elcome to CS183B. I am Sam Altman, I'm the President of Y Combinator. Nine years ago, I was a Stanford student, and then I dropped out to start a company and then I've been an investor for the last few. So YC, we've been teaching people how to start startups for nine years. Most of it's pretty specific to the startups but thirty percent of it is pretty generally applicable. And so we think we can teach that thirty percent in this class. And even though that's only thirty percent of the way there, hopefully it will still be really helpful.

We've taught a lot of this class at YC and it's all been off the record. And this is the first time a lot of what we teach is going to be on the record. We've invited some of our guest speakers to come and give the same talks they give at YC. We've now funded 725 companies and so we're pretty sure a lot of this advice we give is pretty good. We can't fund every startup yet, but we can hopefully make this advice very generally available.

I'm only teaching three. Counting YC itself, every guest speaker has been involved in the creation of a billion plus dollar company. So the advice shouldn't be that theoretical, it's all been people who have done it.

All of the advice in this class is geared towards people starting a business where the goal is hyper growth and eventually building a very large company. Much of it doesn't apply in other cases and I want to warn people up front, that if you try to do these things in a lot of big companies or non-startups, it won't work. It should still be interesting, I really think that startups are the way of the future and it's worth trying to understand them, but startups are very different than normal companies. So over the course of today and Thursday, I'm going to try to give an overview of the four areas you need to excel at in order to maximize your success as a startup. And then throughout the course, the guest speakers are going to drill into all of these in more detail.

Ideas, Products, Teams and Execution Part I

So the four areas: You need a great idea, a great product, a great team, and great execution. These overlap somewhat, but I'm going to have to talk about them somewhat individually to make it make sense.

You may still fail. The outcome is something like idea x product x execution x team x luck, where luck is a random number between zero and ten thousand. Literally that much. But if you do really well in the four areas you can control, you have a good chance at at least some amount of success.

One of the exciting things about startups is that they are a surprisingly even playing field. Young and inexperienced, you can do this. Old and experienced, you can do this, too. And one of the things that I particularly like about startups is that some of the things that are bad in other work situations, like being poor and unknown, are actually huge assets when it comes to starting a startup.

Before we jump in on the how, I want to talk about why you should start a startup. I'm somewhat hesitant to be doing this class at all because you should never start a startup just for the sake of doing so. There are much easier ways to become rich and everyone who starts a startup always says, always, that they couldn't have imagined how hard and painful it was going to be. You should only start a startup if you feel compelled by a particular problem and that you think starting a company is the best way to solve it.

The specific passion should come first, and the startup second. In fact, all of the classes we have at YC follow this. So for the second half of today's lecture, Dustin Moskovitz is going to take over and talk about why to start a startup. We were so surprised at the amount of attention this class got, that we wanted to make sure we spent a lot of time on the why.

The first of the four areas: a great idea. It's become popular in recent years to say that the idea doesn't matter. In fact, it's uncool to spend a lot of time thinking about the idea for a startup. You're just supposed to start, throw stuff at the wall, see what sticks, and not even spend any time thinking about if it will be valuable if it works.

And pivots are supposed to be great, the more pivots the better. So this isn't totally wrong, things do evolve in ways you can't totally predict. And there's a limit to how much you can figure out without actually getting a product in the hands of the users. And great execution is at least ten times as important and a hundred times harder than a great idea.

But the pendulum has swung way out of whack. A bad idea is still bad and the pivot-happy world we're in today feels suboptimal. Great execution towards a terrible idea will get you nowhere. There are exceptions, of course, but most great companies start with a great idea, not a pivot.

If you look at successful pivots, they almost always are a pivot into something the founders themselves wanted, not a random made up idea. Airbnb happened because Brian Chesky couldn't pay his rent, but he had some extra space. In general though if you look at the track record of pivots, they don't become big companies. I myself used to believe ideas didn't matter that much, but I'm very sure that's wrong now.

The definition of the idea, as we talk about it, is very broad. It includes the size and the growth of the market, the growth strategy for the company, the defensibility strategy, and so on. When you're evaluating an idea, you need to think through all these things, not just the product. If it works out, you're going to be working on this for ten years so it's worth some real up front time to think through the up front value and the defensibility of the business. Even though plans themselves are worthless, the exercise of planning is really valuable and totally missing in most startups today.

Long-term thinking is so rare anywhere, but especially in startups. There is a huge advantage if you do it. Remember that the idea will expand and become more ambitious as you go. You certainly don't need to have everything figured out in your path to world domination, but you really want a nice kernel to start with. You want something that can develop in interesting ways.

As you're thinking through ideas, another thing we see that founders get wrong all the time is that someday you need to build a business that is difficult to replicate. This is an important part of a good idea.

I want to make this point again because it is so important: the idea should come first and the startup should come second. Wait to start a startup until you come up with an idea you feel compelled to explore. This is also the way to choose between ideas. If you have several ideas, work on the one that you think about most often when you're not trying to think about work. What we hear again and again from founders is that they wish they had waited until they came up with an idea they really loved.

Another way of looking at this is that the best companies are almost always mission oriented. It's difficult to get the amount of focus that large companies need unless the company feels like it has an important mission. And it's usually really hard to get that without a great founding idea. A related advantage of mission oriented ideas is that you yourself will be dedicated to them. It takes years and years, usually a decade, to build a great startup. If you don't love and believe in what you're building, you're likely to give up at some point along the way. There's no way I know of to get through the pain of a startup without the belief that the mission really matters. A lot of founders, especially students, believe that their startups will only take two to three years and then after that they'll work on what they're really passionate about. That almost never works. Good startups usually take ten years.

A third advantage of mission oriented companies is that people outside the company are more willing to help you. You'll get more support on a hard, important project, than a derivative one. When it comes to starting a startup, it's easier to found a hard startup than an easy startup. This is one of those counter-intuitive things that takes people a long time to understand. It's difficult to overstate how important being mission driven is, so I want to state it one last time: derivative companies, companies that copy an existing idea with very few new insights, don't excite people and they don't compel the teams to work hard enough to be successful.

Paul Graham is going to talk about how to get startup ideas next week. It's something that a lot of founders struggle with, but it's something I believe you can get better at with practice and it's definitely worth trying to get better at.

The hardest part about coming up with great ideas, is that the best ideas often look terrible at the beginning. The thirteenth search engine, and without all the features of a web portal? Most people thought that was pointless. Search was done, and anyways, it didn't matter that much. Portals were where the value was at. The tenth social network, and limited only to college students with no money? Also terrible. MySpace has won and who wants college students as customers? Or a way to stay on strangers' couches. That just sounds terrible all around.

These all sounded really bad but they turned out to be good. If they sounded really good, there would be too many people working on them. As Peter Thiel is going to discuss in the fifth class, you want an idea that turns into a monopoly. But you can't get a monopoly right away. You have to find a small market in which you can get a monopoly and then quickly expand. This is why some great startup ideas look really bad at the beginning. It's good if you can say something like, "Today, only this small subset of users are going to use my product, but I'm going to get all of them, and in the future, almost everyone is going to use my product."

Here is the theme that is going to come up a lot: you need conviction in your own beliefs and a willingness to ignore others' naysaying. The hard part is that this is a very fine line. There's right on one side of it, and crazy on the other. But keep in mind that if you do come up with a great idea, most people are going to think it's bad. You should be happy about that, it means they won't compete with you.

That's why it's also not dangerous to tell people your idea. The truly good ideas don't sound like they're worth stealing. You want an idea where you can say, "I know it sounds like a bad idea, but here's specifically why it's actually a great one." You want to sound crazy, but you want to actually be right. And you want an idea that not many other people are working on. And it's okay if it doesn't sound big at first.

A common mistake among founders, especially first time founders, is that they think the first version of their product - the first version of their idea - needs to sound really big. But it doesn't. It needs to take over a small specific market and expand from there. That's how most great companies get started. Unpopular but right is what you're going for. You want something that sounds like a bad idea, but is a good idea.

You also really want to take the time to think about how the market is going to evolve. You need a market that's going to be big in 10 years. Most investors are obsessed with the market size today, and they don't think at all about how the market is going to evolve.

In fact, I think this is one of the biggest systemic mistakes that investors make. They think about the growth of the start-up itself, they don't think about the growth of the market. I care much more about the growth rate of the market than its current size, and I also care if there's any reason it's going to top out. You should think about this. I prefer to invest in a company that's going after a small, but rapidly growing market, than a big, but slow-growing market.

One of the big advantages of these sorts of markets - these smaller, rapidly growing markets - is that customers are usually pretty desperate for a solution, and they'll put up with an imperfect, but rapidly improving product. A big advantage of being a student - one of the two biggest advantages - is that you probably have better intuition about which markets are likely to start growing rapidly than older people do. Another thing that students usually don't understand, or it takes awhile, [is that] you can not create a market that does not exist. You can basically change everything in a start-up but the market, so you should actually do some thinking to be sure - or be as sure as you can be - that the market you're going after is going to grow and be there.

There are a lot of different ways to talk about the right kind of market. For example, surfing some one else's wave, stepping into an up elevator, or being part of a movement, but all of this is just a way of saying that you want a market that's going to grow really quickly. It may seem small today, it may be small today, but you know - and other people don't - that it's going to grow really fast.

