Benetech Landmine Detector Project
Lessons Learned Essay
August 2007

Introduction

Benetech has been working in the humanitarian landmine detector field for more than six years. For most of that time, we were exploring and thinking, expending money at a small rate. In 2004, we decided that the time was right to take this project to the next stage, and actively work to build an affordable landmine detector for humanitarian use. In March 2007 we changed our mind and put the project on the back burner again.

Why? What happened? What can we learn from this? Answering those questions is the goal of this post mortem essay. We want to explicitly thank the Lemelson Foundation, Armand Neukermans and the Flora Family Foundation for their support of this project and this retrospective. The willingness to take risk and to fail on occasion is essential to make progress on major social issues, and their leadership needs to be acknowledged.

Benetech's Project Choice Rubric

Benetech operates as a hybrid organization encompassing elements of a high tech company, a foundation and an operating nonprofit. Because we incubate a number of projects, we have developed a methodology for making choices with our limited funds. This is quite similar to the choices that foundations, venture capitalists or business development teams make in deploying their capital for maximum impact.

At any one point in time, Benetech has dozens of good possible projects in the area of socially beneficial applications of technology. Roughly once a year, we work through these projects and pick those that are ready for more investment. The criteria are extensive, but we can focus on a short list of the major ones:

- Social Return on Investment
- Unique Opportunity
- Low technical risk
- Deal Size (500k-3 million)
- Leverages other Benetech projects
- Fundable
- Exit Options

Until we feel we have the right set of conditions, we leave projects sitting on the back burner. Since we focus on problems the market is unable to solve, those projects left on the back burner are unlikely to be addressed by other parties even as years go by.

Project History

In early 2000, Fruchterman attended the American Association for the Advancement of Science Annual Meeting in Washington DC to give a talk on speech technology for people with disabilities. Fruchterman was interested in landmine work, and attended a

session on this topic that showcased DARPA's research program in landmine detection. Fruchterman met Dr. Patrick Ball (now Benetech's Chief Scientist) there. Dr. Ball was the AAAS staffer assigned to coordinate this session, and their meeting was a turning point that led to the creation of Benetech's human rights program. Fruchterman also met Dr. Regina Dugan, who was then running the DARPA landmine detector research program.

Dr. Dugan was enthusiastic about the Benetech model. She saw it as a likely solution to the limited market potential of humanitarian landmine detectors. A month after the meeting, Dr. Dugan sent Fruchterman a five page concept paper on how Benetech could get involved. She also introduced Benetech to her most likely technology: the nuclear quadrupole resonance (NQR) detector from Quantum Magnetics.

Benetech got along well with the QM staff. They were very interested in seeing their technology applied to the humanitarian need, since their primary focus was on military applications. But their technology wasn't ready, so we stayed in touch with them over several years. During that time, the military pumped money into the technology and it got steadily better.

Finally, in 2004, Benetech thought the technology was ready. Armand Neukermans, a top scientist and the project's leading fundraiser, went with Fruchterman to see working prototypes in QM's San Diego facility. They brought along Ted Driscoll, an experienced Silicon Valley venture capitalist and the former Vice President of Engineering for Diasonics, a company in the magnetic resonance field. Driscoll recognized a couple of the employees at QM because they had worked for him at Diasonics.

Benetech hired Driscoll part-time, raised more money and wrote a business plan. We started negotiating with QM to get access to the technology. About this time, General Electric bought InVision, the parent company of QM. Although we did not expect this acquisition to affect our plans, we were wrong. InVision had some problems and the deal took almost a year to close. After the acquisition, the GE attorneys continued to move slowly. There were apparently patent negotiations with another party. Although both sides were favorably inclined towards our work, we were bogged down. Finally, in early 2006 Benetech concluded a Memorandum of Understanding with GE that allowed us to move forward. This was also a key contingency to receive funding from our largest funder, Lemelson. We hired a top engineer from Sun Microsystems, Parag Mody, to work on the project, although only part-time.

We then ran into another roadblock. The Office of Naval Research at the Department of Defense turned down what we thought was a routine request to release the confidential data on the technology and test results. We had received informal assurances that such a request would be approved soon before GE made the request on our behalf. We immediately began work on reversing the ONR decision.

During this time, we did research, visited humanitarian landmine groups and kept working on the project, but without access to the core technology. Since our major technical goals were to reduce the cost of the technology and test it with humanitarian groups, this put a significant crimp in our work. After another long period of time, during which we whittled back what we were asking ONR to allow us to do, we finally received permission from ONR to have access to the technology.

