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# WHAT IS A DISASTER?

Perspectives on the question

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## INTRODUCTION

The basic question, its importance, and how it  
is addressed in this volume

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What is a disaster? That is the question that this volume addresses. A dozen answers are posed by researchers from six different social science disciplines and from half a dozen different societies.

The background of this particular quest is as follows. From my earliest involvement with research in the area initiated more than four decades ago, I have struggled with how to define and conceptualize the term "disaster." This concern has provoked me at different times through the years to advance various conceptions and to analyze what others were proposing (as examples, see Quarantelli 1966, 1977, 1985b, 1987a, 1987b, 1989a, 1989b, 1992a, 1993a, 1993b, 1994). Our starting point was the varying ideas set forth in the earliest days of systematic disaster research in the social sciences, in the 1950s and 1960s. Among the more relevant ideas were those expressed by Endleman (1952), Powell, Rayner and Finesinger (1952), Killian (1954), Williams (1954), Moore (1956), Fritz (1961), Barton (1963, which preceded his later better known work, 1969), and Stoddard (1968). There were also some earlier ideas expressed by Carr (1932) and Sorokin (1942), while known to a few, were very seldom explicitly acknowledged by most of the pioneers in disaster studies.

At one point I even traced historically some of the earlier discussions and formulations of the term. In particular, I noted a move from the use of a label with a referent to primarily a physical agent to one which mostly emphasized social features of the occasion (see Quarantelli 1982). However, while the indicated writings as a whole represented initial worthwhile clarifications of the problem and a movement in the right direction, they still fell far short in my view of what was needed, especially for social science research purposes. In fact, as I evaluated the field about five years ago, it did not appear to me that the overall situation had materially changed that much since my earlier analysis about a decade earlier.

However, in 1992, the International Institute of Sociology requested

papers for its Congress to be held at the Sorbonne in Paris in June 1993. I seized upon this opportunity to propose a session on the concept of "disaster." Since European scholars had seemed consistently and proportionately more interested in the problem than their American counterparts (e.g. Westgate and O'Keefe 1976; Clausen, Conlon, Jager and Metreveli 1978; Ball 1979; Dombrowsky 1981; Pelanda 1982a, 1982b; Schorr 1987), I asked a number of them to prepare papers for the meeting. In very general terms, they were asked to put together a statement on how they thought the term "disaster" should be conceptualized for *social science research purposes*, and to indicate at the same time their questioning and/or criticizing of past and existing conceptions. The session was entitled: "Disasters: Different Social Constructs of the Concept."

Some of those invited could not participate, but eventually I selected five papers, four by Europeans and one by an American. At the meeting itself, four papers were presented: by the sociologist, Dombrowsky, from Germany; by the political scientist, Gilbert, from France; by Horlick-Jones from Great Britain, at that time in the Department of Geography at the London School of Economics; and by Gary Kreps, a sociologist from the United States. I particularly asked Kreps because he has been one of the few Americans who, throughout his professional career, has explicitly expressed an interest in the question of how to define and conceptualize "disaster" (e.g. Kreps 1978, 1983, 1984). Porfiriev from Russia, with a doctoral degree in economics, was unable to come to the meeting as he had intended, but later, after reading the other four papers, also wrote a manuscript.

As coordinator of the roundtable, I did not see it as a proper role for me to present my own point of view at that time on the definitional/conceptual problem (although I did so later in a paper presented at the World Congress of Sociology in 1994 (see Quarantelli 1995b)). However, to understand the background for the papers that were presented, it does seem appropriate for me here to restate in summary form my opening remarks to the roundtable. This is what I said.

Why did my proposal about the roundtable come when it did? Because in my view it is time after nearly half a century of fairly extensive empirical disaster research, to systematically address the central concept of the field. In the early stages of the development of any scientific area it matters little what researchers do and explore. Almost anything empirically found is worthwhile discovering. However, studies in the sociology of knowledge suggest that after a certain period of pioneering work, a developing field will flounder unless there emerges some rough consensus about its central concept(s). Thus, my view is that unless the field of disaster research comes to more agreement about what a disaster is, the area will intellectually stagnate (in another recent paper, I expound more on this thesis, see Quarantelli 1995b).

Empirical work of course could and would continue, but without the intellectual infrastructure and scholarly apparatus any field needs (by way of

explicit models, theories, hypotheses, etc., all of which however require addressing conceptual issues), the research will continue at best to produce only low-level empirical observations and findings, without any significant accumulation of systematic knowledge. (The research funding in most societies unfortunately tends to support very strongly only such low-level applied work rather than more abstract basic research.)