So think about where this is happening in the world. You need this sort of tailwind to make a startup successful.

The exciting thing is the there are probably more of these tailwinds now then ever before. As Marc Andreessen says, software is eating the world. Its just everywhere, there are so many great ideas out there. You just have to pick one, and find one that you really care about.

Another version of this, that gets down to the same idea, is Sequoia's famous question: Why now? Why is this the perfect time for this particular idea, and to start this particular company. Why couldn't it be done two years ago, and why will two years in the future be two late? For the most successful startups we've been involved with, they've all had a great idea and a great answer to this question. And if you don't you should be at least somewhat suspicious about it.

In general, its best if you're building something that you yourself need. You'll understand it much better than if you have to understand it by talking to a customer to build the very first version. If you don't need it yourself, and you're building something someone else needs, realize that you're at a big disadvantage, and get very very close to your customers. Try to work in their office, if you can, and if not, talk to them multiple times a day.

Another somewhat counterintuitive thing about good startup ideas is that they're almost always very easy to explain and very easy to understand. If it takes more then a sentence to explain what you're doing, that's almost always a sign that its too complicated. It should be a clearly articulated vision with a small number of words. And the best ideas are usually very different from existing companies, [either] in one important way, like Google being a search engine that worked just really well, and none of the other stuff of the portals, or totally new, like SpaceX. Any company that's a clone of something else, that already exists, with some small or made up differentiator—like X, beautiful design, or Y for people that like red wine instead—that usually fails.

So as I mentioned, one of the great things about being a student is that you've got a very good perspective on new technology. And learning to have good ideas takes a while, so start working on that right now. That's one thing we hear from people all the time, that they wish they had done more of as a student.

The other is meeting potential cofounders. You have no idea how good of an environment you're in right now, for meeting people you can start a company with down the road. And the one thing that we always tell college students is that more important then any particular startup is getting to know potential cofounders.

So I want to finish this section of my talk with a quote from 50 Cent. This is from when he was asked about Vitamin Water. I won't read it, it's up there, but it's about the importance of thinking about what customers want, and thinking about the demands of the market. Most people don't do this—most students especially don't do this. If you can just do this one thing, if you can just learn to think about the market first, you'll have a big leg up on most people starting startups. And this is probably the thing we see wrong with Y Combinator apps most frequently, is that people have not thought about the market first, and what people want first.

So for the next section, I'm going to talk about building a great product. And here, again, I'm going to use a very broad definition of product. It includes customer support, the copy you write explaining the product, anything involved in your customer's interaction in what you built for them.

To build a really great company, you first have to turn a great idea into a great product. This is really hard, but its crucially important, and fortunately its pretty fun. Although great products are always new to the world, and its hard to give you advice about what to build, there are enough commonalities that we can give you a lot of advice about how to build it.

One of the most important tasks for a founder is to make sure that the company builds a great product. Until you build a great product, nothing else matters. When really successful startup founders tell the story of their early days its almost always sitting in front of the computer working on their product, or talking to their customers. That's pretty much all the time. They do very little else, and you should be very skeptical if your time allocation is much different. Most other problems that founders are trying to solve, raising money, getting more press, hiring, business development, et cetera, these are significantly easier when you have a great product. Its really important to take care of that first. Step one is to build something that users love. At YC, we tell founders to work on their product, talk to users, exercise, eat and sleep, and very little else. All the other stuff I just mentioned—PR, conferences, recruiting advisers, doing partnerships—you should ignore all of that, and just build a product and get it as good as possible by talking to your users.

Your job is to build something that users love. Very few companies that go on to be super successful get there without first doing this. A lot of good-on-paper startups fail because they merely make something that people like. Making something that people want, but only a medium amount, is a great way to fail, and not understand why you're failing. So these are the two jobs

Something that we say at YC a lot is that its better to build something that a small number of users love, then a large number of users like. Of course, it would be best to build something that a small number of users love, but opportunities to do that for v1 are rare, and they're usually not available to startups. So in practice you end up choosing the gray of the orange. You make something that a lot of users like a little bit, or something that a small number of users love a lot. This is a very important piece of advice. Build something that a small number of users love. It is much easier to expand from something that small number of people love, to something that a lot of people love, then from something that a lot of people like to a lot of people love. If you get right, you can get a lot of other things wrong. If you don't get this right, you can get everything else right, and you'll probably still fail. So when you start on the startup, this is the only thing you need to care about until its working.

[Audience member]: Can you go over that slide again?

So you have a choice in a startup. The best thing of all worlds is to build a product that a lot of people really love. In practice, you can't usually do that, because if there's an opportunity like that, Google or Facebook will do it. So there's like a limit to the area under the curve, of what you can build. So you can build something that a large number of users like a little bit, or a small number of users love a lot. So like the total amount of love is the same, its just a question of how its distributed. [audience laughter] And there's like this law of conservation of how much happiness you can put in the world, with the first product of a startup.

And so startups always struggle, with which of those two they should go. And they seem equal, right? Because the area under the curve is the same. But we've seen this time and again, that they're not. And that it's so much easier to expand, once you've got something that some people love, you can expand that into something that a lot of other people love. But if you start with ambivalence, or weak enthusiasm, and try to expand that, you'll never get up to a lot of people loving it. So the advice is: find a small group of users, and make them love what you're doing

One way that you know when this is working, is that you'll get growth by word of mouth. If you get something people love, people will tell their friends about it. This works for consumer product and enterprise products as well. When people really love something, they'll tell their friends about it, and you'll see organic growth.

If you find yourself talking about how it's okay that you're not growing—because there's a big partnership that's going to come save you or something like that—its almost always a sign of real trouble. Sales and marketing are really important, and we're going to have two classes on them later. A great product is the secret to long term growth hacking. You should get that right before anything else. It doesn't get easier to put off making a great product. If you try to build a growth machine before you have a product that some people really love, you're almost certainly going to waste your time. Breakout companies almost always have a product that's so good, it grows by word of mouth. Over the long run, great product win. Don't worry about your competitors raising a lot of money, or what they might do in the future. They probably aren't very good anyway. Very few startups die from competition. Most die because they themselves fail to make something users love, they spend their time on other things. So worry about this about all else.

Another piece of advice to make something that users love: start with something simple. Its much much easier to make a great product if you have something simple. Even if your eventual plans are super complex, and hopefully they are, you can almost always start with a smaller subset of the problem then you think is the smallest, and its hard to build a great product, so you want to start with as little surface area as possible. Think about the really successful companies, and what they started with, think about products you really love. They're generally incredibly simple to use, and especially to get started using. The first version of Facebook was almost comically simple. The first version of Google was just a webpage with a textbox and two buttons; but it returned the best results, and that's why users loved it. The iPhone is far simpler to use then any smartphone that ever came before it, and it was the first one users really loved.

Another reason that simple's good is because it forces you to do one thing extremely well and you have to do that to make something that people love.

The word fanatical comes up again and again when you listen to successful founders talk about how they think about their product. Founders talk about being fanatical in how they care about the quality of the small details. Fanatical in getting the copy that they use to explain the product just right. and fanatical in the way that they think about customer support. In fact, one thing that correlates with success among the YC companies is the founders that hook up Pagerduty to their ticketing system, so that even if the user emails in the middle of the night when the founder's asleep, they still get a response within an hour. Companies actually do this in the early days. Their founders feel physical pain when the product sucks and they want to wake up and fix it. They don't ship crap, and if they do, they fix it very very quickly. And it definitely takes some level of fanaticism to build great products.

You need some users to help with the feedback cycle, but the way you should get those users is manually—you should go recruit them by hand. Don't do things like buy Google ads in the early days, to get initial users. You don't need very many, you just need ones that will give you feedback everyday, and eventually love your product. So instead of trying to get them on Google Adwords, just the few people, in the world, that would be good users. Recruit them by hand.

Ben Silbermann, when everyone thought Pinterest was a joke, recruited the initial Pinterest users by chatting up strangers in coffee shops. He really did, he just walked around Palo Alto and said "Will you please use my product?" He also used to run around the Apple store in Palo Alto, and he would like set all the browsers to the Pinterest homepage real quick, before they caught him and kicked him out, (laughter) and so that when people walked in they were like "Oh, what's this?". This is an important example of doing things that don't scale. If you haven't read Paul Graham's essay on that topic, you definitely should.

So get users manually and remember that the goal is to get a small group of them to love you. Understand that group extremely well, get extremely close to them. Listen to them and you'll almost always find out that they're very willing to give you feedback. Even if you're building the product for yourself, listen to outside users, and they'll tell you how to make a product they'll pay for. Do whatever you need to make them love you, and make them know what you're doing. Because they'll also be the advocates that help you get your next users.

You want to build an engine in the company that transforms feedback from users into product decisions. Then get it back in from of the users and repeat. Ask them what the like and don't like, and watch them use it. Ask them what they'd pay for. Ask them if they'd be really bummed if your company went away. Ask them what would make them recommend the product to their friends, and ask them if they'd recommended it to any yet.

You should make this feedback loop as tight as possible. If your product gets 10 percent better every week, that compounds really quickly. One of the advantages of software startups is just how short you can make the feedback loop. It can be measured in hours, and the best companies usually have the tightest feedback loop. You should try to keep this going for all of your company's life, but its really important in the early days.

