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DISASTER STUDIES: THE CONSEQUENCES OF THE
HISTORICAL USE OF A SOCIOLOGICAL APPROACH
IN THE DEVELOPMENT OF RESEARCH*

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**Disaster Studies: The Consequences of the Historical Use of a
Sociological Approach in the Development of Research***

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An earlier article discussing the initial days of disaster studies, noted that the roots of the area in the applied concerns of research funders led to a pattern of how research was done and what was studied that still prevails today. However, this paper stresses that a certain sociological orientation and particular sociological ideas implicitly came to permeate much of the early work and many of the observations and findings made. We also indicate that the research approach, initiated with a mixture of applied concerns and basic sociological questions, has had up to now primarily functional consequences on the development of the field of study of disasters. But the paper concludes with a statement that the field currently needs a fundamental reconceptualization of disaster. It is argued that the impetus for that is more likely to come out of a questioning of basic ideas as well as the growing internationalization of disaster studies than from practical concerns.

Introduction

In an earlier paper (Quarantelli 1987a) we discussed at length: (1) that the initial roots of research on the human and social aspects of disasters was in rather narrowly focused applied questions or practical concerns, and (2) that there were certain kinds of selective emphases in terms of what and how the research was undertaken with subsequent consequences which we still can see are operative in which is done by most current researchers.

In this article we elaborate the third and fourth points that were briefly mentioned in the first paper, namely, (3) that a certain basic sociological orientation and particular sociological ideas implicitly permeated much of the early research work and many of the answers that were offered, and (4)

* This paper is a companion piece to an earlier one entitled "Disaster Studies: An Analysis of the Social Historical Factors Affecting the Development of Research in the Area" which appeared in a November 1987 issue of this journal. Both papers are primarily drawn from a manuscript prepared for the Symposium on Social Structure and Disaster: Conception and Management held at the College of William and Mary, Williamsburg, Virginia, May 16, 1986.

that the research approach initiated with a mixture of applied concerns and basic sociological questions and continued now for about 40 years has had primarily positive functional consequences on the development of the field of study of disasters.

We conclude with a brief statement on what all this might portend for the future. Basically we take the position that there is little reason to think that any major orientational change in the short run approach to the social science study of disasters is likely to occur in the United States. But there is a need, as well as a potential, for some significant changes of research orientation in the long run. A fundamental reconceptualization of the phenomena being studied is necessary if a qualitative improvement in the research and its application is to happen. Furthermore, our view is that the impetus is more likely to come out of a consideration of basic questions reinforced by the progressive internationalization of disaster studies, than from practical concerns.

The Sociological Influence

As described in our earlier paper, there were three pioneer field teams that initiated disaster studies; the well known one starting in 1949 at the National Opinion Research Center (NORC) at the University of Chicago (Marks and Fritz 1954), and the other two at the University of Oklahoma (Logan et al. 1952), and at the University of Maryland (Powell 1954b), both also starting in the very early 1950s. All these research efforts were sponsored by U.S. military agencies. However, while sponsor interest was in extrapolating from civilian disasters to wartime situations, the social scientific background of the researchers had a major and decisive (but rather different) influence in what was studied and how the research was conducted. In fact, very little if anything was produced that met the wartime interests of the research sponsors.

Several factors affected this deflection of the research effort even in the early 1950s. For one, it is important to note that sociologists predominated simply in terms of the numbers of researchers and in the holding of key administrative research positions. Both the first nominal head of the NORC project (Shirley Star) and the actual everyday operational leader (Charles Fritz) were sociologists. The rest of the professional staff (which at times numbered several dozen part time personnel) were from a variety of disciplines, but all five researchers (on a full time basis) who played a major role in the analysis of the Arkansas tornado data, the key piece of research undertaken (Eli Marks and Fritz 1954) were sociologists (Rue Bucher,

Robert Endelman, Leonard Schatzman, and E. L. Quarantelli), with one exception (Dotsie Earle who was a psychologist by training).

The University of Oklahoma study was formally headed by two sociologists (Leonard Logan and Wyatt Marrs) with the key field team researcher being another sociologist (Lewis Killian). Only in the Maryland study were sociologists not represented; the key person there was a social philosopher rather than an empirical researcher but who nonetheless made research contributions (Powell 1954b).

