

The Disaster Research Center (DRC) Field Studies
of Organized Behavior in the
Crisis Time Period of Disasters

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This chapter discusses the methodology of the field research undertaken by the Disaster Research Center (DRC) between its formation in 1963 to 1989. During that time DRC conducted more than 450 field studies of community crises, the great bulk of them involving natural or technological disaster agents. The major focus was on organized behavior whether in formal organizations or informal and emergent groups, and usually about the social entities involved in the preparedness and response activities in the crisis. After noting the background context within which the Center operated, this chapter summarizes the general methodological approach taken. It depicts the substantial attention DRC paid to the prefield training that was given to the graduate students who did most of the field work. Also described are the in-field procedures followed, particularly the open-ended type of interviewing conducted, the kinds of participant observations made and the systematic document collecting that was done. We also note certain post-field procedures systematized by the Center to measure the quantity and to insure the quality of the gathered data. In the conclusion, we suggest four general implications or lessons for other researchers who might want to follow the DRC model.

INTRODUCTION

DRC since its inception 35 years ago to the present time has used most social science methodologies in its data gathering activities. Even in the first decade or so of its operations, these included as examples, laboratory studies of simulated police department radio dispatching rooms (Drabek 1970); participant observations of civil disturbance situations (Ponting, Fitzpatrick and Quarantelli 1974), content analyses of disaster films (Quarantelli 1980), and traditional population surveys of impacted communities (in the 1972 Wilkes Barre flood and the 1974 Xenia tornado; see Taylor, Ross and Quarantelli 1976). Likewise, different units of analysis have been utilized as well as different time periods of disasters. Thus, the Center has looked at the full range from the micro, the individual, to the macro, the nation state or society. There have been studies, but not equally, of the mitigation, preparedness, response and recovery aspects of disasters.

However, our intent here is to depict only what was the major although not exclusive field research methodology of the Center from 1963 through 1989. In that time, the primary focus was on organized behavior in the emergency/crisis periods of disasters (the latter being what is currently known as the later preparedness and early response phases of disasters). Organized behavior included not only formal organizations but also informal and emergent groups.

The focus on organized behavior was a very conscious one. It is impossible in this chapter to detail the complex of theoretical, professional and practical reasons for the choice. Suffice it to say here that a major reason was that while the earliest studies in the area had already established the parameters of the behavior of individuals in crises (see Quarantelli 1988), almost nothing was known about group behavior. Also, field studies of persons undertaken during the actual crisis times of disasters always have difficult sampling problems with an unknown universe of participants, whereas the universe of most organized groups in a community is known and finite (e.g., typically there is only one fire and police department, a handful of mass media outlets and hospitals, etc.). Finally, even in the pioneering days of disaster research it was evident that the most effective and efficient planning and managing measures for responses during crises would have to be primarily carried out by organizations and could not be done by individual households.

The basic model DRC employed was set early. It was initially drawn from the pioneering field operations of the National Opinion Research Center (NORC) in its 1949-1954 research on disasters (see, Quarantelli 1987; 1988). However, also influential was the research focus on organizations per se for which conventional survey methods during actual crisis times were not appropriate or difficult. There were minor refinements and augmentations over the years. In the main what we depict is the later rather than the earlier versions of the procedures. Also what is set forth, using a Russell Dynes characterization, is the "generic" version rather than all the variants used in the different research projects DRC undertook. Finally, while we generally depict what was intended to happen, we do note difficulties in achieving what was wanted or planned with relation to specific matters and near the end of the chapter indicate some problematical aspects of the whole enterprise.

GENERAL BACKGROUND

The possibilities and problems inherent in the kind of field work DRC undertook requires understanding the general context within which it operated. This is not the place for a history of the Center, but we note selective aspects since they affected the research planned and done.

From its inception, DRC was administratively nested in and informally part of a department of sociology. However, the Center for most of its existence never had any funding from any university source, thus making organizational control by other entities over its operations mostly nominal. Except in very recent years, funds for all DRC activities were from the research grants and contracts it obtained on its own. In the early years most funding came from the predecessors of the Federal Emergency Management Agency (FEMA) (e.g., the Office of Civil Defense---OCD), but that was increasingly replaced in later years by grants from the National Science Foundation (NSF). While the support from OCD came through contracts, the initial relationship that quickly evolved soon allowed DRC, within very broad limits, to do what research it wanted in whatever way it thought best. Overall, DRC had more freedom in its operations than many centers or institutes typically have, a professional advantage the Center used in the studies it independently launched, as illustrated later.

As an organization, DRC was never systematically planned or even formally created. The Center was never a formal part of the Ohio State University. In many sense, it was an emergent group with

decisions on group structures and functions forced by situational contingencies. Thus, its evolution had neither the advantages nor the disadvantages of a structured path. But implicitly at least, DRC from the start was thought of by its founders as primarily a social science research entity. It was never visualized as having teaching functions, providing any formal training or having a consultative role; and as a group no such activities were ever undertaken. Nevertheless, the sharp focus on research did lead to several auxiliary activities that eventually were important in the Center's history, namely the systematic creation of an archive of its own field data, the establishment of the largest specialized library in the world on the human and social aspects of disasters, as well as the development of its own publication program.

From a research perspective, DRC from its inception looked at all kinds of "disasters." The natural versus technological disaster distinction was ignored in its work (contrary to some statements others have made about its focus), although sometimes particular projects because of their funding source focused on a particular kind of disaster rather than another (e.g., the DRC pioneering study of chemical disasters in the early 1980s, see Quarantelli 1984). As an indication of its generic approach, the first ten events studied were a hurricane in Texas, the overflow of a dam in Italy, the Coliseum explosion in Indianapolis, a nuclear plant accident near San Antonio, a nursing home fire in northern Ohio, the dam break in Baldwin Hills California, a plant explosion in Massachusetts, a flood in Cincinnati, and the Alaskan earthquake, and also a student civil disturbance in Columbus, Ohio. Also, as just said, DRC sometime studied civil disturbance and riot occasions (Warheit and Quarantelli 1969), mostly for comparative purposes since it was assumed such conflict type episodes are somewhat substantively different from natural and technological disaster occasions. While DRC found it could use the generic field research methodology in all its work, conflict situations did require adjustments that however are not discussed in this chapter.

