Startup Guide 101

How to Get Startup Ideas

The way to get startup ideas is not to try to think of startup ideas. It's to look for problems, preferably problems you have yourself. The very best startup ideas tend to have three things in common: they're something the founders themselves want, that they themselves can build, and that few others realize are worth doing. Microsoft, Apple, Yahoo, Google, and Facebook all began this way.

PROBLEMS

Why is it so important to work on a problem you have? Among other things, it ensures the problem really exists. It sounds obvious to say you should only work on problems that exist. And yet by far the most common mistake startups make is to solve problems no one has. I made it myself. In 1995 I started a company to put art galleries online. But galleries didn't want to be online. It's not how the art business works. So why did I spend 6 months working on this stupid idea? Because I didn't pay attention to users. I invented a model of the world that didn't correspond to reality, and worked from that. I didn't notice my model was wrong until I tried to convince users to pay for what we'd built. Even then I took embarrassingly long to catch on. I was attached to my model of the world, and I'd spent a lot of time on the software. They had to want it! Why do so many founders build things no one wants? Because they begin by trying to think of startup ideas. That m.o. is doubly dangerous: it doesn't merely yield few good ideas; it yields bad ideas that sound plausible enough to fool you into working on them. At YC we call these "made-up" or "sitcom" startup ideas. Imagine one of the characters on a TV show was starting a startup. The writers would have to invent something for it to do. But coming up with good startup ideas is hard. It's not something you can do for the asking. So (unless they got amazingly lucky) the writers would come up with an idea that sounded plausible, but was actually bad. For example, a social network for pet owners. It doesn't sound obviously mistaken. Millions of people have pets. Often they care a lot about their pets and spend a lot of money on them. Surely many of these people would like a site where they could talk to other pet owners. Not all of them perhaps, but if just 2 or 3 percent were regular visitors, you could have millions of users. You could serve them targeted offers, and maybe charge for premium features. The danger of an idea like this is that when you run it by your friends with pets, they don't say "I would never use this." They say "Yeah, maybe I could see using something like that." Even when the startup launches, it will sound plausible to a lot of people. They don't want to use it themselves, at least not right now, but they could imagine other people wanting it. Sum that reaction across the entire population, and you have zero users.

WELL

When a startup launches, there have to be at least some users who really need what they're making — not just people who could see themselves using it one day, but who want it urgently. Usually this initial group of users is small, for the simple reason that if there were something that large numbers of people urgently needed and that could be built with the amount of effort a startup usually puts into a version one, it would probably already exist. Which means you have to compromise on one dimension: you can either build something a large number of people want a small amount, or something a small number of people want a large amount. Choose the latter. Not all ideas of that type are good startup ideas, but nearly all good startup ideas are of that type. Imagine a graph whose x axis represents all the people who might want what you're making and whose y axis represents how much they want it. If you invert the scale on the y axis, you can envision companies as holes. Google is an immense crater: hundreds of millions of people use it, and they need it a lot. A startup just starting out can't expect to excavate that much volume. So you have two choices about the shape of hole you start with. You can either dig a hole that's broad but shallow, or one that's narrow and deep, like a well. Made-up startup ideas are usually of the first type. Lots of people are mildly interested in a social network for pet owners. Nearly all good startup ideas are of the second type. Microsoft was a well when they made Altair Basic. There were only a couple thousand Altair owners, but without this software they were programming in machine language. Thirty years later Facebook had the same shape. Their first site was exclusively for Harvard students, of which there are only a few thousand, but those few thousand users wanted it a lot. When you have an idea for a startup, ask yourself: who wants this right now? Who wants this so much that they'll use it even when it's a crappy version one made by a two-person startup they've never heard of? If you can't answer that, the idea is probably bad. You don't need the narrowness of the well per se. It's depth you need; you get narrowness as a byproduct of optimizing for depth (and speed). But you almost always do get it. In practice the link between depth and narrowness is so strong that it's a good sign when you know that an idea will appeal strongly to a specific group or type of user. But while demand shaped like a well is almost a necessary condition for a good startup idea, it's not a sufficient one. If Mark Zuckerberg had built something that could only ever have appealed to Harvard students, it would not have been a good startup idea. Facebook was a good idea because it started with a small market there was a fast path out of. Colleges are similar enough that if you build a facebook that works at Harvard, it will work at any college. So you spread rapidly through all the colleges. Once you have all the college students, you get everyone else simply by letting them in. Similarly for Microsoft: Basic for the Altair; Basic for other machines; other languages besides Basic; operating systems; applications; IPO.

SELF

How do you tell whether there's a path out of an idea? How do you tell whether something is the germ of a giant company, or just a niche product? Often you can't. The founders of Airbnb didn't realize at first how big a market they were tapping. Initially they had a much narrower idea. They were going to let hosts rent out space on their floors during conventions. They didn't foresee the expansion of this idea; it forced itself upon them gradually. All they knew at first is that they were onto something. That's probably as much as Bill Gates or Mark Zuckerberg knew at first. Occasionally it's obvious from the beginning when there's a path out of the initial niche. And sometimes I can see a path that's not immediately obvious; that's one of our specialties at YC. But there are limits to how well this can be done, no matter how much experience you have. The most important thing to understand about paths out of the initial idea is the meta-fact that these are hard to see. So if you can't predict whether there's a path out of an idea, how do you choose between ideas?