Apart from the pioneering groups, the great majority of other researchers who did disaster studies in the middle and late 1950s were also sociologists. These included Otto Larsen (1954), Roy Clifford (1955), Hiram Friedsam (1957), and Fred Crawford (1957) who with Fogelman (1958) and particularly Fred Bates (1963) worked together intensively with Harry Moore (1954)—another major pioneer figure in the area in the late 1950s; see also Irving Krauss (1955), Arthur Prell (1955), Irving Rosow (1955), W.E. Bock and Jean Bock (1956), Arturo DeHoyos (1956), Samuel Klausner and Harry Kincaid (1956), Albert Foley (1957), William Form, Gregory Stone and C. M. Westie (1953) later joined by S. Nosow (1958), Nall (1956), Peter New (1957) along with Irving Deutscher (1961), Seymour Weisman (1958), Schatzman (1960) Ellwyn Stoddard (1968) and Meda White (1962) all of whom were sociologists. In fact, only an occasional isolated anthropologist (Belshaw 1951, Wallace 1956, Schneider 1957) or psychologist (Janis 1949, Hudson 1952) were exceptions to the overwhelming presence of sociologists in the field study of disasters in the 1950s (see our later discussion about the work originated by geographers on natural hazards rather than disasters). Many published in mainstream sociological journals (Killian 1952; Quarantelli 1954, 1960; Moore 1956; Form and Loomis 1956; Bucher 1957; Fogelman and Parenton 1959).

The subsequent National Academy of Sciences (NAS) work starting in the late 1950s and terminating in the early 1960s, under the label of a Committee on Disaster Studies and its later successor, the Disaster Research Group, was always headed by sociologists. In fact, for almost all of its productive period, the two key persons were the sociologists, Harry Williams and Charles Fritz (the first sociologist ever employed on a full time basis by the NAS). The Group supported research and disaster related activities by outside scholars in a variety of disciplines, but again those with a background in sociology predominated. For example, of the 19 major disaster publications issued by the NAS, 13 were authored or co-authored by sociologists such as Allan Barton, Bates, Killian and Moore (and three others were written by anthropologists).

Finally, the Disaster Research Center (DRC) established in 1963 was founded by three sociologists (Russell Dynes, Eugene Haas and E. L. Quarantelli), administrated through a sociology department, and in its early years, its professional staff was made up almost exclusively of graduate students in sociology. Also, as we have indicated elsewhere, DRC consciously reflected, in form and substance, much of what had gone on at NORC and to some extent the NAS Disaster Research Group (Quarantelli 1981a). To the extent these disaster research groups had a sociological tone, DRC also had one.

Not only were the majority of the early workers in the disaster area from sociology, but the key NORC group (as well as other researchers such as Killian) also shared other important characteristics. First, a number of them had a major professional interest in the sociological subspeciality of collective behavior. Second, they overwhelmingly and consciously saw themselves as applying sociology to the disaster area, rather than developing a new field of scientific inquiry. The sociology they implicitly applied consisted of a certain general perspective on the topic, as well as particular specific theoretical sociological orientations. These matters all had important substantive and methodological consequences for the way the early stages of disaster research developed in the United States.

A formal survey was never made of the specialty interests of the earliest disaster researchers. But on the basis of our personal knowledge, it is easy to observe that a number of them such as Bucher, Fritz, Killian and Schatzman, among others, shared our professional interest in the sociological specialization of collective behavior. This is important for a variety of reasons.

As a group, those interested in collective behavior could and did resist viewing disaster phenomena as within the province of social problems or involving social disorganization. To most of them, crisis situations represented occasions allowing for the emergence and creation of new behaviors rather than the breakdown of social order or the presence of unfavorably or deviantly viewed social behavior (and they continue to do so today, see Goode 1992). Also, intellectual stereotypes to the contrary, American scholars in the area of collective behavior have never put much stock in notions of irrationality of behavior (Aguirre and Quarantelli 1983), and so those studying disasters also shied away from such formulations or characterization in approaching disaster phenomena, even including panic and scapegoating behavior (Quarantelli 1954, Bucher 1957). Finally, students of collective behavior tend to see social phenomena as having a collective

or group form rather than simply being the behavior of isolated or unconnected individuals. They also did so in the disaster area.

Our general point is that the block of collective behavior specialists involved in the early disaster studies acted as a major barrier to even using certain other sociological ideas because they ran counter to the intellectual biases of the specialty. Conversely, other labels or formulations about disaster behavior were more likely to be advanced because again they better fitted the intellectual orientations of those in the field of collective behavior. It could be argued that the empirical evidence dictated the avoidances and attractions of certain views, but we strongly suspect that theoretical predispositions were far more powerful in influencing what was or was not seen, at least in some cases (this of course is equally true outside of the disaster area). To the extent these intellectual proclivities were operative, the disaster area was directed along certain lines rather than others. If specialists, for instance, in deviancy or social problems had been heavily involved in the initial stages of disaster research, the field would have differently developed. For example, we remember staff meetings at NORC where various social problem labels for and approaches to disasters were rejected in favor of seeing disasters in a larger social change context, a rejection that Stallings (1991) has recently strongly restated. Thus, to the pioneers in the area, disasters were less a trauma to an existing system but more an evolutionary occasion in an ever changing system.

