A Case for the Grand Challenge of Disaster Science

Tricia Wachtendorf

Abstract This work calls for the development of the field of disaster science. Specifically, it calls on those in the disaster research community to develop a grand vision for the field. This vision could include assembling the various disciplines that study disasters; examining large scale community and society disruption, dissembling, and destruction; and concerning itself with the social, technical, and environmental phenomenon that pertain to the causes and recognition of, as well as the reaction and adjustment to, various stages of that process. Incremental aspects of this effort already represent much of the actual work of interdisciplinary disaster researchers. The author provides an argument for why a rethinking of the field is important.

Keywords Disaster research · Grand challenges

Introduction

Consider for a moment a young girl or boy, barely out of kindergarten and just starting her or his educational journey. Full of dreams and brimming with imagination, the child is drawn to discussions of the solar system, dark matter, and of constellations. Perhaps the girl or boy has already started to read and can name scientists such as Einstein, Newton, and Galileo, whose contributions they can tell you have advanced our understanding of the science of space. The child may already, in the early grades of elementary school, articulate a desire to pursue space science as a career path. They may express an interest in the possibility of space travel, or colonization of Mars, or how to better understand the formation of the universe. In those formative years, math and physics may dominate their attention, but later a greater appreciation of fields such as chemistry, engineering, psychology, and sociology may emerge (as well as botany, if they so happen to view the 2016 film *The Martian*).

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It is possible that the child's sibling, meanwhile, has become fascinated by the allure of marine science, and wants to pursue questions that draw upon the disciplinary insights of many fields to advance a more holistic understanding of oceans and the life that relies on them: the mysteries of the deep. These young children are easily persuaded on the greatness of such big questions as: How does the expanse of the universe or the depth of the ocean operate? How do we protect our oceans or traverse outer-space? How will a greater understanding through scientific inquiry help us better understand the world, or universe, around us? What secrets remain that are yet to be discovered?

Less common in those first, second and third grade classrooms around the United States (and, potentially, in countries across the world) are children bustling with a desire to pursue disaster science. It is not that they are left disinterested by the power of tornados, or the stories of Pompeii, or accounts of the latest hurricane to strike the nation's coastline. It is not that they lack engagement when participating in volcano experiments at school or at shake-table exhibits at science museums. What is less common, however, than those young students captivated by space or marine sciences is a sense that there is a field that captures bigger questions meriting a more ambitious scientific inquiry. We cannot fault this perspective in the very youngest among us, for – as I will argue here – scholars have yet to fully articulate what a field of disaster science is. Our own imagination has been stifled with important, but incremental, disciplinary advances.

Running in Circles

Let me take a critical look at one of my own research pursuits: materiel convergence and humanitarian logistics. We know that unsolicited donations collected by informal donation-drives generate extreme challenges for disaster response logistics (Holguin-Veras et al. 2007, 2014; Wachtendorf et al. 2015). They are often inefficient and expensive compared to organized formal efforts sometimes supported with financial donations (Holguin-Veras et al. 2013; Holguin-Veras et al. 2016). We have fine-tuned these results over the years, placing them in context, but they are not fundamentally different from the findings of Fritz and Mathewson (1957) over a half century ago. We do know more about the strengths of different sectors in the acquisition and distribution process (Holguin-Veras et al. 2012). We know more about how challenges are different depending on whether the event more closely resembles an emergency, disaster, or catastrophe (Wachtendorf et al. 2013). We know more about the motivations of donors and those orchestrating donation drives, and why simply telling these individuals not to engage in the problem-generating aspects of disaster relief will not generate the fundamental change in behavior for which we hope (Penta et al. 2015). Our own recent research on this topic has involved social scientists, engineers, and computer scientists, each area representing multiple disciplines and subfields. That, too, has diversified the perspective. But, despite extensive dissemination of results in scholarly and practice domains, the

problems noted continue – arguably made more complicated by social media as people share misinformation for months after the initial postings asking for assistance were made.

