POST-IMPACT FIELD STUDIES OF DISASTERS AND SOCIOLOGICAL THEORY CONSTRUCTION

Robert A. Stallings
School of Public Administration
and
Department of Sociology
University of Southern California

NATURAL HAZARD RESEARCH

Working Paper 60

POST-IMPACT FIELD STUDIES OF DISASTERS AND SOCIOLOGICAL THEORY CONSTRUCTION

Robert A. Stallings
School of Public Administration
and
Department of Sociology
University of Southern California

December 1987

Working Paper 60

This project was supported by the Natural Hazards Research and Applications Information Center through Quick Response research funds provided by the National Science Foundation and by the National Institute of Mental Health. The opinions and conclusions are those of the author.

SUMMARY

As a modest first step toward furthering the development of a sociological theory of social organization in disasters, a Quick Response field study was designed to pretest instruments capable of testing a central proposition from the disaster literature, namely that characteristics of post-impact interorganizational relationships are a function of characteristics of those same relationships prior to impact. While the data that were actually gathered were useful in describing the incident studied, and even produced data sufficient for the multidimensional scaling of relationships among disaster organizations, a multivariate statistical test of theory was not possible. The inability to generate the data required for such a test seemed not to result solely from the capabilities of the investigator, the uniqueness of the incident, the fact that the research instruments were badly constructed, or the nature of the post-impact field study. Rather, the problem can be traced back to assumptions contained within the dominant theory with which sociologists have studied disasters. Other theoretical traditions in the discipline are better suited to the conditions of post-impact field studies, and their use in this area would contribute more to sociological theory. Theory construction will ultimately require the synthesis of microlevel data with existing macrolevel theory.

ACKNOWLEDGEMENTS

My thanks to Susan Tubbesing for the flexibility afforded me in using this Quick Response grant and to Sarah Nathe for her efforts to improve the quality of this document.

I also appreciate the cooperation of various representatives of the following organizations who were interviewed in July of 1986:

Miamisburg Fire Department

Miamisburg Police Department

Miamisburg City Administration

Miami Township Volunteer Fire Department

Dayton Fire Department

Dayton City Administration

American Red Cross, Dayton Chapter

Miami Valley Disaster Services Authority

Ohio Environmental Protection Agency

Robert A. Stallings

PREFACE

This paper is one of a series on research in progress in the field of human adjustments to natural hazards. It is intended that these papers be used as working documents by those directly involved in hazard research, and as information papers by the larger circle of interested persons. The series was started with funds from the National Science Foundation to the University of Colorado and Clark University, but it is now on a self-supporting basis. Authorship of the papers is not necessarily confined to those working at these institutions.

Further information about the research program is available from the following:

William E. Riebsame
Institute of Behavioral Science #6
University of Colorado
Boulder, Colorado 80309

Robert W. Kates
Graduate School of Geography
Clark University
Worcester, Massachusetts 01610

Ian Burton
Institute for Environmental Studies
University of Toronto
Toronto, Canada M5S 1A4

Requests for copies of these papers and correspondence relating directly thereto should be addressed to Boulder. In order to defray production costs, there is a charge of $3.00 per publication on a subscription basis, or $4.50 per copy when ordered singly.

TABLE OF CONTENTS

LIST OF FIGURES

INTRODUCTION

This working paper is not the conventional report of a field study of some disaster, though it contains descriptive materials gathered in the course of a Quick Response field trip. It is also not a primer on "how to" conduct post-impact field studies in disasters. Instead, it is an essay exploring the potential of field studies such as those supported by the Quick Response Program for the development of social science theory.

After more than four decades, research on disasters has become quite voluminous. Several syntheses which render the findings of separate studies into formal propositions have even been attempted. These include the early efforts of Barton (1963; 1969), the work of Mileti, Drabek, and Haas (1975), and the recent impressive compilation by Drabek (1987). Despite these useful beginnings, the study of disasters has yet to turn the corner and move into a phase of systematic, consciously undertaken tests of theory.

The initial purpose of the present study was to take a modest first step toward advancing the process of sociological theory construction. Its focus was on a fundamental proposition in the literature on organizations in disaster:

> As normal daily interaction between organizations increases, the problem of coordination in a disaster decreases (Mileti, Drabek, and Haas, 1975, p. 91).

More generally, the idea was that respondents' satisfaction with interorganizational relationships during the emergency period would be a function of qualities of the relationships with those same organizations before disaster struck.

While the purpose of the proposed study could have been met with any number of propositions in the literature, this one has a number of advantages.

It subsumes several empirical generalizations relating specific aspects of pre- and post-impact interorganizational relationships and can therefore also be used to generate hypotheses about still other aspects. It has great practical significance in that coordination across organizations is a more frequently mentioned problem in disaster responses than are shortcomings in the functioning of individual organizations. Lastly, it represents a research topic with which the Principal Investigator was already familiar (Stallings, 1971).

The proposition that pre-disaster relationships determine to a large extent the nature of post-impact interorganizational linkages, despite being widely accepted by disaster researchers as well as frequently cited in the literature, has never been systematically tested. Reports from field studies continue to suggest that derivatives of this proposition are reasonable hypotheses, but the fact is that these have remained hypotheses. Why is this so? Is there something about the design of post-impact field studies that precludes their use in the generation of the sort of data required for testing sets of interrelated hypotheses? Are there prohibitive problems in measuring phenomena like interorganizational relationships in disasters? Is it possible that hypotheses like these actually defy testing?

The study proposal called for developing instruments for gathering quantitative data on several variables related to interorganizational relationships. (The specific interview schedule developed for the study may be found in the Appendix.) The design called for a single field trip which would serve as a pretest of this instrument. Resulting data would be analyzed using standard multiple regression techniques. The study itself, while perhaps a small contribution in its own right, would form the basis of a proposal for a larger, more sustained research effort.

The issue here is not whether it is possible to gather quantitative data during the post-impact period. The Principal Investigator previously carried out research in which quantitative data, suitable for fairly sophisticated statistical analysis (multiple contingency table analysis and logistic regression) were obtained (Stallings, 1968; 1986a). Nor is the question one of whether or not numeric descriptions of interorganizational linkages can be produced. Recent work by Drabek and his associates (1981) uses such data in producing detailed descriptions of networks of organizations through techniques such as blockmodeling (the complexity of these descriptions has led the authors to refer to diagrams such as Figure III-5, p. 42 in Drabek et al., 1981 as "Charlotte's web"). The question is whether existing theory could be tested with new data rather than the usual use of new data for generating new hypotheses for future testing. Can post-impact field studies be used to gather data on precisely those variables needed for a test of theory, rather than on variables that happened to emerge in the specific disaster situation?

The specific incident chosen for study involved a train derailment and toxic chemical fire that resulted in the evacuation of some 30,000 people. The social setting appeared to be optimal for the study--a small metropolitan area, many different political jurisdictions and levels of government involved, numerous public and private organizations responding, and an emergency involving mixtures of routine and not-so-routine disaster-response activities.

The Principal Investigator arrived at the site at an appropriate moment (for a discussion of the timing of entry into the field in relation to the quality of data, see Stallings, 1986b) and began to arrange for interviews with "key informants." In all respects, the field study was no different than two dozen or so previous studies that he had been involved in over the years.

However, after only two or three interviews, it was apparent that the research instrument--while "correct" in the methodological sense and no different from hundreds of interview schedules used in social science research--would not work. It was inappropriate not because respondents refused or were unable to answer the questions it contained, or because it somehow lacked the "right" questions; it was inappropriate because it asked informants to provide information with which they were unaccustomed to dealing. The remainder of the data-gathering therefore was carried out by using conventional open-ended, in-depth interviews wherein respondents are asked to tell the story of their organization's role in the emergency with follow-up probing on key issues (i.e., specific variables) by the interviewer. In other words, the result was a field study similar in all major respects to those often referred to as an exploratory case study.

The inappropriateness of the pre-planned research design brought to light in a new way the fundamental questions with which the study began. What is it about post-impact studies of emergencies that constrains them when it comes to testing theory? Is this research field capable of generating hypotheses to be tested, but not capable of testing them? Is the qualitative field study the only research design suitable for disaster situations? What is the potential of these field studies, which are the principal type of research supported by the Quick Response Program, for theory construction in the social sciences?

This report is an attempt to answer these questions. The incident is reviewed in the first section which contains a description of major events, a brief outline of some of the unique aspects of this emergency, an examination of the major linkages among groups and organizations during the emergency period, and a discussion of the difference between the ad hoc use of theory to organize data and the use of data to test theory. The second section takes up various methodological issues involved in the construction of a theory of interorganizational relationships as these relate to Quick Response-type post-impact field studies, including a discussion of differing ways of defining research problems, research designs, sampling considerations, measurement, and analysis of data. These methodological issues ultimately derive from one's theoretical approach to the study of interorganizational relationships as phenomena, as well as from one's view of the theory construction process--two issues addressed in the third section of the paper. The final section suggests some ways that different research methods and theories can be joined to advance the construction of social science theories relevant to disasters.

THE INCIDENT

Overview

At 4:30 p.m. on Tuesday July 8, 1986, 15 cars of a Baltimore and Ohio freight train derailed on a trestle spanning a tributary of the Great Miami River southwest of Dayton, Ohio. The thirteenth car of this 44-car "Southland Flyer" was a tanker filled with 12,000 gallons of liquid phosphorous en route to the chemical firm of Allbright and Wilson in Cincinnati. The tanker burst open upon impact, allowing the 90-degree summer air to come in contact with its contents. The resulting explosion and fire sent a plume of toxic gas across the southern section of the Dayton metropolitan area. In its path were the small cities of Miamisburg (1980 population 15,304), West Carrollton (population 13,148), adjacent to and up river from Miamisburg, and Moraine (population 5,325) on the southern edge of the city of Dayton.

As units from the Miamisburg Fire Department and the Miami Township Volunteer Fire Department arrived at the scene of the crash to begin fire suppression activities, a local radio station's traffic plane, aloft to report on rush hour traffic conditions, broadcast the first news of the fire to listeners in the Dayton area. Word of the fire spread quickly among emergency-relevant agencies, and soon units from surrounding fire departments began arriving at the west end of the Sycamore Street Bridge, about one-fourth of a mile from the derailment site. The Miamisburg Fire Chief, designated the "incident commander," established a command post at the foot of the bridge to coordinate firefighting efforts. Units from nearly 50 neighboring fire departments ultimately participated.

The regional hazardous materials team soon was on the scene and, after checking various materials manuals and contacting CHEMTREC, determined that fire fighters were confronted with a phosphorous fire. Phosphorous is used in the manufacture of fireworks, incendiaries, luminescent paints, and some forms of rodent poisons. Vapors from burning phosphorous can produce nausea, vomiting, and diarrhea, as well as burn the eyes and skin. In large doses these vapors can be fatal. Learning this, the director of the regional disaster services authority who had just reached the command post notified the Red Cross to prepare for possible evacuations.

Within an hour or so, the first of two instances of interorganizational conflict developed. Each clash produced controversies that continued long after the fire was declared inactive. This first instance involved the attempt by the Miamisburg Fire Chief as incident commander to determine exactly what was contained in the other cars of the derailed train. Representatives of the railroad, the CSX corporation, later insisted that the chief was shown the manifest listing the contents of each of the train's 44 cars at this time. While admitting that he may have physically had such a list in his hands, the chief maintained that this had been for no more than a minute or so and definitely not at the time when he was seeking information about the contents of other cars derailed in the vicinity of the burning phosphorous tanker. Later that night it was discovered that a second tanker filled with pure sulfur--originally three cars away as the train had been put together--lay next to the burning phosphorous car at the scene of the crash, as did a third car loaded with animal fats.

The interorganizational complexity of the incident increased rapidly. Ham radio operators were called in to provide telephone patches from the command post at the end of the bridge. The state air pollution control agency was notified and sent its emergency response team to the scene. Helicopters and weather monitoring equipment with which to track the speed and direction of the toxic plume were requested. The local medical society sent physicians to the scene, hospital emergency rooms prepared to receive victims of the fire and its toxic vapors, temporary shelters were set up, and the regional transit authority dispatched busses to help transport evacuees. Representatives of federal agencies began to arrive, including those from the National Transportation Safety Board, the Federal Railway Administration, the Federal Bureau of Investigation, and even the Secret Service.

