

PRELIMINARY PAPER #111

DISASTER STUDIES: AN HISTORICAL ANALYSIS OF
THE INFLUENCES OF BASIC SOCIOLOGY AND APPLIED
USE ON THE RESEARCH DONE IN THE LAST 35 YEARS

E. L. Quarantelli
Disaster Research Center
University of Delaware

May 1986

*This is a version of a paper presented under the title of Disaster Studies: Basic Sociology or Applied Research given at the Symposium on Social Structure and Disaster: Conception and Measurement held at the College of William and Mary, Williamsburg, Virginia, May 16, 1986. We thank Charles Fritz for his extensive comments on an earlier draft of this paper, but, of course, all statements and assertions made are our own and do not necessarily reflect his views or positions.

We want to discuss disaster studies and their relationship to basic and applied sociological research. This relationship could be looked at from the viewpoint of what it ought to be, or it could be examined from the perspective of what it actually is. We have chosen to discuss what is really done, rather than, ideally, what ought to be done. In fact, since most of our remarks and examples are drawn from the past, from the first decade or so of disaster research in the United States, we shall be talking not so much of what is done at present, but more about what happened in the past. In other words, we will talk about the very early days of systematic disaster research in this country and try to indicate in what ways applied and basic questions influenced the work that was undertaken, in that sense focusing on the "was," rather than the "should have been."

Our remarks are organized around four major points. First, we will note that disaster studies on human and social aspects had their initial roots, almost exclusively, in rather narrowly focused applied questions or practical concerns. Second, we will illustrate this led to certain kinds of selective emphases in terms of what was first studied in the disaster area, with substantive consequences which we still see operative today. Third, we will observe that, nonetheless, a basic sociological orientation and sociological ideas implicitly permeated much of the early research effort and many of the answers that were offered. Our fourth and last point is, the research approach initiated with a mixture of applied concerns and basic sociological questions, and continued now for about 35 years, has had primarily functional consequences on the development of the field of the study of disasters.

We conclude with a brief statement on what all this might portend for the future. Basically, we take the position there is little reason to think that any orientational change in the short run approach to the sociological study of disasters is likely to occur in the United States, but there is a need, if not the potential, for some major change 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 outcome is to happen, but it is unclear if the impetus for that is more or less likely to come out of practical concerns than from basic sociological questions.

The Applied Orientation of the Earliest Studies

The earliest disaster research in the social science area was almost exclusively supported by U.S. military organizations with very practical concerns about wartime situations.¹ Who were the initial research sponsors and what were their interests? For our purposes, we can look at this from the perspective of the three roughly sequential sets of organized research activities from about 1950 to 1965.

(1) The Pioneering Field Teams.

The first of these sets were the pioneering field team operations of which, unknown to many people, including current disaster researchers, there were three. The one that became famous was at the National Opinion Research Center (NORC) at the University of Chicago between 1950-1954.

This research was commissioned and supported by the Chemical Corps Medical Laboratories of the Army Chemical Center in Maryland.

Military personnel from this chemical center had looked at Donora, Pennsylvania, where in October, 1948, a combination of chemical fumes and a temperature inversion created a concentration of sulfur dioxide which made 43 percent of the population ill and killed 25 persons over the duration of several days. It was observed that some inhabitants of the area who had not been directly exposed to the smog apparently showed the same kind of symptoms as had victims who had been directly exposed. Seeking an explanation of this observation, in 1949, the chemical center approached NORC to do a retrospective study of the Donora episode. In joint discussions, this was eventually rejected as not worthwhile since any field work would have been done too far after the occurrence of the episode.

However, further contact between NORC and the Army Chemical Center led the latter to support a project by NORC on the study of natural and industrial disasters. As said in the research proposal, "it is felt that empirical study of peacetime disasters will yield knowledge applicable to the understanding and control, not only of peacetime disasters, but also of those which may be anticipated in the event of another war." Elsewhere in the proposal, it is said that "careful selection of the natural or industrial disasters to be studied can furnish an approximation of the conditions to be expected in a war disaster." It was acknowledged that there are certain differences between war disaster and peacetime disasters, especially that in the latter, unlike the former, people's adherence to the cause for which the war is being fought will make them willing to make sacrifices on its behalf. The proposal comes back a number of times to the idea that one could learn about the probable wartime behavior of a population from studying how they responded to natural and industrial disasters.²

That primary interest was in the wartime implications can also be seen in two other aspects of the proposal. One is the emphasis on social control, the other is the implicit notion that the basic problems in disasters are to be found in the reactions of people to danger, loss, and deprivation. Thus, it is observed that there is a need for "the reduction and control of panic reactions," that minimum elements in effective disaster control include "the securing of conformity to emergency regulations," that morale is "the key to disaster control; without it the cooperation and conformity needed from the public will not be forthcoming," and so on. Likewise, the research design focused on individual victims and the questionnaire was aimed at answering five general questions:

1. Which elements in a disaster are most frightening or disrupting to people and how can these threats be met?
2. What techniques are effective in reducing or controlling fear?
3. What types of people are susceptible to panic and what types can be counted on for leadership in an emergency?

4. What aggressions and resentments are likely to emerge among victims of a disaster and how can these be prevented from disrupting the work of disaster control?
5. What types of organized effort work effectively and which do not?

The last was conceived primarily in terms of "good disaster leadership" and not in organizational terms. Some informal interviewing of community leaders was projected, but this was to be done for the purpose of uncovering "more expert and informal accounts of the disaster, and description and analysis of public reactions to it, and of the adequacy of control measures, all of which information will be of great value in interpreting and evaluating the popular reactions uncovered by the systematic interviewing."

As one who was involved in the NORC project almost from its inception, we can attest that the actual field work proceeded more or less as indicated in the proposal. The effort made was to find peacetime disasters which appeared to have the closest parallel to a wartime situation (that is, a population subjected to some kind of sudden and widespread attack). The intent of the work was to find out how social control could be exercised by the authorities, and the assumption was made that disaster problems were primarily social psychological in nature, i.e., resulted from the internal states of the victims. However, as we shall note later, the sociological orientation of most of the researchers at NORC working on the disaster project led in the course of the work to certain subtle changes in emphases and observations, and perhaps even findings.

