Black Swan Farming

September 2012I've done several types of work over the years but I don't know  
another as counterintuitive as startup investing.The two most important things to understand about startup investing,  
as a business, are (1) that effectively all the returns are  
concentrated in a few big winners, and (2) that the best ideas look  
initially like bad ideas.The first rule I knew intellectually, but didn't really grasp till  
it happened to us. The total value of the companies we've funded  
is around 10 billion, give or take a few. But just two companies,  
Dropbox and Airbnb, account for about three quarters of it.In startups, the big winners are big to a degree that violates our  
expectations about variation. I don't know whether these expectations  
are innate or learned, but whatever the cause, we are just not  
prepared for the 1000x variation in outcomes that one finds in  
startup investing.That yields all sorts of strange consequences. For example, in  
purely financial terms, there is probably at most one company in  
each YC batch that will have a significant effect on our returns,  
and the rest are just a cost of doing business.   
[1]  
I haven't  
really assimilated that fact, partly because it's so counterintuitive,  
and partly because we're not doing this just for financial reasons;  
YC would be a pretty lonely place if we only had one company per  
batch. And yet it's true.To succeed in a domain that violates your intuitions, you need to  
be able to turn them off the way a pilot does when flying through  
clouds.   
[2]  
 You need to do what you know intellectually to be  
right, even though it feels wrong.It's a constant battle for us. It's hard to make ourselves take  
enough risks. When you interview a startup and think "they seem  
likely to succeed," it's hard not to fund them. And yet, financially  
at least, there is only one kind of success: they're either going  
to be one of the really big winners or not, and if not it doesn't  
matter whether you fund them, because even if they succeed the  
effect on your returns will be insignificant. In the same day of  
interviews you might meet some smart 19 year olds who aren't even  
sure what they want to work on. Their chances of succeeding seem  
small. But again, it's not their chances of succeeding that matter  
but their chances of succeeding really big. The probability that  
any group will succeed really big is microscopically small, but the  
probability that those 19 year olds will might be higher than that  
of the other, safer group.The probability that a startup will make it big is not simply a  
constant fraction of the probability that they will succeed at all.  
If it were, you could fund everyone who seemed likely to succeed  
at all, and you'd get that fraction of big hits. Unfortunately  
picking winners is harder than that. You have to ignore the elephant  
in front of you, the likelihood they'll succeed, and focus instead  
on the separate and almost invisibly intangible question of whether  
they'll succeed really big.HarderThat's made harder by the fact that the best startup ideas seem at  
first like bad ideas. I've written about this before: if a good  
idea were obviously good, someone else would already have done it.  
So the most successful founders tend to work on ideas that few  
beside them realize are good. Which is not that far from a description  
of insanity, till you reach the point where you see results.The first time Peter Thiel spoke at YC he drew a Venn diagram that  
illustrates the situation perfectly. He drew two intersecting  
circles, one labelled "seems like a bad idea" and the other "is a  
good idea." The intersection is the sweet spot for startups.This concept is a simple one and yet seeing it as a Venn diagram  
is illuminating. It reminds you that there is an intersection—that  
there are good ideas that seem bad. It also reminds you that the  
vast majority of ideas that seem bad are bad.The fact that the best ideas seem like bad ideas makes it even  
harder to recognize the big winners. It means the probability of  
a startup making it really big is not merely not a constant fraction  
of the probability that it will succeed, but that the startups with  
a high probability of the former will seem to have a disproportionately  
low probability of the latter.History tends to get rewritten by big successes, so that in retrospect  
it seems obvious they were going to make it big. For that reason  
one of my most valuable memories is how lame Facebook sounded to  
me when I first heard about it. A site for college students to  
waste time? It seemed the perfect bad idea: a site (1) for a niche  
market (2) with no money (3) to do something that didn't matter.One could have described Microsoft and Apple in exactly the same  
terms.  
[3]Harder StillWait, it gets worse. You not only have to solve this hard problem,  
but you have to do it with no indication of whether you're succeeding.  
When you pick a big winner, you won't know it for two years.Meanwhile, the one thing you can measure is dangerously  
misleading. The one thing we can track precisely is how well the  
startups in each batch do at fundraising after Demo Day. But we  
know that's the wrong metric. There's no correlation between the  
percentage of startups that raise money and the metric that does  
matter financially, whether that batch of startups contains a big  
winner or not.Except an inverse one. That's the scary thing: fundraising is not  
merely a useless metric, but positively misleading. We're in a  
business where we need to pick unpromising-looking outliers, and  
the huge scale of the successes means we can afford to spread our  
net very widely. The big winners could generate 10,000x returns.  
That means for each big winner we could pick a thousand companies  
that returned nothing and still end up 10x ahead.If we ever got to the point where 100% of the startups we funded  
were able to raise money after Demo Day, it would almost certainly  
mean we were being too conservative.  
[4]It takes a conscious effort not to do that too. After 15 cycles  
of preparing startups for investors and then watching how they do,  
I can now look at a group we're interviewing through Demo Day  
investors' eyes. But those are the wrong eyes to look through!We can afford to take at least 10x as much risk as Demo Day investors.  