The Miamisburg police lieutenant at the field command post began to draw up plans for a two-phase evacuation of the city. To be evacuated immediately were 2,000 people living on the east bank of the Great Miami River directly across from the accident site. Assisted by officers from the county sheriff's department, the Dayton Police Department, and other law enforcement agencies from surrounding jurisdictions, police officers went door-to-door notifying residents of the evacuation order. In the second phase, the remaining residents of the city between the river and the interstate highway three miles to the east were evacuated. The evacuation was complicated by the presence of three nursing homes within this area.

Still unable to extinguish the fire, and fearing a new and possibly worse explosion if flames were to engulf either the car containing sulfur or the second loaded with animal fat, the fire chief called a "brain storming" meeting of all the chiefs of the fire departments involved for the following day, Wednesday, July 9. A plan was developed in which a cable would be attached to the burning phosphorous tanker so that it could be pulled further away from the other two cars and their dangerous contents. When the effort resulted instead in a second major explosion as new portions of the suddenly disturbed phosphorous came in contact with the air, a second evacuation of 15,000 additional persons downwind of the enlarged toxic plume was ordered. Because police had been forewarned of the attempt to move the burning car, plans for a second evacuation were in place by 6:00 p.m. when the new flare-up occurred. The evacuation was completed by 8:00 p.m., well before dark.

Meanwhile, many of the 17,000 original evacuees were on the move again in the early evening because a change in wind direction had placed some existing shelters in the path of the plume, and because failure of the air conditioning unit at the shelter in the University of Dayton Arena made conditions there intolerable. The Dayton Convention Center was opened as a new temporary shelter at 7:00 p.m. About 2,000 people used the convention center, with approximately 1,000 people spending the night. The Red Cross, supplemented by donations of food from local businesses, served dinner on Wednesday and breakfast the next morning, and by midafternoon Thursday most of the evacuees had made other arrangements for spending the night. In all, 12 different Red Cross shelters handled 3,960 evacuees and served 10,891 meals.

On Thursday, July 10, representatives of the federal Environmental Protection Agency arrived to "take over." The confrontation which followed between representatives of the EPA and the City of Miamisburg was a rancorous one. At issue was who had ultimate decision-making responsibility during the emergency. City officials, chiefly the city manager supported by the mayor, argued that, since the burning tanker lay within the Miamisburg city limits, they were ultimately responsible for the health and safety of the city's residents. The position of the EPA was that the matter belonged in the hands of the federal government since the incident had occurred on one of the nation's railroads and was environmental in nature. (Area officials interviewed in the field study imputed various motives to the federal EPA representatives including the presumption that "small town" people could not handle such a major chemical emergency, the mistaken belief that local firefighting efforts were actually over and that the remaining problems were only those of the environment, overeagerness on the part of two naive "yuppies" from the East, and a desire by the Republican federal administration to "show up" the Democratic governor of Ohio and his political ally, the mayor of Dayton.)

After a heated exchange in the city manager's office, during which the senior EPA representative threatened a Congressional investigation into the city's handling of the incident if it did not yield control to the "feds," the city emerged the winner. The Miamisburg fire chief would retain ultimate firefighting responsibility, and the city manager would be ultimately responsible for decisions affecting the safety and welfare of the city's residents, including decisions about when it was safe for evacuees to return to their homes.

By Thursday the influx of news reporters had become so great that a separate media command post was established at the west end of the Sycamore Street Bridge, some distance from the fire command post. Most city officials, including members of the city council, continued to frequent the field command posts rather than the room set aside for them at city hall. Representatives of several disaster response agencies had been urging city administrators to open an emergency operations center (EOC), but as of Thursday no EOC had been established. It was also discovered that the city's mayor had never formally declared a state of emergency. After meeting with members of the city council, the mayor did issue such a proclamation retroactively.

An emergency operations center was finally opened just before nightfall on Friday, July 11 in the chambers of the Miamisburg City Council. With the assistance of emergency specialists from the Monsanto Chemical Company's research laboratory in Miamisburg, special telephones and microcomputers were installed. All information regarding weather conditions (wind direction, wind velocity, humidity) and the status of fire suppression activities was relayed to the EOC. The EOC itself had to be abandoned temporarily as two tornadoes passed through the area (the council chambers have several panels of full-length glass walls), and even a small earthquake (which many thought was another explosion) shook the EOC.

On Saturday, July 12, at 10:45 a.m., the phosphorous fire was finally declared "inactive," officially signalling the all-clear for the remaining 300 evacuees to return home after four days. Results of the analysis of air samples taken during the first 24 hours after the initial explosion were received on Saturday from the Centers for Disease Control in Atlanta. The analysis confirmed the toxicity of the plume, thus vindicating the decision by city officials to order extensive evacuations. With the threat to public safety over, the emergency operations center closed at 3:15 p.m. Saturday afternoon.

In all, some 600 people had been treated at area hospitals for nausea, vomiting, and other symptoms. More than a dozen other victims were treated as late as one week after the derailment. At least two deaths occurred during the evacuation: one man died when the personal camper he was using for emergency shelter caught fire while he slept; and a 94-year-old woman died after being evacuated from one of the nursing homes. By the end of July, class action law suits seeking recovery of lost income, wages, and profits, as well as damages totalling $1.05 billion, had been filed against CSX (the parent company of the Baltimore and Ohio Railroad), Allbright and Wilson, and Union Tank Car Company (manufacturer of the ruptured phosphorous tank car). An injunction was also being sought to prevent CSX from settling claims for individual losses in exchange for signed agreements to forego any further legal action against the company.

Investigation of the accident was underway, with official speculation as to its cause centering on a "sun kink," a buckling of the tracks caused by the summer heat which resulted in some 35 feet of one of the rails moving as much as five inches. Plans were also being made for clean-up of the creek into which some of the phosphorous had fallen. A new channel had been dug so that the stream could be temporarily diverted. The short-term impact on fish and other forms of wildlife was said to be serious, and plans to study the long-term effects on wildlife were being drawn up. Public health studies were also being planned to measure the long-term effects of human exposure to the phosphorous gases.

Significant Features of the Emergency

Most of the features of this emergency are typical of those found in the disaster literature, but there are a few new twists that have to do with the nature and origins of this chemical emergency. Five features that are relevant to an assessment of post-impact field studies for the construction of a theory of interorganizational relationships are noteworthy.

The most outstanding feature of this incident was the rancorous intergovernmental conflict between the municipal government of the City of Miamisburg and the federal Environmental Protection Agency. Whatever the "backstage" political dimensions of this conflict, it had two characteristics,one substantive and the other formal, that are important for purposes of this assessment. Substantively, it was a struggle over "ownership" of the emergency. Formally, it involved a relationship between two entities alternately characterized by conflict and cooperation (after stormy negotiations).

The second feature of this incident was the controversy surrounding the timeliness of the establishment of an Emergency Operations Center (EOC).

Additionally, whether the EOC was or was not opened in timely fashion, several participants reported the need for an EOC to pull together the different "command posts" that had emerged around various disaster functions. Each functional area (fire suppression, evacuation, security and traffic control, environmental monitoring, public information and media relations) seemed to be operating reasonably well under conditions of uncertainty, but the need to link these separate spheres, each with separate "ownership" of part of the emergency, was gradually felt as the emergency period continued.

The third feature of note was the special problem created by the evacuation of three nursing homes. Special needs in the area of transportation and temporary housing were complicated by the fact that a re-evacuation was necessary 24 hours into the emergency. Given the constraints under which the disaster response organizations were operating, the fact that the evacuation and sheltering went as smoothly as it did was remarkable.

Fourth, interjurisdictional contact seemed to take place more readily and more frequently through channels that had been established before the emergency through occupational and professional ties. In contrast, contact and communication across different professional and occupational spheres were much more problematic. For example, fire chiefs from neighboring jurisdictions contacted the Miamisburg Fire Department directly (often by simply showing up at the scene with whatever firefighting apparatus seemed appropriate), law enforcement agencies contacted the Miamisburg Police Department, city managers contacted the Miamisburg City Manager, and elected officials from a variety of governmental levels contacted either the Mayor of Miamisburg or one of its elected city council members. While some of this was no doubt facilitated by personal acquaintanceships, joint involvement in professional organizations (or membership in a common occupational category) seemed to have been more salient.

Finally, the most lasting impression was the relative ease with which working relationships developed within each of the spheres of functional "ownership" of the emergency. Coordination among the 50 or more fire fighting units was the most noticeable, but equally noteworthy was the handling of large numbers of news reporters and crews, many from out of town. The physical segregation of the press at the Sycamore Street Bridge command post was the first step in the process of social separation that accompanied these task-specific relationships. The other side of the coin became visible later when the need to integrate these separate spheres of activity was felt.

Figures 1 and 2 show the networks of organizations undertaking the principal tasks during the early and late phases of this emergency, respectively. Both figures were constructed freehand from notes taken during field interviews. Figure 1 shows that there were two principal spheres of activity. One involved fire suppression at the scene of the derailment. This figure shows the close working relationships that developed among firefighting agencies and the groups supporting them in the field such as the Ohio Environmental Protection Agency (OEPA), the Ohio Highway Patrol (OHP) whose helicopter crews monitored plume direction, and the specialists from Monsanto who were providing analyses of data as the phosphorous continued to burn. The second sphere involved evacuation and sheltering. The figure shows the relatively close contact between agencies like the Red Cross, which had primary responsibility for operating the shelters, and the Regional Transit Authority (RTA) which provided busses to assist with the evacuation.

The figure suggests two hypotheses, both of which are related to locations in this two-dimensional plot. The greater the physical distance between organizations, the more likely those interviewed in the separate organizations