The NORC team undertook eight field studies of disasters ranging from an earthquake in Bakersfield, California, to three consecutive plane crashes in Elizabeth, New Jersey. The major work, however, was a very systematic population survey of 342 respondents (out of a strict probability sample of 362) in several towns and villages in northeast Arkansas hit by tornadoes in March 1952. Publications by project members from this study continued for some time after the end of the research, although the final report itself was never put out in any regular published form.

An intended counterpart to the NORC work was that done at the University of Maryland in 1950-1954. This, too, was supported by the Army Chemical Corps and was aimed at studying "in depth" the psychiatric aspects of disasters as was partly indicated by the fact that the project was administrated through the Psychiatric Institute at the University of Maryland. The stated purpose of the work, as described in the contract was:

To study the psychological reactions and behavior of individuals and local population in disaster, for the purposes of developing methods for the prevention of panic, and for minimizing emotional and psychological failures.

In an Appendix to the research proposal under a heading of Suggested Areas of Psychological Investigation were listed:

A. Mass Population Behavior of Those Involved

1. Herd Reaction
2. Panic
3. Emergence of Leaders
4. Recommendations for Guidance and Control of Masses

Thus, even more so than in the NORC study, the University of Maryland work had a psychological emphasis and focused exclusively on individual victims. It is clear the findings are to be applied to a wartime civilian context. But, like the NORC work, and also partly perhaps because the projected multidisciplinary staff was never assembled, a somewhat different and more social science oriented end project was undertaken than probably had been originally intended by the research sponsor.

The field workers with, or supervised by, the University of Maryland study, undertook field studies of eleven different episodes. Major disasters studied were tornadoes in Arkansas; Worcester, Massachusetts; and Waco, Texas, but other emergencies researched included a chlorine gas episode, a hospital fire, a methyl alcohol poisoning episode and one of the Elizabeth plane crashes. University of Maryland field workers overlapped with NORC teams in the Arkansas tornadoes and the Elizabeth plane crash. The final report on the project, produced in mimeographed form, was about the only publication to result from the Maryland work.

Finally, the third field team operation was at the University of Oklahoma. This was undertaken in 1950-1952 under a subcontract from the Operations Research Office at John Hopkins University which was conducting a much larger study of the effects of atomic weapons on troops in the field. As part of that effort by the military to understand the psychological aspects of exposure of soldiers to such weapons, researchers in the Department of Sociology at Oklahoma were asked to do several things: to analyze afteraction reports, to observe in the field troops in a Nevada exercise, exposed to an atom bomb test explosion, and also to study civilian behavior in extreme situations such as natural and industrial disasters.

All reports from this work were initially classified and not available to the general public for some years. Declassification of most of the written material and discussions with the key researcher involved indicates that the findings of the research were intended almost exclusively for use by the Army with respect to the training of soldiers that might have to operate in a wartime setting where atom bombs had been used. In fact, in the final report on the work, it is said that "this is a study of the effects of catastrophe. . . among civilian groups, with the ultimate aim of extrapolation to military situations." Focus of the field work, both among the military and civilians, was on social psychological and psychological aspects of behavior under extreme stress. However, as we will again note later, this exclusively sociologically manned field work produced more theoretical results not part of the original research design with its very specific applied focus.

Civilian disaster situations systematically studied in the field included four tornadoes and a major fire in a college dormitory. By far the major study was a historical reconstruction done five years after the event of the Texas City ship explosion of 1947. The Oklahoma team overlapped in its field work with an NORC and a University of Maryland team to the third Elizabeth, New Jersey plane crash disaster.

(2) The Work at the National Academy of Sciences and the Diffusion of the Research Focus

The pioneering field team operations were followed by the work done at the National Academy of Sciences, first under the label of the Committee on Disaster Studies (1951-1957), and later under the name of the Disaster Research Group (1957-1962). This work involved a variety of different activities ranging from a clearing house operation, to directly undertaking field studies, to holding conferences, to producing a publication series, and to supporting field studies by others outside of the Academy. A reading of the titles from the Disaster Study Series Publications gives a flavor of the multifaceted activities of this Committee and Group.

1. Human Behavior in Extreme Situations: Survey of the Literature and Suggestions for Further Research
2. The Houston Fireworks Explosion
3. Tornado in Worcester: An Exploratory Study of Individual and Community Behavior in an Extreme Situation
4. Social Aspects of Wartime Evacuation of American Cities
5. The Child and his Family in Disaster: A Study of the 1953 Vicksburg Tornado
6. Emergency Medical Care in Disasters, A Summary of Recorded Experience
7. The Rio Grande Flood: A Comparative Study of Border Communities in Disaster
8. An Introduction to Methodological Problems of Field Studies in Disasters
9. Convergence Behavior in Disasters: A Problem in Social Control
10. The Effects of a Threatening Rumor on a Disaster-Stricken Community
11. The Schoolhouse Disasters: Family and Community as Determinants of a Child's Response to Disaster
12. Human Problems in the Utilization of Fallout Shelters
13. Individual and Group Behavior in a Coal Mine Disaster

14. Field Studies of Disaster Behavior: An Inventory
15. The Occasion Instant: The Structure of Social Responses to Unanticipated Air Raid Warnings
16. Behavioral Science and Civil Defense
17. Social Organization Under Stress: A Sociological Review of Disaster Studies
18. The Social and Psychological Consequences of a Natural Disaster: A Longitudinal Study of Hurricane Audrey
19. Before the Wind: A Study of the Response to Hurricane Carla

In a sense, we see here the beginnings of a diffusion of the social science research focus in the disaster area as various tasks relevant to the development of an area of study were initiated.

Funding for the work at the Academy came from several sources, but the Committee work was initially supported until 1955 by the Surgeon General Office of the Army, Navy, and Air Force, and in 1955-1957 by the National Institute of Mental Health, and the Ford Foundation. The later Disaster Research Group work was exclusively financed by the Federal Civil Defense Administration and the Office of Civil and Defense Mobilization. It should be remembered that in the years involved here, prior to 1962, civil defense in this country was basically wartime oriented.