As a society, we have systematic research to back up certain recommendations, yet we continue to fall short in instituting change, either in human behavior or in the systemic response that should take such behavior into account. The public continues to direct personal and materiel resources to ineffective activities at the expense of others. As a society interested in helping, we insist, upon all evidence to the contrary, in continuing down a particular path.

For the past half century, sociologists who study disasters have asserted that widespread panic in these situations is rare (see Fischer 1994 for an overview of the findings). Even in light of systematic research, officials consider that sharing information will cause panic. Although media may offer descriptions of chaos and panic in the immediate post-disaster environment, systematic research points to alternative findings. Individual cases of irrational behavior are possible, although not typical in the broader impacted population. Anti-social behavior closely associated with panic is not usually seen until the very moments before a window of opportunity for escape closes. If people do not trust the information they receive, they may engage in behavior that contradicts formal guidance. It is actually quite difficult to get people to break out of their normal behavior: rather than running around in panic they more often engage in milling behavior and cling to routine behavior as if a disaster were not unfolding around them. At least two fundamental problems are apparent here. First, over 50 years of systematic research has failed to substantially alter public perceptions about disaster or shift official thinking so as to adjust their practice of disaster management. Second, unlike Newton's law of gravity outlining that in a vacuum two objects falling within the same gravitational conditions will fall at the same speed regardless of weight, the behavioral phenomena of panic are so contextual, including so many caveats, that it is easy to dismiss them.

Take another example: Scholars know that segments of the community marginalized in the pre-disaster environment will, in all likelihood, continue to experience marginalization when disaster strikes, and that the situation may, indeed, become exacerbated post-event (see Thomas et al. 2013 for an extensive overview). The classic approach saw disasters as generated from forces solely outside the social system. As the field developed over the course of the twentieth century, the social science literature began a reframing of the agents of disasters (Perry 2006). The vulnerability perspective more clearly articulated the drivers internal to the social system that generate or exacerbate disastrous outcomes. It also recognizes and validates the strengths and capacities of even the most marginalized of the community. But, in the decades since this reframing, marginalization continues. Research points to it in almost every single disaster event. Yet we devote our attention to arguments about terminology and definitions (Are people vulnerable? Are they victims or survivors? Should we highlight special needs? Or does everyone have a special need? Is resilience a thing? Or is the term passé now that it was adopted by government?). Although there is merit to those discussions, and using a common language can improve our ability to enhance knowledge, we do little to advance the science if we

stop there. Consider the language around the critical issue of ensuring that people with disabilities are not differentially at risk to the consequences of disasters (Davis et al. 2013). In the United States, using the term *functional and access needs* is quite inclusive and broad in its application. Using the term *disability* invokes the Americans with Disabilities Act of 1990, which places the discourse as a rights-based issue. *Special needs* is ambiguous and, like *functional and access needs,* it carries no legal weight (Davis et al. p.208). Whether we talk about functional and access needs or disability is a starting point, but that terminology discussion should not be an obstacle to addressing the our failures to rethink our social environment where segments of our society are triaged out of our response and recovery efforts.

Instead of working toward that rethinking in an ambitious way, we write that yet another marginalized segment of the community is less studied than others. And moreover, despite such observations, we have seen little more than incremental improvements in the actual disaster response domain. If you don't believe that assertion, read the next set of articles after the next disaster that continue to point to the negative outcomes of institutionalized populations, such as those in nursing homes or prisons, to the lack of attention to childcare post-disaster, the fact that we should not ignore peoples' desire for the well-being of animals under their care – even in catastrophic times, or to the differential treatment of the homeless after an event. Perhaps practices in some locations have improved, but we have not yet launched our metaphoric un-manned rocket, let alone set a course for the moon. The young third grader will not be captivated by a discussion about the value of the term resilience, and if it is a factor or an outcome. Honestly, most scholars quickly lose interest, as well.