The truth is disappointing but interesting: if you're the right sort of person, you have the right sort of hunches. If you're at the leading edge of a field that's changing fast, when you have a hunch that something is worth doing, you're more likely to be right. In Zen and the Art of Motorcycle Maintenance, Robert Pirsig says: You want to know how to paint a perfect painting? It's easy. Make yourself perfect and then just paint naturally. I've wondered about that passage since I read it in high school. I'm not sure how useful his advice is for painting specifically, but it fits this situation well. Empirically, the way to have good startup ideas is to become the sort of person who has them. Being at the leading edge of a field doesn't mean you have to be one of the people pushing it forward. You can also be at the leading edge as a user. It was not so much because he was a programmer that Facebook seemed a good idea to Mark Zuckerberg as because he used computers so much. If you'd asked most 40 year olds in 2004 whether they'd like to publish their lives semi-publicly on the Internet, they'd have been horrified at the idea. But Mark already lived online; to him it seemed natural. Paul Buchheit says that people at the leading edge of a rapidly changing field "live in the future." Combine that with Pirsig and you get: Live in the future, then build what's missing. That describes the way many if not most of the biggest startups got started. Neither Apple nor Yahoo nor Google nor Facebook were even supposed to be companies at first. They grew out of things their founders built because there seemed a gap in the world. If you look at the way successful founders have had their ideas, it's generally the result of some external stimulus hitting a prepared mind. Bill Gates and Paul Allen hear about the Altair and think "I bet we could write a Basic interpreter for it." Drew Houston realizes he's forgotten his USB stick and thinks "I really need to make my files live online." Lots of people heard about the Altair. Lots forgot USB sticks. The reason those stimuli caused those founders to start companies was that their experiences had prepared them to notice the opportunities they represented. The verb you want to be using with respect to startup ideas is not "think up" but "notice." At YC we call ideas that grow naturally out of the founders' own experiences "organic" startup ideas. The most successful startups almost all begin this way. That may not have been what you wanted to hear. You may have expected recipes for coming up with startup ideas, and instead I'm telling you that the key is to have a mind that's prepared in the right way. But disappointing though it may be, this is the truth. And it is a recipe of a sort, just one that in the worst case takes a year rather than a weekend. If you're not at the leading edge of some rapidly changing field, you can get to one. For example, anyone reasonably smart can probably get to an edge of programming (e.g. building mobile apps) in a year. Since a successful startup will consume at least 3-5 years of your life, a year's preparation would be a reasonable investment. Especially if you're also looking for a cofounder. You don't have to learn programming to be at the leading edge of a domain that's changing fast. Other domains change fast. But while learning to hack is not necessary, it is for the forseeable future sufficient. As Marc Andreessen put it, software is eating the world, and this trend has decades left to run. Knowing how to hack also means that when you have ideas, you'll be able to implement them. That's not absolutely necessary (Jeff Bezos couldn't) but it's an advantage. It's a big advantage, when you're considering an idea like putting a college facebook online, if instead of merely thinking "That's an interesting idea," you can think instead "That's an interesting idea. I'll try building an initial version tonight." It's even better when you're both a programmer and the target user, because then the cycle of generating new versions and testing them on users can happen inside one head.

NOTICING

Once you're living in the future in some respect, the way to notice startup ideas is to look for things that seem to be missing. If you're really at the leading edge of a rapidly changing field, there will be things that are obviously missing. What won't be obvious is that they're startup ideas. So if you want to find startup ideas, don't merely turn on the filter "What's missing?" Also turn off every other filter, particularly "Could this be a big company?" There's plenty of time to apply that test later. But if you're thinking about that initially, it may not only filter out lots of good ideas, but also cause you to focus on bad ones. Most things that are missing will take some time to see. You almost have to trick yourself into seeing the ideas around you. But you know the ideas are out there. This is not one of those problems where there might not be an answer. It's impossibly unlikely that this is the exact moment when technological progress stops. You can be sure people are going to build things in the next few years that will make you think "What did I do before x?" And when these problems get solved, they will probably seem flamingly obvious in retrospect. What you need to do is turn off the filters that usually prevent you from seeing them. The most powerful is simply taking the current state of the world for granted. Even the most radically open-minded of us mostly do that. You couldn't get from your bed to the front door if you stopped to question everything. But if you're looking for startup ideas you can sacrifice some of the efficiency of taking the status quo for granted and start to question things. Why is your inbox overflowing? Because you get a lot of email, or because it's hard to get email out of your inbox? Why do you get so much email? What problems are people trying to solve by sending you email? Are there better ways to solve them? And why is it hard to get emails out of your inbox? Why do you keep emails around after you've read them? Is an inbox the optimal tool for that? Pay particular attention to things that chafe you. The advantage of taking the status quo for granted is not just that it makes life (locally) more efficient, but also that it makes life more tolerable. If you knew about all the things we'll get in the next 50 years but don't have yet, you'd find present day life pretty constraining, just as someone from the present would if they were sent back 50 years in a time machine. When something annoys you, it could be because you're living in the future. When you find the right sort of problem, you should probably be able to describe it as obvious, at least to you. When we started Viaweb, all the online stores were built by hand, by web designers making individual HTML pages. It was obvious to us as programmers that these sites would have to be generated by software. Which means, strangely enough, that coming up with startup ideas is a question of seeing the obvious. That suggests how weird this process is: you're trying to see things that are obvious, and yet that you hadn't seen. Since what you need to do here is loosen up your own mind, it may be best not to make too much of a direct frontal attack on the problem — i.e. to sit down and try to think of ideas. The best plan may be just to keep a background process running, looking for things that seem to be missing. Work on hard problems, driven mainly by curiosity, but have a second self watching over your shoulder, taking note of gaps and anomalies. Give yourself some time. You have a lot of control over the rate at which you turn yours into a prepared mind, but you have less control over the stimuli that spark ideas when they hit it. If Bill Gates and Paul Allen had constrained themselves to come up with a startup idea in one month, what if they'd chosen a month before the Altair appeared? They probably would have worked on a less promising idea. Drew Houston did work on a less promising idea before Dropbox: an SAT prep startup. But Dropbox was a much better idea, both in the absolute sense and also as a match for his skills. A good way to trick yourself into noticing ideas is to work on projects that seem like they'd be cool. If you do that, you'll naturally tend to build things that are missing. It wouldn't seem as interesting to build something that already existed. Just as trying to think up startup ideas tends to produce bad ones, working on things that could be dismissed as "toys" often produces good ones. When something is described as a toy, that means it has everything an idea needs except being important. It's cool; users love it; it just doesn't matter. But if you're living in the future and you build something cool that users love, it may matter more than outsiders think. Microcomputers seemed like toys when Apple and Microsoft started working on them. I'm old enough to remember that era; the usual term for people with their own microcomputers was "hobbyists." BackRub seemed like an inconsequential science project. The Facebook was just a way for undergrads to stalk one another. At YC we're excited when we meet startups working on things that we could imagine know-it-alls on forums dismissing as toys. To us that's positive evidence an idea is good. If you can afford to take a long view (and arguably you can't afford not to), you can turn "Live in the future and build what's missing" into something even better: Live in the future and build what seems interesting.