It seems fair to say that insofar as the research supporters were concerned, the major interest was of an applied and wartime nature. In fact, the Offices of the Surgeon Generals, in its statement to the National Academy of Sciences, had requested a program be initiated to conduct research and monitor scientific developments related to "problems that might result from disasters caused by enemy action." There was eventually a shift away from a direct military interest per se with the involvement of the federal civil defense organizations in supporting the work of the Disaster Research Group, but in one sense the basic thrust remained the same insofar as research sponsorship was concerned. The leadership in the Committee and the Group during most of its existence at the Academy was social science oriented and this had important consequences both inside and outside the Academy as we will discuss later. Even after the key leaders (Harry Williams and Charles Fritz) had left it is possible to read that the first annual meeting of the Group's OCDM-NRC Advisory Committee on Behavioral Research had as its objective "to stimulate both within and outside of the Office of Civil and Defense Mobilization behavioral research that will contribute to the Nation's civil defense." Given the kind of leadership left in the last two years and this kind of goal, it is perhaps not by chance that disaster work in the National Academy of Sciences had stopped within two years.

- (3) The Establishment of the Disaster Research Center (DRC) and Its Deepening of Work in the Disaster Area.

The Disaster Research Center was established at Ohio State University in the fall of 1963 (DRC only moved to the University of Delaware in 1985).

That year, the Office of Civil Defense (OCD) gave the Center a rather large contract to initiate field studies of organizational functioning in disasters. It was explicitly stated that the field work was to be on civilian or peacetime disasters. But OCD's interest, and this was at least communicated to DRC, was in extrapolations from peacetime emergencies to wartime crises. In the research proposal itself from DRC to OCD (which had been indirectly discussed before formal submission) the wartime interest was only specifically alluded to in objective E of the proposed work (the only objective added at the explicit request of OCD). The introductory statement about objectives read:

The General Proposal

It is proposed that there be established at The Ohio State University, a Disaster Research Center. The Center would focus on the study of organizations experiencing stress, particularly crisis situations. Generally speaking, the Center would have five major objectives:

- A. To collate and synthesize findings obtained in prior studies of organizational behavior under stress.
- B. To examine, both by field work and other means, pre-crisis organizational structures and procedures for meeting stress.
- C. To establish a field research team to engage in immediate and follow-up studies of the operation of organizations in disaster settings, both domestic and foreign.
- D. To develop, in coordination with a concurrent project, a program for field experiments and laboratory simulation studies of organizational behavior under stress.
- E. To produce a series of publications on the basis of these four objectives, with special emphasis on recommendations concerning the effective emergency operations of organizations and other matters pertinent to civil defense planners.

It is not an accident that the fifth objective is only stated in this part of the proposal and, unlike the other four objectives which are discussed in great detail later, is not even alluded to anywhere else in the proposal. (This essentially reflected the real interests of the sociologists who wrote the proposal).

Irrespective of how the proposal may have read, there was no question the study was being supported only because of what it might say about a wartime situation. In actual fact, it could not have been otherwise, because at that time, OCD, as a federal agency, was actually prohibited from direct participation in planning and/or response to civilian emergencies (the civilian area was the province at the national level of

the Office of Emergency Planning (OEP), which significantly was not supporting any studies of peacetime disasters).

A few months after obtaining the contract from OCD, DRC received a grant from the Air Force Office of Scientific Research (AFOSR) to undertake laboratory or experimental studies of organizations under stress (what is alluded to in objective D of the OCD contract). This research was primarily and clearly seen as having possible consequences for military organizations. No idea was ever expressed that the Air Force had any interest in the results because they might be applicable to civilian agencies or peacetime disasters. How closely it was viewed, as related to Air Force interests is perhaps indicated by the fact that the grant was terminated in about five years, not because the research results were seen as not valid or uninteresting, but because the research as a whole was evaluated as not enough "mission oriented," that is, of very direct relevance for the operation of the Air Force.

DRC did continue to do research along the lines which had been initiated by the earlier pioneering field teams. The Center did build upon some, although not all, of the various disaster-related tasks originated in the research diffusion undertaken by the National Academy of Sciences. Namely, DRC initiated its own publication series and used the archives of the Academy Group to start creating a specialized social science disaster research library. It also, for the first time, deepened research in the disaster area by its continuous and concentrated studies on the planning and response, especially of emergency organizations at the local community level. It should be noted that most of these activities, for example, the publication series and the specialized library, were initiated by DRC. Directly, neither was supported by either funding or any material support from OCD or the AFOSR. Even the deepening of a research focus on organizations was also a DRC initiative, for along certain lines, OCD seemed more interested in social psychological rather than social organizational problems. Put another way, many of the Center's activities were the result of the actions and decisions of the three sociologists who founded DRC. The funding agencies at that time were almost exclusively concerned with the wartime or military organization extrapolations that could be made from peacetime or civilian groups. That overtly was their rationale for providing funding for disaster studies and they had no interest in directly supporting the Center in doing anything else. (It was about a decade before OCD began to exhibit an interest in peacetime disasters.)

To further document the wartime orientation of OCD, we cite a statement covering the 1962 fiscal year (the year before DRC was established). The reference is to the "Social Sciences research program" in OCD. It is said that:

The Social Sciences research program is responsible for (1) developing knowledge of the effects of war and tension upon society and its institutions; (2) determining the reactions of people to conditions before, during and after attack; (3) providing data for developing measures such as shelter, evacuation, and dispersion, for protecting the population; (4) developing data for planning relief and rehabilitation programs,

embracing essential community and government functions;
(5) determining effective means of securing active cooperation of people in promoting civil emergency planning measures throughout the nation.

There is no mention of civilian disasters anywhere in the 25-page summary of past and present social sciences research conducted by the former Office of Civil and Defense Mobilization and the Office of Civil Defense, Department of the Army.

Thus, in the first decade or so of disaster studies in the United States, the federal agencies supporting the research were primarily interested in wartime and/or military applications. There was no noticeable interest in civilian disasters per se, their study was undertaken to see what could be learned that could be extrapolated to a wartime or military setting.³ To the extent that the sponsoring agencies had any disciplinary leanings, they were psychiatric, psychological or, at best, social psychological, rather than sociological. As for the implicit model of behavior under stress they operated with, it appeared to be one of personal breakdowns in disasters. The agencies also assumed that the purported problems that emerged in disasters were to be found in individuals, and the solution to such problems rested mostly in the imposition of directive social control (the command and control model which still prevails in the field today).

