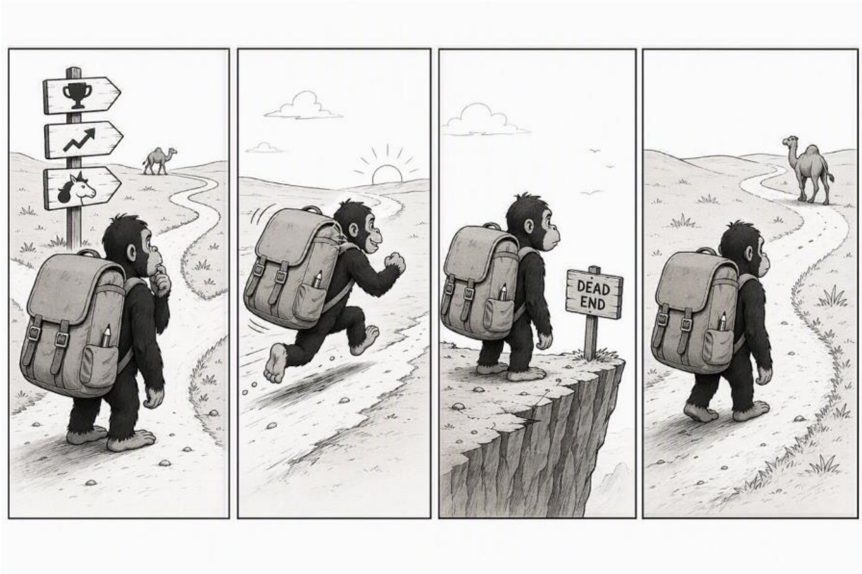
Chapter 1 — Most Founders Do Not Fail Because They Make Bad Decisions

Seat: the founder.



The right path is rarely the loudest signpost.

Core question: What happens when founders make correct decisions before earning the right to make them?

Founders rarely destroy companies with one bad decision. They destroy them with the right decision made six months too early.

I have made every one of the mistakes this book describes, and more. A few nearly ended the company. One taught me how quickly a founder can become the bottleneck while believing he is still the engine.

That is one reason for writing this. The other is that I had time, advice, and some luck. My father was one of the people who, at an early stage of my entrepreneurship journey, challenged weak assumptions before they became fatal decisions. He understood something I did not understand well enough then. A business can fail long before it looks like it is failing.

Many avoidable startup failures begin with something ordinary. A reasonable decision made under the wrong conditions. A founder hires too early. Another raises capital before knowing what it should accelerate. Another scales a sales motion that has never truly worked. Another builds governance around a company that does not yet have a business.

None of those decisions sound irrational. Most sound smart. That is the trap.

Fear of missing out

Most startup mistakes begin with fear — usually not fear of failure, but fear of missing an opportunity — rather than with analysis.

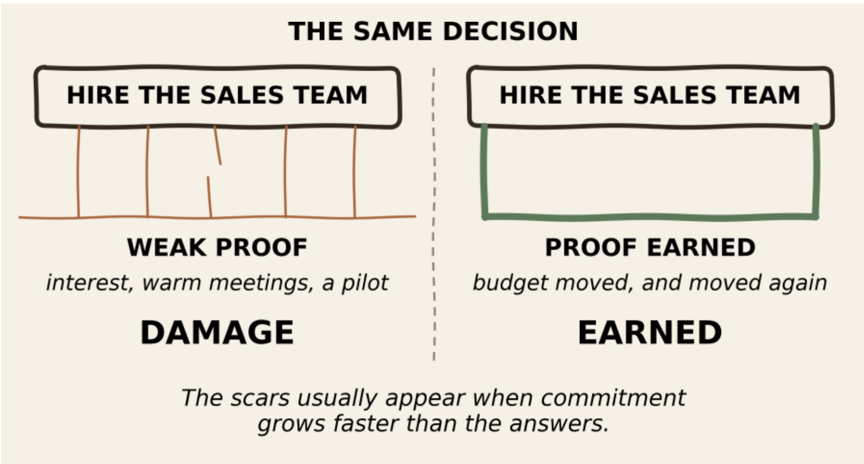
A founder sees a larger customer segment, another country, an adjacent use case, a competitor raising money, a fast-growing market, or a new technology trend, and concludes. We should go there too. The team stops trying to win one thing and starts trying not to miss everything.

Founders rarely earn proof at the speed they add ambition. Instead of winning one market, the company chases several; instead of understanding one buyer context deeply, it serves many; instead of becoming known for something specific, it becomes vaguely relevant to many audiences. The result is predictable. Buyer Proof weakens, Value Proof never converges, scaling gets harder, and the company adds complexity faster than it earns evidence.

