

How to Get and Evaluate Startup Ideas | Startup School

Tar pits, schleps, and boring ideas: how YC evaluates startup ideas

At least 70 percent of the hundred most valuable Y Combinator companies did not sit down and think up their idea. They noticed it. That gap is what the talk keeps circling back to, because founders who set out to invent a startup idea keep inventing the same small set of bad ones.

This is Jared Friedman's Startup School talk on getting and evaluating startup ideas. He is a partner at Y Combinator. He works through three things: the four most common mistakes founders make with startup ideas, ten questions for judging whether an idea is any good, and seven recipes for coming up with new ones. "I've got a lot of content to get through so I'm going to move fast," he says. "Buckle in."

He is also clear that helping you find an idea today is the smaller half of the goal. "If you are looking for a startup idea right now I'm going to try to help, but more importantly I'm going to try to give you the conceptual tools to think about startup ideas in a sophisticated way, the way that we think about them at YC."

Where this advice comes from

Four sources feed the talk. The first is data. "I analyzed the top 100 YC companies by valuation and I looked at how they all got their idea," he says. "So I started with some hard quantitative data on how recent billion dollar companies actually came up with their idea."

The second is a Paul Graham essay called "How to Get Startup Ideas," which he recommends directly. The third is operational: years of helping YC companies that pivot in the middle of a batch and watching which advice actually helps them land on something better. The fourth is the pile of rejections. "It comes from reading thousands of YC applications that we rejected and looking at the mistakes that caused good founders to come up with bad ideas, and those are the mistakes I want to help you all avoid."

He is also careful not to oversell what a good idea buys you. “No one, not even YC, knows for sure which ideas will succeed, and in any case whether your idea succeeds has as much to do with how well you execute as with your initial idea anyway.” He does claim something narrower. “Certain ideas are much more likely to succeed than others, and so my goal here is to help you stack the deck in your favor by starting with a promising idea.”

The four most common mistakes

The first mistake: a solution in search of a problem

The most common mistake is building something that does not solve a real problem for your users.

“Typically you can articulate the problem that you’re solving, you can put it in words,” Jared says, “but when you actually go and talk to the users, it’s just not something that they really care about.” YC has a name for this. A solution in search of a problem, or a SISP.

“A lot of founders come up with an idea with this kind of thought process. They go, hmm, AI is cool, what could I apply AI to? And then they go look for a problem that they could solve with AI.”

That is backwards, and the reason it is dangerous is that the hunt succeeds. “If you do that you’ll probably find a problem, but it will be a superficially plausible problem. It’ll be a made-up problem that people don’t really care about, rather than a real problem that people actually care about. If people don’t really care about the problem, they won’t really care about your solution.”

He wants it run the other way. “You want to fall in love with a problem. The best way to find a startup idea is to start with a high quality problem.”

Then comes the caveat, which he clearly expects to need. Some founders, he says, “decide to interpret that as guidance to work on some huge societal problem, like, I don’t know, global poverty or something.” “No doubt those are real problems,” he adds, “but they’re too abstract to make good starting points for startup ideas. You need something that’s more specific, something that’s tractable with a startup.”

The second mistake: getting stuck in a tar pit

Tar pit ideas fail slowly, and they hold you in place while they do it.

“There’s this certain set of common startup ideas that have been around forever,” Jared says. “They have been applying in droves to YC, batch after batch, for years, and when founders start working on these ideas, it’s like they’ve gotten stuck in tar. They never seem to go anywhere.” Hence the name: tar pit ideas.

They form the same way every time, around a widespread problem that lots of potential founders encounter. Each one looks like something a startup could easily fix. “But it’s an illusion. There’s actually a structural reason why it’s very hard or impossible to solve, which is why after all these years no one has solved it.”

They cost founders time precisely because they look so plausible. “They’re very tantalizing from a distance because they’re so superficially plausible as startup ideas,” he says, which is why they will cause “so many founders to waste months of their life stuck on a tar pit.”

His example has been arriving at YC for two decades, and he acts it out. It is the stereotypical college student idea. “You think, man, every Friday or Saturday night when I’m making plans to meet up with my friends, it’s so inefficient. I’m in all these different text threads and chat groups and we’re trying to make plans to meet up. I’m just going to make an app to make it more efficient.”

