

**DISASTER STUDIES: AN ANALYSIS OF
THE SOCIAL HISTORICAL
FACTORS AFFECTING THE DEVELOPMENT
OF RESEARCH IN THE AREA***

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Almost nothing has been written about the social historical emergence and development of social and behavioral research on disasters. This paper provides a description and a sociology of scientific knowledge analysis of the factors affecting the initiation of studies in the area in the United States. First, we note how disaster research on group and behavioral aspects of disasters had their roots, almost exclusively, in rather narrowly focused applied questions or practical concerns. Second, we point out how this led to certain kinds of selective emphases in terms of what and how the research was undertaken in the pioneering days, but with substantive consequences which we still see operative today.

Very little has been written about the history of social science disaster research, the factors which have influenced the emergence of this field of study, and the ensuing theoretical and methodological consequences for scientific work on the human and group aspects of disasters (for passing observations, see, Fritz 1968; Quarantelli 1972; Quarantel-

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li and Dynes 1977; Quarantelli and Wenger 1985; and Drabek 1986). In fact, apart from some of my earlier writings (Quarantelli, 1981) in response to about the only systematic effort ever made to examine some of the conditions involved in the development of the area (Kreps 1981), almost no one else has written at length or in depth on the topic. The field is only a little more than three decades old which is not much but long enough both to allow and warrant an examination of the problem.

In a meeting in 1986 that focused on the relationships between basic and applied sociological research and disaster studies, we made four major points. First, we noted that disaster studies on behavioral and group aspects had their initial roots, almost exclusively, in rather narrowly focused applied questions or practical concerns. Second, we pointed out this led to certain kinds of selective emphases in terms of what and how the research was undertaken, with substantive consequences which we still see operative today. Third, we observed that nonetheless a basic sociological orientation and sociological ideas implicitly permeated much of the early research work and many of the answers that were offered. Fourth and last, we argued that the research approach initiated with a mixture of applied concerns and basic sociological questions, and continued now for about 35 years, has had primarily positive functional consequences on the development of the field of study of disasters.

In this article we elaborate only on the first two major points; points three and four are discussed in a related article (Quarantelli forthcoming). We essentially take a sociology of science approach to the problem, especially as has been developed in an offshoot of that orientation, namely the sociology of scientific knowledge (for the difference between the two see Tibbetts 1986). This kind of approach to the production of knowledge assumes that the social context of research activities is equally as important if not more so than empirical data in influencing the growth of a field of study (see e.g., O'Neill 1981). This is at variance with the ideal but non-realistic notion that research findings or empirical observations are the prime movers in theory, model building or other scientific development (see e.g., Mannheim 1936; Kaplan 1964; Kuhn 1970; Johnson 1975). As such we try to emphasize the social factors or conditions operative in the early days of disaster research. Another consequence of this approach is the exclusion of individual researchers. Thus,

while an historical time frame is used to organize our remarks, this article is not meant to be a social history of the pioneering disaster researchers. Particular persons are named only if necessary for clarification of the exposition of the social factors affecting the development of the field of disaster research. The research not the researchers is our concern.

Our focus here is almost exclusively on the emergence of social science studies undertaken in the United States on natural and technological disasters. Thus we do not examine the initiation of work in the natural hazards area particularly the research on risk perceptions of floodplains (e.g., White 1964), a line of study out of this subfield of geography which partly converged with disaster studies in the early 1970s. Neither do we deal with accident research which later became partly embodied in risk analysis studies which in turn also came in part to converge with disaster research in the early 1980s. Nor do we look at the parallel pioneering effort in Canada in the very early 1950s (see Tyhurst 1950) and the independently initiated work in the very early 1960s in France (e.g., Chandessais 1966) and in Japan (see Okabe and Hirose 1985 for a short history of research in Japan since the 1960s). Without in any way denying the importance of these activities which we shall not discuss, we focus exclusively on the origins of what clearly is the historical core of what in the last three decades has developed and is known as the social science field of disaster research today. In fact, one of our major purposes is to indicate the historical links between certain early studies we shall discuss and contemporary social science studies of disasters. The other intellectual stirrings we have just mentioned either are not in our view as directly important on the mainstream work or had their influence later than the early development we shall examine.