We returned to GE to get access, and GE was no longer willing to provide the access they had agreed to earlier. They maintained the MOU had expired and that they would have to revisit the entire question of sharing the technology with us. During our long negotiations with ONR, GE had changed management, laid off most of the technical staff and sold many of the rights to the technology to a British military contractor. At the time of this writing, more than six months later, we are still in discussions about gaining this access.

Benetech had been thinking we were on the edge of getting access to the technology for several years. This latest roadblock caused us to assess whether our optimism was justified, and we finally had to admit it was not. Driscoll went back to being a full-time venture capitalist, and Mody joined a startup. We put the project on ice and moved to wrap up what we had learned.

Lessons Learned about the Humanitarian Landmine Field

In the seven years that we have been exploring and researching how to make better tech tools for humanitarian demining, we have learned a great deal about the field. Benetech has extensive internal documentation on this, and a 70-page research project we commissioned is being published at the same time as this essay. The following points summarize the high level lessons at the field level:

1. <u>The humanitarian landmine problem is both better and worse than people think.</u>

The countries and NGOs that want to raise money for mine clearance have an incentive to overstate the mine problem, and they often do. They will emphasize the numbers of mines estimated in the ground (and estimate high). The real issue around landmines is economic. What assets are unutilized or underutilized because of the mine threat? There are a huge number of mines in areas that lack significant economic impact (say, North African desert). The focus of demining over the last decade was on the economically important targets, and good progress has been made. Mines that remain are in areas of diminishing interest, and the cost of finding them is going up because of the sparse numbers of mines per unit area and the difficult terrain.

At the same time, the funders of demining projects have an incentive to understate the problem. The United States, the largest funder of humanitarian demining, has more or less declared that the problem will be over by 2011. Funding is already declining precipitously, and people in the field are moving to other areas of work (or changing focus to cluster bomblets and unexploded ordnance). The landmine ban treaty, while still missing key countries like the U.S., Russia and China, has resulted in decreased use of landmines.

Our opinion is that the truth lies between these extremes. Landmines will not cease to kill or maim people in 2011 and beyond. They will continue to be a significant problem, and it is a morally repulsive problem. Noncombatants should not suffer from these weapons, and yet they will. At the same time, landmine removal will get less and less priority as the major economic impacts are addressed. Societies, both those mine-impacted and those providing demining funding, will choose to shift resources to other problems and accept the level of risk that comes from lowered investment in demining

issues. For example, people in France and Germany continue to discover unexploded ordnance and landmines after more than fifty years, but it doesn't make sense to invest money in finding them beyond taking care when excavating in areas that were heavily bombed in WWII.

2. The dynamics of mine clearance are not what you'd expect.

We came into the field thinking that a laborsaving approach would work and that success would be measured in getting the same demining work done with half the staff. Wrong.

In the most mine-affected countries, landmine clearance work is seen as a jobs program. Getting a job that pays double or triple the prevailing wage is highly prized. Few field demining managers are interested in laying off staff. In Cambodia, mine clearing by people other than the official demining groups is outlawed.

Many countries ARE interested in technology that speeds mine clearing or makes it safer, but they strongly prefer a technology that keeps the same number of workers busy.

Most demining equipment is subsidized by donor governments, which often also subsidize their own industries. For example, we learned that Japan provides much of its aid to Cambodia's mine removal projects in the form of trucks and tractors, and they thereby support those manufacturers in the process. The demining groups often get their equipment free or deeply discounted, so don't have to pay the full cost of it. Cash support is the most valuable kind of aid, because it can be used to meet payrolls (for the jobs program). American aid has been highly valued because it has traditionally come as relatively unencumbered cash. It is not clear that groups would choose to spend as much money on tools if they simply had to buy them at the expense of salaries for staff.

3. New technology for the landmine problem rarely affects actual demining (our project is an example of this, but hopefully a responsible example).

We were quite surprised to find that the technology used in the field has not changed much in fifty years: metal detectors, sharp sticks (probes), dogs and some mechanical solutions (rakes, flails and rollers). The only new technology to be introduced that is beginning to get use is ground penetrating radar (GPR), and that after more than 15 years of trials and prototypes.

Deminers are understandably conservative in adopting new technology. There is danger in changing proven methods. But deminers are pragmatic and open to solutions that meet their real needs. Landmines are an intensely interesting topic to technologists. Hundreds of solutions have been proposed, usually without input from actual deminers. We found the field deminers to be favorably impressed by having the opportunity to discus their requirements with us, before we designed something to meet their needs. It's clear that actual user input is crucial for any future project that hopes to have any chance to make an impact on this issue. It's also clear that any solution needs to actually make economic, operational and political sense.