The Need for Disaster Science

We need disciplinary advances, we need interdisciplinary collaboration that harness those advances, and we need translational research that can improve disaster management practice. But it is time for a field of disaster science that assembles the various disciplines that study disasters; that examines large scale community and society disruption, dissembling, and destruction; and is concerned with the social, technical, and environmental phenomenon that pertain to the causes and recognition of, as well as the reaction and adjustment to, various stages of that process. To be clear, advances in the broader field of disaster science benefit from continued advancements brought about by advancements along disciplinary lines. The depth of knowledge that comes with concentrated study of structural engineering, hydrology, sociology or geography – to list a few – has greatly enhanced knowledge about the intersection of the human, built, and natural environment. Research on disasters can be congratulated for its decades of multidisciplinary collaboration, sometimes – though not always – venturing into true interdisciplinary pursuits. Some of this research is enabled by multi-million dollar calls for proposals from federal funding agencies and foundations. Although once parceled out amongst several disciplines

and institutions, little remains for disciplinary innovation. As we rightfully departed from our disciplinary silos, our disciplinary innovation suffered without significant gains in interdisciplinary transformation. Many disaster scholars are drawn to this field because they want to make a difference, because they really desire to improve the well-being of the world's citizens. Their efforts to disseminate results in non-academic forums are testament to those efforts, and are commendable. Yet as we rightfully engaged with each other on practical concerns of emergency management, we inadvertently lost sight of the grand questions of the disaster universe. Our desire to contribute to the practical questions at hand left us with little vision.

Let us return, for a moment to the perspectives on disaster. In the 1980s and 1990s, disaster scholars increasingly argued that – contrary to early definitions that attributed cause of disaster forces external to a social system – disasters are less concentrated in time and space than generated by forces internal to the social system, and that those forces are often persistent over extended time periods. As an illustration, such assertions may claim that the real precipitators of disaster lie in a society's decades-long privileging of economic development over regulation, or concentrated and accumulated wealth over poverty alleviation and equitable wealth distribution, and not primarily the sudden and severe movement of tectonic plates or the development of a significant tropical weather system. Yet many of the solutions we offer to focus on the reaction of the populace and emergency management decision-makers to periodic events. This is equally true of studies that determine the necessary structural improvements required for a building to withstand shake as it is for the studies that point to the disproportionate vulnerability of particular segments of the society when the community functions fail. Let us be clear, both of these studies are valuable and provide critical insight that have implications for reducing human harm and suffering. That said, what of our ability to recognize, to appreciate, and to fully understand the slowly unfolding disaster that we may be in? If we actually do agree that disasters are internal to the social system, the important incremental questions are not enough.

What is missing from our field of disaster science are immense, glorious questions – ambitious objectives, of the kind the space program demanded. Rather than asking how to get a person to the moon and back, our questions in disaster science have the potential to speak to the fundamental questions of human survival. How do we recognize disaster is imminent? What do we do in this midst of disaster – as our world is falling apart – to set the right course? What do we do to set society right when it *has* fallen apart? How do we imagine disaster, even when we cannot seem to see it right in front of us? How do we survive, and equitably thrive? Questions such as these inspire. They have the potential to catapult the interdisciplinary endeavor of disaster science in remarkable ways. The fundamental disciplinary studies are critical components, but without feeding into more ambitious aspirations we risk those incremental advances getting lost in disciplinary discourse, in pursuit of academic journal impact factor ratings, or in lone devotion to the localized processes of emergency management.

There are other ways we have fallen short in grand ambitions. Large-scaled disasters and events catastrophic in nature do not happen often – thankfully – so we

have concentrated our attention on more routine events, such as emergencies or common – albeit tragic – disasters. One of the shortcomings of this approach is that we presume that all that is required to contend with catastrophe is a scaling up of what we know. We acknowledge the assertions of Enrico L. Quarantelli (2006), but our work then again returns to our standard questions and approaches. Scholars, driven by external funding practicalities, have often looked to the suffering most proximate to them. In the United States, this has meant a focus on events of smaller scale, limiting our imagination of how bad things can really be, even when we look to catastrophic events outside our national sphere.

The emergence of the field in the United State was heavily influenced by sociologists from or training in the tradition of the Second School of Chicago. These sociologists were drawn to questions related to collective behavior phenomenon: convergence, crowd behavior, micro-level interaction. These topics included the absence of wide-spread panic and looting, the contextual processes associated with the former, evacuation and warning behavior, the importance of emergent activity in considering routine organization and institutional frameworks. This should have formed the basis, but not the drive. Understanding how protons, electrons, and neutrons work is important, but no one assumes we stop there in our endeavor to understand the universe. Milling, keynoting, and rumor are important, but we need to go further. Our imagination needs to expand.