Furthermore, perhaps because of the professional socialization many of those had undergone at the University of Chicago, almost all of the sociologists in the early disaster studies saw themselves as legitimately looking at disaster phenomena through sociological eyes rather than as involved in the development of a new "ology." Sociology was good enough for them. Apart from the Chicago scholars, the one formal attempt to start creating a science of disasters (Moore 1956) found no supporters or followers, and there were self conscious admonitions, even in the early days of the research effort, that better research was to be done by doing better sociology. The insistence on viewing disaster phenomena as within a disciplinary boundary was set early, and has manifested itself in the last 40 years in the great difficulty there has been up to the present time in generating a professional association of, or a journal for, disaster researchers as such. Although several instances of both have now been created, they all bear strong single social science discipline orientations; thus the sociologists, the geographers, the anthropologists and the political scientists or public administration specialists as well as risk analysts in the United States have tended to form their own subprofessional formal disaster groupings or associations.

The influence of the sociologists and their commitment to sociology is also indicated by the fact that initial efforts to launch disaster research as a multidisciplinary or interdisciplinary field of study failed, failed early, and failed decisively. The NORC work was supposed to be at least multidisciplinary, and conscious steps were initially taken to recruit representatives of different disciplines into the team (e.g., anthropology, psychology, lay psychoanalysis, etc.). This quickly proved disruptive of team work, but soon became a moot issue as the work proceeded and a certain sociological view of disaster phenomena came to the fore. The University of Maryland group was also supposed to be composed of at least representatives from psychiatry, sociology, medicine and psychology, but it proved impossible to assemble such a team, the leader of which from the start of the work was a social philosopher. The University of Oklahoma field team made no attempt to recruit outside of sociology.

The NAS operation was also supposed to be multidisciplinary, both internally and externally. As Williams, its director, wrote:

In 1952, the research representatives of the Army, Navy and Air Force Medical Services requested the National Academy of Sciences-National Research Council to undertake a program of disaster studies. They suggested a national program to advise, stimulate, coordinate, and collate the results of research on a broad inter-disciplinary basis.

In such a request lies the assumption that disaster research might emerge as a substantive inter-disciplinary field of research interest, with a body of theory, data, methods, and competent practitioners. This has been, and continues to be, a major goal of the Committee on Disaster Studies (1954, p. 6).

Structurally, the Committee on Disaster Studies was located in the Division of Anthropology and Psychology, but non-sociologists played only minor roles in the actual make up of the core workers in the Committee and the Research Group (even though in the advisory boards and committees which oversaw the Academy work, sociologists were a clear minority—e.g., being only three of the 10 members of the executive council of the OCDM-NRC Advisory Committee on Behavioral Research which oversaw the work of the Committee and Research Group). Also, while the NAS did fund the outside work of non-sociologists, as already indicated in our discussion of the publications from the DRG, sociologists were by far the majority of the authors of reports. Partly as the result of the internal problems observed at NORC, and indirectly from what was seen at the NAS, DRC never did seriously consider a multi or interdisciplinary operation. The

early sociologists in the disaster research area essentially remained sociologists, just as they did not become "disasterologists," they also did not become social scientists as might be implied in the then fashionable multi or interdisciplinary approach. (For a discussion of the waxing and waning of an interdisciplinary or multidisciplinary approach in the social sciences generally, see Frank 1988).

The sociological perspective that was brought to bear by the early disaster students consisted both of a certain general view, as well as specific sociological views (some of these have already been discussed in 1981 by Kreps). Thus, the work done from the beginning reflected a general sociological perspective of attributing the important conditions for social phenomena to the social setting rather than the personal or internal attributes of the social actors in the situation. To most sociologists this might seem obvious, but it is not the assumption, for example, of psychologists. It is noticeable too that when geographers first initiated research into hazards (and it was hazards rather than disasters), they went in the direction of psychology rather than sociology. Thus, the early and many current concerns of geographically oriented disaster researchers with attitudes of people towards hazards, with their views of natural hazard risks, etc., approaches and topics almost ignored initially by sociologists in the disaster area (it has been noted with respect to hazard research that "perception studies grew to dominate the parent field in the late 1960s and early 1970s" (Mitchell 1984, p. 38). We mention it to illustrate that disaster studies almost certainly would have had a rather different general explanatory content if the field had been primarily developed by other than sociologists, and that this has to do with disciplinary assumptions far more than empirical data.

There has been a "geographical" approach in the area. We cannot here examine the approach to natural hazards that was developed in geography in the late 1960s (White 1964; Burton and Kates 1964; Kates 1962, 1971; White 1974; Burton, Kates and White 1978; Hewitt 1983; Mitchell 1989). This has resulted in a somewhat different view of the nature of the phenomena that sociologists treat as disasters. A sociology of knowledge and historical examination of the "geographical" approach to natural hazards would be definitively worthwhile (as recently partly attempted in a very good article by Mitchell 1990), especially if the analysis took into account the features of the parallel, although somewhat earlier initiated, development of the "sociological" perspective that we are discussing.