And since risk is usually proportionate to reward, if you can afford  
to take more risk you should. What would it mean to take 10x more  
risk than Demo Day investors? We'd have to be willing to fund 10x  
more startups than they would. Which means that even if we're  
generous to ourselves and assume that YC can on average triple a  
startup's expected value, we'd be taking the right amount of risk  
if only 30% of the startups were able to raise significant funding  
after Demo Day.I don't know what fraction of them currently raise more after Demo  
Day. I deliberately avoid calculating that number, because if you  
start measuring something you start optimizing it, and I know it's  
the wrong thing to optimize.  
[5]  
But the percentage is certainly  
way over 30%. And frankly the thought of a 30% success rate at  
fundraising makes my stomach clench. A Demo Day where only 30% of  
the startups were fundable would be a shambles. Everyone would  
agree that YC had jumped the shark. We ourselves would feel that  
YC had jumped the shark. And yet we'd all be wrong.For better or worse that's never going to be more than a thought  
experiment. We could never stand it. How about that for  
counterintuitive? I can lay out what I know to be the right thing  
to do, and still not do it. I can make up all sorts of plausible  
justifications. It would hurt YC's brand (at least among the  
innumerate) if we invested in huge numbers of risky startups that  
flamed out. It might dilute the value of the alumni network.  
Perhaps most convincingly, it would be demoralizing for us to be  
up to our chins in failure all the time. But I know the real reason  
we're so conservative is that we just haven't assimilated the fact  
of 1000x variation in returns.We'll probably never be able to bring ourselves to take risks  
proportionate to the returns in this business. The best we can  
hope for is that when we interview a group and find ourselves  
thinking "they seem like good founders, but what are investors going  
to think of this crazy idea?" we'll continue to be able to say "who  
cares what investors think?" That's what we thought about Airbnb,  
and if we want to fund more Airbnbs we have to stay good at thinking  
it.Notes[1]  
I'm not saying that the big winners are all that matters, just  
that they're all that matters financially for investors. Since  
we're not doing YC mainly for financial reasons, the big winners  
aren't all that matters to us. We're delighted to have funded  
Reddit, for example. Even though we made comparatively little from  
it, Reddit has had a big effect on the world, and it introduced us  
to Steve Huffman and Alexis Ohanian, both of whom have become good  
friends.Nor do we push founders to try to become one of the big winners if  
they don't want to. We didn't "swing for the fences" in our own  
startup (Viaweb, which was acquired for $50 million), and it would  
feel pretty bogus to press founders to do something we didn't do.  
Our rule is that it's up to the founders. Some want to take over  
the world, and some just want that first few million. But we invest  
in so many companies that we don't have to sweat any one outcome.  
In fact, we don't have to sweat whether startups have exits at all.  
The biggest exits are the only ones that matter financially, and  
those are guaranteed in the sense that if a company becomes big  
enough, a market for its shares will inevitably arise. Since the  
remaining outcomes don't have a significant effect on returns, it's  
cool with us if the founders want to sell early for a small amount,  
or grow slowly and never sell (i.e. become a so-called lifestyle  
business), or even shut the company down. We're sometimes disappointed  
when a startup we had high hopes for doesn't do well, but this  
disappointment is mostly the ordinary variety that anyone feels  
when that happens.[2]  
Without visual cues (e.g. the horizon) you can't distinguish  
between gravity and acceleration. Which means if you're flying  
through clouds you can't tell what the attitude of  
the aircraft is. You could feel like you're flying straight and  
level while in fact you're descending in a spiral. The solution  
is to ignore what your body is telling you and listen only to your  
instruments. But it turns out to be very hard to ignore what your  
body is telling you. Every pilot knows about this   
problem and yet  
it is still a leading cause of accidents.[3]  
Not all big hits follow this pattern though. The reason Google  
seemed a bad idea was that there were already lots of search engines  
and there didn't seem to be room for another.[4]  
A startup's success at fundraising is a function of two things:  
what they're selling and how good they are at selling it. And while  
we can teach startups a lot about how to appeal to investors, even  
the most convincing pitch can't sell an idea that investors don't  
like. I was genuinely worried that Airbnb, for example, would not  
be able to raise money after Demo Day. I couldn't convince Fred Wilson to fund them. They might not  
have raised money at all but for the coincidence that Greg McAdoo,  
our contact at Sequoia, was one of a handful of VCs who understood  
the vacation rental business, having spent much of the previous two  
years investigating it.[5]  
I calculated it once for the last batch before a consortium of  
investors started offering investment automatically to every startup  
we funded, summer 2010. At the time it was 94% (33 of 35 companies  
that tried to raise money succeeded, and one didn't try because  
they were already profitable). Presumably it's lower now because  
of that investment; in the old days it was raise after Demo Day or  
die.Thanks to Sam Altman, Paul Buchheit, Patrick Collison, Jessica  
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