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When to reason from first principles

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8-10 minutes

> *I think particularly as you're trying to figure out new things, reasoning from first principles is a good way to go, as opposed to reasoning by analogy.*
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> *It's computationally easier to reason by analogy and if you tried to reason from first principles all the time, you wouldn't be able to get through your day, but when you're trying to do something new and complicated, that is the way to do it because analogies are not necessarily perfect and they're relying on things that have already occurred, so, if you're trying to make something new then it's not a great way to go.*
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> *What reasoning from first principles really means is boiling something down to the fundamental truths, or what appear to be the fundamental truths, and reasoning up from there, and then having a good feedback loop.*
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> *(Elon Musk at BTA 2012)*

What I struggle with is figuring out *which problems are ripe for this kind of thinking* — areas where a first principles approach is likely to end in a meaningful, material breakthrough. In my experience, the failure mode is to over-invest your time and effort thinking from first principles, only to be left with only an incrementally better solution. If that's the case it would have been better to rely on the commonplace approach in the first place.

In my personal life, over-investing in knowledge seeking is no great loss. Approaching problems originally is difficult and mentally painful, and so training my brain to question conventional truths, and to approach problems and seek answers in novel ways is an opportunity for growth. The more I do it the better I become at it. The process itself has value regardless of result — so what if I lose a few weeks trying to understand something, only to arrive nowhere original. I can take the long view in seeing payoff — i.e., my lifetime on earth.

But as an engineering manager at Dropbox, I really can't afford to be so wasteful with my time and mental energy. Dropbox very much has a first principles culture (one that was instilled by our founders and early employees, and is going strong to this day), and yet every decision we make has time-urgency to it — we have many problems to solve, but too little time. In this context, when is it ok to delay decision-making by relying on first principles thinking?

My solution is to ask one simple question every time I am confronted with a situation in need of a solution: **If I find a much better solution to this problem, will it have a material impact on Dropbox's success?**

First, an underlying assumption is that, as one of 1,500 Dropboxers, I can make a material impact on the company's success. This a very empowering characteristic of working at Dropbox and reflective of the kind of company that Drew and Arash have built. I most definitely did not have the same impact-potential when I worked as an attorney at a top New York firm. Kudos for building such a culture. Unfortunately, it's not representative of most work environments — it's important to recognize the privilege one has even being able to approach problems from a first principles perspective.

Second, the question is in dire need of a definition of the term *better*. I generally use a shorthand: **better = twice as good** (or twice as fast, or twice as performant, or requiring half the resources). The underlying assumption is that the current approach/conventional wisdom is inefficient by a factor of two. If making things twice as good isn't impactful, then it's generally not worth the time investment of first principles thinking — and in practice, most problems won't lend themselves to a 2x better solution.

Third, we have to answer the question whether even a substantially better solution to a problem can materially affect Dropbox (or any company's or project's) success.

For example, improving candidate pass-through rate by a factor of two while keeping candidate quality intact, would have a huge impact. Relative to our generation of companies, Dropbox has the best team in the Valley, and "better" is unlikely to come from better candidates as we already get the cream of the crop. But if we could decrease the time and resources we spent on recruiting (e.g., time spent by engineers interviewing candidates or engineering managers sourcing candidates), we would see big gains in terms of engineering, design and product development (and other functions). As someone who spends a lot of time working on product and front-end engineer recruiting, it thus makes sense for me to challenge most assumptions behind recruiting practices and design our approach from first principles. If I can save every engineer from 2 hours of interviewing/modding/debriefs a week, that's a 5% increase in overall productivity. And if we can provide twice as good a candidate experience as our competitors, we'll set ourselves up for years of future success by maintaining the best team in the industry.

A counter-example would be improving our leveling system — where "twice as good" and "material impact" feel very nebulous. First of all, when you are growing very fast, one's level is a smaller part of being recognized than at a slow-growing company (again, we are very lucky at Dropbox). Leveling simply isn't as important to individual and team growth, learning or recognition. Second, levels exist in the context of our peer employers — employees care about levels in how they relate to, say, Google's levels or Facebook's levels or Goldman Sachs's levels. Here, it actually makes more sense for us to align with the mainstream solution of how peer companies define their levels. It's unclear how a "better" leveling scheme would improve materially either recruiting or retention or morale or happiness. (Side note: suboptimal solutions that cannot easily be improved because of constraints external to what any one actor controls are a great place to look for great startup ideas. That's a topic for another blog post.)

In the end, when I look at the two dozen or so things I do on a monthly basis, only a handful of them answer the question — **If I find a much better solution to this problem, will it have a material impact on Dropbox's success?** — in the affirmative. For everything else, the mainstream solution is good enough.

It's worth noting that there are two serious drawbacks to applying this formula: the factor of two shorthand will incentivize you to not consider moonshots where the actual factor might be much greater. I think that for new products and moonshots, it makes sense to **always** reason from first principles — zero to one is where the biggest rewards lie. Second, where you don't rely on the multiplier shorthand, the formula will lead you to reason from analogy to approximate likely improvements. This, of course, defeats the entire purpose of first principles thinking.

This brings me to the second leg of my approach: **Whenever I reason from analogy, I make a note of it, and every few months I pay down this debt by re-building some of my knowledge foundations.** Or at least I strive to.

Reasoning by analogy is unavoidable. But I think if I find that for a particular topic I am basing most of my knowledge on analogies from other subjects or experiences, it's a clear signal that I am out of my depth, that I lack the right foundations to be truly insightful and successful. Keeping track of these gaps — I call them **knowledge debt** — is the first step in establishing the foundation on which to think from first principles.

Over the past decade, I've worked in a lot of environments where the demands that were put on me shifted so quickly that I worked outside of my realm of expertise for significant periods of time. This is unavoidable in the world of fast-growing companies — it's part of the thrill. But the accompanying discomfort — that feeling that I am just "faking it" — leaves one with very little time to build foundation. Almost ten years later, it's fascinating to me how much I've done "well enough" without developing true depth of knowledge, and without thinking about problems from the ground up. And yet, looking at the data, it's also inescapable that the things I've been best at — the challenges where I had the greatest impact — all derived from me taking some **meaningful time building foundation and developing solutions starting from fundamental truths**.

To another year of identifying and then taking the time to pay down that knowledge debt!

Further reading

- Wait But Why's fantastic series on Elon Musk ends on an essay on building knowledge and thinking from first principles.
- John Kenneth Galbraith's thoughts on conventional wisdom (and the wikipedia article)