FIGURE 1

INTERRELATIONSHIPS DURING THE EARLY PHASE

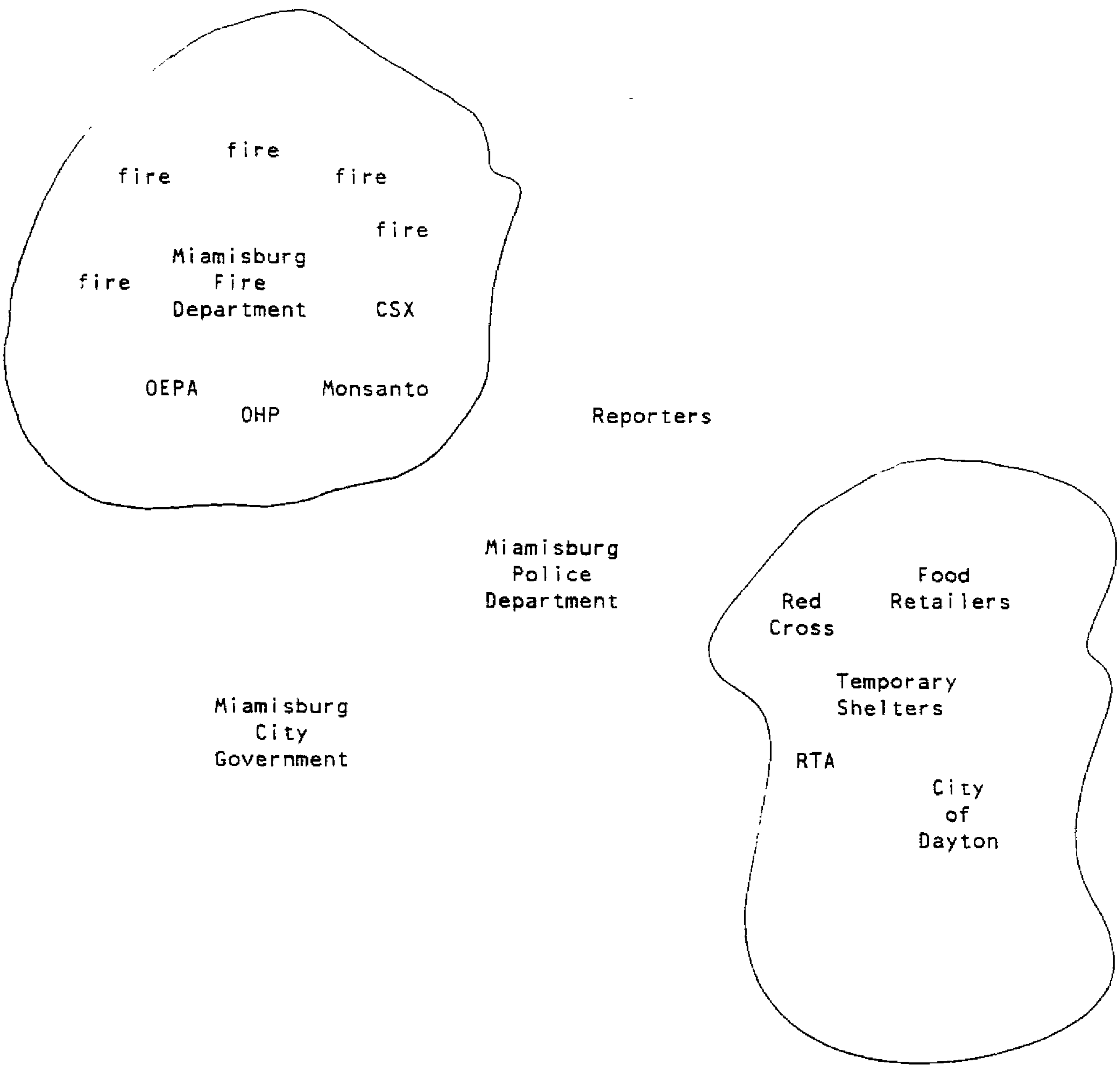
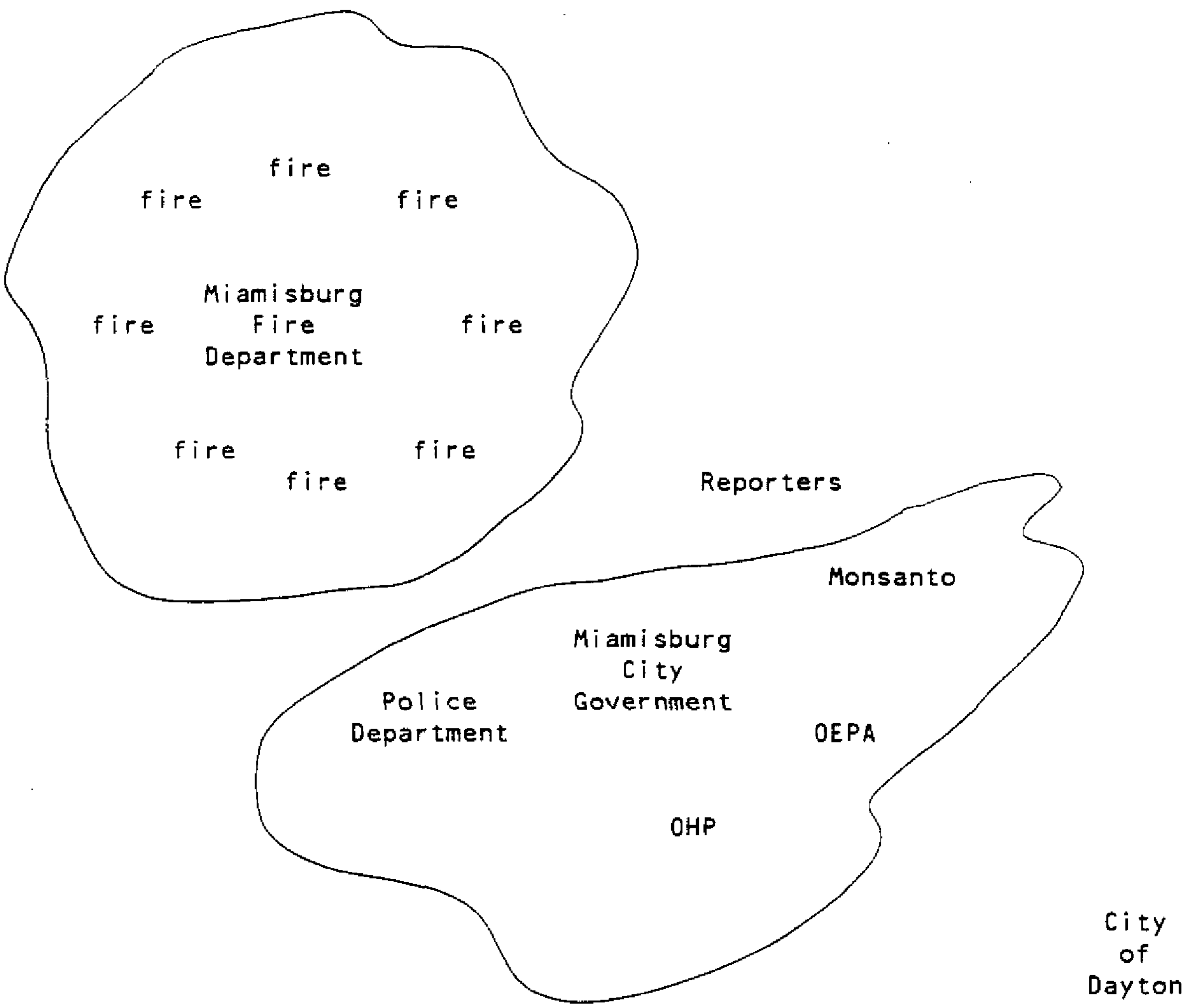


FIGURE 2

INTERRELATIONSHIPS DURING THE LATER PHASE



were to report gaps in communications or delays in receipt of information. The greater the number of other organizations positioned between two organizations, the more likely representatives of each were to report some form of communications problem. Incidentally, the perceived need for an EOC to link the separate spheres of emergency activity (and of information generated by each of them) could be anticipated from the linkages described in Figure 1. By the time the EOC finally became operational, most of the temporary public shelters had been closed (Figure 2).

The Ad Hoc Use of Theory

This rather brief overview and summary of major characteristics of the familiar scenario of a toxic chemical emergency are sufficient to disclose a major weakness in studies of interorganizational relationships in disasters: there is nothing inherent in the description of such relationships that explains either the existence of these relationships or their qualities. While suggesting still more hypotheses, the studies do not actually test either old or new hypotheses. What happens is that theory simply provides a way of organizing one's data. This can be illustrated very quickly by applying in ad hoc fashion a conceptualization of social problems as a way of "explaining" the organizational and interorganizational conflicts arising in the Miamisburg derailment and fire.

Gusfield (1981) uses the concept of "ownership" to discuss responses to public problems (pp. 10-15). Borrowing Gusfield's terms, we may say that "ownership" of an emergency constitutes both the right to define the nature of the problem and to influence the choice of decisions to solve it. Where ownership is taken for granted such as in a routine structure fire, the problem seems to be objectively real and beyond dispute by reasonable people. Where ownership is less clearly defined (is it a routine fire, or does it have special qualities that make it unique?; in whose jurisdiction is it located?), conflict is more likely.

Applied to the interorganizational level, Gusfield's term "explains" the events involved in the Miamisburg incident by subsuming them under the following propositions:

> Interorganizational cooperation in the emergency period will be greater to the extent that the organizations involved have agreed to the ownership of the problem beforehand.

Prior agreements may have come about through negotiation of formal documents like memorandums of understanding (MOUs) or mutual aid agreements, or they may be based upon precedent built up over repeated involvement in situations like the one being encountered.

A related proposition is that:

> Interorganizational cooperation in an emergency will be greater when the responding organizations concur that the specific emergency situation is one fitting the class of such situations covered by prior ownership agreements.

There may have been agreements, in other words, but they are not automatically applicable in each new situation. Cooperative interorganizational relationships are less likely when the responding organizations do not define the situation as one envisioned in the previously agreed upon understandings.

A further proposition is that:

> Cooperative interorganizational relationships are less likely among organizations that are involved in the disaster response, but were not part of previous understandings.

Ownership of problems has both a "vertical" and a "horizontal" dimension. The vertical dimension has to do with the unit of analysis to which the problem is assigned, for example, whether the problem "belongs" to an individual, a family, a neighborhood, a community, or the entire society. Since government is a major problem-solving entity in the United States in the 20th Century, this vertical dimension usually parallels the structure of the intergovernmental system. For example, does ownership of the problem rest with local government and its organizations, or with the state or federal governments?

The horizontal dimension of ownership refers to functional responsibility within these levels of government. For instance, is the problem "a police matter" or one involving public health? Horizontal assignment of ownership identifies the organization brought in to solve the problem.

On the surface, the description of the Miamisburg derailment and fire seem to "confirm" Gusfield's conceptualization of public problems as arenas of contested ownership. Compared with the more frequently enacted roles in natural disasters such floods and tornadoes, the lack of prior consensus over ownership in the vertical sense would have predicted some intergovernmental difficulties between local and federal governments regardless of past or present party politics or of perceptions of competence. The relative ease with which firefighting units were interrelated, as well as the gradual discovery that the various spheres of functional activity needed to be better linked through an emergency operations center, both seem consistent with hypotheses derived from this theoretical approach. Even the appearance of a third dimension, short-term versus long-term ownership and the effort of the railroad to try to "disown" the problem and its consequences as much as possible (see Gusfield, 1981, pp. 12-13), seems to give additional proof that the theory has been supported by the data.

This evidence of support is only superficial, however. What has happened in this illustration using Gusfield's conceptualization of public problems is typical of research reports based upon post-impact field studies. Theory has been used <u>ad hoc</u> to organize and to inform descriptive findings. Those components of the theory so used stand out, while other components that either do not fit or are irrelevant to the case under examination are ignored. This selectivity in the use of theory is often overlooked because interest lies not in explicating theory but in "understanding" the case itself. Over time, repeated use of the same theory in the same way creates the illusion that it has somehow been "tested" in the several instances of its application.

METHODOLOGICAL ISSUES

The argument put forth here is that field studies of disasters and emergencies such as those supported with Quick Response funds use theory, but they do not test theory in the conventional sense. If one's goal is to understand human behavior in disasters, this is perfectly acceptable. If one's goal is to construct theory, however, this is unacceptable. It is especially troublesome if, over time, all these separate applications of theory are mistaken for independent tests of theory.

This section focuses on a single question: What is required for testing a theory of disasters? The focus of the ensuing discussion is on a sociological theory of interorganizational relationships in emergencies, but this is incidental. Almost any topic could have been selected; interorganizational relationships were chosen as a matter of convenience. In order to assess the potential of post-impact field studies for theory construction, the following traditional components of the research process as it relates to the testing of theory will be reviewed: problem selection, research design, sampling considerations, measurement, and analysis of data. Since many of the issues that could be discussed here have been covered elsewhere (e.g., Killian, 1956; Drabek, 1970; Dynes, Haas, and Quarantelli, 1967), this report will focus on problems specific to testing a theory of interorganizational relationships.

Research Problem

While most social scientists would agree with Weber (Gerth and Mills, 1958) that the motives behind one's choice of research topic are irrelevant, the motivation for undertaking post-impact field studies often constrains the way the research problem is defined. Typically the study is of an attention-grabbing incident like the accident at Three Mile Island, the eruption at

Mount St. Helens, and the explosion at Chernobyl. Social scientists may sense a responsibility for rendering a sort of public service by gathering data and interpreting the events unfolding in the news.

There is nothing wrong with this sort of motivation except that it can result in the research problem being defined as one in which the "facts" of the event are the center of attention, rather than focusing centrally on the components of theory to be examined. The starting point is not theory which is about to be challenged by some new data, but rather new data to which some theory will be fitted. With the problem initially defined in this way (perhaps only implicitly so), there is only a slim chance that the theory will not fit the "facts" of the incident, at least in a general way. In technical terms, the research problem has been framed in such a way that it is unlikely that the theory can be falsified by the data.

Research problems framed in this way actually render all studies of this type descriptive exploratory studies. Each new incident, especially if it is of large scale and has attracted much attention in the news, is treated as a unique event, one potentially full of emergent features that will have to be carefully identified and described. The focus is on the uniqueness of the event, rather than on its points of similarity with previous cases. The tendency in studies where theory testing is the goal is to ignore the unique aspects of each case while concentrating on measurements of selected dimensions that hold across all cases.