However, for our purposes here, the "geographical" perspective does illustrate one of our points, namely that disciplinary biases, certainly as much as empirical data, can and do determine what will be seen or not seen

and interpreted as important or not important in the analysis of disaster phenomena. In fact, the geographical preference for the term "hazards" to indicate the focus of the field of study rather than "disasters" is primarily a reflection of disciplinary differences. The tendency also for geographers to try and conceptualize hazards and disasters in physical terms—essentially reifying nature—and often to define them under agent specific labels (both of which was less done by the early sociologists involved in disaster studies), also is a reflection of a disciplinary orientation (Quarantelli 1982).

For reasons probably having to do with the strong quantitative orientation that prevailed in the discipline at the time, there was not much by way of an early development of a systematic "psychological" approach to disasters. There were some very initial formulations such as by Menninger (1952), Spiegel (1957) and Wolfenstein (1957) which were more psychiatric rather than psychological, but see Janis (1954) for an exception. However, outside of the United States, the three major pioneers in disaster studies were psychologically oriented, namely Kitao Abe in Japan (Okabe and Hirose 1985) and Charles Chandessais in France (1966a), and a psychiatrist, Tyhurst (1950, 1957) in Canada. But none of that work led to any definitional, conceptual or theoretical advances.

Most of the early sociologists did, implicitly if not explicitly, operate with a social psychology scheme, but it was "symbolic interactionism", the general sociological choice as compared with other more psychological versions of social psychology. Again, this led the early disaster researchers in certain directions and away from others. For instance, the pioneering sociologists (as well as many even at the present time) doing disaster research were seldom comfortable with attitude studies of how people think they might behave in an actual disaster occasion, and generally did not find them very useful. This downplaying of attitudes as indicators of behavior reflected the symbolic interactionist approach.

On the other hand, the sociologists first working in the disaster area on such issues as warning quickly and consciously brought to bear the sociological dictum that "if a situation is defined as real, it is real insofar as consequences are concerned." (Thomas and Thomas, 1928, p. 572). Therefore, from the start, sociologists have been as much interested in situations that are perceived as being threats as in those involving actual impacts, a bringing together of two crisis situations almost no one else working in the disaster area saw as functionally equivalent. The actual appearance of a physical disaster agent was not seen as crucial as how participants defined the situation. This is a point that most non-sociologists do not even see and they keep attempting to define and conceptualize disasters in physical terms

or with respect to supposedly inherent dimensions of a disaster agent, as if those determine the perception of the actors in the situation (of course to be fair it should be noted that some recent sociologically oriented efforts have regressed to a non-social level formulation also, e.g., Kroll-Smith and Couch 1991). Most of the early disaster researchers to the extent they operated with any social psychological model, used that from symbolic interactionism where reality is seen as socially constructed.

In fact, so pervasive was both the classic collective behavior and symbolic interactionist theoretical orientations in the thinking of many of the early disaster workers, that they seldom made explicit their assumptions of such particular theoretical perspectives. This has led non-sociologists and even some sociologists not well conversant with the fields of collective behavior and symbolic interactionism, to mistakenly assert that the early disaster research undertaken by sociologists was nontheoretical (Mileti et al. 1975, p. 146). The opposite is far truer (Wenger 1986). The two theoretical perspectives used were so deeply ingrained and so taken for granted in the thinking and work of most of them, that the possible use of different theoretical perspectives in collective behavior and social psychology was not often entertained (and it should be remembered that in both these areas at the time disaster studies emerged, that the classic Chicago collective behavior approach and symbolic interactionism were by far the dominant theoretical and research paradigms).

Some early scattered efforts to advance other theoretical perspectives got nowhere. At NORC, an effort to bring the structural functional theoretical orientation of Talcott Parsons to the fore was almost totally rejected, although in an odd way, the classic definition of disasters advanced by Fritz (1961), and recently revised in a minor way by Kreps (1989a), had its root origins in Parsonian theory rather than the symbolic interactionism which predominated in the NORC group. Robert Endelman, a student of Parsons, on the NORC team provided the initial version or definition (1952). But this early explicit rejection of the Parsonian approach is interesting in that a few later critics of the pioneering work have argued, incorrectly in our view, that disaster studies always have had a "functionalist bias" (Blocker et al. 1991, p. 368).

Other particular ideas in sociology which influenced the early disaster researchers were drawn from various parts of the sociological literature, including notions of formal organization and from what has sometimes been called "role theory." Again, as in the case of collective behavior and symbolic interactionism, the views advanced were seldom explicitly linked to the more formal statements in the corpus of sociological theory. None-

theless, the sociologists first involved in disaster research were not unaware of the roots of these particular views in basic sociology.