The term "disaster" is certainly the key concept in the area. Yet even what is assumed in the subtitle of the roundtable—namely, *different social constructs of the concept*, is not fully agreed upon or used. Many in the field assume there are physical happenings out there, independent of human action in any sense (e.g. most, although not all, geographers assume that to have a disaster there must be the physical presence of a hazard, i.e. an earthquake, flood, cyclone, etc.). If workers in the area do not even agree on whether a "disaster" is fundamentally a social construction or a physical happening, clearly the field has intellectual problems. (I might note that even formulations by sociologists and other social scientists that appear to be fully social constructs, but which use geographical space and/or chronological time as dimensions or factors in defining a "disaster," in my view, are not using fully social construction concepts. As I will discuss in the final chapter, they should use social space and social time features, ideas well developed by some sociologists since Sorokin and Merton long ago urged their use.) So a major reason we need clarification is because otherwise scholars who think they are communicating with one another are really talking of somewhat different phenomena. A minimum rough consensus on the central referent of the term "disaster" is necessary.

In saying this, I should stress that in no way am I arguing for agreement on one single, all purpose, view of disaster. As noted even in some of my writings of several decades ago, for legal, operational, and different organizational purposes, there is a need for and there necessarily will always continue to be different definitions/conceptions of when a "disaster" is present. I have no problem with such different views about what constitutes a "disaster."

However, in my view, for *research* purposes aimed at developing a theoretical superstructure for the field, we need greater clarity and relative consensus. For example, some disputes about what appear to be empirical findings mostly stem from different usages of the basic concept in the field. For instance, whether there are serious negative mental health consequences of X (disasters) is hotly disputed, because some of the researchers use such a broad referent that any type of individual or group stress situation is seen as a "disaster" (see Quarantelli 1985a). Of course, the broader the referent the more likely it will be that one will find any given phenomena. Some of us who use a narrower referent for "disasters" exclude a wide range of stress situations, such as conflict situations (e.g. war, imprisonment in concentration or military camps, terrorist attacks, riots and civil disturbances, hostage takings, etc.), where we would not disagree there could be severe negative

mental health consequences. Thus, the dispute in this case is not mostly about the empirical findings, but the referent of the basic term "disaster." The same can be said about antisocial behavior. If conflict situations are treated as "disasters," by definition one will have antisocial behavior. That conclusion results not from empirical findings, at least at one level, but from the definitional referent of the basic term "disaster."

So to be concerned about what is meant by the term "disaster" is not to engage in some useless or pointless academic exercise. It is instead to focus in a fundamental way on what should be considered important and significant in what we find to be the characteristics of the phenomena, the conditions that lead to them, and the consequences that result. In short, unless we clarify and obtain minimum consensus on the defining features per se, we will continue to talk past one another on the characteristics, conditions and consequences of disasters. In concrete historical terms, would all of us agree or not agree that what is currently happening in Somalia and in Bosnia should be treated as "disasters?" From previous discussion among us, we know that we do not fully agree on the answer to that question. There is not agreement because we have different ideas of what should be the defining features of the basic concept of the field.

Finally, as a last point, I would mention that we require clarification because we are also at the threshold of the appearance of certain kinds of relatively new social happenings that will need to be either included or excluded from the rubric of "disaster." Examples are such phenomena as the AIDS epidemic, computer and high tech network failures, biogenetic engineering mishaps, and accidental as well as deliberate large-scale food and drink poisonings and contaminations. What is noticeable about these and similar happenings are that they usually occur independent of particular local communities, can be characterized in terms of social space and time, and often do not result in any or many sudden fatalities/casualties or significant property damage. Yet, they create major social disruptions, are economically costly, and can be very psychologically disturbing. Or, to use a more familiar example, was the Three Mile Island nuclear plant accident a "disaster" in the same sense as was Chernobyl?

Some see the former as not a disaster at all, given the absence of casualties from the occasion, while others such as myself see it as a harbinger of future "disasters." Then there are also a myriad number of happenings frequently captured under the label of "industrial crises" which only partly overlap at best what more traditional "disaster" researchers study (see Mitroff and Kilmann 1984; Shrivastava, Mitroff, Miller and Miglani 1988). Which, if any, of these happenings should also be in the bailiwick of disaster researchers?

The above are paraphrased comments on what I said in opening the roundtable at the Sorbonne. There was no anticipation on my part that the ensuing discussion would result in consensus even among the four researchers involved. This expectation proved correct.

However, the papers and the discussion at the session led me to think it might be worthwhile to try not only to continue but to expand a dialogue among those scholars interested in the topic. This was reinforced by an encounter at a professional meeting in Mexico with Ken Hewitt, a Canadian geographer. In 1983 he had written one of the most detailed and explicit criticisms of the field of disaster studies up to that time. He raised, in my view, very trenchant questions about the definitions and conceptions of disasters that had been and were being used in the social science literature. I proposed to Hewitt that he write a reaction paper to the other five papers.

