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# The Role of Research in Mine Action: A Response to Gasser

by Ian G. McLean, Ph.D. [ Consultant ] and Rebecca J. Sargisson, Ph.D. [ University of Waikato ]

Some years ago, a businessman came to me and said that he would like to start commercially farming lobsters, something that had not been attempted at the time. His key question was, “How long will the development research take?”

“I would think two to four years before a scaling-up exercise to make it commercially viable,” I answered.

Looking genuinely surprised he replied, “Really! I was thinking it should take about two weeks.”

This anecdote portrays a problem that emerges in almost every area of human enterprise. Those-who-do want and need to do right now. Those-who-create need time to design, build, and prove their creation. The time scales are fundamentally incompatible. Russell Gasser refers repeatedly to this problem in his editorial in issue 20.1 of *The Journal of Conventional Weapons Destruction*.<sup>1</sup> He also commits clearly to the side of the doer. His argument: the role of researchers (in mine action) is to service the needs of practitioners. They should do so quickly, cheaply, and with careful attention to the specific needs of the moment.

Gasser argues that incremental research (IR) is more beneficial than research into new technologies. As defined by Gasser, IR consists of improvements in design and/or productivity of equipment already in use. He points to the metal detector as a tool that was improved significantly over time by manufacturers through incremental improvements. Of course, it is unlikely that the manufacturers were improving their products specifically for mine action, which is only a small part of their market. Rather, mine action benefitted from developments that improved a tool for multiple applications. Those improvements would have emerged from research, albeit hidden from the view of operational demining.

Gasser states that much of the research in mine action has not directly benefitted the industry. However, researchers have broader goals than addressing specific and current problems.

For example, research on detection of landmines and unexploded ordnance (UXO) by animals may (or may not) benefit demining at the time, but the lessons learned from that research can cross over into other endeavors. Examples include mine detection rats re-tasked to detect tuberculosis, or dogs, which are well

known explosive and drug detectors that can also detect cancer, weeds, or bed bugs.<sup>2,3,4,5,6</sup> A key benefit of research is that, successful or not, it leads to unanticipated advances and further developments in the same or related fields. In this sense, all research is a process of “incremental improvement” and does not necessarily seek “breakthroughs” as Gasser claims.<sup>1</sup>

The issues with humanitarian mine action that Gasser describes from the 1990s were key reasons for establishing the Geneva International Centre for Humanitarian Demining (GICHD) in 1998. Humanitarian demining was an emerging profession, and there was considerable variation in the quality of the end product (mine-free land). Some technologies, such as mine detection dogs, needed to be better understood, and there were safety issues with human deminers. A combination of quality control through standards and research was needed to stabilize the industry and improve confidence.

The GICHD established itself as a research leader by employing professional researchers to manage projects and work with consultants. It ensured that the results of that research were communicated to the industry through multiple channels including written reports, face-to-face meetings, field-based conferences, and training videos. Most of Gasser’s examples refer to proposals and developments from more than a decade ago. Much of the international interest in the landmine issue in the 1990s and early 2000s can be attributed to Diana,



**“A key benefit of research is that, successful or not, it leads to unanticipated advances and further developments in the same or related fields.”**

Princess of Wales, and the signing of the *Anti-Personnel Mine Ban Convention* (APMBC). It is not surprising that developers of new technology that might (or might not) have a demining application seized the opportunity to seek funding for their projects. After all, research is an enterprise and is subject to the ebbs and flows of societal trends, just like any other business. However, most money budgeted by governments and donors for research must be spent on research. It is not appropriate to imply that the money spent on high-tech developments was somehow taken away from operational demining, as that money was never available to operations anyway. Competition for research funding is intense, and there were presumably opportunity costs in that some other research activity was not funded when the money went to a demining application. That alternative might not have had anything to do with demining.

Nor is it appropriate to suggest that the research was not beneficial, even if it did not result in a useful demining application. In his second point, Gasser states that research should improve equipment that is already in use. He refers to a failed project and notes that the limited learning from it did not benefit deminers. Innovative research is not a process with guaranteed outcomes, as it involves exploration of the unknown. Failure is frequent, and a good researcher will learn from that failure. The IR advocated by Gasser is cautious, limited in scope, and minimizes innovation. True, its outcomes are generally more assured and its failings less costly. But the reality is that both incremental and innovative research are needed if genuine advances are to be obtained.

In his fourth point, Gasser argues that the people who reviewed complex research proposals 20 years ago were either academics or military personnel, neither of whom understood humanitarian demining. The “wrong” projects were supported as a result. While there is some truth to the first part of the argument, he fails to link decisions of the past with the situation today, and we struggled to find any relevance to current realities in his arguments. The people who made those decisions no doubt believed they were the best decisions at the time. Retrospective criticism devalues their hard work without adding any value to future decision making.

However, Gasser is right that cynicism about the value of research was rampant in the industry at the time, and this editorial indicates that it has not waned. The deep gulf between practitioners and researchers captured in the anecdote at the beginning of this commentary is no more easily bridged today than it was in the past. Researchers struggle to find the time to promote the results of their work beyond standard reporting (papers and presentations) and tend to assume that role will

be taken up by others. The GICHD recognized that problem and invested significant resources into building that bridge, although with only partial success.

In his conclusion, Gasser asks for a community of practice and better promotion of low-tech ideas that were developed and implemented on a local level. The latter proposal appears to be similar to the equipment catalogues produced regularly by the GICHD since 2003 but with additional, non-commercial ideas from the field included. Realistically, it should be people who work for support organizations such as the GICHD who capture those ideas and transfer them elsewhere during their work programs. The notion of a community of practice was trialed in several ways by Håvard Bach over some years, but those initiatives foundered on the very realities of demining practice that Gasser describes.

Gasser ends with a question about funding discrepancies. We argue that the discrepancy is mostly a myth, as money spent on high-tech products was unlikely to have been available to demining practitioners. A likely explanation for the funding biases described by Gasser is that projects researching incremental improvements to standard demining tools were of relatively low cost. There are published descriptions of such research from the time of primary interest to Gasser.<sup>7,8</sup> If more was needed, we suspect that funding restrictions were not the reason for its absence. ©

See endnotes page ##

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