The quantity of field work done also partly dictated the need for a standardized field procedure. From 1963 to 1984 while DRC was at Ohio State University, it undertook 457 different field studies (in about a third of the cases this involved more than one actual trip to the site). In several years, field trips were quite numerous, there being 55 different field studies in 1969 alone, 51 in 1972, 50 in 1982, and 46 in 1979. Only in two years did the number of field studies drop below the double digit. As such, it was not rare to have two different teams in the field simultaneously, and occasionally three teams were concurrently in the field.

The great majority of work was done in the United States. However, 18 field studies were done between 1963-1973 in 11 foreign countries (five times in Canada, three times in Italy, twice in Japan, and once each in Mexico, Chile, Greece, El Salvador, Australia, Iran, Curacao and Yugoslavia). These foreign studies mostly ended with the emergence of native disaster researchers in many places.

The actual field work especially after the first few years, was done by graduate students. Depending on the research funding available, these at any given time numbered between four and fifteen (the total DRC personnel once peaked at 59 staff members, but two dozen was the more typical work force). These students appointed as research assistants (GRAs) were mostly drawn from sociology but in later years were also selected from other areas (e.g., political science, journalism,

anthropology, nursing, etc.). In principle they were employed only on a half time basis (20 hours per week), but the informal understanding was that there was no limit in doing field work. While major efforts were made not to disrupt class attendance and examination taking, it was nonetheless an unambiguous stipulation for employment that all GRAs had to be available for field work on any day at any time of the year, university holidays and vacations to the contrary. In part this was related to the fact that DRC never employed GRAs for a specialized work role (e.g., field interviewer, coder, or any other specific task). Instead, it was an explicit condition of employment that all would work on all research aspects ranging from the designing of the field work through data gathering to data processing to data analysis and initial report writing. It was also assumed that GRAs would work on all research projects in being during their employment (usually two or three, in rare instances, more).

Most Center funding was for specific research topics. As an example, DRC once had a five-year contract with OCD to study the major community organizations involved in disasters; under this, particular studies were done of local emergency management agencies, police and fire departments, hospitals and related entities, the public utilities, and the Red Cross. DRC also specifically studied the delivery of mental health services under a grant from the National Institute of Mental Health, and the delivery of emergency medical services through a Health Resources Administration grant.

In addition, DRC at times was funded to study very general topics such as "community coordination" or "organizational functioning," labels deliberately vague but which allowed the Center to venture into different research areas. For instance, DRC studied the military, religious groups and --in the early days of its existence--the mass media, which otherwise probably could not have been attempted at that time. This work did at times lead to more specific studies, for example, a later NSF grant to study news reporting by community mass communication systems. In addition, DRC taking advantage of the professional freedom mentioned earlier, did studies not directly funded by anyone. These were "piggybacked" in various ways on funded projects. Among such research were studies on the handling of the dead in mass casualty situations, disaster induced long run organizational changes, the characteristics of disaster subcultures, state level disaster planning, rumor control centers, and non-riot looting behavior, etc.

DRC chose all the topics it studied. It rejected suggestions by funders that were not of professional interest to the directors or that made little sense on the basis of earlier research (e.g., a proposal by a federal agency to fund the Center for a field study of the "looting" in Hurricane Camille). Important is that a conscious effort was made to keep moving on continually to new topics for study. In only a few cases did the Center replicate or build upon previously studied topics (the major exception was a multi year restudy for FEMA of an earlier five-year OCD study of emergency organizations). There was a logic to this pioneering. For years DRC was the only research entity of its kind anywhere; given that, studying new topics was seen as the best way to call attention to the importance and significance of social science studies of disasters. At another level, a pioneering effort is more of an intellectual challenge, requires innovation in the field design, and is certain to generate unexpected findings. In short, we found it both "fun" and "interesting" to pioneer, and accordingly did so.

DRC research in one sense operated at two levels. At one level, the intent was to understand disaster phenomena per se such as the Conditions that generated disaster problems, the Characteristics of organized disaster behavior, the later Consequences of that, and the Careers of disasters (this jokingly came to be known as the C model). At another level, the goal was to further sociological understanding of emergent groups and organizational behavior. Put another way, the DRC sociologists true to their disciplinary background, assumed that the better sociology they did, the better would be the research on disasters. Therefore a conscious effort was made in analysis and report writing to interplay the descriptive disaster data and sociological ideas, but a balance was not always achieved. While the Center was successful in explicitly resisting the development of a separate field of "disasterology," and had some success in developing the sociology of disasters, the latter was not as much as might have been ideal.

Yet the Center never operated with one explicit theoretical orientation. There was in fact a conscious effort to avoid the development of a Center orthodoxy or "party line." Nevertheless, certain views about social phenomena were implicitly used more than others. A sociological orientation was always present, and not a geographical one as in the early hazards studies by others. In addition, the implicit social psychology framework used was symbolic interactionism. Similarly, ideas from the classical University of Chicago view of collective behavior permeated the approach to emergent behaviors and groups (for the historical background of these two orientations see, Quarantelli 1987, 1994). Organizations, on the other hand, tended to be viewed, but not always consistently, in an amorphous structural functional framework. Our point is that the field operations and research procedures were influenced by the indicated theoretical preference so while a rigid theoretical orthodoxy was avoided, DRC did lean implicitly in certain directions rather than others.

As loose as the theoretical preferences were, the DRC methodology was even more eclectic and catholic. There was to be sure a preference for methods that allowed induction rather than requiring deduction and which allowed qualitative rather than demanded quantitative analyses. In a general sense, what DRC by trial and error evolved, was similar to the "grounded theory methodology" which was being created roughly at the same time by other sociologists with no direct connection to the disaster area (see Glaser and Strauss 1967). However, the Center did not explicitly or consciously borrow from the formal literature on grounded theory. In fact, it was quite a while before DRC consciously recognized that it too had gone down the same methodological path developed by grounded theory scholars.