As we have already indicated, and will detail later, the assumptions and interests of these funding agencies were increasingly being altered by the researchers, mostly sociologists, who actually carried out the research projects. Nonetheless, the applied orientation of the research funding agencies did have some important consequences.

Some Important Consequences of the Applied Focus

There were major consequences in the work done in the disaster area which resulted from the applied orientation of the sponsoring agencies. It is important to note that, as a whole, whatever influences there were from the research sponsor, they were indirect, not direct. This is true despite the fact that most of the funding for the research was of a contract, rather than grant nature, which might imply much directional and substantive control and supervision by the sponsoring groups and their officials. However, our impression is that there was very little effort made to direct what should be studied and/or how it should be studied.

The DRC's initial contract with OCD, for example, was the identical substantive proposal the Center had first submitted as a grant application to the National Science Foundation, except for the addition of objective #5 (see page 7). Informally, it was also understood that DRC should add a concluding chapter with the same theme to reports it was writing about the behavior and problems of different kinds of emergency organizations in natural disasters. The only administrative change in the shift from a grant to a contract proposal was that, at the suggestion of OCD, a substantial increase in both funds and duration of the project was requested and allowed. At no time in the early days of the work did OCD attempt to dictate anything of a substantive nature. The only major problems that

arose was OCD's refusal to allow a DRC publication from OCD funds on the operations of the American Red Cross in disasters. The disallowal of publication stemmed from National Red Cross objections to publishing the Center's observations that Red Cross disaster operations were negatively viewed by other organizations and the public at large. For political reasons, OCD did not want such a finding, which was well documented in the DRC work, to appear in a publication from research it was funding. The Center was eventually able to publish the study results under its own auspices.

As far as we know, all the other early studies by other groups which we have mentioned, likewise, were not subjected to any direct pressure or control. It may be that DRC and the other researchers escaped direct control because usually contract funding was provided for the study of very broad topics such as organizational functioning in disaster. Another possibility is that perhaps the lack of any knowledge about the subject matter on the part of the sponsoring groups provided freedom from direct control or supervision. Our guess is that something more important was operative which allowed considerable freedom from sponsor control. It is that the sponsored research, at least in the early days, was primarily commissioned for reasons other than seeking answers to practical problems. It could be argued that disaster research was initiated (and the initiation came from the agencies and not social scientists) because of internal bureaucratic pressure for agencies to be current with the post World War II phenomena of social science research being on the agenda of many government agencies. Whatever was involved, the sponsoring agencies, military for the most part, and contrary to certain images which developed in the late 1960's, directly dictated very little, if anything at all, in the disaster research area.

However, while the applied orientation of the research sponsors did not lead to direct control or guidance in the research that was done, there were, nonetheless, a number of indirect consequences. Let us mention just three of them. Any one of them alone has had, in our view, important effects on the work done in the last 35 years in the disaster area.

(1) The very conception of what constitutes a disaster was strongly influenced by the applied orientation. Thus, both at NORC and DRC, the prototype of a disaster was visualized, sometimes explicitly, as a major earthquake. In terms of possible extensive impact over a wide area, the sudden and unwarned occurrence of an earthquake was seen as being closest to a bombing attack on a community.⁴ Just as the area of collective behavior has, unfortunately, tended to use the crowd as a prototype of collective behavior, so the disaster area has possibly, also unfortunately, used a sudden, no warning, wide in scope and extensive in damage, etc., earthquake as the prototype of disasters.

It is only possible to speculate, but one cannot help but feel that substantive social science work on disasters would have developed remarkably differently in the last 30 years, if, for example, such diffuse emergencies as famines or droughts or epidemics or even large scale riverine flooding had provided the prototype of what constituted a disaster. We in the disaster research area early implicitly accepted a conception of disaster as a particular kind of event concentrated in time and space, and for various reasons have avoided, until very recently,

facing up to the serious problem of not being at all clear or certain about the core and parameters of what we are studying under the label of "disaster." As will be discussed later, we do not think we can advance significantly on further studies on disasters until we move forward on the conceptual problem. Just as we are hung up in the collective behavior area by taking a very concentrated happening in time and space, such as a crowd, which is relatively infrequent compared to all other collective behavior happenings which are diffuse in space and time, in the disaster area we do the same thing by focusing on one kind of concentrated space-time event, which is also relatively infrequent when compared with other mass emergencies that are usually much more diffuse in time and space.

It should also be noted that, contrary to what has sometimes been said in the literature, the disasters which could be studied by the pioneering field teams included others than those involving only natural disaster agents. All three of the field team operations actually studied explosions, fires, crashes, and other concentrated in time and space human created occurrences. Neither the Academy work or the early DRC work included only natural disaster agents. It is true relatively few non-natural disaster situations were studied, but this appears to be more a function of what occurred during the course of the research periods involved than a deliberate focus only on natural disasters. Our point here is that the restraining conceptual die of disaster as a concentrated time and space occurrence was created at the time of the origin of research in the area, and it was, to a great extent, implicitly and indirectly produced by the applied wartime orientation of the early sponsoring agencies.

(2) The early focus on the emergency time period and on the emergency response in disasters is also, we think, a partial result of the early applied orientation. If war or a military situation is thought of as the generating context, it follows that emphasis in research will be on reaction, not prevention. That the field of geography got around to considering mitigation measures and such issues as land use as part of natural hazard research problems (and the difference in focus on something called "disasters" and on something called "natural hazards" is neither an accidental or unimportant matter in our view) far before sociologists addressed such matters, may be partly a function of disciplinary differences, but we suspect it also has something to do with who initially sponsored studies by sociologists on disasters and by geographers on natural hazards.

The almost complete neglect by the early disaster researchers of the longer run post-impact recovery activities can also be partly attributed to the interests of the funding agencies. DRC did do some longitudinal studies of organizational long run recoveries from disasters, but they had to be done independent of OCD support (see Anderson, 1969). It is not that there was any objections to such studies; in fact, some OCD funding was used to obtain the relevant field data, but there simply was not interest in the results. This matter, of course, is also not independent of the funding cycles and inabilities of most governmental bureaucracies to commit themselves to support for more than one fiscal year at a time. Studies of recovery would usually have to go considerably beyond one post-impact year.

