Journal of Conventional Weapons Destruction

Volume 20
Issue 1 *The Journal of Conventional Weapons Destruction*

Article 3

April 2016

Technology Research in Mine Action: Enough is Enough

Russell Gasser

Photo courtesy of CISR.

Technology Research in Mine Action: Enough is Enough

by Russell Gasser

Twenty years ago I started work on a doctorate thesis asking the question: "Why has research into new technologies for mine action had so little success?"[1] My research discovered that about one billion dollars had been spent by the year 2000 on fundamental and applied research to produce new technologies to solve the mine problem.[1] The resulting benefit for humanitarian mine action was indeed very small. Since then, large-scale spending has continued with limited success. Researchers and their funders have not learned from continued, expensive failure. There is clear cause and effect at work, which means that many research projects and programs have followed a similar route to failure.

New technology has an important role in making mine action faster, safer, cheaper, or some useful combination of these three. Major gains to safety and/or productivity have resulted from the use of satellite and cell phones, GPS, digital cameras, laptops and tablet computers, map plotters, Google Earth mapping, polycarbonate for visors and Kevlar for protective vests and much more. However, none of these gains came from research into new technologies for mine action, they all came from adapting and applying useful, off-the-shelf products. These products could afford the high cost of research and development as they had a large-scale market.

Mutual misunderstanding between researchers and demining organizations began in the early 1990s when research into mine action technology started receiving large amounts of funding. Field practitioners in humanitarian demining wanted better tools and equipment as soon as possible and at affordable prices. Researchers offered to help but didn't manage to communicate that academic and industrial research is expensive and usually several years away from yielding finished products. Too many researchers did not understand why deminers were so reluctant to test unproven equipment in live minefields. Too often both sides felt let down by each other.

What researchers produce is usually several steps away from being usable in the field. Research results need to be turned into realistic prototypes that can be tested, which is the first step. Prototype tests then lead to a production design, and finally a production version that is first tested in simulation and then certified in live areas. However, this does not automatically mean the technology is going to be cost-effective or worth using, and each one of these development steps can cost more than the original research.

Researchers and their funders were highly motivated by what they saw as a moral obligation to focus their efforts on this humanitarian task. There was apparently a widespread assumption that there was no available means of clearing mines and that any advance—no matter how complex or costly—would be a step forward. In fact, manual demining methods were already well developed by the late 1980s. When properly managed, manual clearance was safe and reasonably cost-effective. My investigations showed that as much as 80 percent of the demining research aimed to improve the detection of buried mines, usually minimum metal mines, and ignored the majority of other urgent problems that field managers face. In the 1990s, a minority of researchers began to analyze the problem. The Development Technology Unit of the University of Warwick in the United Kingdom, where I was working, observed deminers in Cambodia from a safe distance. We discovered that they spent up to 70 percent of their time cutting vegetation. Clearly, the vegetation clearance problem was urgent and led to the Tempest mini-flail, locally produced in Phnom Penh.[2]

In terms of reducing the cost and time of returning land to productive use, area reduction (defining the boundaries of the area that has to be cleared) and the resulting release of land without clearance is probably the single most important issue. Although the topic was mentioned at conferences, only a few mine action field practitioners flagged this as an issue and researchers did not pick up the topic.[3]

Expensive research projects continued to produce marginal gains in mine detection by developing equipment suitable for use on flat ground without vegetation. In terms of pure research, this is the obvious path: start with the theory, develop the techniques, and gradually apply them to real world scenarios by developing prototypes to test. But this was not what the mine action world wanted. In 2016, as many countries approach the end of proactive mine clearance and are moving to management of residual contamination (MRC), the need for long-term research is becoming even harder to justify.

In the 1990s, there was a tango that went around and around but led nowhere. At meetings, researchers would ask "What are the key problems that we should be working on?" and field staff would reply, "What are the main areas where you can make a difference?" I remember one well-intentioned project where the researchers gave the mine action staff a long list of issues that the research could address and asked for prioritization. The response was that all the problems were "very important." Thus, no progress was made as no priorities were identified. Priorities cannot be determined by emotional appeal but instead need analysis and tough decisions. Even less common were cost-benefit analyses.

At times, the degree of separation between the research lab and the field led to multiple failures. One research project co-funded by the European Commission discovered that their lab equipment overheated and failed during field trials in Africa. Did their field partner not inform them about the hot weather because it seemed too obvious? Without a prior survey, the manufacturer of a large, mine clearance machine complained that Cambodia had the "wrong type of minefields" despite spending large amounts of donor money to have the machine transported.

