[Startup = Growth], Paul Graham

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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. [1]

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. [2]

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. [3] 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. [4]

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:

1. There's an initial period of slow or no growth while the startup tries to figure out what it's doing.

2. 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.

3. 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. [5]

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. [6]

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. [7]

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. [8]

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. [9]

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.

weekly	yearly
1%	1.7x
2%	2.8x
5%	12.6x
7%	33.7x
10%	142.0x

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. [10]

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. [11]

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. [12]

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? [13]

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. [14]

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.

Notes

[1] Strictly speaking it's not lots of customers you need but a big market, meaning a high product of number of customers times how much they'll pay. But it's dangerous to have too few customers even if they pay a lot, or the power that individual customers have over you could turn you into a de facto consulting firm. So whatever market you're in, you'll usually do best to err on the side of making the broadest type of product for it.

[2] One year at Startup School David Heinemeier Hansson encouraged programmers who wanted to start businesses to use a restaurant as a model. What he meant, I believe, is that it's fine to start software companies constrained in (a) in the same way a restaurant is constrained in (b). I agree. Most people should not try to start startups.

[3] That sort of stepping back is one of the things we focus on at Y Combinator. It's common for founders to have discovered something intuitively without understanding all its implications. That's probably true of the biggest discoveries in any field.

[4] I got it wrong in "How to Make Wealth" when I said that a startup was a small company that takes on a hard technical problem. That is the most common recipe but not the only one.

[5] In principle companies aren't limited by the size of the markets they serve, because they could just expand into new markets. But there seem to be limits on the ability of big companies to do that. Which means the slowdown that comes from bumping up against the limits of one's markets is ultimately just another way in which internal limits are expressed.

It may be that some of these limits could be overcome by changing the shape of the organization — specifically by sharding it.

[6] This is, obviously, only for startups that have already launched or can launch during YC. A startup building a new database will probably not do that. On the other hand, launching something small and then using growth rate as evolutionary pressure is such a valuable technique that any company that could start this way probably should.

[7] If the startup is taking the Facebook/Twitter route and building something they hope will be very popular but from which they don't yet have a definite plan to make money, the growth rate has to be higher, even though it's a proxy for revenue growth, because

such companies need huge numbers of users to succeed at all.

Beware too of the edge case where something spreads rapidly but the churn is high as well, so that you have good net growth till you run through all the potential users, at which point it suddenly stops.

[8] Within YC when we say it's ipso facto right to do whatever gets you growth, it's implicit that this excludes trickery like buying users for more than their lifetime value, counting users as active when they're really not, bleeding out invites at a regularly increasing rate to manufacture a perfect growth curve, etc. Even if you were able to fool investors with such tricks, you'd ultimately be hurting yourself, because you're throwing off your own compass.

[9] Which is why it's such a dangerous mistake to believe that successful startups are simply the embodiment of some brilliant initial idea. What you're looking for initially is not so much a great idea as an idea that could evolve into a great one. The danger is that promising ideas are not merely blurry versions of great ones. They're often different in kind, because the early adopters you evolve the idea upon have different needs from the rest of the market. For example, the idea that evolves into Facebook isn't merely a subset of Facebook; the idea that evolves into Facebook is a site for Harvard undergrads.

[10] What if a company grew at 1.7x a year for a really long time? Could it not grow just as big as any successful startup? In principle yes, of course. If our hypothetical company making $1000 a month grew at 1% a week for 19 years, it would grow as big as a company growing at 5% a week for 4 years. But while such trajectories may be common in, say, real estate development, you don't see them much in the technology business. In technology, companies that grow slowly tend not to grow as big.

[11] Any expected value calculation varies from person to person depending on their utility function for money. I.e. the first million is worth more to most people than subsequent millions. How much more depends on the person. For founders who are younger or more ambitious the utility function is flatter. Which is probably part of the reason the founders of the most successful startups of all tend to be on the young side.