PRIOR OR PREFIELD PROCEDURES

While there was "trial and error" in training the first cohort of GRAs, from the first the prefield training was deemed very crucial for their ability to do well at disaster sites. So much time and effort were spent on training. Among procedures usually followed were providing all new GRAs: (1) a general introduction to the history of disaster research and the Center, (2) the procedures, promises and problems in qualitative field research, and (3) a detailed introduction to the specific research project(s) in which they were to be involved. Indicative of the coverage of this training for new

GRAs are the topics of the 30 sessions listed in the outline for 1987, and given by the DRC directors.

OUTLINE FOR 1987

I. BACKGROUND ON THE DISASTER AREA AND DRC. 1) Nature of disasters and disaster preparedness and response in the U.S.; 2) History of disaster research in general in the social and behavioral sciences; 3) Overall view of substantive disasters findings in general; 4) History and activities of DRC; 5) Past DRC work and resources; and 6) DRC operations including logistical issues.

II. DRC FIELD WORK. 7) General orientation and policies (including ethical issues); 8) Preparations and entry; 9) Interviewing problems and procedures; 10) Observing problems and procedures; 11) Documenting problems and procedures; 12) Processing of field data; and 13) Report writing.

III. THE FEMA STUDY. 14) FEMA as an organization and its interests; 15) Earlier related DRC work; 16) Last's year's work; 17) Projected field work for the coming year; 18) The research designed, new issues and questions that need consideration; 19) Specifics of the research design; and 20) Planning and actually doing the upcoming field work.

IV. THE NSF MASS MEDIA STUDY. 21) NSF as an organization and its interests; 22) Earlier related DRC work; 23) Projected field work for this the coming year; 24) The research design: issues and questions that need consideration; 25) Specifics of the research design; and 26) Planning and actually doing the specific field work.

V. THE NSF MEXICO CITY EARTHQUAKE STUDY. 27) The study design used; 28) The survey work undertaken; 29) The organizational data obtained; and 30) Current status of the work and what needs to be done to finish the study.

After the training sessions, new GRAs typically conducted practice field interviews with officials in local emergency related groups (defined for them as studies DRC was doing of preparedness planning). The tape interviews were then listened to by veteran DRC researchers, with the positive and negative aspects of what had been done individually discussed with each new GRA.

Even more important, at the start of any research project all GRAs were given copies of the funded research proposal. After being told to read the proposal carefully, a series of meetings were held, the first of which started with roughly the following statement: " This formal proposal gives you a vague idea of what we think we are going to do: now we are collectively going to have to work out the details of how we will actually proceed in the real world and what we concretely need to find out." Succeeding sessions particularly focused on developing research design specifics (e.g., the field instruments, early versions of which were often drafted by smaller task forces of GRAs). The basic goal was to involve GRAs in the process so they knew what information was to be sought from whom, why specific questions were asked, and generally how much detail was wanted given the data analysis projected. Fully involving the GRAs in building the research design insured they understood the logic of what DRC would do in the field. Interview and other guides were always produced but by that point, GRAs needed no further guidance on their use. GRAs who joined DRC in the middle of research projects were not as heavily involved in building the research design, but there was always overlap of veteran and rookie GRAs so the latter could informally learn from the former.

A most important idea conveyed in the training was that DRC mostly wanted the overall picture of the occasion, and no one or even several officials or persons together could possibly provide by themselves such a picture (it was stressed that in disasters perceptions are even more selective and narrower than at routine times). So like doing detective work, it was emphasized that the team would have to develop the larger picture by putting together information from a variety of different kinds of field data. Three major mechanisms were used to sensitize the GRAs on this point: (1) emphasizing this process in the prefield training; (2) interviewing at different levels and positions in the groups studied (from administrative heads to liaison personnel); (3) in-the-field team meetings. There is not space here to discuss how inconsistent or contradictory statements were handled, although it was stressed in the training that when the perception and social location of informants/respondents is taken into account, there are far more "seeming" rather than actual irreconcilable statements.

The specifics of what social dimensions were to be examined varied somewhat in each study. However, in general information was almost always sought about the structure and function of the group involved, the formal and informal division of labor in being, its interorganizational contacts, the available material resources and facilities, as well as the group's prior disaster planning and earlier disaster experience. It was always very explicit that the field workers should make a very conscious effort to obtain a picture of the organization as it operated during normal times and how it acted during the crisis period of the disaster being studied. This contrast was frequently talked about as the differences between Time One (preimpact) and Time Two (impact). In its later work, DRC tended to make more of a differentiation between the preimpact or preparedness phase, the impact or transemergency period, and the immediate postimpact period.

Also stressed in the training was that while particular officials or workers would be approached in the field, the picture the Center wanted was how the group or organization of which they were a part operated. DRC only had a secondary interest in the individuals as such. Put another way, the theme was that the focus of the Center was on the group level, what the organized group as a social entity did, not what social roles were played by particular people. This point was not always easy to communicate in the training because even professional sociologists do not always understand the difference between the study of a group as such and a study of the members of the group (the emphasis on using as much participant observation and systematic document collection as possible was also part of the effort to get the GRAs to think in group rather than individual terms; easier to see in using those techniques compared with interviewing individuals). This focus on the group level became even clearer in later DRC work and was therefore made even more explicit in the training.

