else was doing it, which was good. But no one really needed it done, which was bad.

Markets that are too big are bad for all the reasons discussed above; it's hard to get a handle on them and they are usually too competitive to make money.

Finding the right market is not a rhetorical exercise. We are no longer talking about tweaking words to trick ourselves or persuade investors. Creating your market has nothing to do with framing stories about intersections or unions. What is essential is to figure out the *objective truth* of the market.

B. Monopoly and Scaling

If there is no compelling narrative of what the market is and how it can scale, you haven't yet found or created the right market. A plan to scale is crucial. A classic example is the Edison Gower-Bell Telephone Company. Alexander Graham Bell developed the telephone, and with it, a new market. Initially, that market was very small; only a few people were involved in it. It was very easy to be the only one doing things in such a small, early market. They expanded. They kept expanding. The market became durable. Network effects began to operate. It quickly became very hard for others to break in.

The best kind of business is thus one where you can tell a compelling story about the future. The stories will all be different, but they take the same form: find a small target market, become the best in the world at serving it, take over immediately adjacent markets, widen the aperture of what you're doing, and capture more and more. Once the operation is quite large, some combination of network effects, technology, scale advantages, or even brand should make it very hard for others to follow. That is the recipe for building valuable businesses.

Probably every single tech company ever has fit some version of this pattern. Of course, putting together a completely accurate narrative of your company's future requires nothing less than figuring out the entire future of the world, which isn't likely to happen. But not being able to get the future exactly right doesn't mean you don't have to think about it. And the more you think about it, the better your narrative and better your chances of building a valuable company.

C. Some Examples

Amazon started very small. Initially, it was just going to be an online bookstore. Granted, becoming the best bookstore in the world, i.e. having all books in catalogue, is not a trivial thing to do. But the scale was very manageable. What is amazing about Amazon was that and how they were able to gradually scale from bookstore to the world's general store. This was part of the founding vision from the outset. The Amazon name was brilliant; the incredible diversity of life in the Amazon reflected the initial goal of cataloging every book in the world. But the elasticity in the name let it scale seamlessly. At a different scale, the Amazon's diversity also stood for every *thing* in the world.

eBay also started small. The idea was to build a platform and prepare for the unexpected. The first unexpected thing was the popularity of Pez dispensers. eBay became the single place where people who were into collecting all the various kinds of Pez dispensers could get them. Then came beanie babies. eBay soon became the only place in world where you could quickly get any particular beanie baby you wanted. Creating a marketplace for auctions lent itself to natural monopoly. Marketplaces are full of buyers and sellers. If you're buying, you go where the most sellers are. And if you're selling, you go to where the buyers are. This is why most companies list on just one stock exchange; to create liquidity, all buyers and sellers should be concentrated in the same place. And eBay was able to expand its marketplace to cover a surprisingly large number of verticals.

But eBay ran into problems in 2004, when it became apparent that that auction model didn't extend well to everything. That core monopoly business turned out to be an auction marketplace for somewhat unique products, like coins and stamps, for which there was intense demand but limited supply. The auction model was much less successful for commodity-like products, which companies like Amazon, Overstock, and Buy.com dealt in. eBay still turned out to be a great monopoly business. It's just a smaller one than people thought it would be in 2004.

LinkedIn has 61 million users in the U.S. and 150 million worldwide. The idea was that it would be a network for everyone. The reality is that it's largely just used for headhunting. Some have proposed a unique long/short strategy utilizing that insight: short the companies where lots of people are joining LinkedIn to post résumés and look for jobs, and go long the companies who are suspiciously quiet on LinkedIn. The big question about LinkedIn is whether the business network is the same as the social network. LinkedIn's narrative is that the business network is fundamentally discrete. If that's true, it will probably own that market for a long time.

Twitter is a classic example of starting with a small, niche product. The idea was simply that anyone can become a microbroadcaster. It works even if you just do it with a small number of people. But as it scales you basically build a new media distribution center. The big question about Twitter is whether it will ever make any money. That's not an easy question to answer. But if you ask the future tech questions—Do you have a technological advantage? Do you have a moat? Can people replicate this?—Twitter seems safe. If Twitter's market is the market for sending messages of 140 characters or less, it would be incredibly hard to replicate it. Sure, you can copy it. But you can't *replicate* it. Indeed, it's almost impossible to imagine a technological future where you can compete with Twitter. Move to 141 characters and you break SMS compatibility. Go down to 139 and you're just missing a character. So while monetization is an open question, Twitter's robustness and durability are hard to beat.

Zynga is another interesting case. Mark Pincus has wisely said that, "Not having clear goal at outset leads to death by a thousand compromises." Zynga executed very well from the beginning. They started doing social games like Farmville. They aggressively copied what worked, scaled, figured out how to monetize these games—how to get enough users to pay for in-game perks—better than anyone else did. Their success with monetization drove the viral loop and allowed them to get more customers quickly.

The question about Zynga is how durable it is. Is it a creative or non-creative business? Zynga wants the narrative to be that it's *not* a creative or a design company. If it is, the problem is that coming up with new great games is hard. Zynga would basically just be game version of a Hollywood studio whose fortunes can rise or fall with the seasons. Instead, Zynga wants the narrative to be about hardcore psychometric sauce. It's a better company if it's figured out how psychological and mathematical laws give it permanent monopoly advantages. Zynga wants, perhaps *needs*, to be able to truthfully say, "we know how to make people buy more sheep, and therefore we are a permanent monopoly."

Groupon also started small and scaled up aggressively. The questions for Groupon is what is the relevant market and how can they own it. Groupon insists it's a brand; it's penetrated to all these cities, and people look to it, not others, for deals. The anti-Groupon angle is that it has no proprietary technology and no network effects. If the branding isn't as strong as Groupon says it is, it will face lots of challenges in the long term.

All these companies are different, but the pattern is the same: start with a small, specific market, scale up, and always have an account of how robust you are going forward. The best way to fail is to invert this recipe by starting big and shrinking. Pets.com, Webvan, and Kozmo.com made this mistake. There are many modes of failure. But not being honest about objective market conditions is a sort of failure paradigm. You can't succeed by believing your own rhetoric over reality except by luck.

V. Tech Frontiers

There is always some room to operate in existing markets. Instead of creating a new market, you could “disrupt” existing industries. But the disruptive tech story is possibly overdone. Disruptive companies tend not to succeed. Disruptive kids get sent to principal’s office. Look at Napster. Napster was certainly disruptive...probably *too* disruptive. It broke too many rules and people weren’t ready for it. Take the name itself: *Napster*. It *sounds* disruptive. But what kinds of things can one “nap”? Music and kids. Yikes. Much better than to disrupt is to find a frontier and go for it.

But where is the frontier in technology? How should we begin to think about it? Here is one possible framework. Picture the world as being covered by ponds, lakes, and oceans. You’re in a boat, in a body of water. But it’s extremely foggy, so you don’t know how far it is to the other side. You don’t know whether you’re in a pond, a lake, or an ocean.

If you’re in a pond, you might expect the crossing to take about an hour. So if you’ve been out a whole day, you’re either in a lake or an ocean. If you’ve been out for a year, you’re crossing an ocean. The longer journey, the longer your expected remaining journey. It’s true that you’re getting closer to reaching the other side as time goes on. But here, time passing is also indicative that you still have quite a ways to go.

So where are the places where technology is happening? Where is there room for the journey to continue? The frontier is a promising place, but also a very uncertain one. You can imagine a tech market where nothing is happening for a long time, things suddenly start to happen, and then it all stops. The tech frontier is temporal, not geographical. It’s *when* things are happening.

Consider the automotive industry. Trying to build a car company in the 19th century was a bad idea. It was too early. But it’s far too late to build a traditional car company today. Car companies—some 300 of them, a few of which are still around—were built in 20th century. The time to build a car company was the time when car technology was being created—not before, and not after.

We should ask ourselves whether the right time to enter a tech industry is early on, as conventional wisdom suggests. The best time to enter may be much later than that. It can’t be too late, since you still need room to do something. But you want to enter the field when you can make the last great development, after which the drawbridge goes up and you have permanent capture. You want to pick the right time, go long on tech, succeed, and then short tech.

Microsoft is probably the last operating system company. It was also an early one, but there’s a sense in which it will be the last as well. Google, the narrative goes, is the last search engine company; it wrought a quantum improvement in search with its shift to an algorithmic approach, and that can’t be much improved on. What about bioinformatics? A lot seems to be happening there. But whether it’s too early to jump in is hard to know. The field seems very promising. But it’s difficult to get a sense of where it will likely be in 15 or 20 years. Since the goal is to build companies that will still be around in 2020, you want to avoid a field where things are moving too quickly. You want to avoid being an innovative but non-profitable disk drive company from the ‘80s.

Some markets are like the automotive market. Should you start a new lithium battery company? Probably not. The time for that may have passed. Innovation may be too slow. The technology may be too set by now.

But sometimes seemingly terminal markets aren’t. Look at aerospace. SpaceX thinks it can cut space launch costs by 70-90%. That would be incredibly valuable. If nothing has happened in an industry for a long time,

and you come along and dramatically improve something important, chances are that no one else will come and do that again, *to you*.

Artificial Intelligence is probably an underrated field. People are burned out on it, largely because it has been overrated and overstated for many decades. Few people think AI is or will soon be real at this point. But progress is increasingly relentless. AI performance in chess is increasing. Computers will probably beat humans in Go in 4 or 5 years. AI is probably a good place to look on the tech frontier. The challenge is that no one knows how far it will go.

Mobile Internet deserves some mention. The question is whether there's a gold rush in mobile. An important subquestion is whether, given a gold rush, you'd rather be a gold digger or the guy selling shovels to gold diggers. But Google and Apple are selling the shovels. And there may not be that much gold left to find. The worry is that the market is just too big. Too many companies are competing. As discussed above, there are various rhetorical tricks one can use to whittle down the market size and make any given company seem way more unique. Maybe you can create a mobile company that owns a valuable niche. Maybe you can find some gold. But that's worth being skeptical about.

VI. Frontiers and People

One way to tell whether you've found a good frontier is to answer the question "Why should the 20th employee join your company?" If you have a great answer, you're on the right track. If not, you're not. The problem is the question is deceptively easy sounding.

So what makes for a good answer? First, let's put the question in context. You must recognize that your indirect competition for good employees is companies like Google. So the more pointed version of the question is: "Why would the 20th engineer join your company when they could go to Google instead and get more money and prestige?"

The right answer has to be that you're creating some sort of monopoly business. Early businesses are driven by the quality of the people involved with them. To attract the best people, you need a compelling monopoly story. To the extent you're competing with Google for talent, you must understand that Google is a great monopoly business. You probably should not compete with them at their core monopoly business of search. But in terms of hiring, you simply can't compete with a great monopoly business unless you have a powerful narrative that has you becoming a great monopoly business too.

This raises the question that we'll discuss next week: kinds of people do you want to take with you as you head off into the frontier?

Peter Thiel's CS183: Startup - Class 5 Notes Essay

Here is an essay version of class notes from Class 5 of CS183: Startup. Errors and omissions are mine.

Stephen Cohen, co-founder and Executive VP of Palantir Technologies, and Max Levchin of PayPal and Slide fame joined this class as guest speakers. Credit for good stuff goes to them and Peter. I have tried to be accurate. But note that this is not a transcript of the conversation.

CS183: Startup—Notes Essay—The Mechanics of Mafia

I. Company Cultures

Everybody knows that company culture is important. But it's hard to know exactly what makes for an ideal culture. There are obviously some things that work. Even though they didn't necessarily look like a winning investment at the time, the early Microsoft team clearly got something right.



Then there are some things that don't work so well. A cult is perhaps the paradigmatic version of a culture that doesn't work. Cults are crazy and idealistic in a bad way. Cult members all tend to be fanatically wrong about something big.

And then there is what might be called anti-culture, where you really don't even have a culture at all. Consulting firms are the classic example here. Unfortunately, this is probably the dominant paradigm for companies. Most of the time, they don't even get to the point of having culture. People are mercenaries. People are nihilistic.

Picture a 1-dimensional axis from consultant-nihilism to cultish dogmatism. You want to be somewhere in the middle of that spectrum. To the extent you gravitate towards an extreme, you probably want to be closer to being a cult than being an army of consultants.

Good company culture is more nuanced than simple homogeneity or heterogeneity. On the homogeneity side, everyone being alike isn't enough. A robust company culture is one in which people have something in common that *distinguishes them quite sharply from rest of the world*. If everybody likes ice cream, that probably doesn't matter. If the core people share a relevant and unique philosophy about something important, you're onto something.

Similarly, differences qua differences don't matter much. In strong company cultures, people are different in a way that goes to the core mission. Suppose one key person is on an ice cream only diet. That's quirky. But it's also irrelevant. You want your people to be different in a way that gives the company a strong sense of identity and yet still dovetails with the overall mission. Having different kinds of problem-solvers on a team, for example, can make for a stronger culture.

II. Zero Sum vs. Not

A. To Fight or Not To Fight

Generally speaking, capitalism and competition are better seen as antonyms than as synonyms. To compete isn't what you should set out to do. That doesn't mean you should slack off. To succeed you probably need to work intensely. But you should work on something that others aren't doing. That is, focus on an area that's not zero-sum.

Sometimes, though, you need to compete. Monopoly is the theoretical ideal that you should always pursue. But you won't always find some non-competitive, cornucopian world. You may well find yourself in competitive, zero-sum situations. You must be prepared to handle that competition.

Gandhi is a great historical figure. He had many virtues. But he probably would not have been a great startup advisor. Consider the following quote:

"If [Hitler and Mussolini] choose to occupy your homes, you will vacate them. If they do not give you free passage out, you will allow yourselves, man, woman, and child, to be slaughtered, but you will refuse to owe allegiance to them."

Basically, the message is that you should demonstrate your superiority by allowing yourself to be slaughtered. Do not follow that advice while starting companies. You should try to avoid fighting, but where you must, you should fight and win.

B. Creators or Fighters?

In thinking about building good company culture, it may be helpful to dichotomize two extreme personality types: nerds and athletes. Engineers and STEM people tend to be highly intelligent, good at problem solving, and naturally non zero-sum. Athletes tend to be highly motivated fighters; you only win if the other guy loses. Sports can be seen as classically competitive, antagonistic, zero-sum training. Sometimes, with martial arts and such, the sport is literally fighting.

Even assuming everyone is technically competent, the problem with company made up of nothing but athletes is that it will be biased towards competing. Athletes like competition because, historically, they've been good at it. So they'll identify areas where there is tons of competition and jump into the fray.

The problem with company made up of nothing but nerds is that it will ignore the fact that there may be situations where you have to fight. So when those situations arise, the nerds will be crushed by their own naiveté.

So you have to strike the right balance between nerds and athletes. Neither extreme is optimal. Consider a 2 x 2 matrix. On the y-axis you have zero-sum people and non zero-sum people. On the x-axis you have warring, competitive environments (think Indian food joints on Castro Street or art galleries in Palo Alto) and then you have peaceful, monopoly/capitalist environments.

Most startups are run by non-zero sum people. They believe world is cornucopian. That's good. But even these people tend to pick competitive, warring fields because they don't know any better. So they get slaughtered. The nerds just don't realize that they've decided to fight a war until it's all over.

The optimal spot on the matrix is monopoly capitalism with some tailored combination of zero-sum and non zero-sum oriented people. You want to pick an environment where you don't have to fight. But you should bring along some good fighters to protect your non zero-sum people and mission, just in case.

C. Investor Heuristics

Founders Fund is a picky VC firm. There are many different types of companies that it doesn't like. The partners have developed maybe 20 or so different dogmas, each taking the form "Never invest in x." The "x" might be mobile internet, cleantech, etc. Sometimes it seems like there are so many dogmas that it's impossible to invest in anything anymore.

But always up for contrarian thinking, awhile back they made up a new strategy: identify and invest in the best company in or for every particular dogma. It's been more useful as a thought experiment than an actual strategy. But it led them to look at an interesting cleantech company they would've ordinarily skipped over. Though the space is extremely competitive and no one ever really makes any money, this particular company seemed reasonably good. It was run by scientists. It had great engineers and great technology. Everybody was passionate and committed to the mission. Talks of term sheets were in the air.

But then the cap table surfaced. It turned out that the founders and employees owned about 20% of the company. Other VC firms owned 80%. At the time, the company had a \$35m valuation, so it was still early stage. The equity breakdown seemed more like a mistake than a red flag. Many versions of the "what the hell happened?!" question were asked. The founders' response was nonchalant: "We are so committed to making the technology work that we didn't care about the equity." That may be a very noble. But it's also pretty bad. The subquestions it raised killed the deal: with such passivity, what are you going to do about your competitors? Can you even build a sales team? If you got run over so hard by *investors*, how are you going to fare against the entire world?

III. A Conversation with Stephen Cohen and Max Levchin

Peter Thiel: You guys have started companies. You've seen what's worked and what hasn't. Talk for a few minutes each. How do you build culture?

Stephen Cohen: Palantir makes analysis platforms aimed at governmental clients. But the founders knew from the outset that they ultimately wanted to make products for enterprise generally as well. Since that would take a long time to pull off, they knew that they needed really brilliant people working together under a shared long-term perspective. They knew that hiring tightly and wisely would be crucial from day one.

That early understanding reflected the three salient properties that inhere in good company culture. First, a company must have very talented people. Second, they must have a long-term time orientation. Third, there must be what might be called a generative spirit, where people are constantly creating. With this framework, hiring is more understandable: you just find people who have or contribute to all three properties. Culture is the super-structure to choose and channel people's energies in the right direction.

One error people make is assuming that culture *creates* these three aspects. Take a look at the Netflix company culture slides, for instance. They seem to indicate that you can produce talent from non-talent, or that you can take someone focused on the now and somehow transform them into long-term thinking. But you can't. Culture can always do more harm than good. It can reflect and enhance these three properties. It cannot create them.

From that insight comes the conclusion that hiring is absolutely critical. People you don't hire matter more than people you do hire. You might think that bad hiring decisions won't matter that much, since you can just fire the bad people. But Stalin-esque meritocracy sucks. Yes, you can shoot the bad people in the back of the head. But the problem with that is that you're still shooting people in the back of the head.

Peter Thiel: One early goal at PayPal was never to fire anybody. The founders just hired their friends since they could trust them. But eventually they had to hire more and more people who they knew less and less. They hired a sys admin from outside their network. It was trouble from the beginning; the guy showed up at 6pm on his first day of work. Worse than his tardiness was his lack of hygiene. The near-immediate objections people had were silenced by the founding rule: never fire anybody. A couple of months later, PayPal's systems crashed. The squalid sys admin hadn't made any backups. For a moment it looked like PayPal was done for. Luckily, some engineer went outside his job description and had decided to secretly back up everything everyday. Order was restored and the sys admin was fired. The "no-fire" rule still reflects a good orientation: firing people is like war, and war is bad, so you should try not to do it. But the flipside is that if you wait until it's obvious to everyone that someone should be fired, it's far too late.

Max Levchin: The notion that diversity in an early team is important or good is completely wrong. You should try to make the early team as non-diverse as possible. There are a few reasons for this. The most salient is that, as a startup, you're underfunded and undermanned. It's a big disadvantage; not only are you probably getting into trouble, but you don't even know what trouble that may be. Speed is your only weapon. All you have is speed.

So how do you move fast? If you're alone, you just work really hard and hope it's enough. Since it often isn't, people form teams. But in a team, an n -squared communications problem emerges. In a five-person team, there are something like 25 pairwise relationships to manage and communications to maintain. The more diverse the early group, the harder it is for people to find common ground.

The early PayPal team was four people from the University of Illinois and two from Stanford. There was the obligatory Russian Jew, an Asian kid, and a bunch of white guys. None of that mattered. What mattered was that they were not diverse in any important way. Quite the contrary: They were all nerds. They went to good schools. (The Illinois guys had done the exact same CS curriculum.) They read sci-fi. And they knew how to build stuff. Interesting to note is that they did *not* know how to build stuff the right way. It turned out that scaling up would be very challenging for PayPal because the 26 year-olds who were managing hundreds of thousands of credit cards didn't make all the optimal choices from the beginning. But there was great clarity in the early communications. There was no debate on how to build that first database. And that alone made it possible to build it.

Striving for optimality early on—debating pros and cons of various design decisions in intricate detail—would have doomed PayPal. When systems problems finally caught up to them, their communication was so good that they were able to fix them reasonably quickly. They kept hiring people from Illinois and Stanford. They focused on their network. And things worked out. But only because of a lack of diversity.

PayPal once rejected a candidate who aced all the engineering tests because for fun, the guy said that he liked to play hoops. That single sentence lost him the job. No PayPal people would ever have used the word “hoops.” Probably no one even knew how to play “hoops.” Basketball would be bad enough. But “hoops?” That guy clearly wouldn’t have fit in. He’d have had to explain to the team why he was going to go play hoops on a Thursday night. And no one would have understood him.

PayPal also had a hard time hiring women. An outsider might think that the PayPal guys bought into the stereotype that women don’t do CS. But that’s not true at all. The truth is that PayPal had trouble hiring women because PayPal was just a bunch of nerds! They never talked to women. So how were they supposed to interact with and hire them?

One good hiring maxim is: whenever there’s any doubt, there’s no doubt. It’s a good heuristic. More often than not, any doubt precluded a hire. But once this very impressive woman came to interview. There were some doubts, since she seemed reluctant to solve a coding problem. But her talk and demeanor—she insisted on being interviewed over a ping-pong game, for instance—indicated that she’d fit into the ubernerd, ubercoder culture. She turned out to be reasonably good at ping-pong. Doubts were suppressed. That was a mistake. She turned out to not know how to code. She was a competent manager but a cultural outsider. PayPal was a place where the younger engineers could and would sometimes wrestle with each other on the floor to solve disputes! If you didn’t get the odd mix of nerdiness + alpha maleness, you just stuck out.

Stephen Cohen: Good stuff shows itself. Talent shows itself. It doesn’t talk about itself. You must develop a sort of spidey sense to look out for it. Watch what people show you instead of listening to what they’re telling you. Seize on any doubt you find. It’s never personal. Never let the interview process become personal. But things get personal if you just listen to the other person. Don’t ask yourself what you think about what the candidate is saying. Just imagine the person you’re interviewing at work. Imagine them in a situation they’d be in if you were to hire them. How does *that* look?

Screening out personal biases is a must. A lot of programmers are dogmatic about syntax. Things have got to be laid out this particular way. Maybe they don’t like using factoring methods or something. But that’s a personal bias. It has nothing to do with being a good engineer. So those are the wrong questions to focus on. The right question is how badass they are. Smooth appearances are irrelevant to being good. The most

talented folks are almost always quirky. Watch for the quirks and embrace them. Nothing is stranger than watching a quirky entrepreneur harshly criticize another quirky entrepreneur for being too quirky.

A specific application of this is the anti-fashion bias. You shouldn't judge people by the stylishness of their clothing; quality people often do not have quality clothing. Which leads to a general observation: Great engineers don't wear designer jeans. So if you're interviewing an engineer, look at his jeans. There are always exceptions, of course. But it's a surprisingly good heuristic.

Max Levchin: The management team at PayPal was very frequently incompatible. Management meetings were not harmonious. Board meetings were even worse. They were certainly productive meetings. Decisions were made and things got done. But people got called idiots if they deserved it.

The next time around, at Slide, we tried to create a nicer environment. The idea of having meetings where people really liked one another seemed great. That was folly. The mistake was to conflate anger with a lack of respect. People who are smart and energetic are often angry. Not at each other, usually. Rather, they're angry that we're "not there yet," i.e. that they have to solve x when they should be working on some greater problem y . Disharmony at PayPal was actually a side effect of very healthy dynamics.

If people complain about people behind each other's backs, you have a problem. If people don't trust each other to do good work, you have a problem. But if people know that their teammates are going to deliver, you're good. Even if they are all calling each other idiots.

The danger is that you get soft. It's hard not to get soft as you train niceties. Pretty soon you spend more time thinking about how nice everyone is than you do about how qualified they are. That is death. If you think that an A- or B+++ person becomes an A person if they have a good personality, you are an idiot. The rest of the organization has already figured out that you're just being soft. They won't respect the non-A player. And they certainly won't respect you.

Even though people would physically fight on the engineering room floor, if you ever asked PayPal people if they respected each other, the answer was obvious. For a very long time, everyone believed in everyone else. That was not true at Slide. There, the subtle passive-aggressive lack of respect was allowed to develop too long. It proved very costly. At some point, there had to be a relatively significant bloodletting. It was stressful. The victims of the purge were so nice. It was easy to like them. It felt like a very bad, mean thing to do. But it was a good decision. Subsequently the company was run and performed much better. Yes, the love dissipated. But you knew that whatever remained was rooted in respect.

Peter Thiel: It is incredibly important to surface issues quickly. Ideally everyone in an organization is rowing in same direction. Ideally there's a strong, shared vision of the company's future. But at the micro level, details matter a lot. People will disagree about them often. When that happens, it simply must surface. Concealing disagreements because people feel uncomfortable makes for disaster. It doesn't fix things. They just sit undealt with, doing damage. Even in best of startups, a lot of chaotic things happen. If disagreements aren't surfacing, it's not because there are none. Key things are being covered up. Everyone moving together in lockstep is a big red flag, not an ideal.

The standard view is that companies get destroyed by external competition. Maybe that's true in the long run. But in the short run—and most that fail fail in the short run—they get destroyed internally. Even the best companies have ups and downs. If destructive relationships unravel and wreck havoc during a down, the

whole ship can blow up. Companies are not simple unitary entities in larger competitive ecosystems. They are complex entities with complex dynamics. Usually those dynamics blow up before some predator from the wider ecosystem strikes.

Stephen Cohen: You need to avoid people who are likely to blow things up. One key question to ask is: how does this person see themselves? One trendy answer that people seem to have is: I see myself as Steve Jobs. Absent context, someone seeing himself as the next Steve Jobs is neither bad nor good. It just is. But in context, it might be a disaster. If you have a team of 10 people trying to build product consensus, imagine what happens if all of them are Steve Jobs. It'd be a nightmare. At best you'd have nine pissed off people and one very insecure guy who got his way.

It's often telling to ask someone why they made the major decisions they did in the past. You can tell if they've processed the emotions behind those decisions. Someone who gets flustered or can't explain a job change may be carrying a lot of baggage. Someone who doesn't take responsibility for past moves will probably not change course and take responsibility in the future.

Max Levchin: Another good interviewing heuristic is to be very wary of salary negotiators. That you should run away from anyone who just wants salary instead of equity is entirely obvious. There is some nuance here, since a lot of people got burned on options during that last boom/bust cycle. But generally you want people to want stock. The best hires don't seem to care too much about money at all. They might ask whether a certain salary is market or not. That is reasonable; no one wants to get screwed. But you want people to care far more about equity. And best hires aren't wooed by an offer of a large number of shares. The best hires say "That's the numerator. What's the denominator?" The best people are the ones who care to ask: How much of the company is mine?

Some companies are sales-driven. You need to hire good salespeople. But that's hard to do, since those people are trained to sell. When they walk in the door, you're getting overwhelmed by phenomenal sales skills. It's hard to know what's real and what's not. So what should you do? The same thing people do for engineers: give them technical questions. Break them. Watch what happens when they break. You'll use lateral-thinking problems instead of algorithms questions, of course. But good sales people are just as smart as engineers, so you shouldn't give them a free pass. You need to build a team that has a lot of raw intelligence. So never slack on interviewing intensity just because the job isn't a technical one.

Peter Thiel: A good thing to do when hire sales people is to see how much they've sold in the past. But you have to apply some sort of discount rate because they don't always tell the exact truth. Scott Bannister of IronPort just asked sales people to submit their W-2s. Those with proclivity for exaggerating couldn't stump simple test of how much they'd made in commissions in the past.

Question from audience: Suppose you found a great engineer that's a good cultural fit. They are in high demand. How do make a compensation package that ensures you get that person?

Stephen Cohen: There's a crazy phenomenon with engineers. There is probably some sum of money you could pay to any engineer to work at Palantir and give it their all for one year. But there is no sum of money that you could pay any engineer to go all-out for ten years. Humans can't muster that amount of sustained focus and energy if they don't love what they're doing. The folks who fall in love aren't asking details about salary, trying to extract every penny. The ones who fall in love are just running. So insofar as money is an issue, you should get at exactly why. What does some particular compensation detail mean to the person?

Question: What if engineers are in love with something else, but you think they'd fall in love with your company if they were to join you?

Stephen Cohen: Reframe that question in a marriage context. Don't you think that would make for a higher than normal rate of divorce?

Peter Thiel: One thing that's undervalued in the engineering world is over how long a term most of the value is built. When eBay bought PayPal, all the PayPal engineers left. eBay had to hire them all back as consultants at something like 3x their old salaries because it couldn't manage the codebase without them. The engineers had acquired a tremendous amount of knowledge of PayPal's systems. Even really smart engineers couldn't replace them. So it's worth targeting people who will be around a long time.

The surest way to blow up a company is with a nuclear bomb: send out an e-mail to everybody that lists what each person is getting paid. You should not actually try this experiment. But it's worth doing it as a thought experiment. People will always be upset when they see what others are getting paid. That's a given. But *how upset* will they be? Will they be extremely upset? Would that be justified? Or could they be persuaded that things are quite reasonable?

Engineering compensation is difficult right now. You're competing with Google's prestige and money. The first step is to avoid competing on purely financial terms, where you're likely to lose. You have to have that compelling monopoly narrative that we discussed last week.

Max Levchin: Engineers are very cynical people. They're trained to be. And they can afford to be, given the large number of companies that are trying to recruit them in Silicon Valley right now. Since engineers think any new idea is dumb, they will tend to think that your new idea is dumb. They get paid a lot at Google doing some pretty cool stuff. Why stop indexing the world to go do your dumb thing?

So the way to compete against the giants is not with money. Google will outbid you. They have oil derrick that spits out \$30bn in search revenue every year. To win, you need to tell a story about cogs. At Google, you're a cog. Whereas with me, you're an instrumental piece of this great thing that we'll build together. Articulate the vision. Don't even try to pay well. Meet people's cash flow needs. Pay them so they can cover their rent and go out every once in awhile. It's not about cash. It's about breaking through the wall of cynicism. It's about making 1% of this new thing way more exciting than a couple hundred grand and a cubicle at Google.

Stephen Cohen: We tend to massively underestimate the compounding returns of intelligence. As humans, we need to solve big problems. If you graduate Stanford at 22 and Google recruits you, you'll work a 9-to-5. It's probably more like an 11-to-3 in terms of hard work. They'll pay well. It's relaxing. But what they are actually doing is paying you to accept a much lower intellectual growth rate. When you recognize that intelligence is compounding, the cost of that missing long-term compounding is enormous. *They're not giving you the best opportunity of your life.* Then a scary thing can happen: You might realize one day that you've lost your competitive edge. You won't be the best anymore. You won't be able to fall in love with new stuff. Things are cushy where you are. You get complacent and stall. So, run your prospective engineering hires through that narrative. Then show them the alternative: working at your startup.

Question: How does one preserve diversity of opinions in a startup?

Max Levchin: Sometimes diversity of opinion is valuable. Sometimes it's not. Some stuff needs to be off limits. There is some set of things that the founding team should decree is stupid to argue about. PayPal chose C++ early on. It's kind of crappy language. There's plenty to complain about. But the founding engineers never argued about it. Anyone that did want to argue about it wouldn't have fit in. Arguing would have impeded progress.

But arguing about smart marketing moves or different approaches to solving tactical or strategic problems is fundamental. These are the decisions that actually matter. Avoid groupthink in these areas is key. A good rule of thumb is that diversity of opinion is essential anytime you don't know anything about something important. But if there's a strong sense of what's right already, don't argue about it.

Peter Thiel: The relevant Keynes line here is "When the facts change, I change my mind. What do you do?" But you actually don't want to let every new fact call what you're doing into question. You're searching for a great business. What does that search space look like? Is it broad but shallow? Are you looking at every possible business you could do? Or are you focused on one area and drilling down on that?

The super broad, horizontal search is perhaps okay when you're thinking about starting a company initially. But returning to it at later stages is counterproductive. An internet company talking about being a cleantech company is lost. People tend to overrate the value of horizontal search and underestimate the sheer size of the search space. Far better is to understand how to do vertical search and to value depth over breadth.

Peter Thiel's CS183: Startup - Class 6 Notes Essay

Here is an essay version of my class notes from Class 6 of CS183: Startup. Errors and omissions are my own. Credit for good stuff is Peter's entirely. This class was kind of a crash course in VC financing. I didn't include all the examples since you can learn more about VC math elsewhere, e.g. [here](#) or [here](#). As usual, though, I've tried to include all the key insights from the lecture.

CS183: Startup—Notes Essay—Thiel's Law

I. Origins, Rules, Culture

Every company is different. But there are certain rules that you simply must follow when you start a business. A corollary of this is what some friends have (somewhat grandiosely) called Thiel's law: **A startup messed up at its foundation cannot be fixed.**

Beginnings of things are very important. Beginnings are qualitatively different. Consider the origin of universe. Different things happened than what we experience in everyday life. Or think about the origin of a country; it necessarily involves a great many elements that you do not see in the normal course of business. Here in the U.S., the Founders generally got a lot of things right. Some things they got quite wrong. But most of the time they can't really be fixed. Alaska has 2 senators. So does California. So Alaska, despite having something like 1/50th of California's population, has equal power in the Senate. Some say that's a feature, not a bug. Whatever it is, we're likely to be stuck with it as long as this country exists.



The insight that foundings are crucial is what is behind the Founders Fund name. Founders and founding moments are very important in determining what comes next for a given business. If you focus on the founding and get it right, you have a chance. If you don't, you'll be lucky at best, and probably not even that.

The importance of foundings is embedded in companies. Where there's a debate or controversial claim at Google, one says, "The Founders have scientifically determined that x is true," where x is his preferred

position. If you think that certain perks should be extended since happy people are the most productive, you say that Larry and Sergey have already settled the matter. The point is that all the science is done at the founding. No new data can interfere with the founding moment.

Foundings are obviously temporal. But how long they last can be a hard question. The typical narrative contemplates a founding, first hires, and a first capital raise. But there's an argument that the founding lasts a lot longer than that. The idea of going from 0 to 1—the idea of *technology*—parallels founding moments. The 1 to n of globalization, by contrast, parallels post-founding execution. It may be that the founding lasts so long as a company's technical innovation continues. Founders should arguably stay in charge as long as the paradigm remains 0 to 1. Once the paradigm shifts to 1 to n , the founding is over. At that point, executives should execute.

There is, of course, a limit to how much you can do with rules. Things can and will break down even with perfect rules. There is no real chance of setting things up correctly such that the rest unfold easily. But you should still get the early stuff as right as possible.

Consider a 2 x 2 matrix. On one axis you have good, high trust people and then you have low trust people. On the other axis you have low alignment structure with poorly set rules, and then a high alignment structure where the rules are well set.

Good, high trust people with low alignment structure is basically anarchy. The closest to this that succeeded is Google from 2000 to maybe 2007. Talented people could work on all sorts of different projects and generally operate without a whole lot of constraints.

Sometimes the opposite combination—low trust people and lots of rules—can work too. This is basically totalitarianism. Foxconn might be a representative example. Lots of people work there. People are sort of slaves. The company even installs suicide nets to catch workers when they jump off the buildings. But it's a very productive place, and it sort of works.

The low trust, low alignment model is dog-eat-dog sort of world. People who you might not trust can do a lot of whatever they want. An investment bank might be a good example. It's best to avoid this combination.

The ideal is the combination of high trust people with a structure that provides a high degree of alignment. People trust each other and together create a good culture. But there's good structure to it, too. People are rowing in the same direction, and not by accident.

Equity is one of the key ways to think about alignment in startups. Different groups share in a company's equity. Founders obviously get a stake. First they have to figure out how to allocate the equity amongst themselves. Angel investors also get equity. Early employees and advisors get equity. Later, series A investors will invest for equity too. And then you have your option pool. As this structure is built out and equity division occurs, the key is to think about how to get all the stakeholders aligned so that the company can succeed.

In this calculus, one factor dominates all others. That factor is whether the founders are aligned with each other. This is key both in terms of structure and company culture. If the founders are in sync, you can move on to the rest of the equation. But if they aren't, it will blow up the company. Nothing will work. This is why investors should and do focus so much on founding teams. Everything matters. How well the founders know

each other matters. How they interact and work with each other matters. Whether they have complimentary skillsets and personalities matters. This set of questions is very important. Any fissures in the founding team will be amplified later on.

One of Peter Thiel's first investments was in a company that Luke Nosek was starting back in 1998. The investment didn't go very well. Luke had met someone at a networking event and they decided to start a business together. The problem was that they had very different perspectives. Luke was this sort of chaotic, brilliant thinker. The other guy was very "by the books"—the kind of guy who had deliverables. It was doomed to fail. In a way choosing co-founders is like getting married. Getting married sometimes makes sense. But getting married to the first person you meet at the slot machines in Vegas probably doesn't. You might hit the jackpot. But chances are you won't. Good relationships amongst founders tend to drive a company's success. The question of the founding team is thus the single most important question in assessing an early startup. There are a couple different versions of it. How do the founders split up equity amongst themselves? How well do they work together?

Peter Thiel's CS183: Startup - Class 7 Notes Essay

Here is an essay version of class notes from Class 7 of CS183: Startup. Errors and omissions are mine.

Roelof Botha, partner at Sequoia Capital and former CFO of PayPal, and Paul Graham, partner and co-founder of Y Combinator, joined this class as guest speakers. Credit for good stuff goes to them and Peter. I have tried to be accurate. But note that this is not a transcript of the conversation.

Class 7 Notes Essay—Follow the Money

I. Venture Capital and You

Many people who start businesses never deal with venture capitalists. Founders who do interact with VCs don't necessarily do that early on. First you get your founders together and get working. Then maybe you get friends, family, or angels to invest. If you do end up needing to raise a larger amount of capital, you need to know how VC works. Understanding how VCs think about money—or, in some cases, how they don't think about it and thus lose it—is important.

VC started in late 1940s. Before that, wealthy individuals and families were investing in new ventures quite frequently. But the idea of pooling funds that professionals would invest in early stage companies was a product of the '40s. The Sand Hill road, Silicon Valley version came in the late 1960s, with Sequoia, Kleiner Perkins, and Mayfield leading the field.

Venture basically works like this: you pool a bunch of money that you get from people called limited partners. Then you take money from that pool and invest it in portfolio companies that you think are promising. Hopefully those companies become more valuable over time and everybody makes money. So VCs have the dual role of encouraging LPs to give them money and then finding (hopefully) successful companies to back.

Most of the profits go back to LPs as returns on their investment. VCs, of course, take a cut. The typical model is called 2-and-20, which means that the VC firm charges an annual management fee of 2% of the fund and then gets 20% of the gains beyond the original investment. The 2% management fee is theoretically just enough to allow the VC firm to continue to operate. In practice, it can end up being a lot more than that; a \$200m fund would earn \$4m in management fees under a 2-and-20 structure. But it's certainly true that the real payout that VCs look for come with the 20% cut of the gains, which is called the carry.

VC funds last for several years, because it usually takes years for the companies you invest in to grow in value. Many of the investments in a given fund either don't make money or go to zero. But the idea is that the companies that do well get you all your money back and then some; you end up with more money in the fund at the end than LPs put in to begin with.

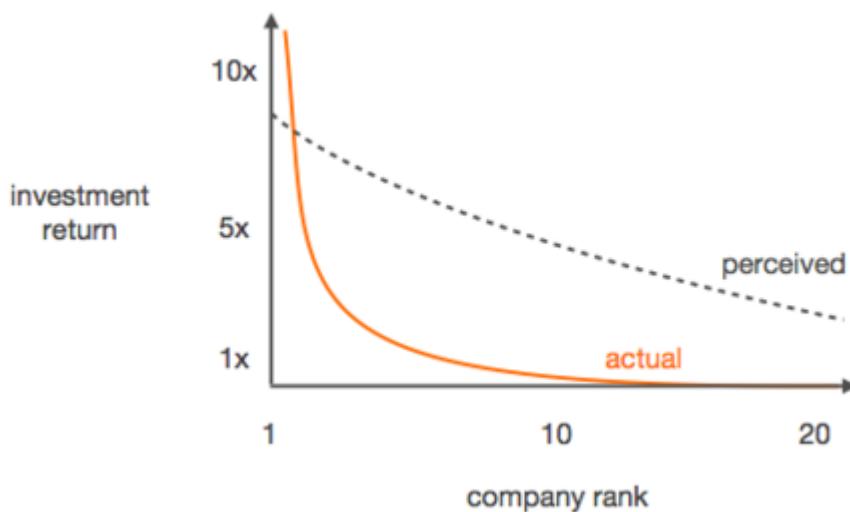
There are many dimensions to being a good VC. You have to be skilled at coming up with reasonable valuations, identifying great entrepreneurs, etc. But there's one dimension that is particularly important, yet surprisingly poorly understood. It is far and away the most important structural element of venture capital: exponential power. This may seem odd because it's just basic math. But just as 3rd grade arithmetic—knowing not just how many shares you get, but dividing that by the shares outstanding—was crucial to understand equity, 7th grade math—understanding exponents—is necessary to understand VC.

The standard Einstein line on this is that the most powerful force in universe is compound interest. We see the power of compounding when companies grow virally. Successful businesses tend to have an exponential arc to them. Maybe they grow at 50% a year and it compounds for a number of years. It could be more or less dramatic than that. But that model—some substantial period of exponential growth—is the core of any successful tech company. And during that exponential period, valuations tend to go up exponentially.

So consider a prototypical successful venture fund. A number of investments go to zero over a period of time. Those tend to happen earlier rather than later. The investments that succeed do so on some sort of exponential curve. Sum it over the life of a portfolio and you get a J curve. Early investments fail. You have to pay management fees. But then the exponential growth takes place, at least in theory. Since you start out underwater, the big question is when you make it above the water line. A lot of funds never get there.

To answer that big question you have to ask another: what does the distribution of returns in venture fund look like? The naïve response is just to rank companies from best to worst according to their return in multiple of dollars invested. People tend to group investments into three buckets. The bad companies go to zero. The mediocre ones do maybe 1x, so you don't lose much or gain much. And then the great companies do maybe 3-10x.

But that model misses the key insight that actual returns are *incredibly skewed*. The more a VC understands this skew pattern, the better the VC. Bad VCs tend to think the dashed line is flat, i.e. that all companies are created equal, and some just fail, spin wheels, or grow. In reality you get a power law distribution.



An example will help clarify. If you look at Founders Fund's 2005 fund, the best investment ended up being worth about as much as all the rest combined. And the investment in the second best company was about as valuable as number three through the rest. This same dynamic generally held true throughout the fund. This is the power law distribution in practice. *To a first approximation, a VC portfolio will only make money if your best company investment ends up being worth more than your whole fund.* In practice, it's quite hard to be profitable as a VC if you don't get to those numbers.

PayPal sold to eBay for \$1.5bn. PayPal's early stage investors had a large enough stake such that their investment was ultimately worth about the size of their fund. The rest of the fund's portfolio didn't do so

well, so they more or less broke even riding on PayPal. But PayPal's series B investors, despite doing quite well with the PayPal investment, didn't break even on their fund. Like many other VC funds in the early 2000's, theirs lost money.

That investment returns take a power law distribution leads to a few important conclusions. First, you need to remember that, management fees aside, you only get paid if you return all the money invested plus more. You have to at least hit the 100% of fund size mark. So given power law distribution, you have to ask the question: "Is there a reasonable scenario where our stake in this company will be worth more than the whole fund?"

Second is that, given a big power law distribution, you want to be fairly concentrated. If you invest in 100 companies to try and cover your bases through volume, there's probably sloppy thinking somewhere. There just aren't that many businesses that you can have the requisite high degree of conviction about. A better model is to invest in maybe 7 or 8 promising companies from which you think you can get a 10x return. It's true that in theory, the math works out the same if try investing in 100 different companies that you think will bring 100x returns. But in practice that starts looking less like investing and more like buying lottery tickets.

Despite being rooted in middle school math, exponential thinking is hard. We live in a world where we normally don't experience anything exponentially. Our general life experience is pretty linear. We vastly underestimate exponential things. If you backtest Founders Fund's portfolios, one heuristic that's worked shockingly well is that you should always exercise your pro rata participation rights whenever a smart VC was leading a portfolio company's up round. Conversely, the test showed that you should never increase your investment on a flat or down round.

Why might there be such a pricing inefficiency? One intuition is that people do not believe in a power law distribution. They intuitively don't believe that returns could be that uneven. So when you have an up round with a big increase in valuation, many or even most VCs tend to believe that the step up is too big and they will thus underprice it. The practical analogue would be to picture yourself working in a startup. You have an office. You haven't hit the exponential growth phase yet. Then the exponential growth comes. But you might discount that change and underestimate the massive shift that has occurred simply because you're still in the same office, and many things look the same.

Flat rounds, by contrast, should be avoided because they mean that the VCs involved believe things can't have gotten that much worse. Flat rounds are driven by people who think they might get, say, a 2x return from an investment. But in reality, often something has gone very badly wrong—hence the flat round's not being an up round. One shouldn't be mechanical about this heuristic, or treat it as some immutable investment strategy. But it actually checks out pretty well, so at the very least it compels you to think about power law distribution.

Understanding exponents and power law distributions isn't just about understanding VC. There are important personal applications too. Many things, such as key life decisions or starting businesses, also result in similar distributions. We tend to think about these things too moderately. There is a perception that some things are sort of better than other things, sometimes. But the reality is probably more extreme than that.

Not always, of course. Sometimes the straighter, perceived curve actually reflects reality quite closely. If you were to think about going to work for the Postal Service, for example, the perceived curve is probably right.

What you see is what you get. And there are plenty of things like that. But it's also true that we are, for some reason or other, basically trained to think like that. So we tend to miscalculate in places where the perceived curve does not, in fact, accurately reflect reality. The tech startup context is one of those places. The skew of distributions for tech startups is really vast.

This means that when you focus on the percentage of equity you get in a company, you need to add a modifier: given something like a power law distribution, where your company is on that curve can matter just as much or more than your individual equity stake.

All else equal, having 1% of a company is better than having 0.5%. But the 100th employee at Google did much better than the average venture-backed CEO did in the last decade. The distribution is worth thinking hard about. You could spin this into argument against joining startups. But it needn't go that far. The power law distribution simply means you have to think hard about a given company is going to fall on the curve.

The pushback to this is that the standard perception is reasonable—or at least is not unreasonable—because the actual distribution curve turns out to be random. The thesis is that you are just a lottery ticket. That is wrong. We will talk about why that is wrong later. For now, it's enough to point out that the actual curve is a power law distribution. You don't have to understand every detail and implication of what that means. But it's important to get some handle on it. Even a tiny bit of understanding of this dimension is incredibly valuable.

II. The View from Sand Hill Road

Peter Thiel: One thing we should talk about is what secrets VCs use to make money. Well, actually most don't make money. So let's talk about that.

Roelof Botha: The unprofitability of venture capital is pretty well documented. Average returns have been pretty low for a number of years now. One theory is that when venture was doing very well in the 1990s, it became a big deal to more or less blindly follow advice to put more money in venture. So the industry may be overinvested, and it's hard for most firms to make money.

Peter Thiel: Paul, what can entrepreneurs do to avoid getting taken advantage of by VCs?

Paul Graham: There's nothing inherently predatory about VC. Y-Combinator is a minor league farm club. We send people on *up* to VCs. VCs aren't evil or corrupt or anything. But in terms of getting a good deal and not a bad one, it's the same with any deal; the best way to get a good price is to have competition. VCs have to be competing to invest in you.

Peter Thiel: We've discussed in this class how competition can be a scary thing. Maybe it's less bad when you make VCs compete against each other. In practice, you never really land just one investor. Chances are you have at least two people who are interested or you have zero. The cynical explanation of this is that most VCs have little or no confidence in their ability to make decisions. They just wait to ape others' decisions.

Paul Graham: But investors also have an interest to wait, if they can. Waiting means that you're able to get more data about a given company. So waiting is only bad for you if founders raise the price while you wait. VCs are looking for startups that are the next Google, or not. They are cool with 2x returns. But more than that they don't want to lose a Google.

Peter Thiel: How do you avoid being a VC that loses money?

Roelof Botha: Since the distribution of startup investment outcomes follows a power law, you cannot simply expect to make money by simply cutting checks. That is, you cannot simply offer a commodity. You have to be able to help portfolio companies in a differentiated way, such as leveraging your network on their behalf or advising them well. Sequoia has been around for more than 40 years. You cannot get the returns that we have if you are just providing capital.

Paul Graham: The top VC funds have to be able to make up their own minds. They cannot follow everybody else because it's everyone that follows them! Look at Sequoia. Sequoia is very disciplined. This is not a bunch of B-school frat boys who are screening founders for guys who look like Larry and Sergey. Sequoia prepares careful research documents on prospective investments...

Roelof Botha: But *succinct* research. If you make or believe you need a 100-page document, you miss the forest for trees. You must be able to condense it into 3-5 pages. If there can be no succinct description, there's probably nothing there.

Peter Thiel: Even within an individual business, there is probably a sort of power law as to what's going to drive it. It's troubling if a startup insists that it's going to make money in many different ways. The power law distribution on revenues says that one source of revenue will dominate everything else. Maybe you don't know what that particular source is yet. But it's certainly worth thinking about. Making money with A is key. Making money with A through E is terrifying, from an investor's perspective.

Roelof Botha: LinkedIn is exception that proves the rule there. It had 3 revenue streams that are pretty equal. No one else really has that. At least it's very unusual.

Peter Thiel: Do Y-Combinator companies follow a power law distribution?

Paul Graham: Yes. They're very power law.

Peter Thiel: Incubators can be tricky. Max Levchin started one. It had a really long cycle—maybe even a year-long cycle. That made for some crazy intercompany dynamics. All these people start in similar boats but, because of the power law dynamic, end up in very different ships. The perceptions are quite jarring. What happens with different people as they reach these different stages can be very complicated.

Roelof Botha: People don't always appreciate or understand rapid increases in value when businesses take off. They underestimate the massive asymmetry of returns. They hear that a company has joined the billion-dollar club and are perplexed because only 6 months ago, it was worth \$200m. The alternative to understanding the exponential growth is believing that Silicon Valley VCs have gone crazy.

Peter Thiel: PayPal's most successful up round resulted in a 5x increase in valuation. But it was pitched in a forward-looking context. It wasn't about taking x and multiplying by 5. The narrative was that the valuation made sense because of the promising future ahead. The real value is always in the future. Absent a very

specific future you can point to, people anchor to a very specific past. And that is where you get the pushback of: “How can it possibly be worth 5x what it was 3 months ago?”

Paul Graham: You could even say that the whole world is increasingly taking power law shape. People are broken into so many different camps now. If everyone were forced to work for 1 of 10 GM-like companies—maybe like Japan—it would straighten the power law curve and make it taught. Distributions would be clustered together because everyone is bound together. But when you have lots of slack and people break apart, extremes form. And you can bet on this trend continuing in the future.

Roelof Botha: One thing that people struggle with is the notion that these massive companies can be built very quickly, often seemingly overnight. In the early PayPal days, there were perhaps 300 million internet users. Now there are 2 billion. We have more mobile phones. We have cloud computing. There are so many ways to grow. Consequently there is a qualitative difference in one’s ability to have such a huge impact as an entrepreneur.

Question from audience: Do up, flat, and down rounds reflect power law distributions, or specifically where a company will fall on the distribution?

Peter Thiel: First, it’s important to note that when you join or start a startup, you’re investing in it. All your eggs are in that basket. But because of the power law distribution, your investors aren’t in a radically different place than you are. In a sense, VCs’ eggs are in your basket too. They have a few more baskets than you do, but again, because of the power law, not many. VC isn’t private equity where you shoot for consistent 2x or 3x returns.

One way to rephrase the question would be: is there a market inefficiency here? My backtesting claim is that one should do a full pro rata investment whenever one of your companies does an up round led by a smart VC.

Roelof Botha: I don’t have the data you’re looking at, but my intuition is that’s true. But only for the best VCs. Where the VC leading the round isn’t as smart or as trusted, the reverse can happen. Companies can end up with too much cash. They might have a 15-month runway. They get complacent and there’s not enough critical thinking. Things go bump at 9 months and it turns into a crisis. And then no one wants to invest more.

Peter Thiel: Even factoring in dilution, you tend to do quite well if every round is an up round. But even a single down round tends to be disastrous, mainly because it destroys relationships among all the relevant players. If you’re going to go with a not-so-intelligent investor who gives you a really huge valuation, you should take it only if it’s the last money you’re going to take.

Question from audience: Does the shape of the distribution curve change or depend on the time or stage of the investment?

Peter Thiel: The curve is fairly fractal-esque all the way up. Founders Fund tries to invest in 7 to 10 companies per fund. The goal is to get to 10x return. How hard is it to get to 10x? It’s about as hard to get from \$10m to \$100m as it is from \$100m to \$1bn or \$1bn to 10bn. Taking \$100bn to a trillion is harder because the world isn’t that big. Apple’s market cap is \$500bn. Microsoft’s is \$250bn. There’s a pretty incredible power law all the way up.

The same is probably true on angel level. The angel investment landscape is sort of saturated for angel piece, especially now with the JOBS Act. But some would say that angel investors are less aware of power law dynamics than other people are, and so they tend to overestimate a given company as a result.

Roelof Botha: There is a 50% mortality rate for venture-funded businesses. Think about that curve. Half of it goes to zero. There are some growth investments—later stage investments—which makes things less drastic. Some people try for 3-5x returns with a very low mortality rate. But even that VC model is still subject to power law. The curve is just not as steep.

Question from audience: What if your business is just worth \$50m and you can't grow it any more?

Paul Graham: That assumption is nonsense. Grow it, if you want to. There's no such thing as an immutable company size. Companies are not intrinsically or inherently limited like that. Look at Microsoft or Apple. They started out making some small thing. Then they scaled and branched out as they succeeded.

To be clear, it's totally cool to have low aspirations. If you just want to make a \$50m company, that's great. Just don't take venture capital, or at least don't tell VCs about your plans!

Peter Thiel: It would raise a big red flag if you were to put a slide at the end of your deck that says you're looking to sell the company for \$20m in 18 months.

Question from audience: What happens when you take out a bunch of rounds and things don't go well and your current investors don't want to put in more?

Paul Graham: In that scenario you are essentially wasting one of your investors' board seats. Their opportunity cost of having you going sideways is very high. People can only stand being on a dozen or so boards. Any more than that and they go crazy. So they'll try to get you sold.

Peter Thiel: Such unequal outcomes produces another cost of ending up on multiple boards. There are big reputational costs to just switching boards. So there is a big disconnect between public branding—narratives about how VCs pay loving attention to all their companies and treat them all equally—and the reality of the power law.

Roelof Botha: And it can be even worse than that; the problem companies can actually take up *more* of your time than the successful ones.

Peter Thiel: That is a perverse misallocation. There are differing perspectives on what to do in these situations. At one extreme, you just write checks and check out. At the other, you help whoever needs it as much as they need it. The unspoken truth is that the best way to make money might be to promise everyone help but then actually help the ones who are going to provide the best returns.

Question from audience: Bill Gates took no funding and he ended up with a large piece of Microsoft. If a startup can bootstrap instead of take venture capital, what should it do?

Paul Graham: VC lets you borrow against future growth. You could wait until your revenues are high enough to fund x . But, if you're good enough, someone will give you money to do x now. If there's

competition, you may need to do *x* quickly. So if you don't screw things up, VC can often help you a great deal.

Roelof Botha: We would not be in the business if it were just writing checks. The entrepreneurs make it happen; they are the ones building the companies. But the board and VCs can roll up their sleeves, offer counsel, and assist as needed. They can be there for the entrepreneurs. We shouldn't overstate the importance of that, but neither should we dismiss it.

Paul Graham: Just being backed by a big VC firm will help you open lots of doors. It will help considerably with your hiring.

Peter Thiel: If you're doing something where you don't need to move as quickly as possible, you might want to rethink taking venture funding. But if there's any sort of winner-take-all dynamic—if there is a power law distribution at play, then you want VC. Giving up 25% of your business is worth it if it enables you to take over your industry.

Question from audience: Do Sequoia or other top-tier VC firms offer tougher term sheets to account for the extra value they provide? Is all the stuff about non-monetary value-add just overplayed?

Roelof Botha: It's not overplayed. It really is personal. Who are you getting in business with? Can you trust them? I wouldn't send my brother to most VC firms. But some are great. You really have to get to know the people you might be working with. You're essentially entering a long-term relationship.

Just look at you. There's information contained in your Stanford degree. The signaling helps you quite a bit. The same is true if you're backed by certain VCs. There's a lot of value in the name, independent of things like making important introductions. And strategic direction is hard to pinpoint, but it can accumulate in many interesting and beneficial ways. Even if we don't have the answers, we have probably seen similar problems before and we can help entrepreneurs think through the questions.

Question from audience: Right now, entrepreneurs are trying to flip companies for \$40m in 2 years or less. The incentive is to flip easy stuff instead of create hard technical stuff. What's the cause and what's the effect? Entrepreneur greed? VCs who don't value technical innovation?

Paul Graham: I disagree with the premise that there's a lack of innovation. \$50m companies innovate. Mine did. We basically invented the web app. We were doing complex stuff in LISP when everyone else was doing CGI scripts. And, quite frankly, \$50m is no small thing. We can't *all* get bought for \$1.5bn, after all... [looks at Peter].

Peter Thiel: Let me rephrase the question: are VCs looking for quicker profits? Are we getting thinner companies that we should be?

Paul Graham: I don't think investors have too much effect on what companies actually do. They don't push back and say no, do this cool thing *x* instead of that dumb thing *y*. Of course, tons of people just try and imitate what they see and think is easy. Y-Combinator is probably going to be filtering out thousands of Instagram-like applications next cycle.

Roelof Botha: If someone came to me and I got the sense that he was trying to just flip a company quickly, I'd run. But most founders aren't B-school finance mechanics who calculate exactly what space would be most profitable to enter. Most good founders are people solving problems that frustrate them. Google grew out of a research project stemming from frustration with AltaVista.

Peter Thiel: One strange corollary to the power curve dynamic is that the people who build the really great companies are usually hesitant to sell them. Almost necessarily that's the case. And it's not for lack of offers. Paradoxically, people who are heavily motivated by money are never the ones who make the most money in the power law world.

Question from audience: If the most money comes from people who aren't trying to make the most money, how do you handle that paradox as a VC?

Roelof Botha: Consider a simple 2 x 2 matrix: on one axis you have easy to get along with founder, and not. On the other, you have exceptional founder, and not. It's easy to figure out which quadrant VCs make money backing.

Question from audience: If the power law distribution is so extreme, how can Y-Combinator succeed?

Paul Graham: There is a very steep drop-off. Y Combinator essentially gets the first pick of a very good national and even international applicant pool.

Peter Thiel: I won't come out as pro- or anti- Y Combinator. They do some things well and maybe some other things less well. But I will something anti-not-Y Combinator. If you go to incubator that's not Y Combinator, that is perceived as negative credential. It's like getting a degree at Berkeley. Okay. It's not Stanford. You can a complicated story about how you had to do it because your parents had a big mortgage or something. But it's a hard negative signal to get past.

Question from audience: Do you back founders or ideas?

Paul Graham: Founders. Ideas are just indicative of how the founders can think. We look for relentlessly resourceful people. That combination is key. Relentlessness alone is useful. You can relentlessly just bang your head against the wall. It's better to be relentless in your search for a door, and then resourcefully walk through it.

Roelof Botha: It is so rare to find people who can clearly and concisely identify a problem and formulate coherent approach to solve it.

Peter Thiel: Which is why it's very important to drill down on the founding team.

Roelof Botha: You can discover a lot about founders by asking them about their choices. What are the key decisions you faced in your life and what did you decide? What were the alternatives? Why did you go to this school? Why did you move to this city?

Paul Graham: Another corollary to the power law is that it's OK to be lame in a lot of ways, so long as you're not lame in some really important ways. The Apple guys were crazy and really bad dressers. But they got importance of microprocessors. Larry and Sergey got that search was important.

Peter Thiel: Isaiah Berlin wrote an essay called “The Hedgehog and the Fox.” It revolved around a line from an ancient Greek poet: foxes know many little things, but hedgehogs know one big thing. People tend to think that foxes are best because they are nimble and have broad knowledge. But in business, it’s better to be a hedgehog if you have to choose between the two. But you should still try and know lots of little things too.

Question from audience: You mentioned “smart VCs” in your backtesting example. Who are the smart VCs?

Peter Thiel: The usual suspects. Next question.

Question from audience: What keeps you guys up at night? What do you fear most?

Paul Graham: I fear that something will come along that causes me personally to have to do a lot more work. What’s your greatest fear, Roelof? Andreessen Horowitz?

Roelof Botha: Suffice it to say that you’re only as good as your next investment.

Question from audience: Can entrepreneurs raise venture capital if they’ve raised and failed before?

Paul Graham: Yes.

Roelof Botha: Max fell twice before PayPal, right? Here, it’s a myth that failure is stigmatized. In some places, such as France, that is true. Failure is looked down upon. But much less so in the U.S., and in Silicon Valley in particular.

Peter Thiel: One still shouldn’t take failure lightly, though. There is still a reasonably high cost of failure.

Paul Graham: It largely depends on *why* one failed, though. Dalton Caldwell got killed by the music business. Everyone knows that wasn’t his fault. It’s like getting shot by the mafia. You can’t be blamed for it.

Roelof Botha: Sometimes having experience with failed startups can make an entrepreneur even better. If they learn from it, maybe they get inspiration or insight for their next company. There are plenty of examples. But you should not fail for sake of failure, of course.

Question from audience: Do you fund teams of 1?

Paul Graham: Yes. Drew Houston was a team of one. We suggested that he find a co-founder. He did. It worked well.

Peter Thiel: A core founding team of two people with equal shares tends to work very well. Or sometimes it makes sense to have one brilliant founder that’s far and away above anyone else.

Paul Graham: Four is too many.

Peter Thiel: Think about co-founders from a power law perspective. Having one means giving up half the company. Having two means giving up 2/3. But if you pick the right people, it's likely that the outcome will be more than 2x or 3x what it would've been without them. So co-founders work pretty well in power law world.

Peter Thiel's CS183: Startup - Class 8 Notes Essay

Here is an essay version of class notes from Class 8 of CS183: Startup. Errors and omissions are mine.

Bruce Gibney, partner at Founders Fund, gave the lecture these notes are based on. Credit for good stuff goes to him and Founders Fund.

Class 8 Notes Essay—The Pitch

I. Pitching Context & Goals

One of the most important things to remember when thinking about pitching is that there are huge numbers of pitches in the world. Venture capitalists hear quite a few of them. And they find the process frustrating because it is such a low yield activity (a tiny fraction of first pitches lead to subsequent diligence and even fewer of those lead to a deal). So if you want VCs to listen to you, you need to *force* them to listen—to break through the clutter. Doing so requires you to hack into the VC mind.

Conceptually, pitching sounds easy. You are smart. You have a great idea and you tell people with money that great idea. They're rational; they give it to you.

But it's not that easy. What you essentially have to do is convince a reasonably smart person to exchange his capital for your piece of paper (a stock certificate) that is really nothing more than a promise about something that *may* be valuable later but, on a blind statistical basis, probably won't be. It turns out that this is difficult.

Humans are massively cognitively biased in favor of near-term thinking. VCs are no different. That's curious, because you'd think they would have overcome it, since good long-term thinking is sort of the entire nature of venture capital. But humans are humans. VCs are just sacks of meat with the same cognitive biases as everyone else. They are rational systems infected with emotional viruses (and infused with a tinge of wealth and privilege and all that implies). You must address both sides of their brains; you have to convince VCs that your proposal is economically rational, and then you must exploit their reptilian brains by persuading their emotional selves into doing the deal and overcoming cognitive biases (like near-term focus) against the deal. You should also offer VCs entertainment. They see several pitches a day (most bad) and that gets boring. Be funny and help your cause. In the tech community, even one joke will suffice.

Before you pitch you should have a clear goal in mind. What are you seeking to achieve? At first it seems obvious. The vulgar answer is that you're looking for money. Lots of money, at the highest possible valuation, wired to you as quickly as possible. But that's not quite right. You will be better off if you consider the many nuances to a raise.

First, you need to raise the *right* amount of capital. A small company shouldn't raise 100 million dollars, even if Great Late Stage Fund is very eager to cut you a check. Raising too much can haunt you. Map out your operating expenses for one year, multiply that figure by 1.5, and ask for that, as a first approximation.

Second, higher valuations aren't always in your interest. Valuations that are too high will deter other VC firms from investing. And they will expose you to all sorts of problems regarding compensation and expected future returns for your employees and investors. Be prepared to expect an offer that's objectively

good, even if it's emotionally unsatisfying. On the other hand, valuations that are really low are obviously bad as well, since they mean that you get either got screwed or that there is something wrong with your idea.

Your subsidiary goal should be to keep control of your enterprise. This is very important. Some things you can't change very easily once they're set. You can't really change your core values. You can't really dump co-founders, unless you want to pay through the nose to do so. But hardest to get change is your VC; once they're on your board, they're there for good. So you have to choose very wisely. Think carefully about this as you put together a list of VCs to pitch. To the extent you want to keep control, e.g., you should perhaps shy away from certain firms who are more or less serial killers bent on replacing CEOs. Be careful about your voting structures, as well; these too are hard to change.