[12] More precisely, this is the case in the biggest winners, which is where all the returns come from. A startup founder could pull the same trick of enriching himself at the company's expense by selling them overpriced components. But it wouldn't be worth it for the founders of Google to do that. Only founders of failing startups would even be tempted, but those are writeoffs from the VCs' point of view anyway.

[13] Acquisitions fall into two categories: those where the acquirer wants the business, and those where the acquirer just wants the employees. The latter type is sometimes called an HR acquisition. Though nominally acquisitions and sometimes on a scale that has a significant effect on the expected value calculation for potential founders, HR acquisitions are viewed by acquirers as more akin to hiring bonuses.

[14] I once explained this to some founders who had recently arrived from Russia. They found it novel that if you threatened a company they'd pay a premium for you. "In Russia they just kill you," they said, and they were only partly joking. Economically, the fact that established companies can't simply eliminate new competitors may be one of the most valuable aspects of the rule of law. And so to the extent we see incumbents suppressing competitors via regulations or patent suits, we should worry, not because it's a departure from the rule of law per se but from what the rule of law is aiming at.

After 20 Years: Updating the Berkus Method of Valuation

By: Dave Berkus, Dave Berkus, "Super angel" investor, tech futurist

This post originally appeared on Berkonomics.com

Well, it had to happen. Originally created in the mid 1990's to help with the imprecise problem of how to value early stage companies, especially those in technology, I developed what soon became known as "The Berkus Method" when published in the popular book, "*Winning Angels*" by Harvard's Amis and Stevenson with my permission in 2001. But a lot of time has passed since then.

There is a universal truth: fewer than one in a thousand start-ups meet or exceed their projected revenues in the periods planned. So how do you use financial projections as valuation metrics when you know the odds of those being accurate predictors of the future are so very unreliable?

I thought then that the best way to value a start-up is to give value to those elements of progress by the entrepreneur or team that reduce risk of success. It is the exact opposite of giving value to projected financial success, except for the hurdle which I use to filter out smaller opportunities. I must believe that the candidate company, if successful, could achieve some level of gross revenue at the end of the fifth year in business. Today, for me, that hurdle number is $20 million.

The Berkus Method assigns a number, a financial valuation, to each of four major elements of risk faced by all young companies – after crediting the entrepreneur some basic value for the quality and potential of the idea itself. Today, the method as explained, adds $500,000 in value for each of the following risk-reduction elements:

Note that these numbers are maximums that can be "earned" to form a valuation, allowing for a pre-revenue valuation of up to $2 million (or a post roll-out value of up to $2.5 million), but certainly also allowing the investor to put much lower values into each test, resulting in valuations well below that amount.

In 2005, Alan McCann created a graphic representation of the Method:

Note that Allan changed the title of the risks from *technology-execution-market-production to investment-marketing-execution-development*. And he added the cohort responsible for each, a nice touch. I should have done more about this subtle interpretation when first seeing this, and will attempt to make up for that now.

Because the Internet has such a long memory and documents from the distant past can be found with ease, a search the "The Berkus Method" today will yield any number of conflicting valuation criteria and element amounts culled from the many subsequent publications of the method over the ensuing years.

It has occurred to me lately that the original matrix is too restrictive, and should be a suggestion rather than a rigid form. That is: a user of the Method should be able to list those risks that are most important to the target company and to the investor, and assign maximum values to each that are appropriate to the industry and to the times.

For example, $500,000 maximum value to each element yields either a maximum pre-money enterprise valuation of $$2 million or $2.5 million if all elements are "perfect" for a target in the eyes of the investor. Yet, the current HALO Report from the Angel Capital Association containing average pre-money valuations for angel investors demonstrates that the US average, at least for the moment, is higher than this amount. Valuations are higher in Silicon Valley, Silicon Beach, New York and the North Carolina corridor than in Oklahoma City, Kansas City or Miami. The Method should allow for this.

The Method should be flexible enough for its users to negotiate or create a maximum valuation they are willing to accept in a perfect situation, and to assign risk elements that might be more important to them than those listed above. For example, in Silicon Valley, a "big data" startup might competitively call for a $1.5 million maximum value per element, while the same startup in Nebraska might find $500,000 appropriate. As for listed elements, a medical device startup might replace "marketing risk" with "FDA approvals risk."

