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METHODOLOGY OF STUDYING DISASTERS

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How can we better understand, explain, and predict group and organizational responses to disasters? Such questions are not unlike many others that social scientists ask. Hence, the methodological problems confronted by persons studying disaster responses are for the most part similar to problems confronted by those analyzing other social phenomena. There is neither a special set of strategies which might be referred to as disaster methodology nor a separate set of techniques which might be labeled disaster research methods.

However, there are some unique problems in disaster research which merit consideration for at least three reasons: (1) some of the solutions which have emerged can be directly applied in other areas, (2) many of the problems have broad implications with clear analogues in several substantive areas, and (3) methodological innovations in a broader sense may lead to new ways of reconceptualizing research strategies in related areas, e.g., studies of police response during disasters may assist specialists in ethnic relations or criminology.

Conducting research in communities just struck by major disasters confronts one with some special problems. Since most disasters are unpredictable, one never knows where or when the next research opportunity will appear. Unless the research program is ongoing and long range, there usually is great haste in preparation to get into the field. Failure to begin data collection immediately may greatly reduce its validity. Funding processes are noticeably slow. Rarely are preimpact data available. Experimental manipulation through random assignment to "treatment groups," and most control procedures are inappropriate, unethical, or simply impossible. Local cooperation may be adequate at the outset. However, as more outsiders arrive with insurance, sales, welfare, and other types of inquiries, research interviewers can become increasingly suspect. For these and numerous other reasons, most disaster research has lacked much methodological sophistication.

There are two earlier statements which deal directly with methodological issues and techniques in disaster research (Killian, 1956; Cisin and Clark, 1962). Many of the above points are elaborated extensively in each of these two statements. Hereafter, we will deal with other issues and place such sharp differences in emphasis that readers especially interested in this topic are urged to review this highly relevant and useful earlier material. Here our strategy will be to explore three areas: (10 types of clarifications which appear critical, (2) selected laboratory and field techniques, and (3) some suggestions for the design of future disaster research.

Some Necessary Clarifications

How do groups and organizations respond to community disasters? Stepping into such settings, many social scientists might be intrigued by this or related questions. However, they would immediately have to make several critical decisions regarding what to observe and where to begin collecting data. Several different types of groups and organizations might attract their interest--local police, volunteer fire units, hospitals, and the like. As they penetrated different aspects of the communities' responses, they most likely would discover many emergent clusters of individuals who quickly organized after the event. Transitory groups such as these may have performed numerous functions in the immediate postimpact period. To further complicate the issue, interorganizational alignments are usually altered as local decision makers attempt to cope with massive demand loads. Boundaries among local organizations are often blurred. The emergence of "synthetic organizations" has been documented by several writers, i.e., temporary interorganizational systems whereby the multiplicity of responding units are somewhat coordinated (Barton, 1969: 171-184). Our point here, however, is that it is useful to distinguish between responses by previously organized collectivities, e.g., police or fire departments, and organized activity patterns that are critical to community functioning but which are highly transitory -- they disappear shortly after the event.

Granted this distinction, a more general point then follows: there are many different kinds of groups and organizations responding in disasters. While it appears that responses vary among different types of groups, what is not clear are the criteria that should be used to define types. Yet, if groups or organizations are to be treated as units of analysis <u>in and of themselves</u>, then the criteria which define the total universe and various subpopulations must be specified. The theoretical importance of decisions on such criteria is obvious. Unfortunately, little assistance is available. The great bulk of previous disaster research has been limited to individual responses. Even in group and organizational research of a more general nature, one finds few guidelines regarding sample selection and universes to which one might generalize. Thus most of us doing such research have been hard pressed to do much more than describe sequences of what appeared to be critical behavior by persons associated with various groups or organizations.

One alternative is to follow the advice of Parsons (1956) and view organizations as instruments designed to accomplish specified goals. Thus, the key differentiating variable is organizational goal. Following this strategy, Dynes and Quarantelli, editors of this issue, emerged with separate articles focused on Red Cross, police, fire departments, and the like. Thus, if one wishes to know how police organizations respond to disasters, one might review descriptive accounts of police behavior in numerous disasters and thereby hope to discern regularities. Such work might serve as the basis for a hypothesis network or theoretical model whereby such behavior might be explained and predicted as suggested in an article by Brouillette and Quarantelli (1970). Eventually someone might subject the model to a fairly rigorous experimental test by randomly assigning police organizations to disaster and nondisaster situations. For the present, this is certainly a strategy with high appeal. But what of the future?