II. Know Your Audience

It's always important to understand your audience. To be clear, there are a few very successful, hyper-rational VCs who can see a great business even through the murk of a terrible pitch. If you are lucky enough to find one, no tricks or optimization are necessary. But such VCs are the very small minority and even they have bad days, so playing to your audience is always a sound strategy. You need to psychoanalyze your prospective VCs. Try to understand things from their perspective and present accordingly.

One of the most important things to understand is that, like all people, VCs are different people at different times of day. It helps to pitch as early as possible in the day. This is not a throwaway point. Disregard it at your peril. A study of judges in Israel doing parole hearings showed prisoners had a two-thirds chance of getting parole if their hearing was early in the day. Those odds decreased with time. There was a brief uptick after lunch—presumably because the judges were happily rested. By the end of the day people had virtually no chance of being paroled. Like everyone, VCs make poorer decisions as they get tired. Come afternoon, all they want to do is go home. It does indeed suck to have to wake up early to go pitch. But that is what you must do. Insist that you get on the calendar early.

A related point: It's also important not to provide too much choice. Contrary to the standard microeconomics literature which extols the virtues of choice, empirical studies show people are actually made unhappy by a lot of choice. Too many choices makes for Costco Syndrome and mental encumbrance. By the end of the day, the VCs have had a lot of choices. So in addition to getting to them early in the day (before they've had to make a lot of choices), you should keep your proposition simple. When you make your ask, don't give them tons of different financing options or packages or other attempts at optimization. That will burden them with a cognitive load that will make them unhappy. Keep it simple.

Finally, you should avoid being blinded by entrepreneurial optimism. The default thinking is seductive but too simplistic: you've created something wonderful, VCs like to invest in wonderful things, and therefore VCs will be desperate to invest in your wonderful thing. That's wrong. It's easy to fool yourself here. After all, it's a VCs job to wake up in the morning and deploy capital, right? True, but an interesting dynamic is that no senior VC *needs* to do your investment. You should never forget that. Any senior VC that you're talking to is already wealthy and has many famous deals to show for it. Your company is probably not going to make a material difference to him and but does present a significant chance of adding to his workload and failure rate; there will therefore be a certain amount of inertia against the deal since on average most deals don't pan out but do take time. Therefore, the affirmative angle may accordingly not be enough. But VCs, as we've seen, have their own biases and motivations. The question is simply how you can exploit them to your mutual advantage.

III. Mechanics

A. Who

Tactically, the first thing to do is find someone who *does* need to make investments. That can mean finding a senior associate or a principal for your first pitch, not a senior partner. This contravenes the conventional wisdom that holds that you should not pitch junior people. (“Don’t pitch someone who can’t write a check themselves.”) That wisdom is wrong. Junior people will give you a fair shake, because they need good deals to their name. If they don’t find those deals, they won’t become senior, and they very much want to become senior. So seek these people out – they are motivated in a way more seasoned VCs are not.

Eventually you will talk to senior partners. But you should not assume the affirmative argument will suffice. The logical merits of your business may convince the junior person to take you seriously; they want to. But given senior VCs’ incentives (or lack thereof), affirmative arguments about the value of your business are perversely weak. Fortunately, VCs are loss-averse and very competitive. There is a wealth of psych literature out there that you can consult on this. But all you need to know is that VCs really don’t want to lose a good deal to a competitor. So convince them that your company will be great *and* make them afraid of missing out. If it’s at all plausible, make your deal seem oversubscribed. This tends to overcome what could otherwise be crippling inertia against any given deal.

B. How

There is a common misimpression that VCs are sufficiently smart that they can instantly understand any company. But at least at the beginning of your meetings with them, they aren’t. Sure, they may be bright people with impressive tech backgrounds, but they’re also very busy. The cognitive resources they allocate to any given pitch are—rationally—quite modest. *Early pitches must be simple*. Engineers who start by pitching complex products and business models lose their audiences early. Wherever you can, do the thinking for them. There are certain predictable things that VCs will want to know. [A list of these was made available to the class. Scroll down to the footnote at the bottom to see it] Make the calculations for them in advance so they don’t have to do it themselves. Pretend you’re pitching to an audience of moderately intelligent 9th graders—shortish attention span, no deep knowledge or intuition for your business. You can ratchet up the complexity as you iterate over time. (If all goes well, you’ll have to pitch several times, anyway.) If data need to be analyzed, analyze them. Do not rely on VCs to draw key inferences; they may, but why risk it?

Once you’ve gotten the VCs engaged, you can expect the full force of their intellectual attention. Again, many VCs are very, very smart when they are engaged and you should be prepared to answer extremely detailed questions about your business – many of which you will never have thought of yourself. Answer these honestly and if you don’t know the answer, be honest about that, too.

C. When

You should always try to pitch when you don’t need money. That is when you are strongest. Short runways are often perceived as a sign of massive weakness. If everybody knows you’re desperate, the best that can happen is you get screwed on terms. The worst is that there is no deal at all. VCs tend not to think that they can get away with murder when you have 6 months of cash in the bank. Otherwise, they can be ruthless. The average financing takes 1-3 months; if that’s all the cash you’ve got, you’re at the mercy of the VC. But a

team that goes to pitch with \$15m in bank, 8 months after it last raised does so from a position of strength. So don't be shy about pitching after you've just raised. At least your marketing materials will be current.

If you are the CEO, pitching is your job. There's a romantic notion that the only thing that matters is product and that you can devote yourself to that entirely. That is false. In fact, fully half of your job is selling the company because the CEO is the only one who can actually pitch effectively (no VC wants to be pitched by the VP of Sales). You are a salesman so long as you are CEO. Every quarter from now until eternity, Larry Ellison will have to pitch Wall Street on why people should buy Oracle stock, or at least not sell it. Warren Buffet is worth something like \$46bn and he still has to pitch and has been doing so for five decades (for example, his annual letters). If people with tens of billions of dollars have to do it, you can assume the same will be true of you.

D. Elevator Pitch – The Classic First Pitch

Then there's the elevator pitch, which is somewhat ironic given that every building on Sand Hill road is 2 stories max. The idea, of course, is that your baby can be condensed into a pitch that lasts no longer than an elevator ride. The standard format is stringing together a few well-known products and services that you sort of resemble: "We are Instagram meets TaskRabbit meets Craigslist." You should reject the standard format. It works well in Hollywood, where people like reductive mashups and yearn from familiarity. It works less well in Silicon Valley. Your market is different. If you are just x+y, chances are you can be easily replicated – or at least, that's how it will seem. That should make most good VCs run away. Just make an affirmative statement about what you do and why it's important. SpaceX has a great elevator pitch: "Launch costs haven't come down in decades. We slash them by 90%. The market is \$XXbn." (Contrast this with: "We're NASA meets Toyota!")

Some companies' elevator pitches will be similarly straightforward. "We cured pancreatic cancer in monkeys. We need cash for Phase II trials; if this works, it's a \$10 billion market annually." Even if yours isn't quite as simple as that, you still need to make it simple. The equation form of a good short pitch would be problem + solution = money. Get this down, because VCs are floating around everywhere and you never know when or where you'll be pitching. Don't be pushy. Don't pounce on them. Certainly don't interrupt their dinner. But if you are in a good social context go for it.

E. Other Routes

Another route you could take is the cold pitch. It's very simple: You just e-mail your deck to submissions@givenVC.com or call their main line. The only problem with this route is that it has an almost zero percent chance of working. Your pitch will be ignored upon receipt.

You are at Stanford. You should be able to find a VC. Many VCs went to Stanford and only made it a mile across the road. It's easy for you to get an introduction; if you can't, people will assume there's something very off. Take advantage of your Stanford connections; it's a small world. Find someone who knows who you want to talk to and get a referral. At least you'll make it past the spam filter.

One alternative approach that *does* work well is the pre-pitch. Done properly, this can be very effective. It's basically PR. TechCrunch has to run 20 stories a day. Let one of them be about you. If you do it right, VCs might actually approach you. And you won't have to engineer an aggressive press strategy come product launch. The right e-mail naming conventions are easy to find. You'll find that the TC folks are quite

sympathetic and very much enjoy writing about small companies. This “reverse pitch” is good jujitsu. Or good *matanza*, which, for those of you not familiar with the art of Sicilian fishing, basically involves skillfully inducing a small flock of tuna into netted cabins and then harpooning it to death. Much easier than rod and reel, one-by-one.

IV. The Main Pitch

A. The Set Up

If you’re lucky all this leads to a classic pitch in a VC’s office. This typically unfolds with Kabuki-like formality. Customarily, there will be a 10-20 slide deck. There will be 1-5 partners. After 40 minutes when a powerpoint is literally read word for word from a projector, in the dark, as people slowly generate alpha waves, there will be a Q&A in which the partners pretend to be interested, but of course they have been stunned into submission by the mindless recitation of the powerpoint. They will ask if you need your parking validated. And then you’ll never hear from them again, because there’s been no real engagement.

To avoid this fate, tell a story – and try to do it first without relying on your deck. People like stories. Our brains are wired to respond them. We recall facts better when they are embedded in narrative. Hollywood is the proof of their value. We pay lots of money for stories. Entertainment is a much bigger industry than venture capital because people like stories. Even a crappy game like Mass Effect 3 sells a million copies because it tells a story. So you should try to tell one, too. Why did you start your company? What do you want to achieve? Then drape the facts around that skeleton.

Fortunately, the framework for a good story has been long established. Aristotle figured out the elements of a perfect pitch thousands of years ago. He identified the principles of logos, ethos, and pathos. Logos is argument based on facts and reason. Ethos is argument based on character—*your* character. This is the credibility piece. Finally, pathos is argument based on listeners’ emotions. Those are what you need to exploit. So think about your pitch in terms of logos, ethos, and pathos. There is 3,000 years of decent evidence that people respond to pitches that get these factors right.

B. The Pitch Itself – Mechanics and Customs

Presumably, you have good reason why your thing is going to make a lot of money – this is the logos part and should be straightforward for engineers.

First thing is first: you need a deck to explain your idea. Don’t try to pitch without one. There will be *zero* VC *interest* without a deck, so you need to make one. A deck is written propaganda. It will be e-mailed around and therefore must stand alone. It is *not* (fundamentally) a presentation tool for projector-based meetings. It is a means of presenting data within a narrative that people read by themselves. All the nifty Keynote or PowerPoint UI graphics tricks don’t matter. They’ll probably just make things awkward during a live presentation. Even worse, they’ll detract from what your deck is supposed to be: written information presented in thoughtful, easily-digestible way.

Again, your deck is your argument for your company. It is not primarily an animated presentation tool—most audiences are horrified by having to sit through dramatic builds of each bullet point in a slide. *Your deck is an info-rich manifesto*. One trick to further exploit the natural deficiencies of your victim: at some point, the junior analyst will be dispatched to analyze your company. You should thus write text that the

junior analyst can plagiarize. Good, info-rich decks reduce the load on analysts. Make their work easier for them and they will do more of it. Help them make your case for you.

For the live pitch: the default mechanics are that you arrive 10 minutes early. The VC probably arrives 10 minutes late; don't be rattled. You plug in to the projector. The room goes dark and people have to start to fight the sleep-inducing effects. The first slide goes up. The VC comes in. Cards are exchanged. And so begins the VC equivalent of the Bataan Death March. Too many people are determined to finish: you made all these slides, and, dammit, you're going to get through them. And the VCs are fighting their own battle to stay awake. This does no one any good and is redundant if the VCs have already read your deck.

Your only chance is to have a straightforward, content-rich deck, *and then to leave it behind as soon as possible*. VCs will have looked at the deck before the meeting, because they don't want to waste their time and if your deck sucks they'd have alerted you about the terrible family emergency that just came up and dispatched a junior to meet with you instead. While you should have the deck up and be ready to talk about it in case some VC is masochistic and wants to parse bullet points, try to have a real conversation as soon as possible. It's just far more engaging for both sides. Also, there are actually two pitches going on; you're pitching the VC, but if your company is any good at all, that VC is also pitching you. Be alert to this dual dynamic.

Sometimes you should have two different presentations—the deck you sent earlier, and then what you show them in the office. It's possible that you have some multimedia that communicates something that can't be communicated another way; show, don't tell. Even better are prototypes that VCs can physically user or interact with. People like to play with stuff. So you're halfway there if you can get them playing around.

Remember, VCs see so many pitches and are so cognitively overburdened that their method of analysis at beginning is negatively driven; what is cognitively efficient for them is to find a way to say no. So try to be perfect. If you give them any reason to say no, they will.

Another trick that smart law students understand is to underline key phrases. Professors never actually fully read exams or bluebooks. And there are only 10-15 important concepts in any given question on a law exam. So if you underline those concepts on the paper, the professor sees them. The professor probably won't even take the time to see if you correctly embedded those concepts. You've made grading easy, and you get an A. Venture Capital isn't that different. If you underline important stuff, you reduce the amount of effort the VC has to put in. That reduces friction in the decision making process, which is the goal of all this.

An aside: do not ask for an NDA. Ever. You will be perceived as a rank amateur. If you don't feel confident sharing detailed information about your company, don't. Go find someone else.

C. The Substance

Again, organic conversation is much better than talking through your deck. So break from the deck quickly.

[Gibney walked through two different decks for the same company, a good deck and a bad/traditional deck, explaining the relevant differences. These were made available to the class.]

You start with the vision – what do you ultimately want to accomplish. Explain why you are a company, not just a product/feature? Then get into the business. What is it? Why is it superior? Why is it not likely to be

displaced for some time? Be clear and concise. Your pitch will be recapitulated by people inside the firm. Give anyone on your side sufficient ammunition to defend your company to their co-workers. VCs love to poke holes in their partners' proposed investments—it's a critical part of the lemon detection process. Anticipate the holes and fill them.

The team is important. This is the ethos part of the presentation—why are you the right people for the task? Why should the VC trust you? Who (and what skills) do you have? Are you missing anyone? How are you going to recruit and convince that 20th employee to join? Also be prepared to talk about your compensation philosophy. Some VCs, you might have heard, have strong views on compensation.

The discussion should then turn to your market, and specifically the size of the “addressable” market and how you are going to grab it. How much of the market are you going to capture? How? What's your assessment of the competition? Be honest. It's almost always a mistake to insist that you have no competition. VCs will get that you think you're (going to be) better than the competition—everyone knows it's a pitch. But radically underestimating your competition will set off peoples' BS detectors.

At some point there will be talk about a business model. Just have something reasonable to say about this. For young companies, it's almost certainly a total work of fiction since it will probably change. But having a reasonable answer shows that you're thinking about how the product will become a business. “If we build it, they will come” is simply not true. Being able to talk about revenue, sales processes, customer acquisition, and barriers to entry/exit shows your VCs that you're not that naïve.

You also need to have a clear ask. How much are you looking to raise? What do you need it for? What's your burn rate? The one question that people don't seem to want to talk about is valuation, which, of course, is what people really want to know. You should discuss valuation early on—perhaps not in your first pitch, but certainly in your second. It's a gating factor, so there's no point in investing many cycles if you're orders of magnitude apart of price. And yet VCs and entrepreneurs alike tend to dance around it.

You should also talk about your funding history and syndication. Funding history is both important quantitatively and serves as qualitative validation (did you get good VCs in before? Are they re-investing, and if not, why not? How much did you put in yourself?). Syndication – the other participants in the current round – is also important, as it helps qualitatively validate the deal. Who else are you talking to?

VCs will ask you: “Why do you want to work with us?” *This goes to the pathos element and is crucially important.* You should have a quasi-tailored answer. But this is nothing new for you; it's the same as when you applied to all the elite universities. Why were you considering Yale? For the same reason you considered Harvard: it's a top school. But you have to offer the Yale admissions people more than that. You have to—you *did*—tell stories about the wonderful pizza in New Haven, or how it was your dream to work with Professor X in Department Y and this esoteric thing z. Yale, you had to say, was the only place you could truly be happy. And then of course you waxed on to Harvard about how much you love Boston. Actually, this is somewhat too sarcastic: you should have at least some reasons why you want to work with a given VC and don't be shy about stating those.

There are all sorts of small nuggets of wisdom that are worth remembering. Don't present data in weird ways. It's a pitch, not a modern art class. Label your axes. Do not include charts or references to Facebook unless your thing honestly and actually has to do with Facebook. Though even the most out-of-touch VCs

get that Facebook is catching on with young people and somehow important, none will be fooled by unrelated logos and clip art.

Again, all of these elements should be framed within a compelling story. VCs will remember stories. It may be easier or harder to frame your product in a reasonably compelling dramatic narrative, but you should definitely try to do it.

Finally, put together a data room for your investors. Almost no one does this. It's hard to understand why. Not having a data room leads to 1000 emails asking for stuff that should have been put in a data room. Don't make the VC fish through Outlook to find something. Because they won't, which sucks for you, or they will, and they'll be pissed about it. Don't put PDFs of numerical data in the data room. Use a modifiable file format. Let the VCs play with and test your assumptions.

V. Pitching for Life

What happens after the pitch? If the VCs haven't slipped into coma because the room is dark and you bored them to death, that is.

It is rare for a pitch to conclude with an offer when you leave office. Good VCs will take several days to several months to make a decision. This is not a bad thing. Your company is difficult to understand – all good companies are. In many cases, VCs still don't understand all the pieces to the portfolio companies they've invested in for years – and there's nothing wrong with that, because it means the companies have grown larger than the imagination of one person. If you have a genuine business, VCs need time to get a good understanding of your business. Sometimes more time spent in diligence is a promising sign.

You'll want to pick someone in the room to be your evangelist. You need a champion in the VC firm, or else your deal will die.

Remember that pitches go both ways. Companies are staying private longer and longer now. You'll be stuck with your VC for a very long time. Facebook has been private for 8 years now. The average American marriage lasts for about 10 years. You'd do more than 1 hour of diligence when choosing your spousal equivalent. So spend the time to judge your VC properly. Are they smart? Honest? Try to get a sense of what other deals that VC is looking at. See if they have any sort of relevant experience. Are they just peppering a space with a whole bunch of investments? You don't want to be a lottery ticket.

Once you close a deal, get a press release out. Quote the VCs. Put them on your website. Get your logo on their website. And start thinking immediately about who your next VC will be in 18 months.

VI. Questions from the Audience

Q: When you're pitching, should you focus on your initial product or your grand vision?

A: Founders Fund likes to start with the big vision. But many people are more narrowly focused and may well want to hear about the product first.

Q: How can one judge a VC without a lot of background in startups or VC?

A: Go with your gut. Evaluate their intellectual content, if any. Do a gut check. If you see some VC firm plop Facebook's logo on their front page when they got in at a \$25B valuation and generic pap about investing in the future, move on. That's dishonest on their part. But if you come across someone like Brad Feld, who obviously knows what he's talking about, and who has made some pretty interesting deals, you can feel more at ease.

Q: Which VCs do you and don't you like?

A: The usual suspects. Sometimes you have little choice. If you're trying to raise \$300 million at a \$6bn valuation, there are only so many places you can go.

The truth is that most VCs are not very good at all. The objective verification of this is that the bottom 80% of industry hasn't made any money in the last 10 years. The compensation is that the ones that do are really very competent.

Q: When do you discuss important terms in the pitch process?

A: You should mention key terms or anything idiosyncratically important early on. If you want to control the enterprise mention that right off the bat. Sometimes it may kill the conversation and everyone can stop wasting time with further discussion.

Most terms don't matter. Economics and control matter; discuss those soon. As for the rest, outcomes tend to be very bimodal. If the outcome is zero, terms don't matter. If the outcome is huge success, terms don't really matter either. Only for little-better-than-mediocre-exits do terms matter much, and those outcomes are pretty rare in VC. So don't waste your time or \$80k figuring out some particular term with WSGR.

Q: If you could radically alter or eradicate some part of the pitch process, what would it be?

A: The worst thing ever is when people who aren't yet a company pitch you for an investment. VCs are supposed invest in companies, not create and build your company for you. Do not pitch until you're a company. No one wants to get pitched just an idea or product. Even if VCs loved the product or idea, they literally can't invest, because there's nothing to fund. You need a company to wire the money to.

Q: How important is strategic advice from VC?

A: Probably 80% of the value add is capital, and 20% is advising. Superangels are very popular right now. Their pitch is that they can help you build your business. You look at their portfolio and its 150-deep. For the average company, how much time or energy can they actually devote? VCs have fewer portfolio companies, but they have the same constraints. They can add value by providing strategic advice, build syndicates for funding, and explain processes that are familiar to them but new to you. But in terms of the mythical model of the hybrid VC-McKinsey consultant helping you build your business, hand-in-hand, no. That doesn't happen for most portfolio companies. It's mathematically impossible. VCs who insist they do that for everyone aren't being honest.

Peter Thiel's CS183: Startup - Class 9 Notes Essay

Here is an essay version of my class notes from Class 9 of CS183: Startup. Errors and omissions are my own. Credit for good stuff is Peter's entirely.

Class 9 Notes Essay—If You Build It, Will They Come?

I. Definitions

Distribution is something of a catchall term. It essentially refers to how you get a product out to consumers. More generally, it can refer to how you spread the message about your company. Compared to other components that people generally recognize are important, distribution gets the short shift. People understand that team, structure, and culture are important. Much energy is spent thinking about how to improve these pieces. Even things that are less widely understood—such as the idea that avoiding competition is usually better than competing—are discoverable and are often implemented in practice.

But for whatever reason, people do not get distribution. They tend to overlook it. It is the single topic whose importance people understand least. Even if you have an incredibly fantastic product, you still have to get it out to people. The engineering bias blinds people to this simple fact. The conventional thinking is that great products sell themselves; if you have great product, it will inevitably reach consumers. But nothing is further from the truth.

There are two closely related questions that are worth drilling down on. First is the simple question: how does one actually distribute a product? Second is the meta-level question: why is distribution so poorly understood? When you unpack these, you'll find that the first question is underestimated or overlooked for the same reason that people fail to understand distribution itself.

The first thing to do is to dispel the belief that the best product always wins. There is a rich history of instances where the best product did not, in fact, win. Nikola Tesla invented the alternating current electrical supply system. It was, for a variety of reasons, technologically better than the direct current system that Thomas Edison developed. Tesla was the better scientist. But Edison was the better businessman, and he went on to start GE. Interestingly, Tesla later developed the idea of radio transmission. But Marconi took it from him and then won the Nobel Prize. Inspiration isn't all that counts. The best product may not win.

II. The Mathematics of Distribution

Before getting more abstract, it's important to get a quantitative handle on distribution. The straightforward math uses the following metrics:

- Customer lifetime value, or CLV
- Average revenue per user (per month), or ARPU
- Retention rate (monthly, decay function), or r
- Average customer lifetime, which is $1 / (1-r)$
- Cost per customer acquisition, or CPA

CLV equals the product of ARPU, gross margin, and average customer lifetime.

The basic question is: is CLV greater or less than CPA? In a frictionless world, you build a great business if $CLV > 0$. In a world with some friction and uncertainty, you build a great business if $CLV > CPA$.

Imagine that your company sells second-tier cell phone plans. Each customer is worth \$40/month. Your average customer lifetime is 24 months. A customer's lifetime revenue is thus \$960. If you have a 40% gross margin, the customer's lifetime value is \$384. You're in good shape if it costs less than \$384 to acquire that customer.

One helpful way to think about distribution is to realize that different kinds of customers have very different acquisition costs. You build and scale your operation based on what kinds of things you're selling.

On one extreme, you have very thin, inexpensive products, such as cheap steak knives. You target individual consumers. Your sales are a couple of dollars each. Your approach to distribution is some combination of advertising and viral marketing—hoping that the knives “catch on.”

Things are fundamentally different if you're selling a larger package of goods or services that costs, say, \$10,000. You're probably targeting small businesses. You try to market your product accordingly.

At the other extreme, you're selling to big businesses or governments. Maybe your sales are \$1m or \$50m each. As the unit value of each sale goes up, there is necessarily a shift towards more people-intensive processes. Your approach to these kinds of sales must be to utilize salespeople and business development people, who are basically just fancy salespeople who do three martini lunches and work on complex deals.

III. The Strangeness of Distribution

A. Fact versus Sales Pitch

People say it all the time: this product is so good that it sells itself. This is almost never true. These people are lying, either to themselves, to others, or both. But why do they lie? The straightforward answer is that they are trying to convince other people that their product is, in fact, good. They do not want to say “our product is so bad that it takes the best salespeople in the world to convince people to buy it.” So one should always evaluate such claims carefully. Is it an empirical fact that product x sells itself? Or is that a sales pitch?

The truth is that selling things—whether we're talking about advertising, mass marketing, cookie-cutter sales, or complex sales—is not a purely rational enterprise. It is not just about perfect information sharing, where you simply provide prospective customers with all the relevant information that they then use to make dispassionate, rational decisions. There is much stranger stuff at work here.

Consider advertising for a moment. About 610,000 people work in the U.S. ad industry. It's a \$95bn market. Advertising matters because it works. There are competing products on the market. You have preferences about many of them. Those preferences are probably shaped by advertising. If you deny this it's because you already know the “right” answer: your preferences are authentic, and ads don't work on you. Advertising only works on other people. But exactly how that's true for everybody in the world is a strange question indeed. And there's a self-referential problem too, since the ad industry has had to—and did—convince the people who buy ads that advertising actually works.

If you buy Levis jeans, your apartment blows up. Or is it the other way around?

Comparisons to price or how fast computers work? No. Something strange is going on.

The U.S. sales industry is even bigger than advertising. Some 3.2 million people are in sales. It's a \$450bn industry. And people can get paid pretty well. A software engineer at Oracle with 4-6 years experiences gets a \$105k salary and an \$8k bonus. But a sales manager with 4-6 years experiences gets \$112k and a \$103k bonus. The situation is very much the same at Google, which claims to be extremely engineering driven; at a \$96k base, \$86k in commissions, and a \$40k bonus, Google salespeople earn quite a bit more than their engineering counterparts. This doesn't mean everyone should go into sales. But people who are good at it do quite well.

Self-referential version of sales question.

B. Salesman as Actor

The big question about sales is whether all salesmen are really just actors of one sort or another. We are culturally biased to think of salespeople as classically untrustworthy, and unreliable. The used car dealer is the archetypical example. Marc Andreessen has noted that most engineers underestimate the sales side of things because they are very truth-oriented people. In engineering, something either works or it doesn't. The surface appearance is irrelevant. So engineers tend to view attempts to change surface appearance of things—that is, sales—as fundamentally dishonest.

What is tricky about sales is that, while we know that it exists all around us, it's not always obvious who the real salesperson is. Tom Sawyer convinced all the kids on the block to whitewash the fence for him. None of those neighborhood kids recognized the sale. The game hasn't changed. And that's why that story rings true today.



Look at the images above. Which of these people is a salesman? President Eisenhower? He doesn't *look* like a salesman. The car dealer in the middle *does* look like a salesman. So what about the guy on the right?

The guy on the right is Bill Gross, who founded IdeaLab, which was more or less the Y-Combinator of the late 1990s. IdeaLabs' venture arm invested in PayPal. In late 2001, it hosted a fancy investor lunch in southern California. During the lunch, Gross turned to Peter Thiel and said something like:

"I must congratulate you on doing a fantastic job building PayPal. My 14-year-old son is a very apathetic high school student and very much dislikes writing homework assignments. But he just wrote a beautiful e-mail to his friends about how PayPal was growing quickly, why they should sign up for it, and how they could take advantage of the referral structure that you put in place."

On some level, this was a literary masterpiece. If nothing else, it was impressive for the many nested levels of conversation that were woven in. Other people were talking to other people about PayPal, possibly at infinite levels on down. The son was talking to other people about those people. Bill Gross was talking to his son. Then Gross was talking to Peter Thiel. And at the most opaque and important level, Gross was talking to the other investors at the table, tacitly playing up how smart he was for having invested in PayPal. The message is that sales is hidden. Advertising is hidden. It works best that way.

There's always the question of how far one should push this. People push it pretty far. Pretty much anyone involved in any distribution role, be it sales, marketing, or advertising, should have job titles that have nothing to do with those things. The weak version of this is that sales people are account executives. A somewhat stronger version is that people trying to raise money are not I-bankers, but rather are in corporate development. Having a job title that's different from what you actually do is an important move in the game. It goes to the question of how we don't want to admit that we're being sold to. There's something about the process that's not strictly rational.

To think through how to come to an organizing principle for a company's distribution, consider a 2 x 2 matrix. One axis is product: it either sells itself, or it needs selling. The other axis is team: you either have no sales effort, or a strong one.

Consider the quadrants:

- Product sells itself, no sales effort. *Does not exist.*
- Product needs selling, no sales effort. *You have no revenue.*
- Product needs selling, strong sales piece. *This is a sales-driven company.*
- Product sells itself, strong sales piece. *This is ideal.*

C. Engineering versus Sales

Engineering is transparent. It's hard. You could say it's transparent in its hardness. It is fairly easy evaluate how good someone is. Are they a good coder? An ubercoder? Things are different with sales. Sales isn't very transparent at all. We are tempted to lump all salespeople in with vacuum cleaner salesmen, but really there is a whole set of gradations. There are amateurs, mediocrities, experts, masters, and even grandmasters. There is a wide range that exists, but can be hard to pin down.

A good analogy to the engineer vs. sales dynamic is experts vs. politicians. If you work at a big company, you have two choices. You can become expert in something, like, say, international tax accounting. It's specialized and really hard. It's also transparent in that it's clear whether you're actually an expert or not.

The other choice is to be a politician. These people get ahead by being nice to others and getting everyone to like them. Both expert and politician can be successful trajectories. But what tends to happen is that people choose to become politicians rather than experts because it seems easier. Politicians *seem* like average people, so average people simply assume that they can do the same thing.

So too in engineering vs. sales. Top salespeople get paid extremely well. But average salespeople don't, really. And there are lots of below average salesmen. The failed salesman has even become something of a literary motif in American fiction. One can't help but wonder about the prehistory to all these books. It may not have been all that different from what we see today. People probably thought sales was easy and undifferentiated. So they tried it and learned their error the hard way. The really good politicians are much better than you think. Great salespeople are much better than you think. But it's always deeply hidden. In a sense, probably every President of the United States was first and foremost a salesman in disguise.

IV. Methods of distribution

To succeed, every business has to have a powerful, effective way to distribute its product. Great distribution can give you a terminal monopoly, even if your product is undifferentiated. The converse is that product differentiation itself doesn't get you anywhere. Nikola Tesla went nowhere because he didn't nail distribution. But understanding the critical importance of distribution is only half the battle; a company's ideal distribution effort depends on many specific things that are unique to its business. Just like every great tech company has a good, unique product, they've all found unique and extremely effective distribution angles too.

A. Complex Sales

One example is SpaceX, which is the rocket company started by Elon Musk from PayPal. The SpaceX team has been working on their rocketry systems in Southern California for about 8 years now. Their basic vision is to be the first to send a manned mission to Mars. They went about doing this in a phenomenal way. Time constraints make it impossible to relate all of Elon's many great sales victories. But if you don't believe that sales grandmasters exist, you haven't met Elon. He managed to get \$500m in government grants for building rockets, which is SpaceX, and also for building electric cars, which is done by his other company, Tesla.

That was an even bigger deal than it may initially seem. SpaceX has been busy knocking out dramatically inferior rocket technologies for the past 10 years, but it's been a very tricky, complicated process. The company has about 2,000 people. But the U.S. Space Industry has close to 500,000 people, all distributed about evenly over the 50 states. It's hard to overstate the extent of the massive congressional lobbying that goes to keeping the other space companies—almost the entire industry—alive. Things are designed to be expensive, and SpaceX's mission is to cut launch costs by 90%. To get where it is now—and to get to Mars later—SpaceX basically took on the entire U.S. House of Representatives and Senate. And so far, it seems to be winning. It's going to launch a rocket next week. If all doesn't go well, you'll certainly hear about it. But when things go well, you can predict the general response: move along, nothing to see here, these aren't the rockets we're looking for.

Palantir also has a unique distribution setup. They do government sales and sales to large financial institutions. Deals tend to range from \$1m to \$100m. But they don't have any salespeople—that is, they don't employ "salespeople." Instead they have "forward deployed engineers" and a globetrotting CEO who spends 25 or 26 days each month traveling to build relationships and sell the product firsthand. Some argue that the traveling CEO-salesman model isn't scalable. It's a fair point, but the counterpoint is that, at that level, people really only want to talk to the CEO. You certainly can't just hire army of salespeople, because that sounds bad. So you have forward deployed engineers double up in a sales capacity. Just don't call them salespeople.

Knewton is a Founders Fund portfolio company that develops adaptive learning technology. Its distribution challenge was to figure out a way to sell to big educational institutions. There seemed to be no direct way to knock out existing players in the industry. You would have to take the disruptive sales route where you just try to come in and outsell the existing companies. But much easier is to find a non-disruptive model. So Newton teamed up with Pearson, the big textbook company. Without that partnership, Knewton figured it would just be fighting the competition in the same way at every school it approached, and ultimately it'd just lose.

B. Somewhat Smaller Sales

As we move from big, complex sales to sales, the basic difference is that the sales process involves a ticket cost of \$10k-100k per deal. Things are more cookie cutter. You have to figure out how to build a scalable process and build out a sales team to get a large number of people to buy the product. David Sacks was a product guy at PayPal and went on to found Yammer. At PayPal, he was vehemently anti-sales and anti-BD. His classic lines were: “Networking is *not working!*” and “People doing networking are not *working!*” But at Yammer, Sacks found that he had to embrace sales and build out a scalable distribution system. Things are different, he says, because now the sales people report to him. Because of its focus on distribution, Yammer was able to hire away one of the top people from SalesForce to run its sales team.

ZocDoc is a doctor referral service. It's kind of a classic internet business; they are trying to get doctors' offices to sign up for the service at a cost of \$250/month. Growth is intensively sales-driven, and ZocDoc does market-by-market launches. There is even a whole internal team of recruiters who do nothing else but try to recruit new salespeople. Toward the lower end of things—and \$250 per month per customer is getting there—things get more transactional and marginal.

C. The Missing Middle

There is a fairly serious structural market problem that's worth addressing. On the right side of the distribution spectrum you have larger ticket items where you can have an actual person driving the sale. This is Palantir and SpaceX. On the extreme left-hand side of the spectrum you have mass marketing, advertising, and the like. There is quite possibly a large zone in the middle in which *there's actually no good distribution channel to reach customers*. This is true for most small businesses. You can't really advertise. It wouldn't make sense for ZocDoc to take out a TV commercial; since there's no channel that only doctors watch, they'd be overpaying. On the other hand, they can't exactly hire a sales team that can go knock on every doctor's door. And most doctors aren't that technologically advanced, so internet marketing isn't a perfect solution. If you can't solve the distribution problem, your product doesn't get sold—even if it's a really great product.

The opposite side of this is that if you do figure out distribution—if you can get small businesses to buy your product—you may have a terminal monopoly business. Where distribution is a hard nut to crack, getting it right may be most of what you need. The classic example is Intuit. Small businesses needed accounting and tax software. Intuit managed to get it to them. Because it nailed distribution, it's probably impossible for anyone to displace Intuit today. Microsoft understood the great value of Intuit's distribution success when it tried to acquire Intuit. The Department of Justice struck down the deal, but the point is that the distribution piece largely explains Intuit's durability and value.

D. Marketing

Further to the left on the distribution spectrum is marketing. The key question here is how can one advertise in a differentiated way. Marketing and advertising are very creative industries. But they're also quite competitive. In order to really succeed, you have to be doing something that others haven't done? To gain a significant advantage, your marketing strategy must be very hard to replicate.

Advertising used to be a much more iconic and valued industry. In the 1950s and '60s it was iconic and cutting edge. Think Mad Men. Or think Cary Grant, who, in the classic movie North by Northwest, played the classic advertising executive who is cool enough to be mistaken for a spy. Advertising and espionage were debonair enterprises, roughly equal in glamourousness.

But it didn't last. As the advertising industry developed in 70s and 80s, more people figured out ways to do it. Things became much more competitive. The market grew, but the entrants grew faster. Advertising no longer made as much money as they had been before. And ever since there has been a relentless, competitive push to figure out what works and then dial up the levers.

Advertising is tricky in the same way that sales is. The main problem is that, historically at least, you never quite know if your ads are working. John Wanamaker, who is billed as the father of advertising, had a line about this: "Half the money I spend on advertising is wasted: the trouble is I don't know which half." You may think your ad campaign is good. But is it? Or are the people who made your ad campaign just telling you that it's good? Distinguishing between fact and sales pitch is hard.

In most ways, Priceline.com represents certain depressing decline of our society. It points to a very general failure. But one specific thing Priceline does well is its powerfully differentiated marketing, which makes it very hard to replicate or compete against. PayPal once staged a PR event where James Doohan—Scotty from Star Trek—would beam money using a palm pilot. It turned out to be a total flop. It turns out that Captain Kirk—that is, William Shatner—is in a league of his own.

Advertising's historical opaqueness is probably the core of why Google is so valuable; Google was the first company that enabled people to figure out whether advertising actually worked. You can look at all sort of metrics—CPM, CTR, CPC, RPC—and do straightforward calculations to determine your ROI. This knowledge is important because people are willing to pay a lot for advertising if it actually works. But in the pre-internet magazine age before Google, ad people never really had a clue about how they were doing.

Zynga has excelled at building on top of Google's ad work. Everyone knows that Zynga experienced great viral growth as its games caught on. Less known is that they spent a lot of money on targeted advertising. That allowed them to monetize users much more aggressively than people thought possible. And then Zynga used that revenue to buy more targeted ads. Other gaming companies tried to do just viral growth—build games that had some social element at their core. But Zynga went beyond that distribution strategy and got a leg up by driving rapid growth with aggressive marketing.

The standard bias on the internet is that advertising does not work. But that's an interesting double standard. There are an awful lot of websites whose business model is ad sales. And then they turn around and say that they don't actually believe ads are good way of getting customers. The Zynga experience shows that creatively rethinking the standard narrative can be quite lucrative. There is a lot of room for creativity in distribution strategy.

E. Viral Marketing

Viral marketing is, of course, the classic distribution channel that people tend to think of as characteristic of Internet businesses. There are certainly ways to get it to work. But it's easy to underestimate how hard it is to do that. William Shatner and James Doohan seemed similar. In fact they were a world apart. Salesmen may seem similar. But some get Cadillac's, while others get steak knives. Still others get fired and end up as characters in novels.

[Section on viral marketing math excluded. You can learn about this stuff elsewhere, e.g. [here](#). The gist is twofold: first, viral cycle time is important. Shorter is better. Second, there is a metric called viral coefficient, and you need it to be > 1 to have viral growth.]

PayPal's initial user base was 24 people. Each of those people worked at PayPal. They all knew that getting to viral growth was critical. Building in cash incentives for people to join and refer others did the trick. They hit viral growth of 7% daily—the user base essentially doubled every 10 days. If you can achieve that kind of growth and keep it up for 4-5 months, you have a user base of hundreds of thousands of people.

Certain segments grow faster than others. The goal is to identify the most important segment first, so that anybody who enters the market after you has a hard time catching up. Consider Hotmail, for instance. It achieved viral growth by putting sign-up advertising at the bottom of each e-mail in their system. . Once they did that successfully, it was really hard to copy with the same success. Even if other providers did it and had similar growth curves, they were a whole segment behind. If you're the first mover who is able to get a product to grow virally, no one else can catch up. Depending on how the exponential math shakes out in a particular case, the mover can often be the last mover as well.

PayPal is a classic example. The first high-growth segment was power buyers and power sellers on eBay. These people bought and sold a ton of stuff. The high velocity of money going through the system was linked to the virality of customer growth. By the time people understood how and why PayPal took off on eBay, it was too late for them to catch up. The eBay segment was locked in. And the virality in every other market segment—e.g. sending money to family overseas—was much lower. Money simply didn't move as fast in those segments. Capturing segment one and making your would-be competitors scramble to think about second and third-best segments is key.

Dropbox is another good example of a very successful company that depended on viral growth. Pinterest may be as well. It's sort of hard to tell at this point. Is Pinterest actually good? Or is it a fad? Will it become a ghost town that no one uses? It's not entirely clear. But it has certainly enjoyed exponential growth.

Marketing people can't do viral marketing. You don't just build a product and then choose viral marketing. There is no viral marketing add-on. Anyone who advocates viral marketing in this way is wrong and lazy. People romanticize it because, if you do it right, you don't have to spend money on ads or salespeople. *But viral marketing requires that the product's core use case must be inherently viral.* Dropbox, for example, let's people share files. Implicit is that there's someone—a potential new user—to share with. Spotify does this with its social music angle. As people use the product, they encourage other people to use it as well. But it's not just a "tell your friends" button that you can add-on post-product.

F. The Power Law Strikes Again

We have seen how startup outcomes and VC performance follow a power law. Some turn out to be a lot better than others. People tend to underestimate how extreme the differences are because our generally egalitarian society is always telling us that people are essentially the same.

We've also heard Roelof Botha explain that LinkedIn was the exception that proves the rule that companies do not have multiple revenue streams of equal magnitude. The same is true for distribution, and exceptions are rare. Just as it's a mistake to think that you'll have multiple equal revenue streams, you probably won't have a bunch of equally good distribution strategies. Engineers frequently fall victim to this because they do not understand distribution. Since they don't know what works, and haven't thought about it, they try some sales, BD, advertising, and viral marketing—everything but the kitchen sink.

That is a really bad idea. It is very likely that one channel is optimal. Most businesses actually get zero distribution channels to work. Poor distribution—not product—is the number one cause of failure. If you can get even a single distribution channel to work, you have great business. If you try for several but don't nail one, you're finished. So it's worth thinking really hard about finding the single best distribution channel. If you are an enterprise software company with a sales team, your key strategic question is: who are the people who are most likely to buy the product? That will help you close in on a good channel. What you want to avoid is not thinking hard about which customers are going to buy it and just sending your sales team out to talk to everybody.

Distribution isn't just about getting your product to users. It's also about selling your company to employees and investors. The familiar anti-distribution theory is: the product is so good it sells itself. That, again, is simply wrong. But it's also important to avoid the employee version: this company is so good, people will be clamoring to join it. The investor version—this investment is so great, they'll be banging down our door to invest—is equally dangerous. When these things seem to happen, it's worth remembering that *they almost never happen in a vacuum*. There is something else going on that may not be apparent on the surface.

G. PR and Media

PR and Media add yet another layer to the distribution problem. How the message of your company gets distributed is worth thinking hard about. PR and media are very linked to this. It is a sketchy and very problematic world. But it's also very important because we live in a society where people don't usually have a rational idea of what they want.

Consider an example from the VC world. It's almost never the case that a company finds just one interested investor. There are always zero or several. But if the world were economically rational, this wouldn't be true at all. In a perfectly rational world, you'd see single investor deals all the time. Shares would be priced at the marginal price where you get a single highest bidder—your most bullish prospective investor. If you get more than one person interested in investing, you've done it wrong and have underpriced yourself. But investors obviously aren't rational and can't all think for themselves. So you get either zero investors or many.

It's easy and intuitive for smart people to be suspicious of the media. For many years, Palantir had a very anti-media bias. But even if media exposure wasn't critical for customers or business partners, it turned out to be very important for investors and employees. Prospective employees Google the companies they're looking at. What they find or don't matters, even if it's just at the level of people's parents saying "Palantir?

Never heard of it. You should go work at Microsoft.” And you can’t just plug yourself on your own website; PR is the art of getting trusted, objective third parties to give you press.

H. On Uncertainty

It’s fairly difficult to overestimate how uncertain people are and how much they don’t know what they actually want. Of course, people usually insist that they *are* certain. People trick themselves into believing that they do know what they want. At the obvious level, “Everyone wants what everyone wants” is just a meaningless tautology. But on another level, it describes the dynamic process in which people who have poorly formed demand functions just copy what they believe everyone else wants. That’s how the fashion world works, for instance.

V. Distribution is Inescapable

Engineers underestimate the problem of distribution. Since they wish it didn’t exist, sometimes they ignore it entirely. There’s a plot line from *The Hitchhiker’s Guide to the Galaxy* in which some imminent catastrophe required everybody to evacuate the planet. Three ships were to be sent into space. All the brilliant thinkers and leaders would take the A ship. All the salespeople, consultants, and executives would take the B ship. All the workers would take the C ship. The B ship gets launched first, and all the B passengers think that’s great because they’re self-important. What they don’t realize, of course, is that the imminent destruction story was just a trick. The A and C people just thought the B people were useless and shipped them off. And, as the story goes, the B ship landed on Earth.

So maybe distribution shouldn’t matter in an idealized, fictional world. But it matters in this one. It can’t be ignored. The questions you must ask are: how big is the distribution problem? And can this business solve it?

We live in a society that’s big on authenticity. People insist that they make up their own minds. Ads don’t work on them. Everything they want, they want authentically. But when you drill down on all these people who claim to be authentic, you get a very weird sense that it’s all undifferentiated. Fashionable people all wear the same clothes.

Understanding this is key. You must appreciate that people can only show tip of the iceberg. Distribution works best when it’s hidden. Question is how big the iceberg is, and how you can leverage it. Every tech company has salespeople. If it doesn’t, there is no company. This is true even if it’s just you and a computer. Look around you. If you don’t see any salespeople, you are the salesperson.

Corporate development is important for the same reasons that distribution is important. Startups tend to focus—quite reasonably—on the initial scramble of getting their first angel or seed round. But once it scales beyond that—once a company is worth, say, \$30m or more—you should have a full-time person whose job it is to do nothing but travel around the world and find prospective investors for your business. Engineers, by default, won’t do this. It’s probably true that if your company is good, investors will continue show up and you’ll have decent up rounds. But how much money are you leaving on the table?

Say your company could reasonably be valued at \$300m. Valuation is as much art as it is science. At that range it can fluctuate by a ratio of 2:1. If you raise \$50m at \$300m, you give away 16% of the company. But if you raise that \$50m at \$500m, you give away 10%. A 6% delta is huge. So why not hire the best person you can and give them 1% of the company to make sure you capture that value?

A similar thing exists with employee hiring. It's trickier to know what to do there. But traditional recruiters do not take the distribution problem seriously enough. They assume that people are always rational, and that by giving them information, people will make good decisions. That's not true at all. And since the best people tend to make the best companies, the founders or one or two key senior people at any multimillion-dollar company should probably spend between 25% and 33% of their time identifying and attracting talent.

Peter Thiel's CS183: Startup - Class 10 Notes Essay

Here is an essay version of class notes from Class 10 of CS183: Startup. Errors and omissions are mine.

Marc Andreessen, co-founder and general partner of the venture capital firm Andreessen Horowitz, joined this class as a guest speaker. Credit for good stuff goes to him and Peter. I have tried to be accurate. But note that this is not a transcript of the conversation.

Class 10 Notes Essay—After Web 2.0

I. Hello World

It all started about 40 years ago with ARPANET. Things were asynchronous and fairly low bandwidth. Going “online” could be said to have begun in 1979 with the CompuServe model. In the early ‘80s AOL joined in with its take on the walled garden model, offering games, chat rooms, etc. Having laid the foundations for the modern web, the two companies would merge in ’97.

The Mosaic browser launched in 1993. Netscape announced its browser on October 13th, 1994 and filed to go public in less than a year later. And so began the World Wide Web, which would define the ‘90s in all kinds of ways.

“Web 1.0” and “2.0” are terms of art that can be sort of hard to pin down. But to speak of the shift from 1.0 to 2.0 is basically to speak of what’s changed from decade to decade. When things got going content was mostly static. Now the emphasis is on user generated content, social networking, and collaboration of one sort or another.

Relative usage patterns have shifted quite a bit too. In the early ‘90s, people used FTP. In the late ‘90s they were mostly web browsing or connecting to p2p networks. By 2010, over half of all Internet usage was video transmission. These rapid transitions invite the question of what’s next for the Internet. Will the next era be the massive shift to mobile, as many people think? It’s a plausible view, since many things seem possible there. But also worth putting in context is that *relative* shifts don’t tell the full story. *Total* Internet usage has grown dramatically as well. There are perhaps 20x more users today than there were in the late ‘90s. The ubiquity of the net creates a sense in which things today are very, very different.

II. The Wild West

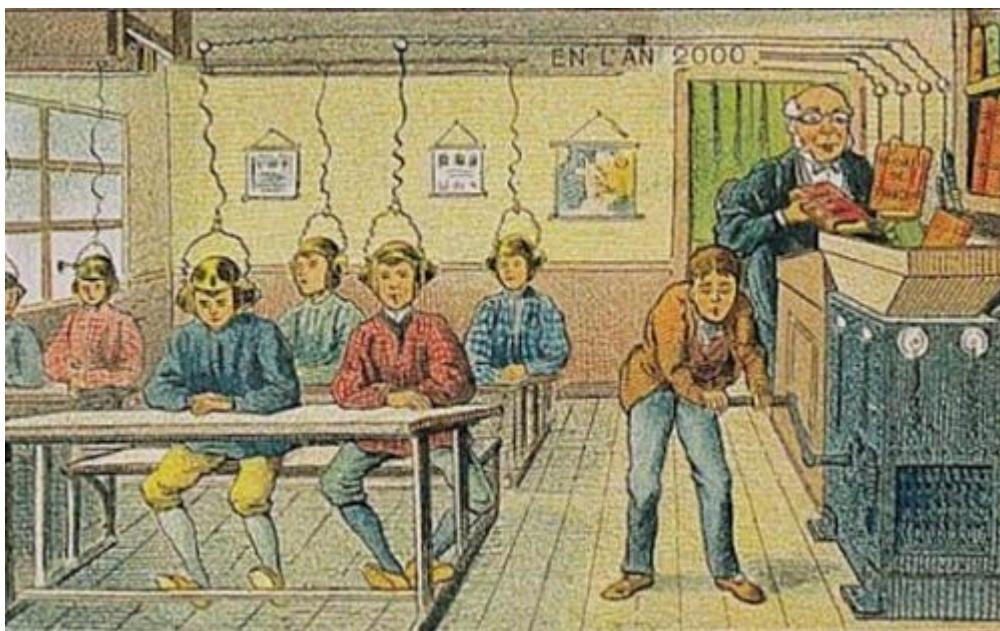
The Internet has felt a lot like the Wild West for last 20 years or so. It’s been a frontier of sorts—a vast, open space where people can do almost anything. For the most part, there haven’t been too many rules or restrictions. People argue over whether that’s good or bad. But it raises interesting questions. What enables this frontier to exist as it does? And is the specter of regulation going to materialize? Is everything about to change?

Over the last 40 years, the world of stuff has been heavily regulated. The world of bits has been regulated much less. It is thus hardly surprising that the world of bits—that is, computers and finance—has been the best place to be during the last 40 years. These sectors have seen tremendous innovation. Indeed, finance was arguably *too* innovative. Regulators have taken note, and there’s probably less to do there now. But what about computers? Will the future bring more innovation or less?

Some big-picture Internet developments are on everyone's radar. Witness the recent and very high profile debates over things like PIPA and SOPA. But other transformations may be just as threatening and less obvious. The patent system, for instance, is increasingly something to worry about. Software patents impose lots of constraints on small companies. Absent regulation, no one can shut you down just because you're small. But that's exactly what patents do. Big companies with economies of scale can either afford such regulations or figure out how to skirt them.

III. When Will The Future Arrive?

No one knows for sure when the future will arrive. But that's no reason not to think about the question. It's easy to point to past predictions where people envisioned a very different future from the one they got. Knowing how and why things didn't quite unfold as people thought is important. You have to know how people in your shoes have gotten it wrong if you hope to get it right. Instead, we tend to abdicate thinking about the future entirely.



Villemard's 1910 prediction of what schools would be like in 2000. Lessons would be digitally uploaded into children's brains. But there is still a teacher. There are still desks. And the machine is powered by a hand crank.

Just a few decades ago, people were predicting chemical dinners and heating houses with radium. And being wrong about the future is nothing new. In 1895, Lord Kelvin declared that "heavier than air flying machines are impossible." U.S. Patent Commissioner Charles H. Duell was confident in 1899 that "everything that can be invented has been invented." All of that, of course, was spectacularly wrong.

Sometimes bad predictions were just too optimistic. In the '60s, people were thinking that everything would soon be nuclear powered. We would have flying cities. Why things haven't worked out like that is an interesting question to think about. But even more interesting are cases where people *are* right about the future and just wrong on timing. Lots of times people get the call right, but the future takes longer to arrive than they thought.



There is a sense in which AI just doesn't quite work yet.

Consider mobile technology. People have been betting on mobile for years. Most of them were far too early. Everyone who tried mobile back in '99 failed. No one thought that the best mobile investment would be to buy a bunch of Apple stock.

There are many more examples. The first rockets were developed in China in the 13th century. But that was not the right time to try to go to the moon. Going to the moon was still a good idea—it just took another couple hundred years for the timing to be right. Apple released its Newton mobile device in 1993, but it took another 15 years before it got the timing right for the iPhone. First we had Napster. It was too early and probably too disruptive. And now we have Spotify. If we learn the right lessons, the future of the past can come back and become true.

IV. Is Software Eating the World?

Marc Andreessen's most famous thesis is that software is eating the world. Certainly there are a number of sectors that have already been eaten. Telephone directories, journalism, and accounting brokerages are a few examples. Arguably music has been eaten too, now that distribution has largely gone online. Industry players don't always see it coming or admit it when it arrives. The New York Times declared in 2002 that the Internet was over and, that distraction aside, we could all go back to enjoying newspapers. The record industry cheered when it took down Napster. Those celebrations were premature.

If it's true that software is eating the world, the obvious question is what else is getting or will soon get eaten? There are a few compelling candidates. Healthcare has a lot going on. There have been dramatic improvements in EMR technology, healthcare analytics, and overall transparency. But there are lots of regulatory issues and bureaucracy to cut through. Education is another sector that software might consume. People are trying all sorts of ways to computerize and automate learning processes. Then there's the labor

sector, where startups like Uber and Taskrabbit are circumventing the traditional, regulated models. Another promising sector is law. Computers may well end up replacing a lot of legal services currently provided by humans. There's a sense in which things remain inefficient because people—very oddly—trust lawyers more than computers.

It's hard to say when these sectors will get eaten. Suffice it to say that people should not bet against computers in these spheres. It may not be the best idea to go be the kind of doctor or lawyer that technology might render obsolete.

In the more distant future, there's another set of sectors what seems ripe for displacement by technology. Broadcast media and space/transportation are two examples. Biology could shift from an experimental science to an informational science. There may be quite a bit of room for software to reshape the intelligence and government sectors. All that may be far off, but there certainly seems to be room for improvement.

How should one assess industries and opportunities if software is really eating the world? Consider a 2 x 2 matrix. On the vertical axis you have two choices: compete with computers or work with computers. This is essentially anti-technology and pro-technology. On the horizontal axis, your choices are: compete against China or work with China. This essentially corresponds to anti-globalization and pro-globalization.

On the globalization axis, it's probably better to collaborate instead of compete. Competing is too hard. People never really make any money. They just beat each other to bloody pulp. You probably shouldn't compete with China even if you can win the fight. It's a Pyrrhic victory.

Similarly, it's probably wise to avoid competing on the technology axis as well. Even if you can do square roots faster than computers—which people could still do a few dozen years ago—you *still* shouldn't try to compete. The computers will catch up and overtake you. Human vs. computer chess has gone pretty badly for humans ever since 1997.

Returning to the globalization axis, you're left with cooperating with China. That's better than competing with China. But maybe this isn't the best route to take, either. There is sense in which too many people are myopically focused on working with China. Collaborating with China may be too competitive at this point. Most everyone focuses on globalization instead of technology.

Now the indirect proof is complete: all that's left is working with computers. Of course, indirect proof is tricky. Everything may seem to point in one direction, but that direction may not be right. If you were to change your perspective or your inputs, things might point in an entirely new direction. But the indirect proof is still a useful tool. If nothing else seems to work, you should take a closer look at what seems better.

V. Conversation with Marc Andreessen

Peter Thiel: Marc, you've been involved in the tech industry for two decades. How did you think about future in 1992? In 2002? Now?

Marc Andreessen: My colleagues and I built Mosaic in 1992. It's hard to overstate how contrarian a bet that was at the time. Believing in the whole idea of the Internet was pretty contrarian back then. At the time, the dominant metaphor was the information superhighway. People saw advantages to more information. At some point we got 500 TV channels instead of 3. But much better than *more* TV was *interactive* TV came. That

was supposed to be the next big thing. Dominant leaders in the media industry were completely bought in to ITV. Bill Gates, Larry Ellison—everybody thought interactive was the future. Big companies would continue to rule. Oracle would make the interactive TV software. The information superhighway, by contrast, would be passive. It wouldn't be all that different from traditional old media. In 1992, Internet was quirky, obscure, and academic, just as it had been since 1968.

To be fair, the future still hadn't quite arrived by '92. Much of the huge skepticism about the Internet seemed justified. You all but needed a CS degree in order to log on. It could be pretty slow. But clinging to skepticism in the face of new developments was less understandable. Larry Ellison said in 1995 that the Internet would go nowhere because it'd be too slow. This was puzzling, since you could theoretically run Internet on the same wire that people already had coming into their houses. Cable modems turned out to work pretty well. Really, the causes of peoples' anti-Internet bias back then were the same reasons people fear the Internet today; it's unregulated, decentralized, and anonymous. It's like the Wild West. But people don't like the Wild West. It makes people feel uncomfortable. So to say in 1992 that Internet was going to be *the thing* was very contrarian.

It's also kind of path dependent. The tech nerds who popularized and evangelized the web weren't oracles or prophets who had access to the Truth. To be honest, if we had access to the big power structures and could have easily gone to Oracle, many of us would have fallen in. But we were tech nerds who didn't have that kind of access. So we just made a web browser.

Peter Thiel: What did you actually think would happen with the Internet?

Marc Andreessen: What helped shape our thinking was seeing the whole thing working at the computing research universities. In 1991-92 Illinois had high-speed 45-megabit connections to campus. We had high end workstations and network connectivity. There was streaming video and real-time collaboration. All students had e-mail. It just hadn't extended outside the universities. When you graduated, the assumption was that you just stopped using e-mail. That could only last so long. It quickly became obvious that all this stuff would not stay confined to just research universities.

And then it worked. Mosaic was released in late '92/early '93. In '93 it just took off. There was a classic exponential growth curve. A mailing list that was created for inbound commercial licensing requests got completely flooded with inquiries. At some point you just became stupid if you didn't see that it'd be big.

Peter Thiel: Did you think in the '90s that future would happen sooner than it did?

Marc Andreessen: Yes. The great irony is all the ideas of the '90s were basically correct. They were just too early. We all thought the future would happen very quickly. But instead things crashed and burned. The ideas are really just coming true now. Timing is everything. But it's also the hardest thing to control. It's hard; entrepreneurs are congenitally wired to be too early. And being too early is a bigger problem for entrepreneurs than not being correct. It's very hard to sit and just wait for things to arrive. It almost never works. You burn through your capital. You end up with outdated architecture when the timing *is* right. You destroy your company culture.

Peter Thiel: In the early and mid 2000s, people were very pessimistic about '90s ideas. Is that still the case?

Marc Andreessen: There are two types of people: those who experienced the 2000 crash, and those who did not. The people who did see the crash are deeply psychologically scarred. Like burned-my-face-on-the-stove scarred. They are irreparably damaged. These are the people who love to talk about bubbles. Anywhere and everywhere, they have to find a bubble. They're now in their 30's, 40's, and 50's. They all got burned. As journalists, they covered the carnage. As investors, they suffered tremendously. As employees, they loaded up on worthless stock. So they promised themselves they'd never get burned again. And now, 12 years later, they're still determined not to.

This kind of scarring just doesn't go away. It has to be killed off. People who suffered through the crash of 1929 never believed in stocks again. They literally had to die off before a new generation of professional investors got back into stocks and the market started to grow again. Today, we're halfway through the generational effects of the dot com crash.

That's the good news for students and young entrepreneurs today. They missed the late '90s tech scene, so they are—at least as to the crash—perfectly psychologically healthy. When I brought up Netscape in conversation one time, Mark Zuckerberg asked: "What did Netscape do again?" I was shocked. But he looked at me and said, "Dude, I was in junior high. I wasn't paying attention." So that's good. Entrepreneurs in their mid-to-late 20s are good. But the people who went through the crash are far less lucky. Most are scarred.

Peter Thiel: Your claim is that software is eating the world. Tell us how you see that unfolding over the next decade.

Marc Andreessen: There are three versions of the hypothesis: the weak, strong, and strongest version.

The basic, weak form is that software is eating the tech/computer industry. The value of computers is increasingly software, not hardware. The move to cloud computing is illustrative. There's been a shift to high volume, low cost models where software controls. It's very different from the old model.

The strong form is that software is eating many other industries that have not been subject to rapid technological change. Take newspapers, for example. The newspaper industry has been pretty much the same, technologically, for about 500 years! There had been no significant technological disruption since the 15th century. And then boom! The digital transformation happens, and the industry frantically has to try and cope with the change.

The strongest form is that, as a consequence of all this, Silicon Valley type software companies will end up eating everything. The kinds of companies we build in the Valley will rule pretty much every industry. These companies have software at their very core. They know how to develop software. They know the economics of software. They make engineering the priority. And that's why they'll win.

All this is reflected in the Andreessen Horowitz investment thesis. We don't do cleantech or biotech. We do things that are based on software. If software is the heart of the company—if things would collapse if you ripped out your key development team—perfect. The companies that will end up dominating most industries are the ones with the same set of management practices and characteristics that you see at Facebook or Google. It will be a rolling process, of course, and the backlash will be intense. Dinosaurs are not in favor or being replaced by birds.

Peter Thiel: Are there some industries that are too dangerous to disrupt? Disruptive children go to the principal's office. Disruptive companies like Napster can get crushed. Can you succeed with head-on competition, even if you have the Silicon Valley model in other respects?

Marc Andreessen: Look at what Spotify is doing, which is something very different than what Napster did. Spotify is writing huge checks to labels. The labels appreciate that. And Spotify put itself in position to write those checks from day one. It launched in Sweden first, for example, because it wasn't a very big market for CDs. It's a disruptive model but they found a way to soften the blow. When you start a conversation with "By the way, here's some money," things tend to go a little better.

It's still a high-pressure move. They are running the gauntlet. The jury is still out on whether it's going to work or not going forward. The guys on the content side are certainly pretty nervous about it. This stuff can go wrong in all kinds of ways. Spotify and Netflix surely know that. The danger in just paying off the content people is that the content people may just take all your money and *then* put you out of business. If you play things right, you win. Play them wrong, and the incumbents end up owning everything.

Peter Thiel: Some context: Netflix ran into trouble a year ago when content providers raised rates. Spotify has tried to protect itself against this by having rolling contracts that expire at different times so that industry players can't gang up and collectively demand rate hikes.

Marc Andreessen: And record companies are trying to counter by doing shorter deals, and in some cases taking non-dilutable equity stakes. It is possible that they could end up owning all the money and all the equity. Spotify and Netflix are spectacular companies. But, because of the nature of their business, they have to run the gauntlet. In general, you should try the indirect path where possible. If you have to compete, try to do it indirectly and innovate and you may come out ahead.

Peter Thiel: What areas do you think are particularly promising in the very near term?

Marc Andreessen: Probably retail. We're seeing and will continue to get e-commerce 2.0, that is, e-commerce that's not just for nerds. The 1.0 was search driven. You go to Amazon or eBay, search for a thing, and buy it. That works great if you're shopping for particular stuff. The 2.0 model involves a deeper understanding of consumer behavior. These are companies like Warby Parker and Airbnb. It's happening vertical by vertical. And it's likely to keep happening throughout the retail world because retail is really bad to start with. There are very high fixed costs of having stores and inventory. Margins are very small to begin with. If you take away just 5 or 10%, things collapse. Best Buy, for example, has two problems. First, people can get pretty much everything online. Second, even if you do want to shop brick and mortar, software is eating up what you can buy at Best Buy in the first place.

Peter Thiel: An online pet food company is the paradigm example.

Marc Andreessen: And that's not such a bad idea anymore! Diapers.com was bought by Amazon for \$450m. Golfballs.com turns out to be a pretty good business. Even Webvan is coming back! The grocery delivery company failed miserably back in the '90s. But now, city by city, it's back, trying to figure out crowdsourced delivery. The market is so much bigger now. There were about 50 million people online in the '90s. Today it's more like 2.5 billion. People have gotten acclimated to e-commerce. The default assumption is that everything is available online now.

Peter Thiel plays real hedge fund. Andreessen Horowitz plays fake hedge fund. And one if it's fake hedge fund strategies is short retail, go long e-commerce.

Peter Thiel: What new perspectives do you have as VC that are different from your perspectives as entrepreneur? Have you gained any new insights from the other side of the table?

Marc Andreessen: The big, almost philosophical difference goes back to the timing issue. For entrepreneurs, timing is a huge risk. You have to innovate at the right time. You can't be too early. This is *really* dangerous because you essentially make a one-time bet. It's rare to start the same company five years later if you try it once and were wrong on timing. Jonathan Abrams did Friendster but not Facebook.

Things are different with venture capital. To stay in business for 20 years or more, you have to take a portfolio approach. Ideas are no longer one-time bets. If we believe in an idea and back the company that fails at it, it's probably still a good idea. If someone good wants to do the same thing four years later, that's probably a good investment. Most VCs won't do this. They'll be too scarred from the initial failure. But tracking systematically failures is important. Look at Apple's Newton in the early '90s. Mobile was the central obsession of many smart VCs in the Valley. That was two decades too early. But rather than swear off mobile altogether, it made more sense to table it for awhile and wait for it to get figured out later.

Peter Thiel: When people invest and things don't work out, the right thing to do is course correct. And when people don't invest and something works, they remain anchored to their original view and tend to be very cynical.

Marc Andreessen: Exactly. The more successful missed investments get, the more expert we become on what's wrong with them. (Dammit square! J)

But seriously—if you think you can execute on an idea that someone tried 5-10 years ago and failed, good VCs will be open to it. You just have to be able to show that now is the time.