There is no question that start-up valuations must be kept at a low enough amount to allow for the extreme risk taken by the investor and to provide some opportunity for the investment to achieve a ten-times increase in value over its life.

Once a company is in revenues, the Method is no longer applicable, as most everyone will use actual revenues to project value over time.

The risk is that these options will make a very simple process two steps more complex, frustrating the original purpose of elegant simplicity. But hopefully, the relaxation of restrictions listed here will allow The Berkus Method to live into another generation, survive the effects of inflation, and address better the specific needs of niche market valuations.

Scorecard Valuation Methodology (Rev 2019): Establishing the Valuation of Pre-revenue, Start-up Companies

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By: Bill Payne, Frontier Angels

This article was originally written in May 2001 and has been updated multiple times. Others have referred to this and similar methods as the Benchmark Method and the Bill Payne Method. The Scorecard Valuation Methodology is useful for investment in most pre-seed and seed stage opportunities, except those with very high capital requirements prior to achieving first revenues (such as some life science and energy deals).

Background

Individual accredited investors in typical angel deals put personal capital at risk for an equity share[1] of growth-oriented, start-up companies. These angel investors generally invest $25,000 to $100,000[2] in a round totaling $250,000 to $1,000,000. In 2018, the valuation of pre-revenue, start-up companies is typically in the range of $3 to $8 million and is established by negotiations between the entrepreneur and the angel investors. For this round of investment, the angels collectively purchase 10-30% of the equity of the company and are seeking a return on investment of 10-30X in a period of five to ten years.

Active angels invest in a diversified portfolio of at least 10 companies, usually spreading their investments over a few years. Experience proves that half of these companies will fail (returning nothing or less than capital invested), another 3-4 will provide a modest return on investment of 1X to 5X, and one or hopefully two of the ten companies will return 10X to 30X on the initial investment over a five to ten year period of time. In the end, such a portfolio might yield the angel investor a total return on investment of 20% per year or more. These anticipated outcomes first reported by Wiltbank's "Returns to Angels in Groups" in November 2007 have since been validated by several angel groups in the US.

Angels typically invest in companies operating in industry sectors with which they are familiar. Diversification across industry sectors is not as easily achieved for angels as could be accomplished in public markets, but can be achieved by co-investing with trusted angel colleagues in a broader set of businesses. A local affiliation of angels can be important to achieving a diversified portfolio. Working within a group of angel investors also expands the pool of expert resources and helps divide the work of screening companies and investment due diligence. Furthermore, angel groups frequently syndicate (co-invest) with other trusted angel organizations in an effort to help fill round of investment for local companies and assist members in diversifying their portfolios of angel investments.

To achieve a satisfactory return on investment, for every ten investments in an angel's portfolio, one or two companies must return 10–30X on the initial investment. It is, of course, impossible to predict at the outset which of these portfolio companies will be successful and which will fail. It is necessary, then, that each investment in the angel

1 A significant fraction of pre-seed and seed stage deals are done as convertible debt. In such deals, this method is useful in determining the cap on the note, that is, the maximum valuation at which the notes will later be converted into equity.

2 Some angels invest as part of an angel or seed fund. Angels in angel funds invest their pro-rata share of the fund in each deal done.

portfolio demonstrates the potential to scale sufficiently to provide a 10-30X return on investment. Due to the high failure rate in startup companies, including a substantial number of investments with lesser growth opportunities in an angel investor's portfolio only reduces the possible return on the entire portfolio.

Scorecard Valuation Methodology

This method compares the target company to typical angel-funded startup ventures and adjusts the median valuation of recently funded companies in the region to establish a pre-money valuation of the target. Such comparisons can only be made for companies at the same stage of development, in this case, for pre-revenue (or minimal revenue) startup ventures.

The **first step** in using the Scorecard Method is to determine the median pre-money valuation[3] of pre-revenue companies in the region and business sector of the target company. Pre-money valuation varies by geography within the US economy and with the competitive environment for startup ventures within a region. In most regions, the pre-money valuation does not vary significantly from one business sector to another[4].