“Turns out that there are some structural reasons why this idea is hard, which is why in like 20 years of people applying to YC with this idea, nobody has actually pulled it off.” He can see why they keep trying. “It’s a problem that almost everyone encounters at some point, and it seems like it would be so easy to solve. You can just imagine the app. It’s just got like a list of events and you invite friends to it. It seems so simple.”

Jared does not tell you these ideas are impossible. “I’m even open-minded that somebody will eventually make the app to meet up with your friends idea work. It’s more accurate to think of them as common ideas that are much harder than they seem.”

So if you want to work on one, do the research first. “First, Google it. It’s amazing how many founders skip the step of just Googling for their own startup idea to see who has worked on it in the past.” Find the people who tried, talk to them if you can, and work out what the hard part is that stopped them.

The third and fourth mistakes: grabbing the first idea, or waiting for the perfect one

Jared treats these two as a pair, because they are opposite ends of the same axis.

On one end: “it is amazing how many founders will basically just jump into the first idea they have without even stopping to consider whether it would actually make a good business.”

On the other end, and more dangerous in his words, are “the founders on the opposite side of the spectrum who sit around waiting for the perfect startup idea, and of course there is no such thing. So these people just never actually start a company.”

Between them sits the place he wants you to land. “If you imagine that there’s a spectrum between picking the first idea that comes to mind and waiting for the perfect idea, somewhere in the middle there is this happy place, which is the place that you want to be.” The framing he borrows to describe it is Paul Graham’s: think of your idea as a good starting point. “No startup idea is perfect, and no matter what you start with it’s probably going to morph anyway. So you just want to have an initial idea that has enough interesting qualities that it can morph in the right direction.”

Ten questions to ask about any idea

The next ten questions are for judging an idea you already have, and they are not weighted equally.

Question one: do you have founder market fit

He puts the most important one first. “If I had to pick the one most important criteria, it’d probably be this one.” Founder market fit means: are you the right team to be working on this idea?

His example is PlanGrid, which makes an iPad app for viewing construction blueprints. The founders were Tracy and Ralph. “Tracy had worked in the construction industry and she knew a lot about construction, and Ralph was an awesome developer who was the perfect person to build this iPad app.”

The test is what team you would have imagined for the job. “If you’re going to imagine a team to start PlanGrid, the team that you would imagine would look something like that. That’s what good founder market fit looks like.” Or, more plainly: “it’s like this team is obviously the right team to work on the idea.”

Then he pushes it past a checklist item. “In fact, founder market fit is so important that I would recast your search for a startup idea.”

“When most people go to pick a startup idea they try to look for a good startup idea in the abstract, and instead I would think about this exercise as an exercise to pick a good idea for your team. You with me?” He follows it through. “It doesn’t matter if something is a good startup idea for someone else if it’s not a good idea for your team, so you may as well just look for ideas that you would actually be good at executing.”

Question two: how big is the market

Startups need a big market, which usually means a billion dollar market. Less obviously, there are two kinds of market worth starting in.

The first kind is markets that are big now. The second is markets that are small but growing fast.

Coinbase shows what the second kind looks like. “When Coinbase got started in 2012, the Bitcoin trading market was minuscule,” Jared says, “but even at that time it was pretty obvious that if Bitcoin succeeded the way people hoped that it would, this would eventually be a billion dollar market.”

Question three: how acute is the problem

This is the counterweight to the first mistake, and he says so himself: “as I said earlier, the most common mistake is just working on something that just isn’t really a problem, or it’s just not a problem that people care enough about.”

Brex sits at the far end. Brex, from the Winter 2017 batch, makes a credit card for startups. “Before Brex, if a startup in YC wanted a corporate credit card, they literally could not get one, because no bank would give a credit card to a startup.”

That is the bar. “That’s a good problem. If the alternative to your solution is literally nothing, that’s what a good problem looks like.”

Question four: do you have competition

Most founders read competition as a reason to stop.

“Most founders think that if you have competition that that’s bad, but counterintuitively it is the opposite. Most good startup ideas have competition.”

There is a condition attached. “If you were going up against especially entrenched competition, you typically need a new insight.”