SCHOOL

That's what I'd advise college students to do, rather than trying to learn about "entrepreneurship." "Entrepreneurship" is something you learn best by doing it. The examples of the most successful founders make that clear. What you should be spending your time on in college is ratcheting yourself into the future. College is an incomparable opportunity to do that. What a waste to sacrifice an opportunity to solve the hard part of starting a startup — becoming the sort of person who can have organic startup ideas — by spending time learning about the easy part. Especially since you won't even really learn about it, any more than you'd learn about sex in a class. All you'll learn is the words for things. The clash of domains is a particularly fruitful source of ideas. If you know a lot about programming and you start learning about some other field, you'll probably see problems that software could solve. In fact, you're doubly likely to find good problems in another domain: (a) the inhabitants of that domain are not as likely as software people to have already solved their problems with software, and (b) since you come into the new domain totally ignorant, you don't even know what the status quo is to take it for granted. So if you're a CS major and you want to start a startup, instead of taking a class on entrepreneurship you're better off taking a class on, say, genetics. Or better still, go work for a biotech company. CS majors normally get summer jobs at computer hardware or software companies. But if you want to find startup ideas, you might do better to get a summer job in some unrelated field. Or don't take any extra classes, and just build things. It's no coincidence that Microsoft and Facebook both got started in January. At Harvard that is (or was) Reading Period, when students have no classes to attend because they're supposed to be studying for finals. But don't feel like you have to build things that will become startups. That's premature optimization. Just build things. Preferably with other students. It's not just the classes that make a university such a good place to crank oneself into the future. You're also surrounded by other people trying to do the same thing. If you work together with them on projects, you'll end up producing not just organic ideas, but organic ideas with organic founding teams — and that, empirically, is the best combination. Beware of research. If an undergrad writes something all his friends start using, it's quite likely to represent a good startup idea. Whereas a PhD dissertation is extremely unlikely to. For some reason, the more a project has to count as research, the less likely it is to be something that could be turned into a startup. I think the reason is that the subset of ideas that count as research is so narrow that it's unlikely that a project that satisfied that constraint would also satisfy the orthogonal constraint of solving users' problems. Whereas when students (or professors) build something as a side-project, they automatically gravitate toward solving users' problems — perhaps even with an additional energy that comes from being freed from the constraints of research.

COMPETITION

Because a good idea should seem obvious, when you have one you'll tend to feel that you're late. Don't let that deter you. Worrying that you're late is one of the signs of a good idea. Ten minutes of searching the web will usually settle the question. Even if you find someone else working on the same thing, you're probably not too late. It's exceptionally rare for startups to be killed by competitors — so rare that you can almost discount the possibility. So unless you discover a competitor with the sort of lock-in that would prevent users from choosing you, don't discard the idea. If you're uncertain, ask users. The question of whether you're too late is subsumed by the question of whether anyone urgently needs what you plan to make. If you have something that no competitor does and that some subset of users urgently need, you have a beachhead. The question then is whether that beachhead is big enough. Or more importantly, who's in it: if the beachhead consists of people doing something lots more people will be doing in the future, then it's probably big enough no matter how small it is. For example, if you're building something differentiated from competitors by the fact that it works on phones, but it only works on the newest phones, that's probably a big enough beachhead. Err on the side of doing things where you'll face competitors. Inexperienced founders usually give competitors more credit than they deserve. Whether you succeed depends far more on you than on your competitors. So better a good idea with competitors than a bad one without. You don't need to worry about entering a "crowded market" so long as you have a thesis about what everyone else in it is overlooking. In fact that's a very promising starting point. Google was that type of idea. Your thesis has to be more precise than "we're going to make an x that doesn't suck" though. You have to be able to phrase it in terms of something the incumbents are overlooking. Best of all is when you can say that they didn't have the courage of their convictions, and that your plan is what they'd have done if they'd followed through on their own insights. Google was that type of idea too. The search engines that preceded them shied away from the most radical implications of what they were doing — particularly that the better a job they did, the faster users would leave. A crowded market is actually a good sign, because it means both that there's demand and that none of the existing solutions are good enough. A startup can't hope to enter a market that's obviously big and yet in which they have no competitors. So any startup that succeeds is either going to be entering a market with existing competitors, but armed with some secret weapon that will get them all the users (like Google), or entering a market that looks small but which will turn out to be big (like Microsoft).

FILTERS

There are two more filters you'll need to turn off if you want to notice startup ideas: the unsexy filter and the schlep filter. Most programmers wish they could start a startup by just writing some brilliant code, pushing it to a server, and having users pay them lots of money.

They'd prefer not to deal with tedious problems or get involved in messy ways with the real world. Which is a reasonable preference, because such things slow you down. But this preference is so widespread that the space of convenient startup ideas has been stripped pretty clean. If you let your mind wander a few blocks down the street to the messy, tedious ideas, you'll find valuable ones just sitting there waiting to be implemented. The schlep filter is so dangerous that I wrote a separate essay about the condition it induces, which I called schlep blindness. I gave Stripe as an example of a startup that benefited from turning off this filter, and a pretty striking example it is. Thousands of programmers were in a position to see this idea; thousands of programmers knew how painful it was to process payments before Stripe. But when they looked for startup ideas they didn't see this one, because unconsciously they shrank from having to deal with payments. And dealing with payments is a schlep for Stripe, but not an intolerable one. In fact they might have had net less pain; because the fear of dealing with payments kept most people away from this idea, Stripe has had comparatively smooth sailing in other areas that are sometimes painful, like user acquisition. They didn't have to try very hard to make themselves heard by users, because users were desperately waiting for what they were building. The unsexy filter is similar to the schlep filter, except it keeps you from working on problems you despise rather than ones you fear. We overcame this one to work on Viaweb. There were interesting things about the architecture of our software, but we weren't interested in ecommerce per se. We could see the problem was one that needed to be solved though. Turning off the schlep filter is more important than turning off the unsexy filter, because the schlep filter is more likely to be an illusion. And even to the degree it isn't, it's a worse form of self-indulgence. Starting a successful startup is going to be fairly laborious no matter what. Even if the product doesn't entail a lot of schleps, you'll still have plenty dealing with investors, hiring and firing people, and so on. So if there's some idea you think would be cool but you're kept away from by fear of the schleps involved, don't worry: any sufficiently good idea will have as many. The unsexy filter, while still a source of error, is not as entirely useless as the schlep filter. If you're at the leading edge of a field that's changing rapidly, your ideas about what's sexy will be somewhat correlated with what's valuable in practice. Particularly as you get older and more experienced. Plus if you find an idea sexy, you'll work on it more enthusiastically.

