

# How to Get and Evaluate Start-up Ideas

Hi, everyone. My name is Jared. I'm a partner at YC. I'm going to talk about how to get startup ideas. This talk is for anyone who doesn't yet have an idea for a startup, and wants to find one. It's also relevant if you have an idea, but aren't sure it's the one you want to stick with.

Now, this is a tricky topic, because startup ideas are subtle. No one, not even YC, knows for sure which ideas will succeed. But at YC, we have seen thousands of startups try thousands of ideas, and we have learned that there are some powerful statistical trends. Certain kinds of ideas, and certain ways of generating ideas are much more likely to succeed than others. And so, my goal here is to point you in directions with a higher likelihood of success.

Here's how it's going to work. First, I'm going to discuss the most common mistakes founders make when coming up with startup ideas. Then, I'm going to talk about how to evaluate startup ideas, and finally, I'm going to talk about recipes for generating startup ideas.

The first, most common mistake is believing that you need an amazing idea to get started. What this usually looks like is someone who believes the key to having a successful startup is starting with a brilliant idea. And so they're waiting to have a brilliant idea before they start anything. It's really easy, when you look at successful companies today, like Google or Facebook, to think, "Wow, that was such a brilliant idea." But I can tell you it only looks that way in hindsight. When Google started, it was, like, the 20th search engine. And when Facebook started, it was, like, the 20th social network. What made them successful wasn't a brilliant initial idea. It was a good enough initial idea, combined with great execution.

The next mistake is exactly the opposite: jumping into the first idea that comes to mind, without really stopping to think critically about whether it's a good idea at all. And it's amazing how common this is. Here's the thing. If your startup is successful, you will spend years of your life working on it. And if you're going to spend years of your life working on something, wouldn't it make sense to spend, say, at least a couple of weeks first, deciding what to work on? Surprisingly few founders do this, and so, if you do it, you are actually ahead of the curve. What I'm arguing here is that if you imagine a spectrum where one end is "try the first idea that comes to mind," and the other end is, "wait for the perfect idea," you wanna be somewhere in the middle. You should think of your initial idea as a good starting point.

Startup ideas morph over time. Take Airbnb, for example. Airbnb was initially literally a website for renting air beds at other people's houses. It morphed over time into all vacation rentals. So the thing you want to do is to pick a good starting point. If you have a good starting point, then even if your initial idea isn't great, it just needs tweaking to get to a great idea. Whereas if you pick a bad starting point, there might not be any way to morph into a good one. You'll have to start over completely.

The third mistake is to start with a solution instead of a problem. And I'll give an example. So, imagine you come up with a startup idea "Uber for plumbers." It's an app, you push a button, a plumber shows up. This is a solution. What problem does this solve? I don't know. Maybe it's hard to find plumbers. That might be a real problem, but my point is if you come up with an idea like this, you are starting with a solution. We see this so commonly at YC that we have a term for it. It's called a "Solution In Search of a Problem," or a SISP. And these are usually not great, because usually, you never actually find the problem. You're much better off starting with a problem, and then looking for solutions.

The last mistake is believing that startup ideas are hard to find. Actually, they're easy to find, because there are many, many real problems left in the world. If you're having trouble finding them, it's just that you haven't yet learned how to do it. Once you learn how to notice good startup ideas, you'll see them all over the place.

Let's talk about evaluating startup ideas. I'm actually going to give you a formula for this. This formula comes from my colleague, Dalton, who has an excellent Startup School talk called "All About Pivoting," which I really recommend. I tweaked it a bit to fit this talk. Here is my idea quality score formula. It's real easy. It is to evaluate your idea on four criteria, and then you take the average of the scores.

The first criteria is how big is this idea? The best way to judge this, I think, is to look for existing large companies that do something similar. So, suppose you want to start a new kind of bank. Well, there already are a bunch of banks, and they're big companies, so that suggests that this idea could be big. Your idea could also score well here if you're doing something in a market that's small today, but has a real chance of being huge in the future. And a good example of this is a company called Coinbase. Coinbase is a YC company that lets you buy and sell cryptocurrency. And when Coinbase was started, in 2012, Bitcoin was still brand new, and still very small. But even at that time, there were people who believed that it might someday, like, replace all existing money. And clearly, if that happens, it'll be very big, which is why Coinbase was a good idea.

Founder/market fit. This is like, are the founders experts in what they're doing? A good example of this is a company called Flexport, which helps companies import products from around the world. And the founder, Ryan, before he started Flexport, had worked in international trade for 10 years, and he knew, like, everything about it. So that's, like, 10 out of 10 founder/market fit.