The good news is that all this is doable. Its hard, it takes a lot of effort, but there's no magic. The plan is at least is straightforward, and you will eventually get to a great product.

Great founders don't put anyone between themselves and their users. The founders of these companies do things like sales and customer support themselves in the early days. Its critical to get this loop embedded in the culture. In fact, a specific problem we always see with Stanford startups, for some reason, is that the students try to hire sales and customer support people right away, and you've got to do this yourself, its the only way.

You really need to use metrics to keep yourself honest on this. It really is true that the company will build whatever the CEO decides to measure. If you're building an Internet service, ignore things like total registrations—don't talk about them, don't let anyone in the company talk about them—and look at growth and active users, activity levels, cohort retention, revenue, net promoter scores, these things that matter. And then be brutally honest if they're not going in the right direction. Startups live on growth, its the indicator of a great product.

So this about wraps up the overview on building a great product. I want to emphasize again, that if you don't get this right, nothing else we talk about in the class will matter. You can basically ignore everything else in the class until this is working well. On the positive side, this is one of the most fun parts of building a startup.

So I'm going to pause here, we'll pick back up with the rest of this on Thursday, and now Dustin is going to talk about why you should start a startup. Thank you for coming, Dustin.

Why To Start A Startup

But yeah, Sam asked me to talk about why you should start a startup. There's a bunch of common reasons that people have, that I hear all the time for why you might start a startup. Its important to know what reason is yours, because some of them only make sense in certain contexts, some of them will actually, like, lead you astray. You may have been mislead by the way that Hollywood or the press likes to romanticize entrepreneurship, so I want to try to illuminate some of those potential fallacies, so you guys can make the decision in a clear way. And then I'll talk about the reason I like best for actually starting a startup, its very related to a lot of what Sam just talked about. But surprisingly, I don't think its the most common reason. Usually people have one of these other reasons, or, you know, they just want to start a company for the sake of starting a company.

So the 4 common reasons, just to enumerate them, are it's glamorous, you'll get to be the boss, you'll have flexibility, especially over your schedule, and you'll have the chance to have bigger impact and make more money then you might by joining a later stage company.

So you guys are probably pretty familiar this concept, when I wrote the Medium post, which a lot of you guys read a year ago, I felt like the story in the press was a little more unbalanced, entrepreneurship got romanticized quite a bit. The movie The Social Network came out, it had a lot of like bad aspects of what it like to be an entrepreneur, but mainly it painted this picture of like, there's a lot of partying and you just kind of move from like one brilliant insight to another brilliant insight, and really made it seem like this really cool thing to do.

And I think the reality is just not quite so glamorous, there's an ugly side to being an entrepreneur, and more importantly, what you're actually spending your time on is just a lot of hard work. Sam mentioned this, but your basically just sitting at your desk, heads down, focused, answering customer support emails, doing sales, figuring out hard engineering problems. So its really important that you go in with eyes wide open. And then its also quite stressful. This has been a popular topic in the press lately: The Economist actually ran a story just last week called "Entreupeneurs anonymous", and shows a founder like hiding under his desk, talking about founder depression. So this is a very real thing. Let's be real, if you start a company its going to be extremely hard.

Why is it so stressful? So a couple reasons. One is you've got a lot of responsibility. People in any career have a fear of failure, its kind of just like a dominant part of the part of the psychology. But when you're an entrepreneur, you have fear of failure on behalf of yourself and all of the people who decided to follow you. So that's really stressful. In some cases people are depending on you for their livelihood, even when that's not true, they've decided to devote the best years of their life to following you. So you're responsible for the opportunity cost of their time. You're always on call, if something comes up—maybe not always at 3 in the morning, but for some startups that's true—but if something important comes up, you're going to deal with it. That's kinda the end of the story, doesn't matter if you're on vacation, doesn't matter if its the weekend, you've got to always be on the ball and be in a place mentally where you're prepared to deal with those things. A sort of special example of this kind of stress is fundraising.

So a scene from The Social Network. This is us partying and working at the same time—somebody's spraying champagne everywhere—The Social Network spends a lot of time painting these scenes. Mark's not in the scene, the other thing they spend all their time on is painting him out to be a huge jerk.

This is an actual scene from Palo Alto, he spent a lot of time at this desk, head down and focused. Mark was still kinda a jerk sometimes, but in this more like fun lovable way, and not in a sociopathic, scorned lover way. So this is just him signaling his intention to just be focused and keep working, not be social.

So then there's the scene demonstrating the insight moment, it's kind of like out of A Beautiful Mind, they literally stole that scene. So they like to paint that scene and jump to these moments from other moments, with partying in between. But really we were just at that table the whole time. So if you compare this photo, Mark is in the exact same position but he's wearing different clothes, so this is definitely a different day. That's what it's actually like in person. I just covered this bullet; this is the Economist article I was talking about a second ago.

So another form of stress is unwanted media attention. So part of it being glamorous is you get some positive media attention sometimes, it's nice to be on the cover of Time and to be the Person of the Year. It's maybe a little less nice to be on the cover of People with one of your wedding photos. It depends on who you are, I really hate it, but when Valleywag analyzes your lecture and tears you apart, you don't want that, you definitely don't want that. Nobody wants that.

One thing I almost never hear people talk about is you're much more committed. So if you're at a startup and it's very stressful and things are not going well, you're unhappy, you can just leave. For a founder, you can leave, but it's very uncool and pretty much a black eye for the rest of your career. And so you really are committed for ten years if it's going well and probably more like five years if it's not going well. So three years to figure out it's not going well and then if you find a nice landing for your company, another two years at the acquiring company. If you leave before that, again it's not only going to harm yourself financially but it's going to harm all your employees. So if you're lucky and you have a bad startup idea, you fail quickly, but most of the time it's not like that.

I should say, I've had a lot of this stress in my own life, especially in the early years of Facebook, I got really unhealthy, I wasn't exercising, I had a lot of anxiety actually throughout my back, like almost every six months, when I was twenty-one or twenty-two, which is pretty crazy. So if you do start a company, be aware that you're going to deal with this. You're going to have to actually manage this, it's one of your core responsibilities. Ben Horowitz likes to say the number one role of a CEO is managing your own psychology, it's absolutely true, make sure you do it.

Another reason, especially if you're had another job at another company, you start to develop this narrative, like the people running this company are idiots, they're making all these decisions and spending all their time in these stupid ways, I'm gonna start a company and I'm going to do it better. I'm going to set all the rules.

Sounds good, makes a lot of sense. If you've read my media post, you'll know what's coming, I'll give you guys a second to read this quote:

People have this vision of being the CEO of a company they started and being on top of the pyramid. Some people are motivated by that, but that’s not at all what it’s like.

What it’s really like: everyone else is your boss – all of your employees, customers, partners, users, media are your boss. I’ve never had more bosses and needed to account for more people today.

The life of most CEOs is reporting to everyone else, at least that’s what it feels like to me and most CEOs I know. If you want to exercise power and authority over people, join the military or go into politics. Don’t be an entrepreneur.

-Phil Libin

This really resonates with me. One thing to point out is that the reality of these decision is nuanced. The people you thought were idiots probably weren't idiots, they just had a really difficult decision in front of them and people pulling them in multiple directions. So the most common thing I have to spend my time on and my energy on as a CEO is dealing with the problems that other people are bringing to me, the other priorities that people create, and it's usually in the form of a conflict. People want to go in different directions or customers want different things. And I might have my own opinions on that, but the game I'm playing is who do I disappoint the least and just trying to navigate all these difficult situations.

And even on a day to day basis, I might come in on Monday and have all these grand plans for how I'm going to improve the company. But if an important employee is threatening to quit, that's my number one priority. That's what I'm spending my time on.

A subset of You're the Boss is you have flexibility, you have control over your own schedule. This is a really attractive idea. So here's the reality:

If you're going to be an entrepreneur, you will actually get some flex time to be honest. You'll be able to work any 24 hours a day you want!

-Phil Libin

This truly resonates with me as well. Some of the reasons for this again, you're always on call. So maybe you don't intend to work all parts of the day, but you don't control which ones.

You're a role model of the company, and this is super important. So if you're an employee at a company, you might have some good weeks and you might have some bad weeks, some weeks when you're low energy and you might want to take a couple days off. That's really bad if you're an entrepreneur. Your team will really signal off of what you're bringing to the table. So if you take your foot off the gas, so will they.

You're always working anyways. If you're really passionate about an idea, it's going to pull you towards it. If you're working with great investors, you're working with great partners, they're going to be working really hard, they're going to want you to be working really hard.

Some companies like to tell the story about you can have your cake and eat it too, you can have like 4 days work weeks maybe, if you're Tim Ferris maybe you can have a 12 hours work week. It's a really attractive idea and it does work in a particular instance which is if you wanna actually have a small business to go after in each market then you are a small business entrepreneur, that makes little sense but as soon as you get past like 2 or 3 people you really need to step it up and be full-time committed.

You'll make more money and have more impact

This is the big one, the one I hear the most especially like candidates applying to a [?], they tell me "You know I'd really like to work for much smaller companies or start my own because then I have a much bigger slice of the pie or have much more impact on how that company does and I'll have more equity so I'll make more money as well". So let's examine when this might be true.