RECIPES

While the best way to discover startup ideas is to become the sort of person who has them and then build whatever interests you, sometimes you don't have that luxury. Sometimes you need an idea now. For example, if you're working on a startup and your initial idea turns out to be bad. For the rest of this essay I'll talk about tricks for coming up with startup ideas on demand. Although empirically you're better off using the organic strategy, you could succeed this way. You just have to be more disciplined. When you use the organic method, you don't even notice an idea unless it's evidence that something is truly missing. But when you make a conscious effort to think of startup ideas, you have to replace this natural constraint with self-discipline. You'll see a lot more ideas, most of them bad, so you need to be able to filter them. One of the biggest dangers of not using the organic method is the example of the organic method. Organic ideas feel like inspirations. There are a lot of stories about successful startups that began when the founders had what seemed a crazy idea but "just knew" it was promising. When you feel that about an idea you've had while trying to come up with startup ideas, you're probably mistaken. When searching for ideas, look in areas where you have some expertise. If you're a database expert, don't build a chat app for teenagers (unless you're also a teenager). Maybe it's a good idea, but you can't trust your judgment about that, so ignore it. There have to be other ideas that involve databases, and whose quality you can judge. Do you find it hard to come up with good ideas involving databases? That's because your expertise raises your standards. Your ideas about chat apps are just as bad, but you're giving yourself a Dunning-Kruger pass in that domain. The place to start looking for ideas is things you need. There must be things you need. One good trick is to ask yourself whether in your previous job you ever found yourself saying "Why doesn't someone make x? If someone made x we'd buy it in a second." If you can think of any x people said that about, you probably have an idea. You know there's demand, and people don't say that about things that are impossible to build. More generally, try asking yourself whether there's something unusual about you that makes your needs different from most other people's. You're probably not the only one. It's especially good if you're different in a way people will increasingly be. If you're changing ideas, one unusual thing about you is the idea you'd previously been working on. Did you discover any needs while working on it? Several well-known startups began this way. Hotmail began as something its founders wrote to talk about their previous startup idea while they were working at their day jobs. A particularly promising way to be unusual is to be young. Some of the most valuable new ideas take root first among people in their teens and early twenties. And while young founders are at a disadvantage in some respects, they're the only ones who really understand their peers. It would have been very hard for someone who wasn't a college student to start Facebook. So if you're a young founder (under 23 say), are there things you and your friends would like to do that current technology won't let you? The next best thing to an unmet need of your own is an unmet need of someone else. Try talking to everyone you can about the gaps they find in the world. What's missing? What would they like to do that they can't? What's tedious or annoying, particularly in their work? Let the conversation get general; don't be trying too hard to find startup ideas. You're just looking for something to spark a thought. Maybe you'll notice a problem they didn't consciously realize they had, because you know how to solve it. When you find an unmet need that isn't your own, it may be somewhat blurry at first. The person who needs something may not know exactly what they need. In that case I often recommend that founders act like consultants — that they do what they'd do if they'd been retained to solve the problems of this one user. People's problems are similar enough that nearly all the code you write this way will be reusable, and whatever isn't will be a small price to start out certain that you've reached the bottom of the well. One way to ensure you do a good job solving other people's problems is to make them your own. When Rajat Suri of E la Carte decided to write software for restaurants, he got a job as a waiter to learn how restaurants worked. That may seem like taking things to extremes, but startups are extreme. We love it when founders do such things. In fact, one strategy I recommend to people who need a new idea is not merely to turn off their schlep and unsexy filters, but to seek out ideas that are unsexy or involve schleps. Don't try to start Twitter. Those ideas are so rare that you can't find them by looking for them. Make something unsexy that people will pay you for. A good trick for bypassing the schlep and to some extent the unsexy filter is to ask what you wish someone else would build, so that you could use it. What would you pay for right now? Since startups often garbage-collect broken companies and industries, it can be a good trick to look for those that are dying, or deserve to, and try to imagine what kind of company would profit from their demise. For example, journalism is in free fall at the moment. But there may still be money to be made from something like journalism. What sort of company might cause people in the future to say "this replaced journalism" on some axis? But imagine asking that in the future, not now. When one company or industry replaces another, it usually comes in from the side. So don't look for a replacement for x; look for something that people will later say turned out to be a replacement for x. And be imaginative about the axis along which the replacement occurs. Traditional journalism, for example, is a way for readers to get information and to kill time, a way for writers to make money and to get attention, and a vehicle for several different types of advertising. It could be replaced on any of these axes (it has already started to be on most). When startups consume incumbents, they usually start by serving some small but important market that the big players ignore. It's particularly good if there's an admixture of disdain in the big players' attitude, because that often misleads them. For example, after Steve Wozniak built the computer that became the Apple I, he felt obliged to give his then-employer Hewlett-Packard the option to produce it. Fortunately for him, they turned it down, and one of the reasons they did was that it used a TV for a monitor, which seemed intolerably déclassé to a high-end hardware company like HP was at the time. Are there groups of scruffy but sophisticated users like the early microcomputer "hobbyists" that are currently being ignored by the big players? A startup with its sights set on bigger things can often capture a small market easily by expending an effort that wouldn't be justified by that market alone. Similarly, since the most successful startups generally ride some wave bigger than themselves, it could be a good trick to look for waves and ask how one could benefit from them. The prices of gene sequencing and 3D printing are both experiencing Moore's Law-like declines. What new things will we be able to do in the new world we'll have in a few years? What are we unconsciously ruling out as impossible that will soon be possible? ORGANIC But talking about looking explicitly for waves makes it clear that such recipes are plan B for getting startup ideas. Looking for waves is essentially a way to simulate the organic method. If you're at the leading edge of some rapidly changing field, you don't have to look for waves; you are the wave. Finding startup ideas is a subtle business, and that's why most people who try fail so miserably. It doesn't work well simply to try to think of startup ideas. If you do that, you get bad ones that sound dangerously plausible. The best approach is more indirect: if you have the right sort of background, good startup ideas will seem obvious to you. But even then, not immediately. It takes time to come across situations where you notice something missing. And often these gaps won't seem to be ideas for companies, just things that would be interesting to build. Which is why it's good to have the time and the inclination to build things just because they're interesting.