Question five: do you want this, and do you know anyone who does

He gets through this one fast. “It’s amazing how often people start companies where the answer to both these questions is no. If that’s the case, you definitely got to worry that maybe nobody wants this. So definitely time to go talk to some users.”

Question six: has this only recently become possible, or necessary

A recent change in the world is usually what opens the window. “Something has recently changed in the world, like a new technology, regulatory change, or a new problem. That is often what creates a new opportunity.”

Checkr came out of exactly that. It runs background checks through an API. The story, roughly, is that delivery services like DoorDash, Instacart, and Uber started to take off, and all of them were hiring huge pools of delivery workers who needed background checks. Plenty of large background check companies already existed. “But they weren’t well suited for this very new use case, and that is exactly the kind of change in the world that creates a new opportunity.”

Question seven: are there proxies

A proxy is “a large company that does something similar to your startup but is not a direct competitor.”

Rappi does food delivery in Latin America. When it started, a few delivery companies in other parts of the world, DoorDash among them, were already doing very well, and they simply had not caught on in Latin America yet. “So DoorDash was a great proxy to show that this idea of doing food delivery in Latin America would probably work.”

Question eight: would you want to work on this for years

Jared flags this one as unreliable.

“This is a tricky one. Sure, if the answer to this question is yes, that’s a good sign, but often it’s not. Often an idea grows on founders over time as it starts to work.”

The reason he distrusts it is that it filters out a category he comes back to in a moment. “A lot of the best startup ideas are in boring spaces, like tax accounting software or something like that. Nobody starts off being passionate about tax accounting software. But tax accounting software is probably a good business, and if you’re actually running a successful business you tend to become passionate about it over time.”

Question nine: is this a scalable business

Short answer, if you are building pure software: yes, and you can move on. “Software scales infinitely, and you can just check this one off.”

It matters for everything else. “The place founders most often get into trouble here is with services businesses, like agencies or dev shops, anything that requires high skill human labor in order to serve your customers.”

Question ten: is this a good idea space

The last question needs a definition first. An idea space is a concept Jared credits to his colleague Dalton, who appears later in the same Startup School course, and it sits one level of abstraction above any particular idea. “It is a class of closely related startup ideas, like software for hospitals, or infrastructure monitoring tools, or food delivery services.”

The spread is the point. “Different idea spaces have wildly different hit rates.” The numbers: “over the last 10 years, if you started a company that did fintech infrastructure or vertical SaaS for enterprise, the probability that your company became a billion dollar company was astonishingly high, whereas if you started something in consumer hardware or social networks or ad tech, the success rate was orders of magnitude lower.”

He immediately declines to turn that into a stock tip. “I can’t say that that will continue to be the case for those specific areas, because spaces flip from hot to cold over time.” He does not drop the idea itself. “It is still worth thinking about picking a good idea space.”

A good idea space is one you expect to have a reasonable hit rate for new ideas, and one where you have founder market fit. It pays off when your first idea turns out to be wrong. “That way, even if your initial idea isn’t quite right, there are probably good adjacent ideas that you can drift into.”

Fivetran drifted its way there. They started by making a tool for data analysis, took it to some companies, and the companies did not want it. So they pivoted and built a different tool for data analysis, went back to the same companies, and the companies did not want that one either.

They were accumulating something other than a product. “Each time they went to companies and tried to sell them some tool for data analysis, they would learn more about what those companies actually wanted. And so eventually they sort of stumbled into an actual problem, into an actual tool for data analysis that companies actually wanted.”

“Because the Fivetran founders were shopping for ideas in a fertile idea space, they put themselves in a good position to bump into a good startup idea. If they had picked a bad idea space, they probably wouldn’t have found anything.”

Three things that make an idea look bad but actually make it good

Before the third part of the talk, Jared stops on a set of ideas that repel founders for the wrong reasons. His argument for hunting them is economic: “most founders will shy away from ideas like these, which leaves them on the table for smarter founders to go and grab them.”

There are three: ideas that are hard to get started, ideas in a boring space, and ideas that have existing competitors.

Hard to get started

Paul Graham wrote what Jared calls a terrific article about this, “Schlep Blindness,” and he recommends reading it. The case study in it is Stripe.