That a focus on the "here and now" of discrete events constitutes an impediment to theory construction has been recognized by researchers for a very long time (see Gusfield, 1981, pp. 1-23). The following quotation is from an early study of recent immigrants to the United States:

> But the things that are practically important may be quite insignificant theoretically, and, on the contrary, those which seem to have no importance from the practical point of view may be the source of important scientific discoveries. The scientific value of a fact depends on its connection with other facts, and in this connection the most commonplace facts are often precisely the most valuable ones, while a fact that strikes the imagination or stirs the moral feelings may be really either isolated or exceptional, or so simple as to involve hardly any problem (Thomas and Znaniecki, 1918, p. 9).

Research Design

All research designs are strategies for organizing data to make assertions about cause and effect. Some designs are better suited for this (for instance, experiments under laboratory or controlled conditions) than others (for example, one-shot *ex post facto* case studies) because they most closely approximate the logical requirements for inferring causal relationships (Mill, 1843, pp. 253-266). The strength of the controlled experiment, apart from its ability to render constant possibly spurious causal variables, is twofold: 1) it compares cases in which the hypothesized cause is present (the test group) with those in which it is absent (the control group); and 2) it makes it possible to measure the dependent variable before, as well as after, the presumed cause has been introduced. Post-impact field studies lack both these properties.

The absence of comparison (or control) groups is not the main weakness of the post-impact case study. These comparisons can be added later when separate field studies are compared or synthesized in some fashion. It is the absence of "before and after" data that cannot be offset by the comparison of several post-impact field studies. The most obvious reason for this is the inability to be certain exactly where and when an emergency will occur so that pre-impact data may be collected in advance.

Sampling

The decision to sample contains two distinct: what case or cases (disasters) are to be selected?; and which units within the case should be selected for purposes of obtaining data? As implied above, the selection of cases should be based on the likelihood that they will contain phenomena relevant to the falsification of theory, but selection is often driven by the fact that a case "made the headlines."

Analyzing interorganizational relationships in emergencies requires a decision about what constitutes the universe of phenomena about which one would like to know. Conceptually one could identify as the universe all logically possible relationships among organizations. But which organizations? All those that are "disaster-relevant"? How is one to determine this in advance? Even if this were possible, would one want to eliminate non-relationships, that is, relationships that looked like they should have been formed during the disaster but in fact were not? Or would it be better to let the universe be represented by some source list of organizations within the affected community? Unfortunately, this would exclude "disaster-relevant" organizations from other levels of social or intergovernmental systems that could become involved in the emergency. Even if a universe of organizations could be identified and defined, a simple random sample might contain so few organizations with any role in the disaster that the number (let alone the substantive significance) of interorganizational relationships could be insufficient for study.

The usual solution to this problem is to define the universe as all interorganizational relationships that existed during the emergency period among organizations involved in the response effort. Defined in this way, most studies actually endeavor to conduct <u>censuses</u> of interorganizational relationships. Sampling (randomly selecting a subset of such relationships for analysis) is not considered. A purposive (non-probability) sample is constructed instead, most often by a "snowball" technique.

In overcoming the practical problems of universe and sample, however, the extent to which the findings hold in general has been compromised. There is the further question of the size of the sample (one interorganizational field, or some number of cases equal to the number of unique relationships identified between pairs of organizations?). In short, post-impact field studies almost always deal with a single case representing either an accidental or a purposive sample (see Kidder, 1981, pp. 424-427).

Measurement

There are also serious measurement problems in gathering data suitable for multivariate statistical analysis using post-event field study designs. The Miamisburg pretest revealed several sources of these problems in the study of interorganizational relationships.

Interorganizational relationships are variable rather than constant over time. For example, relationships between two organizations may be cordial and cooperative for a few hours, then stormy and contentious for awhile, then (perhaps after a "showdown" between the heads of each) cooperative and harmonious for several more hours. Relationships may be frequent for a time, then only intermittent thereafter. Efforts to code relationships between pairs of organizations as either "cooperative" or "contentious," "frequent" or "infrequent," not only distort but also gloss over important phenomena to be explained, namely how and why relationships vary during a short period of time. This problem is not solved by developing numeric scales (of cooperativeness, for example) because the data required dictate that a <u>single</u> score be given to describe a relationship, whereas different scores at different times would have been more accurate.

Measurement problems are further complicated by the fact that not only might relationships between a pair of organizations be alternatively characterized by cooperation and by conflict, but also they may be described as cooperative at one level (e.g., at the top of their respective hierarchies) while there is in the field among lower-level personnel. How does one then "score" the relationship numerically? If separate scores are to be given, how does one decide how many levels of contact between pairs of organizations are to be given separate scores?

A third measurement issue is that of who "speaks for" an organization. Since interorganizational relationships are to be treated as "global" properties (as characteristic of the collectivity as a whole, rather than as properties of individuals), research subjects are normally treated as "informants" rather than respondents. This means that they are asked to provide information "on behalf of" the whole rather than to report data of a personal nature. If relationships between organizations are multifaceted rather than unidimensional, can any single individual speak for an organization's several relationships? What if two different informants from the same organization differ in the descriptions provided?

There are real differences in gathering data from people at the top versus those at the bottom of organizational hierarchies. People at the top (city managers, fire chiefs, department heads and supervisors of various types) deal in "the big picture." They endeavor to reduce large amounts of detail to a small number of essential themes (the executive summary in a lengthy document is one manifestation of this tendency). Indeed, their very success is related to cutting through the debris of disparate facts to grasp the overall picture of what is happening and how it affects their organization. The responsibility of those at the bottom is different. They are responsible for the implementation of the organization's policies. Detail is the essence of their daily work routines. People at the bottom are seldom asked for overall summaries, general trends, and the like.

These differences in outlook affect both the nature of information that each type of organizational participant can provide as well as the form in which that information must be collected. Asking heads of organizations to break down relationships with each of several other organizations into the necessary components of frequency, duration, and direction is often difficult even if time is not a constraint (which it often is), because these people generally prefer to aggregate rather than disaggregate their knowledge of such details. Lower participants, on the other hand, may have both the time and the view of the world that is ideal for detailed descriptions of relationships with other organizations, but they probably have not been in a position to have direct knowledge of more than a handful of such contacts. They do not, in other words, have the sort of organizational role that provides them with a good vantage point for overseeing a wide horizon of differing relationships. Research instruments needed to gather quantitative data for multivariate statistical analysis are therefore least effective with informants in a position to provide the best descriptions of the organization's relationships during the emergency period.

Complex organizations are also stratified systems of communication. Those at the top engage in talk and use verbal skills to a greater extent than those at the bottom. These two levels are distinguished by who spends time giving orders and who spends time taking orders (Collins, 1975, pp. 114-152; see especially his summary of causal propositions, pp. 155-160). The linguistic similarity between academic researchers and informants who head organizations is greater than that between researchers and those at the bottom of organizations. There is, in other words, a correlation between the language skills of people in organizations and different types of interview techniques. The depth interview places people at the top of organizations "center stage" where they can construct a performance around the topic of the interview, whatever it may be. The systematic plodding of the fixed-choice interview and accompanying coding checklist, required to produce numeric data, is foreign to them.

Furthermore, the validity of interorganizational data during the emergency period is hard to establish. This is not merely a problem of faulty memories, skeletons hidden in closets, or harried emergency responders. How does one code the relationship between organizations A and B when a respondent at the top of A describes the relationship as having had high frequency during the emergency period, whereas a second respondent elsewhere in A reports that contacts with B were infrequent? What if persons in organization B describe their relationship with A differently?

Participants in organizations also draw the boundaries of organizations differently than do researchers. For example, in answering questions about relationships between his/her unit and various news organizations, a department head may be unable to differentiate among several specific radio, television (local and network), and newspaper organizations. Indeed, he or she may not have even been aware of which organizations were present or of who represented which news organization. Asking for precise categorical descriptions of a number of dimensions such as the quality, duration, or frequency of each separate interorganizational link can produce numerous "No Response" answers. Put differently, while key respondents may clearly recall that their relations with "reporters" or "the news media" were generally good (or bad, or whatever), they seldom are able to disaggregate specific details of each link.

This problem is lessened when relationships among organizations are infrequent, similar in type (rather than of varying types over time), and confined to interactions among the same small number of individuals. The following hypothetical quote from a respondent is typical:

> Such-and-such agency called about an hour into the emergency and asked what they could do to help. I said, 'Nothing for now,' but that we'd let them know if we needed anything. As it turned out, we didn't need their help afterall. That was our only contact with them.

The most important relationships as far as the emergency response organizations are concerned are of exactly the opposite type, i.e., those involving high frequency, changing characteristics over time, and carried out at several levels of each organization simultaneously. While some of the difficulties of data collection may be peculiar to the study of interorganizational relationships (for instance, the need to measure group-level rather than individual-level variables), the more fundamental issues of quantification seem generic. How does one assign a single score value to a phenomenon whose properties are actually heterogeneous rather than homogeneous during the emergency period? Also common is the tendency for respondents to bound or delineate phenomena in ways that are at odds with the constructs of a theory and hence of the fixed-choice questions derived from it.

In the present case, the hypothesis of interest was that the degree of respondent "satisfaction" with interorganizational relationships during the emergency period was a function of various qualities of relationships with those same organizations before the disaster. A four-page interview schedule consisting of 24 mostly fixed-choice questions had been devised, and a matrix for recording respondents answers was to be used. The key questions were:

"From the standpoint of your organization, how satisfactory were the working relationships with each of these other organizations during the emergency?"; and "During the months before the disaster, how satisfactory were your organization's relations with each of these other organizations?" Answers to both questions were to be coded in terms of a five-point Likert-type scale, ranging from very satisfactory to very unsatisfactory. Other questions included: "For each of the organizations you have mentioned, how frequent was your contact with them during the emergency?"; "At what level of organization did this contact take place?"; and "Have you ever worked with any of these organizations in a previous disaster, say within the past five years?" (The complete interview schedule is reproduced in the Appendix.)

Informants in the best position to provide answers to these detailed questions quickly tired of the systematic and plodding manner of repeatedly considering each question as it pertained to each organization with which they had had contact. It was not that they refused to grant sufficient time for the interview; on the contrary, most respondents probably spent more time being interviewed than would have been the case if they had only been asked to complete these fixed-choice questions. Because they preferred to talk at length--in their own words--about those aspects of such interorganizational relationships that they felt were most significant, it soon became evident that this type of interview schedule was inappropriate for a sample of key informants.

An alternate style of interview--the depth interview--was substituted for the remainder of the field study. The hope was that the same information sought with the structured questionnaire could be obtained by probing informants as they talked. It should be noted here that only someone intimately familiar with the original data collection instruments, as well as their underlying intent (such as the Principal Investigator), could reasonably expect to be successful at making an adjustment like this in the field. Graduate research assistants would have at best varying degrees of success.

Analysis of Data

The type of data most often obtained in post-impact field studies consists of handwritten notes taken during depth interviews with key informants. The use of these notes (and sometimes of transcriptions of tape-recorded interviews) in three forms of data analysis relevant to a theory of interorganizational relationships is examined here: multiple regression analysis, network modeling, and qualitative data analysis. The implication of each for theory construction is considered.

Multiple Regression Analysis

This form of statistical analysis is a conventional approach to controlling for potentially spurious variables in the test of causal hypotheses in non-experimental research. It requires quantitative data on each variable for each interorganizational relationship making up the data set. However, handwritten notes from these depth interviews did not produce data which could be coded for use in multivariate statistical analysis. The reason was not that such data were lost because of the mechanics of handwriting. Data are also lost when depth interviews are tape recorded and later transcribed. The basic problem is that, since depth interview probing does not force respondents to speak about every aspect of each interorganizational link, data suitable for later statistical analysis cannot be constructed for each variable for every observation.

As the number of dimensions of interorganizational relationships for which data are needed increases, the chance also increases that one or more of these dimensions will not be mentioned during the interview, will not be recognized as a piece of datum when expressed by respondents in their own words, or will not be recognizable as data in the field notes. Because the respondent is telling a story in his or her own words, the interviewer is simultaneously trying to follow that story, be alert to how the language used by the informant corresponds to the variables for which data are sought, and keep track of what is not discussed by the informant so that follow-up questions (called "probes") may be asked. Field notes are best at capturing "the story." They can be used to translate fragments of verbal description into values for some of the variables needed for statistical analysis (for example, the frequency of contact between two organizations), but in general they do not contain complete information on each observation.