(3) The related emphasis in early studies, and to this day on planning for, instead of managing disasters, we also believe is an indirect

consequence of the applied orientation of the early funding agencies. The early disaster researchers assumed that they needed better knowledge of what happened in disasters so that better planning for disasters could be instituted. To a considerable extent, we believe this reflected the similar bias in perspective of the military or national civil defense sponsoring agencies, both of which spend a great deal of time, effort and resources on planning for events with low probabilities for occurrence. Management of the military in wars, or of civil defense responses in disasters, is not a frequent occurrence.

There is a difference between disaster planning and disaster management, a crucial distinction still little appreciated even though it took us only 30 years to grasp its significance (Quarantelli, 1965)! The latter does not follow automatically from the former in the same sense as that good tactics do not follow directly from a good strategy. Management, of course, deals with actual happenings, and good managing is what is needed for efficient and effective response and recovery, and, while it does not, and cannot replace planning, it probably needs an equivalent emphasis. Such an emphasis was not present in the early days of disaster research, and it was unlikely to be to the extent researchers reflected the bias of their supporting funding sources. The emphasis on planning also partly reflects a "command and control" model for handling emergency time problems. While disaster researchers, extremely early, criticized "command and control" conceptions of disaster response, none of them essentially challenged the primacy and almost exclusive focus on planning.

We do think it is illustrative of our point that, in DRC's early days, a formal DRC proposal to study the operations and management of the United States Office of Foreign Disaster Relief and an informal one to study the operation and management of the Office of Emergency Planning (OEP) were rejected out of hand. But DRC had little difficulty in obtaining funds to study community emergency planning. The matter, of course, is a complicated one, and even in the examples given, for a variety of reasons, it might be understandable why research into local agencies might be seen as more acceptable than study of national organizations, apart from a preference for a focus on planning than on management. We think the preference needs to be accounted for, and we think it partly has its roots in the early days of disaster research.

There were other indirect consequences for disaster research that, perhaps, stemmed as much from the fact that the sponsoring agencies were American as that they had an applied orientation. Thus, there was an almost necessary focus, not only on the kinds of disasters which occur in American society (e.g. tornadoes rather than famines), but also on relatively small scale and minor impact disasters (compared with the massive casualties, losses and disruptions which occur in some disasters in Latin America, Asia, or Africa). Some of the funding agencies allowed and supported overseas studies by the first American researchers. The events studied, such as floods in Holland, massive fires in Australia, and dam collapses in Italy seemed to be researched because of a perceived similarity or a parallel to potential wartime situations rather than because they might be a learning situation for a potential peacetime catastrophe in the United States (we leave aside that field studies outside of the country might also have been partly supported for totally nonscientific reasons).

The general focus on American disasters also meant that only a certain kind of social structure was studied by the early disaster researchers (e.g., one with a decentralized authority structure, with relatively weak social class differences, and with highly developed social institutions, such as in the mass communication area). For instance, the almost total ignoring of social class as a factor in any way in disaster phenomena is certainly partly attributable to the locus of study used (Taylor, 1978). Similarly, disaster research tended to look at a population with certain sociocultural characteristics (e.g. norms regarding volunteering, beliefs as to governmental responsibilities, values with regard to private property, etc). From this, for example, probably has come some of the concern of American disaster researchers about the citizen's view of emergency organizations.

Our point, of course, is that certain topics have been either focused on or ignored in disaster studies and that this indirectly is related to the applied research funding pattern in American society. To the extent that agencies with strong applied orientations of a particular kind emerged as the research funders rather than governmental organizations supportive of basic research (and it should be remembered that the initial DRC proposal went to NSF not OCD), indirectly there is going to be a reflection of this in what is assumed, studied and reported on by researchers. The applied agencies did not directly dictate much of anything, but, indirectly, from the start they have implicitly provided much of the research agenda and, like all agendas, the one that initially sets the stage became the one that tended to be continued to be used.

The Sociological Input

Although the applied orientation of sponsoring agencies looms large in our accounting for much of what has happened in the development of disaster studies, to leave it at this point would be to present an incomplete picture. Probably equally as important in the development of the area, is the fact that the early students in the area were primarily sociologists. To a considerable extent, they imposed much more of a sociological perspective on how and what was studied than is realized by practically anyone. In our view, the applied orientation was married to basic sociological conceptions and ideas, although neither the research supporters nor the researchers were very aware of it at the time, and most still do not recognize the situation is the same today.

First, it is important to note that just in terms of numbers and the holding of key administrative research positions, sociologists predominated in the early days of disaster studies. Both the first nominal head of the NORC project (Shirley Starr) and the actual everyday operational head (Fritz) were sociologists. The rest of the professional staff were from a variety of disciplines, but all five researchers who played a major role in the analysis of the Arkansas data were sociologists, with one exception.

The University of Oklahoma study was nominally headed by two sociologists with the key field team researcher being another sociologist (Killian). Only in the Maryland study were sociologists not represented; but the key person there was a social philosopher rather than an empirical researcher.

The subsequent National Academy of Sciences work was headed always 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 ever employed on a full time basis by the Academy. 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. Of the 19 major disaster publications issued by the Academy, 13 were authored or co-authored by sociologists such as Moore, Bates, Killian and Barton (and three others were done by anthropologists).

Finally, DRC was founded by three sociologists, was 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 National Academy of Sciences Disaster Research Group (Quarantelli, 1981). To the extent they had a sociological tone, DRC also had one.

Not only were the majority of the early workers in the disaster area sociologists, but many, in our view, also shared three other characteristics. One, a number of them had a major professional interest in the sociological subspeciality of collective behavior. Two, 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 general perspective, as well as certain specific theoretical sociological orientations. These matters all had important consequences for the way the early stages of disaster research developed in this country.

There never was a survey 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 Killian, Fritz, Bucher, and Schatzman, among others, shared our professional interest in the area of collective behavior. This is important for a variety of reasons. As a group, collective behaviorists could, and did, resist viewing disaster phenomena as involving social disorganization or within the province of social problems. To most of them, crisis situations represented instances allowing for the emergence and creation of new behaviors. 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 (see Aguirre and Quarantelli, 1983), and those involved in the disaster area shied away from such formulations or characterizations in approaching disaster phenomena, even including panic behavior. Finally, collective behaviorists tend to see collective or group phenomena rather than individual behavior. They also did so in the disaster area.