Related to the point in the last paragraph was that in almost all projects DRC wanted the multiple perspectives of those involved. Thus, it was fairly standard in the research design that certain work positions or occupational roles were automatically part of the listing of those who should be interviewed (or approached for other kinds of data). It was stressed in the training that it should be expected that DRC would obtain different accounts of "what happened" depending on the perspective of those reporting the happening. The Center did assume that there was no one "true" story. As such, in the training it was pointed out that in almost all cases there were at least a minimum number

of perspectives we wanted to obtain. These usually included the administrative head of the organization, its operational head (which DRC early learned usually knew more specific details about everyday activities of the group whether in routine or crisis times than did the administrative head who was often more concerned with policy and political issues), boundary personnel such as liaison persons or secretaries who represented or linked their organization to the outside world, and communication personnel at almost any level. The last role in particular could be rather low in the formal organizational chart but frequently would not only have substantial information on the what and who in the communication flow, but also often could supply to DRC relevant statistics or at least numbers on the flow. Of course, as was indicated in the training of the GRAs, there was frequently more than one occupant of the indicated roles in organizations and that should be taken into account on who should be approached in the field work. In emergent or informal groups, the kind of social roles just indicated would be less obvious, but for DRC purposes we should seek out those persons who engaged in equivalent activities. In addition to the somewhat fixed sampling of certain work roles, "snowballing" sampling was also used as a very useful procedure given what the Center was often studying.

Apart from the training for substantive issues, DRC also paid much attention to logistic matters in the dispatching of teams. This was important for achieving the major objective of getting to a disaster site as soon as possible, while emergency operations were underway. In the 1964 Alaskan earthquake, DRC had five persons, including all three directors, in Anchorage within the first 24 hours. This process is less complicated than might be believed if routines are developed as DRC did early in its life. This included (1) training the Center's office staff how travel arrangements were to be handled both within and outside the University context, including educating others that normal travel procedures could not be followed and the need sometime to operate outside of usual University hours; (2) preparing a standardized field kit always in the personal possession of all GRAs; (3) insisting that all GRAs at all times have luggage bags prepared for travel; and (4) putting in the possession of all field workers a master check list. A later version of this list specified checking the following:

MASTER CHECK LIST

1. Tape recorder (test by recording and playing back before leaving).
 2. Charger, cord.
 3. Blank cassettes and boxes with identifying cards.
 4. Field kit folder.
 5. Personal DRC business cards.
 6. Personal identification letter with photo.
 7. Auto rental credit card*.
 8. Receipt forms.
 9. Stack of DRC leaflets.
 10. DRC fact sheets.
 11. 11. Forms for reporting expenses.
 12. Sheet with office, home addresses and phones.
 13. List of current telephone billing numbers.
 14. Pens.
 15. Pencils.
 16. Tablets for note taking.
 17. Traveler's checks (as well as enough cash).
 18. DRC card for auto sunvisor.
 19. Interview guides.
 20. Special handouts.
 21. List of prior contacts in the area*.
 22. Data analysis forms.
 23. Data inventory forms, if relevant.
 24. Video cassette and cameras*.
 25. Self addressed stamped envelopes.
- The field coordinator is to have the items indicated as * .

Learning About Disasters

All personnel at the Center were instructed to be attentive to news reports or weather stories

that might have implications for possible field work (although DRC learned early that initial accounts tended to overstate damage and destruction). Usually such information was directed to the Field Director, who had the authority to dispatch field teams. Generally the judgment on whether a team should be sent was made on whether the reported occasion fitted the existing criteria for inclusion in whatever projects the Center had under way (sometimes field data could be concurrently obtained for two different projects). At times a decision had to be made on whether a team could be gotten into a locality before actual impact (e.g., possible in developing hurricanes or floods). Thus, there was more than one occasion where a DRC team managed to arrive on site before the airports in the area were closed. In other cases, where DRC had done previous field studies, knowledgeable officials known to the Center were contacted and information obtained from them.

The value of being on the scene at the height of crises cannot be overstated. It is worthwhile to be in such situations for two basic reasons. First, observations can be made and documents can be collected, that cannot be obtained through later interviewing. The social barriers that normally exist to restrict access to high level officials and key organizations, simply to not exist. A second reason for being on the scene early insures a high degree of access and cooperation. Victims are typically candid, cooperative and willing to talk in ways far more difficult to get later.

Arriving on the scene

Field teams on arrival did two things as quickly as possible. (1) finding living quarters and (2) going to Emergency Operations Centers (EOCs). The last was to learn what was going on as seen from the perspective of community officials, keeping in mind that a "command post" bias would be present. The basic purpose was to obtain an overview of the situation, learn who the key officials and organizations were and also what they were doing, make personal contact with them for later follow up interviews, gather ephemeral observational and documentary data, and lay the groundwork for a later systematic in depth study. Contrary to popular imagery, there can be many periods of lull and inactivity even in the most hectic of disasters where interaction with officials can be initiated, provided entry is gained into EOCs or coordinating centers (although not discussed here obtaining entry often requires considerable skill with a special need to be seen as researchers and not "journalists").

A variety of research techniques were used for data gathering, particularly open ended interviewing, selective participant observing, and systematic document collecting.

Interviewing

The guides and procedures used by DRC were basically for open-ended and in-depth interviewing. The usual lead question was deliberately very general such as: "tell me what happened in x (e.g., the tornado)." Other suggested questions were often also very general putting a premium on the field worker's ability and knowledge of when, where and what to specifically probe. However, while formal interview guides were always prepared (with questions, probes, instructions) and taken to the field, they were never thought of as field manuals to read from. Given the involvement of GRAs

in their production and thus their presumed understanding of what was wanted, the guides were instead used to insure field workers covered all relevant topics.

Certain principles that all field workers were to follow were evolved. In a later explicit version, these were, to paraphrase the oral and written statements provided in training sessions:

(1) Always tell the truth about who we are and what we are doing. Apart from ethical considerations, from a pragmatic point of view it is much easier to proceed in that way since a "cover" story does not have to be remembered. However, you should not volunteer too much information or details, unless asked to do so. Overexplaining often confuses people and may raise unnecessary questions.

(2) DRC does not seek publicity especially in the field, and particularly from the mass media. Try to fend off inquiries by using the DRC handouts which indicate who we are, what we do, etc. If such contacts are unavoidable try to structure the picture of us we want portrayed. The principle to follow regarding mass media contact for information about the study is to refer the problem back to the home office of DRC.

(3) Indicate very explicitly the confidential nature of the information we seek and obtain. Make clear that once in the Center's hands we have the responsibility to protect any data, and we take that very seriously. If any problems on this score have them contact the home office of DRC.