I'll explain these tables. They're a little complex but let's focus on the left first. These are just explaining Dropbox and Facebook, these are their current valuations and this is how much money you might make as employee number 100 coming into these companies especially if you're like an experienced, relatively experienced engineer, you have like 5 years of industry experience, you're pretty likely to have an offer that's around 10 base points. If you joined Dropbox couple years ago the upside you've already locked in is about $10M and there's plenty more growth from there. If you joined Facebook a couple years into its existence you've already made around $200M, this is a huge number and even if you joined Facebook as employee number 1000, so you joined like 2009, you still make $20M, that's a giant number and that's how you should be benchmarking when you're thinking about what you might make as an entrepreneur.

Moving over to the table on the right, these are two theoretical companies you might start. "Uber for Pet Sitting", pretty good idea if you're really well suited to this you might have a really good shot at building a $100M company and your share of that company is likely to be around 10%; that certainly fluctuates a lot, some founders have more than this, some founders have a lot less, but after multiple rounds of dilution, multiple rounds of option pool creation you're pretty likely to end up about here. If you have more than this I'd recommend Sam's post on equity split between founders and employees, you should be probably giving out more.

So basically if you're extremely confident in building a $100M, which is a big ask, it should go without saying that you should have a lot more confidence on Facebook in 2009 or Dropbox in 2014 that you might for a startup that doesn't even exist yet, then this is worth doing. If you have a $100M idea and you're pretty confident you can execute it I'd consider that.

If you think you're the right entrepreneur to build "Uber for Space Travel", that's a really huge idea, $2B idea, you're actually gonna have a pretty good return for that, you should definitely do that, this is also the value only after 4 years and this idea probably has legs, definitely go after that, if you're thinking of building that you probably shouldn't even be in this class right now, just go build that company.

So why is this financial reward and impact? I really think that financial reward is very strongly correlated with the impact we have on the world, if you don't believe that let's talk through some specific examples and not think about the equity at all.

So why might joining a late stage company actually might have a lot of impact, you get this force multiplier: they have an existing mass of user base, if it's Facebook it's a billion users, if it's Google it's a billion users, they have existing infrastructures you get to build on, that's also increasingly true for a new startup like AWS and all these awesome independent service providers, but you usually get some micro-proprietary technology and they maintain it for you, it's a pretty great place to start. And you get to work with a team, it'll help you leverage your ideas into something great.

So couple specific examples, Bret Taylor came into Google as around employee number 1500 and he invented Google Maps, that's a product you guys probably use everyday, I used it to get here and it's used by hundreds of millions of people around the world. He didn't need to start a company to do that, he happened to get a big financial reward, but the point is yet again massive impact.

My cofounder Justin Rosenstein joined Google a little later after Brett, he was a PM there and just as a side project he ended up prototyping a chat which used to be a stand-alone app, integrated in Gmail like you see in the upper right there and before he did that like you couldn't even think you could chat over Ajax or chat in the browser at all and he just kinda demonstrated it and showed it to his team and made it happen. This is probably a product most of you use almost everyday.

Perhaps even more impressively, shortly after that Justin left and became employee around 250 at Facebook and he led a hackaton project along with people like Andrew Bosworth and [?] to create the Like button, this is one of the most popular elements anywhere on the web, totally changed how people use it and then again didn't need to start a company to do it and almost certainly would have failed if he had tried because he really needed the distribution of Facebook to make it work.

So important to keep in mind the context for what kind of company you're trying to start and like where you will actually be able to make it happen.

So what's the best reason?

Sam already talked about this a little bit, but basically you can't not do it. You're super passionate about this idea, you're the right person to do it, you've gotta make it happen. So how does this break down?

This is a wordplay, you can't not do it in two ways. One is you're so passionate about it that you have to do it and you're going to do it anyways. This is really important because you'll need that passion to get through all of those hard parts of being an entrepreneur that we talked about earlier. You'll also need it to effectively recruit, candidates can smell when you don't have passion and there are enough entrepreneurs out there that do have passion so they may as well work for one of those! So this is table stakes for being an entrepreneur. Your subconscious can also tell when you don't have passion and that can be a huge problem.

The other way to interpret this is the world needs you to do it. This is validation that the idea is important, that it's going to make the world better, so the world needs it. If it's not something the world needs, go do something the world needs. Your time is really valuable, there are plenty of good ideas out there, maybe it's not your own, maybe it's at an existing company, but you may as well work on something that's going to be good.

The second way to interpret this is that the world needs you to do it. You're actually well suited for this problem in some way. If this isn't true, it may be a sign that your time is better spent somewhere else. But best case scenario if this isn't true, you outcompete the team for which it is true and it's a suboptimal outcome for the world and that doesn't feel very good.

So drawing this back to my own experience at Asana, Justin and I were reluctant entrepreneurs before we founded Asana, we were working at Facebook and we were working on a great problem. We would basically work all day long on our normal projects and then at night we would keep working on this internal task manager that was used internally at the company and it was just because we were so passionate about the idea, it was so clearly valuable that we couldn't do anything else.

And at some point we had to have the hard conversation of okay what does it mean if we don't actually start this company. We could see the impact it was having at Facebook, we were convinced it was valuable to the world. We were also convinced no one else was going to build it, the problem had been around a long time and we just kept seeing incremental solutions to it and so we believed if we didn't come out with the solution we thought was best, there would be a lot of value left on the table. We couldn't stop working on it and literally the idea was beating itself out of our chests and forcing itself out into the world. And I think that's really the feeling you should be looking for when you start a company, that's how you know you have the right idea.

I'll go ahead and stop there. I'll put some recommended books up here.

Thank you.

欢迎

欢迎来到CS183B。我是Sam Altman，我是Y Combinator的总裁。九年前，我还是斯坦福的一名学生，接着我辍学去创办了一家公司，后来我成为了一名投资人。所以在YC，我们已经指导人创业九年了。尽管其中大部分都是针对特定项目的，但仍然有30%是通用的。虽然这只是创业路上的30%，但仍然希望它能给你们带来帮助。

这堂课的内容我们在YC已经教过很多次，但从来没有被录下来过。这是第一次我们教的很多东西将被录下来。我们邀请了一些我们的演讲嘉宾来做和他们在YC做的相同的分享。YC已经投资了725家公司，所以我们确信这里大部分建议是相当好的。我们没法投资所有创业公司，当我们有希望让所有人都能接触到这些建议。

我只教三课。算上YC自己，每一个演讲嘉宾都曾经参与创办过一家十亿美金以上的公司。所以这些建议不会那么理论化，都是已经被实践过的。

这门课所给出的建议，是面向那些正在创建一门生意的人，他们的目标是高速成长并最终发展成一家大公司。这里许多建议并不能应用到其他场景中，所以我要提前警告大家，如果你打算把这些东西用到大公司或者非创业公司中去，它不会管用的。这是很有意思的，我确实认为创业就是未来，值得我们花工夫去理解它们，但创业公司和普通的公司又有很大的不一样。所以今天和周四的课上，我将要对创业关键的四个方面给出一个概述，来帮助你最大限度地取得创业成功。然后在整个课程中，演讲嘉宾们将会对这些方面进行更详细深入的阐述。

这四个方面就是：你需要一个伟大的创业点子，一个很棒的产品，一个杰出的团队，和完美的执行。尽管这些有部分是重叠的，但我不得不分开讲来把它们说清楚。

你可能仍然会失败。创业最终结果有点像：点子 x 产品 x 执行 x 团队 x 运气，其中运气是一个零到一万的随机数。真的是这样。但如果你在这四个方面做得非常好，你有一个不错的机会至少获得一定程度的成功。

有一个令人兴奋的事情就是，创业就像玩一样。当你年轻又缺乏经验时，你能做这事儿。当你年老又经验丰富时，你也能做这事儿。创业中我特别喜欢的一点就是，有些事情在其他工作场景下很糟糕，例如又穷又无知，但在创业的时候却是一笔巨大的财富。

在我们讨论怎么创业之前，我想先谈谈为什么要创业。对于开这门课我本来是有点犹豫的，因为我不希望你们为了创业而创业。有很多更简单的方式可以让你变得富有，每一个创业者都总说，他们完全没有想到创业是那么艰难那么苦逼。只有在你被一个问题困住，并且觉得创办一家公司是解决它的最好办法的时候，你才应该去创业。

必须现有明确的激情，然后才有创业。事实上，我们在YC的所有课都以此为前提。所以今天课程的后一半，我会交给Dustin Moskovitz，他会谈谈为什么要创业。这门课受到这么多的关注让我们都震惊了，所以我们要确保花了足够多的时间讨论为什么要创业。

四个方面中的第一个：一个伟大的创业点子。最近几年很流行说，点子不重要。实际上，花大把时间思考创业点子一点都不酷。你应该立刻开始，丢掉所有包袱，看是什么拖累了你，你甚至不用花时间想做这些有没有价值。

pivots挺好的，如果多来几次那更好了。所以这也不是完全错误的，事情会以我们完全预想不到的方式演化。所以当你还没有把产品送到用户手上前，你的了解是非常有限的。所以好的执行至少比好的点子重要十倍、难上一百倍。