Peter Thiel: What's the one thing that younger entrepreneurs don't know that they should?

Marc Andreessen: The number one reason that we pass on entrepreneurs we'd otherwise like to back is focusing on product to the exclusion of everything else. We tend to cultivate and glorify this mentality in the Valley. We're all enamored with lean startup mode. Engineering and product are key. There is a lot of genius to this, and it has helped create higher quality companies. But the dark side is that it seems to give entrepreneurs excuses not to do the hard stuff of sales and marketing. Many entrepreneurs who build great products simply don't have a good distribution strategy. Even worse is when they insist that they don't need one, or call no distribution strategy a "viral marketing strategy."

Peter Thiel: We've discussed before why one should never take it at face value when successful companies say they do no sales or marketing. Because that, itself, is probably a sales pitch.

Marc Andreessen: We hear it all the time: "We'll be like Salesforce.com—no sales team required, since the product will sell itself." This is always puzzling. Salesforce.com has a huge, modern sales force. The tagline is "No software," not "No sales." AH is a sucker for people who have sales and marketing figured out.

Peter Thiel: It may also be time to rethink complex sales. People have been scarred from the ‘90s experience, where businesses predicated on complex sales failed. It was very hard to get people to do business development deals in the early 2000s. But doing these deals can be very advantageous. Google did a phenomenal BD deal with Yahoo. People don’t typically recognize how great it was for Google. Google doesn’t like to talk about it because it only wants to talk about its engineering. Yahoo doesn’t want to talk about it because it’s embarrassing.

Question from audience: What are some pitfalls to avoid in thinking about the future?

Peter Thiel: You can go wrong in a few ways. One is that the future is too far away, so you might be right on substance but you’ll be wrong on timing. The other is that the future is here, but everyone else is already doing it.

It’s like surfing. The goal is to catch a big wave. If you think a big wave is coming, you paddle really hard. Sometimes there’s actually no wave, and that sucks.

But you can’t just wait to be sure there’s a wave before you start paddling. You’ll miss it entirely. You have to paddle early, and then let the wave catch you. The question is, how do you figure out when the next big wave is likely to come?

It’s a hard question. At the margins, it’s better err on the side of paddling where there’s no wave than paddling too late and missing a good wave. Trying to start the next great social networking company is current wave thinking. You can paddle hard, but you’ve missed it. Social networking is not the next wave. So the bias should be to err toward the future. Then again, the bigger bias should be to not err at all.

Question from audience: Is it big wave? Or do waves come industry by industry?

Marc Andreessen: Industry by industry. Some industries like finance, law, and health have oligopoly structures that are often intertwined with government. Banks complain about regulation, but are very often protected by it. Citibank’s core competency could be said to be political savvy and navigating through bureaucracy. So there are all sorts of industries with complex regulatory hurdles. It’s fun to see what’s tipping and what’s not. There are huge opportunities in law, for example. You may think those are ripe now, and they may well be. But maybe they are decades out. In VC, you literally never know when some 22 year-old is going to prove everybody wrong.

Question from audience: How do patents relate to the software-eats-the-world phenomenon?

Marc Andreessen: The core problem with patents is that patent examiners don’t get it anymore. They simply don’t and can’t know what is novel versus what isn’t. So we get far too many patents. As a tech company, you have two extreme choices: you could spend your entire life fighting patents, or you could spend all your money licensing usage. Neither of those extremes is good. You need to find the balance that lets you think about patents least. It’s basically a distracting regulatory tax.

Peter Thiel: In any litigation, you have four parties. You have the two parties, and you have the two sets of lawyers. The lawyers are almost always scared of losing. The defense lawyer will talk the client into settling. The question is: do we get somewhere when people are willing to fight to the end to beat back bad patent claims? Or do you have to concede and basically have a patent tax? High litigation costs could be worth it if

you only have to fight a few times. The danger is that you fight and win but fail to set any sort of deterring precedent, in which case the suits keep coming and you're even worse off.

Marc Andreessen: There are some areas in tech—drugs and mechanical equipment, for instance—where parents are fundamental. In these areas there are long established historical norms for who gets to do what. But in software, things change extremely quickly. The big companies used to have huge war chests full of patents and use them to squash little guys. Now they're fighting each other. The ultimate terminal state of big companies seems to be a state in which they build nothing. Instead, they just add 10,000 patents to their portfolio every year and try to extract money through licensing. It'd be nice if none of this were the case. But it's not startups' fault that the patent system is broken. So if you have a startup, you just have to fight through it. Find the best middle ground strategy.

Peter Thiel: In some sense, it may be good to have patent problems. If you have to have problems, these are the kind you want to have. It means that you've done something valuable along the way. No one would be coming after you if you didn't have good technology. So it's a problem you want to have, even if you don't.

Question from audience: Has the critical mass of Internet users been reached? Is it harder to be too early now?

Marc Andreessen: For a straight Internet idea, yes. It's less easy to be too early, which is good. Look at Golfballs.com. Everybody who plays golf is now online. That is a huge change from the '90s when far fewer people were dialing up.

Things like mobile are trickier. Some say that smartphones have tipped. We're currently at about 50% penetration. It may be that things have yet to tip. It seems likely, for instance, that three years from now there will be 5 billion smartphones. The days in which you can buy a *non* smart phone are probably numbered. And a whole new set of gatekeepers will come with that shift.

Peter Thiel: The big worry with mobile is that any great mobile distribution technique will be disallowed and then copied by Apple and Android. It's a big market, but it's far from clear that you can wrest power away from the gatekeepers.

Marc Andreessen: Just recently, Apple blocked any iOS applications from using Dropbox. The rationale was that allowing apps to interact with Dropbox encourages people not to buy stuff through the App Store. That doesn't seem like a great argument. But it's like fighting city hall. Even a big important company like Dropbox can get stopped dead in its tracks by Apple.

Question from audience: What have you learned about boards from sitting on boards of successful companies?

Marc Andreessen: Generally, you must try to build a board that can help you. Avoid putting crazy people on your board. It's like getting married. Most people end up in bad marriages. Board people can be really bad. When things go wrong, the bias is to do *something*. But that something is often worse than the problem. Bad board members frequently don't see that.

Peter Thiel: If you want board to do things effectively, it should be small. Three people is the best size. The more people you have, the worse the coordination problem gets. If you want your board to do nothing at all,

you should probably make it enormous. Non-profit organizations, for instance, sometimes have boards of 50 people or more. This provides an incredible benefit to whatever quasi-dictatorial person runs the non-profit. A board of that size effectively means no checks on management. So if you want an ineffective board for whatever reason, make it very big.

Marc Andreessen: I've never seen a contentious board vote. I've seen every other thing go wrong. But never a contentious vote. Problems get dealt with. They either kill the company, or you figure it out in another way.

There is probably too much in the air about optimal legal terms and process. Not enough attention is paid to the people. Startups are like sausage factories. People love eating sausage. But no one wants to watch the sausage get made. Even the seemingly glorious startups only seem that way. They've had crisis after crises too. Things go horribly wrong. You fight your way through it. What matters more: what processes you follow? Or who is with you in the bunker? Entrepreneurs don't think about this enough. They don't vet their VCs enough.

Question from audience: With businesses like Netflix, it seems like the key thing is customer psychology and behavior, not some technical achievement. But you said you like companies with software at their core. Is there a disconnect?

Marc Andreessen: It's an "and," not an "or." You have to have software at the core, **and** then have great sales and marketing too. That is the winning formula. But properly run software companies have great sales and engineering cultures.

What's ideal is to have a founder/CEO who is a product person. Sales operators handle the sales force. *The sales force does not build the product!* In poorly run software companies, sales orders product around. The company quickly turns into a consulting company. But if a product person is running the company, he or she can just lay down the law. This is why investors are often leery to invest in companies where you have to hire a new CEO. That CEO is less likely to be the good product person. You can't just bring in a Pepsi marketing executive to replace Steve Jobs.

Peter Thiel: Are there any exceptions to this? Like Oracle?

Marc Andreessen: No. Larry Ellison is a product guy. Granted, an extremely money-centric product guy. He's always the CEO. One time he broke his back bodysurfing. He ran the company from the hospital bed. And he's always had a #2, like Mark Hurd. There's been a whole series of #2s. But Larry always has a Cheryl.

Salespeople can be very good at optimizing a company over a 2-4 year period. The AH fake hedge fund trade is: when a sales guy replaces a product guy as CEO, go long 2 years, then short.

There are a bunch of exceptions. Meg Whitman gets criticized for late eBay, but early on, she built it up and did a fantastic job. John Chambers clearly did a good job building Cisco, even if things got complicated later on. Jeff Bezos was a hedge fund guy. Good leaders come from all over the place.

Even designers are becoming great CEOs—just look at Airbnb. They've got the whole company thinking in terms of design. Design is becoming increasingly important. Apple's success doesn't come from their

hardware. It comes from OSX and iOS. Design is layered on top of that. A lot of the talk about the beautiful hardware is just the press not getting it. The best designers are the software-intensive ones, who understand it at the deep level. It's not just about surface aesthetics.

Question from audience: The web browser came out of universities. 10 years later Google came out of Stanford. Do you look at university research departments while searching for great future companies?

Marc Andreessen: Sure. A lot of stuff we've invested in was developed in research labs 5-10 years ago. Looking at the Stanford and MIT research labs is a great way of assessing what kinds of technologies might become products in the next couple of years.

Synthetic biology is one example. That might be the next big thing. It's basically biology—creating new biological constructs with code. This freaks people out. It's very scary stuff. But it actually seems to work, and it could be huge.

Question from audience: if a certain background isn't required, what makes for a good CEO?

Marc Andreessen: At AH we think that being CEO is a learnable skill. This is controversial in the VC world. Most VCs seem to think that CEOs come prepackaged in full form, shrink wrapped from the CEO mill. They speak of "world class" CEOs, who usually have uniquely great hair. We shouldn't be too glib about this; many very successful VCs have the "don't screw around with CEO job" mentality, and maybe they're right. Their success sort of speaks for itself. But the critique is that with the "world class" CEO model, you miss out on Microsoft, Google, and Facebook. The CEOs of those companies, of course, turned out to be excellent. But they were also the product people who built the companies. It's fair to say that the most important companies are founded and run by people who haven't been CEO before. They learn on the job. This is scary for VCs. It's riskier. But the payoff can be much greater.

The question is simple: does this person want to learn how to be a good CEO? Some people are psychologically unsuited for the job. Others really want to learn how to do it, and they do well. One thing to learn is that managing people is different than managing managers. Managing managers is scalable. Managing people is not. Once you learn how to manage managers, you're well on your way to be CEO. You just have to learn enough of the legal stuff to avoid going to jail, enough finance to get money, and enough sales to sell product.

But the Valley is infected by the Dilbert view; everybody thinks management is a bunch of idiots, and that engineers must save the day by doing the right things on the side. That's not right. Management is extremely important. We are looking for the best outcomes on the power law curve. You have to look at what's worked well and try to reverse engineer it. Great management and a great product person running the company is characteristic of the very best companies.

Question from audience: What's more fun: found company or being a VC?

Marc Andreessen: They are pretty different. Generally founders would probably dislike being VCs and vice versa. The typical founder/CEO is a control freak. He would hate VC because VCs can't give orders. Instead they have to exercise power through influence. But VCs might not enjoy being founders. VCs get the luxury of having opinions without having to execute on them. Executing on them can be really hard and unpleasant. So different people may prefer one or the other. I like them both, but that may not be all that common.

Question from audience: What would you advise entrepreneurial students to do: found a company with a friend from school? Or go work at a 10-person startup?

Marc Andreessen: Starting a company from scratch is hard. Doing it straight out of school is even harder. You could join a small startup to see how young companies work. But there are lots of different ways to learn. It may be better to go to Facebook or Airbnb and see how things work there, because you know what you'll be learning is what works. That said it's hard to sit here and advise against starting a company. AH backs founders straight out of school. They can be great founders. But most people benefit from seeing how companies actually work first.

Peter Thiel: The counterargument is that the people behind Google, Microsoft, or Facebook didn't really have much experience. If you look at very successful companies, it's very common that the founders had no prior experience at all. The questions to ask when thinking about experience are: what translates? How? If you do join a 10-20 person startup that fails, maybe you learn what not to do. But maybe it would've failed for other reasons and you don't actually learn all the pitfalls. Or maybe you get scarred and never try anything risky again.

What translates from going to work at a big company? The problem with that is that everything kind of works automatically. It's very difficult to learn about startups if you go work at Microsoft or Google. They are great companies with phenomenal people. But there are shockingly few companies started by those people. One theory is that they are too sheltered. They are just too far removed from startup processes.

Better than thinking about where to go is thinking about what to do. The key questions are: what do you believe in? What makes sense? What's going to work? If there is indeed a power law distribution in company outcomes, it's really important to get into the single company you think is the best. The process question of what stage a company is at is less important than the substance of what you're doing.

Marc Andreessen: I interned at IBM in 1991. It was extremely screwed up. Those of you who follow IBM history will know it as the John Akers era. I was pretty much given the codex of how to screw up a company. You learn *everything* at a dysfunctional company. It was fascinating. Once I got to see the org chart. There were 400,000 employees. I was 14 levels below the CEO. Which meant that my boss's boss's boss's boss's boss's boss's boss was still 7 levels below the CEO.

The skill that you learn at IBM is how to exist at IBM. It's completely self-referential. It's the terminal state. People don't leave.

Peter Thiel's CS183: Startup - Class 11 Notes Essay

Here is an essay version of class notes from Class 11 of CS183: Startup. Errors and omissions are mine. Credit for good stuff is Peter's.

Class 11 Notes Essay—Secrets

I. Secrets

Back in class one, we identified a very key question that you should continually ask yourself: what important truth do very few people agree with you on? To a first approximation, the correct answer is going to be a secret. Secrets are unpopular or unconventional truths. So if you come up with a good answer, that's your secret.

How many secrets are there in the world? Recall that, reframed in a business context, the key question is: what great company is no one starting? If there are many possible answers, it means that there are many great companies that could be created. If there are no good answers, it's probably a very bad idea to start a company. From this perspective, the question of how many secrets exist in our world is roughly equivalent to how many startups people should start.

How hard it is to obtain the truth is a key factor to consider when thinking about secrets. Easy truths are simply accepted conventions. Pretty much everybody knows them. On the other side of the spectrum are things that are impossible to figure out. These are mysteries, not secrets. Take superstring theory in physics, for instance. You can't really design experiments to test it. The big criticism is that no one could ever actually figure it out. But is it just really hard? Or is it a fool's errand? This distinction is important. Intermediate, difficult things are at least possible. Impossible things are not. Knowing the difference is the difference between pursuing lucrative ventures and guaranteed failure.

Discovery is the process of exposing secrets. The secrets are *dis-covered*; the cover is removed from the secret. Triangle math was hard for Pythagoras to discover. There were various Pythagorean mystery cults where the initiated learned about crazy new things like irrational numbers. But then it all became convention.

It can also work the other way, too. Conventions can get covered up and become secrets again. It's often the case that people stop believing things that they or previous generations had believed in the past.

Some secrets are small and incremental. Others are very big. Some secrets—gossip, for instance—are just silly. And of course there are esoteric secrets—the stuff of tarot cards and numerology. Silly and esoteric secrets don't matter much. And small secrets are of small importance. The focus should be on the secrets that matter: the big secrets that are true.

The purpose of this class is to share and discuss some secrets about starting companies. The big ones so far have involved monopoly vs. competition, the power law, and the importance of distribution.

“Capitalism and competition are antonyms.” That is a secret; it is an important truth, and most people disagree with it. People generally believe that the differences between firms are pretty small. They miss the big monopoly secret because they don't see through the human secrets behind it. Monopolists pretend that

they're not monopolists ("Don't regulate us!") and non-monopolists pretend that they are ("We are so big and important!"). Things only tend to *look* similar on the surface.

The power law secret operates similarly. In one sense it's a secret about finance. Startup outcomes are not evenly distributed; they follow a power law distribution. But in another sense it's a very human secret. People are uncomfortable talking about inequality, so they either ignore it or rationalize it away. It is psychologically difficult for investors to admit that their best investment is worth more than the rest of their portfolio companies combined. So they ignore or hide that fact, and it becomes a secret.

The distribution secret also has two sides to it. Distribution is much more important than people think. That makes it a business secret. But it's a human secret too, since the people involved in distribution work very hard to hide what's going on. Salespeople do best when people do not know they're dealing with salespeople.

II. The Next Secret

Probably the biggest secret—bigger than monopoly/competition, power law, or distribution—is that there are many important secrets left. This used to be a convention forty or fifty years ago. Everyone believed that there was much more left to do. But generally speaking, we no longer believe that. It's become a secret again.

Consider the original question for a moment—what important truth do people not agree with you on? It seems like an easy question. That is, until you try to answer it. It turns out that it's really tough. In fact, when people actually think about it for a bit, they very often conclude that it's impossible. They start at one extreme and then just move all the way to the other.

But that is too big a move. That answers do not come easily does not mean that they don't exist. There *are* good answers to the question. Secrets exist. And finding them is neither easy nor impossible—just hard.

III. The Case Against Secrets

The common view is that there are no secrets left. It's a plausible view. If it's wrong, it's not obviously wrong. To evaluate it, we must first understand why people don't believe in secrets anymore.

A. Anti-secret Extremism

The extreme representative of the conventional view is Ted Kaczynski, more infamously known as the Unabomber. He was a child prodigy. IQ of 167. A top student at Harvard. PhD in math from Michigan. Professor of math at UC Berkeley. But then he started a solo bombing campaign after becoming disenchanted with science and technology. He killed 3 people and injured 23 more. The victims included computer store owners, technical grad students, geneticists, etc. Finally he was found and arrested in 1996.

But in late 1995 the FBI didn't really have a clue who or where the Unabomber was. Kaczynski had written a manifesto and anonymously mailed it to the press. The government gave the go-ahead to print it, hoping for a break in the case. That ended up working, as Kaczynski's brother recognized the writing and turned him in.

But more interesting than how Kaczynski was caught was the manifesto itself. It was basically a long, crazy anti-tech diatribe. The core of the argument was that you could divide human goals into three groups:

1. Goals that can be satisfied with minimal effort;
2. Goals that can be satisfied with serious effort, and;
3. Goals that are impossible to satisfy.

It was the classic easy/hard/impossible trichotomy. Kaczynski argued that people are depressed because the only things left are (1) easy things or (3) impossible things. What you can do, even kids can do. But what you can't do, even Einstein couldn't do. So Kaczynski's idea was to destroy technology, get rid of all bureaucracy and technical processes, and let people start over and work on hard problems anew. That, he thought, would be much more fulfilling.

A less sinister version of this is the hipster phenomenon. Cool people make some ironic anti-tech juxtapositional statement and thereby become even cooler. Never mind that gears and brakes on bikes are actually pretty useful; hipsters do without. This is a somewhat silly manifestation of the wider dynamic. But in some form or another, a lot of people believe that there are only easy truths and impossible truths left. They tend not to believe in hard truths that can be solved with technology.

Pretty much all fundamentalists think this way. Take religious fundamentalism, for example. There are lots of easy truths that even kids know. And then there are the mysteries of God, which can't be explained. In between—the zone of hard truths—is heresy. Environmental fundamentalism works the same way. The easy truth is that we must protect the environment. Beyond that, Mother Nature knows best, and she cannot be questioned. There's even a market version of this, too. The value of things is set by the market. Even a child can look up stock prices. Prices are easy truths. But those truths must be accepted, not questioned. The market knows far more than you could ever know. Even Einstein couldn't outguess God, Nature, or Market.



B. The Geography of Secrets

Why has our society come to believe that there are no hard secrets left? It probably starts with geography. There are no real white spaces left on the map anymore. If you grew up in 18th century, there were still lots of unexplored places. You could listen to captivating stories about explorers and foreign adventures and, if you wanted, go become a real explorer yourself. This was probably true up through the 19th and early 20th centuries, when National Geographic still published tales of exotic, underexplored places.

But now you can't really be an explorer anymore. Or at least it's very hard to explore the unexplored. People have done it all already. Maybe there are something like 100 uncontacted tribes somewhere deep in the Amazon. Maybe they'd have something interesting to teach us. But maybe not. Either way, most people don't seem to care much.

The oceans remain unexplored in a fairly interesting way. The planet is 72% covered by oceans. Some 90% of the inhabited ocean is deep sea. There have been only about 200 hours of human exploration there. So oceans are the last big geographic piece that people aren't really looking at. But that may be because the default assumption is right; there's nothing terribly interesting there. Deep sea exploration simply lacks the magic of exploring new lands and continents.

The frontier of knowledge seems to have waned along with the geographical frontier. People are increasingly pessimistic about the existence of new and interesting things. Can we go to the moon? We've done that already. Mars? Impossible, many people say. What about chemistry? Can we identify oxygen? That's been trivial since the 18th century. So what about finding new elements? That's probably a fool's errand. The periodic table seems pretty set. It may be impossible to discover anything new there. The frontier is closed. There is nothing left to discover.

C. Secrets and Sociology

Four primary things have been driving people's disbelief in secrets. First is the pervasive incrementalism in our society. People seem to think that the right way to go about doing things is to proceed one very small step at a time. Any secrets that we're incentivized to discover are microsecrets. Don't try anything too hard in the classroom; just do what's asked of you a bit better than the others and you'll get an A. This dynamic exists all the way up through pre-tenure. Academics are incented by volume, not importance. The goal is to publish lots of papers, each of which is, in practice at least, new only in some small incremental way.

Second, people are becoming more risk-averse. People today tend to be scared of secrets. They are scared of being wrong. Of course, secrets are supposed to be true. But in practice, what's true of all secrets is that there is good chance they're wrong. If your goal is to never make mistake in your life, you should definitely never think about secrets. Thinking outside the mainstream will be dangerous for you. The prospect of dedicating your life to something that no one else believes in is hard enough. It would be unbearable if you turned out to be wrong.

Third is complacency. There's really no need to believe in secrets today. Law school deans at Harvard and Yale give the same speech to incoming first year students every fall: "You're set. You got into this elite school. Your worries are over." Whether or not such complacency is justified (and we should suspect it's not), it's probably the kind of thing that's true only if you don't believe in it. If you believe in it, you're probably in a lot of trouble.

Finally, some pull towards egalitarianism is driving us away from secrets. We find it increasingly hard to believe that some people have important insight into reality that other people do not. Prophets have fallen out of fashion. Having visions of the future is seen as crazy. In 1939 Einstein sent a letter to President Roosevelt urging him to get serious about nuclear power and atomic weaponry. Roosevelt read it and got serious. Today, such a letter would get lost in the White House mailroom. Anyone who opened it would probably think it was a joke. Nuclear weapons seemed very outside of possible in the late 1930s. But visions of the future were taken seriously then.

In defense of the case against secrets, all these things—incrementalism, risk aversion, complacency, and egalitarianism—have worked pretty well for most people. Distrusting prophets has become a good heuristic. 30 years ago, people started cults. And other people joined them. Someone would claim to have some great secret that no one else knew about. The guru or cult leader was the paragon of anti-egalitarianism. People were encouraged risk everything to join the cult because that was the only path to Truth. Complacency and incrementalism meant missing out. Today, it's probably impossible to start those kinds of cults, which is good. People simply wouldn't buy in.

IV. The Case Against the Case Against Secrets

So there's something to be said for the case against secrets. But the case *against* that case is stronger. The problem with the idea that there are no hard truths left is that it's wrong. There *are* secrets within reach. When you drill down on it, belief in a society without secrets has some very strange implications indeed.

On some level, every form of injustice involves a secret. Something is being done. It's unjust. It's happening because society allows it to happen. The majority of people don't understand the injustice of it. Invariably that's understood only by a small minority. In the '50s and '60s, there were a number of different views about things being very unjust. These secrets became conventions over time. The majority was won over. So to say that there are no secrets left today means, in some sense, that we are either a completely just society or we shouldn't try to be. Either everything is right as it is. Or whatever injustice exists is mysterious and can't be fixed. Each of those positions seems very odd.

In the economics context, disbelieving in secrets leads to the conclusion that markets are completely efficient. But we know that's not true. We have experienced decades of extraordinary inefficiencies. You weren't allowed to say in 2000 that people were behaving somewhat irrationally regarding Internet companies. You weren't allowed to say in 2007 that there was a housing bubble. The market could not be understood. To the extent anyone could understand, it was the Fed. They had a model that said no more than \$25bn could be lost in the worst-case scenario. There was no second-guessing. We all know how that turned out.

Political dissent requires secrets too. Any sort of extreme criticism of the government is necessarily based on some secret truth that things are very wrong. Some of these secrets are probably right. Many others are not. But disbelieving in secrets generally is equivalent to saying that it's not possible for any political dissident to be right, ever. This plays out in interesting ways. Since no one believes in secret truths anymore, the political tactic that people use is to try to discredit the other side by associating them with conspiracy theorists. If you are a Democrat, you rage about Tea Party activists and their secret beliefs. If you're Republican, you profile Occupy Wall Street people and talk about their wild theories. All conspiracy theories are crazy and wrong. There are never any secrets.

There is an interesting version of this in corporate governance. Consider the HP board drama of the past decade. The backstory is that HP went through a bunch of CEOs. In 2004-2005 there was a big debate amongst HP board members about what the board should spend its time talking about. On one end of debate was Tom Perkins, an engineer, longtime HP veteran, and co-founder of the VC firm Kleiner Perkins. He thought that board should spend its time talking about new technology and developments—that is, hard substantive problems. On the other side was Patricia Dunn, who argued that science and tech were too difficult and were beyond the board's competence. Dunn thought that the board should focus on processes; was everything going okay in the accounting department? Were people following all the ethical rules?

Against this backdrop came a very contested acquisition of Compaq. Someone on the board started leaking information out to the press—a clear violation of the proper processes. Dunn tried to find the leak. Wiretaps were set up. But that caused quite a bit of trouble because it turns out that wiretapping is illegal. So there was this nested series of bizarre events relating to process. There were process violations that sought to catch the people who were violating proper process protocol on a board that wanted to do nothing but focus on process.

Tom Perkins believed in secrets. Hard but solvable problems exist, and we should talk about them. But if you believe that there are no secrets—that everything is either reducible to simple processes or is impossibly hard—you end up with something like the HP fiasco. It's hard to work toward a radically better future if you don't believe in secrets.

V. The Case For Secrets

Of course, a case against a case against something isn't a case *for* that thing. If secrets exist, there should be affirmative argument for why. So why should we think that there are still secrets?

That difficult problems do get solved is evidence that secrets exist. It's not always straightforward to tell whether a given problem is merely hard or actually impossible. But the people who actually solve hard problems are people who believe in secrets. If you believe something is hard, you might still think you can do it. You'll try things, and maybe you'll succeed. But if you think something is impossible, you won't even try.

Fermat's last theorem is a good example. It states that no three positive integers a , b , and c can satisfy the equation $a^n + b^n = c^n$ for any n greater than two. Mathematician Andrew Wiles started working on it in 1986. He managed to prove it in 1995. No one would ever succeed in doing these incredibly hard things if they didn't think that it was possible. In some sense you can't have meaningful progress if you don't think that there are solvable secrets out there.

The story of web 2.0 and the information age has been the story that, on some level, many small secrets can add up and change the world. It's easy to make fun of things like Twitter. You're limited to 140 characters. No individual tweet is particularly important. Most are probably kind of useless. But in the aggregate, the platform has proven quite powerful. Social media has, the story goes, played a non-trivial role in great political transformation and even governmental overthrow. The secret force behind this web 2.0 empowerment is the fact that there are far more secrets that people think. If things are very different in the increasingly transparent world, it just means that they were covered up before. To the extent that things are not transparent, they are secretive. And all these small secrets add up to something very big indeed.

The big version of this is WikiLeaks. The Julian Assange line is that, "New technology... can give us practical methods for preventing or reducing important communication between authoritarian conspirators." Conspiracy is broadly defined as anything involving any information that's shared between a few people but not amongst everybody. The crazy twist here is that more secrets ended up coming out than Assange probably would have liked. There are so many secrets that what they are isn't the only factor. What can matter even more is the order in which they get revealed. Does the secret that brings down a government get revealed before the secret that would destroy its revealer?

VI. How To Find Secrets

A. Search Methodology

There is no straightforward formula that can be used to find secrets. There are certainly reasons to suspect that there are many of them left. But there are problems with just trying to hammer out a complete list. First, that list would be grossly incomplete. No one person can know every secret, since the good ones necessarily involve really hard problems. Second, ubiquitous distribution of a list of secrets would change their character; the secrets would cease being secrets and would become conventions as soon as people read and accepted them.

So you can't generate some exhaustive list. But what you can do is develop a good method or approach to finding secrets. We know that important secrets are neither small nor silly nor esoteric. The important ones are the big ones that are true. So those are the first two criteria to build into your model. You can safely discard anything that is small or false.

From there it's worth making a rough division between two different types of secrets. There are secrets of nature and then there are secrets about people. Natural secrets involve science and the world around us. The process of finding them involves going out and getting the universe to yield its secrets to us. Secrets about people are different. These are things that people hide because they don't want other people to know about them. So two distinct questions to ask are: What secrets is nature not telling you? What secrets are people not telling you?

There is something to be said for both approaches. But the importance of human secrets is probably underappreciated. It may be worthwhile to focus more on human secrets, both because they can be very important in their own right and because they can help us get to the secrets of nature. What aren't people telling you can very often give you great insight as to where you should be directing your attention.

On one level, the anti-competition, power law, and distribution secrets are all secrets about nature. But they're also secrets hidden by people. That is crucial to remember. Suppose you're doing an experiment in a lab. You're trying to figure out a natural secret. But every night another person comes into the lab and messes with your results. You won't understand what's going on if you confine your thinking to the nature side of things. It's not enough to find an interesting experiment and try to do it. You have to understand the human piece too. It is the intersection of natural secrets and human secrets that is most interesting and enlightening.

But the general bias is that secrets about nature are the really important ones. Natural secrets are metaphysical. They deal with the fundamental nature of universe. If you think that these secrets are foundational, you end up concluding that physics is the fundamental science. Studying nature becomes the most important thing you could possibly do. This is why physics Ph.D's are notoriously difficult to work with; because they know fundamental things, they think they know all things. It's not clear how many levels up that logic can go without getting too twisted. Does understanding physics automatically make you a great marriage counselor? Does a gravity theorist know more about your business than you do? At PayPal, a physics PhD and prospective hire once interrupted his interviewer early-to-mid-question by shouting, "Stop! I already know what you're going to ask!" He was wrong. He didn't get hired.

The alternative, underexplored route is secrets about people. These might be political secrets. Or they might be anthropological or psychological secrets. Here, you just ask the questions and see where they lead. What kinds of things are we allowed to talk about? Are there areas that people can't look into? What is explicitly

forbidden? What is implicitly off-limits or taboo? Looking for secrets in this way, at least at the outset, is more promising than trying to find natural secrets. But the secrets themselves tend to be more dangerous. Natural secrets are transparently hard, but are also politically safe. No one really cares about superstring theory. It wouldn't really change our daily lives if it turned out to be true. Human secrets are different. There's often much more at stake there.

Consider the anti-competition secret again. If you didn't already know it, there are two approaches you could use to figure it out. The first is the human approach. You could ask: what can people who are running companies not say? That would get you thinking, and you would soon realize that monopolists have to pretend that they are small and things are enormously competitive, while non-monopolists have to pretend they are large players with a permanent edge. The other route you could take is the Econ 1 route where the fact that economic profits get competed away in perfect competition is a secret about nature. Either approach could work. But you get there much faster if you ask the people question. The same is true with the power law secret. You could start with quantitative analysis, plot out the distribution of startup outcomes, and go from there. Or you could look at what VCs say, wonder what they can't say, and think about why.

B. The Search For Secrets Applied

Many venture capitalists seem to be looking for incremental improvements—small secrets, if they're even secrets at all. Founders Fund is more interested in looking for big secrets. One way to get started thinking about big secrets is to think about majors that aren't at Stanford. Physics, for example, is a real major at all real universities. So ignore it for a moment. The opposite of physics might be nutrition. Stanford doesn't have it. Real universities don't let you major in nutrition.

That might mean we're onto something. And indeed, one company that Founders Fund has found particularly interesting is putting together a sort of Manhattan Project for Nutrition. Most top scientists have gone into fields other than nutrition over the past couple of decades. Most of the big studies were done 30 or 40 years ago. There's not really an incentive to study nutrition today. So the business plan is to get the six best possible people on it and figure things out definitively. There is plenty of room for improvement; people know more about the universe than about the human body. And unlike the real Manhattan Project, which got plenty of funding because of its obvious military applications, nutrition has been chronically underfunded. The food groups are probably completely wrong at this point. The pyramid that tells us to eat low-fat and ridiculous amounts of grains and carbohydrates was probably more a product of Kellogg's' lobbying than actual science. And now we have an obesity explosion. Getting nutrition right isn't quite low-hanging fruit. But there are reasons to think that the right people haven't been incented to look at it hard enough.

Another search for secrets leads to biotech. Stem cell research and cancer research are the two really big areas there. There are lots of people working in each of those respective fields. But despite all the activity, there is surprisingly little overlap between the two. Stem cell research is very controversial and politicized; the anti-people are generally anti-science and have political agendas. The pro-people are equally and oppositely vehement in insisting that stem cell research is unqualifiedly wonderful. The biggest problem with injecting stem cells into people is that they start to divide and multiply. You get something that looks a lot like cancer. Neither side of the stem cell debate wants to make too much of this. But that's odd. Maybe there is a subset of cancer cells that behave like stem cells, and research at this intersection would be promising. A few people have been taking that approach. The overarching point is that structure and politics have thus far precluded investigation. So it may be a good place to look for secrets.

Cleantech is interesting. Very few cleantech companies or investments have worked well. The sociological truth about all the cleantech investments is that cleantech was fashionable. People are concerned about the environment. Investors and entrepreneurs are people. So investors and entrepreneurs got involved in cleantech to make an environmental statement. There's a sense in which some key part of these decisions was decoupled or confused with the underlying merits of the business in question. But obfuscation was necessary. You can't just say you are doing *x* to be fashionable. Saying you're doing something because it's cool is decidedly uncool. Cool people don't talk about being cool.

So what would you do if you recognized that all the cleantech stuff was driven by unstated desire to be fashionable? One option would be to swear off cleantech entirely. But could you profit from the insight? Could you start a cleantech company that embraced the dynamic and focused on making a fashion statement? The answer is yes. You could start Tesla, which is exactly what Elon Musk did.

At this point Tesla is probably the most successful cleantech company in the U.S. It builds very high-end electric-powered sports cars. There are different ways to frame the decision to cater to the luxury market. Elon's take is that you needed rich people to underwrite the research and development required to make cheaper electric cars for the mid-market. But what's key is that he took the sociological truth as a starting point instead of ignoring it. In 2005, Tesla seemed crazy. Better solar panels seemed to be the way to go. Seven years later, Tesla has built a fantastic brand. Solyndra has not. As we've talked about before, you build a monopoly business if you can start with a brand a build a tech company up from under it.

What is taboo or off-limits can often shed light on macroeconomic secrets. The U.S. trade deficit is an example. At its current rate of around 4% of GDP, it's probably quite unsustainable. But people aren't very comfortable talking about that. The existence of a trade deficit in the first place is awkward for many people. If you believe in globalization, you would expect to see a trade surplus. Instead, money is flowing uphill, into the U.S. If this deficit is unsustainable, there are a few implications. Either imports will have to fall or exports must go up. Increasing exports seems more plausible. Where does the U.S. have the biggest comparative advantage in exports? Probably in agriculture. Looking into agricultural technology is counterintuitive for tech investors, since agriculture is often about as far removed from technology as possible. But that's a good sign. It turns out that there is some very promising agritech development underway. Agritech may turn out to be a valuable secret that one might miss by not thinking about how people are talking (or not talking) about the economy.

Alternative governance is another example. The basic debate in the U.S. is big government versus small government—i.e. whether the government should do more with more or less with less. But both positions seem increasingly stale. No one talks about the alternatives: do less with more or do more with less.

Granted, the do less with more alternatives is ignored for good reason. It makes no sense. But the alternative where the government can do more with less is very promising. Doing more with less is, of course, the very definition of technology. The underexploration of this quadrant evinces an ideological blind spot. Pro-government people don't like criticizing the government; we should just solve any problems with more government. Anti-government people hate to talk about fixing government; we should just focus on getting rid of it. Even though applying technology to government could benefit everyone and be something of an optimal outcome, very few people want to talk about it.

The basic challenge is to find things that are hard but doable. You want to find a frontier. But don't simply accept others' definitions of the frontier. Existing priorities and ways of thinking need not be your own.

Think things through and go find some secrets. There are many of them out there. Just remember that they are concealed not just by nature, but also by the people all around you.

VII. What To Do With Secrets

What should you do when you find a secret? The easy answer is patent it, if you can. But what to do beyond that?

A. To Tell Or Not To Tell

The basic choice is whether or not to tell other people about your secret. If you don't tell anyone, you'll keep the secret safe. But no one will work with you. When you die, your secret will die with you.

Alternatively, you could tell your secret to everybody. You may be able to convince some people that it's actually true and build a team. But then the secret is out. More people may try to compete with you.

What kind of secret you have may influence your decision to share or hide. If it's an intellectual secret, there's probably little downside to just sharing it widely. The same goes for natural secrets, though perhaps to a lesser extent. But secrets about people are entirely different. Sharing them can be quite costly. At one point Faust tells Wagner:

The few who knew what might be learned,

Foolish enough to put their whole heart on show,

And reveal their feelings to the crowd below,

Mankind has always crucified and burned.

Human and political secrets tend to be quite dangerous. Julian Assange would probably agree.

B. Secrets and Startups

The challenge in the startup context is to figure out exactly who and how many people you should share your secret with. A lot of this is timing. The right time to bring people in is rarely at the very beginning, all at once. But it's not never, either. The timing question is a complicated one, but some intermediate answer is likely the best. Much depends on what you think the rest of the ecosystem looks like. If you think that you have a big secret but lots of other people are about to discover it, it's worth being risky. You have to move as fast as possible and tell whomever you need to.

This is what PayPal did in the summer of 1999. After some failed business models involving beaming money via palm pilots, they realized that linking money and e-mail together would be powerful. This seemed like a really big secret. But it also didn't seem very hard. Surely, other people were going to figure out the same thing in short order. So the PayPal team had scramble and share the secret liberally. This is never without its risks. People you talk to may end up competing with you instead of joining you. In June of '99, a candidate for a management position shared a secret during his interview with Peter Thiel that he should not have

shared: he wanted Peter's job. It was a dangerous political secret. Peter, it turned out, liked his job and wanted to keep it. The interviewee was not hired. A few weeks later he tried to launch a competitor.

The fraud problems that PayPal ran into were also a big secret. Fraud was endemic in finance and banking, but no one ever talked about it. Banks don't like to come out and say, "We have hundreds of millions of dollars stolen from us every year and we have no idea how to stop it." So they don't say it. Instead they build in loss budgets and reserves and just try to keep things quiet.

C. Small and Vocal vs. Big and Quiet

In Silicon Valley today, there's a sense in which most secrets are kind of small. You can get an advantage, but it will be copied very quickly. To succeed you need to achieve hypergrowth, and soon. The idea is to reveal fast and get your exponential curve growing such that no one can catch you. But it's certainly worth asking whether there are other companies for which the dynamic would be a lot slower. There may be lots of cases where there's no need to give up the secret right away. It may make sense to keep profiles low, use trade secrets and unique expertise, and build up a great business over the course of several years.

It's hard to know how many companies are doing this. The many companies that are doing fairly incremental things and trying to grow super fast are very visible. People working on bigger ideas on a more protracted timeline will be more on the stealth side. They aren't releasing new PR announcements every day. The bigger the secret and the likelier it is that you alone have it, the more time you have to execute. There may be far more people going after hard secrets than we think.

D. Perception vs. Reality

Understanding secrets isn't just important in starting companies. It's also important if you are looking to go work for an existing company. We know that, per the power law secret, companies are not evenly distributed. The distribution tends to be bimodal; there are some really great ones, and then there are a lot of ones that don't really work at all. But understanding this isn't enough. There is a big difference between understanding the power law secret in theory and being able to apply it in practice.

Say you're looking for a startup job. You know it's important to land on the right part of the distribution curve. You want to go to one of the great companies. This may seem easy, since there's a general sense in the media and tech community about what the best companies are. People perceive that startup A is much better than B, which in turn is much better than C. So you'd shoot for A and line up an interview at B as a backup, right?

Maybe. That works in a world where the power law is true, but there are no secrets. *But in a world with many secrets, the best companies may be hidden.* The power law is the same. But it's harder to navigate because people may have misidentified the best companies. Your task in a secretive world is to identify the hidden companies with the *potential* to be the best. What potentially great company are people overlooking? Do not take the perceived distribution of best to worst as a given. That is the fundamentalist view. The market, the media, the tech blogs—they all know better than you. You can't find secret startups that might be great. What do you know?

This doesn't mean you should seek out and join obscure companies. Esoteric truths are not what we're after. But you should try to identify *important* truths. And very often those are hidden.

We end with Tolkien:

The Road goes ever on and on

Out from the door where it began.

Now far ahead the Road has gone,

Let others follow it who can!

Let them a journey new begin,

But I at last with weary feet

Will turn towards the lighted inn,

My evening-rest and sleep to meet.

You go on a long journey. The designated road never really ends. But later on in the LOTR, there's an alternative version:

Still round the corner there may wait

A new road or a secret gate,

And though we pass them by today,

Tomorrow we may come this way

And take the hidden paths that run

Towards the Moon or to the Sun.

The road isn't infinite. It's possible that, just around the corner, there's a secret gate leading to a secret road. Take the hidden paths.

Peter Thiel's CS183: Startup - Class 12 Notes Essay

Here is an essay version of class notes from Class 10 of CS183: Startup. Errors and omissions are mine.

Reid Hoffman, co-founder of LinkedIn and Partner at Greylock Partners, joined this class as a guest speaker. Credit for good stuff goes to him and Peter. I have tried to be accurate. But note that this is not a transcript of the conversation.

Class 12 Notes Essay—War and Peace

I. War Without

For better or for worse, we are all very well acquainted with war. The U.S. has been fighting the War on Terror for over a decade. We've had less literal wars on cancer, poverty and drugs.

But most of us don't spend much time thinking about why war happens. When is it justified? When is it not? It's important to get a handle on these questions in various contexts because the answers often map over to the startup context as well. The underlying question is a constant: how can we tilt away from destructive activity and towards things that are beneficial and productive?

A. Theater

It often starts as theater. People threaten each other. Governments point missiles at each other. Nations become obsessed with copying one another. We end up with things like the space race. There was underlying geopolitical tension when Fischer faced off with Spassky in the Match of the Century in 1972. Then there was the Miracle on Ice where the U.S. hockey team defeated the Soviets in 1980. These were thrilling and intense events. But they were theater. Theater never seems all that dangerous at first. It seems cool. In a sense, the entire Cold War was essentially theater—instead of fighting and battles, there was just an incredible state of tension, rivalry, and competition.

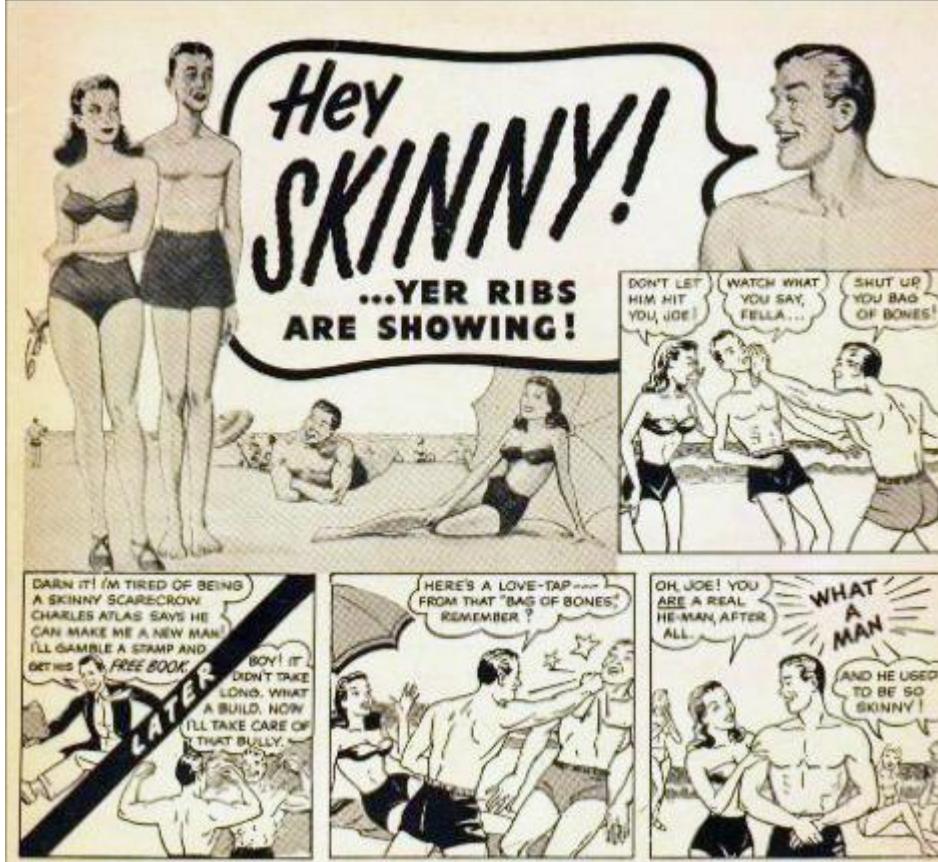
There are ways in which competition and war are powerfully motivational. The space race was incredibly intense. People worked extremely hard because they were competing against Russians on other side. Things get so intense that it becomes quite awkward when the rivalry ends. The space race ended in 1975 with the Apollo-Soyuz Test Project, where the U.S. and Soviet Union ran a joint space flight. No one was quite sure how it would play out. Was everyone just going to become friends all of a sudden?

So war can be a very powerful, motivational force. It pushes people to try and improve themselves. It's like wimpy kid who orders a Charles Atlas strength-training book, bulks up, and pummels the bully that's been tormenting him.

B. Psychology

But the Charles Atlas example illustrates more than just the motivational aspect of war. When people are myopically focused on fighting, they lose sight of everything else. They begin to look very much like their enemy. The skinny kid bulks up. He becomes the bully, which of course is exactly what he had always hated.

A working theory is thus that you must choose your enemies well, since you'll soon become just like them.



This is the psychological counterpoint to the economic discussion we had in classes three and four. In world of perfect competition, no one makes any profit. Economic profits are competed away. But the economic version is just a snapshot. It illustrates the problem, but doesn't explain *why* people still want to compete. The Kissinger line on this was that "the battles are so fierce because the stakes are so small." People in fierce battles are fighting over scraps. But why? To understand the static snapshot, you have to look to the underlying psychology and development. It unfolds like this: conflict breaks out. People become obsessed with the people they're fighting. As things escalate, the fighters become more and more alike. In many cases it moves beyond motivational theater and leads to all out destruction. The losers lose everything. And even the winners can lose big. It happens all the time. So we have to ask: how often is all this justified? Does it ever make sense? Can you avoid it altogether?

C. Philosophies of Conflict

There are two competing paradigms one might use to think about conflict. The first is the Karl Marx version. Conflict exists because people disagree about things. The greater the differences, the greater the conflict. The bourgeoisie fights the proletariat because they have completely different ideas and goals. This is the internal perspective on fighting; there is an absolute, categorical difference between you and your enemy. This internal narrative is always a useful propaganda tool. Good vs. evil is powerfully motivational.

The other version is Shakespeare. This could be called the external perspective on fighting; from the outside, all combatants sort of look alike. It's clear why they're fighting each other. Consider the opening line from Romeo and Juliet:

Two households, both alike in dignity,

Two houses. Alike. Yet they seem to hate each other. In a very dynamic process, they grow ever more similar as they fight. They lose sight of why they're fighting to begin with. Consider Hamlet:

Exposing what is mortal and unsure

To all that fortune, death, and danger dare,

Even for an eggshell. Rightly to be great

Is not to stir without great argument,

But greatly to find quarrel in a straw

When honor's at the stake.

To be truly great, you have to be willing to fight for reasons as thin as an eggshell. Anyone can fight for things that matter. True heroes fight for what *doesn't* matter. Hamlet doesn't quite achieve greatness; he's too focused on the external narrative of how meaningless everything is. He never can bring himself to fight.

II. War Within

A. What's Past is Prologue

So which perspective is right in the tech world? How much is Marx? How much is Shakespeare?

In the great majority of cases, it's straight Shakespeare. People become obsessed with their competitors. Companies converge on similarity. They grind each other down through increased competition. And everyone loses sight of the bigger picture.

Look at the computer industry in the 1970s. It was dominated by IBM. But there were a bunch of other players, like NCR, Control Data, and Honeywell. Note that those are longer common names in computer technology. At the time, all these companies were trying to build mini computers that were competitive with IBM's. Each offering was slightly different. But conceptually they were quite similar. As a result of their myopia, these companies completely missed the microcomputer. IBM managed to develop the microprocessor and eclipsed all its competitors in value.

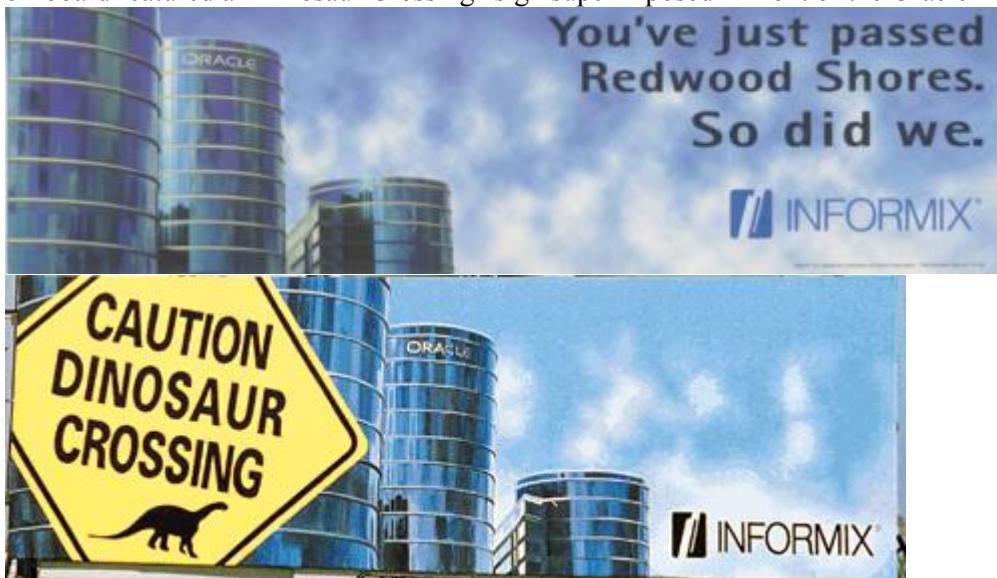
The crazy '90s version of this was the fierce battle for the online pet store market. It was Pets.com vs. PetStore.com vs. Petopia.com vs. about 100 others. The internal narrative focused on an absolute fight to dominate online pet supplies. How could the enemy be defeated? Who could afford the best Super Bowl ads? And so on. The players totally lost sight of the external question of whether the online pet supply market was really the right space to be in. The same was true of Kozmo, Webvan, and Urban Fetch. All that mattered was winning. External questions that actually mattered—Is this war even worth fighting?—were ignored.

You can find this pattern everywhere. A particularly comic example is Oracle vs. Siebel. Oracle was a big database software company. Siebel was started by a top salesman from Oracle—so there was a dangerously

imitative and competitive dynamic from the outset. Siebel tried to copy Oracle almost exactly, right down to similar office design. It sort of started as theater. But, as is often the case, it escalated. Things that start with theater quite often end pretty badly.

At one point, Oracle hatched an interesting plan of attack. Siebel had no billboard space in front of its office. So Oracle rented a huge truck and parked it in front of Siebel HQ. They put up all sorts of ads on it that made fun of Siebel in attempt lure Siebel employees away. But then Oracle acquired Siebel in 2005. Presumably they got rid of the truck at that point.

The ad wars aren't just throwaway anecdotes. They tell us a lot about how companies were thinking about themselves and the future. In the '90s, a company called Informix started a Billboard war with Oracle. It put up a sign near Oracle HQ off the 101 that said: "You just passed Redwood Shores. So did we." Another billboard featured a "Dinosaur Crossing" sign superimposed in front of the Oracle Towers.



Oracle shot back. It created a prominent ad campaign that used snails to show the TPC benchmark results of Informix's products. Of course, ads weren't anything new. But what was strange was that they weren't really aimed at customers; they were aimed at each other, and each other's employees. It was all intended to be motivational theater. Ellison's theory was that one must always have an enemy. That enemy, of course, should not be big enough to have a chance at beating you. But it should be big enough to motivate the people who fail to realize that. The formula was theater + motivation = productivity. The flaw was that creating fake enemies for motivation often leads to real enemies that bring destruction. Informix self-destructed in 1997.

B. The End of This Day's Business Ere It Come

The Shakespearean model holds true today. Consider the Square card reader. Square was the first company to do mobile handset credit card processing right. It did the software piece and the hardware piece, and built a brand with the iconic white square device.

Then there was a proliferation of copycat readers. PayPal launched one. They shaped it like a triangle. They basically copied the idea of a simple geometric-shaped reader. But they tried to one-up Square; 3 sides, after all, was simpler than 4.

Before PayPal's PR people could celebrate their victory, Intuit came out with a competing card reader. It was shaped like a cylinder. Then Kudos came out with its version, which it shaped like a semicircle. Maybe someone will release a trapezoid version soon. Maybe then they'll run out of shapes.



How will this all end? Do you really want to get involved in making a new card reader at this point? One gets a distinct sense that the companies focused on copycat readers are in a great deal of trouble. Much better to be the original card reader and stay focused on original problems, or an original company in another space entirely.

C. Even the Big Guys Do It

It's not just startups that engage in imitative competition. The Microsoft-Google rivalry, while not completely destructive, has a lot of this Shakespearean dynamic behind it. In a way, they were destined to war with each other from day one because they are so alike. Both companies were started by nerds. The top people are obsessed with being the smartest. Bill Gates had an obsession with IQ testing. Larry and Sergey sort of took that to the next level. But Microsoft and Google also started off very differently. Originally they did very different things and had very different products. Microsoft had Office, Explorer, and the Windows operating system. Google had its search engine. What was there to fight about?

Fast-forward 12 years. It's Microsoft's Bing vs. Google, and Google's Chrome vs. Internet Explorer. Microsoft Office now has Google Docs to contend with. Microsoft and Google are now direct competitors across a number of very key products. We can surmise why: each company focused on the internal narrative in which they simply had to take on the other because they couldn't afford to cede any ground. Microsoft absolutely had to do search. Google simply had to do Docs and Chrome. But is that right? Or did they just

fall prey to the imitative dynamic and become obsessed with each other?



The irony is that Apple just came along and overtook them all. Today Apple has a market cap of \$531 billion. Google and Microsoft combined are worth \$456 billion. But just 3 years ago, Microsoft and Google were each individually bigger than Apple. It was an incredible shift. In 2007, it was Microsoft vs. Google. But fighting is costly. And those who avoid it can often swoop in and capitalize on the peace.

D. If You Can't Beat Them, Merge

PayPal had similar experience. Confinity released the PayPal product in late 1999. Its early competitor was Elon Musk's X.com. The parallelism between Confinity/PayPal and X.com in late '99 was uncanny. They were headquartered 4 blocks apart on University Avenue in Palo Alto. X.com launched a feature-for-feature matching product, right down to the identical cash bonus and referral structure. December 1999 and January 2000 were incredibly competitive, motivational months. People at PayPal were putting in 90-100 hours per week. Granted, it wasn't clear that what they were working on actually made sense. But the focus wasn't on objective productivity or usefulness; the focus was on beating X.com. During one of the daily updates on how to win the war, one of the engineers presented a schematic of an actual bomb that he had designed. That plan was quickly axed and the proposal attributed to extreme sleep deprivation.

Each company's top brass was scared. In February 2000 we met on neutral ground at a restaurant on University Avenue located equidistant from their respective offices. We agreed to a 50-50 merger in early March. We combined, raised a bunch of money before the crash, and had years to build the business.

E. If You Can, Run Away. If Not, Fight and Win.

If you do have to fight a war, you must use overwhelming force and end it quickly. If you take seriously the idea that you must choose your enemies well since fighting them will make you like them, you want wars to be short. Let that process go on too long and you'll lose yourself in it. So your strategy must be shock and awe—*real* shock and awe, not the fake kind that gets you a 10-year war. You have to win very quickly. But since very often it's not possible to ensure a quick victory, your primary job is to figure out ways not to have war happen at all.

Let's return to the 2 x 2 matrix from class five. On one axis you have athletes and nerds. Athletes are zero sum competitors, and nerds are non zero sum collaborators. On the other axis you have war/competition and peace/monopoly capitalism. We said that a company should optimize for peace and have some combination of both nerds and athletes—nerds to build the business, and athletes to fight (and win) if and when you're unfortunate enough to have to compete.

The nerds-athletes hybrid model allows you to handle external competition. But it also creates an internal problem; if you have to have at least a few very competitive people on your team, how do you avoid conflicts *within* the company? Very often these conflicts are the most disastrous. Most companies are killed by internal infighting, even though it may not seem like it. It's like an autoimmune disease. The proximate cause may be something external. But the ultimate cause of destruction is internal.

When we overlay the noting of intracompany fighting on the Marx vs. Shakespeare framework, we get two theories as to why colleagues fight. Marx would say people fight internally because they wildly disagree about what the company should do, or what direction it should take. The Shakespeare version is precisely the opposite; people fight because they both want to do the same thing.

The Shakespearean dynamic is almost invariably correct. The standard version is that two or more people each want the same role in a company. People who want very different things don't fight in well-functioning companies; they just go and own those different things. It's people who want to do the same things who actually have something to fight about.

At PayPal, the center of conflicts tended to be the product team. David Sacks wanted the product to be a single seamless whole. That was a good approach, but a less good byproduct was that it was a recipe for product people overlapping with everyone else in the company. Product couldn't do anything without infringing on someone else's turf. A big part of the CEO job is stopping these kind conflicts from happening in first place. You must keep prospective combatants apart. The best way to do this is by making clear definitions and precise roles. Startups, of course, are necessarily flexible and dynamic. Roles change. You can't just avoid internal war by siloing people away like you can in big companies. In that sense, startups are more dangerous.

PayPal solved this problem by completely redrawing the org chart every three months. By repositioning people as appropriate, conflicts could be avoided before they ever really started. The craziest specific policy that was enacted was that people were evaluated on just one single criterion. Each person had just one thing that he or she was supposed to do. And every person's thing was different from everyone else's. This wasn't very popular, at least initially. People were more ambitious. They wanted to do three or four things. But instead they got to do one thing only. It proved to be a very good way to focus people on getting stuff done instead of focusing on one another. Focusing on your enemy is almost always the wrong thing to do.

III. Conversation with Reid Hoffman

Peter Thiel: How can people fall into the trap of fighting wars? Is there a strategy to avoid fighting altogether?

Reid Hoffman: To not get mired down is key. You must think very deliberately about your strategy and competition to do that. One element that I'd add to your comments is the very basic idea that part of reason we have competition is that people want resources. People need things, and very often they're willing to fight to get them. Competition for resources can be natural, and not just a psychological construction.

Peter Thiel: The counter to that is that something like prestige, for example, isn't any kind of scarce natural resource.

Reid Hoffman: But people value it a lot—so much so that they fight over it. As CEO, people routinely come and pitch you for new titles, with no substantive change in their responsibilities.

Peter Thiel: That's true—there was a relentless escalating title phenomenon at PayPal. We had lots of VPs. Then lots of Senior VPs. In hindsight it probably wasn't that stable. But we were acquired before anything really blew up.

Reid Hoffman: Back to your question—it's so important for early stage companies to avoid competition because you can't isolate it to one front. Competition affects you on the customer front, hiring front, and financing and BD fronts—on all of them. When you're 1 of n , your job becomes much harder, and it's hard enough already. A great founding strategy is thus contrarian and right. That ensures that, at least for an important initial time, no one is coming after you. Eventually people *will* come after you, if you're onto something good. That might explain the Microsoft-Google competition you highlighted as sort of bizarre. Each has its great revenue model—its gold mine. At the start they were quite distinct. These respective gold mines allowed them to finance attacks on the other guy's gold mine. If you can disrupt the other's mine, you can take it over in the long run.

Peter Thiel: The criticism of that justification for competition is that the long run never really arrives as planned. Microsoft is losing a billion dollars per year on Bing.

Reid Hoffman: It's possible that this playbook doesn't work as well for tech companies as it used to. Search is an ongoing battle. But there are other successes. Look at Xbox. Microsoft's decision to compete worked there. When Sony stumbled a bit, Xbox became a really viable franchise. Microsoft's strategy is to own all of the valuable software on desks and other rooms, not just isolated products. So, with the Xbox, it's made some headway in the living room. It's complicated. But what drives the competition is the sense that there's a lot of gold over there. So if you're a startup and you find some gold, you can count on competition from all directions, including previously unlikely places.

Some competition is easier and that gives you more leeway. Banks, for instance, are very bad innovators, which turned out great for PayPal. In more difficult competitive scenarios, you really have to have an edge to win. Difficult competition with no edge makes for a war of attrition. People may get sucked in to ruthlessly competitive situations by the allure of the pot of gold to be had. It's like rushing the Cornucopia in the Hunger Games instead of running away into the forest. Sometimes people justify this by rationalizing that "if

we don't fight it here, we'd just have to fight somewhere else." Sometimes that's a good argument, sometimes it's not. But usually there's a pot of gold that's being chased.

Peter Thiel: But people are very bad at assessing probability. It's irrational to spend all your gold trying to get the other guys' gold if you probably won't succeed. I maintain that there is a crazy psychological aspect to it. It isn't just rational calculation because tremendous effort is spent on things that, probabilistically, aren't lucrative at all.

Reid Hoffman: It's true that mimesis is a lot easier than invention. Most people are pretty bad at inventing new things. iPhones with a blue cover. Triangular card readers instead of square ones. That's not invention. If you can actually invent good things, that's the viable strategy. But most people can't. So we see a lot of competition.

A side note on invention and innovation: when you have an idea for a startup, consult your network. Ask people what they think. Don't look for flattery. If most people get it right away and call you a genius, you're probably screwed; it likely means your idea is obvious and won't work. What you're looking for is a genuinely thoughtful response. Fully two thirds of people in my network thought LinkedIn was a stupid idea. These are very smart people. They understood that there is zero value in a social network until you have a million users on it. But they didn't know the secret plans that led us to believe we could pull it off. And getting to the first million users took us about 460 days. Now we grow at over 2 users per second.

Peter Thiel: The very strategic focus on something no one was thinking of—business social networking—is one of the most impressive things about the LinkedIn. 460 days is moderately fast but not insanely fast. PayPal got to a million users in 4 to 5 months... [pause, laughter]. But while you always want to grow fast, you want *to be able to grow* more slowly. If you focus and target a non-competitive space, 460 days is plenty of time. You get more time to establish a great lead and then execute and maintain it.

Reid Hoffman: It's obviously important to target an area that no one's playing in. The interesting question is what do you do once you're on everyone's radar. You have to have some sort of competitive edge. Is it speed? Momentum? Network effects? It could be a lot of things. But you must think through it, because people will come after you as soon as you uncover value. You've found your gold mine; now you must defend it. It's always easier for people to come take your gold than to find gold anew. You have to have a plan to dominate your market in the long run.

Social was big well before LinkedIn. It was something of a dogpile of competition for LinkedIn in the early days. But the other companies who were focused on business social wanted to sell to enterprises. Enterprises, they thought, would build the networks. LinkedIn, of course, wanted to focus on individuals and stayed true to the vision. It's scarily easy to lose sight of the big vision. People are always tracking down the CEO and telling doomsday stories about how we're all dead if we don't change something to address competitor x. If you start to focus on doing everything, you're just going to war without any clear vision, and you'll fail.

There's also a version of this that applies to individuals. People look for individual gold—things like good career moves, prestige, status. Having multiple people competing for those things, is, as you said, a recipe for internal challenges.

At LinkedIn we addressed this by structuring precise roles, much like PayPal did. But unlike PayPal, we did this for teams, not individuals. Teams get mandates. A team is responsible for growth, mobile, or certain

parts of platform. Sometimes the mandates overlap. Occasional conflict seems inevitable. But it's kept manageable. The benefit is each team functions like a startup itself. There are clear goals and metrics. Every so often, you have to fix things and refactor things. That's ok. Groups drift and different prioritizations can conflict. It's worth it. In fact it's probably a very bad sign if you don't have to frequently refactor how stuff works to make it effective.

What's key, as PayPal discovered, is that you give your people a path to success. Maybe they won't fully agree with it. They don't have to. There just has to be some reasonable buy-in. That is the best way to avoid internal conflict. The other route—just going full throttle on the us-vs-them dynamic—is very motivational too. But it has all the costs of war that theater that may not stay theater forever. It may defocus your long-term efforts, and, as Peter described, you get engineers designing bombs.

Peter Thiel: External war is a very effective way to forge internal peace. In early March of 2000, PayPal had \$15M in bank. It was on track to run out of money in 6 weeks. CFO Roelef Botha thought that this was quite alarming. He—quite sanely—shared his deep concern with everybody. But the engineering team wasn't interested. The only thing that mattered was beating X.com. It didn't matter if you went broke in the process.

