Attempting to Commercialize an Original Robotics Concept

Lessons Learned

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May 2026

The Merits of Biting Off More Than You Can Chew

In an earlier white paper [1], I presented an original concept for the actuation of a robotic hand. The main motivation behind that design was to propose a simpler and cheaper alternative to the approach currently used in the industry.

In the summer of 2025, I attempted to design, prototype, and produce a small series and establish a commercially viable solution based on that approach.
At that time, I was a self-financed undergraduate student in physics, working on my project with a starting capital of 22’000 CHF (23’800 €). I had no prior knowledge of industrial design or production techniques and, most importantly, no notion of strategic thinking. To complete the picture, I worked mostly alone.

Unsurprisingly, and by all accounts, the enterprise turned into an engineering and entrepreneurial disaster: I failed to produce a single working prototype, became over-absorbed in a number of (very exciting but) commercially unproductive technical challenges that exhausted both my time and financial resources. I completely underestimated the difficulty of producing integrated systems and high-precision mechanical parts, leading to impossible commercial goals. Because of that latter failure, I was unable to obtain any tangible result that I could have used to raise money, and my misguided efforts were effectively stopped in their tracks.

However, aiming exceedingly high showed me all the more clearly the number and nature of my mistakes, and I believe I learned more about planning and strategy in three months of failure than I could have in 3 years spent reading about other people’s successes.
This project wasn’t only a very enjoyable adventure; it was also a very instructive one. This little essay is a collection of the lessons I learned, both technical (on prototyping and production) and more general (about strategy and organization) during the time spent in my parents’ garage.

[1] Robotic Hands: Replacing BLDC Motors with Mechanical Transmission Systems

Part I.
Building

-In this part, I discuss my more practical lessons. If you are more interested in the business side of things, you can skip straight to the "Planning and Strategy" part.-

Integrated Systems Are Hard

Building integrated mechanical systems is a complex task on many different levels at once. A robotic hand presents a host of independent problems that must all be tackled separately.
At its minimum it requires properly dimensioned joints, all of which must be connected to actuators. Those actuators must in turn be connected to a computer. Finally, some program or programming language must be used to control the motors from the computer. And this assumes the robotic hand does not have sensing capabilities. If it does, you must create a new stack, from the sensor to the computer, and likely develop some way to translate that signal into usable motor commands. Each of these individual tasks might seem relatively simple when considered independently, but tackling them simultaneously can make the solution all the more delicate. And naturally, all problems must be solved, otherwise the whole system remains inoperative.

Not only are integrated systems hard to design, it is also hard to estimate how time-intensive it will be to establish a working prototype. When solving an integrated system, the solution of each subproblem depends on the solution to the other subproblems. This ripple effect makes it very difficult to estimate how hard each subsequent problem will be to solve.
For example, it is very hard to know exactly how hard programming a robotic hand will be until you know the mechanical specifications of that hand.

Finally, it is important to keep in mind that, as users of technology, we tend to assume we understand the inner workings of that technology much better than we actually do. This was eloquently shown by Frank Keil in his paper on the *Illusion of Explanatory Depth* [2]. Many people aren't actually all that clear about how the flushing mechanism of their toilet works. And if you're unconvinced, you can try to figure out how the wheels of your car can turn at different speeds while being connected to the same motor. This "illusion of knowledge" can become extremely costly when designing integrated systems. It is indeed easy to underestimate the complexity of each task separately, since one cannot investigate all the problems at once. One can thus end up with a very large systematic error in estimating the time or cost of solving a given integrated system.

[2] Leonid Rozenblit and Frank C. Keil, "The Misunderstood Limits of Folk Science: An Illusion of Explanatory Depth," *Cognitive Science* **26**, no. 5 (2002): 521–562.

High Mechanical Precision Is Hard

Prototyping can either be done in-house or outsourced. There are today a large number of companies proposing prototyping services in many different industrial areas; from custom electronic parts (like printed circuit boards (PCBs)) to custom mechanical parts in a wide variety of materials (from wood to plastic to steel and aluminum). Different companies offer different types of services, going from more hobbyist-grade parts, to high-level mechanical tolerances for industrial use. However, high-precision mechanical parts remain expensive to manufacture (at least with a 20k CHF budget).

In-house prototyping of mechanical parts requires either a 3D printer or a CNC machine. Broadly speaking, widely accessible 3D printers have relatively low precision, especially when it comes to engineering uses. Over the course of the last decade, however, many low-cost CNC machines have become available which can provide good-enough accuracy for many low-tolerance mechanical parts.

In summary, both in-house and outsourced production of high-precision mechanical parts remains an expensive affair.
With engineering experience, it would probably have been clear from the start that the project required a level of mechanical precision that was simply incompatible with a small budget, and that further funding would become necessary relatively quickly.

At the time, I did not have that engineering experience. And to anyone who likes to troubleshoot and see the tangible results of one's own work, *prototyping* (and especially in-house prototyping) *is fun*.

So I bought a *Makera CNC Machine* and started prototyping.

While the use of most consumer 3D printers is very straightforward, using CNC machines is an art in itself. In particular, in the production of precise mechanical elements, the experience and speed of the user play an important role in the precision and throughput of the machine. The materials must be properly placed, properly fastened to the machine bed, and due to the inherent limitations of the machine, its overall precision is anisotropic (meaning the part will be able to respect higher tolerances in one axis than in the other). Nonetheless, despite the difficulty of using the CNC machine quickly and precisely, it was in theory possible to produce individual parts with sufficiently tight tolerances.
However, the final system was to be a many-part system. The full hand system required no less than 23 independent transmission systems. The challenge was thus not only in producing one transmission system, but in making 23. This became a significant increase in complexity. Imprecisions in multi-part systems are known to compound, so that 5 independently good-enough parts can be impossible to properly assemble all together. Each independent transmission system required no less than 22 parts each, so that the total system would require no less than 500 parts, without accounting for the robotic hand in itself, or housing for all the parts.