Startup = growth

A startup is a company designed to grow fast. Being newly founded does not in itself make a company a startup. Nor is it necessary for a startup to work on technology, or take venture funding, or have some sort of "exit." The only essential thing is growth. Everything else we associate with startups follows from growth. If you want to start one it's important to understand that. Startups are so hard that you can't be pointed off to the side and hope to succeed. You have to know that growth is what you're after. The good news is, if you get growth, everything else tends to fall into place. Which means you can use growth like a compass to make almost every decision you face.

REDWOODS

Let's start with a distinction that should be obvious but is often overlooked: not every newly founded company is a startup. Millions of companies are started every year in the US. Only a tiny fraction are startups. Most are service businesses — restaurants, barbershops, plumbers, and so on. These are not startups, except in a few unusual cases. A barbershop isn't designed to grow fast. Whereas a search engine, for example, is. When I say startups are designed to grow fast, I mean it in two senses. Partly I mean designed in the sense of intended, because most startups fail. But I also mean startups are different by nature, in the same way a redwood seedling has a different destiny from a bean sprout. That difference is why there's a distinct word, "startup," for companies designed to grow fast. If all companies were essentially similar, but some through luck or the efforts of their founders ended up growing very fast, we wouldn't need a separate word. We could just talk about super-successful companies and less successful ones. But in fact startups do have a different sort of DNA from other businesses. Google is not just a barbershop whose founders were unusually lucky and hard-working. Google was different from the beginning. To grow rapidly, you need to make something you can sell to a big market. That's the difference between Google and a barbershop. A barbershop doesn't scale. For a company to grow really big, it must (a) make something lots of people want, and (b) reach and serve all those people. Barbershops are doing fine in the (a) department. Almost everyone needs their hair cut. The problem for a barbershop, as for any retail establishment, is (b). A barbershop serves customers in person, and few will travel far for a haircut. And even if they did, the barbershop couldn't accomodate them. Writing software is a great way to solve (b), but you can still end up constrained in (a). If you write software to teach Tibetan to Hungarian speakers, you'll be able to reach most of the people who want it, but there won't be many of them. If you make software to teach English to Chinese speakers, however, you're in startup territory. Most businesses are tightly constrained in (a) or (b). The distinctive feature of successful startups is that they're not. IDEAS It might seem that it would always be better to start a startup than an ordinary business. If you're going to start a company, why not start the type with the most potential? The catch is that this is a (fairly) efficient market. If you write software to teach Tibetan to Hungarians, you won't have much competition. If you write software to teach English to Chinese speakers, you'll face ferocious competition, precisely because that's such a larger prize. The constraints that limit ordinary companies also protect them. That's the tradeoff. If you start a barbershop, you only have to compete with other local barbers. If you start a search engine you have to compete with the whole world. The most important thing that the constraints on a normal business protect it from is not competition, however, but the difficulty of coming up with new ideas. If you open a bar in a particular neighborhood, as well as limiting your potential and protecting you from competitors, that geographic constraint also helps define your company. Bar + neighborhood is a sufficient idea for a small business. Similarly for companies constrained in (a). Your niche both protects and defines you. Whereas if you want to start a startup, you're probably going to have to think of something fairly novel. A startup has to make something it can deliver to a large market, and ideas of that type are so valuable that all the obvious ones are already taken. That space of ideas has been so thoroughly picked over that a startup generally has to work on something everyone else has overlooked. I was going to write that one has to make a conscious effort to find ideas everyone else has overlooked. But that's not how most startups get started. Usually successful startups happen because the founders are sufficiently different from other people that ideas few others can see seem obvious to them. Perhaps later they step back and notice they've found an idea in everyone else's blind spot, and from that point make a deliberate effort to stay there. But at the moment when successful startups get started, much of the innovation is unconscious. What's different about successful founders is that they can see different problems. It's a particularly good combination both to be good at technology and to face problems that can be solved by it, because technology changes so rapidly that formerly bad ideas often become good without anyone noticing. Steve Wozniak's problem was that he wanted his own computer. That was an unusual problem to have in 1975. But technological change was about to make it a much more common one. Because he not only wanted a computer but knew how to build them, Wozniak was able to make himself one. And the problem he solved for himself became one that Apple solved for millions of people in the coming years. But by the time it was obvious to ordinary people that this was a big market, Apple was already established. Google has similar origins. Larry Page and Sergey Brin wanted to search the web. But unlike most people they had the technical expertise both to notice that existing search engines were not as good as they could be, and to know how to improve them. Over the next few years their problem became everyone's problem, as the web grew to a size where you didn't have to be a picky search expert to notice the old algorithms weren't good enough. But as happened with Apple, by the time everyone else realized how important search was, Google was entrenched. That's one connection between startup ideas and technology. Rapid change in one area uncovers big, soluble problems in other areas. Sometimes the changes are advances, and what they change is solubility. That was the kind of change that yielded Apple; advances in chip technology finally let Steve Wozniak design a computer he could afford. But in Google's case the most important change was the growth of the web. What changed there was not solubility but bigness. The other connection between startups and technology is that startups create new ways of doing things, and new ways of doing things are, in the broader sense of the word, new technology. When a startup both begins with an idea exposed by technological change and makes a product consisting of technology in the narrower sense (what used to be called "high technology"), it's easy to conflate the two. But the two connections are distinct and in principle one could start a startup that was neither driven by technological change, nor whose product consisted of technology except in the broader sense.