The next criteria is how sure are you that you're solving a big problem? Ideally, you have personal experience with this problem. And finally, do you have a new, important insight into this idea? And a great example of this is Airbnb. When Airbnb got started, most people thought that the idea of allowing strangers you met on the internet to sleep in your apartment was really weird, and probably dangerous. But the Airbnb founders had actually tried this. They'd invite strangers from the internet to stay with them, and they realized it was really fun. So they had this insight that everyone else was wrong about this, and that's what made Airbnb such a good idea.

Here are some other signs to look for that your idea is probably a good one. You're making something you personally want to have. It turns out it's much easier to start a successful company if you're making something you yourself want, instead of something other people want. For one, you start with at least one user, yourself, but more importantly, when you're building for yourself, you can trust your intuition about what to build. If you're building for other people, you have to guess what they would want.

Another sign is that this only recently became possible. If something recently changed in the world, like a new technology or a new product you can build on top of, that's often a sign that a new opportunity has been created. Or, there are successful companies that do something similar. A good example of this is Rappi, which does food delivery in Latin America. When Rappi got started, there were already food delivery companies in other parts of the world, like DoorDash, that were doing very well. It just hadn't caught on in Latin America yet.

My last topic about evaluating startup ideas is to talk about filters. Filters are bad reasons to reject startup ideas. And the most dangerous part of filters is that they will cause you to reject your best startup ideas unconsciously, before they even bubble up to your conscious mind, and there are four big ones that are extremely important to understand. Almost everyone has these filters. And one of the most impactful things you can do to become good at generating startup ideas is to learn what these are, and to turn them off.

The first is rejecting ideas that seem hard to get started. Paul Graham wrote a terrific article about this, called "Schlep Blindness," which I really recommend reading. And the example Paul discusses is Stripe. Stripe is a YC company that makes it easy to integrate credit card payments to your website. And the fascinating thing about Stripe is that when Stripe launched, there were thousands of developers who already knew that this was a problem. They had tried to integrate credit card payments to their site, and they realized that the existing options sucked. But not one of them tried to fix it. Why is that? The reason is that to start Stripe, you had to do a bunch of things that seemed difficult. You had to get a deal with a bank. You had to learn a lot about the nitty gritty details of the world's credit card infrastructure. Those things sounded so hard that thousands of would-be founders who could have started Stripe earlier unconsciously shied away from the idea.

The second filter is ideas that are in a boring space. And a great example of this is Gusto, which makes payroll software. There were, once again, thousands of programmers who must have realized that payroll software sucked, but because it was a boring problem, nobody tried to fix it.

The third one is ideas that seem too ambitious. Sam Altman wrote an excellent blog post on this, called "Hard Startups." Founders instinctively shy away from really ambitious-sounding ideas because they are intimidating. But often, these are the ideas that turn into really big companies.

The last one is that founders instinctively shy away from spaces where there are existing competitors. You should actually err on the side of doing things with existing competitors. When founders go into spaces with no existing competitors, they usually find out that the reason there are no competitors is because nobody wants the product. The ideal situation is a market where there are existing competitors, but you have noticed something that they all seem to have missed. A great example of this is Dropbox. When Dropbox launched, there were already dozens of file syncing and storage solutions, but none of them had gotten very many users. Dropbox launched with a better product, and was able to beat all the competitors.

Now the part that you're probably waiting for, how to generate startup ideas. It is possible to sit down and explicitly think of startup ideas. And in a moment, we'll talk about how to do this. But it is actually not the best way to have startup ideas. The best way to have startup ideas is to notice them organically. There are great startup ideas all around you, and when you have a prepared mind, you see them everywhere. The problem is that when people sit down and try to think of startup ideas, they tend to think of bad ones. Whereas startup ideas that occur to you organically are actually quite likely to be good ones.

So first, I want to talk about how to have startup ideas organically. If you're not starting a company right now, this should be your preferred approach. Learn how to notice good startup ideas, then let the good ones occur to you. To have startup ideas organically, you first have to know what a good startup idea looks like, so you notice them when you encounter them. Then, you just keep an eye out for things that seem missing in the world.

If you're planning to start a startup way in the future, it's very helpful to become an expert on something valuable, and the best way is probably to get a job at the forefront of some field. If you're working at the forefront of some field, really any field, you'll see good startup ideas before other people. It's particularly helpful to go work at an existing startup. Many of the most successful YC companies came from ideas that the founders had while working at someone else's startup.