After two months of work, it became clear from an engineering standpoint that producing all the parts for the full-hand transmission system was completely impossible given the time and money constraints I was under. This essentially put a halt to the attempt to produce a decent in-house prototype.

Part II.
Planning and Strategy

-I should point out that at the beginning of the project, although I had some technical training as a physics undergrad, I had no notion whatsoever of commercial planning or strategy.-

When it comes to technical knowledge or field-specific engineering, developing new skills boils down to one's ability to try, fail and try again. That is the learning process. *How does one design integrated systems? Or produce high-precision mechanical parts? Or develop electronic solutions?* This is know-how and know-how is fun, but time-intensive; there aren't any real shortcuts to that.

On the commercial side of things, however, and on the art of keeping the lights on, I believe there are a few tricks one can use that can significantly increase one's chances of success. *How does one measure one's success? And how can one increase the likelihood of future success?* This is planning and strategy at its core.
However, although these two topics are often thought of together, the circumstances of the project mostly made my lack of planning the most blatant.
Strategy is about envisaging your commercial solution in the customer/competitor landscape. *How can you attract the one and beat the other?* These are really thrilling questions, but because of my failures in planning, the stage where strategy would have become relevant was never truly reached. So as far as the project went, strategy mostly ended up being something for the future.

Set your Goals Early

Setting your goals from the start is probably the first and in a sense simplest step towards success. Especially because it allows you to permanently re-assess your progress along the way.

For this project, my objectives were two-fold. On one hand was the technical curiosity of getting to know if my original concept could actually be implemented. A host of technical questions were left unsolved, especially as they pertained to the actual implementation of the scheme.

On the other hand was the following question: *Could the implementation of the concept lead to a useful commercial application?*.

In technical research in the private sector, these two questions are almost always tightly intertwined and they lie at the core of modern R&D. For myself, I have found that balancing financial usefulness with pure innovative curiosity is a thrilling exercise. Over-focusing on technical efforts as I did can seem very rewarding to a technically-minded person such as myself, but you won't be able to keep the lights on for very long if you do. And commercial success will definitely bring a completely new dimension to any strictly technical success.

Break Down Your Dream in a Thousand Baby Steps

I started devising a commercial application at the same time as I started working on the first prototype. Optimistically I assumed that the full-hand prototype would work. Even more optimistically, I assumed that the electronic control of the prototype would be relatively easy. I also assumed that some remote control of the robotic hand would be possible for a human operator. Such a remote-controlled robotic hand had many potential applications (from dangerous tasks such as de-mining and firefighting to delocalizing cheap labor in fields like construction or hospitality). So my first (and very naive) approach was to imagine a single and final commercial solution based on the technical concept. From then on, success or failure depended exclusively upon my ability to deliver on this obviously over-ambitious scheme.

I knew from the start that I was very likely to fail in achieving such an outcome. What I didn't know is that there existed another (and much smarter) approach to developing a commercial (or at least self-financed) solution. Only much later (and financially too late) did I realize that such a strategy existed.
A much safer and more productive approach would have been to indeed set some very long-term goal (like a remote-controlled robotic hand). That sort of goal is good for morale because it is exciting. Commercially, it is also about developing a story that you will be able to build investor- (or customer-) relations around.
Setting big goals is good, but much more important at that stage, would have been to break down the very ambitious project into a sequence of much smaller, easier steps.

A natural breakdown would have been something along the following lines: (1) Design a working prototype of a single transmission mechanism. If I had been able to spend a larger percentage of my budget on this part of the problem, it is likely that I would have been able to achieve some working prototype (basically by outsourcing production of the parts). After that, it would have been pretty natural to look for outside financing. There exist many student start-up competitions in Switzerland where I might have been able to get more funding at this point. (2) Design the robotic hand. This would have been again a good moment to raise money. (3) Produce all 23 transmission mechanisms, housing and main motor connection and connect them to the hand (or some part of the hand) to test the use of the transmission mechanism with the robotic hand. (4) Design some scheme to control the robotic hand naturally and efficiently.

As can be seen, the further the step, the less clearly defined it becomes. It is not clear if goal (3) should be about connecting the hand to the whole transmission system, or only to a few transmission mechanisms (e.g., to only actuate a single finger, or two fingers). It is not clear either what sort of control logic should have been used in step (4). Trying to reach such distant goals all in one go (even assuming that unlimited financing was available) is fundamentally undesirable, *because you don't know yet how to get there*!

Defining a clear and workable sequence of steps would have been a much more flexible method to achieve long-term goals. It's important to choose which mountain to climb, but it is also important to know you probably don't know how to reach the top yet. There is no way around this fundamental limitation. The only option is to become and remain flexible along the way by setting steps you're sure you can perform, and then re-assessing what is the best next step (always keeping in mind that the summit is the final destination).

Proper planning is not only productive financially, as it allows you to show a clean and productive track record to potential investors. It also makes the technical side of things more straightforward and more flexible.

This is mostly planning; not strategy. Because of my failure in implementing proper planning financially and technically, I never reached a point where strategy would have become very relevant. That is the point where resources must be devoted not only to building some good working prototype, but to building a prototype with the greatest possible competitiveness and usefulness. There it becomes necessary to think about your potential customers and your competitors. About why customers might need you, but also why they might want or need you more or less than your competitors.

These questions of strategy are thrilling. They have complicated and many-valued functions as answers. And solving them is clearly something to look forward to. But as far as my 2025 robotics project went, I never reached that stage, so that's another story.