

## Introduction.

If there is one message that venture capitalists like to stress above all else, it is the importance of focus in the life of a young company. I have come to believe that, while this is always advice that serves the interest of the investor, it is only sometimes good advice for the entrepreneur, and the company itself.

Why is this? When is it good advice? And why do the interests of venture capitalists and entrepreneurs frequently diverge with respect to this issue? Read on.

## Investor's and entrepreneur's interests diverge

There's no question: entrepreneurs engaged in trying to create a new business need to be focused on just that — creating a new business. Working on multiple projects in parallel, pursuing long term research projects without immediate payoff, or a lack of focus on attaining tangible business milestones are all worrying red flags to the investor.

*The question is not whether focus is necessary, but how tight that focus should be on a single, high risk bet.*

For the investor in an early stage, pre-revenue startup there are three broad classes of risk.

- 1) *Technology risk* is about whether we can make the product we want to make, and wrestling with scientific and technical unknowns.
- 2) *Market risk* has to do with whether there is really a need for the class of product or service we are developing, and whether our product will be better than competing ways to meet that need.
- 3) *People risk* has to do with whether or not the team we choose can deliver on our expectations.

Early stage, private equity investment pools are usually structured as a portfolio, in order to diversify against each of these three risk categories. The investor's goal is to maximize the overall return of the portfolio. In contrast, the goal of the entrepreneur is to maximize the chance of success of his or her single venture, since entrepreneurs have not yet found a good way to diversify across a portfolio of startups, being typically restricted to one at a time, and at most a handful over a lifetime.

Tension between these two objectives commonly arises in a situation where there are multiple, quite disparate product categories that could potentially be

enabled by the company's core technological capability, and there is a high level of scientific risk associated with each of them.

Entrepreneurs, keen to reduce risk, often prefer to commence several parallel projects, attacking more than one of the possible product concepts — winnowing them down to one project once technical and market risk are reduced to modest levels. Taking this parallel approach costs more than pursuing a single concept, thus turning a small, high risk investment into a larger, but less risky one.

In contrast, the instinct of the venture capitalist is to focus on a single product opportunity initially, only expanding to others if the first is successful. After all, if an investor really wants to invest in multiple product concepts, why not set up multiple separate companies with multiple management teams — thereby allowing diversification to reduce both the technical risk and the people risk of the overall portfolio?

Usually the debate is framed as being about the extent to which the company should be forced to focus. However the real debate is about whether to maximize the overall risk-adjusted return of an entire investment portfolio, or to maximize the probability of success of the startup in question — albeit at the cost of some additional investment to reduce its risk.

Because the investor has a portfolio while the entrepreneur does not, and because the investor would like to diversify against people risk, while the entrepreneur is stuck being one of the risks, it should be no surprise that interests diverge here. Because diversification against technical risk requires additional investment, and is not usually in the interests of the investor (at least beyond a modest level), the outcome of this tension is usually predictable.

However, as the example following illustrates, sometimes entrepreneurs do have choices. In such cases it is important to be quite clear what is at stake.

## A case study

Summit Technology, Inc.<sup>1</sup> (of Boston, MA, USA) was founded in 1985 to develop medical systems using a new type of laser — the excimer laser. Today, the

<sup>1</sup> From 1986-1989 the author was employed by Summit Technology, and was responsible for management of all its product development activities. As VP, Technology Development he was a member of a three man executive team that managed the company at the time of its IPO in 1987, under the leadership of its founder and CEO.

primary medical application of this type of laser is *photorefractive surgery* (PRK & LASIK), in which a thin layer of the front surface of the eye is precisely sculpted to change the shape of the eye, and thereby eliminate the need to wear eyeglasses.

Summit Technology was one of two companies that developed the first products for photorefractive surgery, initially leading the race and subsequently falling into second place. By 2004 over eight million patients had been treated using this family of procedures in the US alone<sup>2</sup>. Summit went public in 1987 and exceeded a stock market value of \$100M by 1989; \$500M by 1992; and \$1Billion by 1995. It was ultimately acquired by Alcon (then a subsidiary of Nestle) in 2000 for \$948M.