Recently many persons have expressed concern with the variable of organizational goal (e.g., Yuchtman and Seashore, 1967). The concerns have been first with past conceptualizations and second with the frequent use of this variable as a single criterion in differentiating among organizations. Objections of the first type might be handled by reconceptualizing the variable. Picking up on leads by Simon (1964) and Thompson (1967: 29), it seems most useful to define organizational goals as sets of expectations held for a particular social unit. With this definition, we can speak of police organizations as social units for which there are particular sets of expectations. Phrased this way, we allow for the variable of dissensus; not everyone shares the same expectations, e.g., Negroes compared to Minutemen or police officers. Also, we can then recognize that not only is the substantive content of the expectations, e.g., police should secure disaster areas, control traffic, and so on, a possible dimension of differentiation, but the level of specificity, number of expectations, and degree of consensus are additional dimensions of importance. Hence, we do not drop the variable entirely, but rather reconceptualize it in a more complex fashion. The recent study by Stallings (1968) focusing on a hospital is an example of a move in this direction.

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Taking an additional step, several types of universes of systems might be defined with various analytic criteria. And from these universes, samples of systems could be drawn with similar analytical characteristics. Generalizations regarding responses by numerous units with common analytical properties (much more sophisticated than just collections of police or fire departments) might then be tested and retested. Willer (1967: 97-115) suggests that such generalizations be labeled "conditional universals," i.e., empirically tested relationships with the universe to which they apply specified. This type of methodological strategy is implicitly similar to the organizational taxonomy proposed by Haas, Hall, and Johnson (1966) and has obvious relevance for group and organizational research, in general.

This strategy, then, is simply an elaboration of what is implicit in the articles in this issue dealing with specific organizations, e.g., police, fire, and so on. However, the selection of criteria deserves much consideration in the future. Among the criteria which appear to be of at least equal importance to goal, are system permanence and system complexity. Hence, emergent groups of persons clearing up debris and performing search-and-rescue operations are highly transitory in comparison to family groups. Similarly, both of these are much less complex interaction systems than organizations or communities. Emergent systems of interorganizational relationships, frequently referred to as synthetic organizations, are highly complex but very transitory since such behavior patterns disappear in a few days at most. The four-fold typology of organizations and groups developed by Dynes and Quarantelli (1968), is an attempt to struggle with the problems suggested in this paragraph.

Through this line of reasoning, we can greatly clarify the object of study. Differentiation and comparisons among objects of study can be precisely made through specification of definitional criteria. It is essential to recognize that there are many possible alternative criteria from which to choose. Too often we have tried to focus on activities of many different types of groups in a single event. What this strategy suggests is that we consider the study of many units of the same type in several events. But our notion of event also needs clarification.

Having seen the need to clarify the procedures whereby we define our objects of study, we can return to the second aspects of the initial question: how do organizations and groups respond to community disasters? But what is a community disaster? Does a large-scale blackout qualify? Must it be natural or can it be man-made? Must it actually occur or might a threat qualify? Two points are necessary for clarification. First, it appears to be more fruitful theoretically to recast the issue and ask how do systems respond to severe environmental disruption? This emphasizes the interaction between the focal system and sectors of the environment, and it places disaster research within a larger research perspective.

Hence, whether environmental change is threatened or actual is irrelevant; what matters is the content of information exchanges between the two. Position incumbents respond to threatened or actual environmental change in terms of shared information, not in terms of objective reality. Earthquakes and explosions, hurricanes and fires, tornadoes, dam breaks, and the like, all qualify as types of environmental disruptions--threatened or actual. This conception of disaster is implicit in the title of the Ohio State University Disaster Research Center's newsletter, <u>Unscheduled Events</u>. System Development Corporation refers to their Emergency Operations Symposia (Brictson, 1966) in a similar manner. This broader conceptualization also helps to integrate disaster studies into other substantive areas, e.g., civil disturbances and revolutions (Quarantelli and Dynes, 1970).