If insufficient local data is available, angels can refer to recent national studies published by the Angel Capital Association in the "Angel Funders Report," as is shown below:

3 Pre-money valuation is the value of a startup enterprise just before investment. Post-money valuation is the value of this enterprise just after investment. Hence: pre-money valuation + investment = post-money valuation.

4 As mentioned above, some startups in life science and other sectors required huge amount of capital to finalize revenue generation. For these companies, the Scorecard Method may be less useful.

For purposes of this report, let's assume the midpoint between the average Pre-Seed Deal ($4M) and Seed Stage Deal ($5M) is an appropriate median local pre-money valuation, that is, $4.5 million (our starting point for this example). The Scorecard Method then adjusts this starting point by considering the strengths and weaknesses of the target startup.

The **next step** in determining the pre-money valuation of pre-revenue companies using the Scorecard Method is to compare the target company to your perception of similar deals done in your region, considering the following factors:

Strength of the Management Team	0-30%
Size of the Opportunity	0-25%
Product/Technology	0-15%
Competitive Environment	0-10%
Marketing/Sales Channels/Partnerships	0-10%
Need for Additional Investment	0 – 5%
Other	0 – 5%

A Valuation Worksheet is provided in the appendix to this document to assist readers in judging the relative strength of target companies for the above categories.

The subjective ranking of factors (above) is typical for investor appraisal of startup ventures. Some are surprised to find that investor rankings of product and technology are below those of the management team and the size of the opportunity. In building a business, the quality of the team is paramount to success. A great team will fix early product flaws, but the reverse is not true. And, as has been discussed earlier, scalability is critical to investor returns. Good product and intellectual property are important, but the quality of the team is key.

Making the Valuation Calculation

To provide an example, assume a company with an average product and technology (100% of norm), a strong team (125% of norm) and a large market opportunity (150% of norm). The company can get to positive cash flow with a single angel round of investment (100% of norm). Looking at the strength of the competition in the market, the target is weaker (75% of norm) but early customer feedback on the product is excellent (Other = 100%). The company needs some additional work on building sales channels and partnerships (80% of norm). Using this data, we can complete the following calculation:

COMPARISON FACTOR

RANGE

TARGET

COMPANY

FACTOR

Strength of Entrepreneur and Team

30% max

125%

0.3750

Size of the Opportunity

25% max

150%

0.3750

Product/Technology

15% max

100%

0.1500

Competitive Environment

10% max

75%

0.0750

Marketing/Sales/Partnerships

10% max

80%

0.0800

Need for Additional Investment

5% max

100%

0.0500

Other factors (great early customer feedback)

5% max

100%

0.0500

Sum

1.1550

Multiplying the Sum of Factors (1.155) times the median pre-money valuation of $4.5 million (the appropriate median local pre-money valuation discussed above), we arrive at a pre-money valuation for the target company of just over $5.2 million.

Summary

Key to the Scorecard Method is a good understanding of the median (and range) of pre-money valuation of pre-revenue companies in a region. With this data in hand, the Scorecard Method gives angels subjective techniques to adjust the median valuation of local startups in similar business sectors to arrive at a pre-money valuation for a target seed stage company. Savvy entrepreneurs can use these tools to prepare for negotiations of valuation with investors.

Appendix to Scorecard Valuation Methodology

Armed with median and range of pre-money valuations for pre-revenue, start-up companies in the region, what determines where in that range is a fair valuation for a specific company?

The worksheet below is an empirical Valuation Worksheet. This worksheet will not provide the investor with a neat and tidy dollar valuation for a pre-revenue, start-up company! This worksheet provides the investor with a basis for deciding if a start-up company should be valued near the top or bottom of the range of values that might reasonably be applied to such an early stage venture.