RATE

How fast does a company have to grow to be considered a startup? There's no precise answer to that. "Startup" is a pole, not a threshold. Starting one is at first no more than a declaration of one's ambitions. You're committing not just to starting a company, but to starting a fast growing one, and you're thus committing to search for one of the rare ideas of that type. But at first you have no more than commitment. Starting a startup is like being an actor in that respect. "Actor" too is a pole rather than a threshold. At the beginning of his career, an actor is a waiter who goes to auditions. Getting work makes him a successful actor, but he doesn't only become an actor when he's successful. So the real question is not what growth rate makes a company a startup, but what growth rate successful startups tend to have. For founders that's more than a theoretical question, because it's equivalent to asking if they're on the right path. The growth of a successful startup usually has three phases: There's an initial period of slow or no growth while the startup tries to figure out what it's doing. As the startup figures out how to make something lots of people want and how to reach those people, there's a period of rapid growth. Eventually a successful startup will grow into a big company. Growth will slow, partly due to internal limits and partly because the company is starting to bump up against the limits of the markets it serves. Together these three phases produce an S-curve. The phase whose growth defines the startup is the second one, the ascent. Its length and slope determine how big the company will be. The slope is the company's growth rate. If there's one number every founder should always know, it's the company's growth rate. That's the measure of a startup. If you don't know that number, you don't even know if you're doing well or badly. When I first meet founders and ask what their growth rate is, sometimes they tell me "we get about a hundred new customers a month." That's not a rate. What matters is not the absolute number of new customers, but the ratio of new customers to existing ones. If you're really getting a constant number of new customers every month, you're in trouble, because that means your growth rate is decreasing. During Y Combinator we measure growth rate per week, partly because there is so little time before Demo Day, and partly because startups early on need frequent feedback from their users to tweak what they're doing. A good growth rate during YC is 5-7% a week. If you can hit 10% a week you're doing exceptionally well. If you can only manage 1%, it's a sign you haven't yet figured out what you're doing. The best thing to measure the growth rate of is revenue. The next best, for startups that aren't charging initially, is active users. That's a reasonable proxy for revenue growth because whenever the startup does start trying to make money, their revenues will probably be a constant multiple of active users.

COMPASS

We usually advise startups to pick a growth rate they think they can hit, and then just try to hit it every week. The key word here is "just." If they decide to grow at 7% a week and they hit that number, they're successful for that week. There's nothing more they need to do. But if they don't hit it, they've failed in the only thing that mattered, and should be correspondingly alarmed. Programmers will recognize what we're doing here. We're turning starting a startup into an optimization problem. And anyone who has tried optimizing code knows how wonderfully effective that sort of narrow focus can be. Optimizing code means taking an existing program and changing it to use less of something, usually time or memory. You don't have to think about what the program should do, just make it faster. For most programmers this is very satisfying work. The narrow focus makes it a sort of puzzle, and you're generally surprised how fast you can solve it. Focusing on hitting a growth rate reduces the otherwise bewilderingly multifarious problem of starting a startup to a single problem. You can use that target growth rate to make all your decisions for you; anything that gets you the growth you need is ipso facto right. Should you spend two days at a conference? Should you hire another programmer? Should you focus more on marketing? Should you spend time courting some big customer? Should you add x feature? Whatever gets you your target growth rate. [Judging yourself by weekly growth doesn't mean you can look no more than a week ahead. Once you experience the pain of missing your target one week (it was the only thing that mattered, and you failed at it), you become interested in anything that could spare you such pain in the future. So you'll be willing for example to hire another programmer, who won't contribute to this week's growth but perhaps in a month will have implemented some new feature that will get you more users. But only if (a) the distraction of hiring someone won't make you miss your numbers in the short term, and (b) you're sufficiently worried about whether you can keep hitting your numbers without hiring someone new. It's not that you don't think about the future, just that you think about it no more than necessary. In theory this sort of hill-climbing could get a startup into trouble. They could end up on a local maximum. But in practice that never happens. Having to hit a growth number every week forces founders to act, and acting versus not acting is the high bit of succeeding. Nine times out of ten, sitting around strategizing is just a form of procrastination. Whereas founders' intuitions about which hill to climb are usually better than they realize. Plus the maxima in the space of startup ideas are not spiky and isolated. Most fairly good ideas are adjacent to even better ones. The fascinating thing about optimizing for growth is that it can actually discover startup ideas. You can use the need for growth as a form of evolutionary pressure. If you start out with some initial plan and modify it as necessary to keep hitting, say, 10% weekly growth, you may end up with a quite different company than you meant to start. But anything that grows consistently at 10% a week is almost certainly a better idea than you started with. There's a parallel here to small businesses. Just as the constraint of being located in a particular neighborhood helps define a bar, the constraint of growing at a certain rate can help define a startup. You'll generally do best to follow that constraint wherever it leads rather than being influenced by some initial vision, just as a scientist is better off following the truth wherever it leads rather than being influenced by what he wishes were the case. When Richard Feynman said that the imagination of nature was greater than the imagination of man, he meant that if you just keep following the truth you'll discover cooler things than you could ever have made up. For startups, growth is a constraint much like truth. Every successful startup is at least partly a product of the imagination of growth.