Our general point is that the bloc of collective behavior specialists involved in the early disaster studies acted as a major barrier to even certain sociological ideas because they ran counter to the intellectual biases of the speciality. Conversely, other labels or formulations were more likely to be advanced because they better fitted the intellectual orientation of collective behavior. It could be argued that the empirical evidence dictated the avoidances and the 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. To the extent this was operative, the disaster area was directed along certain lines rather than others because collective behavior specialists, rather than specialists in deviancy or social problems, for example, were heavily involved in the initial stages of disaster research.

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. One formal attempt to start creating a science of disasters found no supporters (Moore, 1956), 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 35 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 both have now been created in the last five years).

The influence of the sociologists and their commitment to sociology is also indicated by the fact that 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 some initial steps were taken to recruit representatives of different disciplines into the team. This quickly proved disruptive of team work, but soon became a moot issue as the work proceeded and a sociological view of disaster phenomena came to the fore. The University of Maryland group was supposed to be composed of at least representatives from psychiatry, sociology, medicine, and psychology, but it proved impossible to assemble such a team, the core of which from the start of the work was a social philosopher. The National Academy of Sciences operation was also supposed to be multidisciplinary, both internally and externally. As Harry Williams, its director, once 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 may 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. (1954:6)

Structurally, the Committee on Disaster Studies was in the Division of Anthropology and Psychology, but non-sociologists played only minor roles in the actual makeup 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 ten members of the executive council of the OCDM-NRC Advisory Committee on Behavioral Research). Also, while the Academy did fund the outside work of non-sociologists, as already indicated in our discussion of the publications from the Disaster Research Group, 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 Academy, DRC never did 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 a multi or interdisciplinary approach.

The sociological perspective that was brought to bear by the early disaster students consisted both of a 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 the 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 sociologists this might seem obvious, but it is not the assumption, for example, of psychologists. It is noticeable that when geographers first entered into hazards or disaster studies, they went in the direction of psychology, rather than sociology. Thus, the early and current concern with geographically oriented disaster researchers was with attitudes of people, with perceptions of risk analysis, etc, approaches and topics almost ignored by sociologists in the disaster area. We mention it to illustrate that disaster studies almost certainly would have had a rather different general explanatory content if the field had been developed by psychologists and not sociologists and that this has to do not with empirical data, but disciplinary assumptions.

In this gathering of sociologists, we do not have time to consider that an approach to natural hazards was developed in geography in the late 1960s, which has resulted in a somewhat different view of the nature of disaster phenomena. A sociology of knowledge and historical examination of the "geographical" approach to natural hazards and disasters would be worthwhile, especially if the analysis took into account the features of the parallel although somewhat earlier initiated development of the "sociological" perspective we are discussing in this paper. 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. There has been a geographical approach to disasters even though there has been nowhere as much of a development of a "psychological" approach to disasters (some very early formulations, such as by Wolfenstein, 1957, were much more psychoanalytical than psychological).

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, it appears this led the early disaster researchers in certain directions and away from others. Thus, sociologists doing disaster research have seldom been comfortable

conducting attitude studies of how people think they might behave in an actual disaster. On the other hand, the sociologists first working in the disaster area on such issues as warning quickly and consciously brought to bear W. I. Thomas' dictum that "if a situation is defined as real, it is real insofar as consequences are concerned." 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 see as functionally equivalent. In fact, most non-sociologists do not even understand the point being made and attempt to keep defining and conceptualizing 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. Most of the early disaster researchers to the extent they have operated with any social psychological model, used that from symbolic interactionism.

In fact, so pervasive was both collective behavior and symbolic interactionism in the perspective of many of the early disaster workers, that they seldom made explicit their assumption of such particular theoretical perspectives. This has led non-sociologists, and even 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 non-theoretical (Mileti, Drabek and Haas 1975:146). The opposite is far truer (see also, Wenger, 1986). The two theoretical perspectives 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 within sociology was not often entertained.

A few scattered efforts to advance other theoretical perspectives got nowhere. At NORC, an effort to bring the structural functional theory of Talcott Parsons to the fore was almost totally rejected, although, oddly, the classic definition of disasters advanced by Fritz (1961), and recently revised in a minor way by Kreps, had its root origins in Parsonian theory rather than the symbolic interactionism which predominated in the NORC group.

Other particular theoretical ideas in sociology which influenced the early disaster researchers were drawn from various parts of the sociological literature including Weberian notions of formal organization and from what sometimes has 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. Nonetheless, the sociologists first involved in disaster research were not unaware of the roots of these particular views in basic sociology. For example, the notion of "role conflict" was advanced a long time ago by Killian (1952) in conjunction with his work on the University of Oklahoma field team, but it was not until this year that discussions of role conflict in disasters were squarely placed into the extensive body of literature that exists on the topic (Dynes and Quarantelli, 1986).

If there is a question 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. It is possible that because the early students frequently were communicating with very small

numbers of like minded colleagues, there was no felt need to assert explicitly the theoretical ideas involved. 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 areas of collective behavior and symbolic interactionism. Perhaps the somewhat inductive nature of much field research in the disaster area discouraged 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 disaster researchers were field workers who had very direct 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 1952 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 is 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 to existing theory in such areas as perception, learning, stress, reference groups and social organization. (1954: 66-67)

In discussing these matters, our point is to emphasize that a sociological perspective and basic sociological idea 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 not often 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 disasters. 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 careers also worked on non-disaster topics, with not a few attaining recognition as contributors to sociology in these other areas.

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, and we have suggested, we can still see some of those consequences in the research undertaken today. The sociological orientation of the early disaster researchers also had important consequences, and these too still partly affect studies at the present time. It seems incontestable that if initial research in the disaster area had been carried out by other than sociologists (especially by sociologists with particular sociological biases), the field of disaster research would now be substantively rather different.