There is the further problem that these informant descriptions frequently contain information not on specific organizations, but rather on entire classes organizations (such as "the news media"). Consequently, even though there may be information on most of the interorganizational variables needed, this information is not usable because the nature of each separate link cannot be isolated. Indeed, it is probably impossible to tell how many such links there were in the first place.

In-depth interviews themselves have a dynamic that makes it difficult to use different types of questions during a single interview session. Most researchers consider the first three to five minutes of the interview to be crucial in structuring the relationship between question-asker and question-answerer. During this initial phase, the informant decides what is appropriate behavior for a question-answerer. After spending several minutes "telling a story" in his/her own words, it is difficult for the respondent to switch to a different question-and-answer style. Informants do not give up center stage easily. They tend to expand upon each follow-up probe at considerable length. This makes it difficult for the interviewer to fill in detail on all of the interorganizational links that have been identified in the "story-telling" portion of the interview.

The result is a set of field notes that cannot be coded in such a way as to provide quantitative measurements for all variables. Without a checklist, there is also no way to tell if all existent links with other disaster-response organizations have even been identified. Statistical analysis cannot be performed because there are too few cases for which complete data are available. Hence the ability to perform this conventional form of hypothesis test is lost.

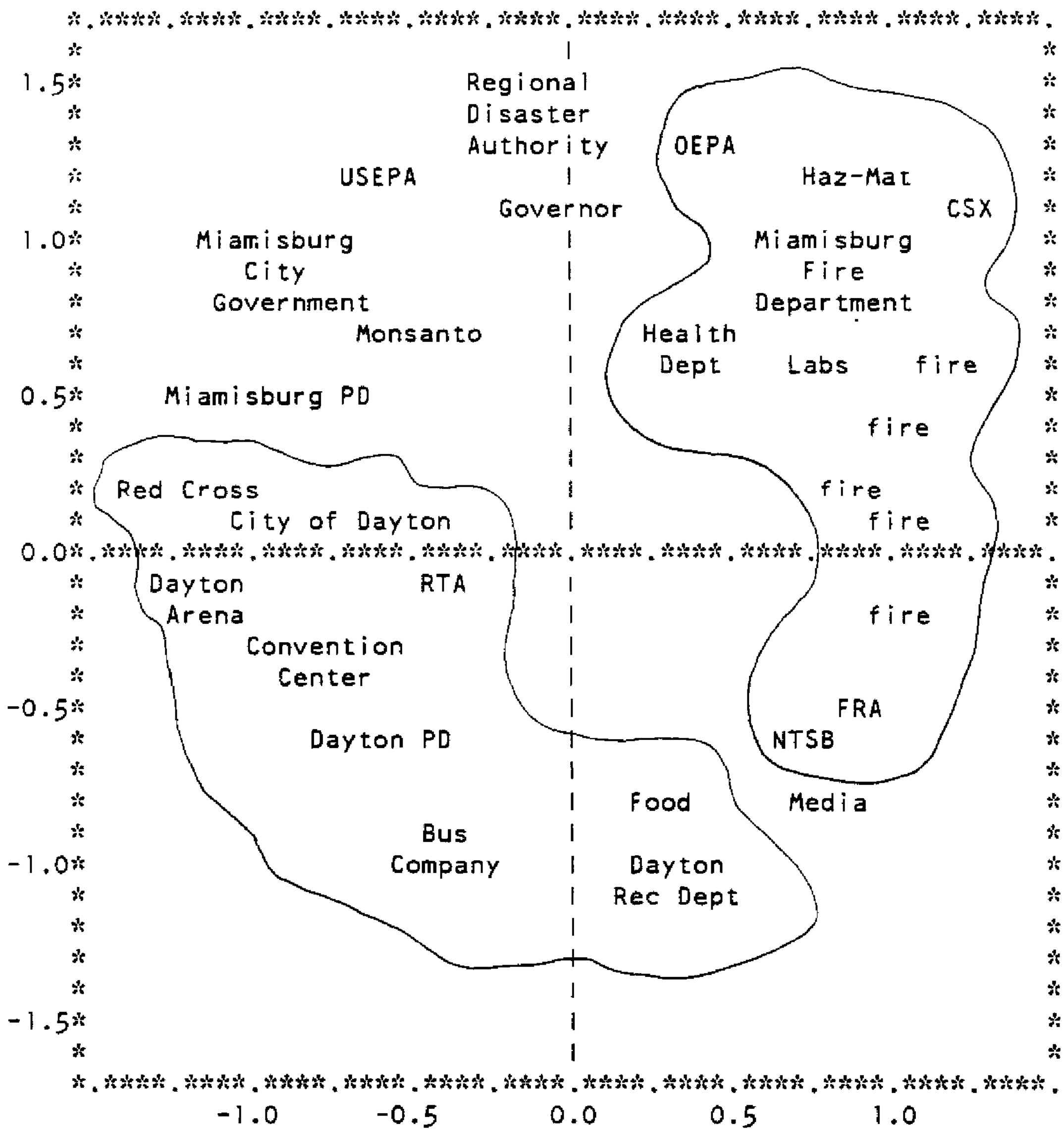
Network Models

A second approach was to analyze the field data on interorganizational relationships from the Miamisburg field study was to see if a network model could be constructed using one of the computer programs that produce multidimensional scales (for overviews, see Burt, 1978, 1980; Knoke and Kuklinski, 1982). Generally speaking, the data demands of these techniques are less severe than those for multivariate statistical analysis. At a minimum, binary descriptions of the presence or absence of links between organizations are acceptable data. Figure 3 contains output from one such program--the ALSCAL program written by Young, Lewyckyj, and Takane (1986). The input data set for this program was a matrix consisting of the number of organizational links during the emergency period described in the interview by informants.

The ease with which computer programs can produce such mathematically precise descriptions from modest data can easily obscure some of the methodological weaknesses of these models. Most obvious is that, for all the sophis-

FIGURE 3

ALSCAL MODEL OF EMERGENCY TIME RELATIONSHIPS



ticated hardware and software used to produce the model, it yields no appreciable improvement over the picture of linkages drawn freehand before the data were coded. Indeed, Figure 3 is really the mirror image of Figure 1, above. This should not be surprising since the same information--handwritten interview notes--was used for both.

There are other limitations as well. Verbal accounts of field notes contained an uneven picture of the data on these linkages. While some links were discussed in considerable detail by one or another of the informants, other linkages were commented upon less extensively. Other relationships were mentioned by an informant in only one of the participating organizations. In still other cases a link seemed to be of such a peripheral nature that arranging to interview an informant in the other organization was unwarranted. And as mentioned above, other links were referred to only generically, as in the examples above involving "the news media" rather than Channel 8, WWWW radio, or the Evening-Gazette News,

More subtle yet is the theoretical limitation of these network models. Unless tied to a pre-existing theory of interorganizational networks, models such as the one in Figure 3 represent the description of a pattern to be explained rather than an explanation of that pattern. It is a precise mathematical description of the research question, but its raw materials consist of an after-only measure of the dependent variable. Unless one is able to combine these values with those describing other characteristics of these same linkages, no test of theory in the conventional sense is possible.

Qualitative Data Analysis

The most frequent use of the verbal descriptions contained in field notes such as those made in Miamisburg is in the qualitative description of patterns that at best can be considered hypotheses. These often contain detailed and highly suggestive explorations of emergency-period patterns of interorganizational relationships (see the recent monograph by Drabek, et al., 1981; see also the earlier studies by Stallings, 1967 and by Ross, 1976).

The same weaknesses noted above characterize these data when used in such a manner. They provide better descriptions of emergency period than of pre-emergency period linkages because the latter have to be reconstructed in the post-impact period (the absence of before-and-after data issue). Even at that, one is never certain that the linkages identified are exhaustive or even representative of all those that existed during the emergency period.

Conclusion

The strategy for data collection deemed necessary for advancement from hypothesis generation to hypothesis testing did not work in Miamisburg. Resorting to conventional in-depth interviews produced data that were usable for some types of analysis (multidimensional scaling), but not for the critical multivariate statistical tests that were desired. Rather than providing data for testing theory in a pilot study, the situation was one in which theory could only be used once again in ad hoc fashion to organize and make sense out of the qualitative data. The danger of this is that the more the same theory is used in this manner, the more it appears to have undergone repeated empirical "tests." Thus, the chief methodological failing of the post-impact field study is that it fails to create a situation in which it is possible to falsify theory. Though extremely valuable for elaborating theory and for suggesting new hypotheses for future testing, the field study is inherently limited in carrying the process of theory construction any further than this.

THEORETICAL ISSUES

There is a second side to the process of theory construction that is seldom examined with respect to these issues. If certain research methods are better suited to the constraints of the post-impact period, might not certain theories also be better suited to the type of data produced under the constraints of the post-impact field study?

Like most forms of research, the study of interorganizational relationships in the emergency period has three principal aims: to describe the pattern of relationships that existed between specific organizations during the emergency period; to explain those patterns; and to understand the consequences of those patterns. Where, exactly, does the "reality" of these patterns exist? Are they "out there" to be described by those who witness and participate in them, i.e., the informants? Or are they, as critics of survey research charge (for example, Phillips, 1971), "created" by the process of asking and answering questions during the interview? Is there some other explanation for these patterns, some other level of reality in them?

Patterns found in the notes of interviews with key informants are verbal descriptions of specific activities in time and space. A pattern may be defined as two or more verbal descriptions having the same form and content (for example, fire department A worked closely with fire department B throughout the emergency period). If these patterns are the result of key informants "telling the story" in their own words, is it the phenomenon they are describing (in this case, relationships among organizations) which characterized by such a pattern, or is there something else pattern, or is something that the storytellers themselves have in common to storytellers that producers separate stories containing the same pattern? The accounts of key informants, in other words, may tell us more about the informants themselves than about the "facts" we assume they are describing for us.

What common threads tie together the types of key informants commonly interviewed in post-impact disaster field studies? Two obvious ones are the class and occupational cultures to which they belong. In interviewing informants in depth about interorganizational relationships of which they are aware, are we getting descriptions of something which exists "out there," or are we getting bits and pieces of what stood out from the perspective of the everyday world-taken-for-granted of these organizational officials?

There are abundant clues that the latter is just as likely as the former. In telling their interorganizational stories, respondents in emergency-relevant organizations, the majority of which are quasi-military in texture (like police and fire departments), very often focus on two aspects of the situation. They see whatever problems there were as basically problems of communication having technological origins (lack of appropriate hardware, radio frequencies that were incompatible; see Stallings, 1971). They are troubled by things that did not go according to prearranged plan or agreements, especially violations in the chain of command (which organization is to be in charge of what type of activity). Even accurately captured by field notes or transcripts, verbal accounts may say little about interorganizational relationships as objective phenomena, but they may speak volumes about the organizational cultures of those who give orders in emergency-relevant organizations.

The theoretical issue here is not whether verbal or numeric descriptions of interorganizational relationships are better. Rather, the issue is whether there is a reality to these patterns reflected in informants' descriptions, or whether the reality of the patterns <u>is</u> the informants' descriptions. To further separate theoretical issues from methodological ones, a closer look at the difference between qualitative and quantitative research is required.

Qualitative vs. Quantitative Research Approaches

Qualitative methods in the social sciences enjoyed renewed popularity in the late 1960s at a time when both disaster research and social unrest in the United States were on the rise. The anti-positivism of the qualitative approach to research fit comfortably with other elements of the anti-establishment movements of the period. Numeric data produced in the course of survey research reduced human conduct to a series of statistical relationships rather than to relations among real human beings, it was claimed. The large budgets required for gathering, coding, and analyzing such data made researchers dependent upon government agencies and large private foundations, institutions unlikely to fund research on topics disapproved of by the power elite. Qualitative methods, typically employing direct observation of people in their "natural habitat," had the twin virtues of dealing with behavior holistically while freeing the qualitative researcher both intellectually and monetarily from external funding sources.