Failed expansions usually follow the same pattern. The company expands assumptions faster than learning.

Premature scaling is only the visible and most studied version. The same mistake appears earlier in hiring, fundraising, metrics, governance, product development, and traction. The company misreads its stage, then imports later-stage logic too early.

The question is never whether a decision is smart in general, but whether the company has earned the right to make it now.



That is why intelligent founders fail while doing things that work elsewhere. A company still proving that a painful problem exists should not be run like a retention machine. A company with weak retention should not behave like a scaling business. Different stages require different knowledge and work. First learning what is real, then proving repeatability, and only then scaling what has been proved.

It is also why startup advice sounds contradictory. Raise early, stay lean, hire experienced people, do not hire too early, expand, focus, move fast, slow down, learn. Most of that advice belongs in different contexts rather than being wrong.

A scar: the good deal we still had to miss

In 2010, during a strategy session at Harvard Business School, one sentence changed how I think about FOMO. The professor said something I have carried since. A clear strategy emerges after enough no-decisions. At the time I heard it as advice about strategy. It took me years to realise it was a diagnosis of my own biggest weakness.

FOMO tempts investors too. As an investor, I have passed on companies that later became category leaders or generated returns I would have gladly taken. They did not look bad. They did not fit our investment thesis at the time. That is the version everyone assumes hurts most. It is not the dangerous one.

I eventually realised my version of FOMO was an inability to say no to money or business that looked attractive, rather than the excitement people assume.

A no-decision is not a life sentence — it holds for the present, not forever. It can change later, when the evidence changes. Saying no today does not close the door. It just refuses to open it before the evidence does.

As a young angel investor, I had not learned that yet. My gut said no, clearly, every time, and I overruled it anyway. There was no real justification for overruling it, not one I can honestly reconstruct now. What overruled it was the pull almost everyone feels. The temptation to dream big, to believe the story before the evidence exists to support it. I lost substantial amounts of money behaving this way. The bad checks were the visible cost. Focus was the real one.

It took enough failures before I started treating this as something to study rather than something to feel guilty about. The first real step was comparing the investments that worked with the ones that didn't. I had only counted the losses, not compared them to the wins.

That comparison also taught me something less comfortable. Recognising FOMO after the loss is easy. Recognising it in the room, in real time, is not. A gut feeling can come from real signal (pattern recognition earned through failure) or it can come from wanting the story to be true. The two feel identical from the inside. I still get them wrong.

I remember the anger at myself, not the losses. When the first failure hit me and the money was lost, I first blamed the founders; for a long time I was merciless about it, there was only one person to blame, and I knew where he lived. But soon I returned to my entrepreneur's method. No one is a master at the beginning, and it requires disciplined learning to become one. That comparison, losses against wins, forced me to articulate an investment thesis: first my own, later Gorilla Capital's. The two are quite different: my own thesis does not look for exit but for profitable growth, while Gorilla's is built entirely on exit.

Scar line: *Discipline means missing some good opportunities on purpose.*

A scar: the refusal that steered me for years

Not every refusal is that disciplined. Long before any of this, there were several discussions about a distribution opportunity for a games console that had not yet launched, from a company that was a total outsider to gaming. I was asked to pitch for the business. I never did. Three reasons, and I remember them precisely: I was afraid of the size of the commitment for a company of our size, I doubted my own understanding of what it would take, and I did not believe a total outsider could become the market leader.

The fear had a specific root, not a vague one. In 1991, the Finnish currency was devalued overnight, and our debt roughly doubled overnight with it. We survived by raising prices across the board overnight to cover the sudden cost increase. That is the kind of near-miss that stays in a founder's body long after the balance sheet recovers, and it was still there through those discussions.

It was not obviously destined to succeed. The company behind it had never made a game console before; it was known for television sets and stereo equipment. One established leader controlled nearly the entire market when it launched, and plenty of people close to the industry gave it little real chance. It took about two years before it overtook that leader and became the leader itself. At the time I refused, none of that had happened yet. The decision felt rational.

Years later I realised I had not rejected the opportunity. I had rejected the chance to learn.

By the rules in this book, the refusal almost passes. A commitment larger than the company's proof is what Rule 1 (never make a commitment larger than your proof, defined in full in Chapter 5) exists to prevent, and fear of size can be discipline in disguise. But honesty requires the dissection, because only one of my three reasons pointed at evidence. A disciplined refusal names the missing proof and the test that would supply it. Mine named my fear, my ignorance, and a prior, outsiders do not win here, that I never tested against anything. The doubt felt like analysis. It was a guess wearing analysis's clothes, and the market graded it in public. Success correlates with known risk, deliberately taken and understood. It does not correlate with gambling. I had refused to find out which one this was.