By most accounts this would be considered a successful venture. Interestingly, there was a point of time in 1987 when the company came within less than four weeks of running out of cash. I will argue that only the company's *lack of excessive focus* saved it. Here is the story.

### *The potential*

Summit was founded to exploit a class of laser, called Rare Gas Halide Excimer lasers (we will call them excimer lasers here), which had been invented in 1974. These lasers were available commercially only in a rather primitive form, which was quite unsuitable for medical applications. Making them was somewhat of an art form, limited to a small number of individuals: primarily research oriented scientists.

Early experiments in several laboratories around the world had suggested that this type of laser could cut tissue with precision that simply could not be attained by other techniques. In particular, the experiments held out the promise of sub-micron sculpting of tissue, without affect to surrounding tissue — the functionality that made PRK & LASIK possible.

At the time, a property that appeared to have even greater potential was the ability for excimer laser light to be delivered along a flexible, thin fiberoptic catheter, and to cut a diverse range of arterial plaque tissue (including calcified plaque). This promised a substantial improvement in a procedure called *laser angioplasty*, akin to rotor rooting out a patient's arteries. We hoped that the excimer laser would both expand the range of patients that could be treated, and perhaps also reduce the incidence of restenosis. This adverse effect (in which the reopened artery closes again within

months of the procedure) was one of the most significant complications associated with the (then) rapidly emerging procedure of balloon angioplasty.

### *Our plans*

Summit Technology had just received its first round of investment when I joined it in early 1986, fresh from academia, and had only a handful of employees. While the initial investment had been targeted at laser angioplasty, Summit was also pursuing a skunk works project to develop a prototype of a photorefractive surgery product for ophthalmology. One of my most pressing initial tasks was to build a core team capable of moving both of these projects along in parallel.

Not only did we plan to develop two quite different commercial medical products, based on a new family of laser. We also planned to collaborate with the medical community to pioneer two completely new medical procedures: *photorefractive surgery*, and a modified form of *laser angioplasty*. In order for the company to be successful we needed at least one of those procedures to become widely used.

Had I been more experienced I would have considered these goals to be over-ambitious. However luck and science favored our efforts, and by 1987, after lots of hard work, we had developed working prototypes and were doing tests of them for both angioplasty and photorefractive surgery.

We had also developed a prototype of a third product, called an intravascular ultrasound (IVUS) imaging catheter, for the observation of the inside of patients' arteries. This third project was initiated because we realized that laser angioplasty was unlikely to work the way we wanted it to, unless we also developed some form of guidance technique to help determine which pieces of artery needed to be excised. The intraluminal imaging technology had the potential of providing such guidance, and of becoming a valuable diagnostic product in its own right.

In each of these three product areas we were in close competition with rival startups, but in each case we managed to reach key proof-of-principle milestones first. This had a big effect on our ability to continue to raise money, and to attract the best employees and medical collaborators. As a side comment, this illustrates one of the most important of the lessons I have learned about commercializing technology. Once you are behind, everything gets harder and harder. It is hard to overstate the importance of effective and speedy execution.

<sup>2</sup> Wall St Journal, pg 1. Oct. 26, 2004.

### *Many Challenges*

Each of the three product concepts we were pursuing came with huge challenges. In each case, we were trying to do something that had never been done before, and each of the product development projects faced significant technological hurdles. However, by far the biggest challenges were associated with the need to develop new medical procedures, and the impossibility of predicting how biology would cooperate with our plans.

For example, in the early days of the laser refractive surgery project, the clinicians with whom we were working started to become concerned about the tendency of tissue to remodel itself following the resculpting process. Many other possible pitfalls became apparent as the projects progressed, ranging from the potential of the laser radiation to cause cellular damage if the wrong wavelength of light was used, to potential damage from laser induced shock waves if the laser was adjusted incorrectly.

There was clearly very high “science risk” associated with each of our programs, and in retrospect there was also very high business risk. For example, it was far from clear that the world would embrace an elective surgical procedure to reshape an otherwise healthy eye, and there were many who told us no-one would ever buy one of our products, even in the unlikely event we could make them work.