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

consequences 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 up to now 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 worthwhile enough, so that those with applied concerns have been willing to provide fairly constant funding. For example, when the pioneering field team efforts phased out, the succeeding Academy work was supported. Then DRC has had in principle, if not in fact, unbroken continuous contract support starting with OCD through successor agencies on through FEMA. Similarly, in recent years, other agencies with applied interests have provided some support for various researchers; these include NIMH, the Administration on Aging, the Health Resources Administration, the Department of the Interior, and EPA. Less of a case for research support had to be made with these agencies, given that earlier governmental support for disaster research 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 generation of a critical mass of researchers at the present time. In turn, the existence of a critical mass not only indicates, but also allows, the institutionalization of the whole field of social science studies of disasters. This is discussed and documented in great detail in a volume edited by Dynes and Pelanda and entitled Sociology of Disaster: Contributions of Sociology to Disaster Research (1986).

It is true that now a government funding agency with a more basic science orientation is in being, namely, the National Science Foundation (NSF). Actually, of course, the research support for disasters from within NSF has always had some roots in an applied base. But more important, it is doubtful any NSF unit with an interest in disasters would have ever been established without the results of what had been done elsewhere earlier as a result of applied interests and research support.

We think the basic sociological infusion has prevented the field of disaster studies from going down unfruitful paths with respect to such matters as accuracy of conceptualizations and priorities for research. Sociological notions have, for instance, called attention to questions and issues which might have otherwise been ignored, and provided non common sense interpretations of empirical findings. 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 us any clues for the future? Our very brief reading of the last two decades or so reveals that applied concerns still provide the impetus for funding and support. Certain topics and questions therefore continue to be implicitly attended to while others are ignored. Sociologists, while perhaps no longer a majority but still a plurality of the disaster researchers, continue to approach their work as sociologists, and therefore, the same pluses and minuses exist as in the early days of the area. Put in other words, the general stance of disaster studies is still more or less the same as it was when the area started. The work is methodologically more complex and somewhat more theoretically explicit, but no researcher from the 1950s would have any trouble understanding the "what" and the "how" of the area in the 1980's (although the converse is not always true). It is perhaps rather telling along certain lines that the NORC survey of the Arkansas tornadoes in 1952 is still the best sample drawn of any disaster impacted population, 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 1986.

This is the way we see the situation in the field at present. It is the result of what has happened in the past. But is the way the past developed the best way for future development in the area? (We leave aside here, for reasons indicated earlier, the probable continuing development of the "geographical" approach to natural hazards.)

Scholars working on sociology of science and knowledge problems 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 sometimes work in an area then flattens out unless some major reformulations occur. In a very rough sense, we think this is where we now stand in the sociological study of disasters. Three or so 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. Our sense of the field is that unless some major changes are soon undertaken in the field, we will soon 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 contrary to that of many others, is that we should not primarily think that the path to follow is in the direction of tighter research designs or more quantitative kinds of studies. Rather, we should be attempting to reformulate the basic concepts and theoretical models we currently employ, most at an implicit level.

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 interpretations of empirical data (see, for example, Quarantelli, 1985a; Sowder, 1985). More or even better data

or different empirical studies will not resolve those controversies. There would be some advances if some consensus was attained 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 as the way by which science develops, we would not deny that our view here is close to his (1962).

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?

The current efforts in this country by Kreps (1984), Drabek (1986) and ourselves (Quarantelli, 1982), and elsewhere by other sociologists such as Pelanda in Italy, Britton in Australia, and Dombrowsky in Germany to better define and conceptualize "disaster" is to us a harbinger of where the field as a whole might most fruitfully initiate a major reformulation of that which goes under the name of disaster studies.

The definitional problem is not confined strictly to research issues. As co-editor of the journal, Mass Emergencies and Disasters, we can say that the question of what is a disaster has increasingly become an issue as to what such a journal should publish and review in its pages. In fact, the very title of the journal reflects an initial ambiguity as to subject coverage and the term "Mass Emergencies" was included partly to reflect the uncertainty the disaster researchers who founded it, as to what their field did and/or should cover.

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?

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 as FEMA, NIMH, or 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 support anything that might be considered of a more basic nature. In principle, there would be some possibilities available in the National

Science Foundation, but we believe the very existence of a somewhat applied disaster unit within NSF actually precludes disaster research obtaining more funding from the basic sociology component of the agency. So we see little change in the short run.

As we have already indicated, we do think some major change is necessary in the long run. What are the possibilities in the long run? 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) would almost seem to demand attention to fundamental questions about the similarities in the full spectrum of emergencies including natural disasters, technological accidents, resource shortages, wartime attack on civilians (and possibly terrorism and arson) for which the system is presumably designed. How can IEMS be implemented, taught, trained for, etc. if the core and outer boundaries of the phenomena for which the system has been generated, is not conceptually clarified. Also, as 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 reading 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 basic sociology? In some ways, the conference at which this paper is being presented might suggest optimism. For whatever reasons, a number of sociologists prominent in the sociological theory area and who up to now probably were not even aware of the disaster research area, are bringing their knowledge and intellects to bear on the question of what basic sociology might contribute to the disaster area. Totally apart from this, there is a book soon to be published which examines what sociology has and could further bring to the study of disasters (Dynes and Pelanda, 1986). 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 more such activities. It seems unlikely sociologists from outside disaster research will continue to have an interest in the area. As for sociologists within the disaster field, apart

from the fact few show much inclination to do other than empirical studies, those interested are unlikely to find much support for their interest in conceptual problems. We leave aside larger matters such as, that sociology, according to many such as Howard Becker, 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 on for much help in conceptual clarification when it cannot even clear up its own basic focus.

So, while we think some basic changes are necessary in the disaster area, 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 the best that can be said is that this conference is a step forward and as a Chinese proverb says, a journey of a thousand miles has to start out with a first step. Maybe we have taken this first step today.

NOTES

1. Unless otherwise indicated, all quotations, citations and references in the rest of the article to people and activities are drawn from unpublished and not publically circulated documents, papers, memos, questionnaires and other generally non-printed material in the archival collections of the Disaster Research Center. Since almost all such material is very fugitive, with much only available in the DRC collection of archives, no references to specific items will be provided. Anyone interested in specifics can contact the DRC library for further information.

2. The Army Chemical Corps never had an opportunity to use its chemical weapons during World War II. Thus, its interest in the disaster area could be interpreted as an attempt by the organization to carve out a new future role for itself. Possibly more important, was simply the widespread impression in the American military that the civilian population of the United States had never experienced a major external bombing raid, and, therefore, there was consequent fear that civilians would react badly to future wartime attacks that might involve the dropping of atomic bombs.