Many of the statements we make such as about the intellectual orientations or positions taken by many of the early researchers have been derived from personal involvement and observations, informal conversations, and a series of interviews for an oral history record we have initiated with the pioneers of disaster studies. As such they are impossible to reference directly although in time the oral history interviews being archived at the Disaster Research Center (DRC) library will become available for scholarly use. Similarly, many of the never publicly circulated documents which we cite, such as research proposals, organization,

al memos, field questionnaires, etc. are very fugitive with many of the only known copies in existence being in the personal possession of the author. These typed and written historical records are being slowly deposited in the archival collection of DRC and will also become accessible to interested scholars. It should be assumed that a non-referenced material (quotations, minutes of meetings, etc.) in the article is drawn either from these kinds of personal sources and/or non-printed records.

THE APPLIED ORIENTATION OF THE EARLIEST STUDIES

The earliest disaster research in the social science area was almost exclusively supported by U.S.A. military organizations with very practical concerns about wartime situations. 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.

The Pioneering Field Teams

Unknown to many current disaster researchers, there were three different pioneering field team operations. The one that became famous in disaster circles was at the National Opinion Research Center (NORC) at the University of Chicago between 1950-1954. Its 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. Approximately 43 percent of the local population became ill and 25 persons died over the duration of several days. It was observed that some inhabitants of the area who had not been directly exposed apparently showed the same kind of symptoms as had victims who had been directly exposed. Seeking an explanation of this observation, the chemical center in 1949 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

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 stated 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. Nevertheless, 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.

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 U.S.A. had never experienced a major external bombing raid and, therefore, there was consequent concern that civilians would react badly to future wartime attack that might involve the dropping of atomic bombs. That the U.S. Strategic Bombing Surveys (1947) done for the Air Force showed that civilian populations in Germany and Japan held up remarkably well under sustained bombing attacks was either unknown or ignored.

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

forthcoming," and so on. Likewise, the research design focused on individual victims and the field instruments to be developed were 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 question 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 generally proceeded more or less as indicated in the proposal. The effort made was to study 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, e.g., resulted from the internal states of the victim. However, as we shall note later, the sociological orientation of most of the researchers at NORC employed 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 airplane 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 north-east Arkansas hit by tornadoes in March 1952. Publications by project members from this study continued for some time after the end of the research (e.g., Bucher 1957; Fritz and Marks 1954; Quarantelli 1960; Schatzman 1960) although the final report itself was never put out in any regular published form (Marks et al. 1954).

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 were to be applied to a wartime civilian context. But like in 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 (see Powell 1954a).

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 Johns 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 The University of Oklahoma were asked to do several things: to analyze afteraction reports, to observe troops in the field exposed to an atom bomb test explosion in a Nevada exercise, 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 (e.g., see, Logan et al. 1952) and discussions with the key researcher involved (the sociologist Lewis Killian) 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

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 a NORC and a University of Maryland team in the third Elizabeth, New Jersey plane crash disaster.

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 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 of 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. *The Occasion Instant: The Structure of Social Responses to Field Studies of Disaster Behavior: An Inventory*
15. *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 even

ment of the federal civil defense organizations in supporting the work of the Disaster Research Group, but 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, 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.

The Establishment of the Disaster Research Center 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 moved to the University of Delaware in 1985). That year, the Office of Civil Defense (OCD) gave the Center a rather large contract (\$200,000) 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 informally communicated to DRC, was in extrapolations from peacetime emergencies to wartime crises. In the research proposal 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

- 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 or 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 was only stated in this part of the proposal and, unlike the other four objectives which were discussed in great detail later, was not even alluded to anywhere else in the proposal. The wording 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. 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 enough 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. The Air Force never expressed any interest in results that 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 (see Quarantelli 1967; Quarantelli and Roth 1969; Drabek and Haas 1969; Drabek 1970) were judged

as invalid 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 sociologists who directed 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 a direct interest in peacetime disasters.)

The wartime orientation of OCD is illustrated in a statement covering the 1962 fiscal year (the year before DRC was established). It was written that insofar as OCD was concerned:

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 this 25-page summary of past and present social sciences research conducted by the then 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 would be learned that could be extrapolated to a wartime or military setting. Such explicit statements as were made about the extrapolation 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 the first head of the National Academy of Sciences group:

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 psycho-cultural 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: p. 5).

To the extent that the sponsoring agencies had any implicit 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 which 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 certain disaster oriented circles today--for a discussion of this perspective see Dynes 1983)

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 agency memo it is said:

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 very 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 researchers may also be reluctant to expanding the disaster area to include "war" phenomena for the same reason.