The worksheet is a listing of issues and factors that should be considered in judging the value of a company. Note the following features of the worksheet:

1. The major factors are listed in order of importance in valuing start-up companies.
2. Each major factor has been assigned a weighted ranking. For example, the “Strength of the management team” is worth 30% of the valuation of the company, while the “Need for subsequent funding” is ranked as only 5% of the value of the company.
3. Within each major factor, the impact of each issue has been assigned a valuation ranking from +++ (very positive) to – – – (very negative), to assist the investor in deciding the overall weighted ranking to be assigned to the valuation of a start-up company.
4. Some factors, such as the entrepreneurs’ lack of “coachability” may be deal-killers. Those are indicated with asterisks (**).

No two angel investors will value a company (or business plan) the same, however, with some practice, this worksheet will allow investors to compare one company to the next and assist angels in deciding if a company's valuation should be near the high end of a reasonable range in valuation or, on the other hand, near the bottom of the range of valuations.

[The Only Thing That Matters], Marc Andreessen

This post is all about the only thing that matters for a new startup.

But first, some theory:

If you look at a broad cross-section of startups—say, 30 or 40 or more; enough to screen out the pure flukes and look for patterns—two obvious facts will jump out at you.

First obvious fact: there is an incredibly wide divergence of success—some of those startups are insanely successful, some highly successful, many somewhat successful, and quite a few of course outright fail.

Second obvious fact: there is an incredibly wide divergence of caliber and quality for the three core elements of each startup—*team*, *product*, and *market*.

At any given startup, the team will range from outstanding to remarkably flawed; the product will range from a masterpiece of engineering to barely functional; and the market will range from booming to comatose.

And so you start to wonder—what correlates the most to success—*team*, *product*, or *market*? Or, more bluntly, **what causes success?** And, for those of us who are students of startup failure—**what's most dangerous: a bad team, a weak product, or a poor market?**

Let's start by defining terms.

The caliber of a startup *team* can be defined as the suitability of the CEO, senior staff, engineers, and other key staff relative to the opportunity in front of them.

You look at a startup and ask, will this team be able to optimally execute against their opportunity? I focus on effectiveness as opposed to experience, since the history of the tech industry is full of highly successful startups that were staffed primarily by people who had never "done it before".

The quality of a startup's *product* can be defined as how impressive the product is to one customer or user who actually uses it: How easy is the product to use? How feature rich is it? How fast is it? How extensible is it? How polished is it? How many (or rather, how few) bugs does it have?

The size of a startup's *market* is the the number, and growth rate, of those customers or users for that product.

(Let's assume for this discussion that you can make money at scale—that the cost of acquiring a customer isn't higher than the revenue that customer will generate.)

Some people have been objecting to my classification as follows: "How great can a product be if nobody wants it?" In other words, isn't the quality of a product defined by how appealing it is to lots of customers?

No. Product quality and market size are completely different.

Here's the classic scenario: the world's best software application for an operating system nobody runs. Just ask any software developer targeting the market for BeOS, Amiga, OS/2, or NeXT applications what the difference is between great product and big market.

So:

If you ask entrepreneurs or VCs which of *team*, *product*, or *market* is most important, many will say *team*. This is the obvious answer, in part because in the beginning of a startup, you know a lot more about the team than you do the product, which hasn't been built yet, or the market, which hasn't been explored yet.

Plus, we've all been raised on slogans like "people are our most important asset"—at least in the US, pro-people sentiments permeate our culture, ranging from high school self-esteem programs to the Declaration of Independence's inalienable rights to life, liberty, and the pursuit of happiness—so the answer that team is the most important *feels* right.

And who wants to take the position that people don't matter?

On the other hand, if you ask engineers, many will say *product*. This is a product business, startups invent products, customers buy and use the products. Apple and Google are the best companies in the industry today because they build the best products. Without the product there is no company. Just try having a great team and no product, or a great market and no product. What's wrong with you? Now let me get back to work on the product.

Personally, I'll take the third position—I'll assert that *market* is the most important factor in a startup's success or failure.

Why?

In a great market—a market with lots of real potential customers—the market *pulls* product out of the startup.

The market needs to be fulfilled and the market *will* be fulfilled, by the first viable product that comes along.

