# Early Work

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One of the biggest things holding people back from doing great work is the fear of making something lame. And this fear is not an irrational one. Many great projects go through a stage early on where they don't seem very impressive, even to their creators. You have to push through this stage to reach the great work that lies beyond. But many people don't. Most people don't even reach the stage of making something they're embarrassed by, let alone continue past it. They're too frightened even to start.

Imagine if we could turn off the fear of making something lame. Imagine how much more we'd do.

Is there any hope of turning it off? I think so. I think the habits at work here are not very deeply rooted.

Making new things is itself a new thing for us as a species. It has always happened, but till the last few centuries it happened so slowly as to be invisible to individual humans. And since we didn't need customs for dealing with new ideas, we didn't develop any.

We just don't have enough experience with early versions of ambitious projects to know how to respond to them. We judge them as we would judge more finished work, or less ambitious projects. We don't realize they're a special case.

Or at least, most of us don't. One reason I'm confident we can do better is that it's already starting to happen. There are already a few places that are living in the future in this respect. Silicon Valley is one of them: an unknown person working on a strange-sounding idea won't automatically be dismissed the way they would back home. In Silicon Valley, people have learned how dangerous that is.

The right way to deal with new ideas is to treat them as a challenge to your imagination not just to have lower standards, but to switch polarity entirely, from listing the reasons an idea won't work to trying to think of ways it could. That's what I do when I meet people with new ideas. I've become quite good at it, but I've had a lot of practice. Being a partner at Y Combinator means being practically immersed in strange-sounding ideas proposed by unknown people. Every six months you get thousands of new ones thrown at you and have to sort through them, knowing that in a world with a power-law distribution of outcomes, it will be painfully obvious if you miss the needle in this haystack. Optimism becomes urgent.

But I'm hopeful that, with time, this kind of optimism can become widespread enough that it becomes a social custom, not just a trick used by a few specialists. It is after all an extremely lucrative trick, and those tend to spread quickly.

Of course, inexperience is not the only reason people are too harsh on early versions of ambitious projects. They also do it to seem clever. And in a field where the new ideas are risky, like startups, those who dismiss them are in fact more likely to be right. Just not when their predictions are weighted by outcome.

But there is another more sinister reason people dismiss new ideas. If you try something ambitious, many of those around you will hope, consciously or unconsciously, that you'll fail. They worry that if you try something ambitious and succeed, it will put you above them. In some countries this is not just an individual failing but part of the national culture.

I wouldn't claim that people in Silicon Valley overcome these impulses because they're morally better. [1] The reason many hope you'll succeed is that they hope to rise with you. For investors this incentive is particularly explicit. They want you to succeed because they hope you'll make them rich in the process. But many other people you meet can hope to benefit in some way from your success. At the very least they'll be able to say, when you're famous, that they've known you since way back.

But even if Silicon Valley's encouraging attitude is rooted in self-interest, it has over time actually grown into a sort of benevolence. Encouraging startups has been practiced for so long that it has become a custom. Now it just seems that that's what one does with startups.

Maybe Silicon Valley is too optimistic. Maybe it's too easily fooled by impostors. Many less optimistic journalists want to believe that. But the lists of impostors they cite are suspiciously short, and plagued with asterisks.
[2] If you use revenue as the test, Silicon Valley's optimism seems better tuned than the rest of the world's. And because it works, it will spread.

There's a lot more to new ideas than new startup ideas, of course. The fear of making something lame holds people back in every field. But Silicon Valley shows how quickly customs can evolve to support new ideas. And that in turn proves that dismissing new ideas is not so deeply rooted in human nature that it can't be unlearnt.

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Unfortunately, if you want to do new things, you'll face a force more powerful than other people's skepticism: your own skepticism. You too will judge your early work too harshly. How do you avoid that?

This is a difficult problem, because you don't want to completely eliminate your horror of making something lame. That's what steers you toward doing good work. You just want to turn it off temporarily, the way a painkiller temporarily turns off pain.

People have already discovered several techniques that work. Hardy mentions two in A Mathematician's Apology:

> Good work is not done by "humble" men. It is one of the first duties of a professor, for example, in any subject, to exaggerate a little both the importance of his subject and his importance in it.

If you overestimate the importance of what you're working on, that will compensate for your mistakenly harsh judgment of your initial results. If you look at something that's 20% of the way to a goal worth 100 and conclude that it's 10% of the way to a goal worth 200, your estimate of its expected value is correct even though both components are wrong.

It also helps, as Hardy suggests, to be slightly overconfident. I've noticed in many fields that the most successful people are slightly overconfident. On the face of it this seems implausible. Surely it would be optimal to have exactly the right estimate of one's abilities. How could it be an advantage to be mistaken? Because this error compensates for other sources of error in the opposite direction: being slightly overconfident armors you against both other people's skepticism and your own.

Ignorance has a similar effect. It's safe to make the mistake of judging early work as finished work if you're a sufficiently lax judge of finished work. I doubt it's possible to cultivate this kind of ignorance, but empirically it's a real advantage, especially for the young.