但是这个事情已经有点脱离正轨了。糟糕的点子依旧是糟糕的，并且在这个现如今乐于创业的世界中，我们还依然感觉良好。基于糟糕的点子，即使有非常好的执行，你也无法拥有好的结果。当然，有很多例外，但是大多数伟大的公司是从一个伟大的点子开始的。

如果你看那些成功的案例, 它们通常都是从创始人自身的需求衍生的，而不是一个随机产生的想法。Airbnb的源起，是因为创始人Brian Chesky付不起他的房租，而且他还有空余房间。如果你回过头看看这些成功案例的过去的发展，通常来讲他们没可能变为大公司。 我个人呢，过去会认为点子其实不重要，但是我现在非常确信这是错误的。

点子的定义，像我们讨论的，是非常广泛的。它包括市场的规模和发展空间，公司的发展策略、保护策略等。当我们评估一个点子的时候，需要考虑所有这些方面，而不仅仅是产品本身。实现一个想法，你需要计划将未来十年的时间花费在这上面，这样你才有足够多的时间全面思考产品和公司的价值。即使计划本身是没有意义的，计划的执行却会非常有价值。这一点在当今的大多数创业公司中都缺失了。

长期思考虽然在其他领域非常少见，但是在创业公司中却存在着。进行长期思考具有非常多的好处。请记住，随着你的思考，你的点子也会变的更加宽泛且向各个领域延伸。你当然不用将你未来执行过程中的每一步都搞清楚，但你确实需要从一个核心点开始。并且，你需要以一些有意思的方式展开。

当我们查看各种点子的时候，有一点经常被创始人们忽略，那就是你需要建立一个别人难以复制的产品或业务。而这对于一个好点子来说非常重要。

我想要重申这个观点，因为这非常重要：先有点子，再有创业。当你拥有一个感觉必须要去探索的点子之后，再创业吧。这也可以用于选择好的点子。如果你有几个点子，选择那个你不在工作时在你脑海中出现最多的点子吧。我们从创业者听到的最多见的就是，他们希望他们当初并不那么着急创业，而是去等待他们最深爱的那个点子出现。

另外一种思考就是，大多数伟大的公司都有使命感。除非大公司有使命感，否则很难把握大公司业务需要的关注点的数量。而且，没有一个根本的伟大的点子也通常会很难。有使命感的好处就是你自己会全身心的投入。建立一个伟大的创业公司需要花费很多年，通常十年以上。如果你不热爱或者不相信你所建立的事业，你很可能在这过程中放弃。我觉得如果没有那种“目标必须实现”的信仰，你不可能度过创业过程中的痛苦与艰辛。很多创业者，尤其是学生，他们认为创业只需要花费2到3年，然后他们就可以继续做他们真正热爱的事情。这样是无法成功的。好的创业，通常要花费十年。

使命感的第三个好处是公司外部的人员会更愿意帮助你。相比复制其他项目而言，开始一个重要的且充满挑战的项目，你会得到更多的帮助。所以对于创业而言，开启一个充满挑战的创业项目，比开始一个简单的创业项目，反而会更容易。这属于与正常认知相反的情况，是需要很长时间理解的。很难讲拥有使命感到底有多重要，所以我想要最后一次强调：基本没有全新观点与视野的公司，就是那些复制其他已有点子的公司，不会使人们兴奋，也就不会使团队成员足够努力工作，从而难以获得成功。

Paul Graham下周会来讲讲如何获取创业点子。这是很多创业者纠结的地方，但我相信你会在实践中更好的掌握这点，而且这真的非常值得尝试。

获取伟大的点子中最难的部分，就是最好的点子在最开始会显得非常糟糕。第十三代搜索引擎，且没有任何网站门户功能？大多数人会觉得这个没有意义。搜索已经被做得差不多了，而且没有那么重要吧，门户才是价值所在。第十个社交网络，而且只提供给那帮没有消费能力的大学生？也是个糟糕的点子。MySpace已经成功了，谁想要大学生变成用户？反正怎么想都很糟糕。

这些点子听上去很糟糕，但是最后却很成功。如果它们起初听起来很酷，有可能就会有很多人都开始尝试做。正如Peter Thiel 将要在第五节课上讨论的那样，你会希望你的点子获得垄断地位。但是你不可能直接获取垄断。你必须找到一个小的市场来获得垄断然后迅速扩张。这就是为什么伟大的创业点子一开始看起来会很糟糕。如果你能这样说那就很好了：“今天，只有一小部分人会使用我的产品，但是我会获取这部分全部的用户。未来，几乎所有人都会使用我的产品。”

今后会经常提到的一个主题就是：你需要信仰、信念，并且愿意忽略其他所有人的反对意见。困难的部分在于，这会是一条非常狭窄的坚持之路。一边是正确，另一边是疯狂。但是你需要记住的是，如果你有一个伟大的点子，大部分人会觉得这是糟糕的。你应该为此感到高兴，这说明他们不会来和你竞争。

这就是为什么，告诉其他人你的想法并不危险。真正好的想法听上去并不会那么值得抄袭。你需要的是一个你能这样描述的点子：“我知道这听上去像一个坏点子，但它实际上它是一个伟大的点子，具体原因如下……” 你得听上去很疯狂，但是实际上又得是正确的。你得要一个其他很多人都不会去做的点子。而且，即使这个点子最初听起来不是个大生意，也没关系。

创业者中的一个普遍错误，尤其在初创者中，就是他们认为产品的第一个版本，也就是他们点子的第一个版本，需要听上去可以覆盖很大的市场。但其实不是。它只需要覆盖一小部分市场，并且从那里开始扩展。这是大多数伟大公司开始的方式。不流行但是正确，是你努力的方向。你需要一个听上去很糟糕的，但实际上又非常好的点子。

你也需要花时间去思考，市场如何进化。你需要一个十年内会发展很大的市场。现在大部分投资者会很在意市场规模，但他们并不思考市场以后会如何进化。

事实上，我认为这是投资者犯的最大的系统级错误之一。他们会思考创业公司本身的发展，却不思考市场的发展。我更在意市场的发展速度，而不是当前的规模。而且我还会在意这个市场会不会因为某种可能达到最大。你应该思考一下这点。我更喜欢投资的的公司，他们所专注的，是当前规模小但发展速度非常快的市场，而不是一个现在很大，发展却很缓慢的市场。

这些规模较小但发展快速的市场有一个极大的优势，那就是目标用户们通常极度渴望一个解决方案，因此，他们会选择一个不那么完美，但是快速成长的产品。作为学生的一个巨大的优势在于，你比年龄更大的那些人对哪些市场将要开始快速发展有更好的直觉，这也是学生的两个最大的优势之一。另一件事，通常情况下学生自己并不理解，或者需要花上一段时间才能理解的，那就是你不可能创造一个完全不存在的市场。在一家创业公司，你基本上可以改变任何事，但是唯独市场是无法改变的，所以你最好先仔细想想来确定你要进入的市场是真实存在且是会发展增长的，至少你要尽可能地确定这件事。

在形容一个对的市场的时候有很多种方式，比如，赶上其他人的浪潮，步入了一部上升的电梯，或者说成为一次变革中的一员，但这些都只是你想进入一个即将非常快速发展的市场的不同说法。这个市场今天看起来很小，也许现在它的确只是个小市场，但是你知道——而其他人都不知道——这个市场即将极为快速地发展起来。

所以，请仔细思考下，这件事正发生在世界上的哪个地方。你需要借着这股顺风的力来帮你把一个创业公司做成。

令人兴奋的事情是，当下这种顺风力可能要比以往任何时候都多。就像Mac Andreessen说的那样，软件正在吞噬世界。各个领域都充斥着太多伟大的点子了。你只需要从中挑一个，并且找到那个你真正特别关心的。

这个问题的另一个版本考虑的是相同的观点，也就是红杉资本的著名问题：为什么是现在？为什么现在是实现这个特别的点子的完美时间点，为什么要在现在来建立这样一个特别的公司，为什么两年前不做，而且为什么未来两年又会太迟？所有我们接触到的最为成功的创业公司，都有着非常毓秀（伟大）的点子，并且对这个问题有个完美的答案。如果你不是这样的，那你至少应该多多少少对这个问题心存下疑虑。

大多数情况下，创造些你自己需要的东西那是最好不过的了。这样你对产品的理解，要比你通过和一个目标用户聊天的方式来建立最初的版本强很多。如果你在开发的东西是自己不需要而是别人需要的，那你需要明白，你自己正处于一个巨大的弱势中，务必保证自己和你的用户要走的非常非常近。如果可以的话，尝试着在他们的办公室里工作，如果不行，那也要保证一天之中会和他们多次交谈。

另一件有过一个好点子的多少违反直觉的事情是，这些好点子基本上都是非常容易解释清楚并且非常容易去理解的。如果一句话不能解释清楚你在做的事情，那么这通常情况下是一个信号，你的点子太过复杂了。你的点子应该能够通过少数几个词来非常清楚明晰有条理的表达出来。一般情况下最好的点子会与现有公司大不相同，这种不同或者体现在某项非常重要的业务流程上，比如，Google作为搜索引擎要完爆其他的那些门户网站，又或者就是你的点子是全新的，比如SpaceX。任何一家公司，如果去克隆一些已经存在的东西，不管是做出一些小的业务变化或者在外观上做一些差异化——比如做出更漂亮的设计，或者专门服务一些喜欢红酒的人，他们通常情况下都会以失败告终。