4. The biggest need from the field remains unaddressed: area reduction technology.

The clearest message we received was that there is demand for a product we haven't yet been able to create: an area reduction tool. As dense minefields are cleared, the landmine problem more and more becomes a problem of sparseness: there are relatively few mines

per a unit of area. The dream tool was described as something like the following. Park the instrument in a spot, leave it overnight, and in the morning it tells you whether there are any mines within a given radius (100 meters, 500 meters). If it says no, you move on to the next area to test. If it says yes, then you know that it's worth using the traditional close-range detection techniques in that area.

5. Deminers were interested in the tool we wanted to build.

An actual explosives detection tool would be valuable to deminers. It needs to be affordable (under $10,000) and work reasonably quickly (a couple of minutes would be acceptable). It would not replace metal detectors or mine detecting dogs, but would be a useful addition to their toolkit.

Political Lessons

We didn't know how big a deal this was. We ran afoul of numerous political barriers, and we were relatively naïve about how to deal with them. Historically, Benetech simply waves the intelligent good-guy flag, and people help us. We started with pretty decent political connections: a strong relationship with the CEO of QM and a good meeting with the U.S. State Department. But our relationship with the U.S. Department of Defense never progressed. Our interactions with Congressional staff and legislators were weak. As GE took more and more control of QM (the QM CEO left soon after the acquisition), we never made connections with more senior GE managers.

This project was one where political issues came to the fore, and we were not effective in addressing them. We were unfortunately trying to get government assistance during an administration which is fighting an ugly war in a distant land. The DoD isn't feeling particularly humanitarian, and is protective about explosive detection technology. In hindsight, our timing was poor.

Lessons about the Benetech Process

The main issue here is how to improve our process. Let's subdivide this into questions about our original decision, our continuation of the project in the face of delays and what to change about our process.

1. Was our original decision a good one?

We knew we stretched a bit at the time with the decision to move forward, but we justified each variation. We didn't have the technical expertise in-house, but we felt we could obtain it with Driscoll and our star-studded advisory panel (and later Parag Mody). This was going to be Benetech's biggest and most expensive project, but we felt we could raise the money. We weren't completely sure about the sustainability model or exit strategies, but we had plausible ones.

We were certain this was an important problem to solve, and that our approach was more likely to have an impact than prior efforts in the field. Our team and board were enthusiastic, and we felt that this was a marquee project, something everyone could understand. Based on our discussions with the QM staff, we waited until we thought the technical risk had been reduced. We underestimated the export permission risk, but we

had just gotten export permission for our Martus software, which was also classified as a munition under export law.

All in all, every project we take on involves risk. This one was riskier than most, but we felt the risk-reward tradeoff justified moving forward. We thought that our original decision was a good one, knowing what we knew at the time.

2. What about our operation of the project? Did we keep going too long?

It is clear that this was an area where we could have done better. Our decision to ramp up spending before we had access to the technical information was definitely premature. We kept spending going (even if only at the two people part-time) on the belief that access would happen any minute. Unfortunately, this access was not granted.

3. What should we change in our process?

We talk about non-technical risk in our process document, but we paid little attention to it compared to technical risk. We should mark down the attractiveness of projects with strong political aspects unless we have a partner who can handle this. We should not expend significant money when the project's viability depends on getting access to controlled technology. We should explicitly add interim milestones to revisit the go decision on a project, and identify what negative issues or events would drive suspending a go project. This latter idea has been suggested as a pre mortem step: try to anticipate scenarios where things would go wrong.

Conclusions

Benetech is an ambitious organization trying to have maximum impact on global society. We should expect to have failures. If we have none, it raises the question of whether we've aimed high enough.

At the same time, failure hurts. Users who spent time with us hoped we could deliver value to their mission of demining. Donors committed funds to us because they believed in our chances of success. Staff and advisors invested time and intellectual energy because they wanted to make a difference.

If we're going to have failures, we want to fail smart rather than fail dumb. We need to identify these findings and disseminate them. Maybe someone else will be more likely to succeed because of something they learned from us. Going forward, we need to integrate what we've learned to improve our chances of success and avoid making certain mistakes again.

We still believe the landmine problem is worthy of effort. We still believe that new technology could help the field. However, we think that at this time, continued investment of donor funds in our particular project is no longer the right choice and have suspended our activities in the sector.