

# Blake Masters

"Your mind is software. Program it. Your body is a shell. Change it. Death is a disease. Cure it. Extinction is approaching. Fight it."

<http://blakemasters.tumblr.com/peter-thiels-cs183-startup>

## 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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April 03, 2012

## 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.

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### 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 17<sup>th</sup> century through about the late 1960's or so—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.

April 06, 2012

## 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—April 4—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 31<sup>st</sup>. 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 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 instrumentalism. 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—April 9—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 21<sup>st</sup> 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 11<sup>th</sup> 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

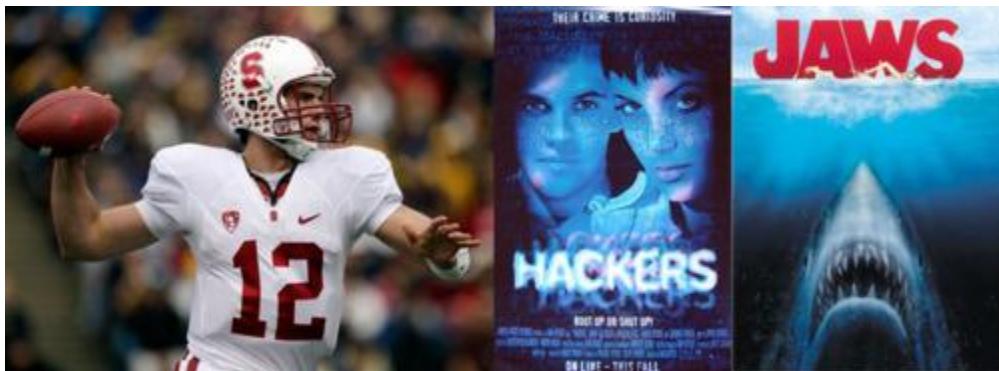
People also tell lies about markets. Really big markets tend to be very competitive. You don't want to be a minnow in a giant pool. You want to be best in your class. So if you're in a business that finds itself in a competitive situation, you may well fool yourself into thinking that your relevant market 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 19<sup>th</sup> 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 20<sup>th</sup> 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 20<sup>th</sup> 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 20<sup>th</sup> 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?

Tags: cs183

## 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—April 16—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 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 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.