所以如我所说，做一个学生最好的事之一就是你对新技术总是有着极好的观点。学会找到一个好点子需要花费一段时间，所以，你最好现在就立刻开始在这方面努力。我们总是能听到人谈起的就是，他们总希望他们还是学生的时候能多做一些事。

另一件极为不错的事情就是你可以发现一些潜在的联合创始人。你们已经意识不到你们现在处于的环境是有多么的好，你随处可以见一些你能和他一起开启一家公司的人。而有一件我们会经常和学校学生们说的事就是认识一些潜在的联合创始人远比任何一家独特的创业公司要重要。

所以我今天想用一段来自50 Cent（注：美国说唱歌手）的话来结束我这部分的演讲。这段话来源于他被问及Vitamin Water广告的时候。我不会读它，它就在那，但重要的是你要想清楚消费者要的是什么，并且想清楚市场的需求是什么。大多数人不做这个，尤其是大多数学生。如果你能把这一件事做了，如果你能紧紧学着实现考虑市场，你会领先其他开始创业的人一大步。不实现思考市场，不提前去思考人们想要的是什么，这大概也就是我们大多数情况下不看好一些YC应用的原因。

在下一节中，我将会讨论如何建立一个好的产品。在这里我要再一次用下一个较为宽泛的产品的定义。一个产品包括用户支持，你写下的产品解释，以及你的用户群与你为他们开发的东西之间产生的一切交互。

要建立一家伟大的公司，首先你必须将一个好点子变成一个好产品。这很难，但却至关重要，好在这个过程充满乐趣。虽然很棒的产品对世界而言都是推陈出新的，要给你在做什么东西方面给个建议也很难，但是新产品之间有着大量的共性，我们可以就如何创造新产品给你诸多建议。

作为一位创始人，最首要的任务之一就是确保公司能推出一个伟大的产品。直到你推出一项很棒的产品前，别的什么都不重要。每位真正意义上成功的创始人在讲述他们创业早年时光时，几乎都是坐在电脑前研究自己的产品或者和客户们进行交流。几乎所有的时间都在干这些。他们很少做别的事情，如果你的时间安排与此大不相同的话，那你应该仔细考虑下自己的时间安排了。当你有了一个好的产品之后，创始人们努力去解决的其它诸多问题，诸如融资、宣传、招聘、业务拓展等等都会变得容易很多 。所以，推出一样很好的产品才是你需要最先关注的。第一步就是创造一样用户们热爱的东西。在YC，我们让创业者们做的事情就是研究自己的产品、和用户交流、运动、吃饭和睡觉，别无其它。所有其它我刚刚提到的事情----诸如公关、会议、招聘顾问、寻求合作伙伴等，你都应该忽略掉。你所需要做的就是做成一样产品并通过和你的用户沟通，让你的产品尽可能变得更好。

你的工作就是创造一些用户热爱的东西。几乎没有一家公司可以在没有首先把产品做好的情况下还获得卓越成功的。很多只善于纸上谈兵的创始人的失败原因就在于他们仅仅做出了人们喜欢的产品。做出人们想要的东西，但仅仅只能到达让人喜欢的程度，这很大可能会导致失败，而这些创始人甚至都不明白自己为什么失败。所以说，喜欢和热爱是完全不同的两回事 。

我们在YC中经常说到一个观点，就是最好推出的是一小部分人热爱，大多数人之后会喜欢的产品。当然，如果能做出这种让大量用户热爱的产品是最好的了，但是实际上对于v1来说这种机遇是很少的，而且这些机会通常创业公司难以把握。所以现实中，你最终会折中处理这个问题。你们会推出一些大多数用户有点喜欢的东西，或者少数用户极度热爱的东西。这点建议很重要。推出一些少数人酷爱的东西。将一个少数人酷爱的产品推广到很多人喜欢，然后从大众喜欢再到很多人酷爱，会容易很多。如果你做对了，那么在其它很多事情上发生错误（也没有大问题）。如果你这点没有做对，那么即使你把其他所有的事情都做对了，你仍有可能失败。所以当你开始创业的时候，这就是你唯一需要做的事情，你必须坚持到底，直至见到成效。

【观众】：你能再放一遍那个幻灯片么？

所以在创业时你是有选择机会的。所有领域里最棒的事情就是推出一样很多人真正热爱的产品。而事实上，你一般实现不了这一点。因为一旦有这样的机会，Google或者Facebook就会去做。所以，就好像这条曲线下方的区域面积，也就是我们可以创造的东西，是有限的。所以，你可以选择创造大量用户还算喜欢的产品或者少数用户极度酷爱的产品。所以就想爱的总量是一定的，就看你怎么分配了。【观众笑声】这就是这次交谈的原则，通过你的创业中的首个产品，你能给这个世界带来多少欢乐。

所以创业者经常纠结于应该选择哪个目标。这两种目标实现的结果看起来是一样的，对吧？因为他们在这条曲线下方实现的面积是一样的。但是我们再来看一遍这个，他们其实是不一样的。如果你创造了某些人喜爱的东西，那么要推广这些东西则非常容易。你可以将这些东西推广成为其他很多人都喜爱的东西。但是如果你刚开始就抱着矛盾模糊的态度，或者投入的热情不够，还试图将产品推广。那么你的产品永远都不会得到很多人的喜爱。所以，我们的建议是：定位一小群使用者，并让他们爱上你的产品。

能让你意识到这种做法起作用的事，就是你的产品就会通过人与人的口耳相传而为人所知。如果大家喜爱你的产品，他们就会告诉自己的朋友。这对消费类产品和企业产品同样有效。当大家真的喜爱某样东西的时候，他们就会跟自己的朋友介绍这样东西，那时候你就会看到产品的有机增长了。

如果你发现自己认定会有强大的合作伙伴救你于水火或者有类似的解决麻烦的后路，故而认为产品销量没有增长也无妨的话-,这大抵是有真正麻烦的迹象 。销售和市场固然重要，我们稍后会有两堂课程来说。然而一样很棒的产品是却是保证长期增长的秘诀所在。你首先必须掌握这项秘诀。将推出一样很棒的产品这一工作推迟的做法并不会让一切变得更加容易。如果你试图在拥有一样能让一些人真正喜爱的产品前创建一个实现销售增长的机制。毫无疑问，你是在浪费时间。实现真正突破的公司几乎都有一个非常出色的产品，然后产品的知名度会通过口耳相传而增长。长时间后，很好的产品就会胜出。别去管你的竞争对手筹集了多少资金或者接下来要做什么。很可能他们根本就不是很好。很少有创业者是在竞争中被打败的。很多创业者失败是因为他们没有创造出用户喜爱的产品，他们把时间浪费在了其它事情上。所以，你应该担心的是这些。

要创造用户喜爱的产品的另一条建议是：从一些简单的东西入手。如果你能创造出简单的东西，那么要推出一样伟大的产品便会非常非常容易。即使你最终计划的产品超级复杂（我们也希望是这样）。你几乎都是从解决你认为最不起眼问题的小设备开始的。要创造一样出色的产品很困难，因此你应该从尽可能小的表面区域开始。想想那些真正成功的公司，他们刚起步的时候在干什么，然后想想那些你真心喜爱的产品。这些产品整体使用起来都极为简易，尤其是刚开始使用的时候。第一版本的Facebook简单得三岁小孩都会用。Google的第一版本也仅仅是一个只有一个文本框和两个按钮的页面；但是谷歌总能给你最好的搜索结果，这就是为什么使用者喜欢它的原因。Iphone用起来也要比其它比它更早推出的智能手机更为简单，所以它也成为了用户真正喜欢的第一部智能机。

商品简单可以带来好处的另一个原因是因为这可以迫使你把一件事做的极度好。你必须得这样做才能做出人们喜爱的产品。

在你们听成功创始人在讲述他们对自己产品的想法时，你们能经常听到“狂热”这个词。创始人们会谈及他们对于关注产品小细节的质量方面的嫉妒狂热。他们狂热的方面有：对说明自己产品如何定位正确的狂热，以及表达自己对客户支持态度的狂热。事实上，一个与YC公司成功相关的因素之一是这些公司的创始人将页面任务连接到他们票务系统中，这样一来，即使用户在半夜创始人睡着的时候发邮件，他们仍然可以在一个小时内收到回复。公司实际上在开始阶段会这样做。在产品不好的时候，公司的创始人会感觉浑身不自在，经常会半夜修复产品。他们不会推出垃圾产品，但如果推出了垃圾产品，他们也会很快将它修好。因此，要造出出色的产品，创始人对产品的痴迷也是有相当水平要求的。

你需要一些用户来帮助你完成反馈循环。但是你只能人工来找到这些用户---你应该亲自征募这些使用者。早期招募首批用户时，不要通过诸如购买Google广告之类这样的方式。你不需要很多使用者，你只需要一些能每天给你反馈信息并最终会喜爱你的产品的使用者。因此，不要通过Google广告词来寻找这些人。全世界范围内，你只需要那么几个能胜任的使用者。所以，亲自去寻找这些用户。