On the other hand, as pundits later liked to say about our company, “our potential market was a third of the human race”! This was truly a high risk, high reward venture.

### *Our perspective at the time*

My view at the time was that we had a novel technology, which was capable of creating a variety of tissue effects that appeared to be medically useful and certainly could not be duplicated using other equipment. I felt confident that if we persevered we would find a medical application that was valuable, even if it was not one of the initial two on our list. Pursuing multiple options in our product development seemed a logical way to maximize our chance of success.

The usual argument against attacking multiple projects in parallel is that it costs more than pursuing one (which it does), and that each project proceeds more slowly than it would if the company were totally focused on it alone. I have never been a believer in this latter argument: so long as the senior technical management has the skills to manage multiple projects; and each project is led by an outstanding product champion who

is focused solely on that project. At Summit we were able to attract an excellent group of individuals, and I remain convinced that far from being deleterious, our pursuit of multiple parallel programs was a great benefit — leading to cross pollination of ideas, and allowing us to assemble a larger critical mass of brainpower with which to tackle the really difficult problems we faced.

### *Financing*

Summit Technology’s financing history was somewhat unusual. Had our initial investors been conventional venture capitalists, I think it unlikely that we would have been allowed to pursue our parallel product development strategy.

Summit’s first round of investment came through a R&D limited partnership raised by Prudential Bache, and channeled through a larger company that could supposedly provide a degree of adult supervision to our projects. In our case the larger company (small at the time, but now an industry titan) was Boston Scientific. Because of the absence of traditional venture capital influences, and because our founder and CEO, David Muller, was a forceful negotiator, we were able to pursue our parallel development more or less as we wished through 1986 and early 1987.

While my team and I were moving the various product development projects forward (they were at that point really at the proof-of-concept stage), David Muller had his head down, working with our experienced CFO, looking for the next round of investment. They talked with all of the early stage venture investors in the Boston area, but none of them seemed ready to invest. We had several very promising interactions with large, potential acquirers, but none had as yet quite made it across the finish line. Meanwhile, our cash balance was beginning to drop alarmingly, and in our weekly management meetings we monitored carefully how many weeks of cash remained.

At this point we established contact with a Wall Street firm that specialized in taking very early stage companies public. As discussions with them progressed, the idea of doing an IPO moved from a far fetched possibility to our most attractive option. We moved forward, filed the red herring, went to Wall St to meet the brokers, and did all the other things one does preparatory to going public.

In the course of writing the prospectus there was considerable discussion about focus, and about whether we should emphasize ophthalmology, or cardiology, or perhaps even intraluminal ultrasound, which then appeared closest to becoming an actual product.

Everyone felt that the ophthalmology project was the “sizzle” and so that was highlighted. Luckily for us, the other projects were given significant weight too.

We reached a point in time when our IPO date was imminent, but we had only four weeks of cash left in the bank, and no additional sources of credit. Each week we debated whether we should lay off some of our team, thereby buying us a few more weeks of solvency. As the deadline got closer and closer we got more and more nervous. David Muller was determined not to begin dismantling our team, each member of which had been recruited only after great effort, and so we resisted making any staff cuts — a risky call, which proved to have been correct.

With this as background, a few days before our IPO was scheduled to go out, we awoke one morning to a press release announcing issuance of several hitherto unknown (to us) patents, which read directly on our core application of laser refractive surgery. The underwriters convened a panic meeting of all concerned, and decided to put everything on hold for a week while we learned more about the patents and evaluated their likely impact on our business.

Well, the patents turned out to be very fundamental. They clearly did read on our refractive surgery application. While there was some question as to their validity, and we had our own patents that ultimately enabled Summit to cross license them, there was no quick way to eliminate the obstacle this newly discovered patent portfolio posed to our IPO.