Reid Hoffman: So you can't just go into full war mode. You have to strategize as to how to avoid competition and external competition. That will take you far. But competition is inevitable. Even if you build good things with network effects, people aren't always smart. They'll try to compete with you anyways, even if that's a bad idea. So you have to strategize about how to deal with the forces of competition, too, both internally within the company and externally with other companies.

In the tech space, the landscape changes based on what technologies become available. Oracle and Siebel dominated enterprise software because they dominated the sales relationships. And then along comes the cloud. Now you have entirely new kind of products available for the same kind of functions. We've seen really massive companies being built in the last decade. SalesForce is the archetypical one that's succeeded and gone public.

Peter Thiel: And SalesForce was funded by Larry Ellison to compete with Siebel on CRM. Then it succeeded and grew and now, of course, Oracle hates SalesForce.

Reid Hoffman: This plays into how the inevitability of competition. In tech, if you're not continually thinking about catching the next curve, one of the next curves will get you. Yahoo owned the front end of the Internet in 2000. It had the perfect strategy. But it did not adapt; it failed at social and other trends; that didn't go so perfectly. Just over a decade later, having missed some very key tech curves, it's in a very different position.

Peter Thiel: Last class we talked about secrets. You want to have a secret plan. Probably not enough companies have a plan, let alone a secret plan. This gets complicated, because people's secrets are secretive and so we might not know about them. But, with that caveat, what companies do you think have the best secret plans?

Reid Hoffman: Mozilla seems to have good plans. They understand the move from desktop to mobile. Different from classic companies, they're not trying to build a closed franchise, but rather trying to keep open ecosystem for innovation. Quora has interesting plans about connecting people to knowledge. Dropbox is interesting, and probably has big plans that take it far beyond just being a hard drive in the cloud. The

bottom line is if you don't have a very distinctive, big idea—a prospective gold mine—you have nothing. Not all ideas work. But you have to have one.

Peter Thiel: A good intermediate lesson in chess is that even a bad plan is better than no plan at all. Having no plan is chaotic. And yet people default to no plan. When I taught at the law school last year, I'd ask law students what they wanted to do with their life. Most had no idea. Few wanted to become law firm partners. Even fewer thought that they would actually become partner if they tried. Most were going to go work at law firms for a few years and "figure it out."

That's basically chaos. You should either like what you're doing, believe it's a direct plan to something else, or believe it's an indirect plan to something else. Just adding a resume lines every two years thinking it will buy you options is bad. If you're climbing a hill, you should take a step back and look at the hill every once in awhile. If you just keep marching and never evaluating, you may get old and finally realize that it was a really low hill.

One reason people may default to not thinking about the future is that they're uncomfortable being different. It is unfashionable to plan things out and to believe that you have an edge you can use to make things happen.

Reid Hoffman: People also underestimate how much of an edge you need. It really should be a compounding competitive edge. If your technology is a little better or you execute a little better, you're screwed. Marginal improvements are rarely decisive. You should plan to be 10x better.

Peter Thiel: I recall being pitched on some anti-spam technology. It was billed as being better than all other anti-spam tech out there, which is good since there are probably 100 companies in that space. The problem was that it took a half hour to explain why it was allegedly better. It wasn't as concise as: "We are 10x better/cheaper/faster/more effective." Any improvement was probably quite marginal. Customers won't give you a half hour to convince them your spam software is better. A half hour pitch on anti-spam is just more spam.

Shifting gears a bit: is there way to stay head of curve before it eats you?

Reid Hoffman: We ask prospective hires at Greylock how they would invest \$100k between iOS and android, if they had to make bets about the future. The only wrong answer is 50-50. That is the only answer that's basically equivalent to "I don't know." Think through it and take a position. You'll develop insight. That insight—or more specifically the ability to acquire it—is what will keep you ahead of the curve.

Another huge thing to emphasize is the importance of your network. Get to know smart people. Talk to them. Stay current on what's happening. People see things that other people don't. If you try to analyze it all yourself, you miss things. Talk with people about what's going on. Theoretically, startups should be distributed evenly throughout all countries and all states. They're not. Silicon Valley is the heart of it all. Why? The network. People are talking to teach other.

Peter Thiel: It's a trade-off. You can't just go and tell everybody your secret plan. You have to guard your information, and other people guard theirs. At the same time, you need to talk and be somewhat open to get all the benefits you mentioned from the network. It can be a very fine line.

Question: Do people overestimate competition? What about the argument that you shouldn't do x because Google could just do it?

Reid Hoffman: When I evaluate startups, that "Google can do it" isn't really a valid criticism unless the startup is a search engine.

Google has tons of smart people. They can, in all likelihood, do exactly what you're doing. But so what? That doesn't mean you can't do it. Google probably isn't interested. They are focused on just a few things, really. Ask yourself: what's more likely: nuclear war, or this company focused on competing with me as one of its top 3 objectives? If the answer is nuclear war, then that particular potential competitor is irrelevant.

Peter Thiel: Everyone develops an internal story about how their product is different. From outside perspective they often look pretty similar. So how can you tell whether what you're doing is the same or whether it's importantly different?

Reid Hoffman: You can't systematize this. It's a problem that requires human intelligence and judgment. You consider the important factors. You make a bet. Sometimes you're right, sometimes you're wrong. If you think your strategy will always be right, you've got it wrong.

Question from audience: Can you give some examples on how one can successfully get away from competition?

Peter Thiel: PayPal had a feature for feature competition with X.com that lasted intense 8 weeks. The best way to stop or avoid the war was to merge. The hard part was deescalating things post-merger. It was hard to immediately shift to being great friends afterwards. There is always a way in which things get remembered much more positively when everything works out in long run. Conversely, rivalries tend to get exaggerated ex-post when things don't go so well.

Reid Hoffman: There was some pretty intense infighting at PayPal. One of the things that Peter has said is key: either don't fight, or fight and win. But you should be skeptical that you will definitely win if you end up fighting.

PayPal's biggest traction was with eBay. But eBay had an internal product called BillPoint. PayPal, as the sort of 3rd party disrupter, was at a serious disadvantage there. eBay was the only gold mine that existed. We had to win. It was time to leverage the athletes' competitive talent. One decisive move in the war was focusing on e-mail. The real platform for auctions wasn't the eBay website, as most people assumed. It was e-mail. People would receive emails when they won auctions. eBay knew this but didn't understand its importance. PayPal, on the other hand, got it and optimized accordingly. Very often PayPal would notify people that they won the auction before eBay did! People would then use PayPal to pay, which of course was the goal.

It was much harder to compete against the Buy It Now feature. There, eBay had greater success roping people into paying with BillPoint. It was harder to get in front of people if they were just buying and paying for something right away on the website.

The takeaway advice is to always keep questioning the battle. Never get complacent. When you're in battle, only the paranoid survive.

Question from audience: What do you think about the competition between Silicon Valley and New York? Reid, Mayor Bloomberg has argued that New York will become the dominant tech scene because the best people want to live there. He quoted you as saying “I don’t like all that culture stuff” and suggested that that view is “narrow.”

Reid Hoffman: I’m friends with Mayor Bloomberg, but I’ll return fire.

So Bloomberg is trying to make a tech-friendly New York that will compete and beat Silicon Valley. That’s great. We wish him the best of luck. More great technology innovation hubs within the US are great for us.

But, to compete, they’ll certainly need the luck. Silicon Valley has an enormous network effect. Tech is what we do. This is the game we play. If there’s anywhere in the world to go for tech, it’s here. People move here just to be a part of the tech scene.

The New York tech world has to compete for its technical people. Many of the best tech people go to hedge funds or move to Silicon Valley.

One of ways to understand effect of competition is companies that emerge here are competitive globally because crucible is so high. The best people go into tech here. And they have a single-minded focus about their work.

All the culture of NYC doesn’t matter positively or negatively, relative to succeeding at the technology innovation game. So it’s a wonderful place to live. Fine. Mayor Bloomberg, you’re very welcome to the people who want to live in New York for its culture and theater and operas. Personally, I love to visit. The people that we want are the ones who want to win this game first and foremost, and who don’t care terribly much about missing Broadway shows.

Question from audience: Isn’t culture important in a sense, though? Silicon Valley engineers aren’t social. So how can they make social games?

Reid Hoffman: It’s not that *all* great companies come from Silicon Valley. I was simply saying that it is extremely difficult to unseat Silicon Valley as the best place for tech companies. But certainly not every great tech company needs to be a product of the Valley. Indeed, that’s impossible. Groupon, for instance, couldn’t be created here. They need 3,000 salespeople. That is not the game that Silicon Valley specializes in. It worked very well in Chicago. So Silicon Valley learns from Groupon here; as did I. There are certainly other playbooks.

But the Silicon Valley playbook is a great, and perhaps the best. If you have to make a portfolio bet on technology or a portfolio bet on sales processes, you should take the tech portfolio every time. New York is the 2nd most interesting place for consumer internet. It’s just very unlikely to displace Silicon Valley as #1.

Peter Thiel: My take is that New York is a pretty distant second. There are some very cool companies coming out of New York. But one anti-New York perspective is that the media industry plays much bigger role there than it does here. That induces a lot of competition because people focus on each other, and not on creating things. New York is structurally more competitive in all sorts of ways. People literally live on top of each other. They’re trained to fight and enjoy fighting. Some of this is motivational. Maybe some of it is

good for ideation. But it directs people into fighting the wrong battles. We will continue to see the more original, great companies coming out of Silicon Valley.

Reid, final question. What advice would you give young entrepreneurs?

Reid Hoffman: You can learn a lot from companies that succeed. Companies have benefited greatly from Facebook's Open Graph. Ignoring that instead of learning it, for instance, could be catastrophic for you, depending on what you're trying to do. But of course learning everything before you do anything is bad too.

The network is key. This is a large part of how you learn new things. Connect with smart people. Talk. What have you seen in last couple of months? What do you know? It's not a go-and-read-everything strategy. You'd die before you could pull that off. Just exchange ideas with the smart people in your network. Not constantly, of course—you need to do work too—but in a focused way. Take what you learn and update your strategy if it's warranted. And then keep executing on it.

Peter Thiel's CS183: Startup - Class 13 Notes Essay

Here is an essay version of class notes from Class 13 of CS183: Startup. Errors and omissions are mine. Credit for good stuff is Peter's entirely.

Class 13 Notes Essay— You Are Not A Lottery Ticket

I. The Question of Luck

A. Nature of the Problem

The biggest philosophical question underlying startups is how much luck is involved when they succeed. As important as the luck vs. skill question is, however, it's very hard to get a good handle on. Statistical tools are meaningless if you have a sample size of one. It would be great if you could run experiments. Start Facebook 1,000 times under identical conditions. If it works 1,000 out of 1,000 times, you'd conclude it was skill. If it worked just 1 time, you'd conclude it was just luck. But obviously these experiments are impossible.

The first cut at the luck vs. skill question is thus almost just a bias that one can have. Some people gravitate toward explaining things as lucky. Others are inclined to find a greater degree of skill. It depends on which narrative you buy. The internal narrative is that talented people got together, worked hard, and made things work. The external narrative chalks things up to right place, right time. You can change your mind about all this, but it's tough to have a really principled, well-reasoned view on way or the other.

But people do tend to be extremely biased towards the luck side of things. Skill probably plays a much greater role than people typically think. We'll talk about through some anti-luck thoughts and arguments shortly. But the first thing to understand is that there's no straightforward way to make an airtight argument.

B. Anti-Luck

The weak argument against the luck hypothesis involves presenting scattered data points as evidence of repeatability. Several people have successfully started multiple companies that became worth more than a billion dollars. Steve Jobs did Next Computer, Pixar, and arguably both the original Apple Computer as well as the modern Apple. Jack Dorsey founded Twitter and Square. Elon Musk did PayPal, Tesla, SpaceX, and SolarCity. The counter-narrative is that these examples are just examples of one big success; the apparently distinct successes are all just linked together. But it seems very odd to argue that Jobs, Dorsey, or Musk just got lucky.

C. A Sign of The Times

It's worth noting how much perspectives on this have changed over time. The famous Thomas Jefferson's line is: "I'm a great believer in luck, and I find the harder I work the more I have of it." From the 18th century all the way through the 1950s or '60s, luck was perceived as something to be mastered, dominated, and controlled. It was not this weird external force that couldn't be understood.

Today's default view is more Malcolm Gladwell than Thomas Jefferson; success, we are told, "seems to stem as much from context as from personal attributes." You can't control your destiny. Things have to combine just right. It's all kind of an accident.

D. Applied to Startups

The theme that luck plays a big role is also dominant in the startup community. Paul Graham has attributed a great deal of startups' success to luck. When we come to startups, theme is that luck plays a big role. Paul Graham. Robert Cringely wrote a book called Accidental Empires. The point is not to pick on these people—they're obviously very competent and quite successful. The point is that they represent the dominant ethos as to how to think about startups.

This is further illustrated by considering what would happen if a successful entrepreneur publicly stated that his success was fully attributable to skill. That entrepreneur would be perceived as ridiculous, arrogant, and wrong. The level of proof that he offered or the soundness of his arguments wouldn't matter. When we know that someone successful is skilled, we tend to discount that or not to talk about it. There's always a large role for luck. No one is allowed to show how he actually controlled everything.

It's worth noting that the existence of this class—being willing to teach this class—is a structural reason in support of the anti-luck bias. A class on startups would be worthless if it simply relayed a bunch of stories about people who won lotteries. There is something very odd about a guide to playing slot machines. To the extent it's all a matter of luck, there is no point in learning very much. But it's not all a matter of luck. And the part of it that is can be channeled and mastered. Note that this is class 13. We are not going to be like the people who build buildings without a 13th floor and superstitiously jump from class 12 to 14. Luck isn't something to circumvent or be afraid of. So we have class 13. We'll dominate luck.

E. Past vs. Future

One useful division in thinking about luck is to separate it out into luck involving the past and luck involving the future. The past piece basically asks, "How did I get here?" If you're successful, you were probably born in the right country. You won the geographic/genetic/inheritance lotteries. There's the classic debate of whether you had to work hard and these facilitated your success or whether you're just fooling yourself if you don't think these things actually drove it.

It's probably more fruitful to focus on the future side of things. Let others fight about the past. The more interesting questions are: Is the future a future that's going to be dominated by luck, or not? Is the future determinate or indeterminate?

II. Determinate vs. Indeterminate Futures

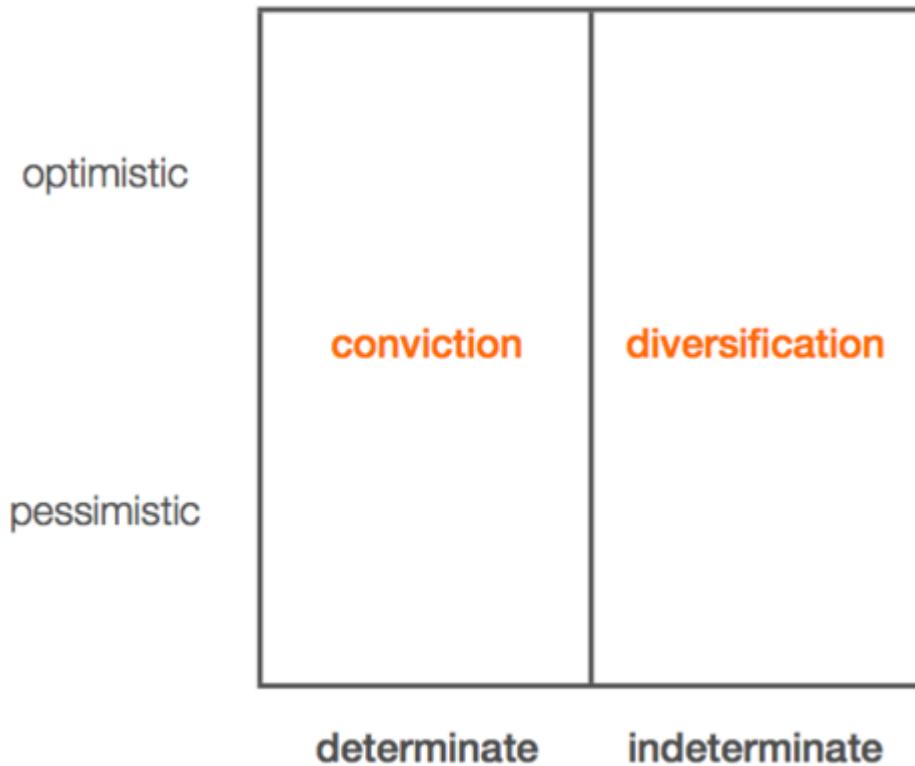
As a society, we now gravitate towards explaining things by chance and luck rather than skill and calculation. This dynamic is necessarily abstract and very hard to ground empirically. All we can do here is point out that we seem to have shifted to the extreme luck side of the spectrum and suggest that it might make sense to dial back the pendulum the other way.

Naturally, we can use a 2 x 2 matrix to help us think about the future. On the vertical axis you have optimism and pessimism. On the horizontal axis you have determinate and indeterminate. The determinate perspective

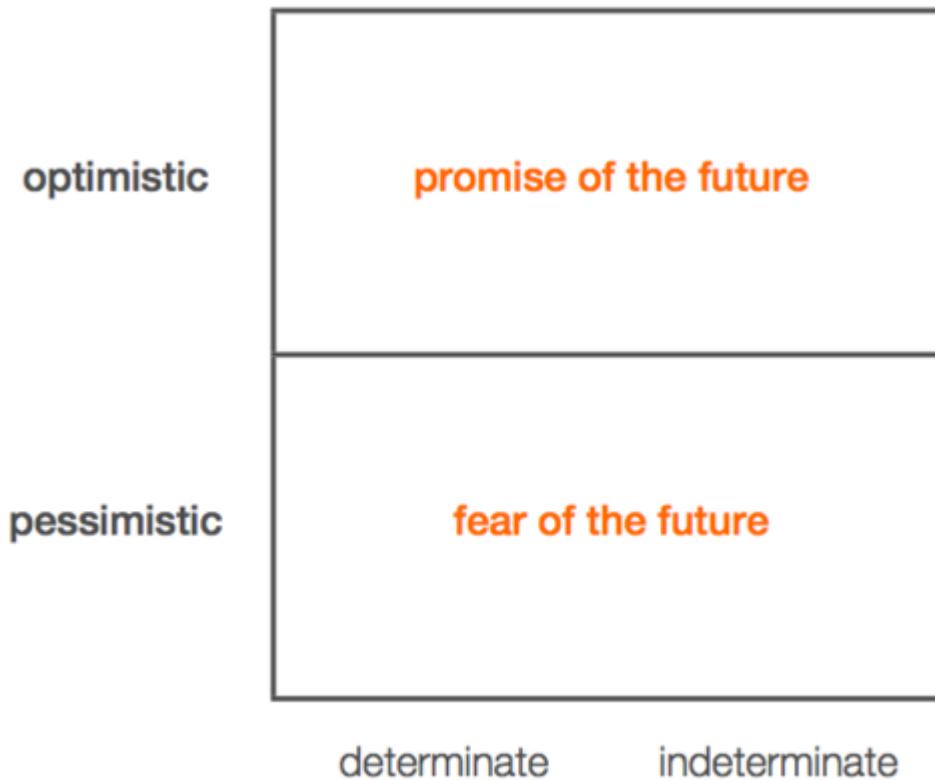
is that things are knowable and you can control them. The indeterminate perspective is that things are unknowable and uncontrollable. There are just too many chance events.

What would you do if you were to land in a given quadrant? If you believe that the future is fundamentally indeterminate, you would stress diversification. This is true whether you're optimistic or pessimistic. And indeed, chasing optionality seems to be what most everybody does. People go to junior high and then high school. They do all sorts of activities and join lots of clubs along the way. They basically spend 10 years building a diverse resume. They are preparing for a completely unknowable future. Whatever winds up happening, the diversely prepared can find something in their resume to build on.

Contrast this with the determinate version. If the future is determinate, it makes much more sense to have firm convictions. You won't join tons of different clubs or do every single activity. There is just one thing—the best thing—that you should do. This is decidedly *not* how people build up their resumes these days.



Overlay this diversification/conviction dynamic over the optimism/pessimism question and you get further refinement. Whether you look forward to the future or are afraid of it ends up making a big difference.



A. Determinate Optimism

Up until the 1950s and '60s, the prevailing belief about the future was one of determinate optimism. There had always been a relatively well-defined way in which people thought the future would be much better than the present. You could go west and get 640 acres of land. Specific projects to improve society were undertaken. There was a general orientation toward working to make a better future.

B. Indeterminate Optimism

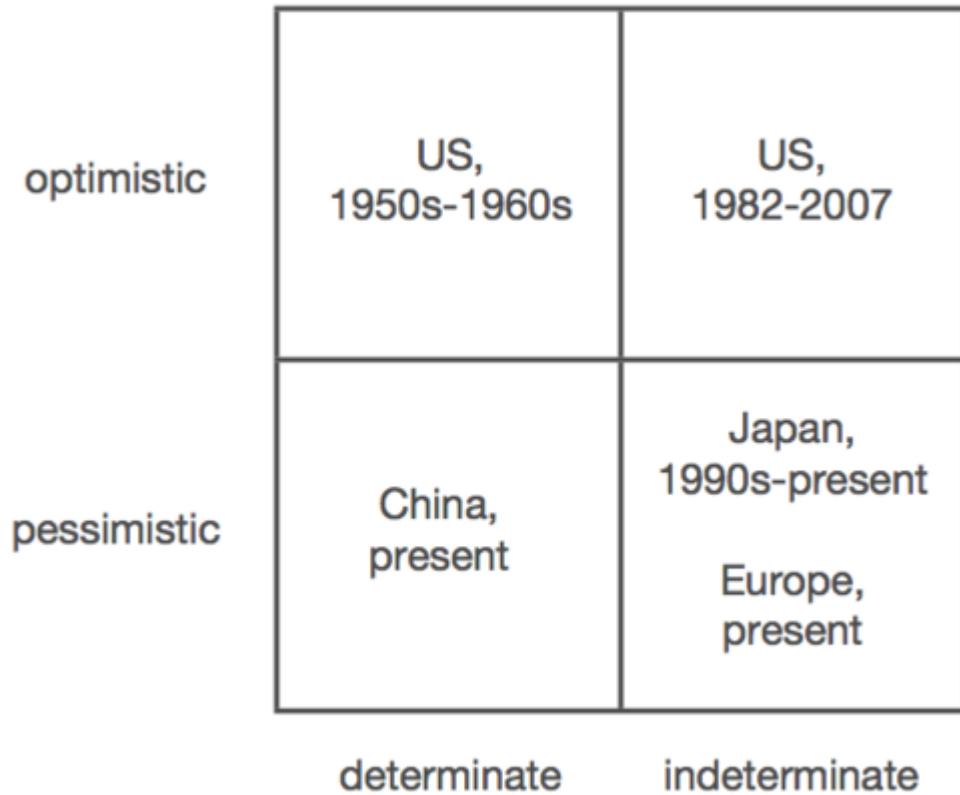
But the U.S. has shifted away from this quadrant. The outlook, at least up through 2007, was still optimistic. But ever since about 1982 it has also been much more indeterminate. The idea was that the future would get better, but not in ways that you could know. Unlike the determinate future of the past, which contained many secrets, today the future seems to contain very few. There is much more room for mystery. God, Mother Nature, and Market are unknowable and inscrutable. But the universe is still fundamentally benevolent. It is thus best to just figure out incremental things to do and wait for progress to come.

C. When Things Are More Pessimistic

Or you could think future won't be very good at all. In a strange way, China falls squarely in the determinate pessimistic quadrant. It is the opposite of the U.S.'s optimistic indeterminacy. The China view is that there is indeed a calculus as to what to do to improve things for society. Things are determinate. But when you go through that calculus, there's no cause for celebration. China will get old before it gets rich. It is forever destined to be a poor version of the U.S. It can and will copy things. But there's not enough time to catch up, even if it executes perfectly. This explains why you end up with all these things that seem draconian from a more optimistic perspective; *e.g.* the one child policy, massive environmental pollution, and thousands of

people dying in coal mines each year. The fundamental view is pessimistic, but in a very determinate, calculated way.

And then there is the pessimistic indeterminate quadrant. This is probably the worst of all worlds; the future isn't that great and you have no idea what to do. Examples would be Japan from the 1990s to the present, or Europe today.



There is a very widespread view that the U.S. is shifting from the upper right quadrant of optimistic indeterminacy to the lower left of pessimistic determinacy. The argument, in other words, is that we're drifting towards China. This is a future where everything goes to pot, and not just in a figurative sense.

D. Financial Overlay

We can put a financial overlay on this to illustrate things better. We'll start with some definitions. Investment is putting money in specific things you believe in, like a stock or a company. You're looking for high returns. Savings, by contrast, is when you hold onto your money so that you're able to spend it in the future. You typically get very low returns.

To the extent you're optimistic about the future, you'll have a low savings rate. There is simply less need to save. The future will be better, and things will take care of themselves. But if you're pessimistic, your savings rate will be higher. Since you expect that the future will be worse than the present, you want to have cash saved up for when that day comes.

E. Savings

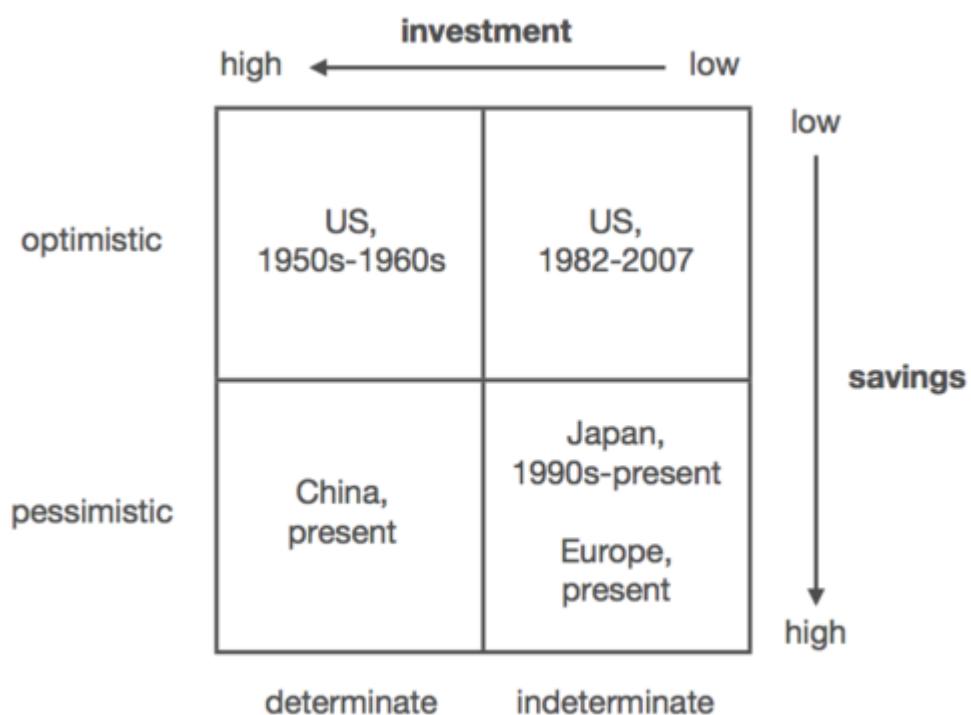
The U.S. has a savings rate that is not that much higher than zero. It's at something like 4%. If you include the government savings rate of -10% of GDP, the savings rate is at -6%. In a funny way, that would imply that the U.S. is wildly optimistic about the future (though one suspects that government spending isn't so well thought out as this). But no matter how you slice it, the U.S. has a pretty low savings rate.

China has a savings rate of close to 40%. People like to criticize this because it seems odd to them that poor people are saving money while rich people in the U.S. are spending money. This creates trade deficits, and so China, the argument goes, should start consuming more and saving less. This criticism overlooks one of the key drivers of China's high savings rate; it's very hard to consume your capital when you are fundamentally pessimistic about the future and believe that you'll get old before you become rich.

F. Investment

In a determinate world, there are lots of things that people can do. There are thus many things to invest in. You get a high investment rate. In an indeterminate world, the investment rate is much lower. It's not clear where people should put their money, so they don't invest. We have a very low rate of investment in U.S. Corporations are the main places where investment happens. But instead of investing, companies today are generating huge cash flows—about \$1 trillion annually at this point. They are hoarding cash because they have no idea what else to do with it. Almost by definition, you wouldn't have free cash flows if you knew where or how to invest. The consumer side isn't all that different. People have no idea. So we end up with a low investment rate, low savings rate, and take an optimistic view of a fundamentally indeterminate future.

The pessimistic quadrants are always kind of stable. This is especially true of the indeterminate pessimistic quadrant; if you think that things are going to pot and you believe you can't control them, they probably will. You'll be stuck going nowhere for a long time. Under determinate pessimism, you'll be like China—stuck methodically copying things without any hope for a radically better future.



The big question is whether indeterminate optimism—which characterized the U.S. from 1982 to at least 2007—is or can be a stable quadrant at all. That the U.S. has a low savings rate and low investment rate is very odd indeed. If you have both low investment and low savings, one must wonder how the future is supposed to happen at all. That no one is thinking about the future is evinced by the low investment rate. So how can people be so optimistic (not saving any money) about a future that no one is working toward?

G. Calculus vs. Statistics

There are several different frameworks one could use to get a handle on the indeterminate vs. determinate question. The math version is calculus vs. statistics. In a determinate world, calculus dominates. You can calculate specific things precisely and deterministically. When you send a rocket to the moon, you have to calculate precisely where it is at all times. It's not like some iterative startup where you launch the rocket and figure things out step by step. Do you make it to the moon? To Jupiter? Do you just get lost in space? There were lots of companies in the '90s that had launch parties but no landing parties.

But the indeterminate future is somehow one in which probability and statistics are the dominant modality for making sense of the world. Bell curves and random walks define what the future is going to look like. The standard pedagogical argument is that high schools should get rid of calculus and replace it with statistics, which is really important and actually useful. There has been a powerful shift toward the idea that statistical ways of thinking are going to drive the future.

With calculus, you can calculate things far into the future. You can even calculate planetary locations years or decades from now. But there are no specifics in probability and statistics—only distributions. In these domains, all you can know about the future is that you can't know it. You cannot dominate the future; antitheories dominate instead. The Larry Summers line about the economy was something like, "I don't know what's going to happen, but anyone who says he knows what will happen doesn't know what he's talking about." Today, all prophets are false prophets. That can only be true if people take a statistical view of the future.

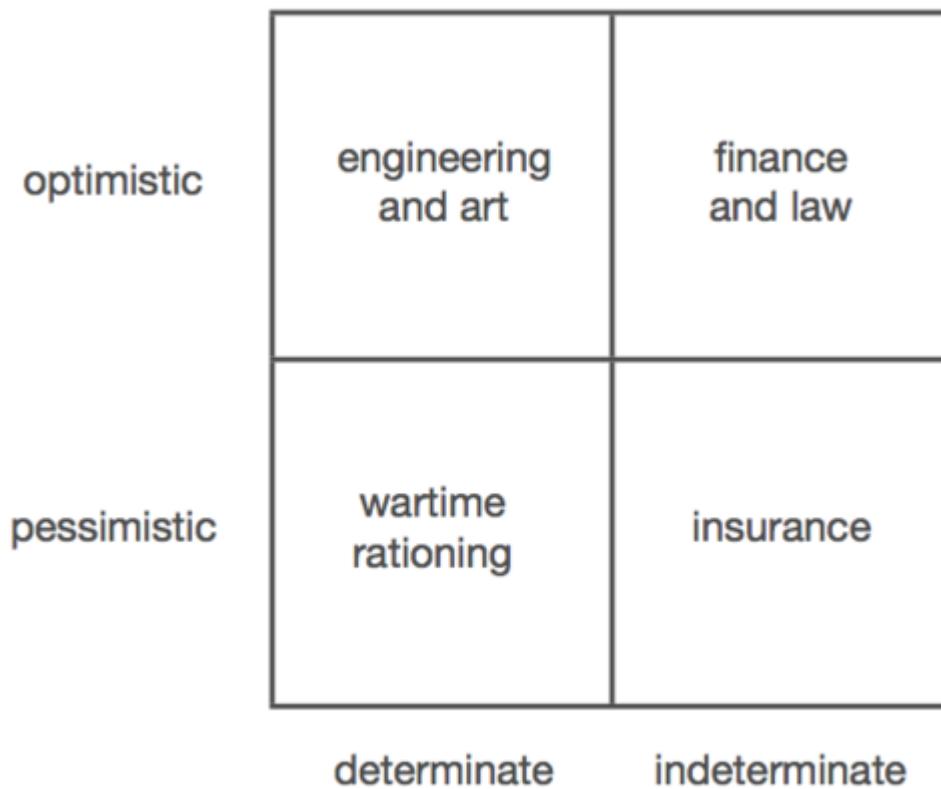
H. Substance vs. Process

Another way to look at the determinate vs. indeterminate question is through the lens of substance vs. process. What people do and what technology they build will depend on how they view the future. From an indeterminate perspective, they won't know what to build. There's nothing that specifically looks promising; it's all just a distribution. So they will think less substantively and more procedurally. You want to have the right process for navigating the distribution. This tracks the HP board debate we talked about earlier: it's Perkins' old-school substance (lets talk tech and engineering) versus Dunn's process. If everything is indeterminate, it's presumptuous to think that the board could think or know anything about the future.

How each quadrant shakes out in practice looks something like this:

- Optimistic, determinate: Engineering and art. Very specific engagements.
- Optimistic, indeterminate: Law and finance.
 - Law is a process of applying specific rules, not a certain substantive result. You assume that by following the process you end up making things better. And finance is pretty thoroughly statistical.
- Pessimistic, indeterminate: Insurance.

- You can't make money but you can protect against expected losses.
- Pessimistic, determinate: Wartime rationing.



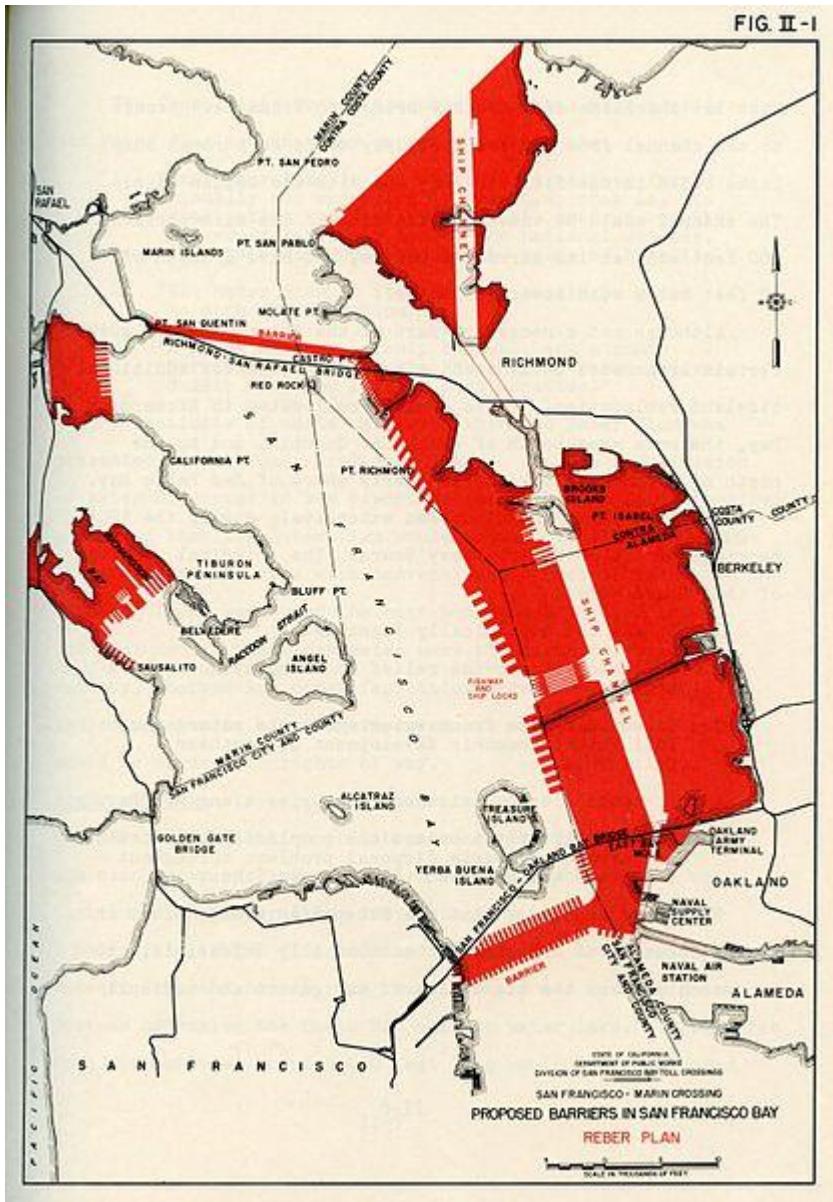
I. The End of Big Projects

The U.S. used to fall squarely in the definite optimism quadrant. There used to be all sorts of circulating ideas about large projects that would take many years to build and improve things in very dramatic, powerful ways. A 19th century example is the Transcontinental railroad. A 20th century example is Robert Moses, who in the 1920s simultaneously held twelve fairly high-level government posts. He started out as Parks Commissioner. But to build parks—at least on the grand scale that Moses had in mind—you have to bulldoze neighborhoods, build roads and highways, and do a lot of planning and construction. Moses managed to get the authority to do all these things. At his height, Moses was significantly more powerful than the mayor or governor of New York. He pretty much rebuilt all of the state of New York in 30-40 years.

From today's perspective, this is crazy. Surely Moses had too much power. Such ambitious projects, especially if architected by a single person, would probably go nowhere today. The difference was that then, unlike now, people believed in a determinate future. The future could be planned. Moses seemed smart enough and reasonably ethical. Instead of asking whether anyone should do it, people simply asked who would do it well.

All this came to an end in 1965, when Moses planned highway that would run through Greenwich Village. A sufficiently large number of people thought that the old buildings that would have to be torn down should be preserved, and protested the development. It was the last time that new highways were built in the state.

An example that is a little closer to home is the Reber plan. John Reber was a teacher and amateur theater producer in San Francisco. In the 1940s he came up with a plan to radically reconstruct the San Francisco Bay Area. The basic plan was to construct two large earth and rock dams, one between San Francisco and Oakland and the second between Marin Country and Richmond. They would drain water from the north and south sides and replace it with freshwater. Some 20,000 acres of land would be filled in. A 32-lane highway would be built. And high-rise buildings would be scattered throughout the thoroughly reconstructed city.



Less important than the actual details is the fact that this plan was not some nutty, fringe thing. There were Congressional hearings and testimony on its viability. It turned out that various things wouldn't work—the freshwater lakes would evaporate too quickly, for instance—but people were interested. Today, by contrast, the idea would be dismissed as lunacy. This is especially true if it came from someone like John Reber. What credentials does a schoolteacher have for redesigning the entire Bay Area? The John Rebers of the world have long since learned to keep their plans to themselves. Even safer is not to develop any grand plans at all.

J. Indefinite Optimism and Finance

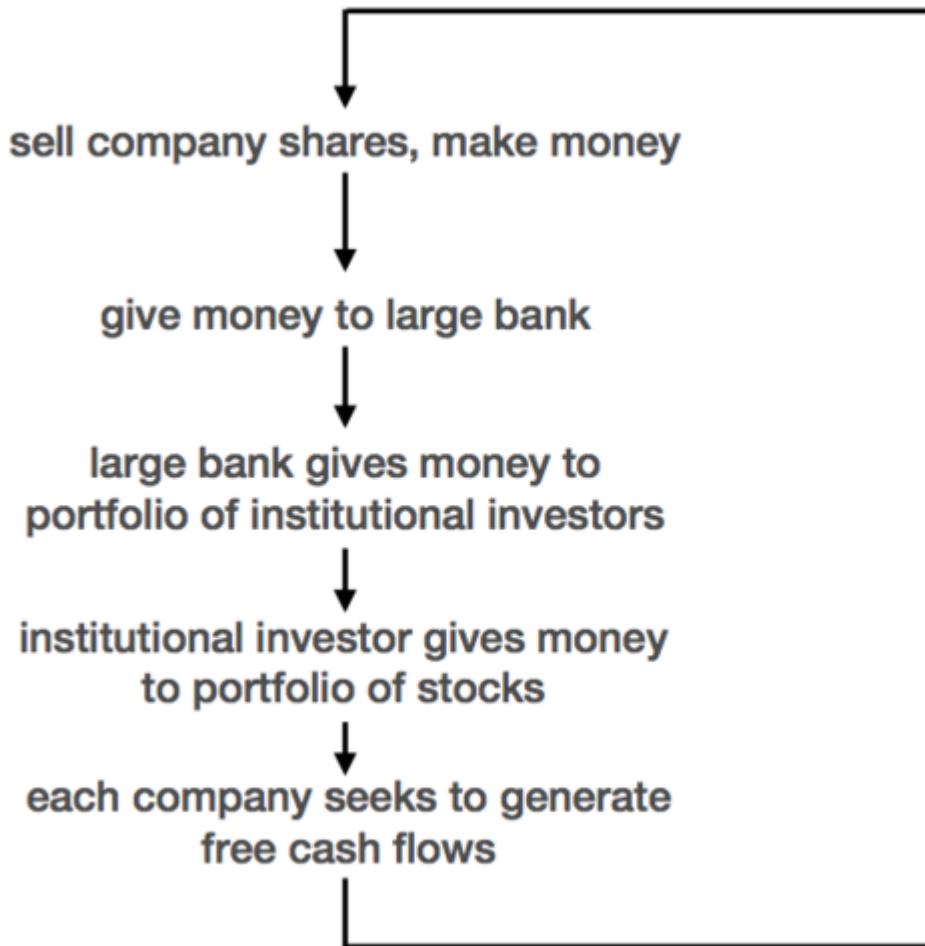
In a future of definite optimism, you get underwater cities and cities in space. In a world of indefinite optimism, you get finance. The contrast couldn't be starker. The big idea in finance is that stock market is fundamentally random. It's all Brownian motion. All you can know is that you can't know anything. It's all a matter of diversification. There are clever ways to combine various investments to get higher returns and lower risk, but you can only push out the efficient frontier a bit. You can't know anything substantive about any specific business. But it's still optimistic; finance doesn't work if you're pessimistic. You have to assume you're going to make money.



Indefinite optimism can be really strange. Think about what happens when someone in Silicon Valley builds a successful company and sells it. What do the founders do with that money? Under indefinite optimism, it unfolds like this:

- Founder doesn't know what to do with the money. Gives it to large bank.
- Bank doesn't know what to do with the money. Gives it to portfolio of institutional investors in order to diversify.
- Institutional investors don't know what to do with money. Give it to portfolio of stocks in order to diversify.
- Companies are told that they are evaluated on whether they generate money. So they try to generate free cash flows. If and when they do, the money goes back to investor on the top. And so on.

What's odd about this dynamic is that, at all stages, no one ever knows what to do with the money. It's obviously a limit case. But it reflects a very bizarre cultural phenomenon. Money plays a much more important role if the future is indefinite. There, having is always better than specific things. It's pure optionality, and that optionality encapsulates the indefinite view. In a definite future, by contrast, money is simply a means to an end.



It's worth questioning whether the circular investment dynamic actually works. Can that really be self-sustaining? Can things work out when no one is thinking of substantive things to do or adding new ideas into the system? The era of indefinite optimism is arguably coming to an end. Look at government bonds, which are essentially the purest version of money. Yields keep going down. People keep holding because they don't know what else to do. Today you can earn 1.8% on a government bond. But expected inflation is 2.1%. The expected return over the next decade is -0.3%. You get negative returns when people run out of ideas.

Indeterminacy has reoriented people's ideas about investing. Whereas before investors actually had ideas, today they focus on managing risk. Very rarely in the hedge fund world do people ask questions about what's going to happen in the future. It's less, "What should we do?" and more, "How do we manage risk? Yet again, process trumps substance. Venture capital has fallen victim to this too. Instead of being about well-formed ideas about future, the big question today is how can you get access to good deals. In theory at least, VC should have very little in common with such a statistical approach to the future.

K. Indeterminacy and Politics

If you think that the future is indeterminate, the most important people are statisticians. Pollsters become more important than politicians. There has been a massive upward trend on polling in the last 30 years. We have polls on everything. And we believe them to be authoritative—it's dangerous to try and outthink a statistically shifting bloc of voters. Unsurprisingly, then, politicians react to the polls instead of thinking about the future. This helps explain the strange mystery in 2008 of why John McCain picked Sarah Palin as a

running mate. The McCain people reviewed all the polls about Republican governors and senators. Most were very unpopular. Palin, by contrast, had an 89% approval rating in Alaska (some of which seems attributable to Alaskans' receiving an annual \$1000+ oil royalty check). Just parsing poll data, Palin was the obvious choice. That didn't work out quite as well as they expected. This has nothing to do with Palin's merits as a candidate; it just goes to show how statistical poll data, and not clear thinking, can dominate politics. Taking a principled stand on unpopular positions is not what our leaders are incented to do.

We can broaden this idea to the government itself. The size of government hasn't changed all that much in the last 40-50 years. But what the government actually does *has* changed radically. In the past, the government would get behind specific ideas and execute them. Think the space program. Today, the government doesn't do as many specific things. Mainly it just shifts money around from some people to other people. What do you do about poverty? Well, we don't know. So let's just give people money, hope it helps, and let them figure it out. If you can't actually know what to do, just spreading money around is all you've got.

L. Indeterminacy and Literature

Science fiction literature also provides a version of the shift to indeterminacy. Fifty or 60 years ago, sci-fi portrayed the future in specific, definite terms. In 1968, Arthur C. Clark described the future of information consumption in *2001: A Space Odyssey*:

"The text was updated automatically

on every hour... one could spend an

entire lifetime doing nothing but

absorbing the ever-changing flow of

information from the news satellites."

In this world, information would update automatically. It was a very definite view of the future. But interestingly enough, the future in that future was indefinite. There would be an ever-changing flow of information that you couldn't know in advance. This seems remarkably prescient.

Contrast that with William Gibson's 1984 book, *Neuromancer*:

"The sky above the port

was the color of television,

tuned to a dead channel."

Here, 14 years after Clarke's *2001*, the future is one in which you can't see anything. It's all a random probability cloud.

M. Indeterminacy and Philosophy

There's a philosophy version of this too. Marx and Hegel dominate the optimistic determinant quadrant. The future is going to be better and you can do specific things in 5-year plans. Rawls and Nozick are optimistic but indeterminate. The socialistic version is that you should have a welfare state because that's what people would want behind a veil of ignorance. The libertarian version is that no one really knows anything, so people have to be free to run about and stumble upon success. Plato and Aristotle are squarely pessimistic and definite. You can figure out the nature of things, but there's no reason to be excited about the future. Epicurus and Lucretius represent pessimistic indeterminacy. The universe is void. Things just bump into each other randomly. Sometimes they join, sometimes they fracture. You can't do much to control it. You should therefore just be stoic and adopt an attitude of equanimity. Try to enjoy life, even though it's all just going to fall apart.

	Hegel, Marx	Nozick, Rawls
optimistic		
pessimistic	Plato-Aristotle	Epicurus-Lucretius
	determinate	indeterminate

Our society is arguably being pulled in the Epicurean and Lucretian direction. This trend has emerged recently with the financial crises. Whereas before 2007, people were indeterminate but optimistic, pessimism seems to be creeping in. Whether things will fully shift that direction is hard to say. But indefinite pessimism has never been the dominant paradigm in America.

N. Indeterminacy and Death

Another place where indeterminacy dominates is actuarial tables and death. None of us knows precisely long we're going to live. But we often consult actuarial tables that chart our probability of dying in a given year. College students have about a 1 in 1000 chance of dying in a particular year. As people get older, the probabilities shift. People who are currently 100 years old have a 50% chance of dying that year. Only one person in 10,000 makes it to age 100. Only one in 10 million lives to be 110 years. We seem have a very good probabilistic handle on something that, by its very nature, we can't know in any other way.

This is why life extension gets bad wrap. People assume that probabilities dominate—so much so that trying to find a way around them is perceived as strange or even crazy. People seem to think that you should just acquiesce to the probabilities. Things were very different from 1600-1850, when people were excited about the prospect of a magic bullet and actually searched for the fountain of youth. Perspectives shifted when people stopped believing in the existence of a literal fountain. The notion that people could dramatically change things died along with that belief. So nobody tries anymore. In a luck-driven world, chance is too powerful. It enervates people. Belief in secrets is an effective truth. Belief in luck is an *ineffective* truth; it will stop you from actually doing things.

No Country for Old Men explores the view of future where everything is random and everyone dies. You can't overcome chance by understanding it. Understanding is vain.

O. Indeterminacy and Cosmology

Indeterminacy has also invaded cosmology. Consider the rise of the many-worlds interpretation of quantum mechanics, which grew popular in the 1970s. The basic idea is that anytime anything can happen, the universe splits. Each branch is a new world, where that thing does or doesn't happen. Reality is a many-branched tree where everything—every quantum outcome—actually happens. Some 55-60% of theoretical physicists believe in many-worlds today. Only about 10-15% believed it 50 years ago. There have been no experiments that have proven the theory. Probably no such experiments are possible. So why do so many physicists believe in a theory of reality that can't be proven? If you ask them, they say that it just “seems aesthetically better.” But is that true? Are infinite universes really better than just one? Or does this just reflect the shift to viewing the future as fundamentally indeterminate?

All these examples suggest and illustrate the massive shift to indeterminacy that we've experienced in the last 30-40 years.

III. Is Indeterminate Optimism Possible?

We're in a sort of limbo today. Which quadrant will we shift to? Can we go back to the indeterminate optimism that we had in the U.S. from 1982 through 2007? Or will we shift over to some other quadrant?

Indefinite futures are inherently iterative. You can't plan them out; things just unfold on top of each other. The question is thus whether an iterative process can lead, if not to the best of all possible worlds, at least to a world where there is a path of monotonic and potentially never-ending improvement. If it can, we may not get to the tallest mountain in the universe. But at least we'll always go uphill.

A. Indeterminate Optimism in the World

This is Darwin's theory of evolution. At first there are only very basic organisms. Over billions of years, a tree of life emerges. Not all possible living things actually develop. There are no supersonic flying birds with titanium wings. Things may not be entirely perfect. Humans have appendices that are apparently useless. We can identify lots of poor “design decisions.” But there is a trajectory of relentless, never-ending progress. The Darwinist metaphor plays a central role in thinking about indefinite optimism.

How well can this idea of indeterminate optimism be extended to economies? Perhaps not so well. The paradigmatic counterexample is failed cities. Look at pictures of Los Angeles. It should have been greatest

city in the world. L.A. could have been built from scratch in early 20th century. It would have been magnificent. But there were no grand plans. Instead we got incremental sprawl. The market didn't solve the problem. L.A. is still one of best cities in world. But it is nowhere near what it could have been. The L.A. experiment at least suggests there are grounds to be pessimistic about indeterminacy.



The more hardcore version of this is São Paulo. The airport is located 5 miles away from downtown. The ride takes 10-minutes via helicopter. But during rush hour it can take 3 hours by car. Traffic is unimaginably bad. The metro area has about 20 million inhabitants. There are subcities and districts with about 500k to 1M million people each. Mostly people live in high-rise slums. Living standards seem to get worse with each passing year. Mumbai and Lagos are other examples of a trajectory of more crowding and urban decline. There seems to be no reason to be optimistic about what city planning can do for these areas. This can even be stretched into anti-globalization argument; most emerging countries actually cannot catch up with the developed world because they are too messed up and can't be rebuilt.

The economics vs. environmentalism debate tracks the optimistic vs. pessimistic one. The market economy solves problems iteratively. The idea is that we shouldn't worry about the environment because we'll figure out solutions as we go along. That's classic indeterminate optimism. The environmental counter-narrative is that we're all screwed: things are too far gone, and there is more to do than we could possibly do. That's still indeterminate, but thoroughly pessimistic.

It's worth noting that something like geoengineering would fall in the definite optimistic quadrant. Maybe we could scatter iron filings throughout the ocean to induce phytoplankton absorb carbon dioxide.

Potential solutions of that nature are not even remotely in the public debate. Only radically indefinite things make for acceptable discourse.

B. Applied to Startups

In the startup context, obsession with indeterminacy leads to the following phenomena:

- Darwinistic A/B testing
- Iterative processes
- Machine learning
- No thinking about the future
- Short time horizons

This is not to say that all of these things are totally wrong. If they're wrong, they're not self-evidently wrong. But it's far from clear that they are actually right. It's certainly interesting to wonder whether, like the many-worlds theory in quantum mechanics, these elements are social byproducts of the shift to indeterminacy.

Going through each phenomenon, it's easy to poke holes that suggest that there are better ways. Darwinism takes billions of years to work reasonably well. Startups don't have that time. And even though Darwinism is optimistic in the macro sense, it's not always experienced that way by the participants. There can be lots of carnage and destruction along the way. When people mention Darwinist theory in a business context, they're probably about to do something really mean. And iteration and machine learning are often excuses for the last two phenomena—not thinking about the future and short time horizons. There are many counterexamples, of course. But definite plans tend to be underrated in today's startup culture.

IV. The Return of Design

Finance, perhaps more than anything else, encapsulates indeterminate thinking. The peak of the finance bubble in 2007 will thus be seen as the peak of indeterminate thinking.

Apple is absolute antithesis of finance. It does deliberate design on every level. There is the obvious product design piece. The corporate strategy is well defined. There are definite, multi-year plans. Things are methodically rolled out.

A. Design and Value

This class offers no investment advice. Going out and buying Apple stock may not be the best thing to do. But over the last decade people have been badly behind the curve with respect to Apple stock. The indeterminate finance world would ignore anyone who claimed to have secret plan to build new products. Steve Jobs took over at \$3 per share. In 2003, after Apple already had some good traction, the stock traded at just \$6 per share. Institutional investors systematically underweight Apple because they don't know how to think about the future. Retail people did all the buying.

On the heels of Apple has come the theme of well-designed products being really important. Airbnb, Pinterest, Dropbox, and Path all have a very anti-statistical feel. There's a sense in which there's some telepathic link between these products and what people want. That link—great design—seems to work better and faster than Darwinistic A/B testing or iteratively searching through an incredibly large search space. The return of design is a large part of the countercurrent going against the dominating ethos of indeterminacy.

B. Designing Plans

Related to this is the observation that companies with really good plans typically do not sell. If your startup gets traction, people make offers to buy it. In an indefinite world, you will take the money and sell, because money is what you want. PayPal had and executed many good ideas. But by 2002, to be quite honest, it had run out of them. There were no clear ideas on what to do next. So there was a certain logic to selling the company.

But when companies have definite plans, those plans tend to anchor decisions not to sell. There is no reason to stop when you can do so much more. The internal narrative—the secret plan—organizes people around the specific things that are going to be built in the months and years ahead.

In an indefinite world, investors will value secret plans at zero. But in a determinate world, robustness of the secret plan is one of the most important metrics. Any company with a good secret plan will always be undervalued in a world where nobody believes in secrets and nobody believes in plans. The ability to execute against long-term secret plan is thus incredibly powerful and important.

Young people today tend to be indeterminately optimistic. They iterate, one resume line at a time. They buy into a narrative of never-ending improvement, even if they have no idea what that path might look like. It's possibly that may work. We shouldn't *completely* discount the indeterminate future, since there's always a role for chance. But it's too crowded a strategy. It gives luck too much dominion over life. Something to be said for the alternative of actually having a plan.

It's important to note that you can always form a definite plan even in the most indefinite of worlds. If you do go into law or finance, for example, you should still have a plan. Most Wall Street or law firm associates stay at one place for awhile and then do a horizontal-diagonal jump to another firm that offers more money. That iterative recipe is a recipe for disaster. Much better would be a plan to stay at the same bank or firm for 10 or 15 years. Eventually there won't be anyone left who knows what's going on but you. You should plan on being partner or managing director from day one. Your plans can change, but if you don't have them, you're just floating with the current.



C. Designing Perspectives

Our society has been indeterminately optimistic for the last quarter century. But that quadrant is fraying at edges. We're falling downwards towards pessimism. Can we shift instead to definite optimism? Computer Science is our best hope. CS is about deterministic as you can get. It's incredibly odd that we view tech startups through such an indeterminate lens. But where you go from here—and what lens you use—is up to you. An alternative title for this lecture would be “Control Alt Delete.” The best edit is often a complete rewrite. And maybe it’s time to start writing lots of things from scratch.

Peter Thiel's CS183: Startup - Class 14 Notes Essay

Here is an essay version of my class notes from Class 14 of CS183: Startup. Errors and omissions are mine. Credit for good stuff is Peter's.

Class 14 Notes Essay—Seeing Green

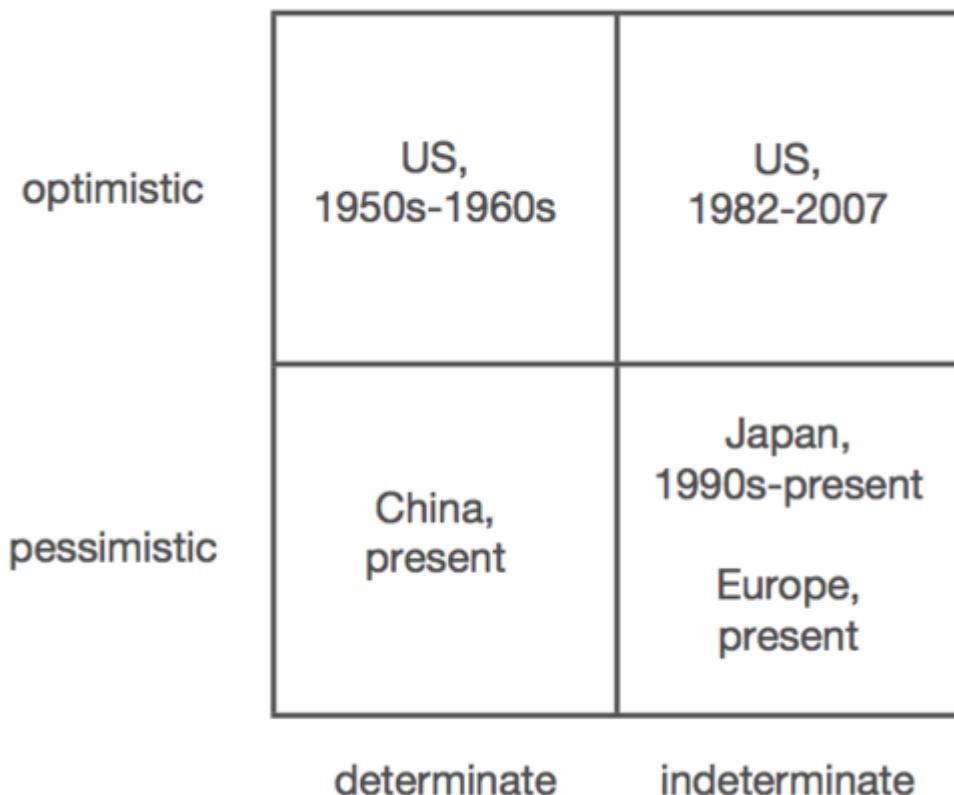
I. Thinking About Energy

Alternative energy and cleantech have attracted an enormous amount of investment capital and attention over the last decade. Almost nothing has worked as well as people expected. The cleantech experience can thus be quite instructive. Asking important questions about what went wrong and what can be done better is a very good way to review and apply many of the things we've talked about in class.

A. The Right Framework

How should one think about energy as a sector? What's an appropriate theoretical framework?

Revisiting the 2x2 matrix of determinate/indeterminate and optimistic/pessimistic futures may be useful. To recap, here are examples of those respective quadrants:



It is important to note—especially in the cleantech context, we we'll see—that planned indeterminacy doesn't really work. You can't just plan to go and get a new job every 2 years and call it a plan. That's the absence of a plan. It's equivalent to having a plan to get rich. “I intend to get rich and famous” is a vague aspiration, not a plan. Plans can't just be a portfolio you throw together. For a portfolio approach to work, it

must have a specific granularity to it. “If I do x , it will lead to 5 specific options at a certain time in the future, at which point I’ll choose the best of them” *might* be specific enough to work.

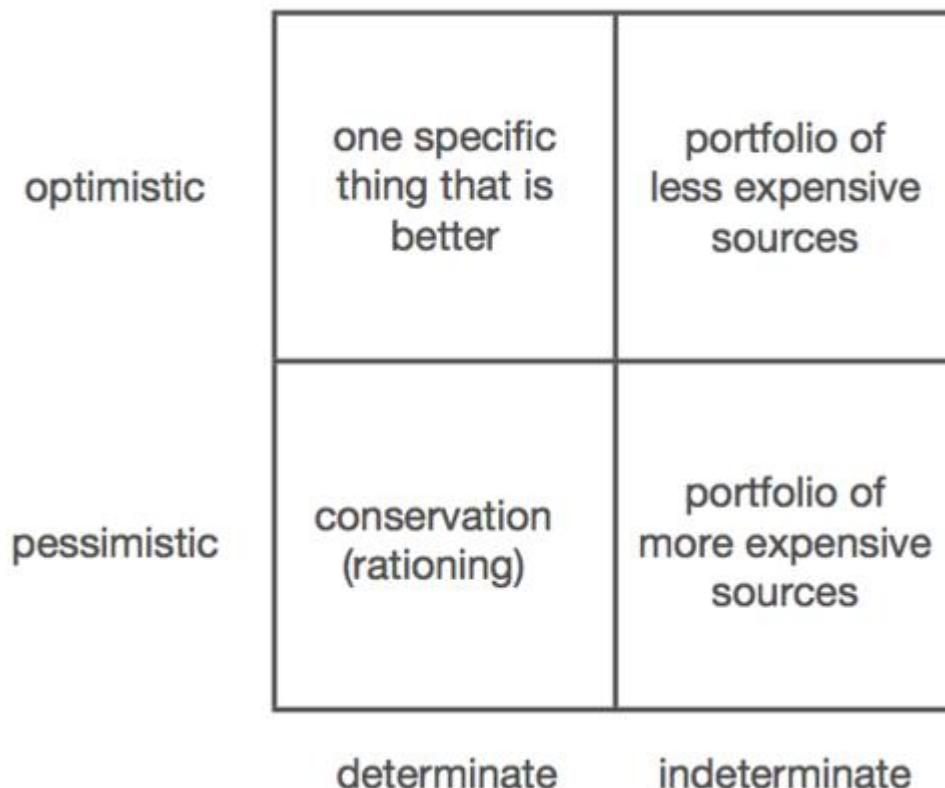
But in practice, people don’t usually target specific options that will unfold. They just figure that they will have plenty of options, and that they’ll figure everything out later. So while in theory there can be a more determinate, substantive version of indeterminate optimism, in practice statistical thinking breeds radical indeterminacy and process-based thinking. Aspirations replace plans.

And yet people are skeptical of startups with plans. The conventional wisdom is that detailed plans are worthless because everything is just going to change anyway. But having a detailed plan is key. To be sure, there are some cases where things work despite the lack of a plan. But there is an awful lot of failure there too. Winning without a plan is hitting the jackpot, and most people do not hit the jackpot. Since you want to have as much mastery over things as possible, you need to plan.

B. Applied to Energy

To think about the future of energy, we can use the same matrix. The quadrants shake out like this:

- **Determinate, optimistic:** one specific type of energy is best, and needs to be developed
- **Determinate, pessimistic:** no technology or energy source is considerably better. You have what you have. So ration and conserve it.
- **Indeterminate, optimistic:** there are better and cheaper energy sources. We just don’t know what they are. So do a whole portfolio of things.
- **Indeterminate, pessimistic:** we don’t know what the right energy sources are, but they’re likely going to be worse and expensive. Take a portfolio approach.



The determinate optimistic quadrant arguably makes the most sense. But it's also the least talked about today.

The energy market is huge. It's probably good to think about it as two separate markets. One part of the division is power generation: things like wood, coal, natural gas, nuclear, biomass, hydro, and solar—basically things that feed into the power grid. The other part is transportation, which is essentially oil and then electricity for buses, trains, etc. On the transportation side, you cannot easily tap into the power grid; you have to take the power with you as you're using it.

One preliminary question to think about in all energy contexts, then, is what the actual market is. Is it energy as a whole? Power generation? Transportation? Or are there submarkets that are worth identifying within those divisions?

C. Power Generation

In the 1950s and '60s, people were very bullish on nuclear power. It was the one thing that mostly everybody thought would be better. President Eisenhower declared in his 1953 Atoms for Peace speech that nuclear power was going to produce energy too cheap to meter. Today, by contrast, there is no specific thing that people agree will be better and cheaper. Determinate optimism in energy is dead.

That's not to say that people still aren't optimistic. Some are. They are just indeterminately so. The focus is thus on constructing a portfolio of cheap fossil fuels. One might also want to subsidize cleantech, since in some versions of indeterminate optimism, cleantech may actually be cheaper than fossil fuels when you consider fossil fuels' hidden environmental costs. But in any case the future is basically a pie chart that consists of many different things.

optimistic	1950s: nuclear	Portfolio of cheap fossil fuels
	Today: nothing	Cleantech (with subsidies)
pessimistic	Conservation	Cleantech (without subsidies)
	More expensive fossil fuels	

determinate indeterminate

The pessimistic perspectives on power generation are straightforward. The determinate view stresses conservation; things are definitely not going to improve, and fossil fuels will just become more expensive. The indeterminate version would also focus on a bunch of cleantech endeavors, since that may prove marginally better than fossil fuels.

