TangibleFuture™

Lightbit: from rags to riches to chips to China

by Larry R. Marshall

The Lightbit Saga:

It was 2000, the banner year of optical networking; billions had been made & the promise of 10's of billions was in the air. The *All Optical Network* was the talk of the day, and one leading analyst presented a matrix of leading telecom system companies and the key technologies they had acquired & had yet to acquire; and there we were, *Lightbit*, the only company in the coveted *all-optical wavelength converter* column. We received a soft offer from a large US Semiconductor company for $100M, which our Board immediately rejected; and they were right — they had just rejected a $1B offer on my buddy's company and it turned into $3B! We were going to be worth Billions — well, so much for our 15 minutes of fame.

Ironically we actually built solid beta versions of the product, & in doing so, we tamed one of the most difficult materials problems known in the optical space — making optical signal processors out of sensitive (easy to break) lithium niobate. We marketed to Carriers (our customer's customer) to create genuine end-user pull and believed, for a while, as some of them did, that we held the key to the all optical network. However, our market intelligence was also strongly influenced by the larger System vendors who were our direct customer. In their desire to compete, they keenly pursued the new technologies & the dream of the all-optical network, which in turn confused the carriers as to what was possible & when it might become reality. Now we weren't stupid, and we hadn't entirely drunk the koolade — we knew the transition to all-optical would be hard, and pretty unlikely, but the potential payoff if it happened would be huge — and its that payoff that we were hungry for. In fact we tried hard to find a near-term application, that would feed us while we developed the product — we focused on eliminating dispersion & nonlinearities using the same optical chip, to ease (& accelerate) deployment of 10G. We generated enough interest (or froth) that we received a 2nd approach, this time down to $50M, from a large US Telecom company. The falling price should have told us something — right? Unfortunately, we listened to the wrong carriers (& analysts) and discovered too late that 10G had already been largely deployed, despite fiber & dispersion problems. Carriers & large OEMS continued testing our beta for *optical 2R regeneration*, and loved the technology, but didn't buy. The key to crossing the chasm from technology to real market, is for customers not investors alone, to fund the transition. Without those early adopters, there is little justification to deploy a new technology no matter how "*exciting*".

Then came 9/11, and we had to raise $ during the resulting flight of capital — it was really tough, but our customer support got us through, and in fact it was a slight up-round. Despite everything, there was still hope, and a customer out there at the end of the tunnel — we thought. It was a year later that it became very clear that these technologies would never make it into the market- telecom companies were selling for $1.5M regardless of how much capital they had raised (and spent), and regardless of their revenues. Companies that had raised $80-120M in capital and had revenues of a few $M were all selling for only a few $Million, and most of that in stock. What hope did we have, with no paying customer & a cool technology that the market didn't really need?

Fortunately, I had previously been lucky enough to participate in an IPO in the biotech space, and in a classic skunkworks approach we had embarked a year earlier, on a development program to create a biotech chip/device. We quickly secured a partnership with a key value-chain company, and, paid primarily by them, we created the needed product. So a year later as we approached the cliff, we were able to orchestrate bidding between several large public companies – our best offer was a multiple of the dreaded fixed tag, but it turned out that the CEO who made that offer didn't really have a mandate from his board. We turned to the 2nd bidder, and as we began drafting definitive documents, their CEO was replaced & the new guy froze all M&A. So now were out of $, and options.

Fortunately, our team were pretty seasoned guys who had gone through the pre-telecom optical startup phase — as I had — where you work for equity & don't get paid except by a customer — so we soldiered on & were able to secure a deal with a Chinese investment group. It was in many ways better than any of the other deals, and seemed like a "hail mary" pass — we actually sold at a multiple of the industry average when most other technology plays hit the wall without even so much as an IP auction. The earnouts on that deal should turn it cash positive this year.