Disaster research in the 1960s never seemed to get caught up in the political and intellectual unrest of the period, which is at least mildly surprising given its close ties with the Department of Defense for much of its funding during this period. Nevertheless, it was affected at least indirectly by these anti-establishment trends. Post-impact case studies had always been the most frequent type of research on the emergency period of disasters (beginning with Prince, 1920). Given the relative underdevelopment of the field as a research area after only 20 years of attention, it seemed entirely appropriate to continue to approach disaster research with exploratory and descriptive designs. Qualitative post-impact case studies were ideally suited for such an underdeveloped theoretical area, where hypothesis generation was needed more than was hypothesis testing. The legitimacy of such techniques as research tools had been reasserted by the anti-positivist intellectual movements of the time.

A division of labor of sorts had emerged in the social sciences, especially in sociology, as a theoretical compromise between qualitative and quantitative research adherents. It was conceded that, to truly test theory, quantitative data and statistical tests were required. Qualitative research had an important and legitimate role to play, primarily at the very early stages of the theory construction process in the discovery of hypotheses for subsequent testing. (This compromise is best articulated in the first edition of the textbook on qualitative research written by John Lofland, 1971; others made the more radical claim that qualitative methods were appropriate for both theory generation and theory testing, such as Glaser and Strauss, 1967.)

The model of theory construction favored by qualitative researchers was one of accumulating detailed case studies (see Yin, 1984). Patterns (for example, of interorganizational relationships) would become visible and empirically established as separate studies pointed to identical conclusions. This logic is a variant of John Stuart Mill's Method of Agreement (1843). A more formalized variant of this approach has recently been called meta-analysis (Glass, McGaw, and Smith, 1981; Hunter, Schmidt, and Jackson, 1982; Rosenthal, 1984; see also Wolf, 1986).

The conflict between qualitative and quantitative research spilled over into the debate between adherents of different theories as well. In sociology, the dominant theory of the time--Parsonian structural functionalism (see Parsons, 1951)--was criticized as reactionary by those who favored various of its many alternatives: Marxian conflict theory, phenomenology, and ethnomethodology. These theoretical debates swirled around, but left largely untouched, the theoretical approaches of the leading disaster researchers of the time. At both major centers of academic disaster research in the United States--The Ohio State University and the University of Colorado--the post-impact qualitative case study continued to be used in research largely informed by a conception of complex organizations rooted in structural functional theory (see, for example, Haas and Drabek, 1973). At Ohio State there was a conscious effort to synthesize social organization theory and the more subjective process-oriented symbolic interaction theory (the best single example of this synthesis is probably Dynes and Quarantelli, 1968).

In short, by the 1960s the appropriateness of qualitative case studies of the emergency period of disasters was generally accepted both by researchers and by program managers in the agencies that were funding the bulk of this research. It was believed that there would be advances in understanding over time as case studies were "stacked up" one on top of the over. Qualitative research seemed ideally suited to the constraints of disasters as "unscheduled events." Although research methods appropriate for disaster research were consciously debated during this time, alternative theories were never as explicitly considered, especially after the fusion with symbolic interactionism added a dynamic or processual quality that seemed to be missing in structural functionalism (see also the typologies in Brouillette and Quarantelli, 1971; Weller and Quarantelli, 1973). The fit between method and theory was never questioned.

Interorganizational Relationships as "Social Facts"

Let us look more closely at the dominant theoretical tradition from which sociological studies of organizational and, later, interorganizational aspects of disaster were being conducted. To discover whether the difficulties encountered in the Miamisburg field study might stem from limitations of theory as well as those of method, one must look beyond the alleged static nature of structural-functionalism that framed this research and its so-called conservative tendencies. The question is whether the assumption that organizational and interorganizational phenomena are "out there" in some objective sense is a useful one for post-impact research.

Ritzer's (1980, pp. 35-82) characterization of this dominant theoretical tradition as the "social factist" paradigm encompassing both functionalists and their opponents, the conflict theorists, is helpful in focusing in on the problem. More important than the functionalism of the Durkheimian tradition in sociology is its "external and constraining" view of the nature of social organization. Durkheim's argument for treating social forces as "things in themselves" and his success in demonstrating that society cannot be willed away by individual intentions produced a reified post-Durkheimian assumption that social facts are objectively real. The principal 20th Century statement of this reification assumption is Warriner's "Groups Are Real" (1956; see Ritzer, 1980, p. 41).

Transposed to a sociological interest in interorganizational relationships in disasters, this implies that these linkages are "out there" in the real world with properties that can be measured. Given the constraints of the disaster situation, the best that can be done in measuring these properties is either to observe them firsthand or, since the opportunity for the researcher to be where everything is happening at just the right moment rarely presents itself, to have people who were there report what happened in their roles as "key informants."

Whether one observes firsthand or relies on secondhand accounts, there is a further problem related to the measurement issues discussed in the preceding section, as Ritzer points out:

> The observation method is not well-suited to the study of social facts. One cannot actually see most social facts. The process information obtained by observation is often seen as different from the structural information required by those who accept the social facts paradigm (1980, p. 67).

If one assumes that quantitative data on the objective features of interorganizational relationships in disasters are necessary for theory construction, as is the case among adherents of this "social factist" tradition, then it is unlikely that post-impact case studies will ever produce them. The constraints of the post-impact period, especially those on sampling and measurement, make the descriptive research designs that are best suited to these conditions of little use in advancing theory construction as understood within this sociological tradition. The logical alternative is to see if there are other theoretical approaches to the study of organizations that can take better advantage of the types of research most successfully conducted during the emergency period.

Interorganizational Relationships as "Social Definitions"

There is such a theoretical tradition, although it has never been consciously and systematically applied in disaster research. This is an approach to organizations within the tradition Ritzer calls the "social definition" paradigm (Ritzer, 1980, pp. 83-140). The fundamental difference between the social factist and the social definitionist approaches to organizational (and interorganizational) structures is their respective assumption about where these structures ultimately exist as phenomena. Whereas the Durkheimian social factist tradition assumes that interorganizational relationships exist as objective realities which are only partially and imperfectly glimpsed by participants in those relationships, the definitionist tradition assumes that the only thing "real" about interorganizational relationships is the mental picture people have of them. There is no multiorganizational field "out there" to be observed, either directly (through participant observation by the researcher) or indirectly (by interviewing informants who "saw" those relationships firsthand). There are only individuals interacting or not interacting with one another at specific times and places. Interorganizational relationships <u>are</u> the images people have of these experiences. They are subjective rather than objective phenomena.

Even the term "crisis" has a different meaning in this definitionist tradition. In the social factist tradition, an interruption in social (organizational) routines brought about by the disaster produces a rational response containing mixtures of standby resources and emergent patterns (see Perry, 1982, pp. 21-26; also Gillespie and Perry, 1976). The shift is from one form of rational decision making to another (Thompson and Hawkes, 1962).

In the social definitionist tradition, crisis means a shift to conscious decision making from the everyday practice of "non-decision making." The hallmark of the everyday world in this tradition is that it contains a set of typical solutions for typical problems, solutions which are habitually invoked with almost no conscious effort. Some writers use the term "culture" to refer to this complex of solutions. Others (for example, Giddens, 1979) use the term "structure" in the same way.

The post-impact field study of the emergency period provides an excellent "natural laboratory" for studying this routine-problem/typical-solution nexus. The strategy is to use disasters to enhance the understanding of <u>everyday</u> life. As Fritz (1961, p. 655) put it, "... disaster studies provide the social scientist with perhaps the best opportunity to develop generalizations about human nature and the basic processes of social interaction." Instead, most disaster studies attempt to understand the exception--the emergency--and the reaction to it (see the discussion of research problems in the previous section). Again, the limitation is not the post-impact field study itself, but how it is used.

The theoretical rationale for approaching disasters as subjective definitions is as follows. Society is possible because of what is taken for granted. "Our strongest social principle is to leave the interpretations alone, lest we see how flimsy they are and reveal the unfoundedness beneath" (Collins, 1985, p. 210). To make these assumptions "come out into the open," so to speak, requires a breaching experiment like those conducted by Garfinkel and his students (Garfinkel, 1967). Natural disasters represent a massive natural breaching experiment. They are unique opportunities for studying the hidden meanings actors use to organize their activities formally (such as through complex organizations), as well as informally.

A definitionist approach to disasters would deal with the question of how actors organize their sensory experience, specifically those experiences construed as representing contact with individuals from one or more other organizations. What mental categories do they carry into the emergency period from the moments before impact? How are these mental categories (sometimes called typifications) used or selected to construct images of the relationships among organizations that represent the subjective reality of those relationships during the emergency period? How are images of organizational structure and interorganizational relationships used to settle disputes over the "ownership" of the disaster? (Bittner, for example, discusses what he refers to as the "methodical use of the concept of organization by competent users" in resolving disputes when conflict over power arises in organizations; see Bittner, 1965.) How are these images used retrospectively to render past events meaningful (Bittner, 1965, p. 115)?

Especially useful for both practical and theoretical reasons would be investigations of the categories or typifications that actors use to evaluate the quality of interorganizational relationships. What cues, actions, or inactions become evidence that things are going well? Against what baseline of acceptable qualities of interorganizational relationships are these cues judged? What cues and baseline qualities do actors cite when they report that relationships had negative aspects?

The difference between social factist and social definitionist theories of interorganizational relationships is clearly evident in discussions of the "good" and "bad" dimensions of the phenomenon. Theorists of both traditions would probably agree that "good" and "bad" are not themselves part of the objective reality of things, i.e., that they exist independently of individual judgements. Social factists, however, assume that there are objective qualities of the relationships "out there" to be judged. Social definitionists deny that there are any such aspects "out there." Both the image of the aspect and the valuation of the aspect exist only "in the eye of the beholder," that is, in the definitionist approach.

Definitions vs. Facts

The in-depth interviewing which encourages research subjects to express disaster experiences in their own words and which utilizes probes to elaborate portions of those verbal renditions is ideally suited for the social definitionist approach. There is no reason it cannot be applied to the experience of people dealing with individuals from other organizations. These interviews, as the Miamisburg field study illustrates, are not well suited either at the time they are conducted or when later transcribed to producing detailed quantitative data on qualities of phenomena assumed to exist over and above the individuals participating in them. The inability to "schedule" a disaster at the appropriate moment in the course of a conventional research project, such as after a probability sample of organizations has been drawn and pre-impact data gathered, further limits the disaster as a relevant research site for testing hypotheses derived from a social factist theory of interorganizational relationships.

Ultimately there are differences as to what constitutes theory between these two theoretical traditions as well. In the social definitionist tradition, theory is simply another typification belonging to an "outsider." The outsider is a human being like those whose typifications he or she is studying. Theory as a set of (perhaps more formal) typifications is no different either. (In fact, the common thread between the typifications of actors and the typifications represented by social science theory is well captured by the term "quasi-theories" as it is used by Hewitt and Hall, 1973.) In contrast, both theory and theorist in the social factist tradition are "external" to the world of their investigations. The model is that of the positivistic philosophy of the physical sciences.

Relatively few studies of organizations have been conducted from the social definitionist point of view (for a review, see the literature discussed in the chapter on radical humanism in Burrell and Morgan, 1979). The most systematic statement remains that by Silverman (1971). Though dealing with the somewhat dated question of the relation between formal and informal structures in organizations, Bittner's (1965) early essay remains highly suggestive. The theoretical assumptions of this approach make it ideally suited for studies of organizations during the post-impact phase of disasters.

THE POST-IMPACT FIELD STUDY AND THEORY CONSTRUCTION: CONCLUSIONS

So what is to be done? Post-impact field studies of disasters such as encouraged and supported by the Quick Response program should continue since they provide unique opportunities for detailed explorations of society with its hidden assumptions exposed. The process of theory construction should also continue since the aim of theory is better explanation of the world around us. However, a better "fit" between theory and disaster studies will be necessary if the process of theory construction is to advance to a new stage.

The post-impact field study will never provide the conditions under which theory of the social factist variety will be satisfactorily tested in the positivistic tradition. The methodological limitations of this type of study are inherent in the unpredictability of disasters as unscheduled events. Quasi-experimental designs will need to be implemented. These are best supported by conventional research grants and conducted under everyday rather than disaster conditions.

On the other hand, post-impact field studies provide an excellent way to advance organizational theory following a social definitionist approach. The cliche about disasters being a natural laboratory is appropriate. The disaster creates a large-scale breaching experiment in which phenomena not available for examination under ordinary conditions present themselves in abundance. Studies such as those supported by the Quick Response Program are ideally suited to the radical empiricism of the social definitionist tradition, whether applied to the study of organizations or some other topic.

