METHODOLOGICAL ISSUES

The argument put forth here is that field studies of disasters and emergencies such as those supported with Quick Response funds <u>use</u> theory, but they do not <u>test</u> 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.

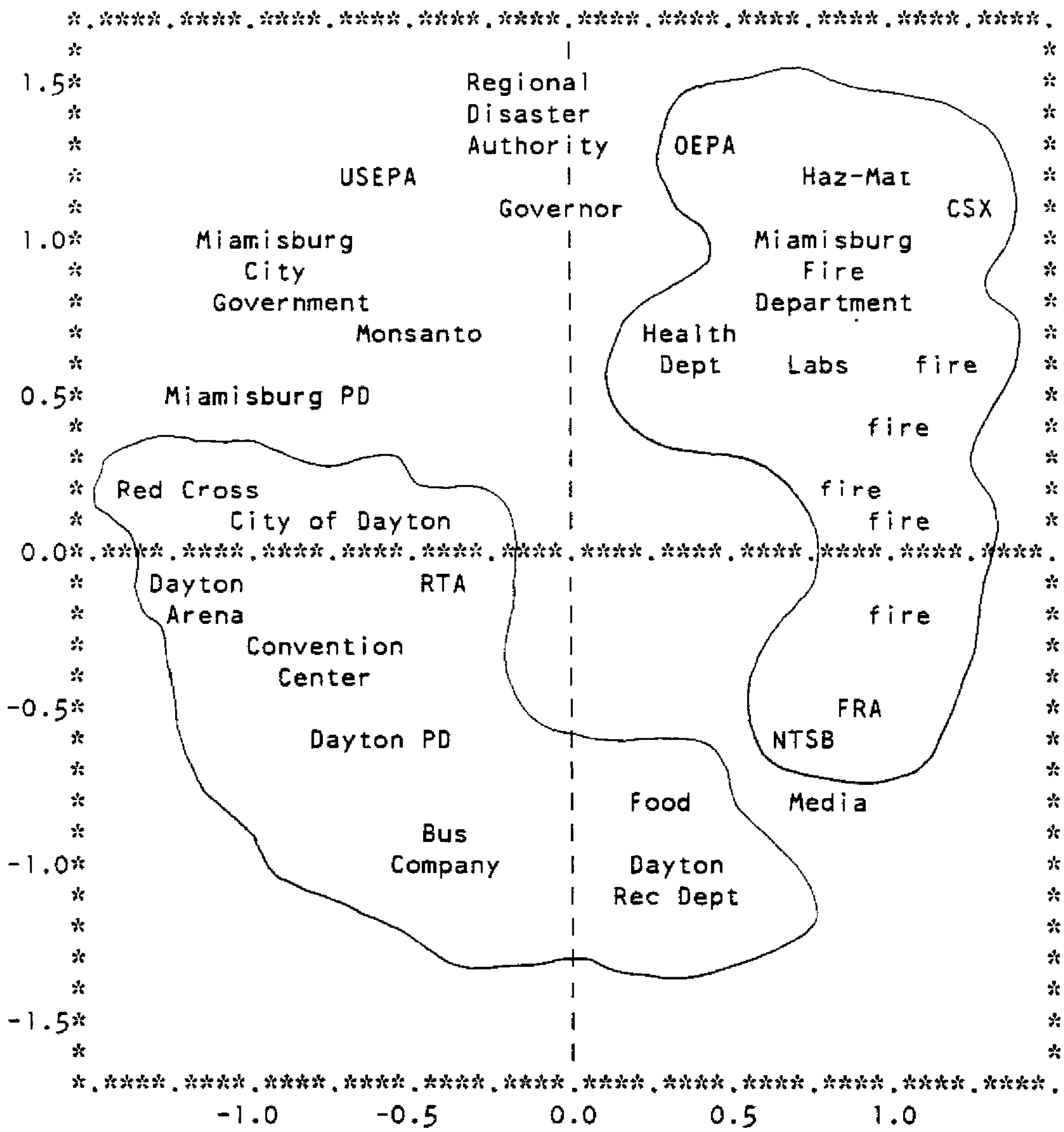
Network Models

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 <u>Evening-Gazette News</u>,

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.

<u>Qualitative Data Analysis</u>

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.