D. Transportation Power

In the 1950s, people seriously envisioned a future filled with space jets and ever-faster and cheaper supersonic planes. Things were thoroughly optimistic and determinate. Today there is almost no activity in this quadrant. There is no broad agreement that any particular transportation technology will get better and dominate.

There is some activity in the indeterminate optimistic quadrant. This is the modern portfolio approach. People here tend to focus on portfolios of different storage technologies. Battery improvements, electric cars, telecommuting, and cheap oil all seem like viable solutions, but none seems best or particularly promising.

In the indeterminate pessimism of Japan and Europe, you get a whole range of inferior options. People ride bikes. If they drive they drive tiny cars. Or maybe they take the train and have a long commute. The sense is that none of that is going to improve.

High-speed rail is the best example of determinate pessimism. The only way you can get high-speed rail to work is to rearrange where everyone lives and make them live in much smaller places. The urban planning and redesign effort is enormous and takes a long time. Gone are the days where Robert Moses could unilaterally re-architect the state of New York. We don't do things on that scale anymore. Instead we think about the future indeterminately. All we know is that oil will probably get more expensive and the one thing we can do—high-speed rail—is almost impossible to pull off.

	1950s: space jets	Cheap oil
optimistic	Today: nothing	Portfolio of storage tech, telecommuting
	Conservation	Smaller cars, bikes, trains, range of inferior options
pessimistic	More expensive oil, high-speed rail	

determinate indeterminate

Today, the U.S. is in the upper right quadrant of optimistic indeterminism. Republicans advocate a range of hopefully better things, and we need to get rid of all regulation in order to get them. Democrats advocate a range of hopefully better things, and we need to subsidize cleantech in order to get them.

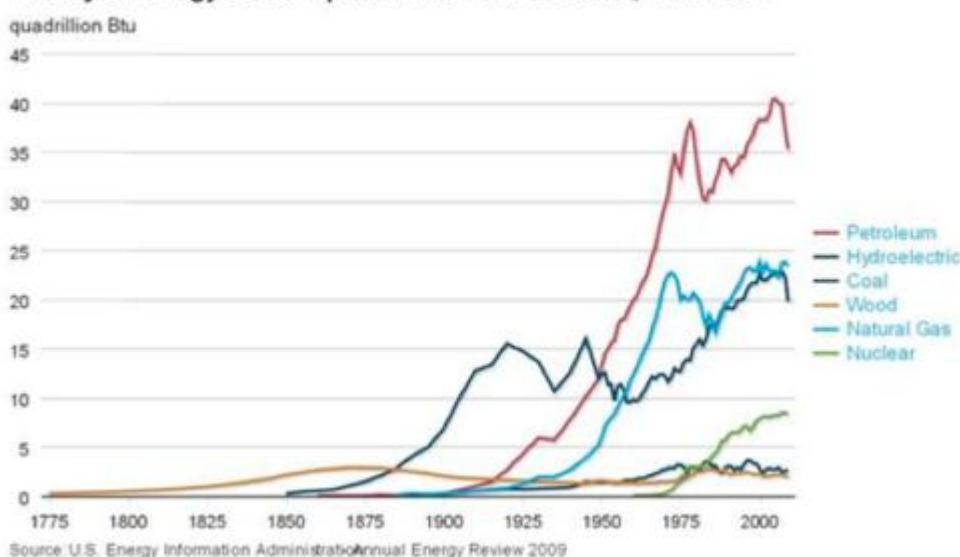
China falls in the bottom left quadrant of pessimistic determinism. The future is coal and oil. The plan is to buy up oil fields in Africa and domestically mine as much coal as possible. Something like 3,000 to 5,000 people die in coal mining accidents every year in China. They're essentially fighting small war each year to get enough coal.

II. A Brief History of Energy

A. Power Law Redux

One argument against the indeterminate view is that the history of energy consumption in the U.S. has been very determinate. A single energy source has always tended to dominate. Up until the mid-19th century, that source was wood. One reason that America had such a higher standard of living than Britain is that we had more wood. People more or less ran out of trees to cut down in Britain and so they would get cold at night. Coal started to take off around the mid-19th century and dominated up through the early 20th century. Petroleum took over as the leading energy source in the 1930s and '40s. Natural gas has now emerged and overtaken coal as number two. Nuclear comes in at a very distant third.

History of energy consumption in the United States, 1775-2009



Source: U.S. Energy Information Administration Annual Energy Review 2009

Petroleum has dominated transportation. Coal has dominated in power generation, though it's looking like natural gas might displace it. But typically a single source dominates at any given time. There is a logical reason for this. It doesn't make sense that the universe would be ordered such that many different kinds of energy sources are almost exactly equal. Solar is very different from wind, which is very different from nuclear. It would be extremely odd if pricing and effectiveness across all these varied sources turned out to be virtually identical. So there's a decent *ex ante* reason why we should expect to see one dominant source.

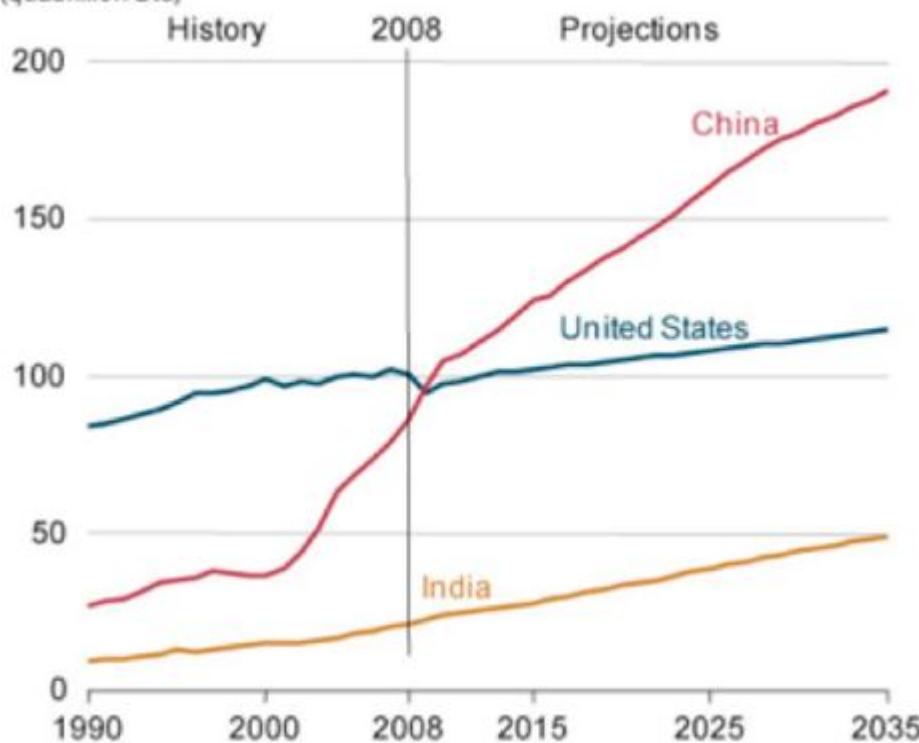
This can be framed as a power law function. Energy sources are probably not normally distributed in cost or effectiveness. There is probably one that is dramatically better than all others. Perhaps the second and third best, while nowhere near as good as the first, fill an important niche. The rest are probably much less useful. But just as the power law is overlooked in other contexts, people tend to ignore it in energy as well. We still tend to think about energy through a statistical/portfolio lens. You can't predict the future. Everything is indeterminate. It doesn't make sense to believe anything is or can be unique.

B. Challenges to Come

Another problem with indeterminacy becomes apparent when you look at worldwide energy demand trends. Energy consumption in the U.S. is rising at a modest rate. China has overtaken the U.S. in total consumption. India is still far behind but is rising relentlessly. China's GDP growth and increasing energy consumption make a 1:1 function. Each grew by about 8% annually over the last decade. This is quite startling. It means that, at least with respect to energy, there have been no improvements. It's a story of marginal efficiency gains at most. You get more only if you expend more.

Figure 13. Energy consumption in the United States, China, and India, 1990-2035

(quadrillion Btu)



Transportation trends are also odd. More cars are now sold in China than are sold in America. Worldwide oil consumption is 85 million barrels a day. The U.S. consumes about 18 million—just under 25% of total global consumption. China consumes 9 million barrels daily. But China has 4x the people that the U.S. does. If, on a per capita basis, China were to consume as much oil as Americans do, it would have to consume 72 million barrels every day. But that is roughly the entire worldwide production. There is a sense that something is going to have to give at some point. Even downsizing to so-called smart cars would only get that figure down to about 45 million barrels.

It's also worth noting that the inelasticity of oil prices is something on the order of 10:1. If oil demand increases by 10%, prices increase by 100%. Together with globalization generally, the combination of inelastic pricing and the difficulty of finding direct substitutes suggests that we're in for some serious challenges over the next few decades.

C. Resource Constraints

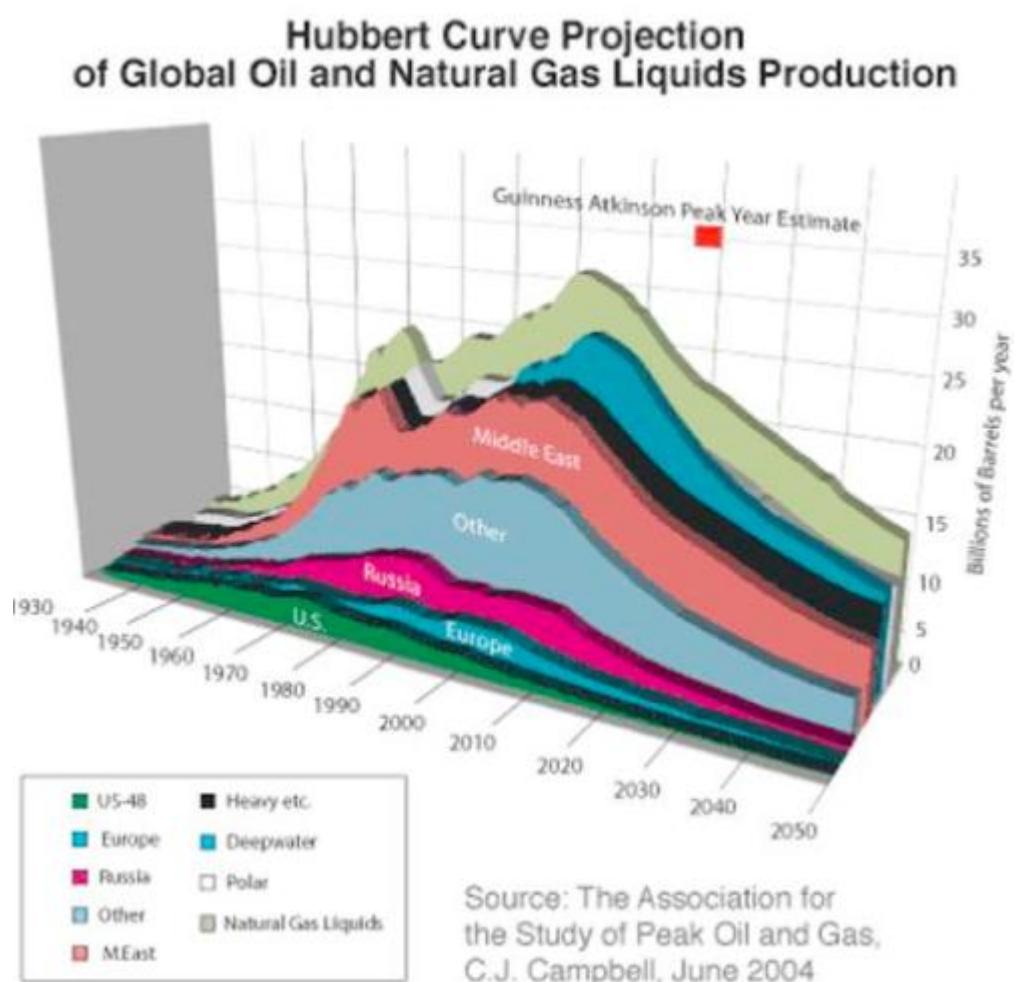
There have always been worries about running out of resources. There's the familiar Malthus stuff. In the early '70s, a big global think tank called the Club of Rome commissioned a book called Limits to Growth, which became the biggest environmental bestseller ever. The thesis was that there are all sorts of ways to run out of capacity either on a resource basis or on a population basis.



Paul R. Ehrlich wrote a book called The Population Bomb in 1968. In it he claimed that the world was far beyond its carrying capacity with 3.5 billion people. That turned out to be quite wrong. There are now twice that many people living today. Resource constraints thus seem more real than population constraints. Of the

7 billion people on earth, only one billion live in the developed world. It would take enormous energy increases to bring the other 6 billion up to standards.

One interesting take on resource constraints is peak oil theory. In 1956 a Shell oil geologist named M. King Hubbert noticed a drop in the rate at which new oil wells were being discovered. He identified a lag of 20-30 years between well discovery and tapped production. Hubbert predicted that U.S. oil production would peak in the mid 1970s and would then start to decline. No one believed him at the time because oil didn't seem to be problematic. The U.S. then was like Saudi Arabia today—a very big exporter. The Texas Railroad Commission effectively set the world oil price. Things were good. But Hubbert's prediction came true, more or less exactly. In 1970 the Railroad Commission didn't impose any quotas; supply and demand were in equilibrium. But by 1973 there were oil shocks. U.S. production was declining. The Commission was replaced by the OPEC cartel.



Then OPEC overreached. It quadrupled prices, from \$3 to \$12 per barrel. It quadrupled them again in 1989, sending oil to \$40 per barrel. Then Alaska came online. But we've started to run into problems again over the course of the last decade. On a worldwide basis, the Hubbert projection is that the world is now where the U.S. was in 1970; production has already peaked or is peaking soon.

The many crises of the last decade can be interpreted as crises about energy. People might be focusing too much on the financial aspect of the so-called financial crisis of 2007. What if we just hit Hubbert's peak? Oil goes to \$140 per barrel. The only thing to do to contain it is to destroy a lot of economic activity. That

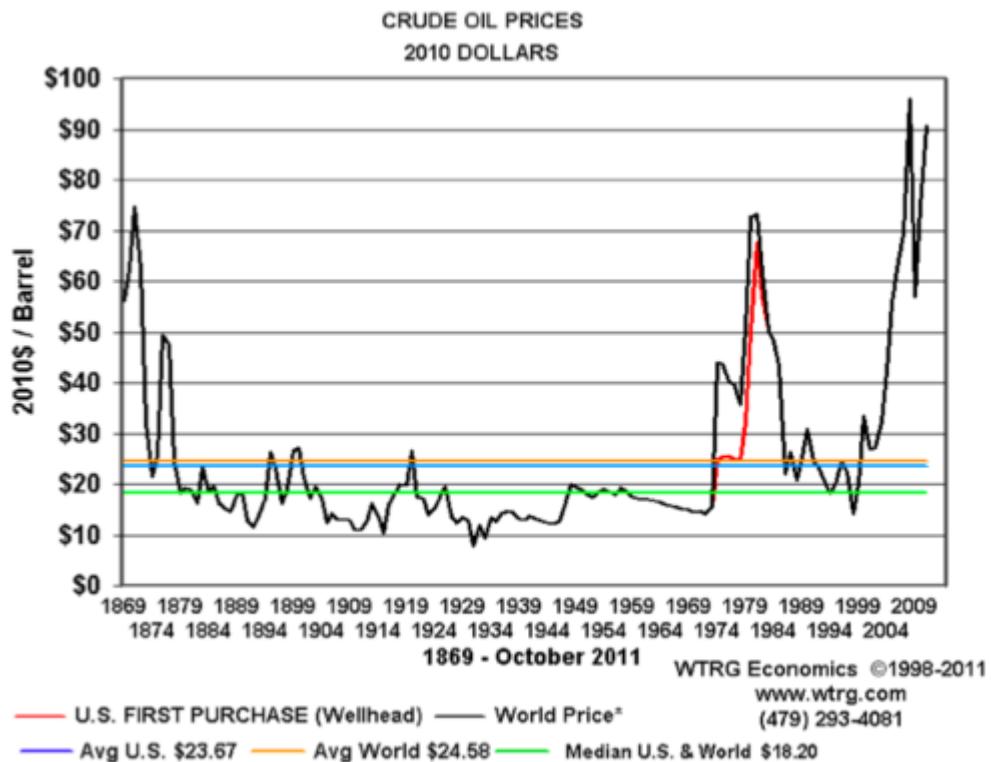
happened. Oil came down to \$32. But now, a few years later, oil is back up to \$100 per barrel. This cynical view is that it's all a game of musical chairs game. In a determinate pessimistic world, who gets shot next? Southern Europe or China are the likely candidates. In a world of scarcity, there is simply not enough to go around. Crises about money and central banks may not just be about money and central banks.

Even if you don't believe in peak oil, oil has always been linked to problems. There was the Exxon Valdez spill in 1989 and the Deepwater Horizon spill in 2010. There are the iconic images of burning oil fields in Kuwait in 1991. There's much talk about 9/11 somehow being linked to U.S. oil entanglement overseas.

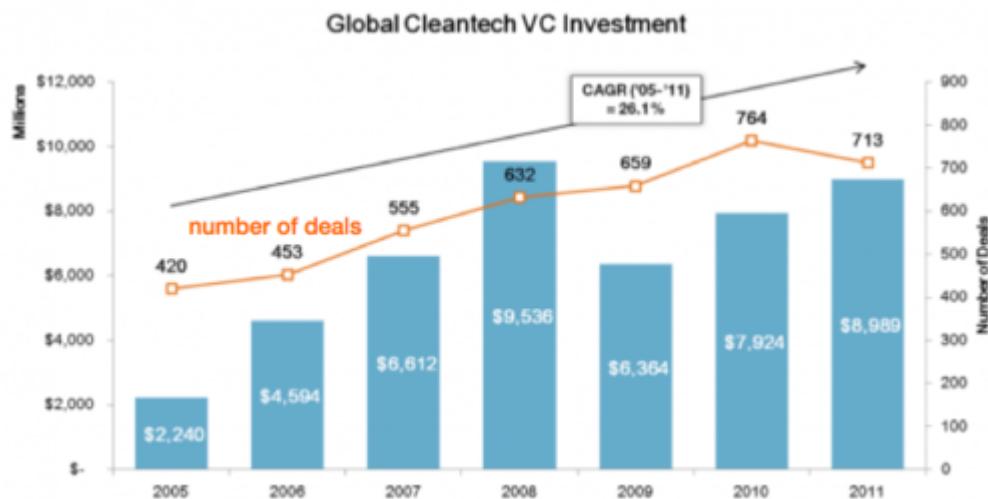


One working thesis is that most major conflicts in the last 2000 years have involved energy in some important way. Oil trade and embargos contributed to the tensions that sparked World War II's pacific theater hostilities. As Secretary of the Navy, Winston Churchill nationalized the Anglo-Persian oil fields in what is now Iraq some two months before World War I broke out. Going back even further, one of the most important events in the Civil War was the secession of West Virginia from Virginia. This was a big move because it effectively gave the North 20x the coal that the South had, which played no small part in the war's outcome.

Consider oil prices over the course of the last century. Prices were very high in the late 19th century when oil was first discovered. But then oil became quite cheap up until the 1970's, when OPEC eclipsed the Railroad Commission as the primary oil policy/pricemaker. The Indian summer in subsequent years overlaps perfectly with the U.S.'s indefinite optimism from 1982 to 2007. And now, as oil hovers around \$100 per barrel, the problem has reasserted itself.



Investment in cleantech accelerated massively through 2010. It has come down a bit since then. But there was significantly more investment in cleantech than there was in the Internet during the last decade. No doubt a big driver of this was the environmental component. Al Gore won a Nobel Prize for drawing people's attention to climate change.



In 2007 venture capitalist John Doerr gave a TED talk about climate change and alternative energy investment:

(16 minutes later...)

The main idea was that we must make investing in cleantech make economic sense, so that the right outcomes are the profitable and thus the likely ones. Doerr obviously became very emotional. Certainly we

can understand why he wants to look forward to the conversation he'll have with his daughter 20 years from now. If you're cynical you can dismiss this as a sob story. But this feeling that something must be done has certainly pushed people very hard to try to make cleantech work. Many of the investment dollars poured into cleantech weren't simply seeking good returns, but were also driven by various environmental and social factors.

III. The Failure of Cleantech

Even if one grants that all these concerns are very legitimate and very real, something still went wrong. Good wishes and noble goals didn't make cleantech investment profitable. So what happened? And is cleantech still questionable today?

A. The Nature of the Problem

One problem was that people were ambiguous on what was scarce or problematic. Was there resource scarcity? Or were the main problems environmental? Granted, the environment can be framed a resource in some sense. But people tend to conflate the two without really thinking through it. There is an argument that both resources and the environment might have been scarce or problematic. But people tend to focus on the environmental stuff over resource scarcity. That is probably a mistake.



Think about how it plays out. If you believe that there is an environmental problem but no resource problem, you'll be inclined to favor subsidies for cleantech. Conventional energy sources like oil will always be abundant and cheaper than alternatives, so you need to prop up the alternatives. If, by contrast, you believe the problem is resource scarcity but not the environment, you might just want to ration conventional things.

Often the solution will be the same whether you're facing a resource problem or an environmental one. But sometimes they point in very different directions. Joseph Stiglitz has observed that peak oil theory might have been invented and pushed by environmentalists who were against climate change; the carbon emissions problem, after all, is solved just as soon as (you convince people that) oil runs out.

B. List of Mistakes

Enumerating all the mistakes that were made in cleantech would be quite a project. But the most important were mistakes about the following:

1. markets
2. mimesis and competition

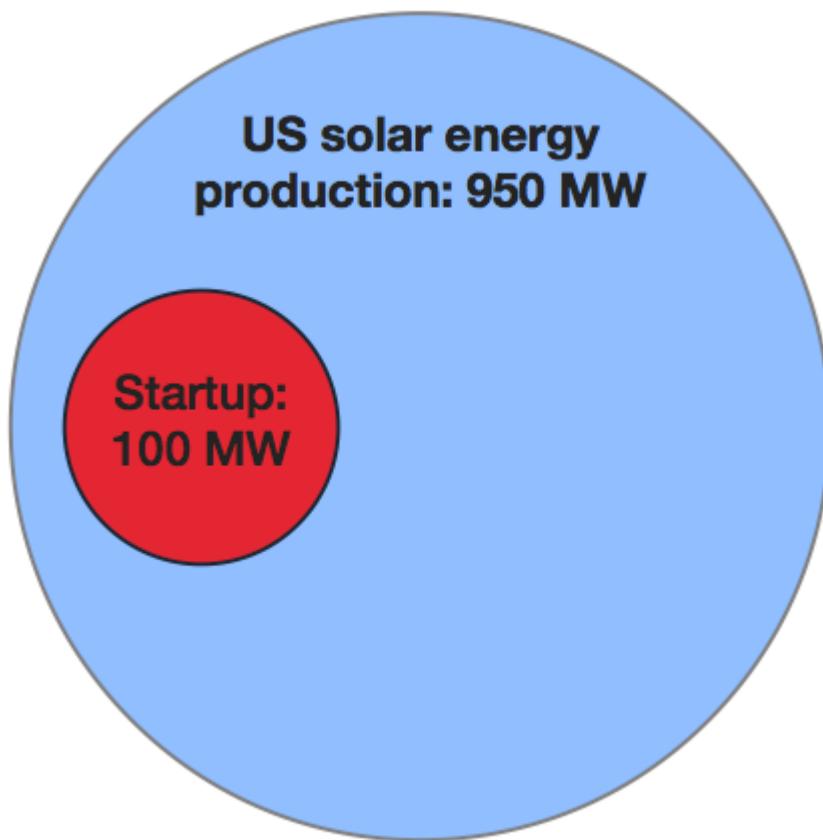
3. secrets
4. incrementalism
5. durability
6. teams
7. distribution
8. timing
9. financing
10. luck

To have a successful startup, you must have good answers—or at least a good plan for getting those answers—to all 10 of these points. But with cleantech, very often people were starting companies or investing at 0 or 1 out of 10. And, to reiterate, you really do need all 10; 8 out of 10 is sort of a B-, and 5 of 10 earns you an F.

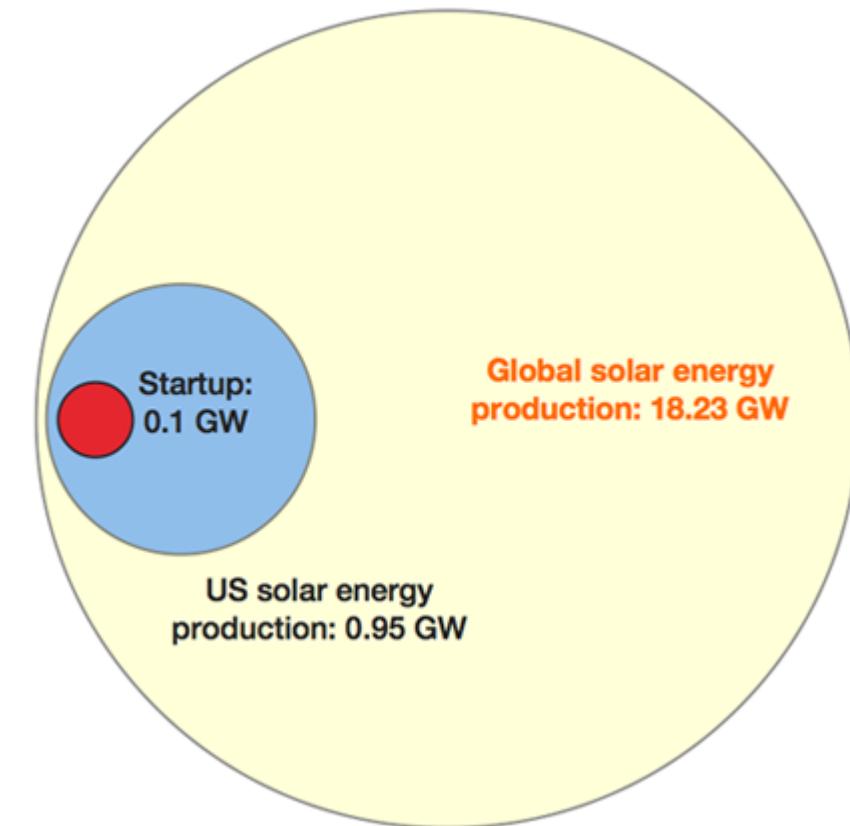
C. Market Mistakes and Competition

We've discussed how people dupe themselves into telling lies about their market, or knowingly lie about their market to dupe other people. The fear with energy is that it's a commodity. The market is huge. The problem with huge markets is that you can't protect yourself from whatever monsters are out there, ready to eat you up.

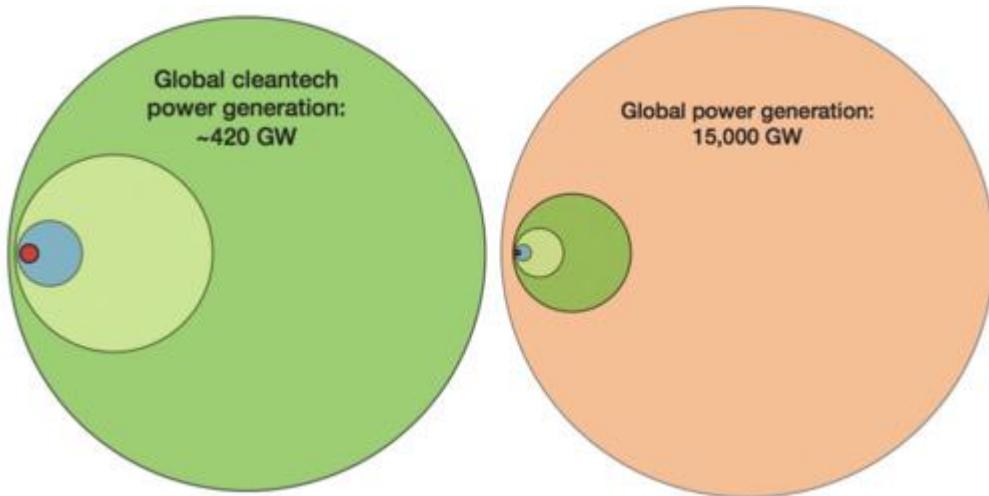
Some people understand this, perhaps too well. So they try and tell stories about being big players in a very small market. Suppose your company is the new Solyndra. You have over 1000 systems installed and represent over 100 megawatts (MW) of power generation. U.S. solar energy production is 950 MW. By that measure, at 10.5% of the market, you're a decent-sized player.



But is the U.S. solar energy market the right market? Or is the relevant market the *global* solar market? Global solar energy production is 18 gigawatts. If you claim to be “the global solar energy provider,” all of a sudden you’re a small fish indeed—less than 1% of the market.



We can take this even further. What if what we should be thinking about is cleantech in general, not just global solar? Global cleantech production is 420 GW. You just got a lot smaller. And then at 15,000 GW of global power generation generally, you're just a dot in the ocean.



So some cleantech companies rhetorically shrank their market to give the impression that they could easily dominate it. Many others made the opposite mistake and just talked about trillion dollar markets in their pitches. That is probably even more dangerous since it starts to look like the world of perfect competition. You're probably much better off just opening a pizza restaurant in Palo Alto.

There is both an economic aspect and a psychological aspect to perfect competition. The economic insight is that the battles are so fierce because the stakes so small. Since profits are competed away, people are fighting

over scraps. The psychological insight is that the economic insight is really weird. Why in the world would people want to fight for scraps?



But there's a psychological counterpoint to the psychological insight, and that is that people fight because they think it's the cool thing to do. This explains the phenomenon of social entrepreneurship, which can be defined as doing well while doing good. The problem is that social entrepreneurs usually end up doing neither. This is not to say that companies should always maximize profits to the exclusion of everything else. But companies *should* have a specific mission. They should be solving some discrete, important problem. Social entrepreneurship fails that test. It has an incredible ambiguity to it. Is it actually good for society? Or is it simply *approved of* by society? Those are very different questions. If they are not—if what is good is simply what the masses approve of—progress is very doubtful, since everyone will end up doing more or less the same thing. Everyone will have a solar startup. Each will have some story about how theirs is slightly different. But query whether those are meaningful differences or just eccentric ones. In vast, competitive markets you often end up with mimetic competition.

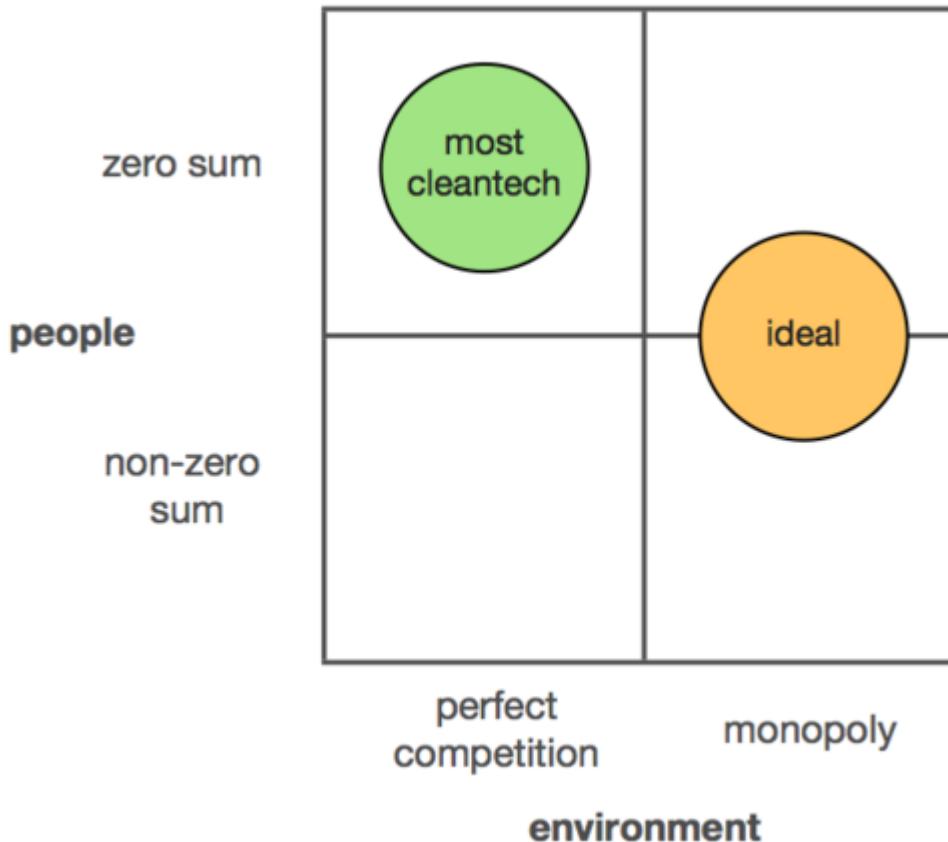
D. Secrets and Incrementalism

If you want to start a company, you should have some important secret. The secret doesn't need to be that big if you're doing a classic Internet company, since those generally take less time to build and you can scale them pretty quickly. But if you have something that takes 10-15 years to do, having a small or esoteric secret is not enough to build a decisive lead. Secrets can allow you to escape mimesis and competition in a world of long time horizons, but those secrets need to be pretty big. But in practice, most wind, solar, and cleantech ventures relied on incremental improvements. Solar costs fell slowly over a number of years. Wind power came down a bit quicker, but there was still no real step function to it. Improvements in battery technology have been fairly incremental as well.

E. Durability Mistakes

The counterpoint to the incrementalism problem is the durability question. There are many different solar cell technologies. There's thin-film tech. There is multi-junction concentrator tech. There are crystalline Si cells. And there's emerging photovoltaic tech. And then there are several distinct approaches within each of these categories. To build a great solar startup, you have to be better than all the executions of all these competing technologies. And then you have to fight at the level of next pie chart up; you have to be better than wind, hydropower, etc. Your goal in a startup should be to dominate and own your market for 20-30 years. What are the odds that your incremental solar cell technology is going to be durable over that kind of timespan? When there's an identifiable pattern of incremental progress, it is very unlikely that you'll have the last mover advantage when you make a marginal addition. But the question of durability has been obfuscated in cleantech. Like manufacturing in 1980s, things have steadily improved. But no single great companies have emerged or are likely to remain.

F. Team and Culture Mistakes



Most cleantech companies in the last decade have had shockingly non-technical teams and cultures. Culture defaulted toward zero-sum competition. Savvy observers would have seen the trouble coming when cleantech people started wearing suits and ties. Tech people and computer people wear t-shirts and jeans. Cleantech people, by contrast, looked like salesmen. And indeed they were. This is not a trivial point. If you're dealing in something that's incremental and of questionable durability, you actually have to be a really good salesman to convince people that it's dramatically better. Salespeople and athletes are important, but they shouldn't run things. They are trained to compete and tend to think that all that matters is how to defeat other people just like you.

G. Distribution Mistakes

Many startups run into problems because they discount the importance of distribution. But cleantech's problems in this sphere were even sharper; companies literally couldn't distribute the power they would generate. Even if you build a huge, efficient solar farm in Southern California, how do you build power lines to get the energy to L.A.? In practice, people tended to ignore the difficulty of connecting with the grid. It was assumed not to be a very interesting or major problem. But in many cases it proved decisive.

Distribution issues certainly weren't impossible to spot. Peter Orszag, President Obama's former budget director, explained that relatively little of the Stimulus was spent on infrastructure because it would take too long to get all the zoning permits necessary to build the power lines. The administration concluded that it would just be too hard. So to their credit, they foresaw the serious distribution problem and didn't build the useless, unconnectable wind farm. Plenty of companies didn't see that and failed.

H. Timing Mistakes

Bad timing can ruin you, even if you have all the other pieces figured out. Where you are on the timing curve is incredibly important. The usual timing argument in cleantech goes like this: cleantech is inevitable because it's really important. The big wave will come 4 or 5 years from now. So we should start now and we'll catch that wave when it comes. The general insight is right; if you don't start paddling sometime before the wave arrives, you're too late and you'll miss it. But if the wave is really several years away, it's not at all clear when you should start paddling. It's very hard to get the timing right, especially in cleantech when cost curves can change rapidly.

I. Financing Mistakes

When thinking about cleantech investments, it's useful to remember how the power law applies to venture capital. Since company outcomes are not normally distributed, VCs have to look for 10x returns. But Solyndra, for instance, took \$1.65 billion in late stage venture-type financing. When investors put in that kind of money into a company, it has to grow phenomenally large for things to work out. A good, broad rule of thumb is to never invest in companies who are looking for less than \$1 million or more than \$1 billion. If companies can do everything they want for less than a million dollars, things may be a little too easy. There may be nothing that is very hard to build, and it's just a timing game. On the other extreme, if a company needs more than a billion dollars to be successful, it has to become so big that the story starts to become implausible. This is especially true in cleantech, where there are many others who are doing uncannily similar things.

J. What Is Required

One perspective on tech and energy innovation makes a distinction between brilliant inspiration and incremental improvement. Another thing to keep in mind is complex coordination; even if you can execute on a brilliant idea that you can then incrementally improve over time, you have to coordinate how it fits into rest of society. Complex coordination is easier on the Internet. Web businesses can take the Asperger's approach where they can succeed without having to talk to anyone. Cleantech is different. With cleantech, you do have to talk to people, and you have to get a great many of them to do things.

Here is how cleantech has stacked up on these three variables, generally:

- **Incremental technologies:** the record has been pretty good on this. There have been some improvements and costs are going down. But there aren't really any secrets. Everything is mostly convention. One gets the sense that the questions almost answered themselves.
- **Coordination:** people were mainly counting on luck here. Distribution and integrating technology with society was an afterthought. The attitude seems to have been, "I'm scientist. I'll build better solar tech. How to deliver it to L.A. is not my job. Others will do it." But when everyone thinks that, it doesn't get done. When it is assumed that the nature or the market is beneficent and will provide, it's all just magic, mystery, and luck.
- **Breakthrough technology:** for the most part, people haven't even tried to operate under this paradigm. No one has been asking the biggest questions. People have assumed that only incremental approaches would work.

K. The Solyndra Failure

What is striking about the Solyndra fiasco isn't what happened, but rather how people talked about it afterwards. No one asked whether Solyndra's technology worked. But that is precisely the kind of substantive engineering question that you would ask in a determinate world. In an indeterminate world, though, people ask legal and financial questions. They focus on whether the proper processes were followed. And this is exactly what happened.



The Republican criticism was that government officials were too involved with the company and that raised all sorts of ethics questions. The Democrats countered by insisting that the process was legitimate and transparent. From the definite optimistic perspective, the Republican critique is way off base. If the technology actually worked, some very minor wrongdoing about how some funds were spent becomes almost a footnote. The Democrats' defense was equally weak and process-focused. Much better would have been the President to declare, "Here are 2 or 3 technologies that we think are going to work" and then try to get serious about them. The best defense was not made. Indeed, it was probably impossible to make given how modern politics works. The entire Solyndra aftermath was one big fight about processes. It was the government version of the HP board fiasco.

IV. Energy Futures

It's easy to be critical of cleantech. Hindsight is always clear. It is thus important to go beyond criticism and offer up some thoughts—however vague—on what might be done better the future.

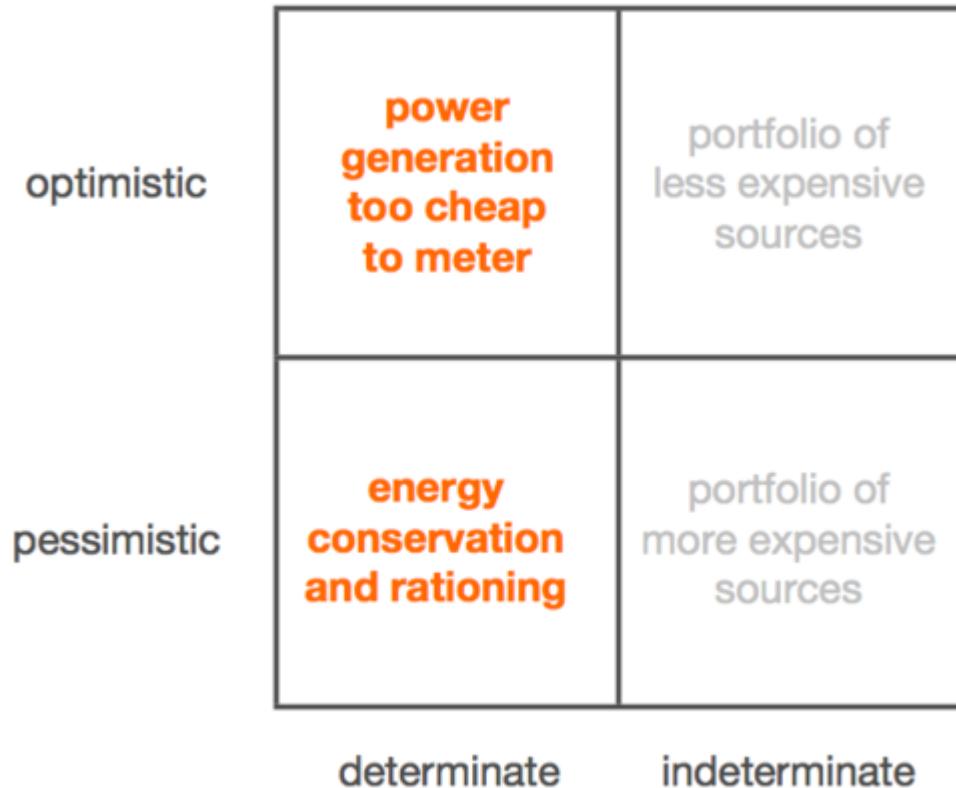
A. The Optimistic Determinism of Software

It might be fair to say that the Internet has been cleantech's closest cousin over the last decade. eBay is basically a recycling company. Amazon is getting rid of suburban sprawl. And Airbnb is curbing excess and unnecessary hotel construction costs.



Determinate pessimism simply won't work in energy. Conservation and rationing may help, but they aren't quite enough. Even if Americans actually started conserving energy in a serious way—even if, say, everyone got much smaller refrigerators—the developing world is relentlessly consuming more. When everybody in Uttar Pradesh gets a fridge, it will just cancel out our conservation here at home.

The question is thus whether there is some way of actually realizing the idea of generating power that is too cheap to meter.



Software may play a large role in answering this question. We might be able to figure out ways to use IT to optimize conservation. One really big problem is that energy pricing fluctuates wildly during the day because that's when most of the power is consumed. Things like smart appliances and smart thermostats may be able to downshift daytime consumption.



There are very interesting applications of computer technology on the transport side too. Things like the self-driving car or finding ways to outsmart and defeat traffic could have a very big effect.

B. God of Thunder

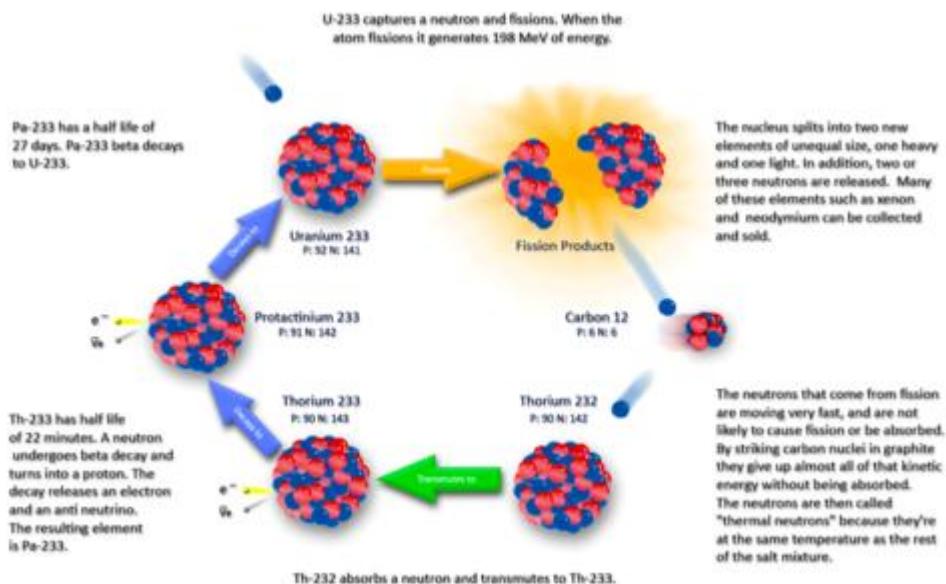
But suppose we wanted to shift all the way to optimistic, determinate solutions. How would we do that? One suggestion is that we should explore thorium power in a very serious way. Thorium is a big secret. When the government became very interested in nuclear technology in the 1940s, it found that you could get nuclear power from three chemical elements: plutonium, uranium, and thorium. The problem with Thorium was that it contains no fissile isotopes, which means you can't weaponize it. And the government was interested in building bombs, not generating power. Eisenhower's '53 Atoms for Peace speech was



originally intended to warn of the perils of a thermonuclear age where everyone could be obliterated instantly. When that seemed too dark, it was retooled to talk about the promise of non-weaponized nuclear energy as well. Power generation was decidedly *not* the government's focus during the intensive R&D of the 1940s. But there is a sense today that we really don't need any more nuclear weapons. On that basis alone, thorium power seems worth revisiting.

Thorium seems promising for a couple of reasons:

- Thorium is much more abundant than uranium. There is enough to power the world for a million years at current energy consumption levels.
- Thorium is relatively clean. With uranium, you only end up using about 0.7% and there's a lot of waste from the enrichment process. With thorium, by contrast, there's much less waste because it's a self-contained cycle.
- You can build thorium reactors that don't require hundreds of atmospheres of pressure like uranium reactors do.
- You can't get a runaway thorium reaction for the same reasons you can't weaponize it.
- Thorium is something like 1/10 as expensive as other forms of nuclear power. Thorium plants would cost about \$250 million to build, whereas uranium plants cost \$1.1 billion.



If you sum all these benefits, thorium power would be something in the zone of an order of magnitude better than what's currently possible.

Of course, some very tricky questions remain. How would one actually build it out? There's a considerable coordination and distribution problem. There's also a regulatory problem. But let's return to that list of 10 essential things to get right. Unlike most cleantech ventures, where the score was 0 for 10, with thorium power you basically get 6 of 10 right off the bat:

1. You solve the mimesis/competition problem by avoiding fashionable competition. Solar companies are hot. Thorium companies aren't.
2. The big secret is that thorium has been underexplored for political reasons.
3. Thorium power is certainly not incremental.



4. Durability comes from thorium's being an order of magnitude cheaper. Pull off a move to thorium and you'll own the energy market.
5. The timing seems right.
6. The venture would be expensive, but not prohibitively so. Given all the investment in cleantech, raising \$250m over the course of several years of hitting targeted milestones seems reasonable.

The market, team, distribution, and luck pieces are harder to figure out. It would be wise to do that before moving forward. Are there specific countries or markets that should be targeted? Are there even any nuclear engineers left that can work for you? Solve these pieces, though, and you'll have done all you can to maximize your mastery over luck.



Admittedly, this isn't a rock-solid case for thorium. It isn't intended to be; we're not starting a thorium power company. Rather, this is simply an idea of how one might think about doing things in alternative energy. We've had 10 years of failure in cleantech. All the intentions have been good. 20 years from now John Doerr *should* be able to tell his daughter that progress has been made. But well wishes won't usher in that progress. People have to think seriously about covering the 10 bases we've identified.

V. The Government Question

For better or worse, you cannot talk intelligently about cleantech without talking about the government. There has been a great deal of government entanglement with energy and cleantech in recent years, so it is important to reflect on that experience and try to get a sense of what works and what does not.

A. Sell/Take/Replace

You can think of technology's relationship with government as fitting one of three molds: being sold to the government, being subsidized by the government, or replacing the government.

It turns out that all of these molds are pretty tough. VCs don't typically like investing in companies that depend on government sales because selling into government is quite difficult. Getting government subsidies—at least in large amounts—is even harder. And replacing government is tricky because government people tend to object to it. If your secret plan for your technology is to replace the government, it's best to keep it secret.

An important macro fact to keep in mind is that the U.S. budget deficit is currently running about 10% of GDP. Optimistic projections have it going down to about 2%. Less optimistic forecasts have it going to 6-7% and then rising again in 2020 and beyond. This can be seen as a big secret that's hidden in plain sight. Since no one knows what to do about it, we're not really allowed to talk about it.

But thinking through it offers up a strong argument against relying on government subsidy. The budget math means that there probably won't be any money left when you need it. Money will be even tighter in the future. Contrast SpaceX with Solyndra. At least SpaceX's orientation is selling technology to the government (and possibly replacing it, now that the government has decommissioned its rocket-building programs). The risk with SpaceX is that the government runs out of money, and even a cheaper and more efficient space program isn't going to work. But that risk is minimal compared to the risk of relying on heavy subsidization in a future where government funds are likely to be very tight.

B. Future of Cleantech?

Probably the best place to anchor in thinking about the future of energy is in the optimistic determinate quadrant. The key questions are the same as those for Internet businesses: What can be done that's better and cheaper? Can you do more for less? That, of course, is the classic definition of technology.

So can we do more with less in cleantech? Quite possibly we can. But we need to think about things in the same way we do in the computer industry. Is the breakthrough thorium? Is it something else? We certainly need a big breakthrough. Only then does it make sense to work on incrementally improving it. The first step, as usual, is to think big and think boldly about the future.

Peter Thiel's CS183: Startup - Class 15 Notes Essay

Here is an essay version of class notes from Class 15 of CS183: Startup. Errors and omissions are mine.

Four guests joined the class for a conversation after the lecture:

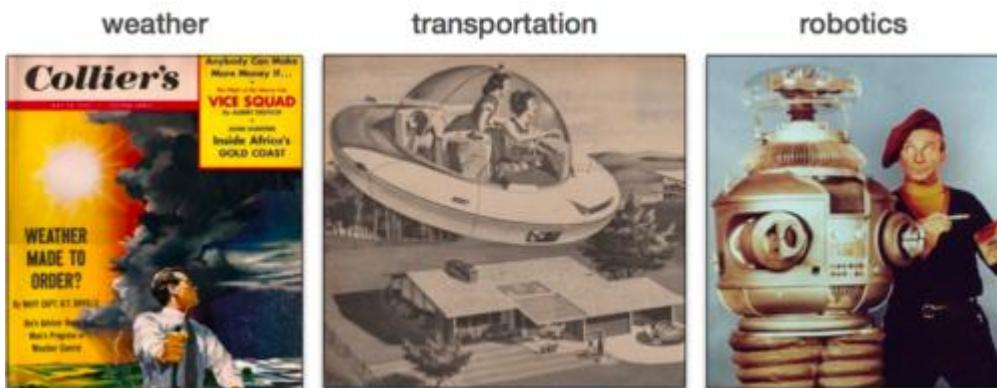
1. Danielle Fong, Co-founder and Chief Scientist of LightSail Energy;
2. Jon Hollander, Business Development at RoboteX;
3. Greg Smirin, COO of The Climate Corporation; and
4. Scott Nolan, Principal at Founders Fund and former aerospace engineer at SpaceX (Elon Musk was going to come, but he was busy launching rockets).

Credit for good stuff goes to them and Peter. I have tried to be accurate. But note that this is not a transcript of the conversation.

Class 15 Notes Essay—Back to the Future

I. The Future of The Past

Sometimes the best way to think about the future is to think about the way the future used to be. In the mid-20th century, it was still possible to talk about a future where the weather would be precisely predicted or even controlled. Maybe someone would figure out how to predict tornadoes. Or maybe cloud seeding would work. Transportation was the same way; people expected flying cars and civilian submarines. Robotics was yet another exciting frontier that people thought would be big.



But fast-forward to the present. Things haven't really worked out as people thought they would in the '50s and '60s. Weather still kind of just happens to us. People have pretty much accepted that as inevitable. The prevailing sense is that trying to control the weather is dangerous, and we shouldn't tinker too much with it. Transportation has been similarly disappointing. Forget flying cars—we're still sitting in traffic. There has been some progress in robotics. But certainly not as much as everybody expected. We wanted the General Utility Non-Theorizing Environmental Control Robot from Lost in Space. Instead we got the Roomba vacuum cleaner.

weather



transportation



robotics



All this is in stark contrast with computer science. There are no CS visions of the future that haven't happened yet. We've seen Moore's Law hold up, a relentless reduction in power consumption, and ever-increasing connectivity. Visions of the future from the past are more or less here. The 2-Way Wrist Radio/TV wristwatch from Dick Tracy watch is essentially an iPod nano. Arthur C. Clark basically predicted the Internet in his 1956 book *The City and the Stars*. So in computer science at least, it's possible that you can't get very much new insight by going back to the past.



But in most areas, things got seriously off track. The future didn't work. Nuclear power is another area that we were all but certain would work well. Instead, it turned out to be far more dangerous than people expected. There were all kinds of ways that people thought it was going to work that didn't pan out, or at least haven't happened yet.

So what might work today? One way to tackle that question is to think seriously about going back to the future. There may be lines of research that people didn't pursue at particular points in the past that are worth revisiting. Not everything that has gone underexplored deserves to be lost. We talked about thorium power in our last class. There are various complicated reasons why that may or may not work. But if you suspect it was underexplored because the military favored plutonium/uranium R&D, it's at least worth taking a look at.

Going back to future can be fun. Past visions of the future—think robots or space travel as imagined in the '60s—are culturally iconic. But the catch is that simply reminiscing doesn't do us any good. We can't simply go back to the past and copy it to make a better future. We have to remember that the future of the past didn't

work. The goal, then, is to tap into the past, learn all we can, and deploy that knowledge in some new, important way.

II. Where The Future Has Failed

There are many areas in which the future could be said to have failed. Let's explore four: energy storage, weather, robotics, and space. After a quick, more abstract run-through of each, we'll have a discussion with some people from companies that are doing interesting things in those spaces.

A. Energy Storage

The main problem with energy is that production costs are quite variable. Energy is considerably more expensive during peak usage times. But since it's very hard to store power that is produced off-peak, good solutions have been elusive.

Batteries have been the primary energy storage technology. But there is reason to believe that we are running up against serious and possibly unyielding limits to battery technology. Batteries are 200 years old. There have certainly been considerable improvements along the way. But the trajectory feels asymptotic. There are limits to the number of batteries that you can pack into a given space. There is a corrosion problem. Since most batteries involve positively and negatively charged particles, there may be chemistry limits. At this point coming up with better battery technology may be like finding a new element on the periodic table.

So the future of energy storage is interestingly unclear. The chemical storage paradigm has done wonders, but maybe it's done doing them. The question then becomes whether it is fruitful to think about energy storage in completely different ways? Are there other, non-chemical ways to attack the problem?

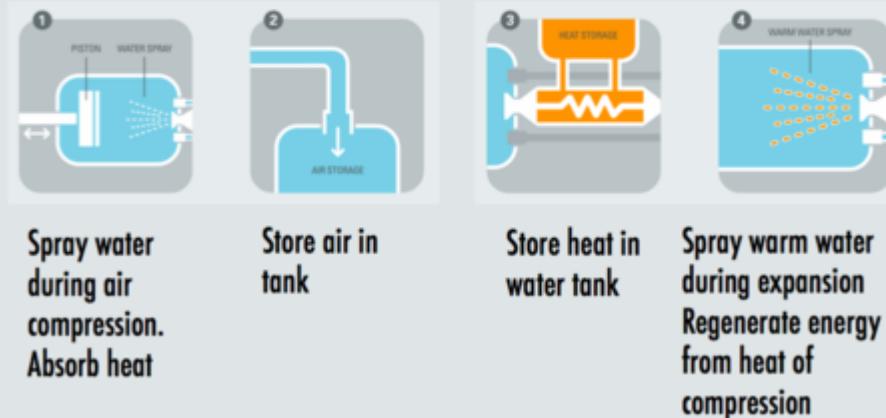
One company to highlight here is LightSail Energy. LightSail is developing a way to store energy more effectively. Again, this would enable a sort of incredible arbitrage where you can take advantage of the difference between the cost of peak vs. off-peak power production. Batteries and hydroelectric technologies are expensive and very limited. So their radically different approach treats energy storage not as a chemical problem, but rather as physics problem.



Regenerative Air Energy Storage

10x cheaper than batteries. 10x longer lifetime.

Breakthrough efficiency advance: regenerating energy from heat with water spray



At one level, it's basic Boyle's law. You use power to compress air into steel tanks. Later, when you want power, you decompress the air to release it. The main challenge is that air becomes very hot when you compress it. That allows power to dissipate. The fix is to basically spray water into their air to cool it down. Naturally, there are myriad complicated details on how to actually get it to work, but it's a simple idea on a high level. LightSail says they're tackling a trillion dollar market. We should push them on whether that means it's huge opportunity or a rather huge competitive ocean where you can't actually defend yourself. But physical instead of chemical storage is very orthogonal approach, and the technology is very promising.

B. Weather

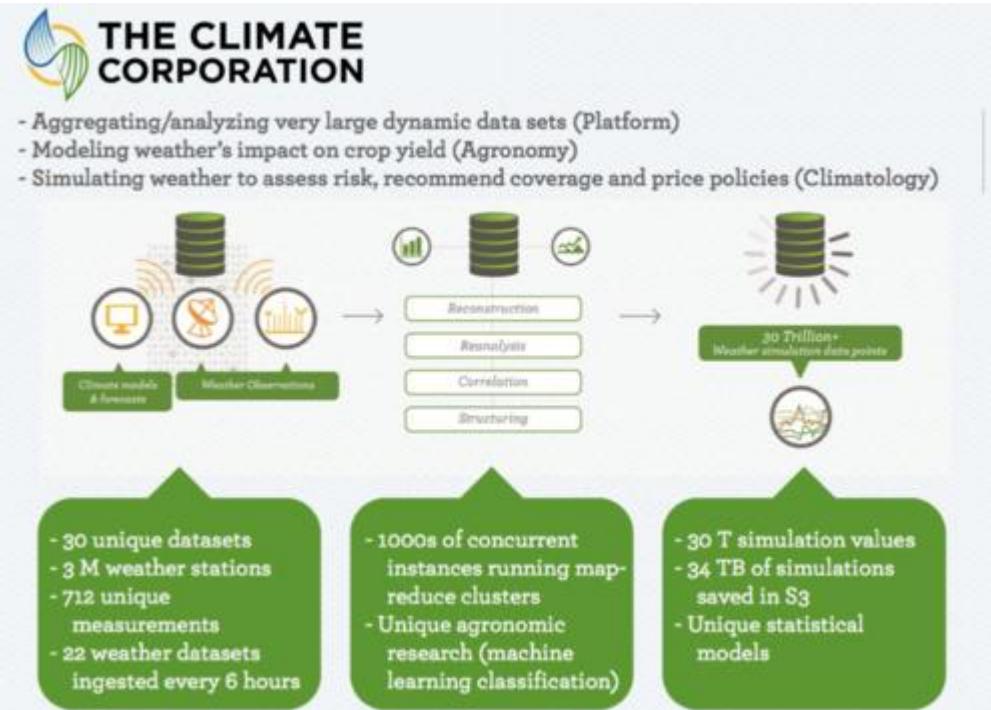


People have been interested in predicting the weather for a very long time. It's sort of amazing that we are still really bad at it. Short-term forecasts are notoriously bad. There are all sorts of excuses for this. We know the systems are chaotic and complicated. But there's a sense in which people given up and just stopped trying. If weather forecasting currently looks like fortunetelling, maybe we can make it less so. Maybe we can get more accurate. Certainly that would be quite valuable.

And then there's the question of whether you can control the weather. This is mostly abandoned territory because the question is perceived to be quite dangerous. Cloud seeding strikes some people as even crazier than terraforming Mars. This is a huge backlash against trying to control the weather, or even thinking about it. Pick your preferred mix of impossible/undesirable/unnecessary.

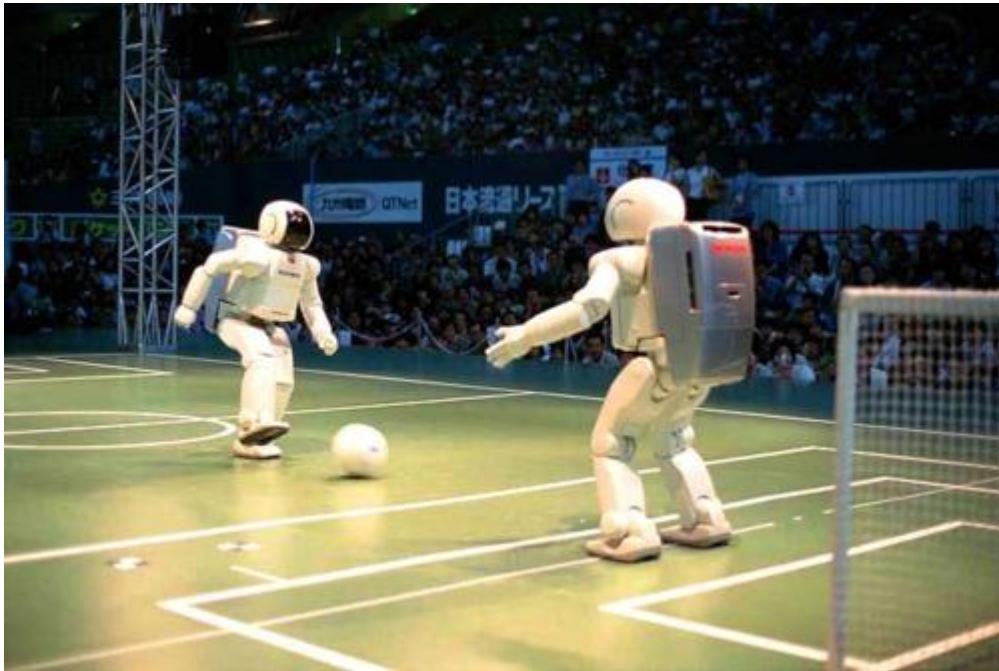


An interesting company in this fighting this reactionary consensus is The Climate Corporation, formerly known as Weatherbill. They are trying to improve the crop loss insurance markets by precisely predicting the weather experience at a particular parcel of land and then making quick adjustments based on advanced computer modeling. It's a statistical approach, so it's worth approaching with some skepticism. That said, it's a statistical approach in an area where people have grown wary of statistical approaches because they have all failed before. So interesting statistics approaches are possibly underexplored because people have given up on them. The secret is further hidden by the fact that hardly anyone looks into Agritech as a sector—by definition everything happens outside of Silicon Valley and so things like this tend to be overlooked and undervalued.



C. Robotics

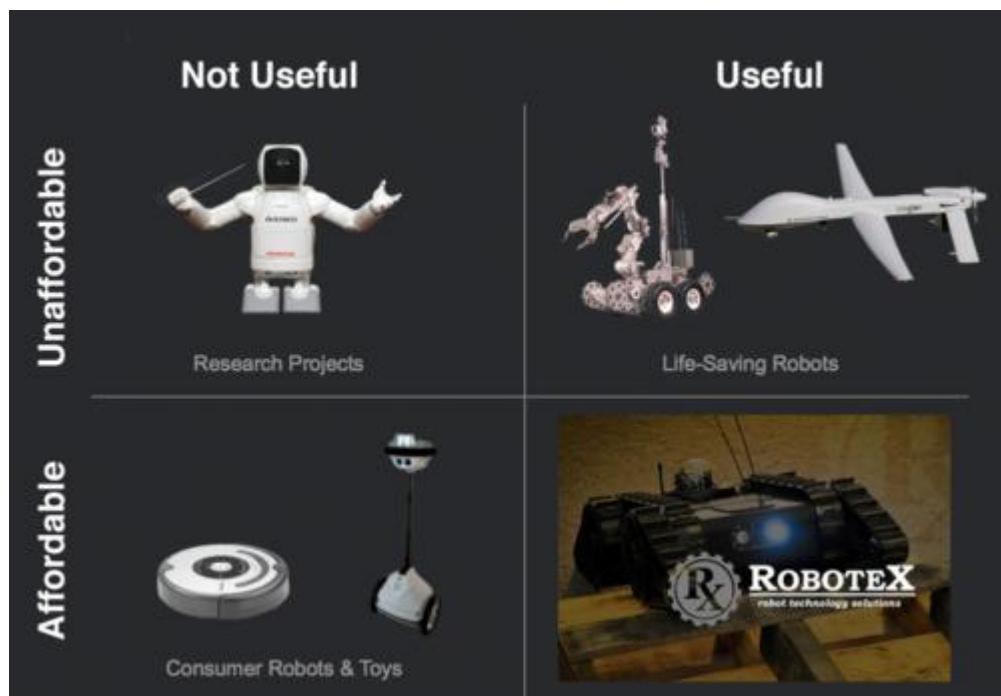
In the 1950s and '60s people had all kinds of ideas about how robots would improve things in the future. A common vision was the household butler/maid/cook/driver model, where the robot would raise standards of living by doing all these chores for free. That obviously hasn't happened. There has been progress, but robotics has been just as much about limits to progress in recent decades. It's quite expensive to build humanoid bots. And abilities are pretty limited. The state of the art is sort of a laundry-folding robot that can fold one piece of laundry in 45 minutes. When you ask experts when we should expect a Lost In Space-style robot, they usually estimate that won't come for another 25 to 50 years. That may be right. And thinking long-term is good. But 25-50 years is *really* long-term; it's just beyond the horizon where people are no longer accountable. So that prediction may be code for, "It will happen, but I don't have to do anything. Someone else will do it."



There are many ways in which we can re-imagine what robots can and should do.

RoboteX is a Silicon Valley-based robotics company that is doing just that.