How does this help the process of theory construction? It will not help unless the two theoretical traditions can be merged. Otherwise they will continue on separate, parallel paths. Required to bridge the gap between these theories--and what holds the most promise of increasing the contribution of post-impact field studies to the theory construction process--is the development of a protocol with which the verbal descriptions containing respondents' typifications of organizational phenomena can be translated into the "typifications" represented by the constructs of positivistic organizational theory.

The reverse needs to be done as well. That is, the constructs of organizational and interorganizational theory need to be translated into verbal descriptions using the typifications of actors in disaster-response organizations. Perhaps later this can be extended to the construction of fixed-choice questions for producing quantifiable responses suitable for statistical analysis. For the moment, however, if the "buzz words" of the occupational and organizational cultures within which respondents work can be isolated, then these may be used to structure the probing of in-depth interviews.

Fortunately, several social scientists are at work on tasks involved in synthesizing the microlevel descriptions of the social definitionist tradition with the macrolevel theories of the social factists (see, for example, the collection of papers edited by Knorr-Cetina and Cicourel, 1981). A theoretical framework is provided by Giddens in his theory of structuration (for example, Giddens, 1979) which deals with the use and perpetuation of structural forms by actors in specific space-time settings. An exemplar for the necessary synthesis is provided by the work of Collins (1975), especially in his propositions focusing on talk as a basic process wherein typifications of social structure are both used and reconstituted in their use. Theoretical advancement in the study of disasters will come from thinking of creative ways to use these emergency settings theoretically, rather than from trying to apply methods of research that do not fit them.

REFERENCES

Barton, Allen H.
1961 Social Organization Under Stress: A Sociological Review of Disaster Studies. Washington, D.C.: National Academy of Sciences-National Research Council.

Barton, Allen H.
1969 Communities Under Stress: A Sociological Analysis of Collective Stress Situations. Garden City, N.Y.: Doubleday.

Bittner, Egon
1965 "The Concept of Organization." Social Research 32: 239-255.

Brouillette, John R., and E. L. Quarantelli
1971 "Types of Patterned Variation in Bureaucratic Adaptations to Organizational Stress." Sociological Inquiry 41: 39-46.

Burrell, Gibson, and Gareth Morgan
1979 Sociological Paradigms and Organizational Analysis. London: Heinemann.

Burt, Ronald S. (ed.)
1978 "Applied Network Analysis." Sociological Methods and Research 7:123-256.

Burt, Ronald S.
1980 "Models of Network Structure." Annual Review of Sociology 6:79-141.

Collins, Randall
1975 Conflict Sociology. New York: Academic Press.

Collins, Randall
1985 Three Sociological Traditions. New York: Oxford University Press.

Drabek, Thomas E
1970 "Methodology of Studying Disasters." American Behavioral Scientist 13: 331-343.

Drabek, Thomas E.
1987 Human System Responses to Disaster: An Inventory of Sociological Findings. New York: Springer-Verlag.

Drabek, Thomas E.. and associates
1981 Managing Multiorganizational Emergency Responses: Emergent Search and Rescue Networks in Natural Disaster and Remore Area Settings. Boulder, Colorado: Institute of Behavioral Science, University of Colorado.

Dynes, Russell R., J. Eugene Haas, and E. L. Quarantelli
1967 "Administrative, Methodological and Theoretical Problems of Disaster Research." Indian Sociological Bulletin 4: 215-227.

Dynes, Russell R., and E. L. Quarantelli
1968 "Group Behavior Under Stress: A Required Convergence of Organizational and Collective Behavior Perspectives." Sociology and Social Research 52: 416-429.

Fritz, Charles E.
1961 "Disaster." Pp. 651-694 in Contemporary Social Problems, edited by Robert K. Merton and Robert A. Nisbet. New York: Harcourt, Brace, and World.

Gerth, Hans, and C. Wright Mills
1958 From Max Weber: Essays in Sociology. New York: Oxford University Press.

Giddens, Anthony
1979 Central Problems in Social Theory. Berkeley and Los Angeles: University of California Press.

Gillespie, David F., and Ronald W. Perry
1976 "An Integrated Systems and Emergent Norm Approach to Mass Emergencies." Mass Emergencies 1: 303-312.

Glass, Gene V., Barry McGraw, and Mary Lee Smith
1981 Meta-Analysis in Social Research. Beverly Hills, Calif.: Sage.

Gusfield, Joseph R.
1981 The Culture of Public Problems: Drinking-Driving and the Symbolic Order. Chicago: University of Chicago Press.

Haas, J. Eugene, and Thomas E. Drabek.
1973 Complex Organizations: A Sociological Perspective. New York: Macmillan.

Hewitt, John P., and Peter M. Hall
1973 "Social Problems, Problematic Situations, and Quasi-theories." American Sociological Review 38: 367-374.

Hunter, John E., Frank L. Schmidt, and Gregg B. Jackson
1982 Meta-Analysis: Cumulating Research Findings Across Studies. Beverly Hills, Calif.: Sage.

Kidder, Louise H.
1981 Research Methods in Social Relations. Fourth Edition. New York: Holt, Rinehart, and Winston.

Killian, Lewis M.
1956 An Introduction to Methodological Problems of Field Studies in Disasters. Washington, D.C.: National Academy of Sciences - National Research Council.

Knoke, David, and James H. Kuklinski
1982 Network Analysis. Beverly Hills: Sage.

Knorr-Cetina, K., and A. V. Cicourel
1981 Advances Advances in Social Theory and Methodology: Toward an Integration of Micro- and Macro-sociologies. London: Routledge and Kegan Paul.

Lofland, John
1971 Analyzing Analyzing Social Settings: A Guide to Qualitative Observation and Analysis. Belmont, Calif.: Wadsworth.

Mileti, Dennis S., Thomas E. Drabek, and J. Eugene Haas
1975 Human Systems in Extreme Environments: A Sociological Perspective. Boulder: Institute of Behavioral Science, University of Colorado.

Mill, John Stuart
1843 (1919) A System of Logic. London: Longmans, Green.

Parsons, Talcott
1951 The Social System. New York: Free Press.

Perry, Ronald W.
1982 The Social Psychology of Civil Defense. Lexington, Mass.: D. C. Heath.

Phillips, Derek L.
1971 Knowledge From What?: Theories and Methods in Social Research. Chicago: Rand McNally.

Prince, Samuel H.
1920 Catastrophe Catastrophe and Social Change: Based Upon a Sociological Study of the Halifax Disaster. New York: Columbia University Press.

Ritzer, George
1980 Sociology: A Multiple Paradigm Science. Revised Edition. Boston: Allyn and Bacon.

Rosenthal, Robert
1984 Meta-Analytic Procedures for Social Research. Beverly Hills, Calif.: Sage.

Ross, G. Alexander
1976 The The Emergence and Change of Organization Sets: An Interorganizational Analysis of Ecumenical Disaster Recovery Organizations. Columbus: unpublished Ph.D. dissertation, Department of Sociology, The Ohio State University.

Silverman, David
1971 The Theory of Organisations. New York: Basic Books.

Stallings, Robert A.
1967 An Description and Analysis of the Warning Systems in the Topeka, Kansas Tornado of June 8, 1966. Columbus: Disaster Research Center, The Ohio State University.

Stallings, Robert A.
1968 Structural Change in Professional Organizations: A Hospital Response to Disaster. Columbus: unpublished Master's thesis, Department of Sociology, The Ohio State University.

Stallings, Robert A.
1971 Communications in Natural Disasters. Columbus: The Ohio State University, Disaster Research Center, Report No. 10).

Stallings, Robert A.
1986a "Reporting to Work Voluntarily: Multivariate Analyses of the Mobilization of Standby Resources in Disaster." Paper presented at the annual meeting of the Pacific Sociological Association.

Stallings, Robert A.
1986b National Science Foundation Field Report: The Miamisburg (Ohio) Train Derailment and Toxic Fire of July 8, 1986. Los Angeles: unpublished report, School of Public Administration, University of Southern California.

Thomas, William I., and Florian Znaniecki
1918 The Polish Peasant in Europe and America: Monograph of an Immigrant Group. Volume I. Boston: The Gorham Press.

Thompson, James D., and Robert W. Hawkes
1962 "Disaster, Community Organization, and Administrative Process." Pp. 268-300 in Man and Society in Disaster. Edited by George W. Baker and Dwight W. Chapman. New York: Basic Books.

Warriner, Charles
1956 "Groups Are Real: A Reaffirmation." American Sociological Review 21: 549-554.

Weller, Jack M., and E. L. Quarantelli
1973 "Neglected Characteristics of Collective Behavior." American Journal of Sociology 79: 665-685.

Wolf, Frederic M.
1986 Meta-Analysis: Quantitative Methods for Research Synthesis. Beverly Hills, Calif.: Sage.

Yin, Robert K.
1984 Case Study Research: Design and Methods. Beverly Hills, Calif.: Sage.

Young, Forrest W., Rostyslaw Lewyckyj, and Yoshio Takane
1986 "The ALSCAL Procedure." Pp. 1-16 in SUGI Supplemental Library User's Guide, Version 5 Edition. Cary, N.C.: SAS Institute, Inc.

APPENDIX

Interview Schedule

1. Why don't you begin by telling me what your organization did from the time you first learned of the threat of a ________ until now (or until the emergency was over).

   (RECORD UP TO FIVE TASKS ON FORM A)

1a. Which of the activities you just mentioned would you normally expect to perform in an emergency of this type?

   (CIRCLE EXPECTED TASKS ON FORM A)

   (HAND RESPONDENT THE LIST OF EMERGENCY ORGANIZATIONS)

2. Here is a list of some organizations often involved in an emergency of this type. Some of them you may already have mentioned. Would you go down this list and tell me which of these your organization had contact with during the emergency?

   (CHECK THE ORGANIZATIONS MENTIONED ON FORM B)

2a. Are there any other organizations you were in contact with that are not on this list? Is so, what were they?

   (ADD THE NAMES OF THESE ORGANIZATIONS TO FORM B)

   (LET ME BE SURE THAT I UNDERSTAND WHICH OF THESE ARE IN THE PRIVATE SECTOR AND, FOR PUBLIC ORGANIZATIONS, WHICH ARE CITY, COUNTY, STATE, ETC.)

2b. For public or private:
   - 0 - public
   - 1 - private and/or mixed public-private

2c. For public organizations:
   - 1 - city
   - 2 - county
   - 3 - joint city/county
   - 4 - special district
   - 5 - regional
   - 6 - state
   - 7 - federal
   - 8 - other ________
   - 9 - not applicable (i.e., private sector)

3. For each of the organizations you have just mentioned, how frequent was your contact with them during the emergency?
   1 - continuously
   2 - about once an hour
   3 - every few hours
   4 - about once a day
   5 - less than once a day

   (RECORD ANSWERS TO QUESTIONS 3 THROUGH 8 ON FORM B)

4. At what level of organization did this contact take place?
   1 - administrative level
   2 - operations level
   3 - both

5. How did this contact take place?
   1 - in person (i.e., face-to-face)
   2 - directly, by telephone or radio
   3 - by messenger
   4 - through a third organization
   5 - other ______________

   (CAN RECORD MORE THAN ONE ANSWER TO QUESTION 5.)

6. Which of these contacts, if any, was the result of a previous disaster plan?
   0 - not planned
   1 - planned

7. Have you ever worked with any of these organizations in a previous disaster, say within the past five years?
   0 - no previous working experience in a disaster
   1 - previous working experience in a disaster

8. From the standpoint of your own organization, how satisfactory were the working relationships with each of these other organizations during the emergency?
   1 - very satisfactory
   2 - somewhat satisfactory
   3 - neither/don't know/can't say
   4 - somewhat unsatisfactory
   5 - very unsatisfactory

9. Now let's compare your recent experiences in the emergency with a typical period, say six months before the disaster. Looking at this list of organizations, which of these do you normally have contact with?

   (CHECK THOSE LISTED ON FORM B)

9a. Are there any organizations you normally have contact with that are not listed there?

(ADD THESE TO FORM B)

(LET ME BE SURE THAT I UNDERSTAND WHICH OF THESE NEW ORGANIZATIONS YOU MENTIONED ARE IN THE PRIVATE SECTOR AND, FOR PUBLIC ORGANIZATIONS, WHICH ARE CITY, COUNTY, STATE, ETC.)