The product doesn't need to be great; it just has to basically work. And, the market doesn't care how good the team is, as long as the team can produce that viable product.

In short, customers are knocking down your door to get the product; the main goal is to actually answer the phone and respond to all the emails from people who want to buy.

And when you have a great market, the team is remarkably easy to upgrade on the fly.

This is the story of search keyword advertising, and Internet auctions, and TCP/IP routers.

Conversely, in a terrible market, you can have the best product in the world and an absolutely killer team, and it doesn't matter—*you're going to fail*.

You'll break your pick for years trying to find customers who don't exist for your marvelous product, and your wonderful team will eventually get demoralized and quit, and your startup will die.

This is the story of videoconferencing, and workflow software, and micropayments.

In honor of Andy Rachleff, formerly of Benchmark Capital, who crystallized this formulation for me, let me present *Rachleff's Law of Startup Success*:

The #1 company-killer is lack of market.

Andy puts it this way:

- When a great team meets a lousy market, market wins.
- When a lousy team meets a great market, market wins.
- When a great team meets a great market, something special happens.

You can obviously screw up a great market—and that has been done, and not infrequently—but assuming the team is baseline competent and the product is fundamentally acceptable, a great market will tend to equal success and a poor market will tend to equal failure. **Market matters most**.

And neither a stellar team nor a fantastic product will redeem a bad market.

OK, so what?

Well, first question: Since team is the thing you have the most control over at the start, and everyone wants to have a great team, what does a great team actually get you?

Hopefully a great team gets you at least an OK product, and ideally a great product.

However, I can name you a bunch of examples of great teams that totally screwed up their products. Great products are really, really hard to build.

Hopefully a great team also gets you a great market—but I can also name you lots of examples of great teams that executed brilliantly against terrible markets and failed. *Markets that don't exist don't care how smart you are*.

In my experience, the most frequent case of great team paired with bad product and/or terrible market is the second- or third-time entrepreneur whose first company was a huge success. People get cocky, and slip up. There is one high-profile, highly successful software entrepreneur right now who is burning through something like $80 million in venture funding in his latest startup and has practically nothing to show for it except for some great press clippings and a couple of beta customers—because there is virtually no market for what he is building.

Conversely, I can name you any number of weak teams whose startups were highly successful due to explosively large markets for what they were doing.

Finally, to quote Tim Shephard: "A great team is a team that will always beat a mediocre team, given the same market and product."

Second question: Can't great products sometimes create huge new markets?

Absolutely.

This is a best case scenario, though.

VMWare is the most recent company to have done it—VMWare's product was so profoundly transformative out of the gate that it catalyzed a whole new movement toward operating system virtualization, which turns out to be a monster market.

And of course, in this scenario, it also doesn't really matter how good your team is, as long as the team is good enough to develop the product to the baseline level of quality the market requires and get it fundamentally to market.

Understand I'm not saying that you should shoot low in terms of quality of team, or that VMWare's team was not incredibly strong—it was, and is. I'm saying, *bring a product as transformative as VMWare's to market and you're going to succeed*, full stop.

Short of that, I wouldn't count on your product creating a new market from scratch.

Third question: as a startup founder, what should I do about all this?

Let's introduce *Rachleff's Corollary of Startup Success:*

The only thing that matters is getting to product/market fit.

Product/market fit means being in a good market with a product that can satisfy that market.

You can always feel when product/market fit isn't happening. The customers aren't quite getting value out of the product, word of mouth isn't spreading, usage isn't growing that fast, press reviews are kind of "blah", the sales cycle takes too long, and lots of deals never close.

And you can always feel product/market fit when it's happening. 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. Reporters are calling because they've heard about your hot new thing and they want to talk to you about it. You start getting entrepreneur of the year awards from Harvard Business School. Investment bankers are staking out your house. You could eat free for a year at Buck's.

Lots of startups fail before product/market fit ever happens.

My contention, in fact, is that they fail *because* they never get to product/market fit.