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The second point follows from the first. What types of environmental disruption are we interested in and what criteria are to be used in differentiation? Traditionally, sirple weather categories have been used, or as Ellwyn Stoddard (1968) puts it, "where the oxygen happens to be." However, there are a variety of criteria that appear to be highly relevant. For example, in assessing family responses to a major flood in Denver, Colorado, five such criteria (Drabek, 1969) appeared significant in classifying the event:

- <u>suddenness</u>--in contrast to typical warning periods of two or three days, only five hours elapsed between first warnings and the flood waters;
- (2) unfamiliarity--few families had prior experience with floods;
- (3) unexpectedness--earlier weather data made no mention of a flood danger;
- (4) <u>highly localized in scope</u>--a strip was cut throughout the metropolitan area, but most of the city remained untouched; and
- (5) <u>warning contexts varied</u>-depending on family location due to ecology and different police jurisdictions.

While seemingly important in understanding family responses to this event, these criteria are far from being complete. The point is that we need to begin making more complex classifications of the types and degrees of environmental disruptions using such kinds of analytical criteria to define comparability in events rather than gross weather categories. Greater sensitivity to and use of these strategies will permit disaster research to develop as a part of the social sciences rather than as an isolated entity.

Types of Disaster Studies

Field studies of a highly descriptive nature comprise the bulk of existing disaster literature (Barton, 1969: 54). Only recently have efforts of a more analytical nature been made. To illustrate very simply the numerous types of studies which might be done, we can use three dimensions: (1) time after event, (2) level of abstraction, and (3) systemic level being studied. These can be diagramed as in Figure 1. Hence, the study by Moore and his colleagues (1963) of family responses to Hurricane Carls would be located at points 1A, 2B, 3A. While primarily a descriptive account, many specific hypotheses were proposed and tested. Form and Nosow's (1958) study of organizational response to the Flint, Michigan tornado would be 1A, 2A, 3B. Longitudinal studies such as Anderson's (1969a) analysis of change precipitated in twenty-three organizations over a year and a half following the 1964 Alaska earthquake would register 1C, 2B, 3B. Our point here is not to classify various studies already completed, but rather to illustrate in a simple fashion the large ratge of studies possible. Hopefully future work will vary on axes 1 and 3, but will nore frequently register B or C on axis 2.

A commonly made distinction is between field and laboratory research. However, few have chosen to use the laboratory to study group responses to disaster. In contrast to 114 field studies, only four quasi-laboratory studies were listed in the Disaster Research Group Inventory published in 1961. This rough ratio has remained unchanged since then.



Figure 1: TYPOLOGY OF DISASTER STUDIES

Let us consider three important points regarding laboratory research. First, as has been suggested, various types of disaster exercises might be used as research laboratories. Boocock and Coleman (1966) have developed a disaster "game" through which many research questions could be explored. More elaborate simulations using organizational personnel have been conducted. For example, Disaster Research Center staff members have observed simulated plane crashes at Air Force Logistic Command Bases where hundreds of military and civilian personnel participated. As yet, these types of training sessions have not been exploited extensively as research possibilities.

Second, as disaster research becomes more analytical, laboratory experiments will probably be used more often for theory development (Weick, 1967). While there are obvious operational and ethical problems in creating disaster situations in the laboratory, shifts in levels of abstraction can recast the research problem so as to render the laboratory more useful. Zelditch (1969) has stated the case well--the linkage is theory. "Experiments are relevant to theory, and <u>theory</u> is applied to natural settings" (Zelditch, 1969: 539). Given the present scarcity of theory in disaster research, laboratory studies represent a unique potential for theory development through improved specification and measurement of variables which might later guide data collection in field settings. As the types of strategies mentioned above result in more rigorous specification of events and objects of study in analytical terms, and complex relationships among clusters of variables are better identified, laboratory experiments in several related substantive areas (e.g., stress) may be synthesized with findings from disaster field studies.