Another way to get through the lame phase of ambitious projects is to surround yourself with the right people to create an eddy in the social headwind. But it's not enough to collect people who are always encouraging. You'd learn to discount that. You need colleagues who can actually tell an ugly duckling from a baby swan. The people best able to do this are those working on similar projects of their own, which is why university departments and research labs work so well. You don't need institutions to collect colleagues. They naturally coalesce, given the chance. But it's very much worth accelerating this process by seeking out other people trying to do new things.

Teachers are in effect a special case of colleagues. It's a teacher's

job both to see the promise of early work and to encourage you to continue. But teachers who are good at this are unfortunately quite rare, so if you have the opportunity to learn from one, take it.
[3]

For some it might work to rely on sheer discipline: to tell yourself that you just have to press on through the initial crap phase and not get discouraged. But like a lot of "just tell yourself" advice, this is harder than it sounds. And it gets still harder as you get older, because your standards rise. The old do have one compensating advantage though: they've been through this before.

It can help if you focus less on where you are and more on the rate of change. You won't worry so much about doing bad work if you can see it improving. Obviously the faster it improves, the easier this is. So when you start something new, it's good if you can spend a lot of time on it. That's another advantage of being young: you tend to have bigger blocks of time.

Another common trick is to start by considering new work to be of a different, less exacting type. To start a painting saying that it's just a sketch, or a new piece of software saying that it's just a quick hack. Then you judge your initial results by a lower standard. Once the project is rolling you can sneakily convert it to something more.
[4]

This will be easier if you use a medium that lets you work fast and doesn't require too much commitment up front. It's easier to convince yourself that something is just a sketch when you're drawing in a notebook than when you're carving stone. Plus you get initial results faster.
[5]
[6]

It will be easier to try out a risky project if you think of it as a way to learn and not just as a way to make something. Then even if the project truly is a failure, you'll still have gained by it. If the problem is sharply enough defined, failure itself is knowledge: if the theorem you're trying to prove turns out to be false, or you use a structural member of a certain size and it fails under stress, you've learned something, even if it isn't what you wanted to learn. [7]

One motivation that works particularly well for me is curiosity. I like to try new things just to see how they'll turn out. We started Y Combinator in this spirit, and it was one of main things that kept me going while I was working on Bel. Having worked for so long with various dialects of Lisp, I was very curious to see what its inherent shape was: what you'd end up with if you followed the axiomatic approach all the way.

But it's a bit strange that you have to play mind games with yourself to avoid being discouraged by lame-looking early efforts. The thing you're trying to trick yourself into believing is in fact the truth. A lame-looking early version of an ambitious project truly is more valuable than it seems. So the ultimate solution may be to teach yourself that.

One way to do it is to study the histories of people who've done great work. What were they thinking early on? What was the very first thing they did? It can sometimes be hard to get an accurate answer to this question, because people are often embarrassed by their earliest work and make little effort to publish it. (They too misjudge it.) But when you can get an accurate picture of the first steps someone made on the path to some great work, they're often pretty feeble. [8]

Perhaps if you study enough such cases, you can teach yourself to be a better judge of early work. Then you'll be immune both to other people's skepticism and your own fear of making something lame. You'll see early work for what it is.

Curiously enough, the solution to the problem of judging early work too harshly is to realize that our attitudes toward it are themselves early work. Holding everything to the same standard is a crude version 1. We're already evolving better customs, and we can already see signs of how big the payoff will be.

Notes

[1]
This assumption may be too conservative. There is some evidence that historically the Bay Area has attracted a different sort of person than, say, New York City.

[2]
One of their great favorites is Theranos. But the most conspicuous feature of Theranos's cap table is the absence of Silicon Valley firms. Journalists were fooled by Theranos, but Silicon Valley investors weren't.

[3]
I made two mistakes about teachers when I was younger. I cared more about professors' research than their reputations as teachers, and I was also wrong about what it meant to be a good teacher. I thought it simply meant to be good at explaining things.

[4]
Patrick Collison points out that you can go past treating something as a hack in the sense of a prototype and onward to the sense of the word that means something closer to a practical joke:

> I think there may be something related to being a hack that can be powerful  the idea of making the tenuousness and implausibility a feature. "Yes, it's a bit ridiculous, right? I'm just trying to see how far such a naive approach can get." YC seemed to me to have this characteristic.

[5]
Much of the advantage of switching from physical to digital media is not the software per se but that it lets you start something new with little upfront commitment.

[6]
John Carmack adds:

> The value of a medium without a vast gulf between the early work and the final work is exemplified in game mods. The original Quake game was a golden age for mods, because everything was very flexible, but so crude due to technical limitations, that quick hacks to try out a gameplay idea weren't all that far from the official game. Many careers were born from that, but as the commercial game quality improved over the years, it became almost a full time job to make a successful mod that would be appreciated by the community. This was dramatically reversed with Minecraft and later Roblox, where the entire esthetic of the experience was so explicitly crude that innovative gameplay concepts became the overriding value. These "crude" game mods by single authors are now often bigger deals than massive professional teams' work.

[7]
Lisa Randall suggests that we

> treat new things as experiments. That way there's no such thing as failing, since you learn something no matter what. You treat it like an experiment in the sense that if it really rules something out, you give up and move on, but if there's some way to vary it to make it work better, go ahead and do that

[8]
Michael Nielsen points out that the internet has made this easier, because you can see programmers' first commits, musicians' first videos, and so on.

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