(4) We should try to have ourselves identified as researchers and from a university. Such labels evoke positive reactions. You must make conscious effort to avoid being misidentified as "journalists" or "investigators." We are not there to judge, to evaluate; why word "investigation," "investigator" etc. should never be used. The term "government" also needs careful use since not everyone is positive to it.

(5) Adjust to what is going on rather than trying to fit others into our study. In crises (and even outside in many emergency related groups), there is no 8am-5pm weekday schedule. This means that field teams can make contacts literally around the clock and in nonroutine locations (e.g., at EOCs, emergency shelters).

(6) Make field decisions on the basis of future consequences. The question of pressing for a particular interview, entry into a specific organization, seeking information about some sensitive topic, etc. is a field team decision. The judgment should be based on the future rather than the immediately present situation. Before pressing regarding something, ask would it matter and in what way if you or another researcher were to later contact the involved parties again? Since we do follow up and repeat studies, our reputation is important.

(7) Under no circumstance should any DRC field worker get in the way or interfere with any emergency operation or personnel. Similarly, never under any circumstance should you

participate in any emergency disaster response even though there are times when you may be asked to do so, and the task may be minor.

(8) There may be crises where there might be some general personal risk involved for team members. It is understood that no research result is worth any personal risk. Thus, use your common sense and avoid or move away from such situations.

(9) There is the danger that hearing more or less the same "story" over and over again, it may be assumed that the overall picture is clear. However, team members should ask the same question to everyone and to probe the answers, because that assumptions should not be made. Beware also of the "collective consensus" and "retrospective redesccribing" that often occurs the later from the time of the actual occurrence of any action. Keep placing your interviewee back to the time period being reported on. Ask for a step by step temporal and spatial chronology. Stress that our interest is in the interviewee's own perception of happenings.

(10) Keep in mind that for diplomatic reasons, some non-substantive interviews may have to be conducted. In complex organizations, it is not wise to interview lower and middle level personnel without first obtaining an interview with a high level official (where the field study and topics to be probed can be noted). Interviews at different levels of an organization assure getting different perceptions of what occurred, because of the DRC assumption that there is not only one "true" story.

(11) Never forget our distinction between informants and respondents as this is understood in sociology. Different kinds of information are obtainable from each perspective. That informants are discussing matters in which they were not personally involved does not mean that the information is not useful. Always disentangle statements involving the two perspectives because in any interview, interviewees may wander from one to another.

(12) Field teams are not micromanaged. While consultation back to DRC is encouraged, teams should make their own decisions about field problems and questions. You have considerable autonomy; you will not be second-guessed on your decisions.

These instructions seemed to work well. Not only were outright refusals to participate in the study extremely low (in most field studies none ever occurred) in the thousands of contacts made, but the cooperation was such that candor was the norm rather than the exception. We could attribute all of this to the excellence of the DRC planning and training and its implementation in the field, but that would be an overstatement. Our field work was helped by several psychological reasons, primarily that persons under stress are far more willing to talk without reservation than during normal times, and secondarily by the fact that respondents and informants were often very impressed that the team had traveled from afar to try and learn from their experiences on how other communities elsewhere might cope with a disaster.

A member of the field team was usually designated as the field coordinator with final responsibility for field trip decisions. For the educational experience, this temporary position was typically rotated among the GRAs (when the Field Director was not a member of the team). This

choice and the actual composition or mix of the team when choices were available required some tact and sensitivity in the decision making. As in any work situation, not everyone liked everyone else, various kinds of rivalries and conflicts sometime existed, and despite attempts to standardized the role, there were varying views of what constituted responsibility. So while DRC found liking others was not necessary for good field performance having a professional attitude about the GRA role was crucial. Overall, most team efforts went well but there were occasions when because DRC had put together a poor field team mix, there was a negative effect on the data obtained in field operations.

Observing

Although DRC did work out several systematic field observational guides for crowds and civil disturbance situations, it never developed systematic and general parallel ones for disastrous occasions (in retrospect this is probably a major shortcoming of the DRC field research methodology). Nonetheless, field team members were trained to be alert to make relevant observations of unscheduled as well as scheduled happenings (briefings, press conferences, etc.), and if possible to tape record their observations live (including, for example, photographing or drawing diagrams of EOCs, etc.). The simple fact is that field workers who do good participant observations can "see" things that cannot or will not be reported on in a later interview.

Let us illustrate through a personal example from Hurricane Betsy which hit New Orleans. At the height of the crisis we were able to walk into the Mayor's office which then was in a top floor of a high rise building which was informally being used as the major EOC and sit in his chair without having to go through a single secretary or obtain the permission of anyone. We just walked in. While there, we monitored the radio messages being sent. At one point a police car reported several stores with broken windows might be vulnerable to "looting." To our surprise, a half hour later the dispatcher ordered two other police cars to go to that location to stop the "extensive looting going on." But the initial police car had only reported certain open store fronts were susceptible to looting! It is very doubtful later interviewing would have evoked the sequence of events that we saw and heard, especially since the informal log used only recorded the later sending of the two cars.

Also, observations were often intermingled at the same time with other procedures for obtaining field data. For instance, in one study the Center focus was on outside-of-the-hospital handling of mass casualties. As indicated in the following outline reproduced from what was used by the GRAs, much of the information sought could be obtained in observing particular situations, although others of it might require at least some informal interviewing. In addition, the cues and ideas picked up earlier while in an observational mode could be made part of a later interview with some of the observed emergency medical service providers at a triage point (Again crucial for this to be done well required that the GRAs have a very good picture of what the project was about, what questions DRC wanted answered, and how different answers might fit together).