Carried a step further, I believe that the life of any startup can be divided into two parts: *before product/market fit* (call this "BPMF") and *after product/market fit* ("APMF").

When you are BPMF, focus obsessively on getting to product/market fit.

Do whatever is required to get to product/market fit. Including changing out people, rewriting your product, moving into a different market, telling customers no when you don't want to, telling customers yes when you don't want to, raising that fourth round of highly dilutive venture capital—whatever is required.

When you get right down to it, you can ignore almost everything else.

I'm not suggesting that you *do* ignore everything else—just that judging from what I've seen in successful startups, you *can*.

Whenever you see a successful startup, you see one that has reached product/market fit—and usually along the way screwed up all kinds of other things, from channel model to pipeline development strategy to marketing plan to press relations to compensation policies

to the CEO sleeping with the venture capitalist. And the startup is still successful.

Conversely, you see a surprising number of **really well-run startups** that have all aspects of operations completely buttoned down, HR policies in place, great sales model, thoroughly thought-through marketing plan, great interview processes, outstanding catered food, 30" monitors for all the programmers, top tier VCs on the board—**heading straight off a cliff due to not ever finding product/market fit**.

Ironically, once a startup is successful, and you ask the founders what made it successful, they will usually cite all kinds of things that had nothing to do with it. People are terrible at understanding causation. But in almost every case, the cause was actually product/market fit.

Because, really, what else could it possibly be?

[Editorial note: this post obviously raises way more questions than it answers. How exactly do you go about getting to product/market fit if you don't hit it right out of the gate? How do you evaluate markets for size and quality, especially before they're fully formed? What actually makes a product "fit" a market? What role does timing play? How do you know when to change strategy and go after a different market or build a different product? When do you need to change out some or all of your team? And why can't you count on on a great team to build the right product and find the right market? All these topics will be discussed in future posts in this series.]

[Default Alive or Default Dead?], Paul Graham

October 2015

When I talk to a startup that's been operating for more than 8 or 9 months, the first thing I want to know is almost always the same. Assuming their expenses remain constant and their revenue growth is what it has been over the last several months, do they make it to profitability on the money they have left? Or to put it more dramatically, by default do they live or die?

The startling thing is how often the founders themselves don't know. Half the founders I talk to don't know whether they're default alive or default dead.

If you're among that number, Trevor Blackwell has made a handy calculator you can use to find out.

The reason I want to know first whether a startup is default alive or default dead is that the rest of the conversation depends on the answer. If the company is default alive, we can talk about ambitious new things they could do. If it's default dead, we probably need to talk about how to save it. We know the current trajectory ends badly. How can they get off that trajectory?

Why do so few founders know whether they're default alive or default dead? Mainly, I think, because they're not used to asking that. It's not a question that makes sense to ask early on, any more than it makes sense to ask a 3 year old how he plans to support himself. But as the company grows older, the question switches from meaningless to critical. That kind of switch often takes people by surprise.

I propose the following solution: instead of starting to ask too late whether you're default alive or default dead, start asking too early. It's hard to say precisely when the question switches polarity. But it's probably not that dangerous to start worrying too early that you're default dead, whereas it's very dangerous to start worrying too late.

The reason is a phenomenon I wrote about earlier: the fatal pinch. The fatal pinch is default dead + slow growth + not enough time to fix it. And the way founders end up in it is by not realizing that's where they're headed.

There is another reason founders don't ask themselves whether they're default alive or default dead: they assume it will be easy to raise more money. But that assumption is often false, and worse still, the more you depend on it, the falser it becomes.

Maybe it will help to separate facts from hopes. Instead of thinking of the future with vague optimism, explicitly separate the components. Say "We're default dead, but we're counting on investors to save us." Maybe as you say that, it will set off the same alarms in your head that it does in mine. And if you set off the alarms sufficiently early, you may be able to avoid the fatal pinch.