当所有人都认为Pinterest只是个笑话的时候，Ben Silbermann通过在咖啡店聊天的方式招募了他的第一批用户。他的确是这么做的。他当时就在Palo Alto转悠，然后跟别人说“请问你要不要试试我的产品？”他也跑遍了Palo Aloto的苹果店，并试图在店员发现他并把他赶出去之前尽快将所有的浏览器主页变成Pinterest。（笑声），这样一来，当人们进来的时候，他们就会问“哎呀，这是什么呀？”这是一个关于细节决定成败的典型例子。如果你还没有看过Paul Graham写的一篇关于该话题的论文，你绝对有必要看一下。

所以，亲自找一些用户，记住，你的目标是要让这一小群人喜欢你。要对这一小群人了解透彻，要和他们保持很密切的关系。听取他们的意见，你会发现几乎大多数情况下他们都愿意给你提供反馈意见。就算你是在为你自己创造商品，也去听听外部用户的意见，他们会告诉你怎样让你的商品变得让人们乐意去花钱买。尽一切努力让他们喜欢你，让他们知道你在干什么。因为他们也可能会帮助你推广你的产品，帮你找到下一批用户。

如果你想在公司内部设立一种机制来将用户的反馈转化为产品决策 。那么就按照用户的方式回复反馈信息。问问他们喜欢哪些地方，不喜欢哪些地方，并看看他们如何使用你的产品。问问他们觉得哪些地方花钱会比较值当。问问他们如果你的公司离开了，他们会不会真的觉得失望。问问他们哪些地方值得他们把产品介绍给他们的朋友。以及他们是否已经向朋友推荐了该款产品。

你应该尽可能保证这一反馈环节的紧密性。如果你的产品每周优化10%，这样的增加是很快的。软件创业的一个优势就是你可以将反馈环路制定得很短。可以以小时计算，最好的公司往往拥有最紧凑的反馈环路。你应该尽量让这一反馈环路在你公司的整个生命周期中都存在。然而，这在早期阶段尤为重要。

好在以上这些都是可行的。虽然这很难，也需要很多努力，但是这并不是什么不可思议的事情。至少计划的目的很明确，最终你将会创造出一件很出色的产品。

出色的创始人是不会让其他人介入他和用户之间的。这些公司的创始人在早期阶段连销售和客服工作都是亲力亲为的。将这一环节植入到企业文化中十分关键。事实上，我们经常看到的斯坦福创业中存在具体问题是，基于某些原因，学生试图直接雇佣人员来完成销售和客服工作，你必须要自己来干这些事情，这是唯一正确的做法。

你真的需要通过定量来让自己保持坦诚。公司也的确会按照CEO决定的任何标准来进行考核。如果你建立了一项网络服务，就不要在乎总注册量之类这样的东西-不要谈论它，也不要让公司里的任何人谈论-你需要做的是关注增长和活跃用户、活跃水平、留存率，收益和净推介值。这些指数才能说明问题。如果这些指数都没有按照理想的趋势发展，那么你要绝对坦诚。创业靠的是增长，只有增长才能说明一样商品是否出色。

所以，这就是的如何创造伟大产品的的所有内容。我想再强调一遍，如果你没有做到这些，我们所讲的其它东西都没什么意义。在这点起作用之前，你可以忽略课堂上讲的所有其它内容。积极点来讲，这可是创业过程中最有趣的部分。

所以在这里我将暂停一下。我们将在周二讲述剩下的部分。现在Dustin将讲一下为什么创业。欢迎Dustin。

为什么创业

耶……Sam让我讲讲为什么创业。我经常听到的人们关于创业的普遍理由数不胜数。知道你自己创业的原因很重要，因为别人的原因在某些环境下是合理的，有些实际上则会将你引上歧途。你可能已经被好莱坞或者媒体上的被浪漫化创业意图所误导，所以我将试着对以下可能的谬论进行说明，这样你们就会更清醒地做决定。首先我想说的是我最喜欢真正创业的理由，这个理由与Sam刚刚讲的很多内容相关。但是呢，我认为这并不是一个最普遍的理由。通常人们创业的理由都是这些其它理由中的一个，或者，你知道，他们只是为了开公司而开公司。

所以列举起来，有四个理由是比较吸引人的。那就是：你们将会成为老板；你们在各方面，尤其是在时间安排上将会更灵活；你们将有机会发挥更大的影响力，赚更多的钱之后你们可能会加入一家后期公司。

你们可能对这一概念会比较熟悉，当我写Medium Post的时候，你们当中很多人一年前就读过，我感觉在新闻界发生的这一故事多多少少有点失真。创业变得有些太过浪漫主义化。电影《社群网站》上映后，对于如何成为一名企业家这一观念产生了很多不好的影响。但是，电影主要刻画的场面像是，有很多场聚会，你周转其中，对所有的事物都具有敏锐的洞察力，让人觉得这样做真的很酷。

但我的认为是，现实很残酷。作为一名企业家，首先有丑陋的一面，然而更为重要的是，你们实际上会花大把的时间来做很多辛苦的工作。Sam刚刚也提到了这一点，你所做的基本上就是坐在桌前，集中注意力、埋头回复客服邮件、搞销售并解决困难的工程问题。因此，你睁大眼睛投入工作至关重要，当然这也会让你很有压力。最近在新闻界的热门话题是：经济学人上一周刊登了一篇名为“匿名企业家”的故事。这个故事讲述了一位创始人喜欢躲在自己的桌子底下，诉说创业者的压抑。所以，这是很真实的。大家都实际些，如果你要开一家公司，那将是非常非常艰苦的。

为什么会如此有压力呢？有两个原因。首先是你将担负很多责任。从事任何的职业都害怕失败，但这只是一种普遍的心理状态。但是当你成为一名企业家时，你对失败的恐惧不仅代表了你自己的恐惧，还代表了所有决定跟随你的人的恐惧。所以，这让你很有压力。有时候人们将自己的生计寄希望于你，即使不是这样，他们也决定将自己最好的年华贡献出来跟随你。所以，你要对他们的时间的时机成本负责。大家可能随时需要你，如果出现状况-----可能不会经常出现在凌晨三点，对于某些创业者来说的确是这样----但是如果发生什么重要的事情，你都要去解决。这大概就是这个故事的结局。不管你是在度假，还是在过周末，你要时刻留心，思想上要时刻准备着去处理一些诸如此类的事情。这种压力的特例就是融资。

《社交网络》中有这样一个场景：大家都在聚会，同时也在工作。有的人把香槟喷的到处都是-《社交网络》关于这样的场景很多。Mark没有出现在场景中，电影花了大量时间在想方设法将mark描绘成一个十足的混蛋

这是一个发生在Palo Alto的真实场景，他更多的时间是在这张桌子旁边，低着头，注意力集中。Mark有时候仍是一个坏蛋，但是他的这种坏更让人觉得有趣、萌萌哒，并不是一种反社会的、一个被鄙视的情人的那种坏。所以，这就是他意在集中精神，专注工作，不参与社交的写照。

然后，还有一幕展现了这一顿悟的时刻，有点像《美丽心灵》中，他们完全引用了那个场景。因此他们想描绘这一场景，并从这些时刻跳到另外一些时刻，中间都是在参加聚会。但事实上，我们一直都是坐在桌旁。所以，如果你比较这张图片，Mark的姿势从未变过，图片唯一的区别是，他每天都穿了不同的衣服。这就是我本人的实际感受。我就是这样的，这就是刚刚我谈论的经济学人刊载的那篇文章。

因此对创业者来说不受欢迎的媒体关注对他们来说是另一种压力。一般来说，只有得到媒体关注正面曝光时，才能让他们出名。很荣幸，我今年成为了《时代》的封面人物。但如果上《人物》封面的是你的婚纱照，那么也许就不太不适了。其实这取决于你是谁,我真的很讨厌这样,但是当Valleywag八卦你的讲座和让你泪奔时，你不想要这样,你绝对不希望这样。没人想要。

我几乎从未听人谈论他们自己是多么的负责。所以如果你在初创公司不开心,压力大，事情进展不顺，你可以离开。作为创始人来讲,你也可以离开,但这是极不冷静的做法,在你以后的事业发展中也会使你前程黑暗。如果顺利，你一定要挺住10年，如果不顺利也要尽量挺住5年。三年,是你判断公司运营不好的时间点，如果之后你为公司找到了好归宿，那么剩下的两年就要收购这家公司。在强调一遍，如果在这之前离开，你伤害的不止是公司的财政,而是你公司的所有员工。如果你幸运的话,你有一个糟糕的创业点子,你会迅速失败,大多数情况剧情并非如此发展。

因此，在我的生命力曾承受过很多类似的压力，特别是在Facebook刚刚创建时，那是我身体很糟糕，最没精力的时候。在过去的的日子里，实际上，几乎每六个月，我都会处于很焦虑的状态，二十一二岁是，最为严重。所以如果你确定要创建公司了，就一定要意识到你会面临这些，你一定要解决好身体的问题，这是你的责任心的核心。 Ben Horowitz习惯说CEO的首要任务是照顾好身体，这话说得非常对，一定要这样做。

另一个原因,特别是如果你在另一个公司还有工作,你开始这样想,比如说：运行这个公司里面的人都是傻子,他们事无巨细都要去讨论决定，而且把时间都花在了这些愚蠢的讨论和决定上,我要开一个公司一定能做得更好。我会设立所有的规章制度去优化。