Stripe makes it easy to add credit card payments to a website. Nobody who could see the problem built it. “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 strangely, not one of them even tried to start Stripe.”

Jared calls that a fascinating question, given how many people were positioned to see it. The schlep is the answer. “Getting started building Stripe required some things that seemed really hard. You had to get a special deal with a bank, you had to learn a lot about the nitty gritty details of credit card infrastructure.”

That scare-off is the whole mechanism. “Those things seemed so hard that they scared off all the other people who might have started Stripe, which caused them to leave this 100 billion dollar opportunity on the table for the Stripe founders to go and pick up.”

Boring

This is the category that question eight already brushed against. Gusto makes payroll software, and payroll software is pretty boring.

“There are thousands of people who must have realized that payroll software sucked,” Jared says, “but because it was kind of a boring problem, nobody tried to fix it until the Gusto founders came along.”

Gusto is not a one-off. “Because most founders shy away from ideas like this, boring ideas like payroll software have a much higher hit rate than fun ideas like apps to find new restaurants to eat at, or apps to find the next song to listen to. Fun ideas get picked. Boring ideas get left on the table for a long time.”

He anticipates the objection and puts it in the reader’s mouth, by name: “now you might be thinking, Jared, why would I want to work on a boring idea? That sounds boring.”

Because a fun idea does not produce a fun job. “Even if you work on an idea that sounds fun at the outset, the day-to-day reality of your startup is going to be mostly the same anyway. Either way you’ll be mostly writing code, fixing bugs, talking to users, pretty much the same stuff.”

He flags the conclusion as his own argument. “I would argue that once the initial excitement of your idea has worn off and you are 6 or 12 months in and you are grinding out the execution that makes your idea actually work, how fun the initial idea sounded actually has little to no correlation with how much fun you will actually be having working on your company.”

Already has competitors

This returns to question four with a longer example, and Jared states the error before the fix: “founders incorrectly shy away from spaces where there are existing competitors. Counterintuitively, most good startup ideas have existing competitors.”

“When founders go into spaces with no existing competitors, they often find out that the reason that there are no competitors is because no one wants the product.” The situation he wants instead: “a market where there are existing competitors, but you’ve noticed something that they all seem to have missed, or they all just kind of suck.”

Dropbox was roughly the twentieth cloud file storage company to launch. “Naively you might have thought that that made this a bad market. You would have gone and looked and said, oh, there are already 20 competitors, it seems like a bad market to go into.”

“But if you were savvy about startup ideas, you would have realized that it actually made it a great market.” The inversion turns on one fact about those twenty. “Even though there were like 20 companies doing this, most people didn’t use any of them, and that strongly suggests that there actually was a problem here but the existing products hadn’t solved it.”

Drew, Dropbox’s founder, “had a very specific insight about what all of them were missing. His insight was basically their UI sucked.” It sucked in a particular way: “at the time, the way that they all worked is you had to go to their website and manually upload your files one at a time into their website, which sucked, of course.”

Drew’s fix skipped the website entirely. “If he integrated directly into the host operating system, he could just sync your files automatically without you having to do anything. And that was a real step function change in how convenient these services were to use, and that was the right insight.”

Organic ideas beat manufactured ones

Jared opens the third part of the talk by undercutting the method he is about to teach.

“It is possible to sit down and explicitly think of startup ideas, and in a moment I’m going to talk about how to do this, but it is actually not the best way. The best way to have startup ideas is to just notice them organically.”

The number he opens the section with comes from his own data. “If you look at the YC top 100 companies, at least 70 percent of them got their startup ideas organically rather than by sitting down and explicitly trying to think of a startup idea.”

Deliberate ideation goes wrong for a reason he has already covered. “When people sit down and try to think of startup ideas, they tend to think of bad ones. They’re especially likely to think of the same set of tar pit ideas that I talked about earlier, whereas startup ideas that occur to you organically are more likely to be good ones.”

If you are not starting a company imminently, there are three ways to put yourself in the path of an organic idea.

The first is to become an expert in something valuable. “If you’re working at the forefront of some field, you’ll see good startup ideas in that field.”