After a week, and many intense discussions between investors, underwriters, brokers, and numerous attorneys, it was decided to proceed with the IPO on the basis that ophthalmology was just one of three possible revenue sources for our nascent business. The patents were disclosed as a risk, the price of the offering was tweaked, and after a bit more time the IPO went out and Summit raised \$5 million. *Less than 3 months later, the stock market crash of 1987 took place, and for a considerable time speculative underwritings such as ours were no longer possible.*

### Too much focus would have been a bad thing

In retrospect, of the three applications we were working on, laser refractive surgery proved to have the greatest business potential, and to be the place where the company ultimately realized commercial success. So would it not have been better if we had just focused on that from the start?

Well, no. First, as described above, if we had bet the ranch on ophthalmology our IPO would most likely have been withdrawn, and the crash of 1987 would have taken place before we raised money. Since no other source of investment appeared close to producing a check, it is very likely that the company would have failed, and that the multi-hundred millions of dollars of stock market value captured by Summit’s shareholders would never have been created.

Second, it was by no means so clear when we were developing photorefractive surgery that it would become successful, and in many ways the other two applications we were pursuing appeared better bets.

Each of our competitors at the time was pursuing a single bet. So there were a number of companies developing laser angioplasty products of various flavors, several companies developing intravascular ultrasound (IVUS) imaging products, and several developing photorefractive surgery systems. The laser angioplasty companies all either went out of business or were morphed into quite different businesses. The best of the IVUS businesses achieved success (being acquired for roughly \$100 million), as did the better of the other ophthalmic businesses. Had we chosen only one application at that early stage, who is to say it would have been the right one?

In this case study, two of the three applications we were attacking ended up being commercially viable. This is a far better ratio than the average for novel applications of this type, and it would not have been surprising for only one (or even none) of these to achieve commercial success.

I have come to believe that three is a good number for such risky, very early stage projects with high degrees of science risk. Any less, and the risk gets unattractively high and it becomes hard to attract and motivate the right people. Any more and management gets complex, and costs get excessive. Besides, I believe that experienced technical management, *allied with strong up-front market analysis*, should be able to pick at least one out of three winners.

### The next stage of growth

Before being acquired in 2000, Summit was to raise a total of more than \$200 million in a series of public offerings. After moving each of the three projects further toward the market during 1988 and early 1989, it became clear that refractive surgery was by far the biggest opportunity; that intraluminal ultrasound was a lesser, but still substantial opportunity that warranted

being spun off and could be financed as a stand alone entity; and that laser angioplasty looked increasingly unlikely to be a big winner.

The company began to refine its focus, and ended up concentrating solely on commercializing laser refractive surgery: in the process building a business with revenues of \$111M in 1999, the year before it was acquired.

### Narrowing focus over time

This article argues that in the early stages of projects with high science risk, too much focus is a mistake. A similar argument applies to the early stages of market testing associated with businesses with high market risk.

However, once the major risks have been tested and reduced in both market and technical areas, the operational complexity of a new business grows rapidly. At that point it is no longer as valuable to maintain diversification against technical and market failure, and the distractions of managing multiple projects begin to significantly outweigh the benefits.

Furthermore, a strong case can be made that once projects pass the initial risk reduction points, and move into a business expansion phase, they really need full time, dedicated management by executives versed in the full range of business creation skills — and that this is best done in an autonomous setting.

Perhaps the ideal model, then, is an incubation phase in which several projects with high risk in either science or market areas are incubated together, after which the projects that pass the test of viability are spun off into a series of stand alone entities.

The key features of such an *incubator model* would be that

- entrepreneurs have a way to diversify their risk across a small number of high risk projects, thereby enhancing the probability of at least one success, albeit at slightly reduced overall financial return compared with a focused home run.

- after a risk reduction phase, successful projects are spun out into stand alone entities, with committed and focused management intent on business creation.

This is quite different to most so called “incubators”, which primarily involve the sharing of office space and similar support services, and conspicuously lack any form of entrepreneur risk diversification.

### An opportunity?

As I look at the landscape of innovation as it is practiced today, examples of the model described above are rare in fields based on the physical sciences, but are somewhat more common in biotechnology. Traditional, non-biotech venture models emphasize focus on a single project, with a common analogy being to put “all the wood behind a single arrow”. This model is best suited to a business for which the largest risks are those of execution and financing, rather than one in which quirks of nature may raise unanticipated and insurmountable barriers to success.