OUTLINE

INSTRUMENT III. EMS DISASTER SITE DATA NEEDED

WE WANT A CHRONOLOGY OF INVOLVEMENT, THE INTERORGANIZATIONAL LINKS THAT EXISTED AND THE RESOURCES USED. TO MOVE TOWARD THAT PICTURE THE FOLLOWING QUESTIONS NEED TO BE ANSWERED:

1. How many triage sites?
 2. Was there any observable overall coordination of multiple sites (if such existed)?
 3. Who was in "charge" at each triage site?
 4. What organizations participated at each site?
 5. What were the modes of transportation used from each site?
 6. Was there any communication from the sites with the hospitals?
 7. Was there any communication from sites to transportation units?
 8. Which organizations handled transportation and what % was handled by each of the parties?
 9. What hospitals were destinations from each site?
 10. How many casualties were sent off from each site?
 11. Were casualties treated and/or examined but not sent to hospitals from each site?
 12. Was there any other kind of communication observable from site to medical and/or general coordination centers? What?
 13. When was the triage site set up?
 14. How long did it exist?
 15. By when had the bulk of the casualties been sent off?
- MAKE SURE YOU IDENTIFY MEDICAL COMPETENCE (i.e., physician, nurse, EMT, medical aide, etc.) AND AFFILIATIONS (organizational name) OF ALL INVOLVED.

In connection with the observational procedures, in later years sporadic efforts were made to have GRAs take photographs in the field. For various reasons, this only occasionally produced good results. Some of what DRC attempted to obtain is noted in a 1986 statement for field workers:

PHOTOGRAPHS that SHOULD BE TAKEN OR OBTAINED BY DRC FIELD TEAMS.

We are not interested in photos of physical damage or destruction; for the most part these can be obtained from mass media sources or appear in souvenir books put together by professional photographers. What DRC primarily needs are photos that either illustrate some social or even better sociological aspects of the disaster situation. For instance, there could be photos showing convergence on disaster sites, of search and rescue teams going through debris, of officials working in a very crowded EOC, of non-medical personnel handling the injured, of victims waiting in line at relief centers, of a very large evacuation area holding only a handful of evacuees, of backups at road blocks (shots that show misconceptions of disaster behavior would be especially valuable), of announcements and blackboards with information (including those that cannot be read very well), of people laughing against a background of damage or children playing in the debris, of emergency organization switchboards at the height of the crisis period, of mass media personnel crowding around victims or officials, etc. Basically keep in mind what is known about disaster behavior and if you come across scenes either supporting or contradicting such supposed knowledge, shoot it. We are social scientists and our photographs should be shots of social not physical phenomena.

Documenting

For each project DRC frequently had two lists of documents to be collected. One, the master list was used in all field work. It listed items that were always to be obtained. This included, for instance, local phone books, maps of the area, police statistics of their activities, etc. In addition, there often was another list, a special one, which was more specific and addressing particular research questions. For example, the GRAs once were given a memo to take into the field about the kind of statistics and from which organization they ought to be gathered. This was for the purpose of addressing research questions dealing with what features might be used to identify disruption of community life. The memo specified that:

MEMO

In all future disasters of any magnitude, with appropriate modifications, DRC shall attempt to obtain statistics, afteraction reports and whatever other documentary data are available from the listed organizations. The intent is to try and to develop measures of a quantitative sort of the disruptions and difficulties a community undergoes as a result of a disaster. Unless otherwise indicated, we should get the statistics on a three week period around the disaster (assuming an emergency period of one week), and a comparable three weeks a year before.

1. Police (city, county, state) Crime and traffic reports/arrest statistics. Note looting cases.
2. Fire (city, county) Statistics on calls and alarms.
3. Public health dept. (city and county) Whatever vital statistics maintained.
4. Utilities: water, gas, electricity. Usage statistics.
5. Telephone co. Statistics on local and long distance calls.
6. Western Union. Statistics on telegrams.
7. Post office. Mail delivery statistics
8. Hospitals (all general ones) In/out patients and emergency room statistics.
9. Chamber of Commerce. Business and economic statistics.
10. Airport. Statistics on plane, freight and passenger traffic.
11. Port. Statistics on shipping activity
12. Railroad yard. Statistics on freight car loadings and movements.
13. Bus terminal. Statistics on traffic.
14. School system. Statistics on absentee rates.
15. Rental agencies. Statistics on turnovers.

We should note that in the DRC framework, the term "document" was used to cover anything of a physical nature that could either be obtained or copied. Thus, for example, it included, at one end, relevant graffiti, signs on buildings, notes placed on EOC bulletin boards, informal organizational logs and group minutes, citizen recordings of the event, jokes circulating about the occasion (including gallows humor), to at the other end, official handouts, public proclamations and press releases, written organizational data (e.g., charters, budgets, annual reports, disaster plans, manuals, afteraction reports), printed community data (e.g., Chamber of Commerce profiles, telephone books), statistics from emergency related organizations (with similar data from the previous week and year), and mass communication stories.

The first sets of documents are quite ephemeral and if not quickly gathered on site, will be forever lost. They can be quite important. For instance, a team member in one disaster was told by a police major that he had ordered and then quickly rescinded an evacuation order concerning police cars in an endangered area, and showed the informal logs on it. The DRC research assistant alertly asked for and obtained a copy of the log and brought it back to DRC. Later when the Center obtained the official organizational logs, the order and rescinding of it was not at all noted. This was not a cover up. It turned out the official logs only listed actions taken; since the police cars never left the area, this was never recorded. Yet it surfaced the problems of organizational officials attempting to follow plans that do not indicate what is to be done when contradictory advice is offered.

Developing a field consensus about the data.

Very important is that at the end of the day in the field, the team members met and collectively discussed what they had learned in their interviewing and observing. When done every day, this allowed a slow reconstruction of happenings during the disaster, and also provided clues on where the research attention ought to be directed the next day (e.g., in studies of emergent groups where the "snowballing" sampling ought to proceed). Also, as pointed out in training sessions, this meant that after several days the DRC team would collectively have a broader and more comprehensive picture of the disaster than would be known to even the most heavily involved organizational official. The danger of assuming too much because of this knowledge required that team members had to be very alert in asking the same questions and probing appropriately, later respondents and informants. This did not always occur. DRC early established that later interviews in the field were without good reason often shorter in length than earlier ones.