The outcome of all this was presented in a special issue (that of November 1995) of the *International Journal of Mass Emergencies and Disasters* (IJMED). The issue consisted of the original five papers (with the ones by Horlick-Jones and Kreps being updated since their oral presentation in Paris), and the reaction paper by Hewitt. In addition, I asked each of the original five authors to write a short reaction paper to what Hewitt had said about their own individual contributions. The format used of having original papers, a general reaction paper, and reactions paper to the general reaction paper was my effort to generate something of a dialogue between the scholars involved.

As might be expected, some authors in their papers were circumscribed in explicitly criticizing in any way the views of others. However, fulfilling my hope, some did specifically, directly, and candidly address what others had written. The purpose, of course, was not to generate conflict but to force the writers to more explicitly expand on their own positions.

Even at that time I was aware that there were important and different points of view regarding the conceptualization problem that were not clearly represented in the special issue. Dynes, for example, had advanced a new and novel perspective on the whole problem (see 1994a, 1994b). In addition, there were others, such as Rosenthal and his colleagues (1989a, 1989b), Oliver-Smith (1993) and Mitchell (1990, 1993), who had written statements that were only partly reflected in the journal issue.

This led me to the idea to have a second round of papers that would follow the same format as the first round that appeared in the journal issue. I asked five other prominent disaster researchers (Dynes, Kroll-Smith who added a co-author in Gunter, Oliver-Smith, Rosenthal, Stallings) also to write original essays, and to include their views about the writings in the first round. In turn, doing the same as Hewitt had done, Ronald Perry, who had accepted my invitation to do so, wrote a reaction paper on the five new papers. My choice of Perry was strongly influenced not only by the excellence of his research in the disaster area, but also by my awareness of his encyclopedic knowledge of many theoretical and methodological issues in sociology and related social science disciplines. Again, paralleling what had been done before in the first round, each of the authors also wrote a brief reaction note to Perry's article. All of these appear in this volume. The exception is the article by Horlick-Jones that I dropped from the collection.

In addition, there are (revised) introductory and (original) epilogue chapters by myself. All the separate references in each of the papers have been consolidated into one master bibliography included at the end of the volume.

My intent was to obtain as many different disciplinary perspectives from the social sciences as possible. I was successful in obtaining contributions from scholars whose backgrounds were in anthropology, economics, geography, political science and public administration, and social psychology and sociology. Psychology is the only major field not represented, but then explicit and extended theoretical discussions of what is a disaster—although not absent—are not common in that discipline (most theoretical discussions are only peripheral in connection with larger discussions of disaster-related mental health problems; for example, see Berren, Beigel and Ghertner 1980; Wright, Ursano, Bartone and Ingraham 1990). Now, whether the provided contributions truly and fully represent the indicated disciplines is a question others might want to consider. However, I would argue that at least part of the intellectual dialogue that took place actually reflects different views both within and between disciplines on how disasters should be conceptualized.

To a degree, the authors are from those countries where the greatest amount of social science studies of disasters are currently being undertaken. The major exception is Japan and a few developing societies where studies have accelerated in recent years. Unfortunately, my efforts to obtain contributions from these places did not work out. However, to maintain proper perspective it should be noted that while there is much Japanese empirical research, theoretical work is not prominent in that country, and the great majority of studies in developing societies are of a very applied nature. Nevertheless, in my concluding chapter, in which I make some suggestions as to where the field of disaster research ought to go in the future, I do indicate why work from developing countries might soon be a very good starting point for suggesting a radically different or revolutionary view on the question of what is a disaster.

My choice of different disciplinary backgrounds and national scientific circles was an attempt to mirror in some crude way the current research setting in the disaster area. To the extent that my rough sampling was anywhere near valid, the papers written for this volume provide a gross reflection of how social science researchers think about disasters. Or, perhaps more accurately, the articles in the rest of this volume reflect the perspectives of those in the field who have most explicitly and consciously thought about the central concept of the field.

Finally, it should be noted that the first set of authors who wrote for the IJMED journal were asked to react primarily to the basic question: What is a disaster? On the other hand, the second set of authors were asked to indicate their positions not only about the basic concept, but also additionally what they thought about what the original five authors had written. As such, the last set of authors wrote whatever they did within a larger reactive

framework, and therefore as a whole tended somewhat to discuss broader issues in their articles than those who had only originally written their papers for the IJMED. Thus, such differences as are manifested in the number of matters addressed are more a function of the two formats within which each set of authors had to write.