For instance, even the focus on formal organizations did not see them in Weberian bureaucratic terms but instead implicitly approached them as the result of a "negotiated order", more organized behavior than an organized entity, an idea derived also from symbolic interactionism. In addition, the notion of "role conflict" derived from the literature of the time was advanced a long time ago by Killian (1952) in conjunction with his work on the University of Oklahoma field team, even though it was not until another 34 years had passed that the discussion of role conflict in disasters was squarely placed into the extensive body of research that exists on the topic (Dynes and Quarantelli 1986).

If there is a question to be asked here, it is not the absence of a number of basic theoretical underpinnings in what the early researchers were doing and saying, but why they were not made more explicit. Possibly because the early students were frequently communicating with very small numbers of like minded colleagues, there was no felt need to assert explicitly the theoretical ideas involved (it should be noted that members of the three pioneering teams very early informally established and carried on communication with one another). The key participants at NORC, for example, had all been deeply indoctrinated in their professional training to the basic models, theories and ideas prevalent in the sociological specialties of collective behavior and symbolic interactionism.

Perhaps also the somewhat inductive nature of much field research in the disaster area discourage efforts at explicit theoretical deductions, but of course it could be argued that the absence of the latter led to the presence of the former. On the other hand, maybe because the early disaster researchers were field workers who had very direct and extensive concrete contact with what they were studying, unlike in many other areas of sociology, they may have had enough to keep them occupied so they did not need to engage in abstract speculations. This is hinted at in a statement by Killian who once wrote:

We in disaster research certainly are not among those scientists described by Louis Wirth as being interested only in problems "uncontaminated by any relationship to reality." Yet it is a strength of disaster study that researchers have been quick to see the basic theoretical implications of their findings and the contributions which disaster research can make to broader areas of theory. Equally desirable has been the persistent attempt to relate disaster

findings in existing theory in such areas as perception, learning, stress, reference groups and social organization (1954, pp. 66-67).

In discussing these matters, our point is to emphasize that a certain sociological perspective and particular basic sociological ideas permeated the thought and work of the early researchers in the area. As sociologists they saw and reacted to disaster phenomena in sociological terms. They were often not explicit, but at an implicit level, it truly could be said that most were not disaster researchers but sociologists who were engaged in the study of the social aspects of disaster occasions. It is not an insignificant fact that almost all of these early workers in the disaster area not only maintained their professional identity as sociologists, but most of them in their career lifetimes also worked on non-disaster topics, with many attaining professional recognition as contributors to sociology in these other areas (e.g., Barton, Bates, Bucher, Dynes, Form, Killian, Quarantelli, Schatzman).

Let us sum up our observations to this point. The applied orientation of the first funders of disaster studies had substantive consequences at the time as we illustrated in our earlier paper, and we can still see some of the consequences in how and what is researched today in the disaster area. The sociological orientation of the early disaster researchers also had important consequences, and these too still partly affect studies and thinking at the present time. For example, in a recent examination and summarization of what the research literature says about organizational effectiveness in disasters, of 58 specific sources cited, 52 are by sociologists with 35 of them from DRC associated researchers (Mileti and Sorensen 1987). It seems incontestable that if initial research in the disaster area had been carried out by other than sociologists (and also by those with different sociological biases), the field of disaster research would now be substantively rather different. Thus, as an example, the explicit use of emergent norm theory and the grounded theory methodology sometime set forth today (Drabek 1986) has its root deep in the orientation of the pioneer researchers; it appears doubtful that if structural-functionalist theoretical oriented researchers and/or quantitative survey research methodologists had been dominant in the beginning, that the field of disaster research would look the way it currently looks.

Functional Consequences

The marriage of the applied and basic sociological orientations has had, up to now, primarily functional consequences for the field of disaster studies. We have already indicated some possible dysfunctional effects and others of both a substantive and non-substantive nature might be advanced.

But on balance, the field would not have developed as much as it has if it had not been rooted in practical concerns. On the other hand, the quality of the research undertaken would not have been as good as it is if there had not been an early infusion of a sociological perspective. Obviously these are evaluative assessments for which it is almost impossible to adduce systematic evidence one way or another. But we think a case can be made that the results have been more positive than negative.

Thus, it seems that the early studies were judged worthwhile enough to lead the government funding agencies, with the exception of the military, to continue or to initiate support. It has apparently been useful enough so that those with applied concerns have been willing to provide fairly continuous funding. For example, when the pioneering field team efforts phased out, the succeeding Academy work was supported. Also, DRC has had in principle if not in fact unbroken continuous contract support starting with the Office of Civil Defense through successor agencies on through the Federal Emergency Management Agency (FEMA) at the present time. Similarly, in recent years other agencies with applied interests have provided some support for various researchers; these include the National Institute of Mental Health, the Administration on Aging, the Health Resources Administration, the Department of the Interior, and the Environmental Protection Agency. Less of a case for research support had to be made with these agencies given that earlier governmental support for disaster studies could be cited. The early funding allowed the development of a body of knowledge, uneven though it was and is. New researchers did not have to start at ground zero as did the pioneers. The initial funding generated a core of researchers with commitments to this field of study. This in turn has helped in the development of a critical mass of researchers at the present time. In turn, the existence of a critical mass not only indicates, but also allowed the institutionalization of the whole field of social science studies of disasters. This is discussed and documented in great detail in the volume, *Sociology of Disaster: Contributions of Sociology to Disaster Research* (Dynes et al. 1987).