It would be safe to be default dead if you could count on investors saving you. As a rule their interest is a function of growth. If you have steep revenue growth, say over 5x a

year, you can start to count on investors being interested even if you're not profitable. [1] But investors are so fickle that you can never do more than start to count on them. Sometimes something about your business will spook investors even if your growth is great. So no matter how good your growth is, you can never safely treat fundraising as more than a plan A. You should always have a plan B as well: you should know (as in write down) precisely what you'll need to do to survive if you can't raise more money, and precisely when you'll have to switch to plan B if plan A isn't working.

In any case, growing fast versus operating cheaply is far from the sharp dichotomy many founders assume it to be. In practice there is surprisingly little connection between how much a startup spends and how fast it grows. When a startup grows fast, it's usually because the product hits a nerve, in the sense of hitting some big need straight on. When a startup spends a lot, it's usually because the product is expensive to develop or sell, or simply because they're wasteful.

If you're paying attention, you'll be asking at this point not just how to avoid the fatal pinch, but how to avoid being default dead. That one is easy: don't hire too fast. Hiring too fast is by far the biggest killer of startups that raise money. [2]

Founders tell themselves they need to hire in order to grow. But most err on the side of overestimating this need rather than underestimating it. Why? Partly because there's so much work to do. Naive founders think that if they can just hire enough people, it will all get done. Partly because successful startups have lots of employees, so it seems like that's what one does in order to be successful. In fact the large staffs of successful startups are probably more the effect of growth than the cause. And partly because when founders have slow growth they don't want to face what is usually the real reason: the product is not appealing enough.

Plus founders who've just raised money are often encouraged to overhire by the VCs who funded them. Kill-or-cure strategies are optimal for VCs because they're protected by the portfolio effect. VCs want to blow you up, in one sense of the phrase or the other. But as a founder your incentives are different. You want above all to survive. [3]

Here's a common way startups die. They make something moderately appealing and have decent initial growth. They raise their first round fairly easily, because the founders seem smart and the idea sounds plausible. But because the product is only moderately appealing, growth is ok but not great. The founders convince themselves that hiring a bunch of people is the way to boost growth. Their investors agree. But (because the product is only moderately appealing) the growth never comes. Now they're rapidly running out of runway. They hope further investment will save them. But because they have high expenses and slow growth, they're now unappealing to investors. They're unable to raise more, and the company dies.

What the company should have done is address the fundamental problem: that the product is only moderately appealing. Hiring people is rarely the way to fix that. More often than not it makes it harder. At this early stage, the product needs to evolve more than to be "built out," and that's usually easier with fewer people. [4]

Asking whether you're default alive or default dead may save you from this. Maybe the alarm bells it sets off will counteract the forces that push you to overhire. Instead you'll be compelled to seek growth in other ways. For example, by doing things that don't scale, or by redesigning the product in the way only founders can. And for many if not most startups, these paths to growth will be the ones that actually work.

Airbnb waited 4 months after raising money at the end of Y Combinator before they hired their first employee. In the meantime the founders were terribly overworked. But

they were overworked evolving Airbnb into the astonishingly successful organism it is now.

Notes

[1] Steep usage growth will also interest investors. Revenue will ultimately be a constant multiple of usage, so x% usage growth predicts x% revenue growth. But in practice investors discount merely predicted revenue, so if you're measuring usage you need a higher growth rate to impress investors.

[2] Startups that don't raise money are saved from hiring too fast because they can't afford to. But that doesn't mean you should avoid raising money in order to avoid this problem, any more than that total abstinence is the only way to avoid becoming an alcoholic.

[3] I would not be surprised if VCs' tendency to push founders to overhire is not even in their own interest. They don't know how many of the companies that get killed by overspending might have done well if they'd survived. My guess is a significant number.

[4] After reading a draft, Sam Altman wrote:

"I think you should make the hiring point more strongly. I think it's roughly correct to say that YC's most successful companies have never been the fastest to hire, and one of the marks of a great founder is being able to resist this urge."

Paul Buchheit adds:

"A related problem that I see a lot is premature scaling—founders take a small business that isn't really working (bad unit economics, typically) and then scale it up because they want impressive growth numbers. This is similar to over-hiring in that it makes the business much harder to fix once it's big, plus they are bleeding cash really fast."