VALUE

It's hard to find something that grows consistently at several percent a week, but if you do you may have found something surprisingly valuable. If we project forward we see why. A company that grows at 1% a week will grow 1.7x a year, whereas a company that grows at 5% a week will grow 12.6x. A company making $1000 a month (a typical number early in YC) and growing at 1% a week will 4 years later be making $7900 a month, which is less than a good programmer makes in salary in Silicon Valley. A startup that grows at 5% a week will in 4 years be making $25 million a month. Our ancestors must rarely have encountered cases of exponential growth, because our intuitions are no guide here. What happens to fast growing startups tends to surprise even the founders. Small variations in growth rate produce qualitatively different outcomes. That's why there's a separate word for startups, and why startups do things that ordinary companies don't, like raising money and getting acquired. And, strangely enough, it's also why they fail so frequently. Considering how valuable a successful startup can become, anyone familiar with the concept of expected value would be surprised if the failure rate weren't high. If a successful startup could make a founder $100 million, then even if the chance of succeeding were only 1%, the expected value of starting one would be $1 million. And the probability of a group of sufficiently smart and determined founders succeeding on that scale might be significantly over 1%. For the right people — e.g. the young Bill Gates — the probability might be 20% or even 50%. So it's not surprising that so many want to take a shot at it. In an efficient market, the number of failed startups should be proportionate to the size of the successes. And since the latter is huge the former should be too. What this means is that at any given time, the great majority of startups will be working on something that's never going to go anywhere, and yet glorifying their doomed efforts with the grandiose title of "startup." This doesn't bother me. It's the same with other high-beta vocations, like being an actor or a novelist. I've long since gotten used to it. But it seems to bother a lot of people, particularly those who've started ordinary businesses. Many are annoyed that these so-called startups get all the attention, when hardly any of them will amount to anything. If they stepped back and looked at the whole picture they might be less indignant. The mistake they're making is that by basing their opinions on anecdotal evidence they're implicitly judging by the median rather than the average. If you judge by the median startup, the whole concept of a startup seems like a fraud. You have to invent a bubble to explain why founders want to start them or investors want to fund them. But it's a mistake to use the median in a domain with so much variation. If you look at the average outcome rather than the median, you can understand why investors like them, and why, if they aren't median people, it's a rational choice for founders to start them.

DEALS

Why do investors like startups so much? Why are they so hot to invest in photo-sharing apps, rather than solid money-making businesses? Not only for the obvious reason. The test of any investment is the ratio of return to risk. Startups pass that test because although they're appallingly risky, the returns when they do succeed are so high. But that's not the only reason investors like startups. An ordinary slower-growing business might have just as good a ratio of return to risk, if both were lower. So why are VCs interested only in high-growth companies? The reason is that they get paid by getting their capital back, ideally after the startup IPOs, or failing that when it's acquired. The other way to get returns from an investment is in the form of dividends. Why isn't there a parallel VC industry that invests in ordinary companies in return for a percentage of their profits? Because it's too easy for people who control a private company to funnel its revenues to themselves (e.g. by buying overpriced components from a supplier they control) while making it look like the company is making little profit. Anyone who invested in private companies in return for dividends would have to pay close attention to their books. The reason VCs like to invest in startups is not simply the returns, but also because such investments are so easy to oversee. The founders can't enrich themselves without also enriching the investors. Why do founders want to take the VCs' money? Growth, again. The constraint between good ideas and growth operates in both directions. It's not merely that you need a scalable idea to grow. If you have such an idea and don't grow fast enough, competitors will. Growing too slowly is particularly dangerous in a business with network effects, which the best startups usually have to some degree. Almost every company needs some amount of funding to get started. But startups often raise money even when they are or could be profitable. It might seem foolish to sell stock in a profitable company for less than you think it will later be worth, but it's no more foolish than buying insurance. Fundamentally that's how the most successful startups view fundraising. They could grow the company on its own revenues, but the extra money and help supplied by VCs will let them grow even faster. Raising money lets you choose your growth rate. Money to grow faster is always at the command of the most successful startups, because the VCs need them more than they need the VCs. A profitable startup could if it wanted just grow on its own revenues. Growing slower might be slightly dangerous, but chances are it wouldn't kill them. Whereas VCs need to invest in startups, and in particular the most successful startups, or they'll be out of business. Which means that any sufficiently promising startup will be offered money on terms they'd be crazy to refuse. And yet because of the scale of the successes in the startup business, VCs can still make money from such investments. You'd have to be crazy to believe your company was going to become as valuable as a high growth rate can make it, but some do. Pretty much every successful startup will get acquisition offers too. Why? What is it about startups that makes other companies want to buy them? Fundamentally the same thing that makes everyone else want the stock of successful startups: a rapidly growing company is valuable. It's a good thing eBay bought Paypal, for example, because Paypal is now responsible for 43% of their sales and probably more of their growth. But acquirers have an additional reason to want startups. A rapidly growing company is not merely valuable, but dangerous. If it keeps expanding, it might expand into the acquirer's own territory. Most product acquisitions have some component of fear. Even if an acquirer isn't threatened by the startup itself, they might be alarmed at the thought of what a competitor could do with it. And because startups are in this sense doubly valuable to acquirers, acquirers will often pay more than an ordinary investor would.

UNDERSTAND

The combination of founders, investors, and acquirers forms a natural ecosystem. It works so well that those who don't understand it are driven to invent conspiracy theories to explain how neatly things sometimes turn out. Just as our ancestors did to explain the apparently too neat workings of the natural world. But there is no secret cabal making it all work. If you start from the mistaken assumption that Instagram was worthless, you have to invent a secret boss to force Mark Zuckerberg to buy it. To anyone who knows Mark Zuckerberg, that is the reductio ad absurdum of the initial assumption. The reason he bought Instagram was that it was valuable and dangerous, and what made it so was growth. If you want to understand startups, understand growth. Growth drives everything in this world. Growth is why startups usually work on technology — because ideas for fast growing companies are so rare that the best way to find new ones is to discover those recently made viable by change, and technology is the best source of rapid change. Growth is why it's a rational choice economically for so many founders to try starting a startup: growth makes the successful companies so valuable that the expected value is high even though the risk is too. Growth is why VCs want to invest in startups: not just because the returns are high but also because generating returns from capital gains is easier to manage than generating returns from dividends. Growth explains why the most successful startups take VC money even if they don't need to: it lets them choose their growth rate. And growth explains why successful startups almost invariably get acquisition offers. To acquirers a fast-growing company is not merely valuable but dangerous too. It's not just that if you want to succeed in some domain, you have to understand the forces driving it. Understanding growth is what starting a startup consists of. What you're really doing (and to the dismay of some observers, all you're really doing) when you start a startup is committing to solve a harder type of problem than ordinary businesses do. You're committing to search for one of the rare ideas that generates rapid growth. Because these ideas are so valuable, finding one is hard. The startup is the embodiment of your discoveries so far. Starting a startup is thus very much like deciding to be a research scientist: you're not committing to solve any specific problem; you don't know for sure which problems are soluble; but you're committing to try to discover something no one knew before. A startup founder is in effect an economic research scientist. Most don't discover anything that remarkable, but some discover relativity.