The second is the route he points to for getting there, and he cites Harj’s talk the week before: go work at a startup. “If you’re working at a startup you will become an expert in the thing that that startup does, and that is really putting yourself in a position to have great startup ideas.”

The third is for programmers: build things you find interesting even when they are not businesses and not clearly startup ideas. “Sometimes they turn into them over time.” Replit is his example: “this is exactly how Replit started. It was just something that Amjad found interesting. It wasn’t supposed to be a startup originally.”

The seven recipes

For founders who need an idea now rather than eventually, Jared gives seven recipes, ordered deliberately. “I’ve tried to list these in order of how likely they are to lead to actually good ideas, so start with the first ones.”

One: start with what your team is especially good at

This is the best recipe because of what it produces automatically. “Think of ideas that take advantage of your expertise. The reason that this is so effective is that any idea you come up with this way has automatic founder market fit.” He calls it a shortcut: “do you see how this is almost like a hack to generate the set of ideas that has founder market fit?”

The Rezi founders did this, and built something like an Opendoor for rental apartments. They had worked in real estate and debt financing and were experts in both. When they got into YC they spent the first month looking for ideas. “The smart thing that they did is they only looked at ideas in that idea space, at the intersection of real estate and fintech.”

“That is a very fertile idea space for startups. Many billion dollar companies have come out of that idea space. And the Rezi founders were experts in that idea space.” The search finished fast. “Their search for a startup idea was pretty quick and painless, and pretty quickly they came up with the idea for Rezi, which is an excellent idea and has perfect founder market fit.”

The instruction is blunt. “If you have specific expertise like the Rezi founders did, you should definitely start by looking at ideas in the things that you’re experts in.”

In the application pile he sees the mirror image. “It is weird how many startups apply to YC and we look at their applications and the founders are actually legitimate experts in something, but the idea that they’re applying with is something completely different.”

He also marks who this recipe is not for. “If you’re a young founder, if you’re in college or something, you may not have had a chance to develop the level of domain expertise that the Rezi founders had, so this may not be the right recipe for you.”

Two: a problem you were in an unusual position to see

The framing Jared adds to the familiar advice is the second half: a problem you have personally encountered, “ideally one that you’re in an unusual position to see.”

Vetcove is a website where veterinarians order supplies, an Amazon for vets. The founders are brothers, and their dad is a veterinarian. Growing up, they watched how he ordered supplies. “You’d have to call up a supplier on the phone and order stuff through a 1-800 number or something. It was very obvious that you could build an amazon.com kind of thing that would replace that.”

“I love the story of Vetcove,” he says, “because it’s such a great example of what a great startup opportunity looks like.” The gap runs in both directions, and that is what makes it an opportunity. “Thousands of veterinarians must have known that this was a real problem, that it was really annoying, that there wasn’t a basic website where you could just go to order supplies. But the thing is, veterinarians don’t start tech startups very often.”

And on the other side of that gap: “you had thousands of programmers in Silicon Valley who were banging their heads against SISP’s and tar pit ideas, totally unaware that over here there was a really great genuine problem to work on.”

Nobody else was there. “When the Vetcove founders got started they had no competition. This amazing idea just got left on the table for years.”

For these first two recipes Jared gives a specific exercise, the one he offers YC founders who are pivoting and need a new idea.

“For each founder on your team, go through every job you’ve ever had, plus all your internships, plus other life experiences, and think really carefully about each of them. What problems did you come across? What did you learn that other people don’t know? What are problems or opportunities that you’ve been in kind of a special position to see? Those are the best places to start looking for startup ideas.”

Three: things you wish existed

The classic advice, and the one Jared qualifies hardest.

DoorDash did it the clean way. “The founders of DoorDash were undergrads at Stanford, and the thing they really wanted was to be able to order food from local restaurants and have it delivered to their dorm. And before DoorDash, you couldn’t do that. So they started DoorDash.”

The warning is the tar pit again. “This is a great recipe, but this is the recipe that is most dangerous and potentially leading to tar pit ideas. So if you’re using this recipe, you just gotta stop and think for a second: is there a reason why this thing doesn’t exist yet?”