3. Such explicit statements as were made about extrapolation from civilian disasters to wartime situations almost always stressed that concern was with how the American population could be better prepared to withstand attacks from enemy sources. This position is well stated in remarks by Williams:

Social science has been presented with several great challenges since World War II. Understanding the problems of technologic assistance to underdeveloped countries is one of these. Understanding psychocultural warfare and the true nature of subversion is another. A third great challenge is to develop a scientific understanding of the human effects and problems of disasters, both present and potential.

One reason why this should be so is clear: American cities can now be attacked with the weapons which have led to dubbing our time the "age of mega-deaths." Such a prospect presents staggering problems--ranging from how to foster the most adaptive possible responses by threatened or stricken populations and how to care for millions of casualties and homeless persons, to the prospect of large-scale social, economic, and demographic reorganizations, if our urban complexes are gutted. Fundamentally, it has become necessary to know how Americans react to disaster and how they deal with it. (Williams, 1954:5)

However, it is possible to find some occasional references among funding agencies to an "offensive" rather than "defensive" use of extrapolations from peacetime to wartime situations. Thus, in one rare written statement it is said that:

Not only do we need to know how to protect our soldiers and populace against the psychological ravages of an attack using chemical agents; in addition, we must know how to exploit to the utmost the psychological effects of toxic agents when used against an enemy.

Nonetheless, it is important to stress that we are unaware of any instance in the past up to the present of where funding agencies have attempted to spell out the "offensive" possibilities. We have never encountered even an indirect reference to such possibilities in the disaster research literature per se. In fact, such use of research would be radically at variance with the ideological liberal or left tendencies of the large majority of American social scientists, especially sociologists. Nevertheless, all scientific knowledge can be put to "good" and "bad" purposes and it would be foolish to deny that disaster research could not also be used both ways. While this possibility does not seem to have affected researchers involved in studies of natural and technological disasters, the possibility has discouraged some students of collective stress situations from studying "terrorism." Although it is not our position, it is possible that some such researchers may also be reluctant to expanding the disaster area to include "war" phenomena for the same reason.

4. Powell, the major researcher in the University of Maryland pioneering field studies, in a little noted article, raises an interesting speculative question in this connection. He wrote:

As has been suggested, American urgency about disaster study grows out of our uncertainty about how we will act if war is ever brought directly into our continent: modern war, especially atomic war. Our anxiety over our own prospective performance is, I think, demonstrated by the spotty and perhaps guilt-motivated concentration on disasters approximating atomic explosion. (If we had dropped nerve gas or a virulent toxin on Japan, what would our focus of study be now?) (Powell, 1954 :61)

REFERENCES

- Aguirre, B, and E.L. Quarantelli
 1983 "Methodological, Ideological, and Conceptual Theoretical Criticisms of the Field of Collective Behavior." *Sociological Focus* 16:195-216
- Anderson, William
 1969 *Disaster and Organizational Change. Book & Monograph Series #5.* Newark Delaware: Disaster Research Center, University of Delaware.
- Barton, Allan
 1969 *Communities in Disaster.* Garden City, N.Y.:Anchor
- Drabek, Thomas
 1986 *Taxonomy and Disaster: Theoretical and Applied Issues.* Unpublished paper.
- Dynes, Russell & Carlo Pelanda (eds)
 1986 *Sociology of Disasters: Contributions of Sociology to Disaster Research.* Milan, Italy: Franco Angeli.
- Dynes, Russell & E.L. Quarantelli
 1986 "Role Simplification in Disaster." Pp 23-37 in *Role Stressors and Support for Emergency Workers.* Washington, D.C.: US Government Printing Office.
- Fritz, Charles
 1961 "Disasters." in R. Merton and R. Nisbet (eds.) *Social Problems.* New York: Harcourt, Brace and World.
- Killian, Lewis
 1952 "Some Accomplishments and Some Needs in Disaster Study." *Journal of Social Issues* 10:66
- Kreps, Gary
 1984 "Sociological Inquiry and Disaster Research." *Annual Review of Sociology* 10:309-330.
- Kuhn, T.S.
 1962 *The Structure of Scientific Revolutions.* Chicago: University of Chicago
- Mileti, Dennis, Tom Drabek, & E.J. Haas
 1975 *Human Systems and Extreme Environments.* Boulder: Institute of Behavioral Science, University of Colorado
- Moore, Harry E.
 1956 "Toward A Theory of Disaster." *American Sociological Review* 21:733-737.
- Powell, John
 1954 "Gaps and Goals in Disasters Research." *Journal of Social Issues* 10:61-65.

Quarantelli, E.L.

- 1981 "A Response to the Description and Evaluation of the DRC Work in the Disaster Area." Pp. 122-135 in Peter Rossi, et al. Social Science and Natural Hazards. Cambridge, Mass.: Abt Books.
- 1982 "What Is a Disaster?" Pp. 453-478 in Barclay Jones and Mike Tomezevic (eds.) Social and Economic Aspects of Earthquakes. Ithaca, New York: Program in Urban and Regional Studies, Cornell University.
- 1985a "An Assessment of Conflicting Views on Mental Health: The Consequences of Traumatic Events." Pp. 173-215 in Charles Figley (ed.) Trauma and Its Wake: The Treatment of Post-traumatic Stress Disorder. New York: Brunner/Mazel.
- 1985b The Functioning of the Local Emergency Services Offices in Disasters. Final Report #32. Newark, Delaware: Disaster Research Center, University of Delaware.

Sowder, Barbara

- 1985 Disasters and Mental Health: Selected Contemporary Perspectives. Washington, D.C.: United States Government Printing Office.

Taylor, Verta

- 1978 "Future Directions for Study." Pp. 251-280 in E.L. Quarantelli (ed.) Disasters: Theory and Research. London: Sage.

Wenger, Dennis

- 1986 The Role of Archives for Comparative Studies of Social Structure and Disaster. Preliminary Paper #112. Newark, Delaware: Disaster Research Center, University of Delaware.

Williams, Harry

- 1954 "Fewer Disasters, Better Studied." Journal of Social Issues 10:5-11.

Wolfenstein, Martha

- 1957 Disaster: A Psychological Essay. Glencoe, Illinois: Free Press.