The development of the field in the United States, a dominant leader in disaster social science scholarship, has blinded the field to contributions from non-US scholars who have arguably done a better job at including creeping disasters such as famine and armed conflict events into their work. Even one of the key pioneers of the field, Russell R. Dynes, identified the need for such a shift in the early 2000s (Dynes 2004). Again, the funding mechanisms in the United States have pushed us in our insular direction. The early work by disaster scholars, conducted during the mid-twentieth century, focused on the use of disasters as social occasions to understand the threat of an attack on the country. Although the researchers had interests beyond the attack scenario, it certainly pushed them to ask questions in alignment with this focus. We became disaster researchers, missing the potential of disaster science. The calling to grapple scientifically with the grand challenges of disaster reaches to a desire to better understand the social condition.

An Idea Whose Time Has Come

How do you get something off the ground into space and get it back again, with a human on board? For that, one needs to know physics, chemical reactions, influence of the process on the body, psychological well-being. Space science involves any of the scientific fields concerned with space travel or phenomena occurring in space, including other planets. And the scientific inspiration began many centuries before twentieth century space travel became a remote possibility. Then let us consider war ravaged Allepo, Syria in 2017, or a naturally induced agent causing a similar level

of destruction. How does one reorganize, survive, rise again from the ashes? The dominant questions are social scientific, best supported by expertise in the natural, physical, and engineering sciences. Perhaps for that reason, disaster science has not gained the same traction as our comparative interdisciplinary sciences. It could be that progress in space, marine or health sciences were at least partially a consequence of the dominance of natural science fields in these areas.

How do you take a place destroyed and build back in a thriving equitable way? We don't know the answer to that question. But a bigger question could be how do you get people to notice that conditions have changed, that they are no longer acting in what they understand as "normal," and then how do you get them to figure out correctly what that new normal is? The answers lie with the integration of not only the social, engineering, and geological, and atmospheric science, it demands the participation of public health, the humanities, and the arts.

Can we not take a cue from disaster movies? The blockbusters, as rife as they are with errors and misconceptions, capture the idea of transcending the chaos of disaster. Science fiction, be it in literature or on the big screen, was able to thrust our space travel ambitions from the imagination to reality, and it continues to move us to this day.

It is time the disaster science community consider if we have the right institutions to figure out what our "moon shot" really is. It is time to reconsider the way we fund disaster research. The solution, I would argue, is not solved by a Center of Excellence mechanism – concentrating projects within a collaboration of individually underfunded projects vulnerable to the whims of political mandates. It is not solved by only directing money to the problem, although financial support is essential, particularly when funding and publication is so contingent on citing the right person that we are confined in our thinking. No, rather, the community needs to demand a mission that ambitiously defines our next reach. That inspires us to pull our evidence-based findings towards a grand objective. That captivates our imagination and drives us to reconsider the interaction between human, built, and natural words.

Often research is generating data to support what we know or intuit, but the data is necessary to prove it to others. Again, laudable, but we need more. We need freshness, innovation, and inspiration. We need a national – or global – effort to solve the challenge of disaster. We will not prevent the next hurricane or earthquake. Indeed, our human activities will likely generate new hazards, be it through climate change, fracking, or oil exploration. What are we going to do to better adapt to our dynamic and hazardous world? To do so demands a science of managing the paradoxes of change and continuity, of uncertainty and planning, of disruption and stability.

So to a newcomer to the field, I say this. The practical questions disaster scientists should grapple with cannot be restricted to the very important dilemmas of how to encourage those under hurricane evacuation order to evacuate. Disaster scientists should use as their guiding vision the most ambitious, awe-inspiring questions, the most ambitious of goals. We should endeavor to know how we prioritize future risk over imminent risk. We should have a clear sense of, when all seems lost, how we form society again. Do not take incremental steps without having a clear idea of the larger objective. Think big.

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