Four: what changed recently

COVID opened the most obvious recent window. “When the pandemic started it changed daily life for all of us, and many founders realized that this created the opportunity for new companies.” Some very successful startups came out of it. Gather Town is one. Jared describes it as a fun way to hang out with other people online. Its founders were working on a different idea that was not going so well, and pivoted when the pandemic started. “It was obvious that the change in behavior of the pandemic had created a bunch of new opportunities.”

Five: new variants on recent successes

Nuvocargo is basically Flexport for Latin America, helping US companies import from Mexico, and it is the variant of a company that was already working. Jared calls it the exception: “a good example of explicitly sitting down to try to think about startup ideas and actually finding a good one, which is the thing that I told you is hard to do.”

Deepak, the founder, picked it analytically, after a previous YC idea had failed to work. He went on a systematic search. “He picked it because it was a large market, because there were good proxies from other companies, Flexport.” And the gap in his background did not stop him. “He picked it even though he didn’t have deep domain expertise in the import export space, because he had some connections that would enable him to get started, and he felt like he would just be very good at running this kind of operationally intensive business.”

The outcome, in one line: “that worked really well. Nuvocargo is doing super well.”

Six: go and ask people what their problems are

Jared says this one works, then immediately qualifies it: “the downside with this recipe is it actually requires a lot of skill.”

Two things make it survivable. Pick a fertile idea space first. Then widen who you talk to inside it. “I would also recommend talking not just to potential customers, but also to founders of companies in that idea space, to get advice about what ideas are actually worth pursuing.”

AtoB is the worked example, and Jared spends the most time on it because of how deliberate it was. “The way that they came up with the idea for AtoB was very systematic, and so I’m going to walk you through how they did it and really break it down for you.” AtoB makes fuel cards, a special kind of credit card for truck drivers.

The founders started from the position a lot of stuck founders are in. They “were pretty young and they hadn’t acquired a lot of specific domain expertise like the Rezi founders had yet, but they really wanted to do a startup, and they wanted to do a startup that had a genuinely good idea.” They pivoted on getting into YC and spent the whole batch looking for a new idea.

Step one was picking the idea space: software for the trucking industry. They picked it without being experts in it, on structural grounds. “The trucking industry is a big industry, hasn’t been that disrupted by startups and software yet, so they just felt like there were probably some good problems to work on.”

That left them with a space and no map. “The problem is they didn’t know that much about the trucking industry, so they didn’t know what those problems were. And so they decided that they would turn themselves into experts in the trucking industry.”

In practice that meant driving to truck stops. “They actually physically drove to truck stops, which were places where truck drivers are just kind of milling about, and they would just walk up to truck drivers and start talking to them and ask them questions about what their problems were.” They also talked to founders who had started companies in the trucking space, to find out which problems were worth working on. “They would basically talk to anyone who knew anything about trucking that was willing to talk to them.”

Out of that came a map. “As they did that they began to put together a mental map of the space and where the good ideas were, where the bad ideas were.” They worked through a bunch of candidate ideas before landing on fuel cards.

“I love the example of AtoB,” he says, “because AtoB is one of the best new ideas to come out of YC in several years.” He goes further. “AtoB is a phenomenal company, and the approach that they used to find this idea is something that really anyone could do. Most founders don’t do this because it just sounds like too much work. So if you’re willing to put in the work, this is an amazing way to find a startup idea.”

Seven, and a bonus

The last recipe is one line. “Look for big industries that seem broken. Any big industry that seems broken is probably ripe for disruption.”

The bonus is aimed at people who have neither of the two things they need: find a co-founder who already has an idea. “There are a lot of people on Startup School co-founder matching right now that already have an idea and are looking for a co-founder. So if you don’t have a co-founder and you don’t have an idea, that could be a great hack to getting both at the same time.”

Just launch it and find out

Jared ends by conceding the limits of everything he has just said.

“It’s often hard to tell if a startup idea is good or not,” he says, “and so while I hope the concepts that I talked about here will help, typically the only way to know for sure if your startup idea is good is to just launch it and find out.”

And for anyone still holding an idea and undecided about it: “if after all this you’ve got a startup idea and you’re still kind of on the fence about whether it’s actually a good idea or not, that is my advice for you. Just launch it and find out.”