听起来不错,很有道理。如果你读过我在媒体发表的文章,你就会知道会发生什么,现在给你们一秒钟读这句话:

那些有愿望想自己创建公司当CEO，站在金字塔尖的人，有些人是被地位驱使的，但并非所有人都是。 事实是：除了你的所有人都是老板，你的所有员工，顾客，伙伴，客户，媒体都是你的老板，以前从来没有像现在一样这么多人需要我去考虑。 大多数CEO的生活都对外曝光了，至少我和我熟知的CEO是这样的。如果你想对别人使用权利，那还是去参军或参政，别当老板。 -Phil Libin

这使我产生了共鸣，我想指出的是这些决定的事情很微妙。可能你认为是白痴的人并不白痴,只是在他们面前面临了非常困难的决定,人们的想法各不相同。所以作为CEO最寻常的事就是我不得不花费时间精力去解决其他人带给我的问题，而通常这些问题都是冲突的。人们想做不同选择，顾客想要不同的东西。也许在这方面我有自己的看法，但我的工作就是让那些最让我失望的人和难题都解决掉。

甚至在每天最基本的事情上，我都可以把如何完善公司的宏伟计划拿到周一去解决。但是如果是我的重要的员工辞职了，那就是我最重要的事了，我需要花时间处理它。

作为老板的一点好处就是你有绝对的自由，你可以自己制定自己的日程，这确实是个吸引人的想法。但事实是下面这样的。

Phil Libin  
如果你要成为老板，老实说你确实时间会比较灵活，一天24小时，你想什么时候工作都可以。 -- Phil Libin

这点上也让我很赞同。但我再强调一下，之所以这样的原因是，你要随叫随到，也许你并不想每天的全部时间都工作，但这并不是你能控制的了的。

你是公司的典范，这点非常重要。你可能身体有时好有时坏，有时你会身体特别差想休息几周，但很不幸，你是老板。你的团队只能感受到你在办公桌上带给他们的信号，如果你前腿走了，他们也会这么做。

无论如何，你都得工作。如果你的工作是由兴趣驱使的，兴趣就会拉动你工作，如果你是和牛逼的投资人、和一些牛逼的团队工作，他们会非常努力的干活，他们同样希望你努力工作。

许多公司喜欢讲这样的故事，你可以有好处并享受它，也许你可以一周只工作四天，如果你 是Tim Ferris，你还可以一周只工作12小时，多么吸引人的想法呀！但这只是特例，特例是说你研究完市场后只想运营一家小公司，当小公司的老板。但根本没意义，因为只要捏员工数达到两三人，你还是要建立全职的体系。

你会很有钱并且更有影响力

这是我听到的特别是像申请公司的候选人说到的最可能的一个。他们说：他们确实很想在小公司工作或自己创建小公司，但当我有更大份额或公司更有影响力时，我就会有更多的股份，那样就会赚更多的钱，所以如果真是这样，咱们就一起去研究下。

现在我解释一下这个表格，看起来有点复杂但咱们现在左侧的表。这些都是解释 Dropbox 和Facebook的，这些都是他们当前的估值,然后第二栏代表第100个加入的员工你能赚多少钱，特别如果你是有经验的或者说是相对有经验的工程师，譬如说是有五年工作经验，应该会拿到大约10基点左右的工作。如果是几年前来的Dropbox，你的薪资大概锁定在1000万美元，并且还会有很多上升空间。如果是几年前来的Facebook，你的年薪估计已经达到2亿美元了，这是个大数，就算你是第1000位员工，差不多2009年进入，你也还是拿到了2000万美元，那是个大数，当你思考你可能会作为一个企业家时你就会考虑这些方面。

再看右边的表,这是你可能遇到的两种理论。“宠物坐的Uber”,很不错的主意,如果你真的能做成这个，你可能会有一个很好的机会建立一个1亿美元的公司，你的公司的股份额可能会在10%左右,这方面幅度会很大，有的创始人多些有的创始人少些，但多次股权稀释,多次建立期权池后你很可能最终就拿到那些股份。如果你拥有比这些超出太多，我推荐你研究山姆的创始人和员工之间的关于股权分配的邮件，你应该给员工更多的激励。

所以，基本上来讲，如果你对赚1个亿十分有信心了，这其实是个很大胆的问题。毫无疑问，2009年的Facebook和2014年的Dropbox能让你也更有信心，即便你的创业公司还不存在，这也值得一试，如果你有赚1个亿的想法，你还有信心能做到，我会考虑投资。

如果你认为自己非常适合做“宇宙航行行业的Uber”，这是个伟大的想法，是一个值20亿美元的想法，而你也确实会得到相当丰厚的回报，你显然应该去做这件事。但这是四年后才会看到的价值，这种想法就像有腿一样，也会随之变化。如果你想做这件事，可能你都不应该现在在这上课，直接去创建公司吧。

为什么这件事情在经济上有回报，同时富有影响力呢？我真的觉得经济上的回报和我们对世界的影响力紧密相连，如果你不认同，我会给你举一些专门的例子，暂时先不考虑股权。

为什么会后期加入一个公司有更多的影响力呢 ？这得力于一个增益的机制:他们有的大量的用户基础,如果是Facebook就有十亿用户,如果是谷歌就有十亿用户,他们还有你要创建的基础设施,尽管初创公司AWS和一些小的牛逼的独立服务器可以满足你的需求,加入大公司你会经常得到一些已经打包好的辅助性技术,这是一个很好的起点。而当你开始和团队合作了,它将把想法变成更伟大的事情。

举几个具体例子，Bret Taylor在谷歌1500人时加入的，他开发了谷歌地图，这是你们几乎每天都用的产品，我用地图来到这，而其他人在全世界的各个角落用地图。他无需创建公司做这件事，他只需一笔巨额资金支持，但这个想法至今极具影响力。

不久我的协同创始人贾斯汀Rosenstein在Brett之后也加入了谷歌，他是那的产品经理，就因为一个次级的项目，他关停了一个聊天功能，这是个独立的APP，现在他把它整合在GMAIL里，就像你在右上角看到的那样，在他弄之前，你可能不能想象你能在Ajax或者在浏览器里聊天。而他做了demo，给团队展示并实现了。这个产品也是你们几乎每天都用的产品。

也许更令人印象深刻的是,贾斯汀离开后不久,他成为 Facebook大概第250位员工，他和安德鲁·博斯沃思这些人创建hackaton项目创造了“Like”这个按钮,如今这是网络上最流行的元素之一,完全提升了人们的使用体验,然后这又是不需要创立单独公司去做的项目，如果做了也肯定会失败，因为它的成功是依托于Facebook 的基础。

所以重要的是要记住，你想要创立公司的目的是什么，你用什么方式能把它实现。

那么最好的理由到底是什么？

Sam已经对这个话题讨论了一些，但是基本来讲你有几件事是不得不做的，你对这个好点子极度有热情，你是做这事适合的人，你会让它运转，那么到底是哪出现了问题？

这是个文字游戏，有两件事作为创始人你是必须坚持的，一件是你必须富有激情，无论如何你都会去完成这件事，这很重要，因为热情会使你在创业早期度过很多难关，你还需要办事有效率的员工，他们能够在你干不动的时候感觉到，周围还有很多充满激情的创业团队，员工们也可以离开你去为他们工作。这个是企业家的基础之一，如果你潜意识的状态能时刻被大家察觉，告诉大家你没有激情，那么问题就大了。

另一种理解方式是，世界需要你这么做。想法很重要，这点无可厚非。它能让世界变得更好，所以世界需要好的想法。

如果这些不是世界需要的东西，我们就该去做一些世界需要的东西，你的时间真的很宝贵，每天都会产生巨多好的想法，也许不是你的，也许是公司现有的，你只要把精力花在好的事情上面。

第二种理解方式是，世界必须要你去做这个，实际上，在某方面你是非常适合做这事的，但如果不是这样的，就可能代表你的时间做好去做其它事情。最好的场景是，如果你并不是最适合做这件事的，但你击败了最适合做这件事的团队，这仍然是对于我们的世界来说次优的结果，尽管可能不是那么好而已。

所以回想一下我在Asana的经验。我和贾斯丁在创建Asana前都不愿做老板，我们在Facebook工作，我们在做超棒的项目，我们基本上整白天都在做日常的项目，晚上我们会继续和项目负责人一起工作尝试新的想法，只因我们对这个想法有热情，这点子太有价值以至于我们都不愿意做其它事儿。

在某种意义上，如果不去开公司这意味着什么是需要我们去好好讨论的，我们能看到Facebook的影响，我们确信这对世界是有意义的，我们也确定没人会做这件事，因为这个问题存在很长时间了，我们一直能看到对这个问题的微创新改进方案，所以我们相信，如果我们没有做出我们认为最好的解决方案，会有很多价值没有被发掘。 我们不能停止，这个想法像有生命一样一定要让自己出生、展现在世界眼前，我想，当你想创立一家公司的时候，这应该是你需要去寻找的一种感觉，通过这种感觉你才知道，你的想法是不是对的。

之后我会接着讲在那有个中止，我会把推荐书目写在这下面。 谢谢