9b. For public or private:
- 0 - public
- 1 - private and/or mixed public-private

9c. For public organizations:
- 1 - city
- 2 - county
- 3 - joint city/county
- 4 - special district
- 5 - regional
- 6 - state
- 7 - federal
- 8 - other ____________
- 9 - not applicable (i.e., private sector)

10. For each of the organizations you just mentioned, how frequent was your contact, say six months before the emergency?
- 1 - almost daily
- 2 - about once a week
- 3 - about once or twice a month
- 4 - every few months
- 5 - annually
- 6 - less than once a year

(RECORD ANSWERS TO QUESTIONS 10 THROUGH 16 ON FORM B.)

11. Before the disaster, at what level did contact normally take place with each of these organizations?
- 1 - administrative level
- 2 - operations level
- 3 - both

12. Before the disaster, how did you usually maintain contact with each of these organizations?
- 1 - in person
- 2 - by telephone, or other media
- 3 - through correspondence
- 4 - through a third organization
- 5 - through joint programs
- 6 - other ____________

13. During the months before the disaster, how satisfactory were your organization's relations with each of these other organizations?
    - 1 - very satisfactory
    - 2 - somewhat satisfactory
    - 3 - neither/don't know/can't say
    - 4 - somewhat unsatisfactory
    - 5 - very unsatisfactory

14. How do each of these organizations we have been talking about compare in size to your own organization?
    - 1 - other organization is larger
    - 2 - other organization is smaller
    - 3 - both are about the same size
    - 4 - don't know

15. Which of them seem to be made up largely of volunteers?
    - 0 - no or few volunteers
    - 1 - large numbers of volunteers

16. Are any of these organizations that came into being as a result of the emergency?
    - 0 - existing organizations
    - 1 - emergent organizations

(FINALLY, A FEW QUESTIONS TO HELP ME GET A BETTER PICTURE OF YOUR OWN ORGANIZATION.)

17. Approximately how many paid employees does your organization have?

(RECORD ANSWERS TO QUESTIONS 17 THROUGH 23 ON FORM C.)

18. Approximately how many volunteers does your organization use?

19. Does your organization have an internal written disaster plan?
    - 0 - no
    - 1 - yes

20. Is your organization part of a multi-organization disaster plan?
    - 0 - no
    - 1 - yes

21. To what extent are your organization's work rules, procedures, and policies in written form?
    - 1 - to a great extent
    - 2 - to some extent
    - 3 - to a small extent
    - 4 - not at all
    - 5 - don't know

22. Approximately how many distinct departments or divisions does your organization have?

23. What is the approximate annual budget of your organization?

(THAT'S ALL THE QUESTIONS I HAVE. THANK YOU VERY MUCH FOR YOUR TIME AND ASSISTANCE. IF YOU NEED TO CONTACT ME FOR ANY REASON, MY MAILING ADDRESS AND TELEPHONE NUMBER ARE ON MY CARD.)

NATURAL HAZARD RESEARCH WORKING PAPER SERIES
Institute of Behavioral Science #6, Campus Box 482
University of Colorado, Boulder, Colorado 80309

The Natural Hazard Research Working Papers series is a timely method to present research in progress in the field of human adjustments to natural hazards. It is intended that these papers be used as working documents by the group of scholars directly involved in hazard research, and as information papers by a larger circle of interested persons.

Single copies of working papers cost $4.50 per copy. It is also possible to subscribe to the working paper series; subscription entitles the subscriber to receive each new working paper as it comes off the press at the special discount rate of $3.00 per copy. When a new working paper is sent to a subscriber it is accompanied by a bill for that volume.

1 The Human Ecology of Extreme Geophysical Events, Ian Burton, Robert W. Kates, and Gilbert F. White, 1968, 37 pp.

2 Annotated Bibliography on Snow and Ice Problems, E. C. Relph and S. B. Goodwillie, 1968, 16 pp.

3 Water Quality and the Hazard to Health: Placarding Public Beaches, J. M. Hewings, 1968, 74 pp.

4 A Selected Bibliography of Coastal Erosion, Protection and Related Human Activity in North America and the British Isles, J. K. Mitchell, 1968, 70 pp.

5 Differential Response to Stress in Natural and Social Environments: An Application of a Modified Rosenzweig Picture-Frustration Test, Mary Barker and Ian Burton, 1969, 22 pp.

6 Avoidance-Response to the Risk Environment, Stephen Golant and Ian Burton, 1969, 33 pp.

7 The Meaning of a Hazard--Application of the Semantic Differential, Stephen Golant and Ian Burton, 1969, 40 pp.

8 Probabilistic Approaches to Discrete Natural Events: A Review and Theoretical Discussion, Kenneth Hewitt, 1969, 40 pp.

9 Human Behavior Before the Disaster: A Selected Annotated Bibliography, Stephen Golant, 1969, 16 pp.

10 Losses from Natural Hazards, Clifford S. Russell, (reprinted in Land Economics), 1969, 27 pp.

11 A Pilot Survey of Global Natural Disasters of the Past Twenty Years, Research carried out and maps compiled by Lesley Sheehan, Paper prepared by Kenneth Hewitt, 1969, 18 pp.

12 Technical Services for the Urban Floodplain Property Manager: Organization of the Design Problem, Kenneth Cypra and George Peterson, 1969, 25 pp.

13 Perception and Awareness of Air Pollution in Toronto, Andris Auliciems and Ian Burton, 1970, 33 pp.

14 Natural Hazard in Human Ecological Perspective: Hypotheses and Models, Robert W. Kates (reprinted in Economic Geography, July 1971), 1970, 33 pp.

15 Some Theoretical Aspects of Attitudes and Perception, Myra Schiff (reprinted in Perceptions and Attitudes in Resources Management, W. R. D. Sewell and Ian Burton, eds.), 1970, 22 pp.

16 Suggestions for Comparative Field Observations on Natural Hazards, Revised Edition, October 20, 1970, 31 pp.

17 Economic Analysis of Natural Hazards: A Preliminary Study of Adjustment to Earthquakes and Their Costs, Tapan Mukerjee, 1971, 37 pp.

18 Human Adjustment to Cyclone Hazards: A Case Study of Char Jabbar, M. Aminul Islam, 1971, 60 pp.

19 Human Adjustment to Agricultural Drought in Tanzania: Pilot Investigations, L. Berry, T. Hankins, R. W. Kates, L. Maki, and P. Porter, 1971, 69 pp.

20 The New Zealand Earthquake and War Damage Commission--A Study of a National Natural Hazard Insurance Scheme, Timothy O'Riordan, 1971, 44 pp.

21 Notes on Insurance Against Loss from Natural Hazards, Christopher K. Vaughan, 1971, 51 pp.

22 Annotated Bibliography on Natural Hazards, Anita Cochran, 1972, 90 pp.

23 Human Impact of the Managua Earthquake Disaster, R. W. Kates, J. E. Haas, D. J. Amaral, R. A. Olson, R. Ramos, and R. Olson, 1973, 51 pp.

24 Drought Compensation Payments in Israel, Dan Yarden, 1973, 25 pp.

25 Social Science Perspectives on the Coming San Francisco Earthquake--Economic Impact, Prediction, and Construction, H. Cochrane, J. E. Haas, M. Bowden and R. Kates, 1974, 81 pp.

26 Global Trends in Natural Disasters, 1947-1973, Judith Dworkin, 1974, 16 pp.

27 The Consequences of Large-Scale Evacuation Following Disaster: The Darwin, Australia Cyclone Disaster of December 25, 1974, J. E. Haas, H. C. Cochrane, and D. G. Eddy, 1976, 67 pp.

28 *Toward an Evaluation of Policy Alternatives Governing Hazard-Zone Land Uses*, E. J. Baker, 1976, 73 pp.

29 *Flood Insurance and Community Planning*, N. Baumann and R. Emmer, 1976, 83 pp.

30 *An Overview of Drought in Kenya: Natural Hazards Research Paradigm*, B. Wisner, 1976, 74 pp.

31 *Warning for Flash Floods in Boulder, Colorado*, Thomas E. Downing, 1977, 80 pp.

32 *What People Did During the Big Thompson Flood*, Eve C. Gruntfest, 1977, 62 pp.

33 *Natural Hazard Response and Planning in Tropical Queensland*, John Oliver, 1978, 63 pp.

34 *Human Response to Hurricanes in Texas--Two Studies*, Sally Davenport, 1978, 55 pp.

35 *Hazard Mitigation Behavior of Urban Flood Plain Residents*, Marvin Waterstone, 1978, 60 pp.

36 *Locus of Control, Repression-Sensitization and Perception of Earthquake Hazard*, Paul Simpson-Housley, 1978, 45 pp.

37 *Vulnerability to a Natural Hazard: Geomorphic, Technological, and Social Change at Chiswell, Dorset*, James Lewis, 1979, 39 pp.

38 *Archeological Studies of Disaster: Their Range and Value*, Payson D. Sheets, 1980, 35 pp.

39 *Effects of a Natural Disaster on Local Mortgage Markets: The Pearl River Flood in Jackson, Mississippi - April 1979*, Dan R. Anderson and Maurice Weinrobe, 1980, 48 pp.

40 *Our Usual Landslide: Ubiquitous Hazard and Socioeconomic Causes of Natural Disaster in Indonesia*, Susan E. Jeffery, 1981, 63 pp.

41 *Mass Media Operations in a Quick-onset Natural Disaster: Hurricane David in Dominica*, Everett Rogers and Rahul Sood, 1981, 55 pp.

42 *Notices, Watches, and Warnings: An Appraisal of the USGS's Warning System with a Case Study from Kodiak, Alaska*, Thomas F. Saarinen and Harold J. McPherson, 1981, 90 pp.

43 *Emergency Response to Mount St. Helens' Eruption: March 20-April 10, 1980*. J. H. Sorensen, 1981, 70 pp.

44 *Agroclimatic Hazard Perception, Prediction and Risk-Avoidance Strategies in Lesotho*. Gene C. Wilken, 1982, 76 pp.

45 Trends and Developments in Global Natural Disasters, 1947 to 1981, Stephen A. Thompson, 1982, 30 pp.

46 Emergency Planning Implications of Local Governments' Responses to Mount St. Helens, Jack D. Kartez, 1982, 29 pp.

47 Disseminating Disaster-Related Information to Public and Private Users, Claire B. Rubin, 1982, 32 pp.

48 The Nino as a Natural Hazard; Its Role in the Development of Cultural Complexity on the Peruvian Coast, Joseph J. Lischka, 1983, 69 pp.

49 A Political Economy Approach to Hazards: A Case Study of California Lenders and the Earthquake Threat, Sallie Marston, 1984, 35 pp.

50 Restoration and Recovery Following the Coalinga Earthquake of May, 1983, Steven P. French, Craig A. Ewing, and Mark S. Isaacson, 1984, 30 pp.

51 Emergency Planning: The Case of the Diablo Canyon Power Plant, June Belletto De Pujo, 1985, 63 pp.

52 The Effects of Flood Hazard Information Disclosure by Realtors: the Case of the Lower Florida Keys, John Cross, 1985, 85 pp.

53 Local Reaction to Acquisition: An Australian Study, John W. Handmer, 1985, 96 pp.

54 The Environmental Hazards of Colorado Springs, Eve Gruntfest and Thomas Huber, 1985, 62 pp.

55 Disaster Preparedness and the 1984 Earthquakes in Central Italy, David Alexander, 1986, 98 pp.

56 The Role of the Black Media in Disaster Reporting to the Black Community, Charles H. Beady, Jr. and Robert C. Bolin, 1986, 87 pp.

57 The 1982 Urban Landslide Disaster at Ancona, Italy, David Alexander, 1986, 63 pp.

58 Gender Vulnerability to Drought: A Case Study of the Hausa Social Environment, Richard A. Schroeder, 1987, 75 pp.

59 Have Waste, Will Travel: An Examination of the Implications of High-Level Nuclear Waste Transportation, Ann FitzSimmons, 1987, 145 pp.

60 Post-Impact Field Studies of Disasters and Sociological Theory Construction, Robert A. Stallings, 1987, 65 pp.