Six Primary Reasons Why Mine Action Technology Research Has Yielded Few Results

1. There is a deep-seated psychological need to address the horror of stepping on an unseen, anti-personnel (AP) mine as the top priority. There is also the feeling of "just one more breakthrough and we will be there." Both of these tend to overrule rational analysis. This is not a research issue; fundraising also relies on the public response to the horror of AP mines. Unexploded ordnance kills and injures more people than AP mines, and unplanned explosions of munition stockpiles kill even more. However, the research proposals that seek to improve AP mine detection often focus on relatively uncommon minimum metal mines.

2. While researchers wanted to improve knowledge and its application, field practitioners usually thought the purpose of donor funding was to provide better tools and equipment in the short to medium term. Too much research focused on generating solutions to problems that were not clearly identified. In one case, a project that cost several million Euros of public money showed that the probability of detecting mines was reduced when the project's "data fusion" method was applied. In the project's final evaluation, a university professor declared that the project was a useful contribution in that it showed what did not work, which was true but did not immediately benefit deminers.

Whereas many researchers and donors want to focus on breakthrough technologies, demining needs incremental improvements to well-established methods and technologies. Dismissing incremental improvements because they are somehow less important is a serious error. Metal detectors are an example of a successful, incremental improvement; performance now is far better than it was 20 years ago; sensitivity, background compensation, size, weight and battery life have all significantly improved by manufacturers. Advanced and automatic data fusion methods for multi-sensor detection received millions of research money to seek a breakthrough but made little or no impact in the field.

3. There has been a widespread failure to understand the economics of humanitarian demining. There are two parts to this misunderstanding: the first involves the overall economic purpose of mine action whereas the second concerns the cost of going from lab research to a finished, usable product.

There may be no overall benefit from a modest reduction in clearance costs if the money is diverted away from the local economy in the mine-affected country and instead supports high-tech research in first-world countries. The purpose of mine action is to save lives, reduce injuries and help reestablish livelihoods postwar. Employing hundreds, or even thousands, of deminers is an effective way to stimulate the local economy. The effect multiplies and boosts recovery

"Metal detectors are an example of a successful, incremental improvement..."

efforts as money recirculates around the community, and local people start small businesses. If the objective of mine action is to rebuild war-torn economies and help local people, diverting resources to a rich country to pay for advanced technology in order to get a small gain on price per square meter makes no sense at all. Achieving the overall purpose of mine action is what matters; cost per square meter is only one part of this. Some new technology proposals have even threatened to drive up the cost of clearance. One such project received millions of Euros of public money and was based on detecting explosive using neutrons. The neutron generator required was very expensive, had a short life span and was so powerful it required registration by the user to comply with the Nuclear Non-Proliferation Treaty in force at the time.

The second economic issue is the gap caused by the amount of time and money that successful laboratory research needs to yield a certified product for the field. Transition is difficult, slow and expensive, and usually costs more than the original research.

The market for improved mine action technologies is small and insufficient for expensive commercial development. While I was project officer for new technologies at the European Commission in the early 2000s, many research funding proposals overestimated the potential sales of a future product and underestimated the cost of product development. A few projects predicted that the annual sales of their product would be worth more than the best estimate we had for the global budget for all humanitarian demining equipment worldwide.

4. Risk management has unexpected side effects. Most donors are not specialists and know little about mine action technology. To manage risk, they seek subject experts, who can make decisions on which projects to fund and how to evaluate progress. For some public sector donors, the use of these independent experts is a requirement. Available experts 20 years ago were usually academics with deep knowledge of the technology proposed or one of a group of recognized international mine action consultants who often had limited experience with military demining. It was difficult to recruit active field staff who comprehensively understood humanitarian mine action at the ground level; evaluating research proposals was widely viewed as a complete waste of time for field staff. The situation was exacerbated by the requirement of some agencies for consultants to have advanced university degrees. Non-specialist donors had no understanding of the enormous gap between the pool of available subject experts to decide on research proposals and the field practitioners who wanted better tools and equipment for immediate use in far-off lands.

Another effect of the dominance of military demining experience 20 years ago was large-scale funding for research projects focused on well-established military demining tasks. Some of these had little or no application to humanitarian demining. There was no intention that humanitarian funding should be used for military research, but at times that is what happened for some high-cost technologies later used for military purposes but not for humanitarian demining.