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 conclusion from all the data we have examined 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. Informally, it was also understood that DRC should add a concluding chapter on possible extrapolations of its findings to wartime situations in the reports the Center would write about the behavior and problems of different kinds of emergency organizations in natural and technological 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 problem that arose was OCD's refusal to allow the use of OCD funds for a DRC publication on the operations of the American Red Cross in disasters. The disallowance 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 (Adams 1970).

As far as we have been able to ascertain, all the other early studies by other groups which we have mentioned likewise were not subjected

searchers escaped direct control because the contract funding typically 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 judgment 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 at the highest levels of the agencies for reasons other than seeking answers to practical problems (which however may have not been the point of view of lower level officials who actually negotiated the research agreements with academic researchers). 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 groups. Whatever was involved, the sponsoring agencies, military for the most part, and contrary to certain images which developed in the late 1960's (see, e.g., documented accusation in Fisher 1972, p. 208), 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.

It is only possible to speculate, but we 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 has provided the prototype of what constituted a disaster. In the disaster re-

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" (see, Quarantelli 1987). As will be discussed in a forthcoming article, we do not think we can advance significantly on further studies on disasters until we move forward on the conceptual problem.

In the collective behavior area, a subspecialty of sociology, the development of the field has been handicapped by taking a very concentrated happening in time and space--primarily a crowd--as the prototype of collective behavior even though most of collective behavior phenomena is diffuse in time and space (Aguirre and Quarantelli 1983). We have implicitly done the same thing in the disaster area. We have tended to think of disasters as concentrated space-time events, even though it might be argued that most collective stress situations (to use Barton's term, 1970) are usually much more diffuse in time and space. DRC always has had more problems in deciding in its field work whether to study a widespread riverine flood than a tornado, reflecting its implicit image of disasters.

It is interesting to note the comment of the major researcher in the University of Maryland pioneering field studies. In a little known article he raises an interesting speculative question as to the kind of disasters American disaster researchers came to focus on in their work. 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 1954b, p. 61)

However, it should be noted contrary to what we have heard said at meetings, the disasters which were studied by the pioneering field teams included others than those involving only natural disaster agents. All included others than those involving only natural disaster agents. All

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 that relatively few non-natural disaster situations were studied, but this was more a function of what occurred during the course of the research periods involved than a deliberate focus only on natural disasters. A more recent argument (e.g., Couch and Kroll-Smith 1985) that disaster researchers have neglected chronic or slow moving as over against sudden disasters, is a much more valid criticism.

Our overall point is that we have tended to accept the notion of disaster as a concentrated time and space occurrence. This view, a constraining one on what should be researched, was developed at the time of the origin of study in the area. This conception of disaster was to a great extent implicitly and indirectly produced by the applied wartime orientation of the early research sponsors.

(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 came to focus on 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 major research program in natural hazards initiated in the late 1960's by three geographers was supported by a grant from the National Science Foundation and included studies of such matters as coastal erosion, frost and high wind, humid area drought, urban snow hazard, and water quality (see White, Kates and Burton 1969). These topics would not have interested the military supporters of the initial work in the disaster area.

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 organizations...

to be done independent of OCD support (e.g. Anderson 1969). It is not that there was any objection to such studies; in fact, some OCD funding was used to obtain the relevant field data, but there simply was little 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, who 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 1985). 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 (e.g., Fritz 1961), none of them essentially challenged the primacy and almost exclusive focus on planning instead of managing.

We do think it is illustrative of our point that in DRC's early days, a

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. But 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 (e.g., the volumes by the Institute Voor Social Onderzoek Van Het Nederlandse Volk 1955), massive fires in Australia (e.g., Anderson and Whitman 1967), and a dam collapse in Italy (e.g. Dynes, Haas and Quarantelli 1964) 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--e.g., for agency officials to be able to boast in their own bureaucratic circles, they were supporting research halfway across the world of a disaster that was the focus of international mass media attention.

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 emergency

certainly partly attributable to the locus of study used (Taylor 1978). Similarly, disaster researchers tended to look at populations 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 (it should be noted 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.

ANOTHER IMPORTANT INFLUENCE

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. However, the exposition of this point can not be provided here but will be elaborated upon in a succeeding article (Quarantelli forthcoming).

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