How to Talk to Users

Three things to keep in mind during user interviews:

Talk about their lives, not just your idea. : It is not the time to sell the future product, but the time to extract meaningful information from the users.

Discuss concrete details, not hypothesis. : Discuss specific things happening in real users' lives, and avoid making assumptions like "What if this feature is added?"

Listen more, Talk Less. : Despite entrepreneurs are usually talkative, they should remain quiet and listen attentively.

Five examples of great user interview questions:

What has been the most challenging aspect of doing this? In the case of Dropbox, they asked. "What is the most challenging aspect when working on team projects using school computers?"

Could you please describe in detail the most recent instance where you encountered this problem? Extract the specific context of that situation and listen for rich details.

Why did you find it difficult? The reasons for difficulty vary from person to person. This question helps not only in pinpointing the problem to be solved but also becomes a selling point to explain the value of the future product. Customers do not buy What, they buy "Why" In the case of Dropbox, some challenges included not being able to view the same document at a specific moment, submitting the wrong file to a professor, dealing with complex email exchanges of files with convoluted names like “final_really_final” and more.

What have you tried to solve this problem? If customers haven’t made significant efforts to address the problem, it might not be as pressing. This question helps understand potential points of comparison once the actual product is introduced. In the case of Dropbox, you can inquire about the tools they are using and what they are currently doing to solve the issue. Responses might involve sharing a workspace or forwarding emails, etc.

Why don’t you like the solutions you’ve tried? This can serve as a database for designing potential features. Avoid hypothetical questions like “How would you feel if this feature existed?” Users often don’t realize the full potential of new features. Henry Ford, for instance, found through user interviews that people desired faster horses rather than automobiles.

Three Key Phases of Conversing with Users

If you have an idea, find users experiencing the problem If you have a prototype, seek out the first adopters. If you’re launched, aim for Product-Market Fit(PMF)

Idea Phase Many successful business start with entrepreneurs addressing thier own problems After conceiving an idea, start by discussing it with friends and colleagues. A broad audience is unnecessary You can also meet potential users face-to face. For example, If firefighteres are your target, visiting a fire station directly is an option.

Tips Always take detailed notes

You can keep the conversation casual; formality is not required.

Respect the other person's time; 10–15 min may suffice.

Prototype phase Define your initial customer precisely Pose the following three

‘quantitative’ questions:

1) How moch does this problem cost them? (Degree of Pain)

2) How often does this problem occur?(Frequency)

3) What budget or authority do tyeh possess to solve this problem?(Capability) [3]

Post-Launch phase Iterate towards achieving PMF PMF can be considered reached if

over 40% of respondents would be “very disappointed” if the service disappeared

The Real Product Market Fit

I often talk to founders who believe they’ve found product/market fit when they haven’t. This is a huge problem because they start hiring people, increasing burn, and optimizing their product before they’ve actually discovered what needs to be built. I’m writing this post to help you understand when you’ve really found product/market fit. To start, read Marc Andreessen’s “On product/market fit for startups”. It has been the single most influential post for me as an entrepreneur and was the first time I ever read the term. Here’s how Andreessen defines product/market fit: The customers are buying the product just as fast as you can make it – or usage is growing just as fast as you can add more servers. Money from customers is piling up in your company checking account. You’re hiring sales and customer support staff as fast as you can. To put this another way, you have reached product/market fit when you are overwhelmed with usage—usually to the point where you can’t even make major changes to your product because you are swamped just keeping it up and running. Founders often try to signal they’ve reached product/market by pointing to their number of employees or a major round of funding. What I look for is a frantic founding team trying to deal with ever-growing numbers of happy, loyal, and ideally paying customers. Until then, stay lean, keep burn low, and resemble a Navy SEAL team instead of an Army battalion. Finding product market fit = focusing on the market first Founders often hold too tightly onto solutions and too loosely onto problems. The problem, i.e. the market, is the real opportunity. Your unique and special v1 idea on how to solve that problem is usually wrong and only through launching, talking to customers, and iterating will you actually find a product that reaches product market fit. Founder genius is most often expressed in choosing the right problem to solve. As Andreessen wrote, “the market pulls product out of the startup”. At Sequoia, they talk about finding customers who “have their hair on fire”. As a founder, I never took the time to really understand what that meant and I thought it was just an investor marketing saying. Now, when I talk to founders I extend the metaphor to illustrate it more clearly. If your friend was standing next to you and their hair was on fire, that fire would be the only thing they really cared about in this world. It wouldn’t matter if they were hungry, just suffered a bad breakup, or were running late to a meeting—they’d prioritize putting the fire out. If you handed them a hose—the perfect product/solution—they would put the fire out immediately and go on their way. If you handed them a brick they would still grab it and try to hit themselves on the head to put out the fire. You need to find problems so dire that users are willing to try half-baked, v1, imperfect solutions. At YC, we encourage founders to build MVPs (minimum viable products). In a good market, an MVP is all you need to get your initial customers in the door, interacting with you, and offering the feedback that will inform your v2, v3, and v4. The advantage that small companies have over big ones is that they can move fast, deal with problems by having unusually good customer service, and their customers expect less. So to find product market fit, choose a market where users have a real, meaningful problem, launch quickly, and listen to your users. Once you’ve actually reached product/market fit—congratulations—you can begin optimizing your core product, hiring specialists to increase your efficiency, and make strategic investments. Also, you’ve made it further than most startups ever dream.