Third, numerous types of realistic simulations (Drabek and Haas, 1967) can be done wherein complex environments are presented to organizational subsystems and manipulated in a controlled fashion. For example, Drabek and Haas (1969) used field data to construct a rather complex demand environment for teams of police officers. These communication specialists first confronted simulated demand environments identical to those with which they normally dealt. Then they were presented a simulated air crash. Hence, just as aerodynamics engineers use models (simulates) in wind tunnels (environment) to study aircraft design (theory) under varying environmental conditions, simulated organizations can be subjected to environmental change to test organizational theory. Similarity with a natural setting is desired in such experiments, since complexes of variables may be built into the design. But, as Zelditch (1969) suggests, generalization is based on similarity in analytical criteria, not physical appearances. It is felt, however, that work in such simulations will assist in theory development since results obtained can be compared more easily to similar field data. Through this strategy, functioning of simulated systems can be compared to actual or theoretically contrived systems. Differences in response which are laboratory artifacts may be identified more easily. Hence, realistic simulations provide a critical strategy for developing theory which better bridges the gap between the **field** and laboratory.

Numerous tactics have been used in field studies of group and organizational response to disaster. For the most part, these are similar to those generally used in organizational research. Following a reconnaissance trip in which the type of event, group and organizational involvement, and degree of local cooperation are established, researchers begin interviews. Most frequently, structured and unstructured interviews have been used to develop a descriptive picture of sequences of behaviors following the event and the subjective definitions which lead to them. Organizational members are selected so as to represent different levels of the unit. They are viewed as informants whose responses are to be treated as segments of a whole. Usually samples of members are randomly selected within subunits. Similarly members can interviewed regarding interorganizational activity. In this way, linkages can be traced which depict communication flows, decision making, coordination efforts, and the like. Random samples of victims or recipients of organizational services also can be interviewed to obtain additional data regarding organizational response (Dynes, Haas, and Quarantelli, 1967).

However, once collected, such data are not easily analyzed. Even a series of descriptive accounts as to personal behaviors engaged in or observed are difficult to dovetail into a global descriptive picture unless other types of data are available for cross-validation. To assist in this regard, tape recordings of police and fire radio and telephone communications have proved invaluable to define time periods and sequence events (e.g., Drabek, 1968). In addition, many other types of primary source data are often available, e.g., newspapers, radio and television tapes, teletype copy, and the like. Also, organizational critiques, reports, memoranda, budgets, and other unobtrusive resources can be equally useful in reconstructing sequences of events (numerous excellent strategies are suggested in Sebb et al., 1966).

To get beyond descriptive accounts requires that variables be defined and measured. Statements by Akin and Hage (1967) and Coleman (1969) propose methods whereby survey data gathered via questionnaires or interviews can be processed so as to be indicative of organizational rather than individual characteristics. For example, data can be partitioned into friendship cliques and then analyzed (Coleman; 1969: 526). However, additional inventories of organizational measurement procedures and instruments such as Barton's (1961) are badly needed. Emergent groups which are highly transitory present different problems. A variation of the "snow ball sampling" technique proposed by Coleman (1969) is probably the best method available. As names of persons in such groups are located, they are in turn interviewed and asked to name additional persons with whom they worked. In this way the entire network of work groups can be traced and interviewed with much data obtained for convergent validity. Zurcher (1960) has done a study of a work group that developed in a disaster, and Parr (1960) more recently has examined the conditions leading to emergent groups.

Design Notes About Future Disaster Research

While many kinds of observations could be made regarding future disaster research, four seem most critical. First, studies of immediate response to disasters can be done most effectively by ongoing field teams who are prepared to move quickly. Decisions regarding the particular focus of study at different cities must always be made. But teams with several design alternatives already prepared will far more likely obtain critical data before it becomes overly distorted or lost. Data collected hastily in an unsystematic fashion by social scientists who, by coincidence, happen to be in the disaster area may provide unique insights depending upon the focus of study (e.g., Taylor, Zurcher, and Key, 1969). However, it would appear that fast moving field teams such as those perfected by the Disaster Research Center over the past several years have many advantages.

Establishment of rapport with organizational officials is especially easy as team members become highly identified with the event. Persons arriving a month or so later may be far more likely to encounter cooperation difficulties, apart from other problems such as memory loss and distortion. For related studies focused on different problems, e.g., long term impact, such field teams are not necessary.