POSTFIELD PROCEDURES

Upon returning to the Center the field team would present a detailed briefing to the staff, make its recommendations on whether to do an in-depth study, and process the material it had collected. Reconnaissance trips led to in-depth studies about 40 percent of the time. These initial trips were not wasted abortive efforts since they provided good feedback for improving field instruments, gave valuable insights on substantive issues, and was realistic field training for new GRAs. The directors always made the final decisions on in-depth studies. It was learned early that the best timing for such work was usually two-three weeks after impact and not while the crisis period was still in being.

A field report of about 3-10 pages was usually written for almost all trips taken. Its purpose was twofold: to produce a quick historical record of the study and to force the field workers to think substantively about what they had studied. This is illustrated by a 1978 guide for writing field reports that required the following information.

GUIDE

I. Identification Data: Field trip report #, event name, date of report, author of report, field team members. Type of trip (baseline, planning, actual event, follow-up, other), purpose of trip.

- II. Disaster Agent Characteristics: Briefly describe disaster agent and community context, agent type (see inventory list), scope of impact--localized or diffuse, speed of onset--sudden or gradual, prior warning or not, length of warning period, scope of disaster planning--community wide, organizations mainly, interorganizational, little or no planning. Previous disaster experience in last five years--if so what kind. Losses to community--deaths, injuries, residences and businesses destroyed, evacuees. Was there a federal declaration, if so, date of declaration.
- III. The Organized Response. Note organizations involved in disaster planning and/or response. Specify tasks and responsibilities of each. Note if there were any emergent groups.
- IV. Problems in Communication, Coordination and Control. Note in general terms, problems encountered by responding groups such as conflicting planning, duplication of effort, etc. Note also adaptive mechanism used.
- V. Relevant Comparisons and Contrasts. Tell briefly in what ways the preparations for or the response is similar to or different from the organized response to other events we have studied.
- VI. Methodological Notes. a. What implications does the study of this situation have for future field operations? What implications for data analysis are there in the material gathered? Are there any special problems regarding entree, confidentiality, etc.? Are there any other groups besides DRC doing research in the area--who and what are they doing? Any contact made with them?

There was also the putting together of a master list of the field data obtained. In one form prepared in 1979 the field coordinator had to indicate if and how much of the following had been obtained:

- I. Field Interviews obtained for guide A (preparedness informants), guide B (organizational informants), guide C (organizational respondents); Interview notes made in person and over phone;
- II. Field analysis forms for planning, response, organizational linkages;
- III Participant Observer sheets---(1) photos of activities at site, EOCs, etc., (2) diagrams/charts of EOC layout, etc., (3) description of physical topography of impact sites, (4) other items;
- IV. Documentary data: (1) phone books, (2) maps with disaster site marked, (3) disaster plans--list of organizations, (4) communication tapes, (5) organizational logs, (6) after action reports, (7) minutes of emergency meetings, (8) organizational charts, (9) mutual aid agreements, (10) radio and TV tapes, (11) local newspapers, during, before and after impact, (12) other items--leaflets, booklets, news releases, etc.
- V. Statistical data and sources of: (1) casualties, (2) property damage, (3) injured-treated at hospital, admitted, DOA; (4) other.
- VI. Follow up calls/letters needed as well as anything DRC promised to send back.

In the early days of DRC almost all interviews, the great bulk of which was tape recorded, were fully transcribed. This followed the processing model set up by NORC in its pioneering effort in disaster research (see Bucher, Fritz and Quarantelli 1956a and b). In time this led to the setting up of a massive transcribing operation and more than 6,000 interviews were transcribed. However, the time, cost and effort were too costly to be continued and no funding could ever be obtained after 1974 for such an operation. This was unfortunate since analyzing from tapes is very tedious. The later making of summaries of tape content by the actual interviewers themselves was a partial but not

perfect solution, although it did have the unintended consequence of GRAs spending more time and effort in the field on insuring the quality of recordings!

Although informal case studies following an outline were often written, very few were ever actually formalized or published. DRC never had a major interest in case studies as such, or the disaster history of any given social entity. Instead, to the extent there were case studies, they were aggregated or combined to draw more general observations or conclusions. A worthwhile side product of involving GRAs in writing case studies is that they quickly learned, as a result of finding unnecessary gaps in the information, why it was necessary in their own field work to obtain the details specified in the data gathering procedures.

SOME PROBLEMATICAL ASPECTS

Although the system developed worked well there were some inherent problems. For one, about the time GRAs acquired substantial field experience, they graduated from the university. While a very few of the best were occasionally kept on for a year or two as full time Field Directors, the Center never had the funding to do this on a regular or continuous basis. So while veteran workers were overlapped with new recruits on field teams, the value of having much field experience was always eventually lost to the Center. Second, it was the rare graduate student who was very good on all phases of the research process. So there was a tendency to let senior research assistants gravitate to what they were best at, for example, interviewing or report writing. This, however, generated problems of equity in use of staff members. Third, and related to the second problem, is that not all persons are comfortable and good at working concurrently on several different projects at varying stages of their development. Some people work much better on only one sequentially developing project. Yet the Center had the former rather than the latter style. Fourth, research assistants sometime had "overlearned" their classroom training. Thus, some thought scientific research could only be of a deductive, quantitative and hypothesis testing nature. Such students had difficulty operating in an inductive, qualitative and hypothesis generating framework. Finally, post field data processing (not data analysis) activities are time consuming and not intrinsically interesting. GRAs tended to delay this work with a compound of memory failures. Unless there is a continuous monitoring effort, this work is often not well done leading to a compromising of the gathered data.

IMPLICATIONS AND LESSONS FOR FUTURE RESEARCHERS

This chapter has another goal besides providing a detailed historical documentation of a major pioneering social science research effort in the disaster area. It is also to indicate the implications and lessons that the DRC effort has for future field researchers on crisis time disaster behavior. These will be briefly discussed under four general themes.