It is true that now a government funding agency with a more basic science orientation is in being, namely the National Science Foundation (NSF). But in reality the research support for disasters from within NSF has always had some roots in an applied base in the sense that the supportive entity is a formal part of the engineering structure of the foundation. But more important, it is doubtful any NSF organizational unit with a social science interest in disasters would have ever been established without the research findings from what had been done earlier as a result of applied interests and studies.

We think the basic sociological infusion has prevented the field of disasters studies from going down unfruitful paths with respect to such matters as unrealistic research designs and priorities for research. The earliest sociologists involved made a case for qualitative type research and for studying phenomena at a supraindividual level, the failure of which to take the latter position has hindered the development of the geographical approach to natural hazards. Sociological notions have also called attention to questions and issues which might have otherwise been ignored (e.g., taking the position that the source of many disaster problems is in the responding organizations rather than in individual victims), and have provided non-common sense interpretations of empirical findings (e.g., that it is hope of escape rather than perceptions of being trapped that underlies possible panic flight behavior, see Quarantelli 1981b). Put in other words, sociology has provided a theoretical base for the research effort and scientific legitimacy to the social science study of disasters.

The Future

If this was the past, what does it say about what has happened up to the present, and does it give any clues for the future?

Our very brief reading of the last three decades or so reveals that applied concerns still provide the impetus for almost all funding and support. Certain topics and questions therefore continue to be implicitly attended to while others are ignored. Sociologists, while probably no longer a majority but still a plurality of the current disaster researchers continue to approach their work as sociologists, and therefore the same favorable and unfavorable aspects exist as in the early days of disaster studies. Put in other words, the general stance of disaster studies is still more or less the same as it was when work in the area started. The research is methodologically more complex and somewhat more theoretically explicit, but no researcher from the decade of the 1950s would have any trouble understanding the "what" and the "how" of the area in the early 1990s (although the converse is not always true!) It is perhaps rather telling along certain lines that the NORC survey in the Arkansas tornadoes in 1952 is still the best statistically drawn sample of any disaster impacted population (Quarantelli 1988), and that most of Barton's theoretical discussion in his classic book, *Communities in Disasters*, written in 1969 has yet to be matched in later theoretical work up to 1992 (Gillespie 1988).

This is the way we see the situation in the field at present. It is mostly the result of what occurred in the past. But is the way the past developed the best way for future developments in the area?

We leave aside here, for reasons indicated earlier, the probable continuing development of the "geographical" approach to natural hazards and the more recent "risk analysis" approach especially to more technological types disasters (the latter overlaps more with the sociological than the geographical approach, particularly because of its developed concern with the social nature of perception of risk, see Short 1984; Renn 1992). There also has been in the last decade a considerable number of writings from a psychopathological perspective, but it is overwhelmingly very narrowly focused on clinical reactions to disasters per se, usually ignoring the larger and substantial body of literature on mental health produced by several social science disciplines. Thus, although some are more sanguine than we are (Mitchell 1990), we do not see researchers in the geographical, risk analysis and psychopathology areas as likely to move soon and in any major way to dealing with the conceptual, definitional and theoretical problems of the disaster area.

Scholars working on sociology of science and knowledge problems on any topic have indicated that pioneers in a field can open up an area, and that those immediately following them can consolidate what has been started. But it appears that sometimes work in an area will then flatten out and show little significant development unless some major reformulations occur. In a very rough sense we think this is where we now stand in the sociological study of disasters. About four decades ago, pioneers started to stake out the area. In the last decade or two, others who have joined them to constitute the current critical mass of disaster researchers have brought together much that had been started earlier.

But our sense of the field is that unless some major changes are soon undertaken in the area, we will shortly reach the plateau that students of scientific development hypothesize can occur and stagnate a developing field. What changes can or should occur?

There are a number of possibilities. Our view, which is probably not contrary to that of many others, is that we should not primarily think that the path to follow in the future is in the direction of tighter research designs or more quantitative kinds of studies (at least for the purpose of scientifically developing the field). Rather we should be attempting to reformulate the basic concepts and theoretical models we currently employ, mostly at an implicit level. Most sociologists of knowledge and science tend to agree that significant advances in an area come less from the accumulation of empirical studies, but more from the reconceptualizations of basic ideas and definitions.