Second, additional efforts should be made in programs of research whereby field and laboratory studies might be bridged (Guetzkow, 1962). Shuttling back and forth from the field into the laboratory can be extremely helpful for theory development as in the police simulation study noted above. This is not to argue that laboratory experiments must duplicate the physical appearances of field settings. Of course, as was the case in the police simulation study, such efforts may be desirable at times for theory development. But the bridge between field and laboratory must be theoretical, as it is among any other experimental settings. That is, generalizations are made to other settings with similar theoretical characteristics. And by working in both settings, either simultaneously, or one at a time, researchers will be more highly sensitized to analytical similarities and differences. Other excellent examples are Kryter's (1969) studies in which survey research was combined with laboratory studies to assess public reactions to sonic booms. Simulations of various types appear to offer a promising strategy whereby the gap between the field and laboratory can be better bridged.

Third, the most glaring weakness in the available disaster literature is the simplistic or nonexistent experimental designs. Of course, random assignment of groups, organizations, or communities to disaster is not possible. Since random assignment and other types of controls are possible with simulations of other laboratory strategies, research programs which use both settings simultaneously are desirable. However, there are many other alternatives. Nearly all reported disaster studies are observations made after an event has occurred. There are no predata, no control groups, and no concern with random selection. Sampling has been used in several studies, e.g., Moore (1963) and Drabek (1969). However, in all these studies, families were the unit of analysis. Difficulties in sampling using other kinds of groups and organizations were discussed above. As Price (1969) indicated, even small samples of organizations would be superior to the single organizational case study.

Campbell has proposed several quasi-experimental designs which have special relevance to many of the limitations inherent in disaster studies (Campbell and Stanley, 1966). For example, especially useful is the time series experiment (Design 7) in which numerous observations are made at different points in time both before and after an event ($0_1 0_2 0_3 0_4 \times 0_5 0_6 0_7 0_8$). Ross and Campbell (1968) used this design to assess the impact of changes in enforcement procedures of traffic speed laws in Connecticut. A similar strategy could be used with many types of organizations using official records for data both before and after disasters.

Among the many design problems encountered is randomization of selected units. But by adding several groups to a design, such problems can be reduced to some degree. For example, Key and Drabek (in an ongoing project) have before and after data on experimental (disaster victims) and matched control families. Since neither group was randomly assigned, their degree of equivalence is suspect (despite matching), and generalizations to other groups are open to criticism. The design will be supplemented by selecting additional experimental and control groups on a random basis from lists of victims and nonvictims for which only postdisaster data will be collected. Obviously families were not assigned randomly to victim and nonvictim categories, but neither were they self-selected. Thus, the additional random samples provide a basis for cross-checking any patterns which might emerge between matched experimental and control groups of families. Through more consideration of alternative design strategies, disaster research could be substantially upgraded. However, researchers should recognize without apology that they cannot randomly assign units to disaster and nondisaster situations despite the elegance that this would add to their designs.

Fourth, and finally, research should be comparative. At least three different types of comparisons are desirable. First, regularities across events should be investigated. The earlier NORC studies (Fritz and Marks, 1954) and more recent Disaster Research Center publications (e.g., Wenger and Parr, 1969) are good examples. In Contrast to studies of comparative responses to one type of unscheduled event is Warheit's (1968) research in two disaster impacted and two civil disturbance impacted cities. Thus comparison of same and different event categories are needed.

Second, cross cultural responses to similar and different types of disasters are desired. Clifford's (1956) comparison of Mexican and Texas populations in the 1954 flood on the Rio Grande remains best known but Anderson (1969b) has recently analyzed the response of the military in four different societies. Equally important for comparative analysis is Moore's (1964: 195-221) concept of disaster subculture. Thus, while two populations may have similar cultural backgrounds at a general level, seasonal disaster experience through floods, hurricanes, tornadoes, and the like, may precipitate varied responses. Research using this concept as the comparative base merits much attention. A start has been made on the ongoing study by the Disaster Research Center of those cities that regularly undergo floods. Third, as is reflected in the articles on specific organizations in this issue, comparative analysis among similar groups and organizations is required. Hence, many different types of comparative analyses should serve as bases for future research programs.

Presently, disaster research is more noted for its challenge and methodological simplicity. Yet, as indicated by the articles in this issue, much progress has been made in the past few years. Hopefully, consideration of the ideas, strategies, and problems discussed here will assist persons accepting these challenges in attaining increased methodological sophistication.

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