So what were my lessons from this? Firstly, I already knew well (from 3 previous startups) that it costs 2-3 times more to market & sell a product, than it does to create it (even one as difficult to create as this one) — so why didn't I spend more on marketing? I had an opportunity to engage Richard Caro and his analyst team while he was at RHK, but didn't, and it cost me far more dearly than the $ I thought I had saved by relying solely on our Carrier feedback. When market analysts are interacting with all elements of the value chain, and there is no customer-vendor friction, their information is clearly better than yours.

Secondly, while focus is core to any startup, sometimes it needs to shift — this is why VCs invest in teams, not dreams — the team can change & find a new dream if the market changes (& its always changing). No matter how big the potential payoff, rule #1 for any CEO is don't let the company run out of $ — this rule is a little different for VCs, they don't want you to lose their $, but they are not interested in purgatory either — i.e., its better for them if you fail rather than lingering on in a smaller market — the economics of venture investing rewards the big bang, not the boring if profitable small business. While we were able to refocus on a real market, it was too small to justify venture investment — ironically, about a year after we shifted focus, most of the tunable laser companies chased the same application we had, and added tremendous credibility to that market. We could have misled the VCs into believing that market was much larger, and gotten more funding — in fact, we had lined up term sheets from several, but decided not to take the $. Which leads to my final point — know when to sell.

The key to knowing when to sell, is to understand your real value. In hindsight should we have sold for $100M? Sure, but our board made *exactly* the right decision at the time despite ensuing events — our investors were playing for a Billion, not a few Million. If we had spent more $ on external marketing resources, we would have had a much clearer understanding of our real value, and subsequently communicated that to our investors, and this key information may have enabled them to reach a different conclusion. This was clearly my fault — if I couldn't convince my board to take the deal, then I failed, not them. When we finally did sell, it was for a lot less, but at least it was a sale.

Now, most M&A guys will tell you that you'll never know the real reason someone buys your company — this is the key to game theory — the win-win deal (I mention this because one Aussie business exec I met thought game theory was lying to, i.e., playing games with, your adversary). We argued synergies, and there were — consolidation of FABS, key chip technology to feed into the Chinese consortium's existing products, a key partnership that would lead to good revenue; and core IP that would sustain that revenue for many years. What we didn't know, was the deal also got the Chinese consortium 2 key customers, who enabled them to qualify for up to $100M in Chinese government money…….and we thought we did a great deal.

About the author

Dr. Larry R. Marshall is a serial entrepreneur with 6 successful startups to his credit over the past 16 years in Biotechnology, Photonics, & Semiconductors, with several profitable acquisitions and one IPO. He is currently Chairman of Intersymbol, semiconductor processor company partnered with Intel, and IOA a Communications company partnered with Motorola. He previously served as CEO (& founder) of Translucent developing optical gain in Silicon for interconnects, which was recently acquired after a successful partnership with a large, US, Public semiconductor company. Prior to Translucent, he was IPO (& founder) of Lightbit manufacturing optical processing chips for Telecom Regenerators and Biotechnology chips. Prior to Lightbit, Larry was VP of Iridex, where he enabled the company's IPO, and co-founded spin-offs Iriderm, and Iridex' OEM Group. He was CEO (& founder) of Light Solutions where he rolled out the first visible semiconductor lasers, and formed Iridex. Prior to Light Solutions he ran Business Development, and formerly Technical Programs at Fibertek, where he rolled out the first 1540nm parametric laser. Larry's primary focus is studying new markets, and then developing appropriate technologies to leverage new products into those markets.

Larry holds 19 patents protecting numerous commercial products, and has over 100 publications and presentations. He is a Federation Fellow, Chairman (*emm.*) of *Advanced Solid State Photonics*, an Editorial Advisory Board member to *Laser Focus World*, and holds Board positions on Arasor, AOC, and Photon Engineering. Larry was born in Sydney Australia, received his BS Honors from Macquarie University (Sydney), and PHD from the Commonwealth Center of Excellence.