5. A number of high-profile research projects, often supported by internationally well-known people, have gained public support and leveraged large-scale funding. The projects proposed were often expensive and unfeasible (e.g., reliable, airborne detection of individual buried mines through vegetation; rolling heavy objects over uneven terrain in a random way without recording exactly where they passed), or were so expensive as to be entirely impractical for humanitarian purposes even if the technology worked. The publicity only mentioned the potential benefits, not the costs: "we have a responsibility to get these mines out of the ground and make the land safe for people to live a normal life without fear."[4] These projects not only wasted money but created a false public perception of demining and the role of mine action technology, and marginalized the demining organizations that they claimed to help. Moreover, they ignore the current solution: the properly trained and equipped human deminer.

6. Mine action practitioners have not always shown interest in the best research ideas and, at times have indiscriminately treated all research as equally lacking in value. For example, in 1999, a student research team discovered that oval, cross-section prodders (a cheap and simple tool) significantly reduced the force needed to prod into hard soil compared to normal, round-section prodders. Accidentally detonating mines while prodding in hard soil is a known source of accidents, so this simple, research-based advancement in technology could be expected to be widely used and well-publicized in the mine action community. The risk to deminers could be reduced by specifying oval prodders in operating procedures, contracts and mine action standards. However, the idea has not been widely embraced or shared. Is this the result of a "not invented here" attitude, or just poor communication of ideas?

Conclusion

As mine action in many countries moves from proactive clearance to reactive MRC, there is a real opportunity to improve the take-up and cost effectiveness of new technology. MRC is a well understood process with a long history of success, especially in northern Europe. There is already a wide range of commercial equipment, from simple hand tools to hi-tech systems, that is in daily use in countries still clearing explosive remnants from the two World Wars. There is no significant technology gap that prevents effective MRC from working in Europe.

Adapting existing techniques and solutions for use in new climates and areas without the supporting infrastructure found in Europe will naturally require some resources. However, we cannot possibly justify repetitive research and development in an effort to reinvent the wheel.

For proactive clearance, there are a lot of adaptive and ingenious solutions that have already been developed under field conditions or through appropriate research such as the oval-section prodders mentioned above, or the use of rakes. Many of these solutions are known only locally because they have not been published or shared. Busy field staff rarely have spare time, extra money or interest in the amount of work required to publish an article or attend a conference. An equipment catalogue that is more than a manufacturer's sales sheet is needed. Collecting and sharing information about inventive solutions to regional problems (as well as broader problems) is both urgently needed and far more cost and time effective than high-technology research. An online catalogue that includes photos, videos, interviews and information about actual results, including costs and benefits, would be a valuable resource. Translation is an essential requirement for accessibility, while constant maintenance and updating is necessary.

After the information is collected, it should be made available to people who can use it. This goes far beyond providing a website or a printed document, even beyond more accessible technology such as apps for smartphones and tablets. Sharing information must be an active process to identify, contact, interest and earn the trust of people who could benefit from the information. This is perhaps where research is needed. How do we get field managers, especially national staff, to take an interest in and put aside time to learn about technologies that could benefit their programs? The Croatian Mine Action Centre (CROMAC), United Nations Mine Action Service (UNMAS) and the Geneva International Centre for Humanitarian Demining (GICHD) organize mine action technology conferences, but at the last UNMAS/GICHD conference, out of more than 70 participants, fewer than 10 were national staff from mine affected countries. How can we encourage more people who will select the technologies needed for their country and approve equipment budgets to attend? Why is this not already a priority?

Mine action could learn from other areas where a community of practice has been established to support this type of technology transfer. Building a community of practice is not an easy task but would ensure that mine action technology moves forward in terms of cost effectiveness and deminer safety.

In addition, donors who are interested in funding mine action technology research would benefit from learning about the realities of technical needs, the low probability of getting past the research stage to a production prototype, and the need for cost-benefit and technical appraisal.

Perhaps the most important question to ask is why millions of dollars is available for research into technology that is unlikely to succeed whereas funding to develop and share solutions based on existing technology is sparse. This is the core question that needs to be answered if we are to learn from experience.

It's time to end the current situation where huge expenditures have achieved so little, and technology research continues to deliver poor value for money.

See endnotes page 66

The author wishes to thank Bob Keeley for his comments on the draft.

Russell Gasser

Russell Gasser
Tel: +41 (0) 22 518 0638; +44 (0) 113 403 2015
Email: rg@resultsbased.org
Website: www.resultsbased.org

Russell Gasser is an engineer who started working in mine action 20 years ago. He has been an official of the European Commission in Brussels, an independent evaluation consultant for eight years, and until recently was on the staff of the Geneva International Centre for Humanitarian Demining. His current focus is on results-based management, theories of change, and evidence based evaluation, as well as technology for mine action.