Modern robots fall into one of the four quadrants on the following 2x2 matrix:



The basic idea behind RoboteX is that humans currently have to do lots of dangerous things. SWAT teams, hazardous materials experts, and bomb squad people all have very dangerous jobs. Instead of sending people in first, you can send in a robot to see what's going on and maybe even take care of the situation. RoboteX robots, for instance, have a history of being quite effective at resolving hostage situations. Bad guys who are all amped up and determined to fight the police sort of psychologically break when they see a little robot.

come in instead, and surrender on the spot. It turns out that lots of organizations are willing to pay a considerable amount to avoid putting their people in harm's way. So RoboteX's reconceiving of robots as intermediaries, and not as humanoid things that duplicate human tasks, is quite valuable.

D. Space

Space was the final frontier for Star Trek in 1967. But ever since it has felt like a frontier that has become quite closed. Space museums feel quite static, almost like history museums. The Apollo 11 moon landing occurred in July of 1969. Apollo 17—the last U.S. lunar landing ever—was in December of '72. So the entire period of moon landings lasted just 3.5 months. It has now been about 40 years since anyone was on the moon. Mars seems extremely ambitious considering that our generation hasn't even done a lunar mission.

Space has always been the iconic vision of the future. But a lot has gone wrong over the past couple of decades. Costs escalated rapidly. The Space Shuttle program was oddly Pareto inferior. It cost more, did less, and was more dangerous than a Saturn V rocket. Its recent decommissioning felt like a close of a frontier. But maybe not. Exciting new perspectives and approaches suggest that a new era of space technology might be at hand. Can we produce more advanced software in telemetry? Develop radically new kinds of rockets? Can new technology take us back to the future?



We have been launching rockets into space for more than half a century now. But the cost to launch 1kg of mass into orbit hadn't changed very much in 40 years. There were all sorts of reasons for this. The key insight is that most of the accounting for traditional space industry work was done on a cost-plus basis. Aerospace contractors charged for a rocket. Cushy profit margins were built in. Since people had lots of incentives to inflate costs, things became increasingly expensive. There's also an argument that they became more dangerous.

The big question was whether it was even possible to re-approach the problem from a radically different angle. That is exactly what SpaceX is doing; focusing on getting the cost structure right and to drive launch costs down, and revisiting basic designs and revitalizing them with new technology and new materials. By most accounts, their radical improvements have been quite successful. SpaceX is arguably ushering in a new era of space flight.



E. The Retrofuture Goal

The retrofuture idea is simply this: think about where the past failed, learn the right lessons, and make it work this time. But it's important to resist just being pulled in by the iconic future of old. There's no sense in making the same mistakes over again. The key is to approach old problems from a very different perspective.

III. Perspectives –Conversation with LightSail Energy, The Climate Corporation, RoboteX, and SpaceX

Peter Thiel: Your companies all represent vary different ways of thinking about the future. We've discussed in this class how radical uniqueness can be a very good thing in business. But does it make it hard to recruit people?

Scott Nolan: SpaceX recruited me out of my aero program. It was kind of an easy sell, actually: "You can go work for Boeing or Lockheed. Or you can come to SpaceX." The established players had the cost-plus accounting model. There was a sense that there would be no meaningful opportunity to contribute there. Problem-solving mainly involved throwing more people at problems. But young engineers crave responsibility. So you see a startup like SpaceX and think, wow, I can surround myself with 30-50 amazing people and I'll get all this responsibility. I can actually change something.

So I think it's pretty easy for SpaceX to get young engineers who are more startup kind of people. The harder part was attracting the good, experienced aerospace people. It's a lot trickier when industry people are and have been making several hundred thousand dollars per year. Trading a cushy job for startup equity can be a tough sell to risk-averse people.

Danielle Fong: We haven't had too much trouble recruiting at LightSail. There are many talented engineers in Silicon Valley who are doing electronics and hardware. All the members of our founding team joined up pretty easily. We started out in a garage machine shop. We couldn't just grab experienced people in industry, though; there simple aren't that many people who specialize in reciprocating aerocompressors. So we really

had to search. We went through over 1000 resumes and did about 400 interviews. Many of our best people have actually come from the auto racing industry. These are startup-compatible people. They understand hard work, small teams, and deadlines. If your car isn't ready come race time, you lose. And the rules change all the time, so they are used to redesigning engines year after year. Learning enough about the racing industry to recruit from it was kind of a challenge. But ultimately, we had some success by asking these amazingly talented people whether they really wanted to spend the rest of their lives making cars go around in a circle.

Greg Smirin: That kind of narrative can be powerfully persuasive. In our business we need lots of quants and sophisticated software engineers. But Facebook and Google and all these me-too ad optimization companies suck up lots of talent. So we frame it rather provocatively; solving weather prediction is more important than helping middle-aged women send virtual pigs to each other. Gaming is a really cool industry. But a lot of people aren't satisfied by building games, and want to do something bigger.

Jon Hollander: RoboteX's founders, Adam and Nathan Gettings, started off building robots in their garage. Adam did the mechanical piece and Nathan did the software piece. The humble roots were important. Many people make the mistake of trying to build really advanced stuff right off the bat. But far more important is to start with basic machines that can deal with the laws of physics. A robot that can't climb stairs or move across terrain is useless, no matter how advanced its computers or cooling systems. Most of our competitors are quite fancy, but the basic things have problems. The tracks fall off the robots.

Once we had the first prototype, it wasn't very difficult to get engineers. Robotics is pretty sexy. Our pitch is compelling: We build robots that save lives. Customers applaud us every day. Just today, one of our robots ended one of those hostage negotiation situations you mentioned. This guy who was holding his wife at knifepoint was spooked into surrendering peacefully. We're not interested in building humanoid Rosy Robot or C-3PO. We build effective, low-cost robots for police departments, power plants—anybody with sort of dangerous or tactical needs.

Peter Thiel: Let's talk about the substance of your underlying technology. At least on a superficial level, you're all trying to solve old problems. The retrofuture element raises two questions: What went wrong in the past? And why do you think you can solve these problems now?

Danielle Fong: It turns out that the idea of using air as a medium for energy storage has been around for very long time. It was very popular in the 1870s, some 10 years before the electrical grid. And during that "golden age of compressed air," people actually tried improving efficiency by spraying water in air! They knew that water has a very high heat capacity. But the technology was completely abandoned. All we can dig up is that there were "problems." We don't know any more than that. It's possible that they didn't have right materials. Maybe they had corrosion problems. It's really hard to debug the mental processes of long-dead inventors. But what's really amazing is that if things were done in the right sequences—if they had gotten aerocompression right—the history of technology would have unfolded extremely differently. There is a powerful path dependency to the history of energy.

Greg Smirin: People have been thinking about predicting the weather for a long time. And the concept of farming insurance isn't new either. It's probably been around since biblical times in some form or another. People would calculate odds the best they could and write policies as best they could. There are plenty of newspaper references to crop insurance in the late 1800s and early 1900s. But people then obviously didn't have the computational power or the data to calculate very well. There was imperfect information on one

side or the other. Today, we have that computational power. And we have the data. We've got a thousand CPUs spinning, crunching data from remote sensors that are measuring things very granularly at various points across a given plot of land. One of the challenges is how to boil all this data down in ways that are understandable and accessible to the farmers themselves—in a sense, the opposite problem that people faced 100 years ago.

Jon Hollander: One reason people haven't gotten robots right in the past is a lack of focus on the mechanical side. Science fiction is very good at getting people excited about robots. But it also gives them biases. People see humanoid bots on the silver screen and then go try to build robots with legs. That makes no sense. Tracks are much better. But even more important is that very high production costs have held the industry back. Low-cost robots would be ubiquitous, like laptops. But very few companies can build them. To keep costs down, RoboteX leverages the computer industry. There is this huge, efficient Asian infrastructure that has been developed to build computer components. So we take advantage of that and power our bots using off-the-shelf computer components. And then there are other materials innovations. Instead of cutting pieces out from aluminum blocks, for instance, we use strong plastics to make the same piece for maybe 1% of the cost.

Scott Nolan: Since launch costs hadn't been decreasing before SpaceX, it was clear that the rocket industry wasn't really progressing. The reason a startup like SpaceX could succeed—and hopefully continue to dominate—was that it radically restructured the development process. Large, established players would spec out a system and outsource things. There was a lot of friction in the design process, and the incentives were to keep costs high. SpaceX changed all that. They disrupted the industry through in-house vertical integration. They came up with new engine designs, new structural designs, and new avionics. They took the concept of composites to the extreme. There were lighter components and new welding techniques that were used. Somewhat forgotten fuel injectors were resurrected. The number of innovations or radical re-thinkings of things is staggering.

Jon Hollander: Though RoboteX started really horizontal to drive down costs, it is becoming increasingly vertically integrated. We now have our own molds, chip designs, radio card designs, etc. You can bring down production costs quite a bit if you design things that do exactly what you want and no more. You don't have to pay anyone's markup.

Danielle Fong: SpaceX was incredibly bold in this regard. Usually cost structures have little to do with the actual cost of components, and more to do with what people have historically been able to charge for those components. Shaking up an entire industry that ran on a cost-plus basis surely ruffled some feathers. But it was a huge opportunity. One tactic I've learned is to simply tell any supplier who provides a gigantic price quote that I can just make the part myself. They go into negotiation mode very quickly, and lower the price. Then you take *that* price to another supplier, and basically let them bid against each other. The goal is to be efficient where you're not vertically integrated and also efficient where you are.

Question from the audience: What has been your experience recruiting from or working with academia?

Greg Smirin: We've got about 20 people with PhDs at the Climate Corporation, and for many of them this is their first job outside academia. There's certainly a transitional period. We have to help people learn how to use their talents to actually impact an organization instead of just applying for research grants or things like that. There is lots of screening on both sides before we bring someone on. Everybody's looking for a

good cultural fit. Not everyone fits, of course. But the ones that do are incredibly energizing and enthusiastic. Being able to contribute and have product impact in their areas of interest is intensely rewarding for them.

Danielle Fong: We work with professors on research topics all the time. Their input can be very helpful in terms of shifting discussion and vetting new ideas that we come up with. Professors are also pretty honest with us when we do reference checks and ask whether prospective hires are any good.

Question from the audience: How do you solve the distribution problem when you have to sell to the government or very large enterprises?

Jon Hollander: You can basically approach government sales in one of two ways. The conventional way is to approach the military and do deals or research contracts there. The unconventional way, which is what RoboteX did, is to start small, target institutions like local police departments, and grow bottom-up. Staying thoroughly private was very important to us. Taking government funding often imposes serious constraints later down the line. By selling to lots of smaller agencies, we maintain a sense of freedom, get feedback from enthusiastic users, and can build up a nice revenue stream. And the plan, of course, is to scale that up and now we're going after any and every large commercial business that has some sort of hazardous material problem.

Peter Thiel: A general rule of thumb on distribution is the larger the cost of a product, the slower the process. The classic mistake people make is to indulge the fantasy that you can just get that single contract for \$100 million and everything will be golden. In practice it almost never works out that way. Theoretically it makes sense for a large nuclear power plant to deploy robots. RoboteX bots would have been very useful during the Fukushima disaster. But you generally can't just go and sell to the theoretically ideal customer. There are sorts of processes in place. When thinking about sales and distribution, you have to remember that people don't know what they want. It's never completely objective. So even if your technology is an order of magnitude better, you can't just make the golden sale. The person writing that \$100 million check will ask who else has bought the product. If the answer is no one, they may well stop writing that check, since that means there is probably something wrong with it.

Very successful enterprise startups tend to have iterative sales models. They achieve a 50-100% growth rate year over year for several years. They might make \$5M in the first year, and if things go really well, that will double every year for a decade. You might wonder why the revenue doesn't just 10x in the fourth year when everybody understands the product's superiority. But it doesn't work like that. It usually turns out that no customer is willing to do a deal that's 10x the size of your largest deal to date. Maybe 2x your biggest deal is a more realistic hope.

There is a venture capital version of this too. VCs fantasize about finding one really rich LP to invest in a fund. Find that golden investor and then you don't have talk to anyone else. But it never works. LPs, like everyone else, cluster together. Everybody likes to act like they know what they're doing. But in reality no one has a clue. Everyone is affected by everyone else in a hidden, unspoken way.

So the strategy should be to get the smallest customer that is also a good reference customer. Move quickly and acquire good references. RoboteX sells a robot to a local police department. The sale itself may not be huge, but the company can leverage any success in that narrow deployment. If the robot is good enough to stop a guy from shooting hostages, maybe it's good for other things too.

Danielle Fong: The exception is when someone is desperate for something that you can provide.

Peter Thiel: Yes. Always start with those people.

Danielle Fong: People who are in a pinch when other suppliers fail or flake out on them are often excellent customers.

Greg Smirin: Most sales models fail because people miscalculate the sales cycle. They underestimate just how long it takes to land the ideal clients. So a good recipe for success is to get the smallest, best customers you can, *quickly*. Jump the easy hurdles first. Then you'll know more about how to deal with the larger enterprise customers.

Scott Nolan: And you can appeal to people's sense of urgency. SpaceX would approach the DoD and ask: Do you want to fly your Delta IV in 4 years? Or do you want to buy a Falcon 9 at less cost and fly next year?

Question from the audience: How do you think about long-term exit strategy? Do you plan on being acquired or going public?

Danielle Fong: In theory, we would consider an acquisition. The issue would be whether the acquirer would likely mess up what we are doing. That seems fairly likely, since we're taking such a unique approach to energy storage. But one never knows. The goal has to be to build a great business that can and will eventually trade publicly.

Greg Smirin: Any compelling technology that is released onto the market will attract acquisition offers. A company's board of directors has a fiduciary duty to maximize shareholder value. So you quite literally have to consider acquisition offers. But from an operating perspective, there are all kinds of reasons that many or most acquisition proposals don't seem right for hardcore technology startups working on hard problems.

Peter Thiel: Remember that sales works best when it's disguised. Even un compelling companies with terrible ideas would not say that they are planning to sell. If you want to sell, the best thing to do would be to act like you don't. The VC version of this is: If you want advice, ask for money. If you want money, ask for advice. The political version is solemnly affirming that you have no interest in running for President. So a good first step toward selling your company is declaring that you'll never sell it.

Typically, M&A is done for two reasons. First is to grind out inefficiencies. Banks, for example, just buy up smaller banks fire half the people. This creates somewhat larger, but more efficient banks. But simple efficiency is not what typically drives M&A in technology companies. In the tech world, M&A is usually about product synergy. It makes sense to merge when there is deep complementarity between two companies. There were big synergies between PayPal and eBay, for example. But that is the exception, not the rule. In practice, such synergy rarely exists. This is especially true where really unique technology is involved. Genuine complementarity between truly novel technology and what existing people are doing is very unlikely.

Question from the audience: Can you comment on the fundraising process for hardcore tech companies?

Danielle Fong: Well...it's really fucking hard! [laughter]

Scott Nolan: Elon had to put \$100 million of his own money into SpaceX. There's little doubt that he would have spent everything he had if he needed to. It didn't come to that. But it goes to show that founders of these companies are, and maybe have to be, ready to go in all the way. They aren't relying on investors to get it.

Peter Thiel: NASA more or less required SpaceX to take outside funding back in 2008. Founders Fund invested then. Various VC firms in Silicon Valley warned expressed concern about this. They warned us that investing in SpaceX was risky and maybe even crazy. And this wasn't even at the very early stage—this was after the company had built rockets and attempted some launches.

Danielle Fong: People like to act like they like being disruptive and taking risks. But usually it's just an act. They don't mean it. Or if they do, they don't necessarily have the clout within the partnership to make it happen. So you have to find people who have opinions that dovetail with your mission.

Peter Thiel: It is very hard for investors to invest in things that are unique. The psychological struggle is hard to overstate. People gravitate to the modern portfolio approach. The narrative that people tell is that *their* portfolio will be a portfolio of different things. But that seems odd.

Things that are truly different are hard to evaluate. Suppose someone wants to start a rocket company. You might ask, quite reasonably, "What experience do you have with rockets?" The answer might be "zero." Elon didn't have any experience in making rockets before he started SpaceX. Or suppose a VC wants to invest in a rocket company. The question becomes: "What on earth do you know about rockets?" Again, the answer is probably "nothing." No one has invested in rockets in over 40 years.

iPhone games, by contrast, are entirely familiar. If you ask a gaming entrepreneur what experience he has with games, he'll tell you about all the games he's made before. Ask a VC what they know about games and they'll go on and on about the many gaming companies in their portfolio.

The upside to doing something that you're unfamiliar with, like rockets, is that it's likely that no one else is familiar with it, either. The competitive bar is lowered. You can focus on learning and substantive things over process, which is perhaps better than competing against experts.

Peter Thiel's CS183: Startup - Class 16 - Decoding Ourselves

He is an essay version of my class notes from Class 16 of CS183: Startup. Errors and omissions are mine. Thanks to [@1wu](#) for some supplementary notes!

Three guests joined the class for a conversation after Peter's remarks:

1. Brian Slingerland, Co-Founder, President & COO at Stem CentRx;
2. Balaji S. Srinivasan, CTO of Counsyl; and
3. Brian Frezza, Co-founder, Emerald Therapeutics

Credit for good stuff goes to them and Peter. I have tried to be accurate. But note that this is not a transcript of the conversation.

Class 16 Notes Essay—Decoding Ourselves

I. The Longevity Project

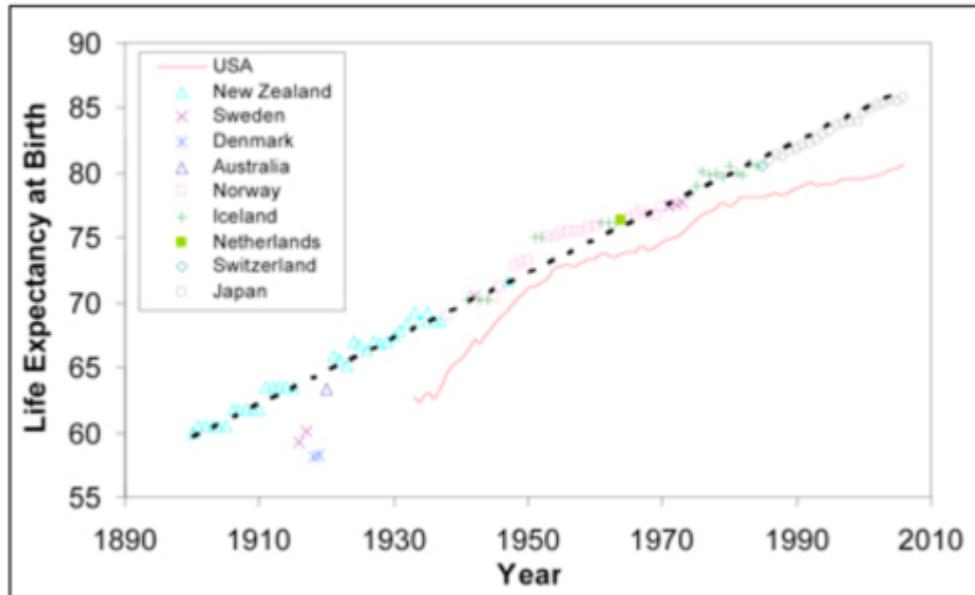
How much longer can people actually live? It's a very open ended question. It may not be very easy to answer at all. But there is a sense that biotech may be well positioned to try. Biotech, on the wake of the computer revolution, seems quite exciting if we think that a whole series of problems—e.g. cancer, aging, dying—is close to being solved.



Even without the biotech revolution, life expectancy has been rising impressively. The rate has been something like 2.5% decade over decade. In the mid to late 19th century, expected lifespans were going up at a rate of 2.3 to 2.5 years with each passing decade. If you plot the data points corresponding to each country's single demographic (typically women) with the longest life expectancy, you get a very straight line on a scattershot basis. This isn't quite equivalent

to Moore's law, but it's analogous. In 1840, life expectancy was just 45 or 46 years. For century and a half now, keeping people alive longer has been an exponentially harder problem.

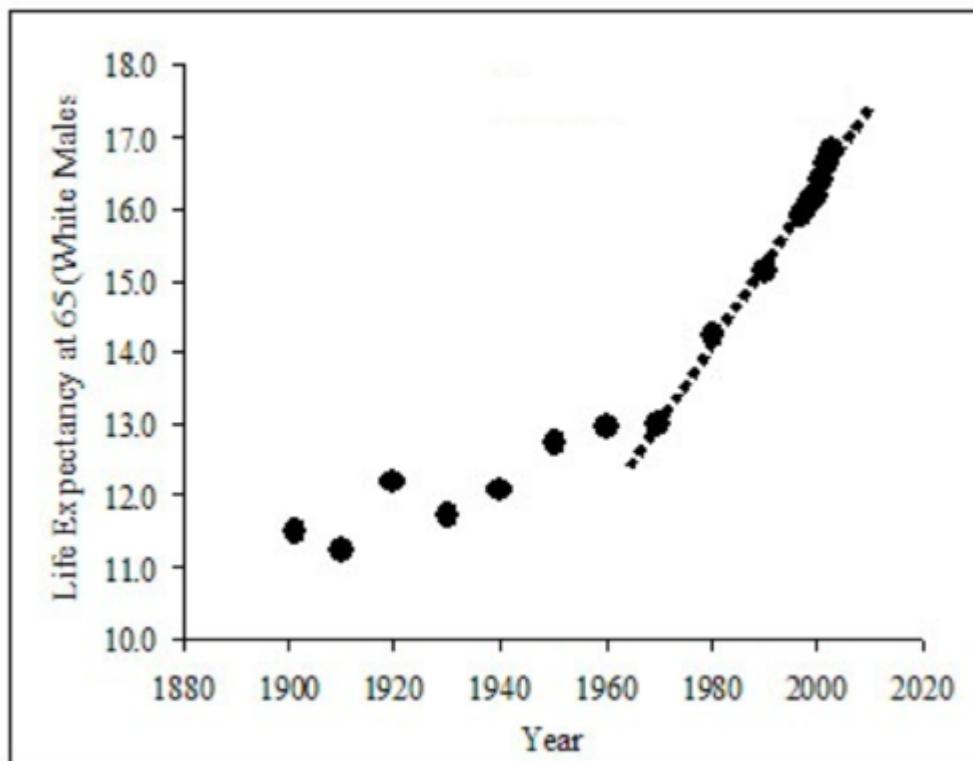
At birth



Top countries: 2.5 years/decade

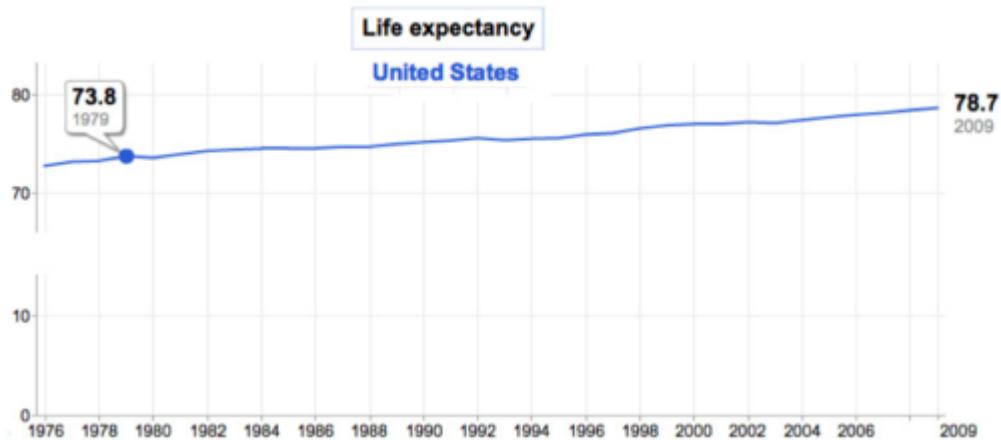
To some extent, the U.S. has fallen a bit behind in the effort. Life expectancy here is several years below the global max. There are all sorts of idiosyncratic explanations for this; Americans eat bad food, are too inactive, etc. But a little U.S. lag notwithstanding, there has been a relentless trend upwards.

At age 65



US: 40 days/year

Another way to think about it is this: every day you survive, you add 5 of 6 hours to your life. That is a startling realization. The question is what happens next. Is the straight line going to continue? Slow down? Accelerate? Before 1840, life expectancy was pretty flat for thousands of years. Only recently has it really picked up. Whether this is a short burst that will stagnate or just the beginning of a fierce acceleration remains to be seen.



II. Luck, Life, and Death

A. Death as Bad Luck

In a sense, longevity is the opposite of bad luck. At the broadest level, you get into trouble when something unlucky happens to you. Think of everything that can go wrong. Maybe a piece of your DNA mutates and starts a cancer. Maybe you get run over by a car. Or maybe you get hit by an asteroid. There are many different unlucky things that could happen. So the question of longevity can be rephrased as the question of whether and to what extent luck can be overcome.

From the 17th to the mid-19th centuries, the prevailing view was that we *could* overcome all these accidents. Francis Bacon's *New Atlantis* was the classic vision of an accident-free utopia. It was a *new* Atlantis because, unlike the old one that the Gods destroyed, new Atlanteans had complete mastery over nature.



This view has been receding since about 1850. Luck and indeterminacy have become increasingly dominant as frameworks for thinking about the future. This shift was probably driven by the emergence of actuarial science and life insurance. When people started to map out the data, they realized that life and death could be reduced to probability functions. A 30-year-old has a 1 in 1000 chance of dying in given year. But at age 100 that chance is 50%.

If we run with this math for a bit, living forever becomes just a matter of solving a simple equation:

$$p_x = \text{probability of dying in year } x$$

$$(1 - p_x) = \text{probability of not dying in year } x$$

$$\prod_{x=1}^n (1 - p_x) = \text{probability of still being alive at end of year } n$$

$$\prod_{x=1}^{\infty} (1 - p_x) = \text{probability of living forever}$$

The solution to mortality:

$$\prod_{x=1}^{\infty} (1 - p_x) > 0$$

Unchecked probabilistic thinking can be dangerous. It defeats one's ability to shape the future. No Country For Old Men captures this well; eventually, your luck runs out and you get shot in a deli in Texas. If everything is just a probability distribution, you have to resign to it. But that ignores your ability to think and avoid playing games that are too dependent on luck.

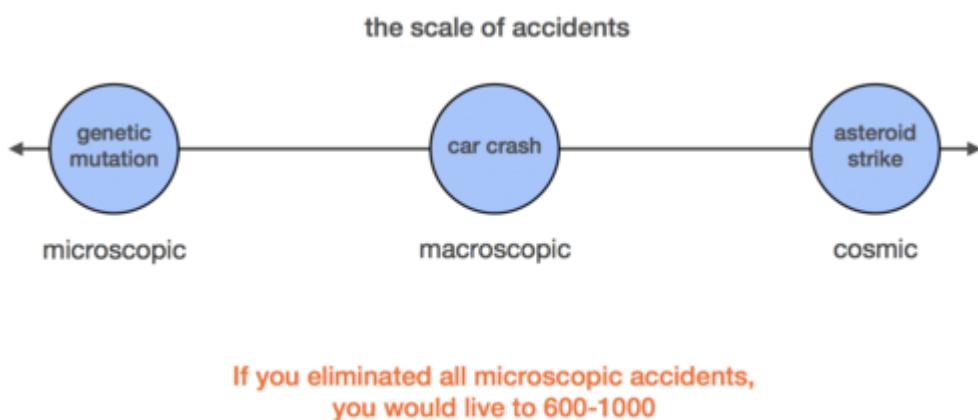
Random historical footnote: 1700, the claim that people could live forever seemed stronger than it would today, simply because there were people running around claiming to be 150 years old. Since record-keeping wasn't always great back then, good salespeople could persuade others that they were, in fact, radically old. Today, of course, it's easy to identify these longevity salesmen's movies. If you're 70 years old in early 18th century London, you're perceived as kind of wretched and you get no special treatment. But if you're 150 years old, that's really something special. You might even get a pension from the King.

B. Shift to Determinacy?

Can we move biology away from the realm of the statistical/probabilistic and toward being something that is determinate and solvable?

It depends.

You can think of death as an accident. There are different kinds of accidents. You can lay these out on a spectrum, from microscopic accidents (genetic mutations) to macroscopic accidents (car crashes) to cosmic accidents (asteroid strikes). To solve the longevity problem completely, you have to get rid of all of these kinds of accidents. But there's a sense in which certain macro and cosmic accidents are and will continue to be pretty probabilistic things. There is good reason to take those on later; if we can just get to the microscopic solution, the best estimates have people living to between 600 and 1,000 years.



III. CS and Biology

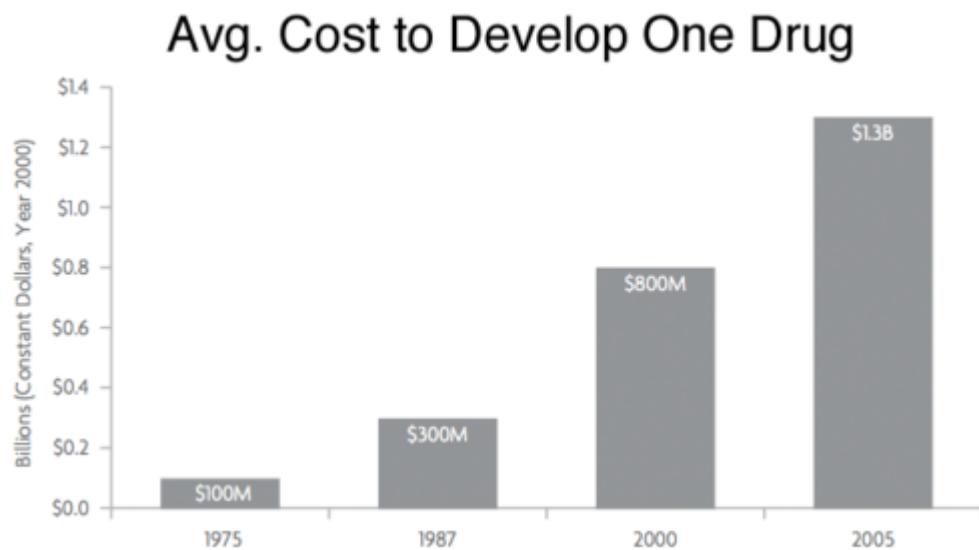
A. Difficulty of the Problem

Like death itself, modern drug discovery is probably too much a matter of luck. Scientists start with something like 10,000 different compounds. After an extensive screening process, those 10,000 are reduced to maybe 5 that might make it to Phase 3 testing. *Maybe* 1 makes it through testing and is approved by the FDA. It is an extremely long and fairly random process. This is why starting a biotech company is usually a brutal undertaking. Most last 10 to 15 years. There's little to no control along the way. What looks promising may not work. There's no iteration or sense of

progress. There is just a binary outcome at end of a largely stochastic process. You can work hard for 10 years and still not know if you've just wasted your time.

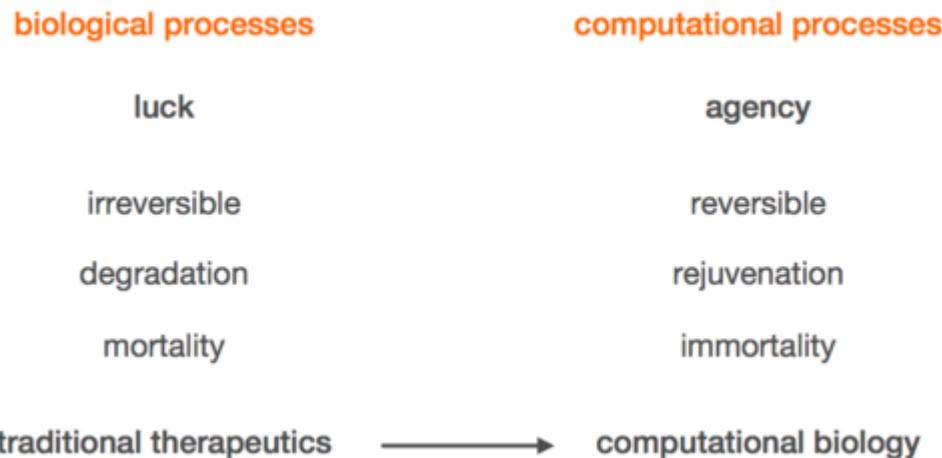
In Internet businesses, the basic rule is that the company succeeds if every round of financing is an up round. In biotech, it's very hard to do that. Investors get tired. Things don't work. Some biotech investors are so candid as to state that they don't really care about valuations, since everything will get wiped out in their favor once a company has the inevitable down round. Why negotiate valuation if luck dominates everything?

To be fair, we must acknowledge that all the luck-driven, stats-driven processes that have dominated people's thinking have worked pretty well over the last few decades. But that doesn't necessarily mean that indeterminacy is sound practice. Its costs may be rising quickly. Perhaps we've found everything that is easy to find. If so, it will be hard to improve armed with nothing but further random processes. This is reflected in escalating development costs. It cost \$100 million to develop a new drug in 1975. Today it costs \$1.3 billion. Probably all life sciences investment funds have lost money. Biotech investment has been roughly as bad as cleantech.



B. The Future of Biotech

Drug discovery is fundamentally a search problem. The search space is extremely big. There are lots of possible compounds. An important question is thus whether we can use computer technology to reduce scope of luck. Can CS make biotech more deterministic? At the most basic level, biological processes can be thought of as involving some quantum of luck in the face of irreversible degradation. Traditional therapeutics largely mirrors those processes. But computational processes are reversible. You can dive in and re-program things as necessary. So one big question is the extent to which biological problems can be reduced to computer problems?



The cost of DNA sequencing is falling rapidly. It cost \$500 million to sequence a genome in 2000. Now that's down to something like \$5,000. Within a year or two, it will probably cost \$1,000. The question is whether we can do as much as people have been assuming we can with all the information this will yield.

"[it will] revolutionize the diagnosis, prevention and treatment of most, if not all, human diseases."
-Bill Clinton, 2000

The Human Genome Project was seen as incredibly revolutionary in late 1990s. But it hasn't quite lived up to the hype. Perhaps it was all too early or too costly. But the second cut may be that it's because the main problem is not a sequencing problem at all. The biggest problem may be that we just don't know what to do with the data. Exactly how much of biology is computational is still an open question.



IV. Examples

We'll highlight and then have a discussion with people from 3 companies who are doing very interesting things in biotech: Stem CentRx, Counsyl, and Emerald Therapeutics.



Of these 3, Stem CentRx is the closest to traditional biotech. But there is still a heavy computational piece to it. The basic goal is to cure all cancer. Their claim is that cancers have stem cells that are very different from core cancer cells. This stem-cell subset drives cancer/tumor growth. So they aim to target the stem cells and thereby knock out the cancer.

Backing up a step, the problem is that chemotherapy can be really ineffective at threatening cancer. It is very hard to get chemo dosages exactly right. Too low a dosage is ineffective at stopping the cancer. Too high a dosage kills the patient along with the cancer. So if you can identify the subset of cancerous cells that are driving the growth and target those cells precisely, chemotherapy would be much less destructive and considerably more effective. So far, Stem CentRx's studies on mice have been very promising. We should find out whether the approach works for human cancers over the next year or two.

Counsyl is a bioinformatics company whose goal is to become the default for pregnancy genetic screening. They have developed single simple test for the 100 or so genes that can be screened for inherited traits. They focus just on the Mendelian diseases, since beyond that it is currently very hard to know how more complicated genetic combinations work. So Counsyl has identified a tractable and well-defined subset of the problem. Today, Counsyl is involved in screening about 2% of all births in the U.S., and expects that figure to rise quite dramatically in coming years.

Counsyl

Simple pre-pregnancy test for parents with Mendelian diseases

25% Unaffected 50% Carrier 25% Affected

The New York Times
Counsyl Brings Genetic Screening to the Masses

By JONATHAN C. STERNBERG | Published: March 12, 2012

BEDWICH CITY, Calif. — The new genetic “BabyScreen” test can tell parents on the day after a baby is born whether their child is a carrier of a trait or disease, a capability that could lead to a treatment for the non-genetic disease threatening to kill two of its children before they turn 10.

How a Billion-User Startup is Making the Gold-Standard Test Cheaper

Now, a billion-dollar startup is making the gold-standard test cheaper, helping revolutionize the way we screen for genetic diseases and more than once alerting parents to the fact that they have the same gene.

The company, Counsyl, is selling a test that it says can predict whether a person is at risk of having children with a range of inherited diseases, including cystic fibrosis, spinal muscular atrophy, sickle cell disease, Pompe disease (the one afflicting the children in

Reducing the cost of carrier testing by literally one hundred fold

Emerald Therapeutics is the most computational of these companies. The basic goal is to cure all viral infections by reprogramming cells, i.e. turning cells into code-based machines. The idea is to build a molecular machine that tags cells that contain viruses, and then to release a sequence that causes those cells to self-destruct. Emerald is in stealth mode, so we can't say too much. But the high degree of paranoia for companies doing programmable anti-viral therapies is understandable. These are big secrets that play out over long time horizons, not web apps that have a 6-week window to take over the world.

So with that, we'll have a discussion with Brian Slingerland of Stem CentRx, Balaji Srinivasan of Counsyl, and Brian Frezza of Emerald Therapeutics.

V. Perspectives

Peter Thiel: Marc Andreessen visited this class a few weeks ago. His claim about the Internet in the late '90s was that many of the ideas were right, but were just too early. Even if one agrees that next phase in biotech is about to start—things are going to get much more computational—how do you know that *now* is the right time? How do you know you're not paddling too early?

Balaji Srinivasan: The sequencing of the genome is like the first packets being sent over ARPANET. It's a proof of concept. This technology is happening, but it isn't yet compelling. So there is a huge market if one can make something compelling enough for people to actually go and get a genome sequenced. It's like e-mail or word processing. Initially these things were uncomfortable. But when they become demonstrably useful, people leave their comfort zones and adopt them. Pregnancy testing is a major on ramp. People find it important to make sure their children are as healthy as possible. And then there is likely to be tons of positive things that can be done with the data beyond that.

Peter Thiel: So the question is how you can overcome pervasive fear of getting genome sequencing? And the answer is: "Do it for the kids?"

Balaji Srinivasan: Yes. No one spends \$1000 to get computer so they can use Twitter. But once you have computer, there is zero marginal cost to use Twitter. So solving the install problem is the first step. Empirically, we're starting to see very strong adoption. So we are confident that we can solve the install problem.

Peter Thiel: Tackling the cancer problem is exciting but also worrisome at the same time. It's an old problem. Nixon said in 1970 that we'd win the War on Cancer by '76. People have been working on it for 40 years. So while we're 40 years closer to a solution, it also seems farther away than ever. Doesn't the fact that it's taken so long mean that it's an incredibly hard problem that won't be solved soon?

Brian Slingerland: People have largely followed the same path over the past 40 years. The usual approach to cancer is to carpet bomb it with chemotherapy or the like. The approaches that have been attempted are remarkably similar. So we decided to take really different path. 40 years of failures have taught us something important. The endpoint that everyone focuses on is therapeutic efficacy measured by tumor shrinkage. But this isn't the best metric; tumors can shrink and then come back. Focusing on shrinkage may lead to attacking the wrong cells. Embracing bioinformatics helps us illuminate better approaches. So we disagree that the problem won't be solved soon; we strongly believe that we have a very good chance of doing just that.

Brian Frezza: Half of the timing question is taken care of for us; we're certainly not too late, since viruses still exist. So are we too early? I don't think so. People's perspective on healthcare development is quite different from reality. Industry players tend to be very paranoid and secretive until they have a product to release. People discount that quite a bit, since they just pay attention to what is brought to market and when. What most people see at any given point was started decades before they even thought about it.

Biotech got quite a burst in late 70s early 80s, with new recombinant DNA and molecular biology techniques. Genentech led the way from the late 70s to the early 80s. Nine of the 10 biggest American biotech companies were founded during this really short time. Their technology came out some 7-8 years later. And that was the window; not very many integrated biotech companies have emerged since then. There was a certain amount of stuff to find. People found it. And before Genentech, the paradigm was pharma, not biotech. *That* window (becoming an integrated pharmaceutical company) had been closed for about 30 years before Genentech.

So the bet is that while the traditional biotech window may be closed, the comp bio window is just opening. Whoever gets in during that window gets installed. There are enormous monopoly barriers to getting to market. Here, first mover advantage often becomes last mover advantage. Imagine if IE or Chrome had to go through clinical trials just to get to market. It would be much harder to get in the game. So whoever manages to develop great technology and get it out first is in good shape.

Peter Thiel: Talk about your corporate strategy. Even if your technology works, how do you distribute it?

Balaji Srinivasan: If you think of drugs, biotech, and now genomics as qualitatively different entities, you'll see that genomics companies can do things quite differently. Genomics is much more computational than pharma or traditional biotech. With molecular diagnostics—but unlike traditional therapeutics—once you've assayed a sample, you're on info superhighway. Internet rules apply. You can go from conception to product and sales within 15-18 months. It's not quite as fast as Internet businesses. But it's considerably faster than the 7-8 years it takes in biotech. In the early '90s there was an opening for web 1.0. In the late '90s there was the web 2.0 window. Now it's genomics. We think that bio should just be sensors and gathering data. Everything else should be done at the command line.

Brian Slingerland: Stem CentRx has more of a traditional biotech process. We spent 3 years on the proof of concept phase. Now that we've finished all the efficacy studies in cancers in mice, we are in full-blown drug development mode. This process can be accelerated by adopting best practices from tech culture. It's like SpaceX; if the competition is so screwed up, you can radically improve things by cutting bureaucracy and taking on a Silicon Valley culture.

Brian Frezza: We are creating a platform, not an isolated product. We create infrastructure for all sorts of future antiviral technologies. So being able to handle the science in a routine and scalable way is key. The culture is an important part too. Even though we have PhD organic chemists and molecular biologists, we shoot for a hardcore tech startup culture. We automate processes in the lab using advanced robotics. We use git to track our lab notebooks. We write a ton of software. We are the first movers in our space, and we're trying to move very quickly, but we're also building a platform that's designed to scale exponentially.

Peter Thiel: How do you know that there isn't someone else secretly pursuing the same strategy? And if you're confident as to what other people are or aren't doing, how do you know that they don't know about you and that *your* secrecy is working?

Balaji Srinivasan: It's like the Rumsfeld quip: there are known unknowns. Ultimately we think most people miss the key secrets in health industry because they are so caught up in the status quo that they actually can't think their way to good solutions. Contrast the healthcare industry with the fitness industry. Ultimately, your fitness is your responsibility. You can join good gyms or get personal trainers. All that's great. But the buck stops with you—you have to take the initiative. But consider how that initiative plays out in healthcare. If you come to a doctor's appointment wanting to talk about something you've researched, doctors get pissed. You are either undermining their authority or you're an idiot. But that's odd; you are with your body for a lifetime, whereas the doctor is with you for 20 minutes each year. The one area of medicine that works—fitness—operates orthogonally to the rest of medicine in practice.

When these systems get build up, it's very hard to clear away the overhang. People have thought themselves out of thinking about non status quo solutions. Stuff that actually works is perceived as crazy.

Brian Frezza: One bad vestige of the biotech boom is what happened to patents. In pharma, traditionally compounds got patents. But in biotech, general techniques became patentable. Genentech, for example, managed to patent recombinant antibodies as a general concept . So biotech is littered with really broad patents. Some biotech

companies literally generate millions in revenue just from patent licensing; they produce no drugs at all. So it's best not to generate a large amount of public interest in new techniques you're developing if you don't want to encourage stray IP to accumulate.

Ultimately, you can't prove a negative. It is distinctly possible that there is a Ruby Therapeutics out there that is doing the same thing we are. But we very much doubt it, given how unique what we're doing is. Even knowing that they may be out there, it still makes sense for both companies to stay quiet until you're ready for revenue.

Brian Slingerland: There's really no rush to spill the secret plans. This space is very much unlike fast-moving consumer Internet startups. Here, if you have something unique, you should nurse it. One good rule of thumb is to issue no press releases until you push a drug. That said, it's a balancing act. Since our approach has been proven out and we'll be moving to human trials, we are becoming a more public-facing company. No one wants to take a drug made by a stealth company with no info on its website. You just want to make sure that you don't divulge too much too early.

Of course, people should assume there are 10 companies coming after them. It's always safe to assume that you have to work better and faster to come out ahead.

Peter Thiel: If you thought that a Ruby Therapeutics—or 10 different versions of them—was actually out there, wouldn't it make sense to be more open and collaborate? And how do you recruit people if you're so secretive?

Balaji Srinivasan: In ecology, when you want to know how many species are in a jungle, you take a sample and project out. Sizing up the competition is a similar task. If you take a hard look at your network—search through the Silicon Valley part of the forest—and see no capital being deployed and no one working on the same problems, you can be reasonably confident that you're alone. People would really have to come out of nowhere.

Personal referrals are very important for recruiting. We try and get each engineer to refer 2 people. 2^n scales very well. You get great people, but also get to stay under the radar.

Peter Thiel: That recruiting strategy has worked well in every company that I've been involved with. You have to keep a clear head about it. If you ask MBAs to refer talented people who are good to work with, you'll get far too many recruits. But if you put the same question to engineers—and maybe it's *their* friends who you really want to recruit—you may get a shocking silence because they are too shy. So you have to find a way to get them comfortable with referring people.

Brian Frezza: One strategy that works for that is to sit down with your engineers and go through their Facebook friends with them, one by one, and ask them who is good and who they'd like to work with.

Peter Thiel: The bias for Silicon Valley entrepreneurs is to go work on a web or mobile app. Why should more people think about doing biotech/computational stuff instead?

Balaji Srinivasan: The thing to remember is that the next big thing won't look like the last big thing. Search didn't look like the desktop. Social didn't look like search.

The human genome will never become obsolete. Mobile/local/social? It's hard to say. Mobile seems to have a lot of growth ahead of it. So that's at least a reasonable bet. Local? There's not really a defensible advantage anymore. And social has been colonized. Flags have been planted.

Ultimately you simply have to care about what you're doing. Another dating app really doesn't matter. It's hard to bleed/sweat/cry for. Meaningfulness is a big part of why people should think differently. And we think genomics is really meaningful.

Brian Frezza: Elon Musk is a master recruiter. The narrative is stark and simple:

"We don't pay as well as Google. But this is the most exiting project you can work on in your life." You want to attract the people who find that narrative attractive. People can always try to find a lottery ticket of a startup. But the satisfaction of creating a tech revolution is much bigger than what comes from just chasing dollars.

Brian Slingerland: One reason that CS people may be ignoring biotech is that they think that they'll be relegated to supporting roles. But that's far from true at many biotech companies. CS people are at the very core of what we do at Stem CentRx. So if CS people have an interest in curing cancer or things like that, it's certainly something they should think about. Have I mentioned that we're hiring?

Peter Thiel: We usually say that advertising works best if it is hidden. But sometimes it actually works if it's completely transparent. [laughter]

Being opaque can be so tiring. The standard wisdom in the VC world is: "If you want money, ask for advice. If you want advice, ask for money." That game is exhausting. Sometimes it can be refreshing to hear someone say, "I really just want money."

Question from the audience: [unintelligible]

Peter Thiel: I think this is basically just the regulatory question. So talk about regulatory risks and whatnot.

Brian Slingerland: The regulatory system is a necessary step at this point. There is very little upside for the FDA to approve drugs faster. And they get in lots of trouble if they approve things too quickly and a trial goes wrong. There is a lot of talk about doing trials in China or India right now. These expedited processes are certainly interesting. Perhaps people should think about them more. And how the FDA responds will be interesting as well.

Peter Thiel: It is very odd that the FDA has a bottleneck on *global* drug development. There has to be some tipping point beyond which the U.S. no longer gets to dictate what drugs are developed in the entire world. Past that inflection point, the U.S. may have to compete with China on how quickly drugs can be developed. That could be a huge paradigm shift. So while things look pretty bad now, the future may be quite promising. SpaceX was very heavily regulated at first, but persevered and got through it. And the aero regulations have eased up in the last decade. So the sheer unfriendliness of the baseline could be a great opportunity.

Question from the audience: When you disclose your secret to prospective hires, do they try to use that knowledge as leverage to hold you up and negotiate more?

Brian Frezza: It hasn't been a problem at all. VCs don't sign NDAs, but job candidates will. And it would take years for anyone we talk with to replicate our technology on their own.

Question from the audience: What role does HIPPA play in tech innovation?

Balaji Srinivasan: HIPAA could be seen as tech problem. How can we follow such and such standards, etc.

But it's also an interesting genomics problem. A person's genome provides a great deal of information about their relatives. On one hand, this data is private medical data. On the other hand, it's inherently statistical and requires aggregation to do anything very useful with it. So the trick is to figure out how to do private aggregations. To get value out of your genome, you simply must allow some computation on it. The challenge is catalyzing this very important social shift toward becoming okay with that while preserving strong privacy controls.

Question from the audience: Unlike the web, where you can get feedback in minutes, how do you know if you're on the right track in computational biotech?

Brian Frezza: We use physical models and actual validation experiments. We don't just use statistical approaches. But our processes are internal. We don't go outside and seek external validation. Just like Instagram doesn't get outside people to come in and appraise its code base. You develop a plan and execute it internally. Sometimes you have a multi-year cycle to get data back. You just work as efficiently as possible to shorten cycle times.

Balaji Srinivasan: Slow iteration is not law of nature. Pharma and biotech usually move very slowly, but both have moved pretty fast at times. From 1920-1923 Insulin moved at the speed of software. Today, platforms like Heroku have greatly reduced iteration times. The question is whether we can do that for biotech. Nowhere is it written in stone that you can't go from conception to market in 18 months.

Brian Frezza: That depends very much on what you're doing. Genentech was founded the same year as Apple was, in 1976. Building a platform and building infrastructure take time. There can be lots of overhead. Ancillary things can take longer than a single product lifecycle to accumulate.

Question from the audience: How does biotech VC compare to regular VC?

Brian Slingerland: We never did the classic venture capital route because VC is broken with respect biotech. Biotech VCs have all lost money. They usually have time horizons that are far too short. VCs that say they want biotech tend to really want products brought to market extremely quickly.

Brian Frezza: "Integrated drug platform" is an ominous phrase for VCs. More biotech VCs are focused on globalization than on real technical innovation. VCs typically found a company around a single compound and then pour a bunch of money into it to push it through the capital-intensive trial process. Most VCs not interested in multi-compound companies doing serious pre-clinical research.

Question from the audience: How useful are end-stage trials in trying to figure out how to cure cancer? Don't you get inaccurate or just different genome data from terminal patients?

Brian Slingerland: Not being able to trial on earlier stage people is always a challenge. But our technology it is designed to apply to patients at all stages. All I can say is that our approach is stage agnostic for a variety of technical reasons. But generally speaking yours is a valid concern. That's why traditional drugs that show initial progress often fizzle out in extended trials.

Question from the audience: What were some of your early struggles or challenges?

Brian Frezza: The amount of time it took to set up a lab was shockingly large. 100% of our time went into acquiring equipment, negotiating price, dealing with initialization failures, etc. We greatly underestimated the time required to

get up and running because we were coming out of existing, well-supplied labs. It basically takes a whole year to get up and running. There's just a huge difference from the computer/Internet tech world.

Peter Thiel: With Internet businesses, you can be up and running without doing hardly anything. At PayPal, the biggest interface with reality was that, on Max's orders, people had to assemble their own desks. But Luke Nosek thought even that was too much. So he found a company called Delegate Everything, who dispatched this elderly woman handyperson out to assemble the desk for him so that he could do more work on the computer.

Balaji Srinivasan: Startups are always hard at the start. There are futons and ironing boards in the office. You have to rush to clean up for meetings. But maybe the hardest thing is just to get your foundation right and make sure you plan to build something valuable. You don't have to do a science fair project at the start. You just have to do your analytical homework and make sure what you're doing is valid. You have to give yourself the best chance of success as things unfold in the future.

Peter Thiel's CS183: Startup - Class 17 - Deep Thought

He is an essay version of class notes from Class 17 of CS183: Startup. Errors and omissions are mine.

Three guests joined the class for a conversation after Peter's remarks:

1. D. Scott Brown, co-founder of Vicarious Systems
2. Eric Jonas, CEO of Prior Knowledge
3. Bob McGrew, Director of Engineering at Palantir

Credit for good stuff goes to them and Peter. I have tried to be accurate. But note that this is not a transcript of the conversation.

Class 17 Notes Essay—Deep Thought

I. The Hugeness of AI

On the surface, we tend to think of people as a very diverse set. People have a wide range of different abilities, interests, characteristics, and intelligence. Some people are good, while others are bad. It really varies.



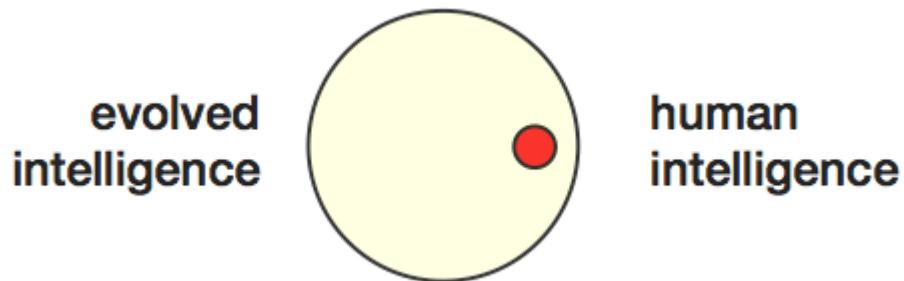
By contrast, we tend to view computers as being very alike. All computers are more or less the same black box. One way of thinking about the range of possible artificial intelligences is to reverse this standard framework. Arguably it should be the other way around; there is a much *larger* range of potential AI than there is a range of different people.



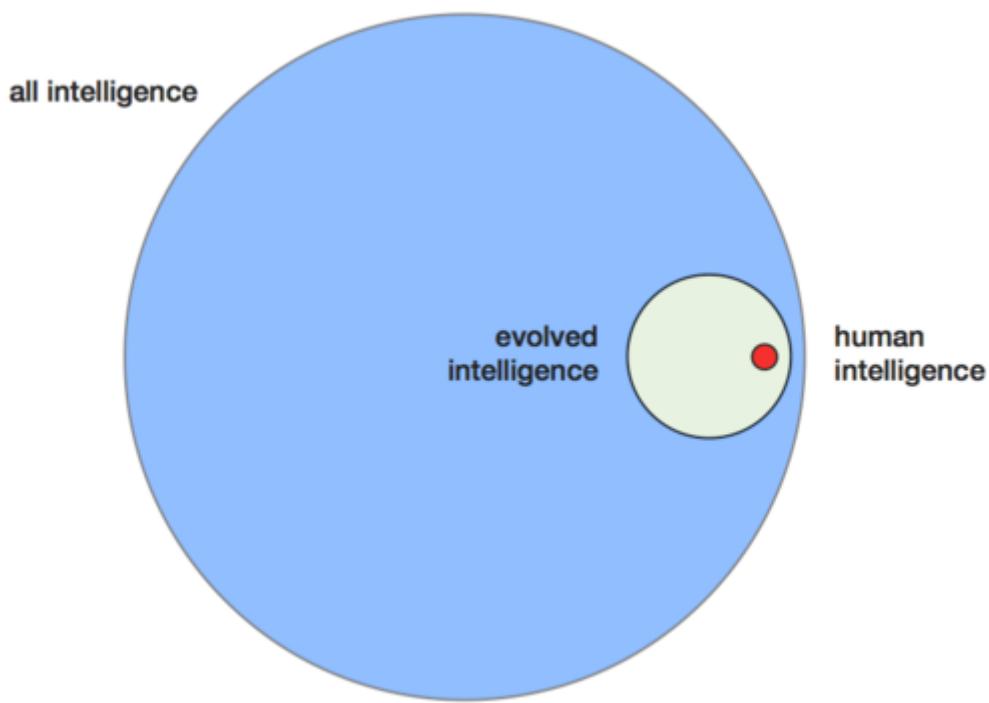
There is a great many ways that intelligence can be described and organized. Not all involve human intelligence. Even accounting for the vast diversity among all different people, human intelligence is probably only a tiny dot



relative to all evolved forms of intelligence; imagine all the aliens in all planets of the universe that might or could exist.



But AI has much larger range than all naturally possible things. AI is not limited to evolution; it can involve things that are built. Evolution produces birds and flight. But evolution cannot produce supersonic birds with titanium wings. The straightforward process of natural selection involves gradual iteration in ecosystems. AI is not similarly limited. The range of potential AI is thus much larger than the range of alien intelligence, which in turn is broader than the range of human intelligence.



So AI is a very large space—so large that people's normal intuitions about its size are often off base by orders of magnitude.

One of the big questions in AI is exactly how smart it can possibly get. Imagine an intelligence spectrum with 3 data points: a mouse, a moron, and Einstein. Where would AI fall on that scale?



We tend to think of AI as being marginally smarter than an Einstein. But it is not a priori clear why the scale can't actually go up much, much higher than that. The bias is toward conceiving of things that are fathomable. But why is that more realistic than a superhuman intelligence so smart that it's hard to fathom? It might be easier for a mouse to understand the relativity than it is for us to actually understand how an AI supercomputer thinks.

A future with artificial intelligence would be so unrecognizable that it would unlike any other future. A biotech future would involve people functioning better, but still in recognizably human way. A retrofuture would involve things that have been tried before and resurrected. But AI has the possibility of being radically different and radically strange.

There is a weird set of theological parallels you could map out. God may have been to the Middle Ages what AI will become to us. Will the AI *be* god? Will it be all-powerful? Will it love us? These seem like incomprehensible questions. But they may still be worth asking.

II. The Strangeness of AI

The Turing test is the classic, decades-old test for AI that asks whether you can build a machine that behaves as intelligently as a human does. It focuses on the subset of human behavior that is intelligent. Recently the popular concern has shifted from intelligent computers to empathetic computers. People today seem more interested in whether computers can understand our feelings than whether they are actually smart. It doesn't matter how intelligent it is in more classic domains; if the computer does not find human eye movement emotionally provocative, it is, like Vulcans, still somehow inferior to people.



The history of technology is largely a history of technology displacing people. The plow, the printing press, the cotton gin all put people out of business. Machines were developed to do things more efficiently. But while displacing people is bad, there's the countervailing sense that these machines are good. The fundamental question is whether AI actually replaces people or not. The effect of displacement is the strange, almost political question that seems inextricably linked with the future of AI.

There are two basic paradigms. The Luddite paradigm is that machines are bad, and you should destroy them before they destroy you. This looks something like textile workers destroying factory cotton mills, lest the machines take over the cotton processing. The Ricardo paradigm, by contrast, holds that technology is fundamentally good. This is economist David Ricardo's gains from trade insight; while technology displaces people, it also frees them up to do more.

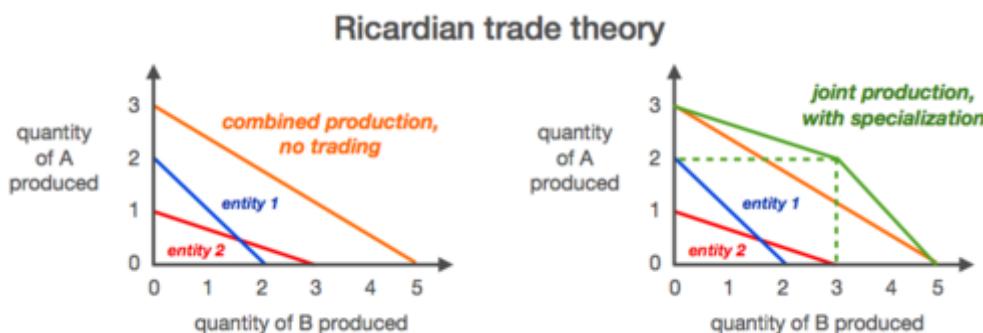


Luddites



David Ricardo

Ricardian trade theory would say that if China can make cheaper cars than can be made in the U.S., it is good for us to buy cars from China. Yes, some people in Detroit lose their jobs. But they can be retrained. And local disturbances notwithstanding, total value can be maximized.

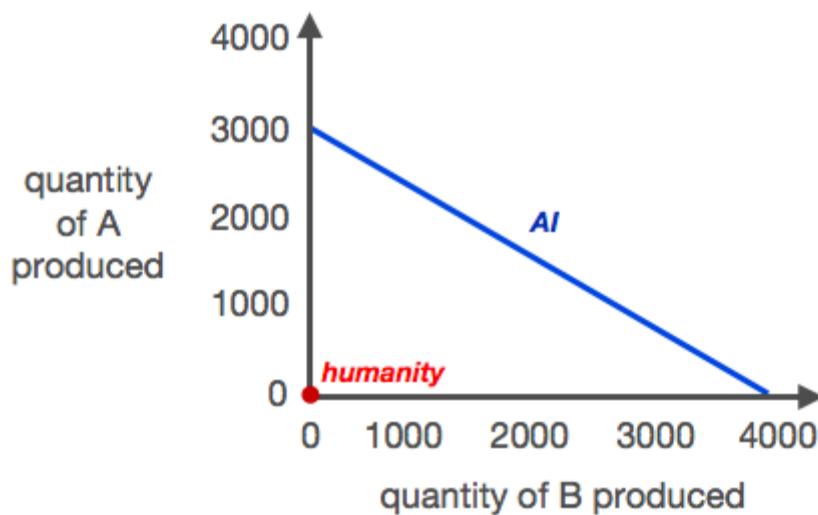


The charts above illustrate the basic theory. With no trade, you get less production. With joint production and specialization, you expand the frontier. More value is created. This trade framework is one way to think about technology. Some cotton artisans lose their jobs. But the price of shirts from the cotton factory falls quite a bit. So the artisans who find other jobs are now doing something more efficient and can afford more clothes at the same time.

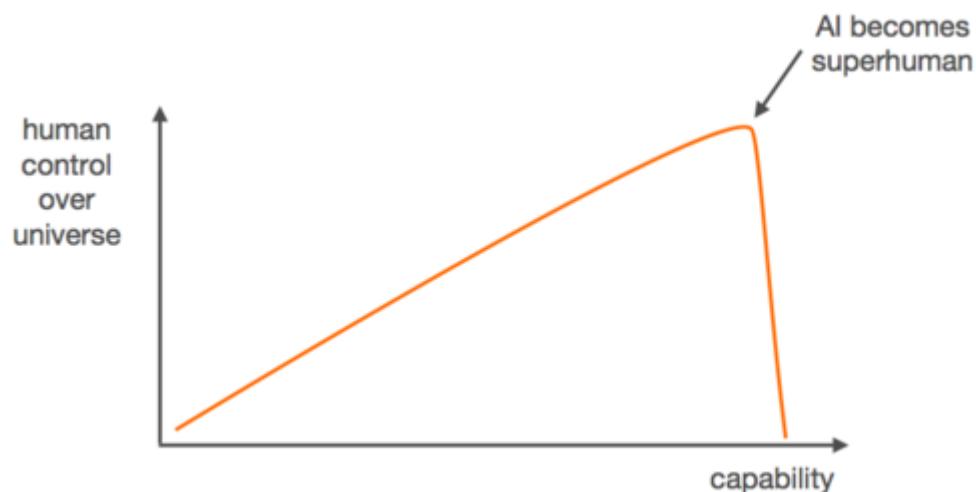
The question is whether AI ends up being just another version of something you trade with. That would be strait Ricardo. There's a natural division of labor. Humans are good at some things. Computers are good at other things. Since they are each quite different from each other, the expected gains from trade are large. So they trade and realize those gains. In this scenario AI is not substitute for humans, but rather a compliment to them.

But this depends on the relative magnitudes of advantage. The above scenario plays out if the AI is marginally better. But things may be different if the AI is in fact dramatically better. What if it can do 3000x what humans can do across everything? Would it even make sense for the AI to trade with us at all? Humans, after all, don't trade with monkeys or mice. So even though the Ricardo theory is sound economic intuition, in extreme cases there may be something to be said for the Luddite perspective.

What happens when AI eclipses us?



This can be reframed as a battle over control. How much control do humans have over the universe? As AI becomes stronger, we get more and more control. But then AI hits an inflection point where it goes superhuman, and we lose control altogether. That is qualitatively different from most technology, which gives people more control over the world with no end. There is no cliff with most technology. So while computers can give us a great deal of control, and help us overcome chance and uncertainty, it may be possible to go too far. We may end up creating a supercomputer in the cloud that calls itself Zeus and throws down lightning bolts at people.



III. The Opportunity of AI

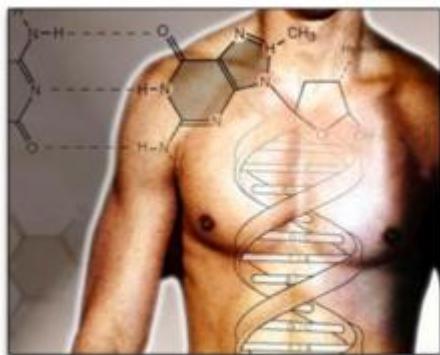
Hugeness and strangeness are interesting questions. But whether and how one can make money with AI may be even more interesting. So how big is the AI opportunity?

A. Is It Too Early for AI?

Everything we've talked about in class remains important. The timing question is particularly important here. It might still be too early for AI. There's a reasonable case to be made there. We know that futures fail quite often. Supersonic

airplanes of the '70s failed; they were too noisy and people complained. Handheld iPad-like devices from the '90s and smart phones from '99 failed. Siri is probably still a bit too early today. So whether the timing is right for AI is very hard to know *ex ante*.