1. The kind of field research described cannot only be done, but can be done well. When DRC first started its work, other social scientists often expressed doubt that such field research could even be undertaken, or if attempted, could produce any worthwhile research results. The question asked was how could one do research in the midst of social chaos and extreme personal stress and

essentially very difficult working conditions? As late as 1976 a highly placed administrator in the U.S. National Academy of Sciences very strongly objected to a proposal that the National Research Council set up quick response field research teams to study disasters. His objections were:

How can the investigative team be alerted and got to the right place in time?

It is difficult for me to see how you can select a site and collect the sort of data outlined . . . Officials are going to be so preoccupied with their own immediate problems that I cannot imagine their talking to researchers in advance of a known emergency. How can the monitoring system envisioned . . . actually be put in place in the face of imminent disaster? (Brooks 1976)

Perhaps because of the now known DRC work, currently such views are seldom anymore expressed by other researchers. Actually the problem of skepticism may exist more outside the research area, among research funders and users. This is increasingly less so in developed societies but skepticism still occasionally surfaces in some developing countries. Possibly this chapter will give "ammunition" and professional support to those who still have to convince policy makers and research funders that such research can be undertaken.

A statement on how well the work was done, might appear self serving. Nevertheless, DRC did obtain thousands of interviews, record innumerable observations, and collect tens of thousands of documents. More important, from those data nearly five hundred publications were produced including 29 Ph.D. dissertations. Of even more value, the published results are a prominent part of the disaster literature, which in turn shows up in introductory sociology texts, summaries of the literature and compilations for research users. Here too what was done can be pointed to by researchers and research users who still have to make a case that not only can field research be undertaken but the end results can be worthwhile.

2. Certain relevant data and assessment of data gathered in other ways can only be obtained by the kind of field study described. There are several aspects to this, depending on whether interviewing, observing or documenting is the research technique in question. For example, as previously illustrated, certain activities can be noted via observations than could probably not be recalled in later interviewing. However, even the episodic and informal interviewing that can sometime be done even during the midst of a crisis at a central site like an EOC, is likely to be more candid and honest than a more formal interview later in a less hectic setting (as one respondent remarked to us in a crisis setting, "I could tell you I know what I am doing, but you can clearly see I'm wildly guessing in much of what I'm doing"). This is apart from the typical reconstruction that occurs toward what should have happened in later interviewing away from the time of the occurrence. This is a serious problem about formal interview data that to this day has been almost completely ignored by disaster researchers, but can be partly counterbalanced by having trained researchers present in the crisis setting. It has also always bothered us that the "decision making" we have observed during actual crises, seldom corresponds to the picture evoked in later interviews outside of the actual crisis context where the process is often depicted as explicit, conscious, individually based and involves the consideration of alternative options.

This is why we think that it is very unfortunate that too many current disaster researchers who are the ultimate analysts of data often not only get the information third hand via first an interviewer and then a coder, but also have absolutely no direct experience in disaster occasions which would give them a larger context for interpreting the data. In part, we are paralleling here what a famous social scientist we had in our graduate studies used to say in his introductory lecture on doing community studies. "The very first thing you must do is to walk very slowly and several times through the area and observe everything you can. Your interpretation of all the statistics you may later play with will differ depending on your observations. In any case they will certainly be more accurate if you make the walk."

Of course many possible observations and many gatherable documents at a time of crisis are very ephemeral and if not collected at the time are lost forever. (DRC did try to get its field teams to photograph and/or tape record such material as much as possible but for unclear reasons the admonition to do so was not consistently followed). Future field researchers should attempt even more than did the Center to collect such kind of fleeting and transitory data.

3. However, for this kind of field research, to be done and done well, requires much prior planning as well as continual monitoring of the implemented measures. The notion that research consists of an initial training and pilot period, and then a moving on to the actual gathering of data and its later analysis, is not an appropriate model/image of what should be in place (although this is the naive view in some guidelines on how to write research proposals or even in some formal courses for students on how to do research generally). The methodology used has to be kept in mind from the start to the end of the work. The quality and quantity of the data obtained will be mostly determined long before the first study in the field. As such, it is important that much time and much effort be explicitly allocated to teaching the methodology involved. However, it is also crucial that a continuous evaluation be undertaken of whatever is done. Even very well trained and experienced personnel will show slippages and regressions back to inappropriate behaviors and actions.

4. Finally, just because something has been done and done well, does not mean that whatever the "traditional" approach has been, should be blindly followed. In particular, we would strongly urge that even for the kind of disaster research just described, it will be necessary to take into account whatever larger social changes are occurring. These changes can both hinder and help future crisis time field studies that might attempt to follow or build upon what has been described in this chapter. For example, the increasing litigiousness nature of Western type societies as well as the increasing tendency of negative disaster effects to cut across national not to mention community boundaries (see Quarantelli 1996) were not major problems that faced DRC researchers. Yet that is part of the future for field students of disasters, and the methodology used will have to take such difficulties into account. However, there are also some positive aspects of the changing social context of disasters. For instance, as suggested elsewhere (Quarantelli 1995), the Internet can now be used to directly collect "real time" data on organizational behavior and interorganizational communications during disasters. And of course, as discussed in other chapters in this volume, there are now many very useful technological tools and machines such as camcorders and laptop computers that were not available to DRC in its early decades of existence. The newer technological devices ought to be

incorporated into the methodology of future field studies, taking into account their negative as well as positive effects.

In the preceding pages the focus has been almost exclusively on the field research undertaken by the Center. However, as noted in the initial paragraph of this chapter, many other methodologies as well as other tools besides tape recorders were concurrently used by the Center. The research results reported by DRC reflected this larger effort. As such, we favor linking field research to other methodologies or incorporating newer tools in future large scale and continuing field research. In fact, any study that merely mirrored what DRC once did, would be an unnecessarily limited effort.

Studies indicate that emergency managers should not plan on the basis of the last disaster they experienced, no matter how efficiently and effectively it was handled. Rather they should plan on what reasonably can be projected in the future. The same is true of disaster field studies. No matter how well past studies were done, future researchers need to continually improve and to add whatever new or novel relevant methodologies and tools become available. If that is done, someone in the 21st Century should be able to write from an historical viewpoint a chapter similar to this one but yet an improvement on it.

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