This suggests to me that there may be a substantial opportunity for an early stage incubation model that would exist as a feeder to traditional venture capital financing, and would focus on risk reduction of very early stage, high risk, high reward projects.

Such a model could be particularly valuable in fields such as nanotechnology, in which there is high science risk, often also high market risk, and substantial business potential. An innovation *front end* of this type could substantially increase the overall success rate of entrepreneurs, and at the same time increase the overall portfolio financial return by weeding out failing projects early.

Rigorous weeding out of failing projects, and timely recycling of the most effective entrepreneurs into new projects are operational aspects of this model that would need to be effectively managed for it to work well.

I plan to develop this idea further. If you have comments or ideas on this topic, please contact me.

## About TangibleFuture, Inc.

Innovation has traditionally been a risky process, with a high failure rate<sup>3</sup>. In our experience, a significant portion of these failures occur due to neglect of a critical set of activities that lie at the intersection of technology and the market. We believe that many of these failures could have been avoided, and that the techniques for doing so can be systematized and applied in a variety of situations. *TangibleFuture, Inc.* was formed to take on these tasks, and to be a resource for high-tech entrepreneurs and intrapreneurs. Our goal is to increase substantially our clients' probability of success.

We work with startups, research organizations, and established companies planning to grow by means of new products, new markets, new business initiatives, or acquisitions. We focus particularly on markets and industries in which things are changing rapidly, or new disruptive technologies are emerging and creating turmoil. Our approach generally involves a blend of strategic advisory services, hands-on execution, and analysis — and is uniquely tailored for each engagement to help accomplish a specific, agreed upon, business objective.

Every client we work with is creating something different. And each one has different strengths and weaknesses. Our role is often to help increase the strengths, and fill in for the weaknesses. So what we do varies for each assignment. Please feel free to contact us to discuss *your* plans and to see how we can help.

Our team has a blend of skills, experience and knowledge that makes us particularly effective. These include

- senior management experience in high technology businesses: we know what its like to be in our client's shoes, and what it takes to succeed;
- a track record of successful consulting engagements with clients ranging from startups to large multinational companies: we know how to provide effective help;
- expertise in both technology and market analysis: we know how to find out what customers will need several years in the future, and understand what a technology could be capable of by then. We can help craft

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<sup>3</sup> In his book *High Tech Startup*, John L. Nesheim reports that “the chances are six in one million that an idea for a high-tech business eventually becomes a successful company that goes public”. Clayton M. Christensen, in *The Innovator's Solution*, estimates that only about one in ten successful companies is able to maintain above market growth rates over a period of a decade or more.

a product definition that will maximize the business opportunity, while remaining within the bounds of what time, money and technical capability make feasible; and

- deep industry expertise and a global perspective: we know already a great deal about the industries in which we specialize, and the activities of their key participants in North America, Europe and Asia.

## Biography: Richard G. Caro



Dr. Richard G. Caro is the President and founder of TangibleFuture, Inc., where he helps entrepreneurs and managers create and grow high technology businesses.

Prior to founding TangibleFuture, Richard was Managing Director at RHK, a provider of advisory services to the communications industry, where he was responsible for leading RHK's Communications Components consulting practice and Innovations competency center. Previous roles include CEO (founder) of Vital Insite, a venture-backed start-up company developing continuous, noninvasive blood pressure monitoring devices; Engineering Program Manager at Coherent, then one of the two largest laser manufacturers in the world; and V.P. Technology Development (CTO & employee #5) of Summit Technology, a pioneer in the laser refractive surgery business. Before entering industry, he was a member of the research staff at Stanford University, working in the fields of applied physics, optics and laser development.

Richard has been responsible for the development of a number of successful products, and has 23 issued patents and over 45 peer reviewed publications. He received a B.Sc. (Hons) degree from Melbourne University, Australia, and a D.Phil., in experimental laser physics and nonlinear optics, from Oxford University — where he was a Rhodes Scholar.

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