The scar steered me for years, and this book tells where it drove me. When the next total outsider arrived with the next console, we were a small company against giants, and we fought for the rights and won them. That story is in Chapter 2. Scars do not only hurt; they steer. The steering is not automatically right either. A scar can make you fast into the wrong room, as the launch that followed proved. But a scar examined honestly is the most durable fuel a founder has. The mechanism from that first refusal is the one your next refusal runs on today.

I have often wondered how stupid I was, and why I overreacted to the warning signs. I have spent sleepless nights on that question, and I have cried over the lost opportunity. But without the mistake, and without the self-reflection that followed it, I would not have evolved, and I would not have won the next pitch.

Scar line: *A disciplined refusal names the missing evidence. A fearful one names your own smallness.*

Capital can hide weak proof

Capital can delay exposure to weak proof. As my partner, Risto, puts it. "The more money you raise, the more likely you are to fail."

The sentence sounds like provocation. It is arithmetic. Each round of capital raises the valuation, and each valuation raises the minimum exit the cap table can accept. The buyer universe shrinks with every step. Fewer acquirers can justify the price, and the ones who remain know they are the only ones left. Meanwhile the money hides weak proof longer, because a full account keeps a team from hearing what customers are saying. The probability of a good outcome can fall while the bank balance rises. Chapter 9 shows the arithmetic in full.

I have lived this from both sides. One example still sits uncomfortably in memory.

In a software startup I worked with as an investor, the team built a sales structure before anyone understood how and what customers actually bought. People were added. Forecasts improved. Calendars filled. It looked like professionalisation. Six months later, the underlying problem had not moved. There was still no repeatable path from interest to purchase.

The company had scaled uncertainty, not revenue. The decision was early rather than stupid. It is also one of the most common mistakes I see in startups, and an extremely common death driver.

Scar line: *Scaling early usually scales uncertainty.*

Founders often treat money as the main constraint. Money matters, but shortage of time is more dangerous. Time to learn, test assumptions, recognise reality, and change direction before the organisation hardens around a false conclusion. I have learned to treat constraint as a tool, not merely a limitation. Capital efficiency is not a posture of weakness; used well, it is a way to preserve options until proof improves and it encourages innovation.

When direction is wrong, correction gets expensive — financially, emotionally, organisationally, and politically. An early hire creates expectations that outlive the reason for it. A misplaced metric changes the conversation. A premature board dynamic replaces learning with pressure. A too-early fundraise locks the company into a story before reality supports it.

I recently sat with a founder where exactly this had materialised. They had found Buyer Proof at last, but the cap table and the P&L are already wrecked by the early wrong turns.

Before building faster, hiring, raising, or scaling: diagnose.

So this book begins with a timing question instead of growth, fundraising, or strategy decks. What stage is the company actually in, and what would prove that assessment wrong?

Carrying a scar

For twenty years I was extremely harsh on myself. There was only one person to blame for my mistakes, because my own doing was the only thing I could change. I have had nightmares about decisions long since closed. Later I learned to self-reflect deliberately instead of punishing myself accidentally; today I do it weekly. I have also become more forgiving. You can make mistakes — you must make mistakes. Just do not repeat them. I have fired people for repeating mistakes they knew about, and I am still friends with them, because the firing was about the repetition, never the person. And do not be too hard on yourself either. Some of my mistakes opened opportunities I could not have seen from the road I was on.

Reality Check

- What stage do you assume the company is in, and what evidence supports that assumption?
- Which current decisions only make sense if the company is further along than it really is?
- Where is activity being mistaken for evidence?
- Which current moves are driven by fear of missing an opportunity rather than by evidence?
- Where have you deliberately said no? Not what you are building, but what you have refused to build, sell, or become, even when the opportunity was real. If the answer is nothing, the strategy is not clear yet.
- Which decisions being made now would be expensive to reverse in six months?

For each major decision in motion, ask whether the evidence proves it belongs in this stage, or whether the decision should wait.

Founder takeaway

Before acting, ask two questions: does this decision belong to this stage, and what happens if it waits?

Companies rarely fail because the right decision was delayed one cycle. They fail because it was made too early and mistaken for progress.

Time is your scarcest resource. Know your runkonopeus, your hull speed.

Do not be too hard on yourself!

Scar line: *The right decision made too early still becomes damage.*

Deciding whether a decision belongs to this stage depends on reading which stage the company is actually in. The most encouraging signals are the least reliable.