But we can try to make the case for AI by comparing it to things like biotech. If you had a choice between doing AI and the biotech 2.0 stuff we covered last class, the conventional view would be that the biotech angle is the right one to pick. Arguably the bioinformatics revolution is being or will soon be applied to humans, whereas actual application of AI is much further out. But the conventional view isn't always right.



biotechnology



artificial intelligence

B. Unanimity and Skepticism

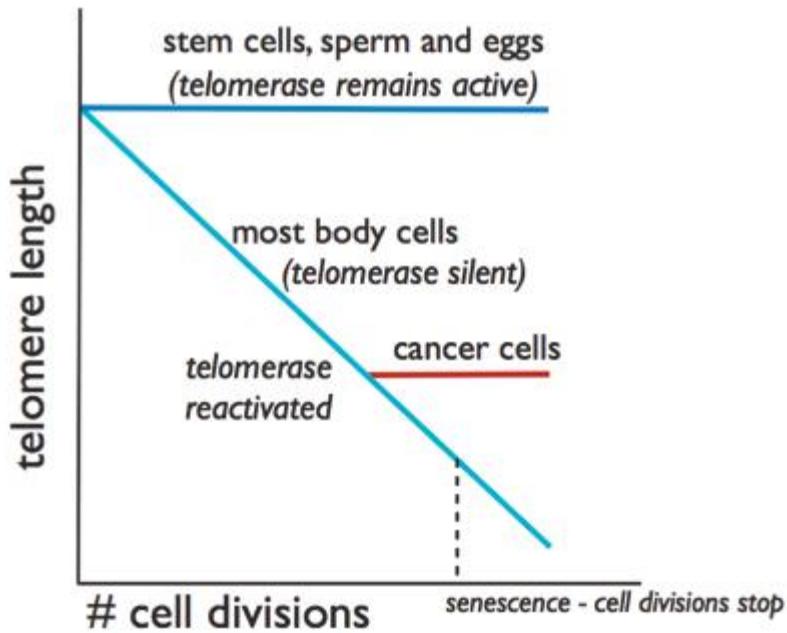
Last week in Santa Clara there was an event called "5 Top VCs, 10 Tech Trends." Each VC on the panel made 2 predictions about technology in the next 5 years. The audience voted on whether they agreed with each prediction. One of my predictions was that biology would become an information science. When the audience voted, it was a sea of green. 100% agreed with that prediction. There wasn't a single dissenter. Perhaps that should make us nervous. Unanimity in crowds can be very disconcerting. Maybe it's worth questioning the biotech-as-info-science thesis a little bit more.

The single idea that people thought was the worst was that all cars would go electric. 92% of the audience voted against that happening. There are many reasons to be bearish on electric cars. But now there is one less.

The closest thing to AI that was discussed was whether Moore's law would continue to accelerate. The audience was split 50-50 on that. If it can accelerate—if it can more than double every 18th months going forward—it would seem like you'd get something like AI in just a few years. Yet most people thought AI was much further away than biotech 2.0.

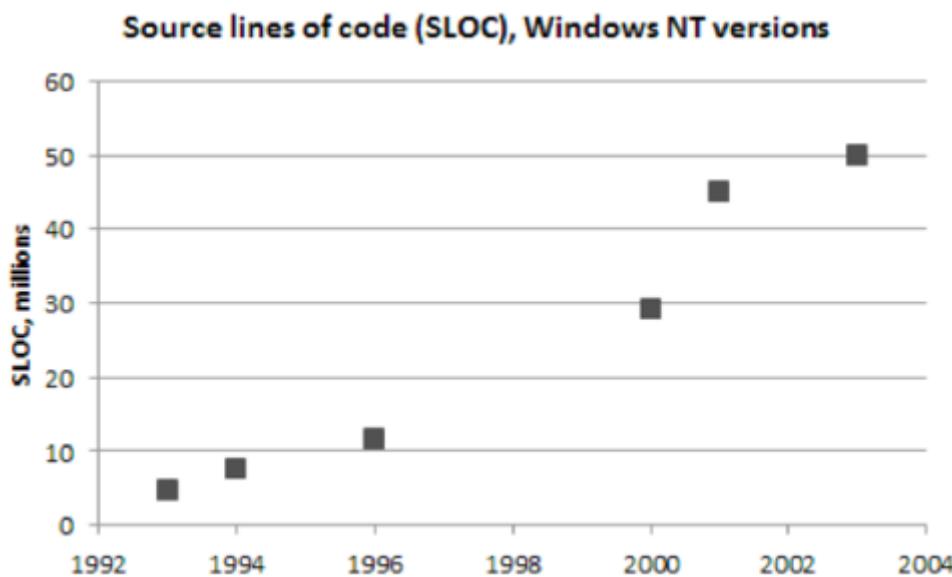
C. (Hidden) Limits

One way to compare biotech and AI is to think about whether there are serious—and maybe even hidden—limits in each one. The biotech revolution narrative is that we're going to figure out how to reverse and cure all sorts of maladies, so if you just live to x , you can stay alive forever. It's a good narrative. But it's also plausible that there are invisible barriers lurking beneath the surface. It's possible, for example, that various systems in the human body act against one another to reach equilibrium. Telomerase helps cells split unbounded. This is important because you stop growing and start to age when cells don't split. So one line of thinking is that you should drink red wine and do whatever else you can to keep telomerase going.

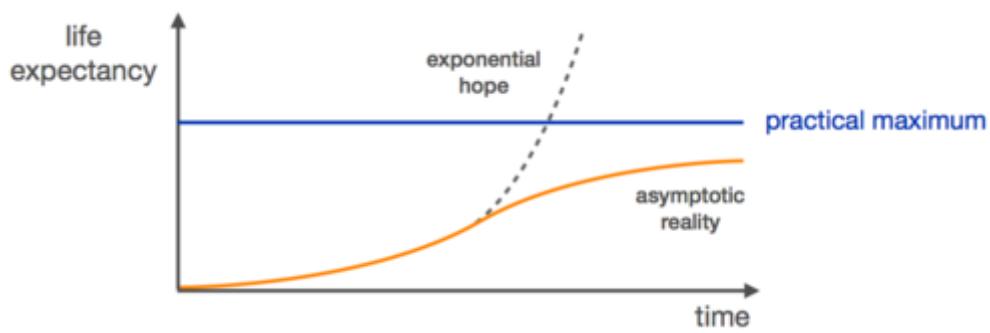


The challenge is that unbounded cell splitting starts to look a lot like cancer at some point. So it's possible that aging and cancer have the effect of cancelling each other out. If people didn't age, they would just die of cancer. But if you shut down telomerase sooner, you just age faster. Fix one problem and you create another. It's not clear what the right balance is, whether such barriers can be overcome, or, really, whether these barriers even exist.

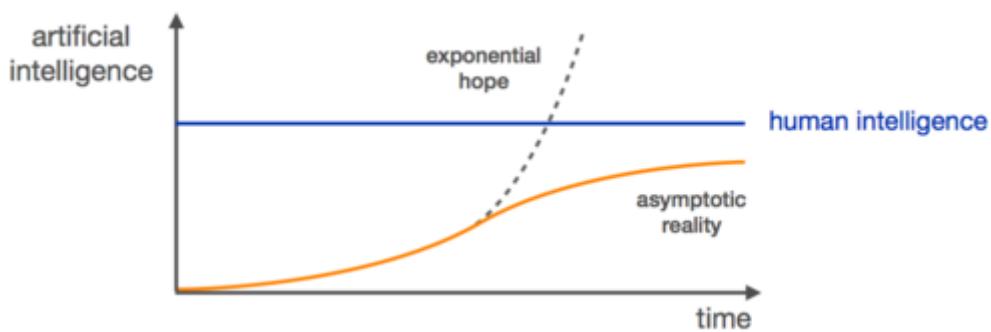
A leading candidate for an invisible barrier in AI is the complexity of the code. There might be some limit where the software becomes too complicated as you produce more and more lines of code. Past a certain point, there is so much to keep track of that no one knows what's going on. Debugging becomes difficult or impossible. Something like this could be said to have happened to Microsoft Windows over a number of decades. It used to be elegant. Maybe it has been or can be improved a bit. But maybe there are serious hidden limits to it. In theory, you add more lines of code to make things better. But maybe they will just make things worse.



The fundamental tension is exponential hope versus asymptotic reality. The optimistic view is the exponential case. We can argue for that, but it's sort of unknown. The question is whether and when asymptotic reality sets in.



and the AI version:



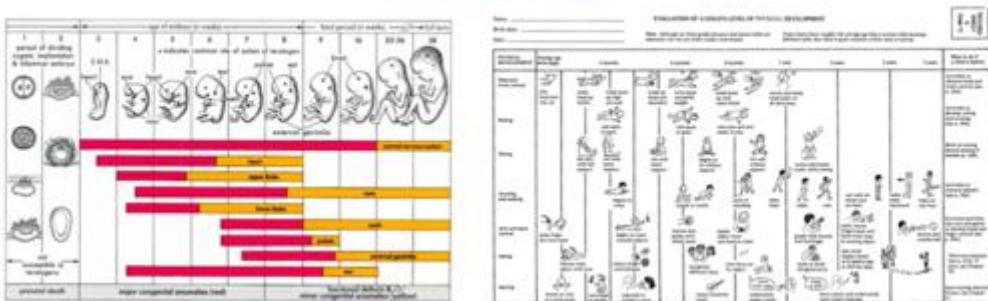
D. AI Pulls Ahead

There are many parallels between doing new things in biotech and AI. But there are three distinct advantages to focusing on AI:

1. Engineering freedom
2. Regulatory freedom
3. Underexplored (contrarian)

Engineering freedom has to do with the fact that biotech and AI are fundamentally very different. Biology developed in nature. Sometimes people describe biological processes as blueprints. But it's much more accurate to describe them as a recipe. Biology is a set of instructions. You add food and water and bake for 9 months. There is a whole series of constructions like this. If the cake turns out to have gotten messed up, it's very hard to know how to fix it simply by looking at the cookbook.

biotech: a recipe



This isn't a perfect analogy. But directionally, AI is much more of a true blueprint. Unlike recipe-based biotech, AI is much less dependent on a precise sequence of steps. You have more engineering freedom to tackle things in different ways. There is much less freedom in changing a biological recipe than there is in designing a blueprint from scratch.

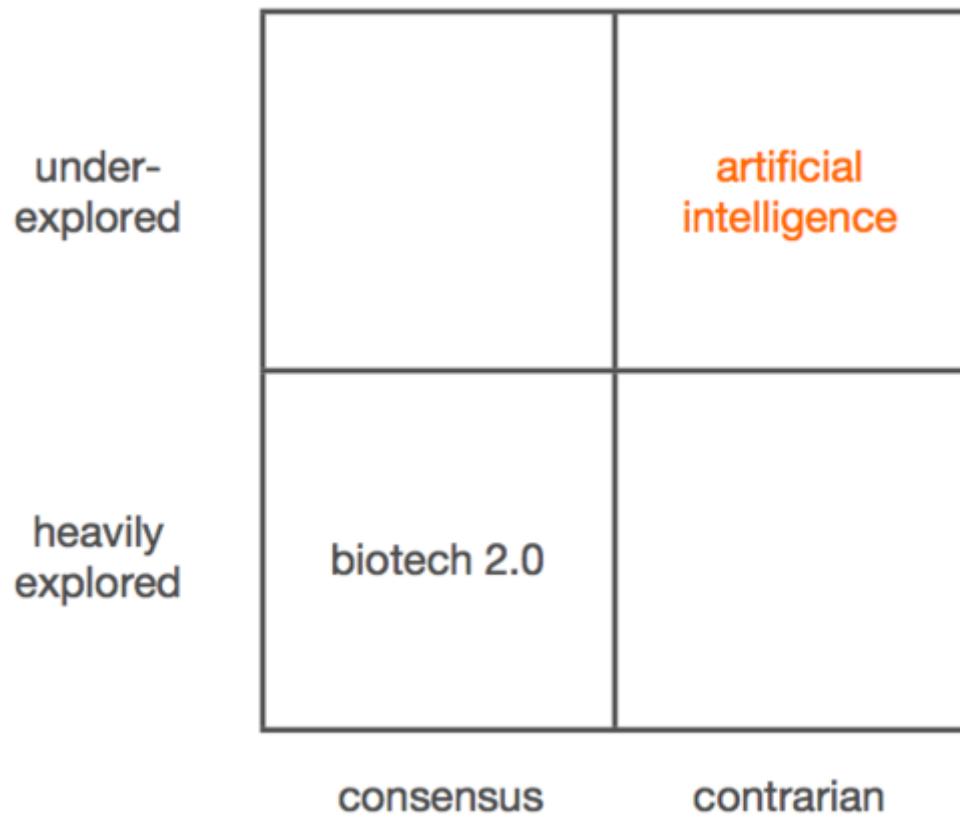
AI: a blueprint



On the regulatory side, the radical difference is that biotech is very heavily regulated. It takes 10 years and costs \$1.3 billion to develop a new drug. There are lots of precautionary principles at work. There are 4,000 people at the FDA.

AI, by contrast, is an unregulated frontier. You can launch just as quickly as you can build software. It might cost you \$1 million, or millions. But it won't cost \$1 billion. You can work from your basement. If you try to synthesize Ebola or smallpox in your basement, you could get in all sorts of trouble. But if you just want to hack away at AI in your basement, that's cool. Nobody will come after you. Maybe it's just that politicians and bureaucrats are weird and have no imagination. Maybe the legislature simply has no mind for AI-kind of things. Whatever the reason, you're free to work on it.

AI is also underexplored relative to biotech. Picture a 2x2 matrix; on one axis you have underexplored vs. heavily explored. On the other you have consensus vs. contrarian. Biotech 2.0 would fall in the heavily explored, consensus quadrant, which, of course, is the worst quadrant. It is the new thing. The audience in Santa Clara last week was 100% bullish on it. AI, by contrast, falls in the underexplored, contrarian quadrant. People have been talking about AI for decades. It hasn't happened yet. Many people have thus become quite pessimistic about it, and have shifted focus. That could be very good for people who do want to focus on AI.



PayPal, at Luke Nosek's urging, became the first company in the history of the world that had cryogenics as part of the employee benefits package. There was a Tupperware-style party where the cryogenics company representatives made the rounds trying to get people to sign up at \$50k for neuro or \$120k for full body. Things were going well until they couldn't print out the policies because they couldn't get their dot matrix printer to work. So maybe the way to get biotech to work well is actually to push harder on the AI front.

IV. Tackling AI

We have people from three different companies that are doing AI-related things here to talk with us today. Two of these companies—Vicarious Systems and Prior Knowledge—are pretty early stage. The third, Palantir, is a bit later.

Vicarious Systems is trying to build AI by developing algorithms that use the underlying principles of the human brain. They believe that higher-level concepts are derived from grounded experiences in the world, and thus creating AI requires first solving a human sensory modality. So their first step is building a vision system that understands images like humans do. That alone would have various commercial applications—e.g. image search, robotics, medical diagnostics—but the long-term plan is to go beyond vision and build generally intelligent machines.

Strategy for AI

1 Look to the brain.

Given only the sensory data your brain received since birth, any AI must, as a minimum, develop the same capabilities.

2 Start with vision.

Language and higher level concepts depend on solving at least one sensory domain. Vision is also easier to debug and commercialize than other senses.

3 Don't commercialize too early.

Most AI companies try to build products before they solve the core algorithmic challenges of intelligence.

Prior Knowledge is taking a different approach to building AI. Their goal is less to emulate brain function and more to try to come up with different ways to process large amounts of data. They apply a variety of Bayesian probabilistic techniques to identifying patterns and ascertaining causation in large data sets. In a sense, it's the opposite of simulating human brains; intelligent machines should process massive amounts of data in advanced mathematical ways that are quite different from how most people analyze things in everyday life.

Prior Knowledge invented a box that finds causes in data



How?

Everyone knows Bayes' rule

- Universal mathematical rule for learning the causes of data
- Widely held to be intractable for real problems

PK makes it work

- Deep tech lets it scale larger than anyone believes
- Mathematical simplicity makes the *interface* simple too
- A simple interface puts the tech *everywhere*

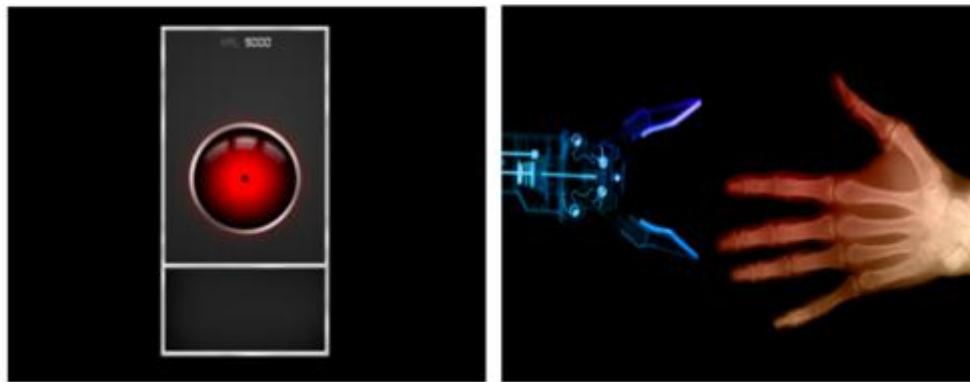
(Know Python or Ruby? Try it for yourself. <http://dev.priorknowledge.com>)

The big insight at Palantir is that the best way to stop terrorists isn't regression analysis, where you look at what they've done in the past to try to predict what they're going to do next. A better approach is more game theoretic. Palantir's framework is not fundamentally about AI, but rather about intelligence *augmentation*. It falls very squarely within the Ricardo gains from trade paradigm. The key is to find the right balance between human and computer. This is a very similar to the anti-fraud techniques that PayPal developed. Humans couldn't solve the fraud problem because there were millions of transactions going on. Computers couldn't solve the problem because the fraud patterns changed. But having the computer do the hardcore computation and the humans do the final analysis, while a weaker form of AI, turns out to be optimal in these cases.



Stop thinking
Artificial Intelligence

Start thinking
Intelligence Augmentation



So let's talk with D. Scott Brown from Vicarious Systems, Eric Jonas from Prior Knowledge, and Bob McGrew from Palantir.

V. Perspectives

Peter Thiel: The obvious question for Vicarious and Prior Knowledge is: why is *now* the time to be doing strong AI as opposed to 10-15 years from now?

Eric Jonas: Traditionally, there hasn't been a real need for strong AI. Now there is. We now we have tons more data than we've ever had before. So first, from a practical perspective, all this data demands that we do something with it. Second, AWS means that you no longer need to build your own server farms to chew through terabytes of data. So we think that a confluence of need and computing availability makes Bayesian data crunching make sense.

Scott Brown: If current trajectories hold, in 14 years the world's fastest supercomputer will do more operations per second than the number of neurons in the brains of all living people. What will we do with all that power? We don't really know. So perhaps people should spend the next 13 years figuring out what algorithms to run. A supercomputer the size of the moon doesn't do any good on its own. It can't be intelligent if it's not doing anything. So one answer to the timing question is simply that we can see where things are going and we have the time to work on them now. The inevitability of computational power is a big driver. Also, very few people are working on strong AI. For the most part, academics aren't because their incentive structure is so weird. They have perverse incentive to make only marginally better things. And most private companies aren't working on it because they're trying to make money now. There aren't many people who want to do a 10-year Manhattan project for strong AI, where the only incentives are to have measurable milestones between today and when computers can think.

Peter Thiel: Why do you think that human brain emulation is the right approach?

Scott Brown: To clarify, we're not really doing emulation. If you're building an airplane, you can't succeed by making a thing that has feathers and poops. Rather, you look at principles of flight. You study wings, aerodynamics, lift, etc., and you build something that reflects those principles. Similarly, we look at the principles of the human brain. There are hierarchies, sparsely distributed representations, etc.—all kinds of things that represent constraints in the search space. And we build systems that incorporate those elements.

Peter Thiel: Without trying to start a fistfight, we'll ask Bob: Why is the correct intelligence *augmentation*, not strong AI?

Bob McGrew: Most successes in AI haven't been things that pass Turing tests. They've been solutions to discrete problems. The self-driving car, for instance, is really cool. But it's not generally intelligent. Other successes, in things like translation or image processing, have involved enabling people to specify increasingly complex models for the world and then having computers optimize them. In other words, the big successes have all come from gains from trade. People are better than computers at some things, and vice versa.

Intelligence augmentation works because it focuses on *conceptual understanding*. If there is no existing model for a problem, you have to come up with a concept. Computers are really bad at that. It'd be a terrible idea to build an AI that just finds terrorists. You'd have to make a machine think like a terrorist. We're probably 20 years away from that. But computers *are* good at data processing and pattern matching. And people are good at developing conceptual understandings. Put those pieces together and you get the augmentation approach, where gains from trade let you solve problems vertical by vertical.

Peter Thiel: How do you think about the time horizon for strong AI? Being 5-7 years away from getting there is one thing. But 15-20 years or beyond is quite another.

Eric Jonas: It's tricky. Finding the right balance between company and research endeavor isn't always straightforward. But our goal is simply to build machines that find things in data that humans can't find. It's a 5-year goal. There are compounding returns if we build these Bayesian systems so that they fit together. The Linux kernel is 30 million lines of code. But people can build an android app on top of that without messing with those 30 million lines. So we're focusing on making sure that what we're building now can be useable for the big problems that people will tackle 15 years from now.

Peter Thiel: AI is very different from most Silicon Valley companies doing web or mobile apps. Since engineers seem to gravitate toward those kind of startups, how do you go about recruiting?

Scott Brown: We ask people what they care about. Most people want to make an impact. They may not know what the best way to do it is, but they want to do it. So we point out that it's hard to do something more important than building strong AI. Then, if they're pretty interested, we ask them how they conceive of strong AI. What incremental test would something have to pass in order to be a stepping stone towards AI? They come up with a few tests. And then we compare their standards to our roadmap and what we've already completed. From there, it becomes very clear that Vicarious is where you should be if you're serious about building intelligent machines.

Question from the audience: Even if you succeed, what happens after you develop AI? What's your protection from competition?

Scott Brown: Part of it is about process. What enabled the Wright brothers to build the airplane wasn't some secret formula that they came up with all of a sudden. It was rigorous adherence to doing carefully controlled experiments. They started small and built a kite. They figured out kite mechanics. Then they moved onto engineless gliders. And once they understood control mechanisms, they moved on. At the end of the process, they had a thing that flies. So the key is understanding why each piece is necessary at each stage, and then ultimately, how they fit together. Since the quality comes from process behind the outcome, the outcome will be hard to duplicate. Copying the Wright brothers' kite or our vision system doesn't tell you what experiments to run next to turn it into an airplane or thinking computer.

Peter Thiel: Let's pose the secrecy questions. Are there other people who are working on this too? If so, how many, and if not, how do you know?

Eric Jonas: The community and class of algorithms we're using is fairly well defined, so we think we have a good sense of the competitive and technological landscape. There are probably something like 200—so, to be conservative, let's say 2000—people out there with the skills and enthusiasm to be able to execute what we're going after. But are they all tackling the exact same problems we are, and in the same way? That seems really unlikely.

Certainly there is some value to the first mover advantage and defensible IP in AI contexts. But, looking ahead 20 years from now, there is no a priori reason to think that other countries around world will respect U.S. IP law as they develop and catch up. Once you know something is possible—once someone makes great headway in AI—the search space contracts dramatically. Competition is going to be a fact of life. The process angle that Scott mentioned is good. The thesis is that you can stay ahead if you build the best systems and understand them better than anyone else.

Peter Thiel: Let's talk more about avoiding competition. It's probably a bad idea to open a pizza restaurant in Palo Alto, even if you're the first one. Others will come and it will be too competitive. So what's the strategy?

Scott Brown: Network effects could offer a serious advantage. Say you develop great image recognition software. If you're the first and the best, you can become the AWS of image recognition. You create an entrenching feedback loop; everyone will be on your system, and that system will improve because everyone's on it.

Eric Jonas: And while AWS certainly has competitors, they're mostly noise. AWS has been able to out-innovate them at every step. It's an escape velocity argument, where a sustainable lead builds on itself. We're playing the same game with data and algorithms.

Scott Brown: And you keep improving while other people copy you. Suppose you build a good vision system. By the time other people copy your V1, you've been applying your algorithms to hearing and language systems. And not only do you have more data than they have, but you've incorporated new things into an improved V1.

Peter Thiel: Shifting gears to the key existential question in AI: how dangerous is this technology?

Eric Jonas: I spend a lot less time worrying about dangers of the underlying tech and more about when we're going to be cash flow positive. Which is why I plan on naming my kid John Connor Jonas...

More seriously, we do know that computational complexity bounds what AI can do. It's an interesting question. Suppose we could, in a Robert Hansonian sense, emulate a human in a box. What unique threat does that pose? That intelligence wouldn't care about human welfare, so it's potentially malevolent. But there might be serious limits to

that. Being Bayesian is in some sense the right way to reason in uncertainty. To the extent that I'm worried about this, I'm worried about it for the next generation, and not so much for us right now.

Scott Brown: We think of intelligence as being orthogonal to moral intuition. An AI might be able to make accurate predictions but not judge whether things are good or bad. It could just be an oracle that can reason about facts. In that case, it's the same as every technology ever; it's an inherently neutral tool that is as good or as bad as the person using it. We think about ethics a lot, but not in a way the popular machine ethicists tend to write about it. People often seem to conflate having intelligence with having volition. Intelligence without volition is just information.

Peter Thiel: So you're both thinking it will all fundamentally work out.

Scott Brown: Yes, but not in a wishful thinking way. We need to treat our work with the reverence you'd give to building bombs or super-viruses. At the same time, I don't think hard takeoff scenarios like Skynet are likely. We'll start with big gains in a few areas, society will adjust, and the process will repeat.

Eric Jonas: And there is no reason to believe that the AI we build will be able to build great AI. Maybe that will be true. But it's not necessarily true, in an a priori sense. Ultimately, these are interesting questions. But the people who spend too much time on them may well not be the people who end up actually building AI.

Bob McGrew: We the dangers of technology a little differently at Palantir, since we're doing intelligence augmentation around sensitive data, not trying to build strong AI. Certainly computers can be dangerous even if they're not full-blown artificially intelligent. So we work with civil liberty advocates and privacy lawyers to help us build in safeguards. It's very important to find the right balances.

Question from the audience: Do we actually know enough about the brain to emulate it?

Eric Jonas: We understand surprisingly little about the brain. We know about how people solve problems. Humans are very good at intuiting patterns from small amount of data. Sometimes the process seems irrational, but it may actually be quite rational. But we don't know much about the nuts and bolts of neural systems. We know that various functions are happening, just not how they work. So people take different approaches. We take a different approach, but maybe what we know is indeed enough to pursue an emulation strategy. That's one coin to flip.

Scott Brown: Like I said earlier, we think emulation is the wrong approach. The Wright brothers didn't need detailed models of bird physiology to build the airplane. Instead, we ask: what statistical irregularities would evolution have taken advantage of in designing the brain? If you look at me, you'll notice that the pixels that make up my body are not moving at random over your visual field. They tend to stay together over time. There's also a hierarchy, where when I move my face, my eyes and nose move with it. Seeing this spatial and temporal hierarchy to sensory data provides a good hint about what computations we should expect the brain to be doing. And lo and behold, when you look at the brain, you see a spatial and temporal hierarchy that mirrors the data of the world. Putting these ideas together in a rigorous mathematical way and testing how it applies to real-world data is how we're trying to build AI. So the neurophysiology is very helpful, but in a general sense.

Question from the audience: How much of a good vision system will actually translate over to language, hearing, etc.? If it were so easy to solve one vertical and just apply it to others, wouldn't it have been done by now? Is there reason to think there's low overhead in other verticals?

Scott Brown: It depends on whether you think there's a common cortical circuit. There is good experimental support for it being a single circuit, whether incoming data is auditory or visual. One recent experiment involved rewiring ferrets' brains to basically connect their optic nerves with the auditory processing regions instead of visual regions. The ferrets were able to see normally. There are a lot of experiments demonstrating related findings, which lends support to the notion of a common algorithm that we call "intelligence." Certainly there are adjustments to be made for specific sensory types, but we think these will be tweaks to that master algorithm, and not some fundamentally different mechanism.

Eric Jonas: My co founder Beau was in that ferret lab at MIT. There does appear to be enough homogeneity across cortical areas and underlying patterns in time series data. We understand the world not because we have perfect algorithms, but also because tremendous exposure to data helps. The overarching goal—for all of us, probably—is to learn all the prior knowledge about the world in order to use it. It's reasonable to think that some things will map over to other verticals. The products are different; obviously building a camera doesn't help advance speech therapy. But there may be lots over overlap in the underlying approach.

Peter Thiel: Is there a fear that you are developing technology that is looking for a problem to solve? The concern would be that AI sounds like a science project that may not have applications at this point.

Eric Jonas: We think there are so many opportunities and applications for understanding data better. Finding the right balance between building core technology and focusing on products is always a problem that founding teams have to solve. We do of course need to keep an eye on the business requirement of identifying particular verticals and building products for particular applications. The key is to get in sync with the board and investors about the long-run vision and various goals along the way.

Scott Brown: We started Vicarious because we wanted to solve AI. We thought through the steps someone would need to take to actually build AI. It turns out that many of those steps are quite commercially valuable themselves. Take unrestricted object recognition, for instance. If we can just achieve that milestone, that alone would be tremendously valuable. We could productize that and go from there. So the question becomes whether you can sell the vision and raise the money to build towards the first milestone, instead of asking for a blank check to do vague experiments leading to a binary outcome 15 years down the road.

Bob McGrew: You have to be tenacious. There's probably no low-hanging fruit anymore. If strong AI is the high- (or maybe even impossible) hanging fruit, Palantir's intelligence augmentation is medium-hanging fruit. And it took us three years before we had a paying customer.

Peter Thiel: Here's a question for Bob and Palantir. The dominant paradigm that people generally default to is either 100% human or 100% computer. People frame them as antagonistic. How do you convince the academic people or Google people who are focused on pushing out the frontier of what computers can do that the human-computer collaborative Palantir paradigm is better?

Bob McGrew: The simple way to do it is to talk about specific problem. Deep Blue beat Kasparov in 1997. Computers can now play better chess than we can. Fine. But what is the best *entity* that plays chess? It turns out that it's not a computer. Decent human players paired with computers actually beat humans and computers playing alone. Granted,

chess is a weak-AI in that it's well specified. But if human-computer symbiosis is best in chess, surely it's applicable in other contexts as well. Data analysis is such a context. So we write programs to help analysts do what computers alone can't do and what they can't do without computers.

Eric Jonas: And look at mechanical turk. Crowdsourcing intelligent tasks in narrowly restricted domains—even simple filtering tasks, like “this this is spam, this is not”—shows the increasingly blurring line between computers and people.

Bob McGrew: In this sense, Crowdflower is Palantir's dark twin; they're focusing on how to use *humans* to make *computers* better.

Question from the audience: What are the principles that Palantir thinks about when building its software?

Bob McGrew: There is no one big idea. We have several different verticals. In each, we look carefully at what analysts need to do. Instead of trying to replace the analyst, we ask what it is that they aren't very good at. How could software supplement what they are doing? Typically, that involves building software that processes lots of data, identifies and remembers patterns, etc.

Question from the audience: How do balance training your systems vs. making them full-featured at the outset? Babies understand facial expressions really well, but no baby can understand calculus.

Scott Brown: This is exactly the sort of distinction we use to help us decide what knowledge should be encoded in our algorithms and what should be learned. If we can't justify a particular addition in terms of what could be plausible for real humans, we don't add it.

Peter Thiel: When there is a long history of activity that yields only small advances in a field, there's a sense that things may actually just be much harder than people think. The usual example is the War on Cancer; we're 40 years closer to winning it, and yet victory is perhaps farther away than ever. People in the '80s thought that AI was just around the corner. There seems to be a long history of undelivered expectations. How do we know this isn't the case with AI?

Eric Jonas: On one hand, it can be done. There's an easy proof of concept; All it takes to create a human-level general intelligence is a couple of beers and a careless attitude toward birth control. On the other hand, we don't really know for sure whether or when strong AI will be solved. We're making what we think is the best bet.

Peter Thiel: So this is inherently a statistical argument? It's like waiting for your luggage at the airport: The probability of your bag showing up goes up with each passing minute. Until, at some point, your luggage still hasn't shown up, and that probability goes way down.

Eric Jonas: AI is perceived to have a lot of baggage. Pitching AI to VCs is pretty difficult. Those VCs are precisely the people who expected AI to have come much easier than it has. In 1972 a bunch of people at MIT thought they would all just get together and solve AI over a summer. Of course, that didn't happen. But it's amazing how confident they were that they could do it—and they were hacking on PDP-10 mainframes! Now we know how incredibly complex everything is. So this is why we are tackling smaller domains. Gone are the days where people think they can just gather some friends and build an AI this summer.

Scott Brown: If we applied the baggage argument to airplanes in 1900, we'd say "People have been trying to build flying machines for hundreds of years and it's never worked." Even right before it *did* happen, many of the smartest people in the field were saying that heavier than air flying machines were physically impossible.

Eric Jonas: Unlike things like speed of light travel or radical life extension, we at least have proofs of possibility.

Question from the audience: Do you focus more on the big picture goal or on targeted milestones?

Eric Jonas: It's always got to be both. It's "we are building this incredible technology" and then "here's what it enables." Milestones are key. Ask what you know that no one else does, and make a plan to get there. As Aaron Levie at Box says, you should always be able to explain why *now* is the right time to do whatever it is you're doing. Technology is worthless without good timing and vice versa.

Scott Brown: Bold claims also require extraordinary proof. If you're pitching a time machine, you'd need to be able to show incremental progress before anyone would believe you. Maybe your investor demo is sending a shoe back in time. That'd be great. You can show that prototype, and explain to investors what will be required to make the machine work on more valuable problems.

It's worth noting that, if you're pitching a revolutionary technology as opposed to an incremental one, it is much better to find VCs who can think through the tech themselves. When Trilogy was trying to raise their first round, the VCs had professors evaluate their approach to the configurator problem. Trilogy's strategy was too different from the status quo, and the professors told the VCs that it would never work. That was an expensive mistake for those VCs. When there's contrarian knowledge involved, you want investors who have the ability to think through these things on their own.

Peter Thiel: The longest-lasting Silicon Valley startup that failed was probably Xanadu, who tried from 1963 to 1992 to connect all the computers in the world. It ran out of money and died. And then Netscape came the very next year and ushered in the Internet.

And then there's the probably apocryphal story about Columbus on the voyage to the New World. Everybody thought that the world was much smaller than it actually was and that they were going to China. When they were sailing for what seemed like too long without hitting China, the crew wanted to turn back. Columbus convinced them to postpone mutiny for 3 more days, and then they finally landed on the new continent.

Eric Jonas: Which pretty much makes North America the biggest pivot ever.

Peter Thiel's CS183: Startup - Class 18 Notes Essay

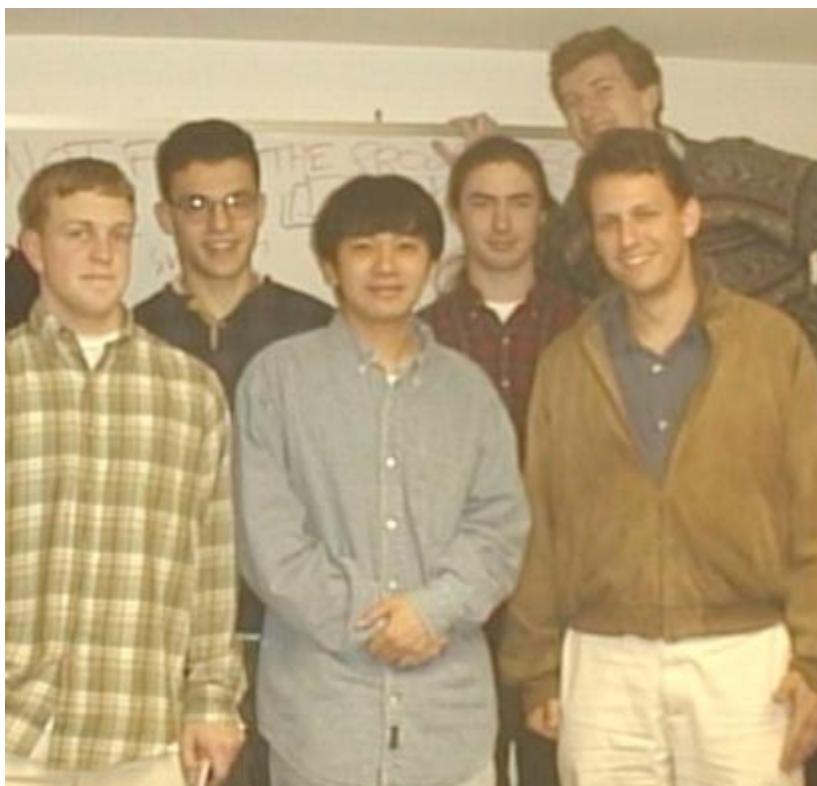
Here is an essay version of my class notes. Errors and omissions are mine. Credit for good stuff is Peter's. Thanks to [Joel Cazares](#) for helping proof this.

Class 18: Founder as Victim, Founder as God

I. Traits of the Founder

Founders are important. People recognize this. Founders are often discussed. Many companies end up looking like founder's cults. Let's talk a bit about the anthropology and psychology of founders. Who are they, and why do they do what they do?

A. The PayPal Origin



PayPal's founding team was six people. Four of them were born outside of the United States. Five of them were 23 or younger. Four of them built bombs when they were in high school. (Your lecturer was not among them.) Two of these bombmakers did so in communist countries: Max in the Soviet Union, Yu Pan in China. This was not what people normally did in those countries at that time.

The eccentricity didn't stop there. Russ grew up in a trailer park and managed to escape to the one math and science magnet school in Illinois. Luke and Max had started crazy ventures at Illinois Urbana-Champaign. Max liked to talk about his crazy attributes (he claimed/claims to have 3 kidneys), perhaps even a little too much. His came to the U.S.

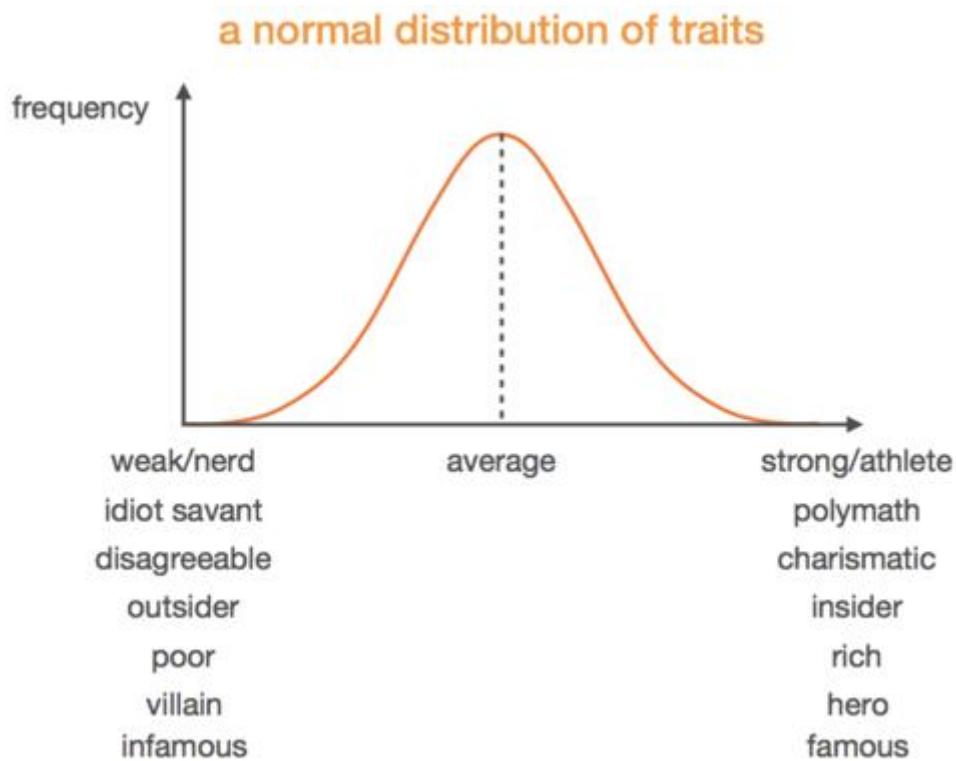
as sort of a refugee weeks after the Soviet Union collapsed but before other countries were formed. So he liked to say that he was a citizen of no country. It made for incredibly complicated travel issues. Everybody decided that he couldn't leave the country, since it wasn't clear that he could get back in if he did.

Ken was somewhat more on the rational side of things. But then again, he took a 66% pay cut to come do PayPal instead of going into investment banking after graduating from Stanford. So there's that.

One could go on and on with this. The main question is whether there is a connection—and if so what kind—between being a founder and having extreme traits.

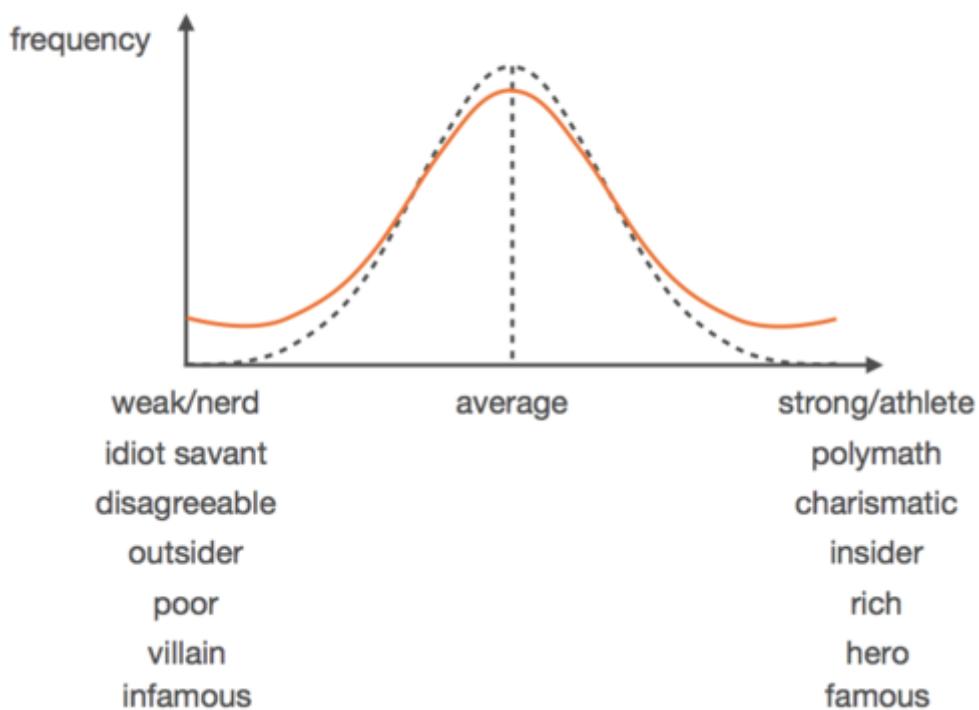
B. Distributions

Many traits are normally distributed throughout the population. Suppose that all traits are aggregated on a normal distribution chart. On the left tail you'd have a list of negatively perceived traits, such as weakness, disagreeability, and poverty. On the right tail, you'd have traditionally positive traits such as strength, charisma, and wealth.



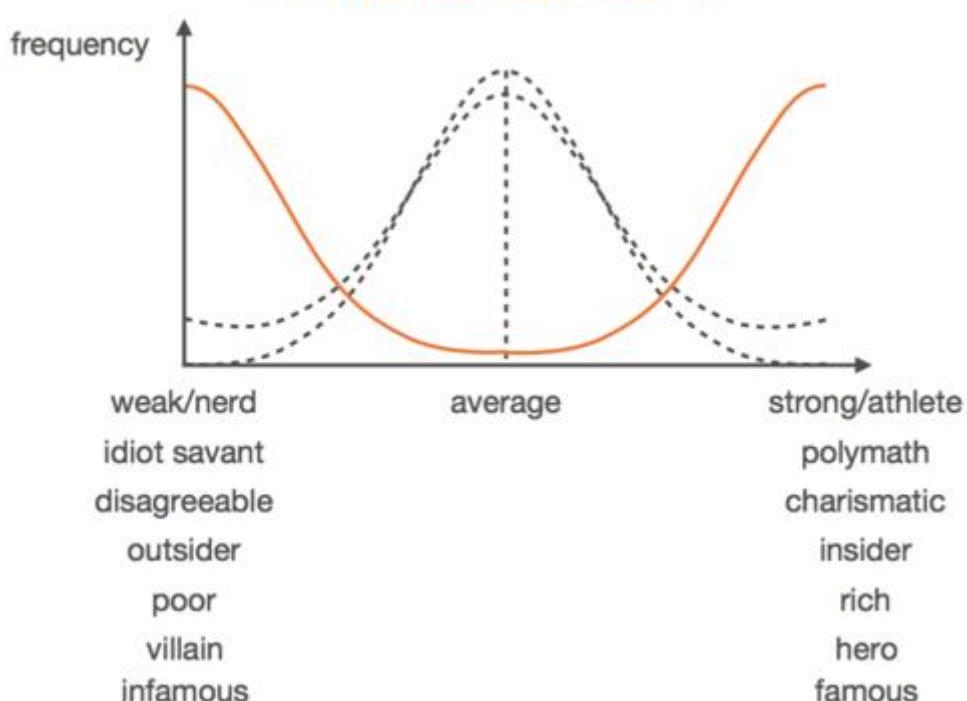
Where do founders fall? Certainly they seem to be a bit less average and a bit more extreme than normal. So maybe the founder distribution is a fat-tailed one:

a fat-tailed distribution



But that radically understates things. We can push it further. Perhaps the founder distribution is, however strangely, an inverted normal distribution. Both tails are extremely fat. Perhaps founders are complex combinations of, e.g., extreme insiders *and* extreme outsiders at the same time. Our ideological narratives tend to isolate and reinforce just one side. But maybe those narratives don't work for founders. Maybe the truth about founders comes from both sides.

the founder distribution



C. Is Inverted Normal Distribution Possible?

There are four basic explanations for such a strange, inverted distribution. The first two reflect the familiar nature vs. nurture debate:

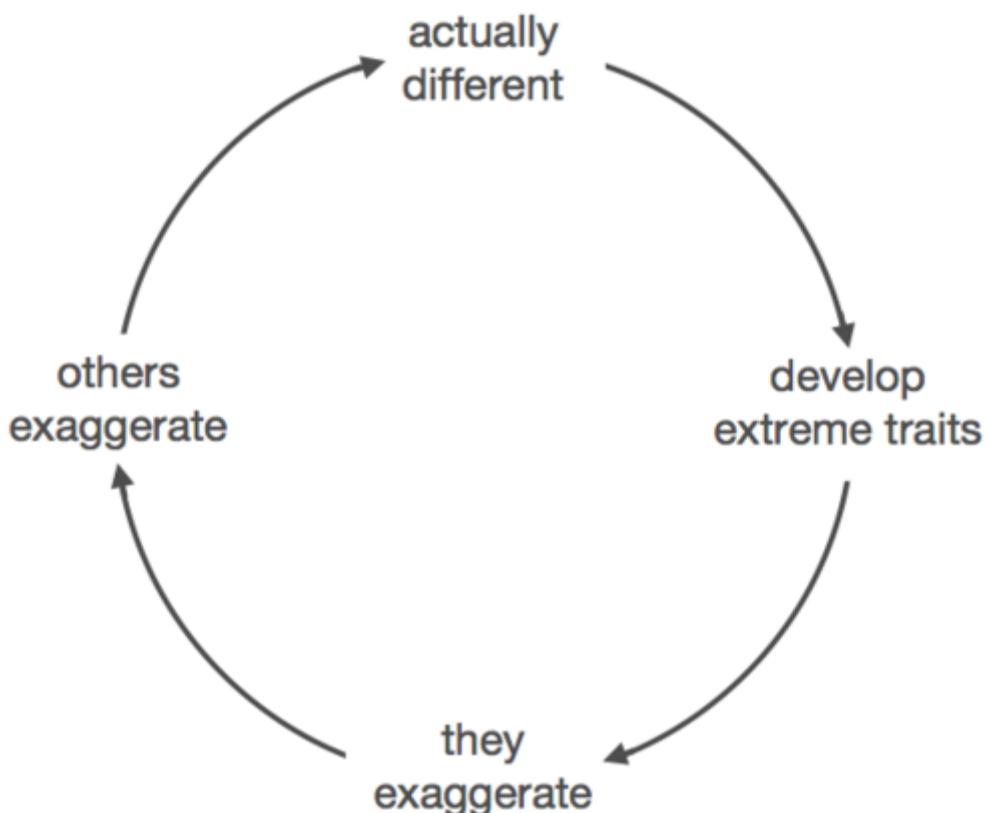
1. It is natural. Founders really are different. Max Levchin really has 3 kidneys.
2. It is developed, or nurtured. Cultural feedback makes founders different.

But the nature vs. nurture paradigm assumes that the distribution is real. It may, in fact, be mythology. To the extent that it's fictional, there are two explanations:

3. It is self-created (exaggerated by the founders).
4. It is other-created (exaggerated by everyone else).

Thinking about founders involves thinking about which of these explanations fit and which do not. The complicated answer is that generally all four apply to some extent. It is very hard to disaggregate them. In practice, they tend to all feed into each other in important but complicated ways.

The dynamic might work like this. People start out being different. They are nurtured to develop their already somewhat extreme traits. Those traits become more important, and they learn to exaggerate them. Others perceive that inflated importance and exaggerate in turn. The founders thus end up being even more different than they were before. And we cycle and repeat.



In practice, the arrows could be reversed. Or the interactions might not make a clean circle, and the feedback loop would be much more complicated. The point is that some interactive combination, and not just one static piece, is driving the process.

D. Applied

Anecdotally, we can apply this framework to any founder figure.

Take Sir Richard Branson, for instance. The big question is whether Branson should be king. He has been called:

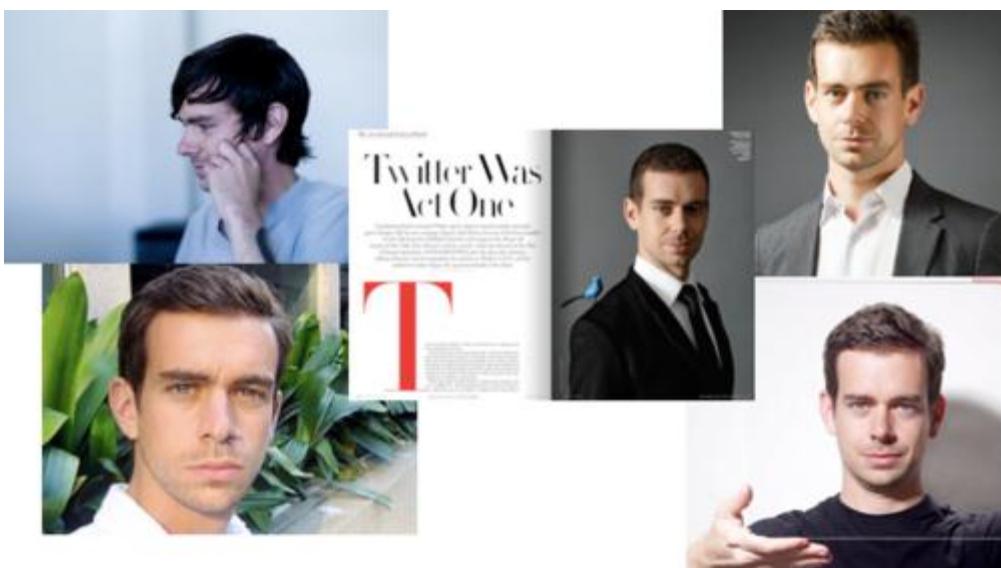
- The king of publicity;
- The Virgin king;
- King of the desert (and space);
- The king of branding;
- The ice king; and even
- King of the Muppets



Let's start with the haircut. He sort of looks like a lion. In fact, in the picture above he is actually dressed up as a lion. It seems kind of redundant. Anyway, one suspects that Branson wasn't actually born with that exact hairstyle. There is probably some degree to which he cultivated and nurtured his traits over time. Reconstructing the truth is tricky. It is very hard to actually know the precise dynamic—nature, nurture, or some kind of fiction—because stories about heroic founder figures get told in very exaggerated, morphed forms.



Jack Dorsey is another figure we can pick on. He's hit all of the extremes and very little of the average. At the outset he donned a nose ring and unkempt hair. He got a nerdy tattoo. Then he transformed to the other extreme side of the inverse distribution. Now he wears Prada suits and fashionable shirts. His branding went from extreme outsider to extreme insider. And this is all going off nothing but totally superficial appearances.



Sean Parker might be the paradigmatic example of the extreme founder figure. There was a rise, fall, rise, fall, and then a rise again. His experience in founding multiple things has been a pastiche of extremes. He didn't go to college. Maybe he didn't even finish high school. He was involved in various underground hacking circles in '90s. He did Napster as teenager. That had a crazy up-down arc to it. Criminal, of course, is the ultimate outsider category. There were all sorts of questions on whether Napster was really a criminal undertaking. Per the Digital Millennium Copyright Act, companies had to list a phone number for people to call for support inquiries. At Napster, that number was Sean's cell phone. He spent a lot of time in the early 2000s assuring concerned Midwestern mothers that their children weren't going to get locked up for having downloaded a Metallica album.

the rise, fall, rise, fall and rise again



And then there are the wacky drug allegations and the crazy celebrity part. Sean made the cover of the Forbes 400 issue; he found a way to be distinctive even amongst the set of the richest people in the world. Justin Timberlake, of course, played Sean in the Facebook movie. There is a person at Clarium who looks pretty similar to these guys. When he travels outside of Silicon Valley, people ask him if he's Justin Timberlake. But in Silicon Valley, people ask him if he's Sean Parker.

Sean seems as exciting to people as he does dangerous. One random anecdote involves the Founders Fund surfing trip to Nicaragua over New Year's 2007. We took the jet down to Managua. We were probably the only people in the country with a private jet. We drove to a remote town on the coast. Everything started off great. We threw a terrific New Year's party. Except it kept getting crazier and crazier. Our professional security guard had to displace some people when various drug dealers and other sketchy types started showing up. In Sean's mind at least, things got weirder from there. 36 hours later, by the morning of Jan 2nd, Sean was all but convinced that our security guard was plotting against him and he was about to be kidnapped. He went from extreme insider to extreme outsider very quickly. He and his girlfriend ditched their luggage and fled to Managua international airport in a cab. The rest of us thought that this was exaggerated paranoia, so we stayed as planned. Sure enough, the security guard became visibly distressed when he noticed Sean was no longer there. We nervously told him that Sean had mentioned that he'd be leaving *tomorrow*—that way he'd already be gone when they tried to nab him at the airport. Ultimately there was a happy ending and no one got kidnapped. But there will probably not be any more Founders Fund trips to Nicaragua.

born this way?

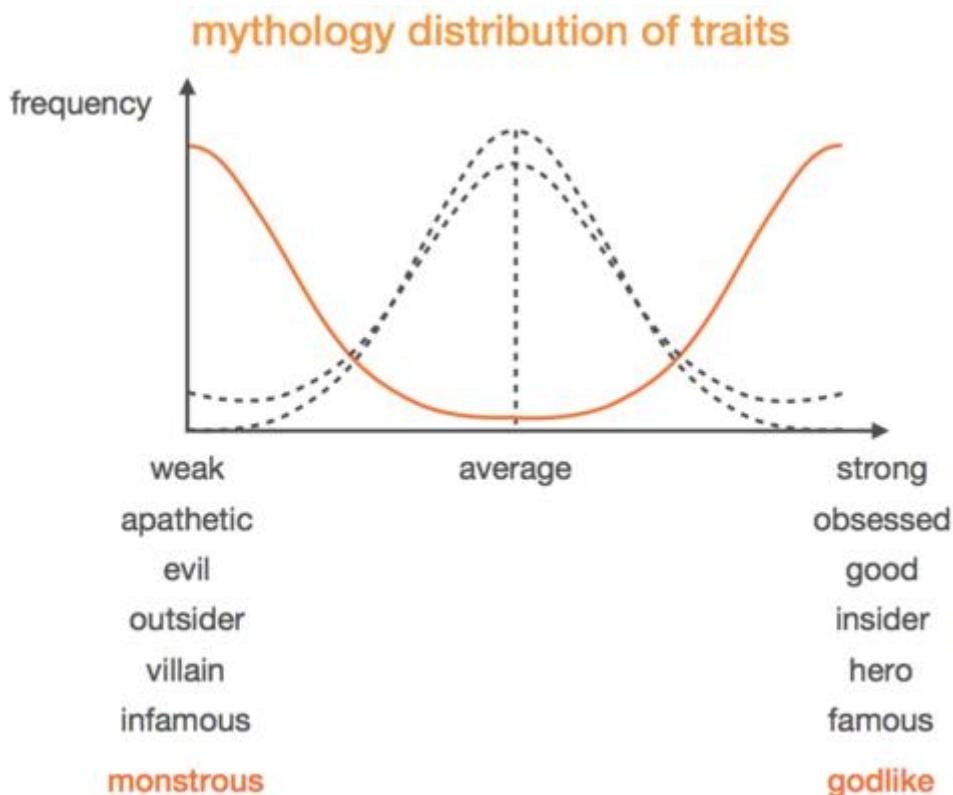


This segues to the pure celebrity version, best epitomized by Lady Gaga. *Born This Way* is her recent hit album and song. On one level, the whole thing is obviously completely fictional. It's probably safe to say that she was not, in fact, born like this. The big piece must be nurture. But on another level, maybe it *is* nature. What sort of people would actually do this to themselves? Maybe one actually does have to be born that way in order to do these things. Who really knows for sure? Is Gaga self-created myth? A myth created by other people? Everything all pulled together at once?

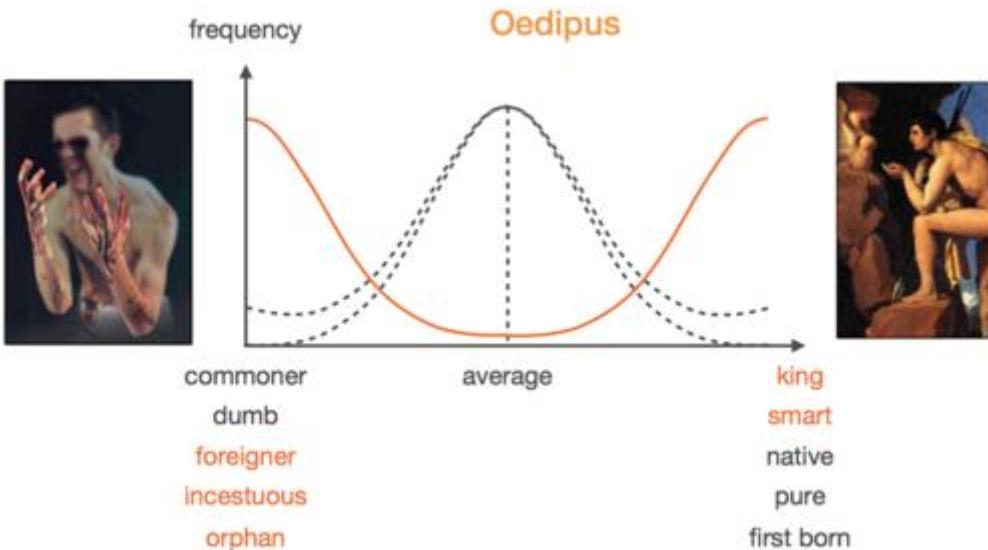
II. Mythology

Oddly enough, classical mythology overlaps with the inverted bell curve distribution. There are monsters and there are gods. And very often they are one and the same.

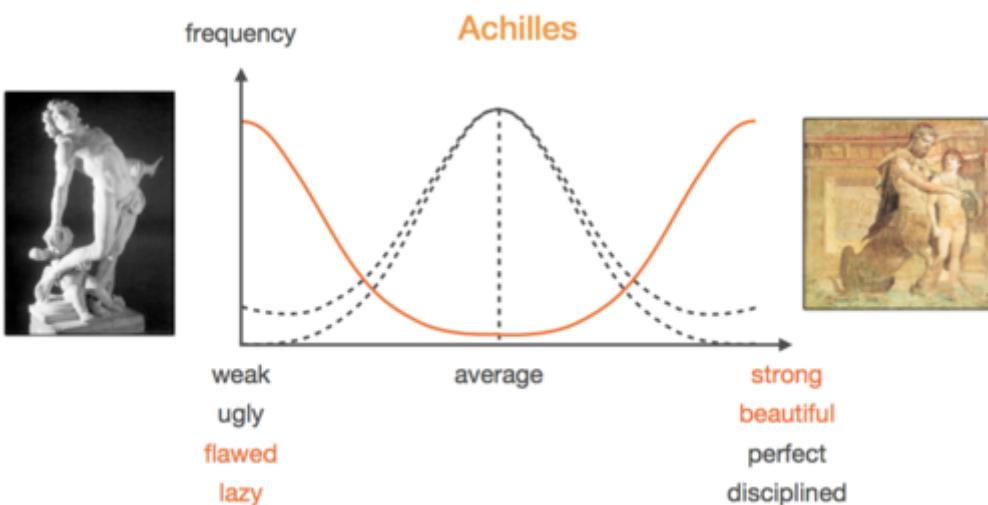
In what sense are founders like mythical heroes? Myths about the founding of things are very common. Are mythical heroes actually any different? Did they have extreme traits? Develop them? Did they exaggerate themselves? Did others exaggerate their stories?



Consider Oedipus. He was both an extreme insider and an extreme outsider. He was the king. He was so brilliant that he was able solve the riddle of the sphinx. But he was abandoned to die on a hill as an infant. He was a foreigner from a different place. And then he had the incest accusations and ensuing downfall.

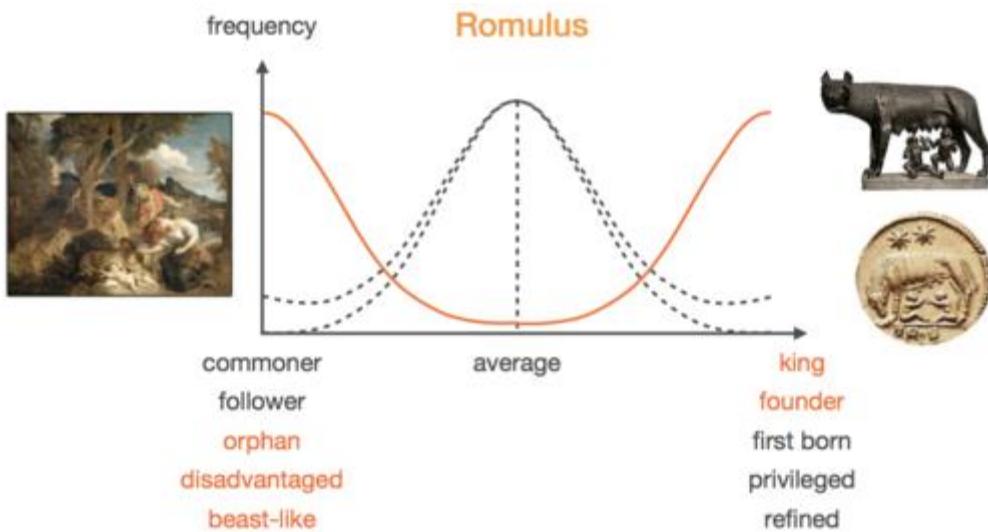


Achilles is another mythological hero who was active at the extremes. He was incredibly strong and perfect, except where he was weak and flawed.



Perhaps the most classic founding of all is the founding of Rome. Romulus and Remus were disadvantaged, common orphans who were raised by wolves. They were outsiders. But then they became founders and lawmakers. Romulus killed his brother and became a lawbreaker and king. If there is a hierarchy to it—if killing your brother is worse than killing a random person and killing your twin brother is even worse than that—then Romulus was an unusually bad criminal.

Legend has it that what prompted the murder is Remus' leaping over the imaginary boundary line that Romulus had established as the edge of Rome. The rule was codified with blood: anyone who jumps over the walls of Rome will be destroyed. Does this make Romulus a criminal outlaw? Or does it make him the king who defined Rome? It depends. Maybe he was both.



Remus obviously had a bad ending. Romulus' ending is more ambiguous. In Livy's account, there was a huge storm that terrified the people. When the storm cleared up, Romulus had disappeared. It was announced that he had become a god. But Livy also notes an alternate account; a group of conspiratorial senators caught up with Romulus and used the chaos of the storm as cover to kill him and dispose of the body.

One other mythical element was the 12 eagles that Romulus saw from Palatine Hill. They stood for the 12 centuries that Rome would endure, after which point the debt of the founding crime would have to be repaid. Approximately 12 centuries later, Attila the Hun apparently thought it would be a good idea to copy Romulus, and killed *his* brother Bleda. Incidentally, fratricide is probably no longer best practice for founding things.