Okay, I'm gonna walk through seven recipes for explicitly generating startup ideas. I'm listing all seven recipes, to be comprehensive. This is basically all the ways I know to generate startup ideas. Now, I know of examples where all seven of these have worked, and led to a successful company, however, they are not all equally good. Some are much more likely to produce good ideas. Others are quite likely to produce bad ideas, and those solutions in search of problems I was talking about earlier. So, I'm going to list them in order, from best to worst.

This first recipe is the best one. It is the one that is most likely to lead to good ideas, and least likely to lead to false positives. Of the most successful companies YC has funded, about half of them can be traced back to roughly this recipe, and here it is. You start with what your team is especially good at, and think of ideas that you would have an unfair advantage in executing. The reason this is so effective is that any idea you come up with this way has automatic founder/market fit. You're basically generating all the ideas that have good founder/market fit.

Let's walk through some examples of how to do this. The first thing I do is I go through every company you've ever worked at. Internships are fine. And for each one, I'd ask yourself what are things you learned there that other people don't know? So, for example, SnapDocs. The founder of SnapDocs, Aaron, had worked in the mortgage industry for 10 years, and learned, like, everything about it. So he started SnapDocs, which builds software for managing mortgage paperwork. Then, ask yourself, for each company, what seemed broken? What parts of company life were clumsy? For example, Lattice is a company that makes software for performance reviews. And the way the founders had this idea is that they worked at a company together, and they noticed how bad their performance review software was. Then ask yourself if there were things that your company built in-house that other companies might need. A good example of this is Mixpanel. Before Suhail started Mixpanel, he worked at a company called Slide, that had built advanced analytics tools for internal projects. Suhail realized that other companies would need those same tools, and that's how he came up with Mixpanel.

Second recipe, think of things you wish someone else would build for you. A great example of this is DoorDash. The DoorDash founders were trying to get Thai food delivered to them in the suburbs, and they realized there was literally no way to do it. So, they built DoorDash.

Third recipe. What would you be excited to work on for 10 years, even if it didn't succeed? What things are you really passionate about? A great example of this is Boom, which is making a supersonic airplane. The founder of Boom, Blake, came up with the idea basically by asking himself this question. He had not worked in aerospace, and he was not a building-an-airplane expert, but he was just obsessed with the idea of supersonic travel. That said, I have seen this recipe lead founders astray, so I wanted to warn you about it. It can lead founders astray by encouraging them to work on something that they're very passionate about, but that does not have a clear path to being a big business. So, for example, you might be really passionate about collecting stamps. Doesn't necessarily make it a good place to look for startup ideas.

Number four, look for things that have changed in the world recently, and think of ideas that are now possible because of this change. For example, a new technology, a new developer platform, a new regulation, a new problem in the world. What are waves that you could ride? A maybe non-obvious example of this is a YC company called PlanGrid, which was acquired for a billion dollars. PlanGrid put construction blueprints on tablets, replacing paper. And what changed in the world that made PlanGrid possible was that Apple had created the iPad.

Number five, find companies that have been successful recently, and look for new variants of them. A good example of this is Standard Cognition. Now, the story of Standard Cognition is that Amazon has recently created Amazon Go, which is a grocery store that automatically tracks the things you take off the shelves, so you don't need to go to the cashier. Standard Cognition realized that every retail store would want the same technology, and that's what they do. This is a very common way that founders look for ideas, but unfortunately, it's not necessarily the best one. Ideas that are generated this way often take a form like "Uber for X," or "Airbnb for Y." These ideas are very often solutions in search of problems, so if you come up with an idea this way, you should be default skeptical, and you should think really hard about whether it is actually solving a problem.

Number six, you can also crowdsource coming up with an idea by talking to people you know, and asking them for problems they want solved. This is especially interesting to do with people who have particular areas of expertise, like people who work in an interesting industry, for example. The downside with this recipe is that most people are bad at seeing startup ideas. That's why this strategy seems to work particularly well if the people you ask are other startup founders. They already know how to notice good startup ideas.

And, number seven. Look for industries that seem broken. Any industry that seems broken is probably ripe for disruption. A good example of this is a YC company called LendUp, which is a replacement for payday lending. And part of the founding story of LendUp is that the founders just noticed that payday lending was an obviously broken and scammy industry. The downside of this recipe is it will often take you to ideas that have poor founder/market fit. It's not likely to work well if you don't know anything about the industry you're hoping to disrupt. LendUp worked well because the founders were already FinTech experts, and so, if you try this approach, I would recommend sticking with industries that you already know something about.