It is not that better research designs are not needed or that more quantitative studies would not give us certain kinds of information we currently lack—few would argue against such activities. But there are limits to what can be achieved by improvements in those ways. The controversies, for example, regarding the “mental health effects of disasters” revolve as much around what is being conceptualized as “disaster” as well as “mental health” as it is around the use of control groups or the quantification of research data (Quarantelli 1985a, Sowder 1985). More or even better data or different empirical studies will not resolve those controversies. However, there might be some advances if some consensus was reached on the central concepts involved. Thus, we side with those who argue that it is mythological to believe that major or fundamental scientific advances come primarily as a result of better or different empirical studies. Instead, we stand with those who say that advancement is achieved through new ways of conceptualizing and explaining phenomena. While this is not the place to discuss Kuhn’s idea of paradigm reformulations (1970) as the way by which science develops, our view is close to his.

The basic current problem we see in the area of disaster studies is that we do not know what we are studying, or more accurately put, we have up to now advanced only very vague notions about our focus of research. There is something wrong about a field of study which attempts to delineate the characteristics of something, tries to depict the conditions leading to that something, and gropes to show the consequences of that something, without having a relatively clear conception of what is the “something.” What are the central and defining features and outer limits of that “something”—in other words, what is a disaster? (Quarantelli 1987b, see also Bates and Peacock 1989 who argue recovery of and from disasters cannot be addressed without conceptualizing the disasters).

The current efforts in the United States by Kreps (1984), Drabek (1986), Rochford and Blocker (1991), Dynes (1992), and ourself (Quarantelli 1982), and elsewhere by other sociologists such as Pelanda (1982b) in Italy, Britton (1986, 1987) in Australia, and Dombrowsky (1981) in Germany to better define and conceptualize “disaster” is to us a harbinger of where the field as whole might most fruitfully initiate a major reformulation of the central concepts in the field of disaster studies (as early as 1968, Stoddard made the same point, but his work remains almost totally unknown at the present time).

The definitional problem is not confined strictly to research issues. The very title of this journal, *The International Journal of Mass Emergencies and Disasters*, raises questions of what such a periodical should publish

and review. In fact, the actual title of the journal reflects an initial ambiguity on the part of the original two editors (including myself) as to subject coverage. The term "Mass Emergencies" was included mostly to indicate the uncertainty of those who founded it as to what their field of interest did and or should cover (Britton 1987, p. 43, who raises a question about the journal title as to what the study of disaster includes in its scope).

Of course a concern with definitions and concepts has not been totally confined to sociologists. A few researchers from other disciplines have also recently struggled with the question of how to talk about "disasters" (Mitchell et al. 1989, Wright et al. 1990). However, the current sociologists who are interested in the matter of conceptualization see it as very crucial to the development of the field of study, whereas the researchers from other disciplines who have a theoretical interest in the concept do not seem to accord the issue such an importance in the future of the area.

If this reformulation of the field is a central issue, can we continue as in the past or is there a necessity to change the applied/basic orientation in the future? That is, might we expect applied considerations to lead us in the direction of a conceptual reformulation? Or is it more probable that we will move towards definitional clarity about disasters if we operate much more from a basic sociological perspective? In other words, is the fundamental change we seek more likely to occur if we continue as in the past to let applied concerns structure our research efforts, or is it more likely to happen if we become increasingly and more explicitly sociological in our studies? (the latter being advocated by Pelanda 1982a, Dombrowsky 1983, Kreps 1989a, among others). The continuation of publication in mainstream sociological journals is also a clear manifestation of this last orientation (Kreps 1984, 1985; Bosworth and Kreps 1986).

Let us first inject our view that we see little in the short run that would seem to alter the current mixed situation in the United States of applied funding concerns married implicitly to basic sociological questions. For a variety of reasons it does not seem to us that such agencies such as the FEMA, the National Institute of Mental Health (NIMH), or the Environmental Protection Agency (EPA) are at all likely to move away from practical concerns about disasters. If anything, they are even less likely in the near future to supported anything that might be considered of a more basic nature (in FEMA actually there is some selective strong opposition especially to social science research of any kind).

In principle, there would be some possibilities available in the National Science Foundation (NSF). But we believe the very existence of a somewhat applied disaster orientation within NSF actually precludes disaster research

obtaining substantially more funding from the basic sociology component of the agency. So we see little change in the long run in American society with respect to funding and its effects on research.

As we have already indicated, we do think some major change is necessary in the long run. What are the possibilities when the longer view is taken? Along one line, it might be argued that U.S. federal funding agencies might, for two reasons, become more interested in the question of the concept of "disasters." FEMA's Integrated Emergency Management System (IEMS) notion would almost seem to demand some attention to fundamental questions about the similarities in the full spectrum of emergencies including natural disasters, technological accidents, resource shortages, wartime attacks on civilians (and possibly terrorism and arson) for which the system is presumably designed. How could IEMS be implemented, taught, trained for, etc. if the core and the outer boundaries of the phenomena for which the system has been generated is not conceptually clarified? Also, as the Nuclear Regulatory Commission (NRC), the Department of Energy, EPA, and some medical/health agencies of the federal government struggle with preparing for and responding to specific kinds of particular emergencies, it would seem they would want to know the similarities and differences between their own hazardous object of concern and other kinds of threats in American society.