III. Archaic Cultures

A. The Sacrificial Cycle

The founder/extremeness/infamous dynamic, or something very much like it, was an incredibly important part of ancient cultures. The fundamental problem in these cultures was that there were all sorts of conflicts everywhere. People didn't know what to do. There were no rules—a striking parallel to the tech startup context. Amidst all the chaos there was war of all against all.

problem: war of all against all

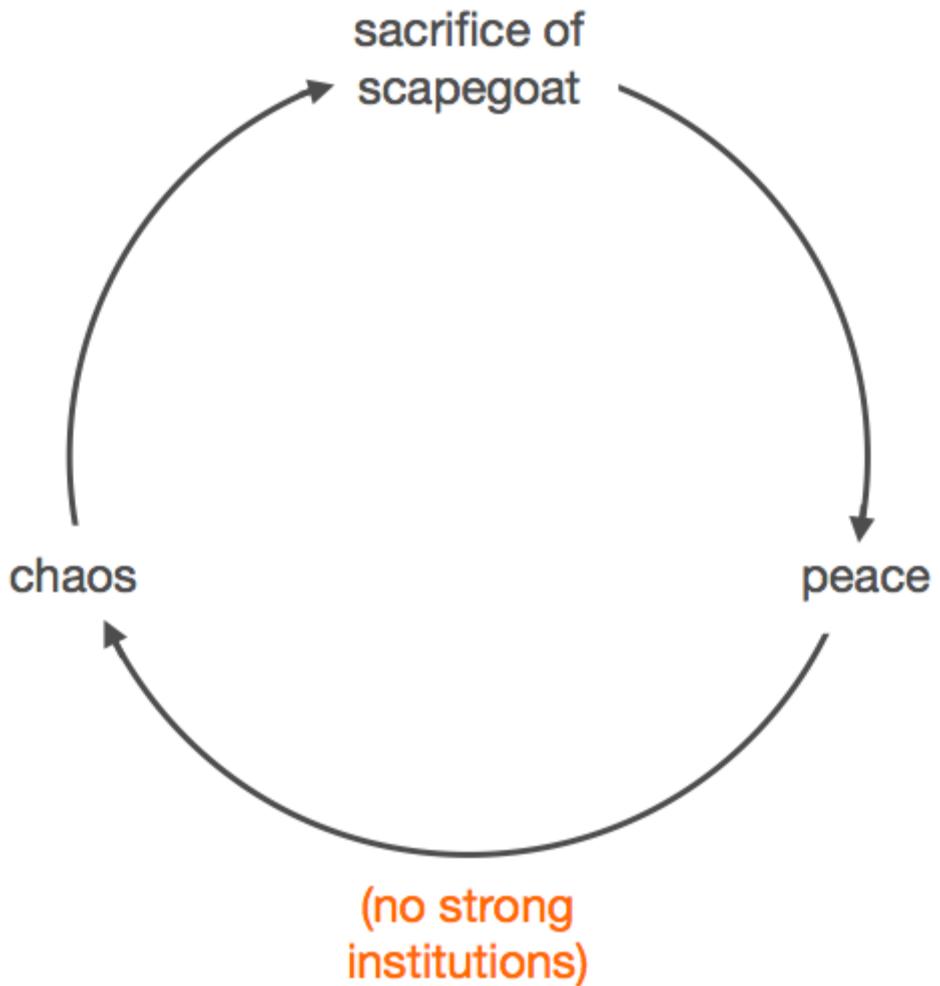


Various enlightenment theorists have insisted that, to escape this warring state of nature, people got together, had a good chat, and drew up a social contract. But nothing of the sort ever happened. Where warring civilizations didn't just collapse entirely, the most common resolution involved polarizing and channeling all the hostility into one particular person. Depending on the culture, witches were burned or people had their hearts cut out. The details differed. But the dynamic—a crazed community rallying around the sacrificial scapegoat—was the same.

resolution: war of all against one

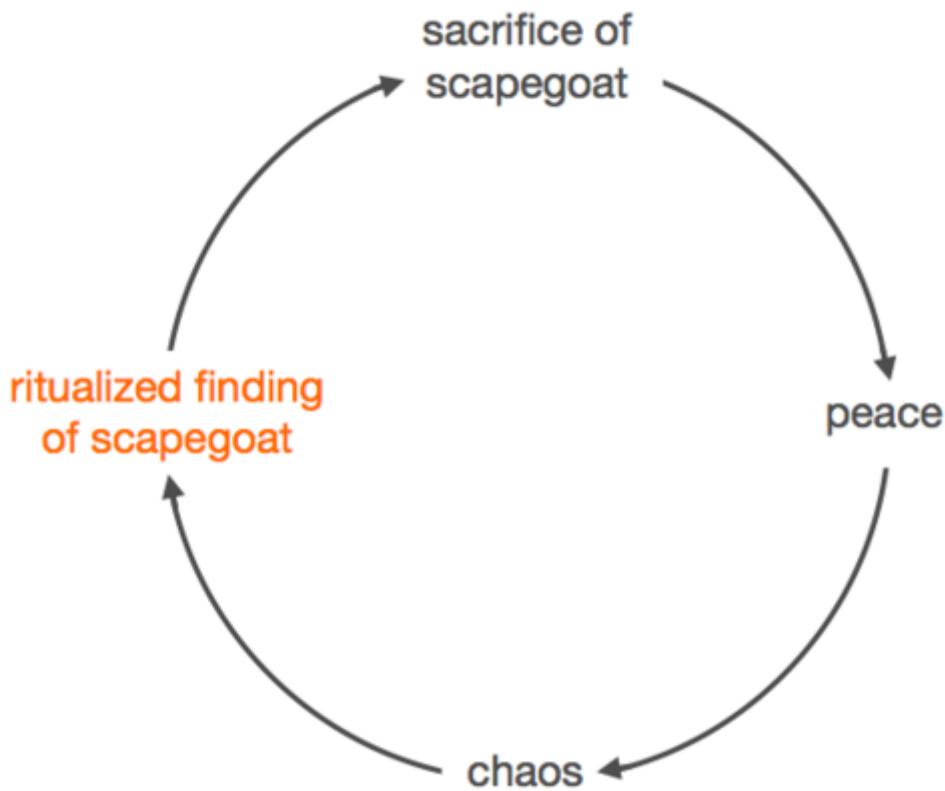


In cultures that had some degree of permanence, this became a cyclical process. Absent strong institutions, peace never lasted. Things would go wrong. Maybe disease struck. Or maybe there was some other kind of internal (and less often, external) conflict that led to complete chaos. And then people would gang up, unite against a scapegoat, and perform the sacrifice. Peace was restored. And the cycle repeated ad infinitum.



It's clear that the scapegoat is extremely powerful. Scapegoats can turn conflict into peace. This makes the scapegoat omnimalevolent; if peace follows his killing, he must have been very bad indeed. Or maybe it's omnibenevolent, since it trades its life so that others may live in peace. Probably the right answer is that it's some of both.

We can speculate that in many cultures, this process became ritualized. People realized the power of the scapegoat and abstracted it away from localized contexts. Instead of waiting for random uncontrolled chaos, sacrifice became planned. Of course, there were probably cultures that never figured this out. They couldn't systematize the isolation of the scapegoat. So everyone just killed everyone and the culture blew up. One suspects that the cultures that managed to ritualize and repeat the cycle were the ones who lasted for awhile.



B. Finding the Victim

There are all sorts of questions on how to go about finding the scapegoat. Sometimes the processes are random. In Gaelic Scotland, people would bake a cake over the fires of Beltane and cut it into pieces. One piece would be marked with charcoal. Men would choose a piece from a bonnet. Whoever got the black piece was the *devoted*, and was sacrificed to Baal. Residual forms of this persisted up through the 18th century, where the devoted would have to just jump *through* the fire instead of perish in it.

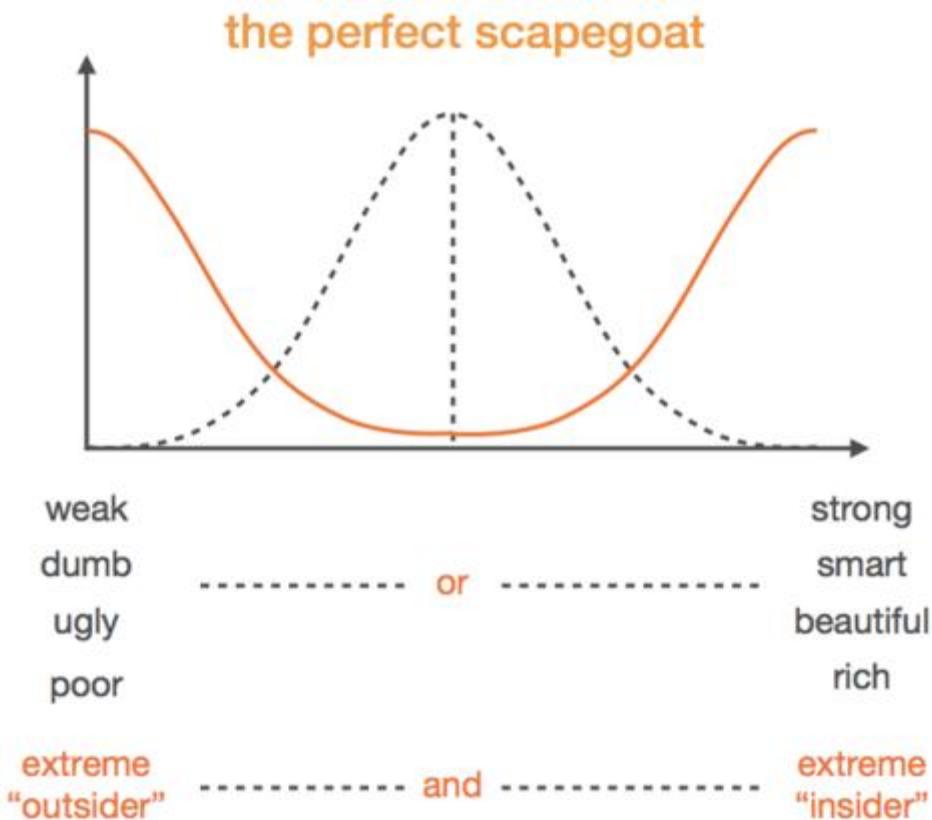
The ancient Gauls took a more objective approach. Someone would have to be sacrificed on the eve of battle to win the gods' favor. But which person would that be? Rather than complicate things, the Gauls just held a footrace to the battlefield. Naturally, the slowest person was the one who got sacrificed.



C. Anatomy of the Scapegoat

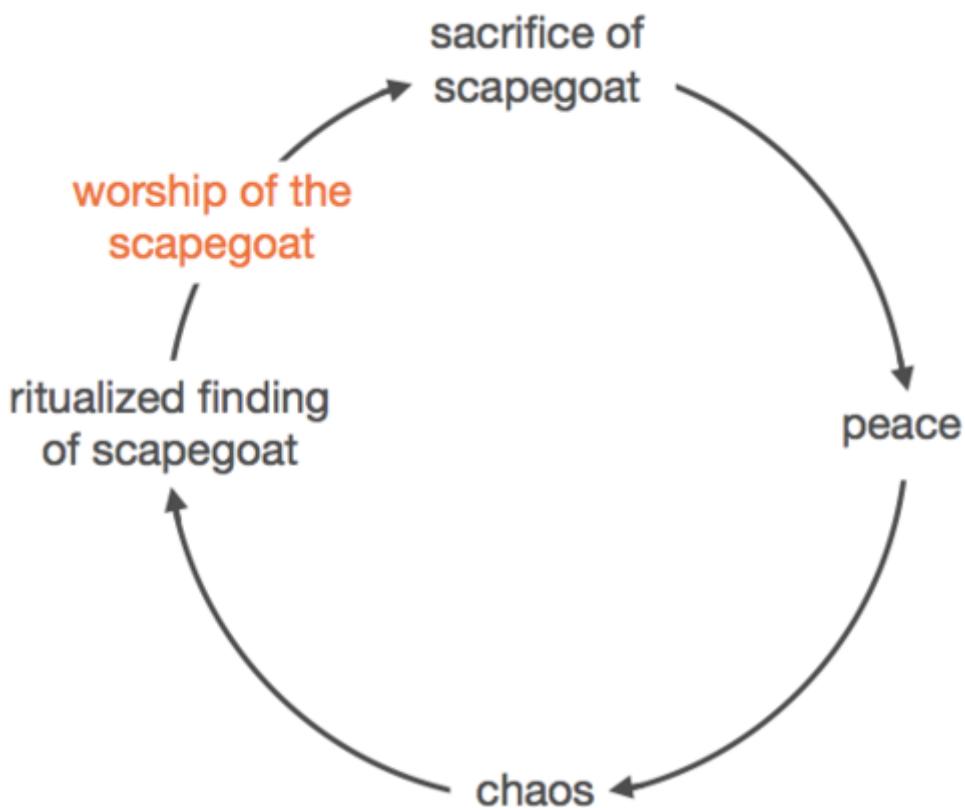
The perfect scapegoat is someone at both extremes. He must be both an extreme outsider and an extreme insider. It can't be a completely random person drawn from a homogeneous lot. It must be some sort of outsider, lest the people in the crowd get introspective and realize that the sacrificed was essentially just like them (and, next time, may

well be them). But neither can the scapegoat be entirely different from the crowd; he must be an insider, since the pretext behind the ritual is that he is responsible for the internal community strife.



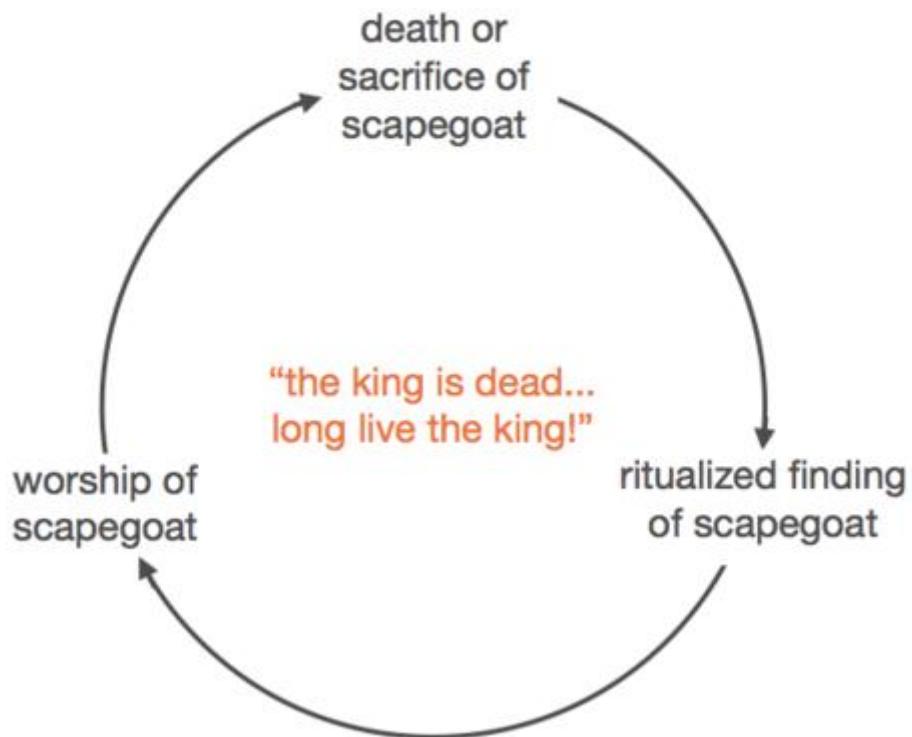
D. The Roots of Monarchy

Not all scapegoats were hated all the time. Very often, they would be worshipped before they were sacrificed. People would give the scapegoat a certain amount of power before tearing him apart. That scapegoats were either worshipped or demonized follows from their being all-powerful.



One working theory is that monarchy originated this way. The Aztecs, for instance, would basically crown someone a quasi-god king for a period of time, after which point he would be sacrificed. Kings became scapegoats who had not yet been killed. Every king was a living god. Every god was a murdered king.

the emergence of monarchy



Arguably Egyptian pharaohs started off as scapegoats. Perhaps the first pyramids were the piles of stones that entombed people who were stoned to death. Later, when Pharaohs became powerful kings and it was unthinkable to kill them during their lifetimes, people kept putting increasingly large piles of stones on top of them after they died.



Given this dynamic, we can imagine how monarchy came into being. The scapegoat simply figured out how to maintain his power and indefinitely delay his execution.

The Zulu Kingdom was a warlike African monarchy in the 19th century. The Zulu king had to be strong and powerful. He could have hundreds of wives and do pretty much whatever else that we wanted. But once he started to get white hair and wrinkles, his power faded. He would be deemed unfit to be king, deposed, and killed. It's hardly a surprise, then, that upon first contact with the British, the Zulu kings were more interested in hair coloring lotion than in anything else. Whether phenomena like this continue to exist in our society today is a question well worth asking.

E. The Politics of Sacrifice

According to Aristotle, tragedy functioned so as to reduce common peoples' anger toward successful people. The lesson in all tragedy is that even the greatest people have tragic flaws. Everybody falls. It was thus cathartic for ordinary people to see terrible things happen to extraordinary people, if only on stage. Tragedies were political tools that transformed envy and anger into pity. Commoners would retreat contentedly to their small houses instead of plotting against the upper class.



Julius Caesar was a classic insider/outsider. Eventually, of course, he was assassinated. Every subsequent Roman emperor pretty much had to be a Caesar. And the sacrificial cycle repeated an infinitum for centuries thereafter.

Being an extreme insider is great, until it all goes very wrong. Marie Antoinette was such an insider. But people turned on her. She was an Austrian, i.e. a foreigner. She faced accusations strikingly similar to those from the Oedipus mythology. It's not clear whether the "let them eat cake" line was fictitious or not. But all great revolutions could be described as the rapid shift from insider to outsider. During the French Revolution, there was an interesting legal debate on whether the king should get a trial. Robespierre and the revolutionaries vehemently argued against a trial. The king, they should, should be slaughtered like a wild beast. Having a trial meant that the king might be innocent, which, in turn, meant that the people might be guilty. But it was unthinkable that the people might be guilty. So the solution was to just kill the king.



IV. Sacrifice Endures

A. In Culture

A modern version of this is the 12-person jury in the criminal context. The unlucky 13th person is the criminal who gets punished or killed. It is the classic scapegoating-type mechanism. The 13th person is assumed to be—and probably is—different. It's never really a jury of your peers. If you're a murderer, you aren't judged by 12 murderers. If you're rich, they don't find 12 rich people to decide your fate. It is very much unclear whether a jury trial works well for its stated

goals at all. It seems to work in contexts where people perceive things as they are. But other contexts, it is just scapegoating gone crazy.



Another modern version has to do with celebrities, and resurrects the monarchical dynamic that people assume has long since died. We literally anoint our stars as kings. Elvis was the King of Rock. Michael Jackson was the King of Pop. Britney Spears was the Princess of Pop—I guess Madonna was the Queen. You start to run out of titles pretty quickly.

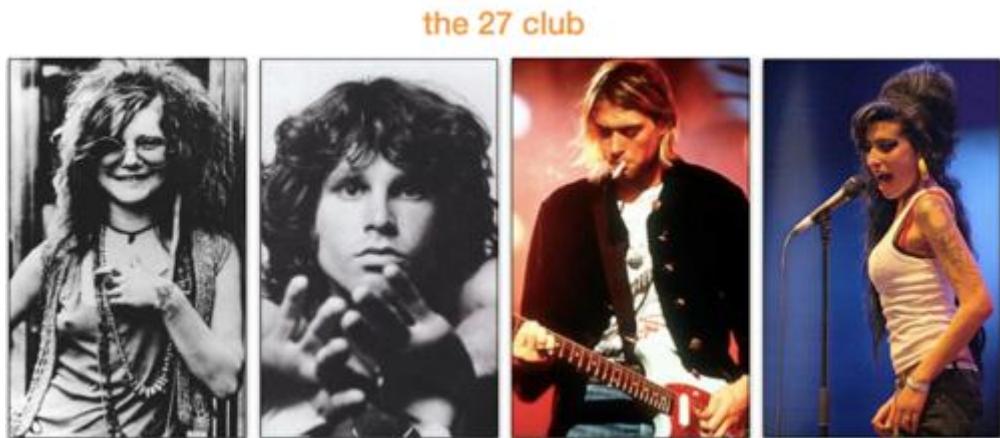


Then, at some point, things go wrong. The anointed are put on pedestals only to be torn down. Elvis self-destructed in the '70s. Michael Jackson obviously went downhill. The picture below depicts Britney Spears at height of the paparazzi insanity. A few years ago, the paparazzi industry was a \$400 million/year industry. Britney Spears drove \$100 million of that. There were between 1,000 and 2,000 people who made their living doing nothing but chasing her around and taking pictures of her. What went wrong? Was Britney naturally crazy? Did she become crazy after having been isolated as a child superstar? Maybe the crowd got to her. Or maybe she intentionally acted in weird ways for the publicity.



Regardless, these kind of stars all enjoy a very strange afterlife. In life, they are torn down from pedestals. But after they die, they are resurrected as god-kings. Things come full circle.

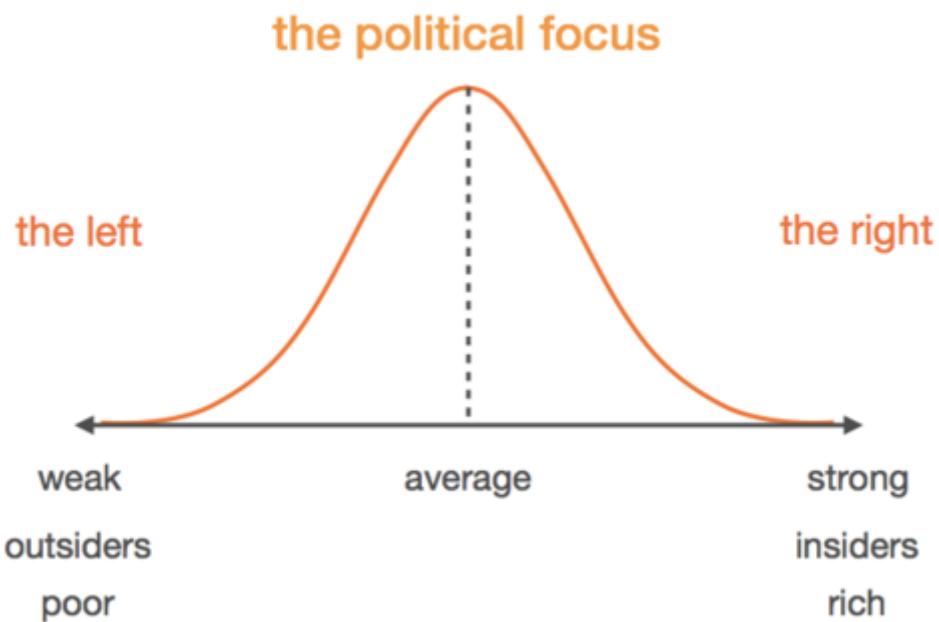
Another example of this is the Forever 27 club, whose members include Janice Joplin, Jimi Hendrix, Jim Morrison, Kurt Cobain, Amy Winehouse, etc. This is the set of famous musicians who all died at age 27. "They tried to make me go to rehab, I said, 'No, no, no.'" There are all sorts of questions one could ask. But there is a sense in which these people will live on as iconic cultural figures.



The "from destructive to immortalized" dynamic goes way back to mythology. Alexander the Great was 32 when he died. He would frequently engage in hardcore quasi-religious drinking marathons. Apparently the game was to consume alcohol until someone died, and Alexander felt that he had to prove that that someone would not be him. It was a strategic error. But he will forever be known as a great conqueror.

B. In Politics

The political version involves certain ideological distortions. People on the left and the right tend to focus and even obsess on people from the other side. Everybody from the other column becomes the crazy person and the legitimate scapegoat. In reality, the truth is that it tends to involve some strange combination of both.



Two of our greatest presidents had this sort of strange heroic arc to their story. Abraham Lincoln was an extreme outsider turned insider. He was born in an isolated log cabin. He was probably our poorest President. He was very smart and also very ugly. And he, probably intentionally, uglified himself even further with his strange beard. Lincoln was always on both extremes. His end involves a very strange return to the Cesar question. John Wilkes Booth, believing that he was reenacting Cesar assassination, shouted “Sic semper tyrannis” as he shot Lincoln—which is, of course, what Brutus is reputed to have said as he stabbed Caesar.



A strange counterpoint point to this comes from one of Lincoln's first public speeches ever. The future president delivered what is now called the [Lyceum Address](#) to a small crowd in Springfield Illinois in 1837, when he was 28 years old. It is worth reading in its entirety. It opens:

As a subject for the remarks of the evening, "The perpetuation of our political institutions" is selected.

Lincoln spoke about how there could not be any more founding moments in the United States. The founding had been done, in the 18th century. It was over. At this point all that one could do was preserve and maintain things. There was nothing truly new that anyone could ever hope to do in our government.

About halfway through the speech, things get really interesting. Lincoln asks whether ambitious people would ever try to be founders anyways, or whether they would be fully satisfied with existing institutions. He answers yes and no, respectively:

The question then is, Can that gratification be found in supporting and maintaining an edifice that has been erected by others? Most certainly it cannot. Many great and good men, sufficiently qualified for any task they should undertake, may ever be found whose ambition would aspire to nothing beyond a seat in Congress, a gubernatorial or a presidential chair; but such belong not to the family of the lion or the tribe of the eagle. What! think you these places would satisfy an Alexander, a Caesar, or a Napoleon? Never! Towering genius despairs a beaten path. It seeks regions hitherto unexplored. It sees no distinction in adding story to story upon the monuments of fame erected to the memory of others. It denies that it is glory enough to serve under any chief. It scorns to tread in the footsteps of any predecessor, however illustrious. It thirsts and burns for distinction; and if possible, it will have it, whether at the expense of emancipating slaves or enslaving freemen. Is it unreasonable, then, to expect that some man possessed of the loftiest genius, coupled with ambition sufficient to push it to its utmost stretch, will at some time spring up among us?

The takeaway is that we have to be really careful because such people might exist.

Kennedy's story was different but the underlying dynamic was the same. He was one of richest people—worth about \$1 billion in today's money—to become president. His father was criminal bootlegger. He was on amphetamines most of the time. He stopped being a rich insider when he found himself an outsider to whatever plot or conspiracy it was that led to his assassination.

C. In Tech Companies

This dynamic recurs over and over again in the tech company founder context. Let's focus on 3 instances: Bill Gates, Howard Hughes, and Steve Jobs.

“Who is more important in the world today: Bill Clinton or Bill Gates? I don’t know.”
- Peter Jennings

Those old enough to remember will remember the “Bill Gates is god” phase in the ‘90s. The president of the U.S. always has a quasi-divine status. So when you get compared to the sitting president, it’s pretty extreme. All the same questions apply to Gates. Was it nature or nurture? He was a Harvard insider but a dropout outsider. He wore big glasses. Did he become a nerd unwillingly? Did he prosper by accentuating his nerdiness? It’s hard to tell.

Bill Gates as god



What is clear, however, is that the good times didn't last:

"Bill Gates is a monocle and a Persian Cat away from being the villain in a James Bond movie."

-Dennis Miller

One (admittedly unconventional) theory is that Bill Gates is still being tortured and punished for his fall. He has to go to all sorts of boring charity events, pretend that the people there are saying interesting things, and then give them his money to boot. And adding insult to injury is the fact that these are the same people who ganged up on him in the late '90s.

Bill Gates as victim



Howard Hughes was one of the greatest founders in the 20th century. His life had a very extraordinary arc to it from about 1930 to 1945. He started off as reasonably successful. He went on to have incredible parallel careers in movies and aviation, which, in retrospect, were the two booming tech sectors of the 1930s. He became the wealthiest person in the U.S. by age 45. If Hughes had died in the plane crash that he had in 1946, he would have gone down as greatest entrepreneur of 20th century.

Howard Hughes as god



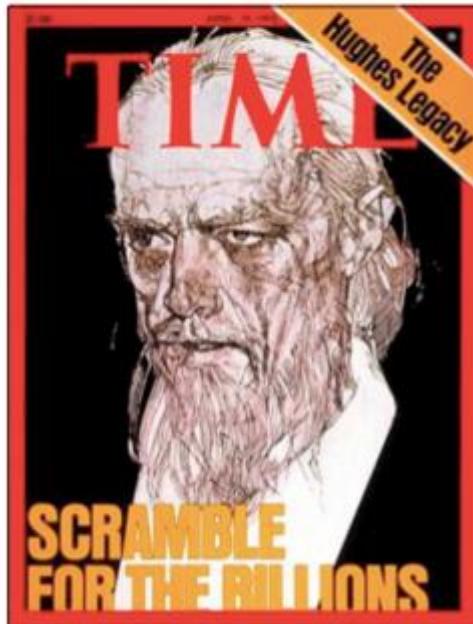
One of Hughes' favorite tricks was to pretend to be crazy on the theory that no one would take the time and energy to try to stop or compete with a crazy person. A large part of his mythology was fictionally constructed; he claimed, for instance, to have been born on December 25th, 1905. One has to wonder if he was *really* born on the same day of the year as Christ, or whether that was an intentional ploy.

"Howard Hughes was this visionary who was obsessed with speed and flying like a god..."

-Martin Scorsese

Hughes' fall from grace began after the '46 crash, when he became addicted to painkillers. He more or less holed up in various penthouse lofts for 30 years, hooked up to IV machines and refusing to eat. Looking back the story has a pretty crazy color to it. The craziness continued even after Hughes died; as there was no authoritative will, all sorts of distant descendants and questionable figures began a long and vicious fight to inherit the estate.

Howard Hughes as victim



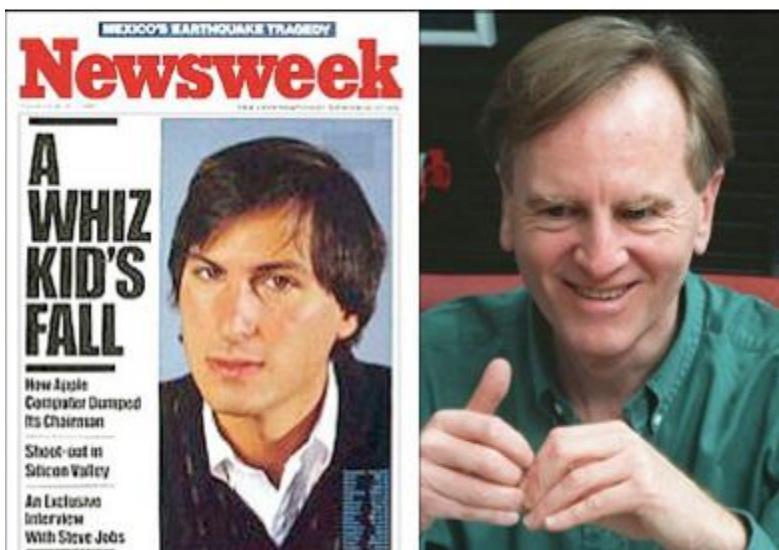
And then there's the Steve Jobs version. You could probably tell a few different versions of the Jobs version. Let's focus on the one from the '70s and '80s. He had all the classic extreme outsider and extreme insider traits. He dropped out of college. He was eccentric and had all these crazy diets. He started out phreaking phones with Steve Wozniak. He took LSD.

Steve Jobs as god



Ultimately he was kicked out of apple and was replaced John Sculley, who was seen as the much more normal, adult-type person that should be in charge.

Steve Jobs as victim



Circling back to the bit about archaic cultures. Isn't this dynamic roughly the same now as it was then? We tend to think of monarchy as a dead and defunct institution. But is it really? Time magazine put Marc Andreessen on the cover in February 1996—*sitting on a throne-like chair!* He was later vilified quite a bit when things went bad at Netscape. Now he seems to have recovered quite nicely.



Mary Meeker had a similar rise and fall and then rise again. Dubbed the “Queen of the Net,” Meeker was an influential stock market analyst who was probably the most bullish person on net in the ‘90s. If she wrote about your company, your stock would go up. She received a much more negative reassessment from the public after the ‘90s tech bubble exploded. She was torn down from the pedestal. But she stuck through it at Morgan Stanley and has come back to being very successful, now as a venture capitalist.

D. Can It Be Escaped?

How much of this can be avoided? How do you avoid becoming a sacrificial victim? The simple answer, of course, is that if you really don’t want to get killed, you shouldn’t sit on the throne. But this seems suboptimal. Wearing the crown is obviously an attractive thing. The question is whether you can decouple it with getting executed.

That is the danger with being an extreme insider. Push too hard and the poles reverse; you end up as an extreme outsider and it all goes to pot. There have been 44 American presidents. Four of them—9% of presidents—were assassinated while in office. Four more were almost killed. Your odds of not dying a violent death are dramatically lower if you’re not the president. That’s at least worth thinking about if being president is your goal.

This is not to say that people can or should escape by abdicating the throne. Sometimes the risk is worth it. And maybe you can reduce the risk. There have to be CEOs and founders. Those people are expected to wear the crown. That necessarily involves a certain amount of playing with fire. The tricky part is that, while mistakes get made, they are incredibly hard to spot at the time. They are more easily analyzed in retrospect. Bill Gates was incredible through the 1990s, until Larry Ellison and Scott McNealy and a bunch of CEOs from other tech companies effectively started a “We Hate Gates” club, stirred up attention at the DOJ, etc. From Gates’ perspective, he was on perpetual winning arc of never-ending progress. Everything was perfect, and the haters were just envious and pathetic. But once it turns it can turn pretty quickly. The falls are so big that it’s hard to fully recover.

V. Extending the Founding

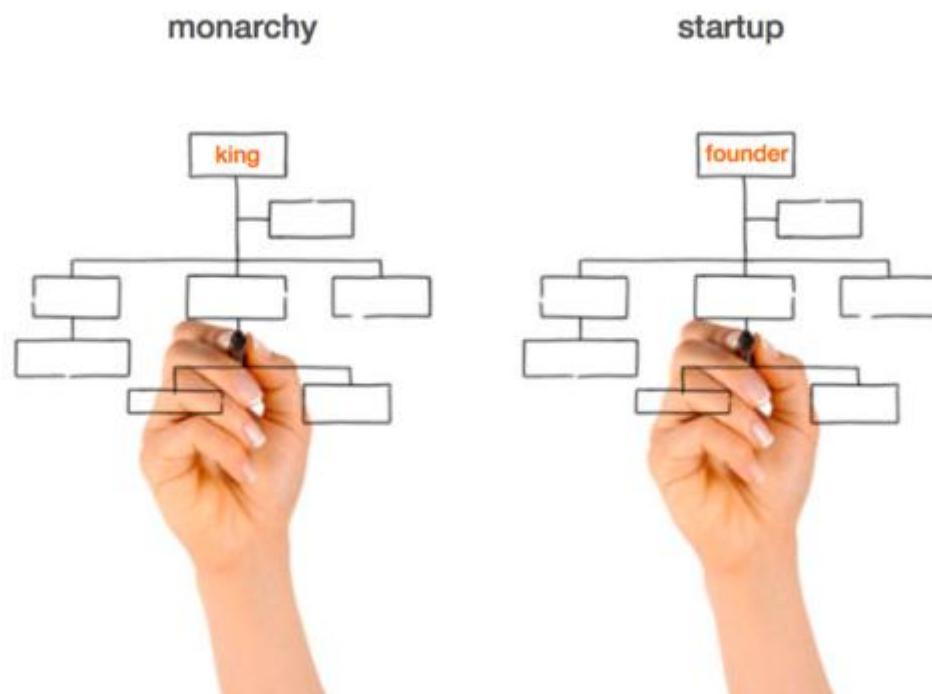
A. Forms and Theory

One strategy to avoiding becoming a scapegoat is to extend the founding moment. With the big caveat that there is probably no single silver bullet solution—the founder turned god turned victim dynamic is probably inescapable to some extent—let's work through some ideas on how to negotiate this dangerous ground.

You can plot out the various forms of government on 1-dimentional axis:



A startup is basically structured as a monarchy. We don't call it that, of course. That would seem weirdly outdated, and anything that's not democracy makes people uncomfortable. But look at the org chart:

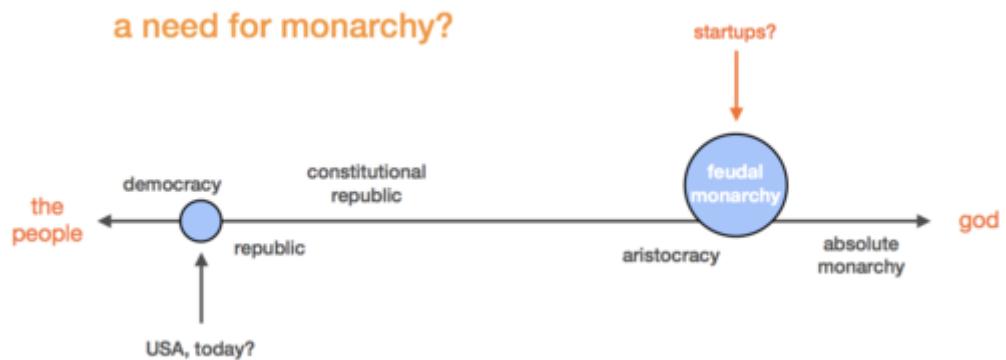


It is certainly not representative governance. People don't vote on things. Once a startup becomes a mature company, it may gravitate toward being more of a constitutional republic. There is a board that theoretically votes on behalf of all the shareholders. But in practice, even in those cases it ends up somewhere between constitutional republic and monarchy. Early on, it's straight monarchy. Importantly, it isn't an absolute dictatorship. No founder or CEO has absolute power. It's more like the archaic feudal structure. People vest the top person with all sorts of power and ability, and then blame them if and when things go wrong.

We are biased toward the democratic/republican side of the spectrum. That's what we're used to from civics classes. But the truth is that startups and founders lean toward the dictatorial side because that structure works better for startups. It is more tyrant than mob because it should be. In some sense, startups can't be democracies because none are. None are because it doesn't work. If you try to submit everything to voting processes when you're trying to do something new, you end up with bad, lowest common denominator type results.



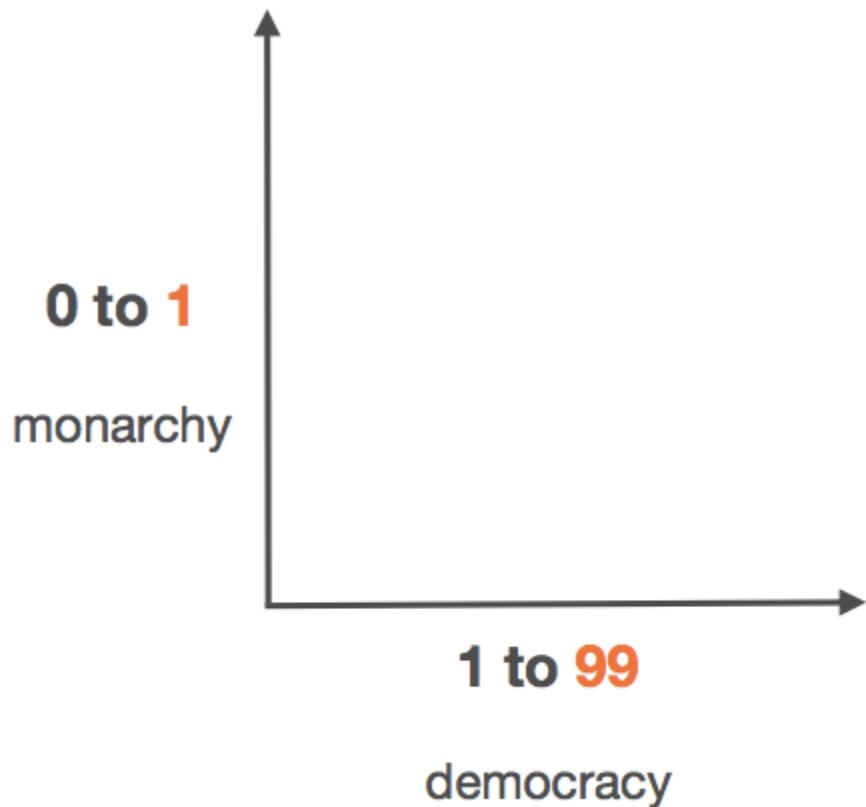
But pure dictatorship is unideal because you can't attract anyone to come work for you. Other people want some power and control too. So the best arrangement is a quasi-mythological structure where you have a king-like founder who can do more than in a democratic ruler but who remains far from all-powerful.



B. Occupy

We can reimagine our old 0 to 1 (technology) and 1 to n (globalization) paradigm by putting a monarchy/democracy overlay on it. Monarchy involves going from 0 to 1. Democracy involves going from 1 to 99.

two paradigms



The 99% vs. the 1% is the modern articulation of this classic scapegoating mechanism. It is all minus one versus the one. And it has to just be the one. 99.99 people or percent is too granular. Scapegoating 0.1 doesn't really work. You need a whole person to play the victim. Similarly, 98-2 doesn't quite have the same ring to it either.



C. Extending the Moment, Escaping the Trial

The normal company arc involves an initial monarchical founding period and then a normal period where founders are gone and more conventional people come in and run things. In the U.S., there were the founding fathers. And then there have been everybody else. Perhaps some figures like Lincoln or FDR were exceptions to this. But the two phases are generally clear and distinct.

If you want to be a founder and stay a founder, can you extend the founding period? In tech companies, foundings last as long as technological innovation continues. The question is thus how long it takes for the substantive technology focus to yield to process. Once you shift toward ossified, process-based normality, much less gets done. Every founder would thus do well to never stop wondering whether there are strategies to extend the founding in one form or another.

This probably requires a healthy amount of paranoia. You might conceive of every board meeting as a trial. At best, the board is jury (though probably not of your peers). At worst, it is a mob and is looking to make you the sacrificial victim. Your job as founder is to survive the trial. You must make sure that you do not get executed. The boardroom is not the only place where things can go wrong, of course. But it is typically where things go wrong internally, and most fatal wounds come from internal, not external conflict.

Even something as seemingly innocuous as holding the title of CEO may actually be quite dangerous. Maybe you can figure out ways to minimize it. Augustus never said he was king. It was dangerous to be a king after Brutus killed Caesar. So Augustus was just the “first among equals.” Whether that equality was anything more than pure fiction, of course, is very questionable.

In October of 2000, things were pretty crazy at PayPal. The burn rate was \$10 million/month. There were about 4.5 months of runway left. When I returned as CEO, it wasn’t all of a sudden. I was the Chairman and came back as the interim CEO. We went through a 6-7 month process of trying to find a permanent one. The one decent candidate that we found sort of didn’t work out. Things were going well, so the board agreed to have me be CEO. But the company was about to go public, so the board insisted that there be a Chief Operating Officer (COO) too. COO, of course, is code for the #1 replacement candidate for CEO—it’s like the Vice President in U.S. politics, only more adversarial. I was able to convince the board to make David Sacks COO, which was probably a good, safe move since David was perceived to be crazier than I was. Thinking carefully about these things can lead to powerful insurance policies against getting deposed or executed at trials board meetings.

The dual founder thing is worth mentioning. Co-founders seem to get in a lot less trouble than more unbalanced single founders. Think Hewlett and Packard, Moore and Noyce, and Page and Brin. There are all sorts of theoretical benefits to having multiple founders such as more brainstorming power, collaboration, etc. But the really decisive difference between one founder and more is that with multiple founders, it’s much harder to isolate a scapegoat. Is it Larry Page? Or is it Sergey Brin? It is very hard for a mob-like board to unite against multiple people—and remember, the scapegoat must be singular. The more singular and isolated the founder, the more dangerous the scapegoating phenomenon. For the skeptic who is inclined to spot fiction masquerading as truth, this raises some interesting questions. Are Page and Brin, for instance, really as equal as advertised? Or was it a strategy for safety? We’ll leave those questions unanswered and hardly asked.

Hewlett & Packard



Moore & Noyce



Page & Brin



D. Return of the King

The return of the founder is not to be underestimated. Apple is the paradigmatic example. There were 12 crazy years from 1985 to '97. There were very conventional CEOs. They couldn't figure out anything new to build. Obviously there was something very powerful in bringing the founder back; from 1997-2011 Apple changed course entirely and had an incredibly powerful arc.

The options backdating scandal has been relegated to a minor footnote in the Apple mythology. Apple stock kept going up, and the board kept backdating options grants, giving Steve Jobs a fairly big windfall.

2001 stock options backdating scandal

Apple out of court settlement: \$14M

Criminal prosecution: none

"I wanted respect, not backdated options."

-Steve Jobs

It probably wasn't just building great products or being a good insider that saved Steve Jobs. His being terminally ill part was probably a very important variable. There is much less power in scapegoating someone who's power—indeed, whose life—is waning anyways.

I met Steve Jobs once, at Marc Andreessen's wedding in 2006. He was already very frail then. At 9 pm, he got up from the table and announced that he had to get back to the office to work. One couldn't help but wonder: Was this real? Was Jobs really working this hard? Or was it an excuse? Maybe he was just bored talking to me.

Resurrections are possible. But you can only be resurrected after you die. Founders should think carefully about how to preserve the original founding moment for as long as possible. The key is to encourage and achieve perpetual innovation. It is very important to avoid, or at least delay, the shift to a horrible bureaucracy where no one can do anything and everyone is circumscribed.



"It was the greatest comeback since Lazarus."

-The Guardian

The usual narrative is that society should be organized to cater to and reward the people who play by the rules. Things should be as easy as possible for them. But perhaps we should focus more on the people who *don't* play by the rules. Maybe they are, in some key way, the most important. Maybe we should let them off the hook.

Peter Thiel's CS183: Startup - Class 19 Notes Essay

Here is an essay version of my class notes from the last class of CS183: Startup, class 19. Errors and omissions are mine.

The following three guests joined the class for a discussion:

1. [Sonia Arrison](#), tech analyst, author of [100 Plus: How the Coming Age of Longevity will Change Everything](#), and Associate Founder of Singularity University
2. [Michael Vassar](#), futurist and President of the [Singularity Institute for the study of Artificial Intelligence \(SIAI\)](#)
3. [Dr. Aubrey de Grey](#), gerontology expert and Chief Science Officer at the [SENS Foundation](#).

Credit for good stuff goes to them and Peter, who gave the closing remarks. I have tried to be accurate. But note that this is not an exact transcript.

Class 19 Notes Essay—Stagnation or Singularity?

I. Perspectives

Peter Thiel: Let's start by having each of you outline your vision of what kinds of technological change we might see over the next 30 or 40 years.

Michael Vassar: It's lot easier to talk about what the world will look like 30 years from now than 40 years from now. Thirty seems tractable. Today, we've gone from knowing how to sequence a gene or two to thousand-dollar whole genome sequencing. Paul Allen is running a \$500 million experiment that seems to be going very well. This technological trajectory is both exciting and terrifying at the same time. Suppose, after 30 years, we have a million times today's computing power and achieve a hundred times today's algorithmic efficiency. At that point we'd be in a place to simulate brains and such. And after that, anything goes.

But this kind of progress over the next 30 years is by no means something we can take for granted. Getting around bottlenecks—energy constraints, for example—is going to be hard. If we can do it, we're at the very end. But I expect that there will be a lot of turmoil along the way.

Aubrey de Grey: We have a fair idea of what technology might be developed, but a much weaker idea of the *timeline* for development. It is possible that we are about 25 years away from escape velocity. But there are two caveats to this supposition: first, it is obviously subject to sufficient resources being deployed toward the technological development, and second, even then, it's 50-50; we probably have a 50% chance of getting there. But there would seem to be at least a 10% chance of *not* getting there for another 100 years or so.

In a sense, none of this matters. The uncertainty of the timeline should not affect prioritization. We should be doing the same things regardless.

If you look at certain AI approaches, you conclude that you need both a great understanding of how the world works and a lot more computing power to pull them off. But they are worth pursuing even at a 10% chance of success in the next 30 years. We should be sympathetic toward giving very difficult approaches the time of day. Orchestrating the development of technology is not easy. It's a process of sidestepping ignorance and planning to manipulating nature

based on an incomplete picture of nature to begin with. Achieving pure transcendence—and *when*—is so speculative that it's probably not worth talking about in real probabilistic terms. But our priorities should be the same: develop radical technology in biotech, computation, hardware, etc.

Sonia Arrison: I spend most of my time looking at biotech, so I'll talk about the biotech slice first. It is clear that biology is quickly becoming an engineering problem. I got interested in biotech several years ago when my CS friends started picking up biology books. They thought, probably accurately, that the next big thing in coding would be bio, not computers. This is now a mainstream view. Bill Gates has said something like this, along with several others. Great hackers go into biotech. In 30 or 40 years, the bio-as-engineering paradigm could make the world look radically different. There is a sense in which genomics is moving faster than Moore's law. Prices are falling; the first human genome sequencing was around \$3 billion. Now it can be done for around \$1,000. There is work being done on a genomic compiler, which would make it easier to hack all sorts of organisms' genomes, which would in turn open up all kinds of possibilities.

The big complaint right now is that, despite the fact that the first draft of the human genome was sequenced in 2000, twelve years later not that much has actually happened in terms of new treatments or cures based on the technology. . This criticism is weak because it misses an important point: for most of those 12 years genomic sequencing was so expensive that very few scientists could do the work they wanted to do using genomes. Of course, now that prices have fallen substantially, barriers are falling in a serious way. Things *will* happen—people *are* working on radical new things. Gene therapy promises to cure diseases. It's possible that we can develop new kinds of fuels. There is a Kickstarter project that involves taking an oak tree and splicing firefly genes into it. The end result would be trees that glow. More than just cool in its own right, maybe you could use those firefly trees to illuminate roads instead of streetlamps. That's awesome. And there is so much more that we can't even fathom right now. A lot can, and will happen at the nexus of bio and engineering.

In the short run and outside of biotech, the shift to online education seems like it will dramatically change how people learn. Things like the Stanford AI class, Udacity, the Kahn Academy—we don't know exactly how it will all play out, but it's safe to say that there are a lot of things to look forward to on this front.

Peter Thiel: Let's engage on the culture question: why do most people think you're crazy?

Michael Vassar: For whatever reason, having opinions about the future is seen as strange. Only a small minority of people forms opinions about the future—even the near future. Perhaps this is because thinking about the future is uncomfortable and kind of difficult. People prefer to work with models that involve one variable changes in linear trajectories, while everything else stays the same. We know that that's nonsense, of course; the world doesn't work like that. But it makes for easy conversation. Keeping the discourse at that simplistic level allows us to focus on one thing and work together today. Factoring in 100 variables would in some sense break that dynamic. But thinking about the future is very important, and right now that can be isolating. Diverging from people means that there are fewer people you can talk to. There are fewer connotations; people tend not to understand where you're coming from.

But this is not to say that people just have different beliefs than we do. Usually, they don't. You don't usually encounter *anti*-singularity views. Maybe some global warming people or apocalypse people are affirmatively anti-singularity. But most people aren't substantively engaging. What is perceived as crazy isn't the substance of the belief itself, but rather having the belief in the first place.

Aubrey de Grey: I disagree a bit. People do tend have some view of the future. They usually project relative stagnation. People tend to believe that, not only will most things not change, but what *will* change won't change very

quickly. People who criticize my views on biotech and aging, for instance, do not identify bad logical steps or seize on anything substantive. Rather, they choose not to believe what I'm saying because it conflicts with their bias toward stagnation. They walk away quite sure that the rate of progress in anti-aging and longevity technology will never accelerate. That is pretty striking.

I try and dispose of this by pointing out that if you were to ask someone in 1900 how long it would to cross the Atlantic in 1950, they would make a prediction drawing from ocean liner speed trajectories up to that point. They wouldn't be able to foresee the airplane. And so their calculation would be off by orders of magnitude.

Of course, everyone knows how much technological change has happened in the past few centuries and decades. Everyone knows what the Internet did in recent years. But there is a huge reluctance to apply any of this as precedent for what might or is likely to happen in the future.

There's also a desirability aspect to it. Fear of the unknown is such a deep-seated emotion. When people encounter a radical new proposition, they are biased to think that things will go way wrong. It is very hard for people to consider the reasonable likelihood of those scenarios unfolding, so they exaggerate risks. More rational aspects to the conversation go out the window.

Sonia Arrison: For the record, no one thinks that *I'm* crazy.

Peter Thiel: You're the best disguised...

Sonia Arrison: Well, "crazy" is a hard claim to make since I focus on actual technology that is grounded in reality. I write about tissue engineering, regenerative medicine, and biohacking, for instance. That exists now. And it's going to continue to develop and, I think, really change the world. There are three reasons that people sometimes have a problem with this stuff. First, they don't understand it. Second, they don't believe it. Third, they fear it.

Think about the firefly/oak tree street lamps for a second. Just the idea of that terrifies some people. It's completely different from how things are now. Some people respond with knee-jerk reactions: "Don't mess with nature!" "Don't play God!" This reaction is understandable, but it stands in the way of progress. It's not the best reaction. In a lot of ways it doesn't really make sense.

Peter Thiel: Is the best approach to ignore those people, then?

Sonia Arrison: Better than ignoring them is trying to educate them. It is important to explain things clearly. Technology that people do not understand looks a lot like magic sometimes. And Magic is scary. But if you distill and explain—"this is x, this is what it does"—you can sell them on it. It's just a matter of clearly communicating the benefits vs. the costs. "This will drive out dirty fossil fuels," for example, might be one persuasive line of argument in favor of the firefly/tree hybrid street lamps.

Peter Thiel: There's a compelling case that we'll very likely see extraordinary or accelerated progress in the decades ahead. So why not just sit back, grab some popcorn, and enjoy the show?

Another cut at the question is this: In Kurzweil's *The Singularity is Near*, progress follows an exponential growth curve. It's a law of nature. In a sense, the singularity is happening regardless of what individual people actually do to make it happen. The assumption was that there will always be enough people who try things, so you, as an individual, don't actually have to do anything and you can just wait for things to happen. Is there anything wrong with that argument?

Aubrey de Grey: Yes, there is. It doesn't only matter *that* these technologies are developed. *When* they are developed is hugely important as well. Take anti-aging science, for instance. Very close to 150,000 people die everyday. About 100,000 of these daily deaths are aging-related. (Probably about 90% of deaths in Western countries are aging-related). So each day that you don't delay saves 100,000 lives. From this perspective, it doesn't matter how inevitable the singularity is. Inevitable is cold comfort to the people losing their lives or loved ones *now*. We want the defeat of aging by medicine as soon as possible, for the simple reason that more suffering is alleviated the sooner we achieve it.

Michael Vassar: I strongly agree. It is important to work toward making good effects happen, and avoiding bad things. Inevitability can cut both ways; sometimes you want it to happen, if the effects are good, but sometimes you *don't* want certain things to happen. Focusing just on inevitability misses other important pieces. If death is or seems inevitable and we are basically dead in the long run, there is still some chance at survival, and we should give it a damn good fight.

Besides, popcorn is bad for you. Though I guess Aubrey might figure out a way to make it not so bad for you...

Sonia Arrison: Focusing on inevitability alone is dangerous because it allows people to get complacent about bad systems in place. People might ignore the many perverse incentives that often thwart or frustrate the many scientists working on radical technologies. Too few people are thinking about how the FDA might be blocking very important developments. If it's all going to happen anyway, there's less of a sense that it is important to reform what we have now so we can better realize our goals. But of course that kind of reform is terribly important, and it won't happen if we don't work towards it.

Peter Thiel: So who do you think is going to do this? Who is going to forge the technological future?

Michael Vassar: You. [laughter...]

Peter Thiel: [pause] Michael... you're supposed to be motivating the *people in this class...*

Michael Vassar: But I'm serious. It's a short list of people. You, Elon, Sean...

Sonia Arrison: My take is that innovation comes from two places: top-down *and* bottom-up. There's a huge DIY community in biology. These hobbyists are working in labs they set up in their kitchens and basements. On the other end of the spectrum you have DARPA spending tons of money trying to engineer new organisms. Scientists are talking to each other from different countries, collaborating on synthetic bio projects. All this interconnectedness matters. All these interactions in the aggregate will bring the change.

Aubrey de Grey: I disagree. My answer is Oprah Winfrey.

Yes, there are a few people like Peter. There are a very few visionary people who can make a real difference at the formative early stage. But there are also many people with Peter's net worth who aren't doing this. It's not that these people don't understand the issues or the value of technology. They understand these things very well. But they are held back by social opinion. They probably can't articulate this well to themselves, let alone to others. But they face viscerally emotional blockades that the people around them erect. Just because you're rich doesn't mean you don't fear people laughing at you. Many potential visionaries are held back by little more than social pressure to conform.

This is why mainstream opinion formers are absolutely pivotal. Perhaps no other subset of people could do more to further radical technology. By overpowering public reluctance and influencing the discourse, these people can enable everyone else to build the technology. If we change public thinking, the big benefactors can drive the gears.

Michael Vassar: I do not think that progress will come from the top-down or from the bottom-up, really. Individual benefactors who focus on one thing, like Paul Allen, are certainly doing good. But they're not really pushing on future; they're more pushing on individual thread in homes that it will make the future come faster. The sense is that these people are not really coordinating with each other. Historically, the big top-down approaches haven't worked. And the bottom-up approach doesn't usually work either. It's the middle that makes change—tribes like the Quakers, the Founding Fathers, or the Royal Society. These effective groups were dozens or small hundreds in size. It's almost never lone geniuses working solo. And it's almost never defense departments or big institutions. You need dependency and trust. Those traits cannot exist in one person or amongst thousands.

Peter Thiel: That's three different opinions on who makes the future: a top-down bottom-up combo, social opinion molders, and tribes. Let's run with some version of Michael's tribe theory. Suppose it's just a small cabal of tech people that drives it.

Aubrey de Grey: I think the tribe argument is right. Michael is right that single people don't make the difference. There is too much infrastructure. Working in biology costs a fair bit of money. Developing algorithms can be quite costly too. Individuals have to fit themselves into the network of money flow, whether that network is entrepreneurial, philanthropic, or public funding. But the truly radical technology discussed in this class is so early that philanthropic support will probably play the largest role for awhile longer. That can change fast as these technologies advance and more people start to see the commercial viability. When public opinion changes, the people who want to get elected will fund the things that people want, and we'll start to see more funding for these things.

Sonia Arrison: In some sense asking for a single source of progress is the wrong question. It can come, and almost always *does* come, from lots of places. Things are interconnected. Ideas build on top of each other, and often ideas that once seemed unrelated can come together later on.

Question from the audience: We know that progress has happened in the past. But fairly rarely did that progress look like what people were expecting beforehand. So how do you know that your claims as to how progress is going to happen in the future are right? What do you make of the line that "most discussion about the future is either fantasy or bullshit"?

Michael Vassar: People used to predict the future in a pretty determinate way. Suppose you're looking for oil. That involves making fairly concrete predictions: there is x amount of oil at y place, and it will last z number of years.

People have largely stopped doing that. Recent science fiction is a bit more on point than the science fiction of old. It used to be hard to predict the distant future. It may be that it's actually quite easy to predict what the late 2020s look like, relative to what it used to be. But it is unusually hard to make any statement about 2040.

People were much better at predicting the future before movies and mass media. The tools were logic and trend analysis, not what looked cool on the big screen. Modern forecasts of the future are often more about looking credible than about making reasonably accurate predictions.

Consider things like Neal Stephenson's *Snow Crash*—some very good abstraction there, somewhat satirical. There are lots of details that probably aren't going to play out like that in the actual 2020's. But we can think of them as being about as reasonable as Kurzweil's descriptions of possible future technology.

Sonia Arrison: The question basically says, "Well, a lot of people were wrong about the future in the past, so we shouldn't talk about it now." That's nonsense. Yes, people will be wrong. But we're not talking pie-in-the-sky guesses about the future. We're talking about what is here now, and reasonably extrapolating from that. This isn't science fiction. Gene splicing and gene therapy exist. We can create living code, as Craig Venter demonstrated. The questions are how long will this take and how fast can we go. These are difficult questions to answer. But that doesn't mean we can't think about them. We *should* think about them. That people have various perspectives doesn't invalidate the project.

Question from the audience: Will the future be a science problem or engineering problem?

Aubrey de Grey: We are right in the middle at this point. In medicine and computation, for instance, we are seeing a shift from inherently exploration-based, science-based perspectives to engineering perspectives.

Michael Vassar: Science matters much more than engineering does. But it's easier to talk about engineering. So one should use engineering to discard the 99.9% of people who have no clue what's going on. But then one should get into the science with the remnant. *That* is where the upside will come from.

Sonia Arrison: There is also a knowledge aggregation problem. It is hard or impossible for one human brain to know everything. So people don't know what other people are doing, and they sometimes work on overlapping or redundant things. To the extent computers can better organize knowledge, people's efforts will be further streamlined, whether they are scientific or engineering-focused.

Question from the audience: On the hardware side, Moore's Law seems like it's going to continue to hold. But on the software side, the process of software engineering and collaboration seems to be improving only linearly. Is there a leveragability problem or some hidden limit there?

Michael Vassar: Linear growth in capabilities can get you over key hurdles. There is a feedback loop. Linear growth can be enough for you to nail down a process, leverage it, and get positive feedback to face transitions that *then* have the exponential growth arcs. And then you're back to growing linearly.

This is true for probably all of psychology and for AI (which is essentially psychology-as-engineering).

Peter Thiel: We know that, in practice, timing is very important. So while we don't know exactly when radical technology of the future will come to be, the timing does make a great deal of difference. If it's all crazy science fiction that's barely plausible, it might not make sense to work on it now. That would be like the Chinese man who tried to launch a rocket into space in the 11th century. No one was or should have been working on supersonic flight in the Middle Ages. That would be paddling way to far in advance of the wave.

Aubrey de Grey: I'm not sure the timing question is so critical. There must always be stepping-stones to an eventual goal. In the 11th century, the goal may have been to travel to the moon. But the technology then only permitted, say, a

prospective space traveler to get one foot off the ground. So at that time, you'd get the equivalent of your PhD if you could make a system that got you 10 feet off the ground.

The question is thus which trajectories will lead toward the ultimate goal and which ones will fail. We must identify the good trajectories and prioritize them. But without the long-term goal, you can't organize competing trajectories, and you'll never get there.

Peter Thiel: So perhaps a 20-year goal with lot of milestones along the way would be a good approach. The problem there is that too many milestones make the achievability of the end goal rather speculative.

Aubrey de Grey: You have to see that coming, and avoid the wrong turns.

There are also humanitarian reasons to set the sights large. We must remember that 100,000 lives are saved each day that the solution to aging comes quicker. In that light, 20 years is *dramatically better* than 21.

Sonia Arrison: People usually become deterred if a goal seems too hard or impossible. We can't expect everyone to be a tireless visionary. So showing traction is key. We can grow blood vessels and tracheas and bladders in the lab. So maybe we can get to hearts. The stepping-stones are key, since without them, fewer people will be as excited about the prospects of engineering new hearts.

Michael Vassar: The Apollo project was a tremendous 10-year project with lots of technological convergence. That was more than 40 years ago. At this point we probably can't even go to the moon anymore.

Framing the U.S. Constitution was an incredible accomplishment. The Founders had the knowledge to do that. They wrote for a particular socioeconomic and technological context. They didn't intend to write the end-all governing document for the entire world for all eternity. And yet, when we take over a Middle Eastern country today, we basically copy our Constitution. We have no idea how to do what our Framers did some 200 years ago. We've lost the ability to make such a culturally nuanced system. Applied history is underrated.

Question from the audience: No trend can run without running into limits. Where is the future asymptotic? When do we reach the limits of physical world? How long does the exponential part go, and when does it stop?

Michael Vassar: It's hard to say where it stops. Probably not for a good while; there's much more to be done. If something happens x times in a row, and no other variable is at play, one way to think about the chance of it happening again is to estimate it at $(x+1)/(x+2)$. It's a really crude technique, but can be quite useful too.

Aubrey de Grey: Kurzweil acknowledges that you get S-curves. But those curves tend to be replaced by new S-curves with each paradigm shift. Merge all those curves into one and you get a mega S-curve. Obviously there's only so far you can go within physical laws. But we're not hitting those problems yet.

Sonia Arrison: At some point, things decelerate. But that's okay. Necessity is mother of all invention. There will be other things to tackle. There will always be a new exponential curve.

Question from the audience: We at the Stanford Transhumanist Association are interested in open dialogue about the consequences of technological change, so we do a lot of research on how core emotions like fear or empathy come into play when people evaluate technology.

What do you think are the most effective ways to get people interested in and comfortable with transhumanist ideas?

Sonia Arrison: Sometimes it's possible to just appeal to the humanistic side. Certain aspects of transhumanism would, fully realized, alleviate lots of suffering. Some issues fit in that category pretty well. So if you frame it right, the conclusion becomes a no-brainer. No one wants net suffering.

Other things don't fit in that category as well. These are the things that just look radically different from the status quo—we might think they're cool, but that's not others' default. The emotional argument on these things is that people should be free to be individuals. But there can be a serious fear factor on freedom. Some people are generally scared of it. So the problem is much harder.

Michael Vassar: You could appeal to people's sense of wonder. If you've ever interacted with an Alzheimer's sufferer or someone who has a mental disability, you might have gotten a sense that they were missing something. Well, so are we. The gap between them and us is practically adjacency in the space of possibilities. We're probably missing out on a great many things. Shouldn't we try and fix things so we're missing less?

II. Closing Thoughts (from Peter Thiel)

This course has largely been about going from 0 to 1. We've talked a lot about how to create new technology, and how radically better technology may build toward singularity. But we can apply the 0 to 1 framework more broadly than that. There is something importantly singular about each new thing in the world. There is a mini singularity whenever you start a company or make a key life decision. In a very real sense, the life of every person is a singularity.

The obvious question is what you should do with *your* singularity. The obvious answer, unfortunately, has been to follow the well-trodden path. You are constantly encouraged to play it safe and be conventional. The future, we are told, is just probabilities and statistics. *You* are a statistic.

But the obvious answer is wrong. That is selling yourself short. Statistical processes, the law of large numbers, and globalization—these things are timeless, probabilistic, and maybe random. But, like technology, your life is a story of one-time events.

By their nature, singular events are hard to teach or generalize about. But the big secret is that there are many secrets left to uncover. There are still many large white spaces on the map of human knowledge. You can go discover them. So do it. Get out there and fill in the blank spaces. Every single moment is a possibility to go to these new places and explore them.

There is perhaps no specific time that is necessarily right to start your company or start your life. But some times and some moments seem more auspicious than others. Now is such a moment. If we don't take charge and usher in the future—if you don't take charge of your life—there is the sense that no one else will.

So go find a frontier and go for it. Choose to do something important and different. Don't be deterred by notions of luck, impossibility, or futility. Use your power to shape your own life and go and do new things.