However, we must admit we see little likelihood of the federal agencies we have mentioned as suddenly exhibiting a willingness to support theoretical and conceptual examinations and perhaps not even empirical studies of "disasters." We think they should, but recognize the bureaucratic and political problems that would discourage moving in such a direction. As social scientists we might bemoan the failure to recognize that theoretical advances in the long run can have very practical consequences. A long time ago, Benjamin Franklin supposedly asked the question of who eventually contributed more to safety at sea—the carpenters who designed better lifeboats or the astronomers who developed models about planetary movements and the stars which allowed better readings of the stars for navigational purposes? Unfortunately in the disaster area it seems almost certain that the funding agencies with applied interests will support—if they support anyone at all—the work of carpenters rather than the work of astronomers.

Is there any hope for impetus for conceptual clarification coming from trends in basic sociology? In some ways a conference at the College of William and Mary in 1986 organized by Gary Kreps might suggest optimism. For whatever reasons, a number of sociologists prominent for work

in the sociological theory area (e.g., David Alexander, Randall Collins, Nicholas Mullins and Walter Wallace) and who up to that time were probably not even aware of the disaster research area, brought their knowledge and intellects to bear on the question of what basic sociology might contribute to disaster studies (for an updated version of the proceedings of the conference, see Kreps 1989b). Totally apart from this, the previously mentioned volume on *Sociology of Disasters* has been published (Dynes et al. 1987). Furthermore, several disaster researchers such as Kreps, Ino Rossi, Dombrowsky, Ralph Turner and Pelanda have all relatively recently written explicitly on various sociological theories and disasters as social phenomena. If these kinds of activities were continued, some considerable advances could be anticipated on the conceptual problems of disaster research.

However, there is little reason to think that future conditions or circumstances will facilitate the continuation or the acceleration of such activities. It seems unlikely that sociologists from outside disaster research will continue to have a major interest in the area. As for sociologists within the disaster field, apart from the fact that except for the few mentioned earlier in and outside of the United States, most other seem inclined to do nothing but empirical studies, those interested are unlikely to find much support for their interest in conceptual problems.

We leave aside here larger matters such as that according to many like Ralph Turner (1990), J. Stephens and Jonathan Turner (1990) and the various writers in the Gans compilation on *Sociology in America* (1990), the label "sociology" covers a very wide range of theories, methodologies and substantive studies which share little more than a common label. If this view is correct, this would not lead us to suppose that sociology could be depended upon for much help in conceptual clarification when it cannot even clear up its own basic focus!

There is also the question of whether a move towards an inter or multidisciplinary social science approach should be encouraged or could occur. We think it is very unlikely to succeed. To expect such an approach to take root in disaster studies when it does not exist in any meaningful fashion in almost any other area of social science research or in university settings, is not very realistic. Questions about disaster phenomena are not inherently any more interdisciplinary in nature than criminal or family behavior.

In concluding, we should note something that we have only primarily alluded to in passing so far, namely the increasing internationalization of

disaster research. We do consider this as an important factor. It can and should contribute to definitional, conceptual and theoretical clarifications.

Not only have a number of studies by European, Japanese and Australian sociologists advanced criticisms of both early and later American disaster research (Britton 1987, Clausen et al. 1978, Clausen and Dombrowsky 1984, Dombrowsky 1981, Jager 1977, Pelanda 1982a, Schorr 1987, Yamamoto 1984), but some have suggested and used radically different theoretical assumptions and models (Pelanda 1982b, Dombrowsky 1987). As such they ought to be considered in any full assessment of how future disaster research will proceed. But since our discussion has come out of an examination of the early days of disaster studies which had been primarily an American enterprise (even recognizing early Canadian, French and Japanese work—Tyhurst 1950; Chandessais 1966a, 1966b; Abe 1972), we have not considered in depth the implications of increasing non-American work in the disaster area.

Actually, along some lines a strong case could be made that conceptual and theoretical developments are more likely to be made by non-American researchers. In fact, we think the non-American input into the field has been very salutary to getting at least a few in the United States to think about the basic concepts and orientations of the field. The lack of attention to social class or stratification in disaster studies in America is an example of what the European critics, non-Marxists as well as Marxists, have correctly emphasized.

So while we think some basic changes are necessary in the study of disasters, we are not sure what would best generate them and even if we knew, it is not clear that conducive conditions will be present. Perhaps we can take heart from an old Chinese proverb that a journey of a thousand miles has to start out with a first step. We hope that this article might have provided a step or two for those interested in the problem.

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