Methodological Issues[1]

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"It's the same, only it's different." This sounds like one of former American baseball player Yogi Berra's malapropisms. Nevertheless, it is appropriate when discussing methods of disaster research. Fifty years ago, Lewis Killian (2002 [1956]) stated it this way: "Basically, the methodological problems of field studies in disasters are those common to any effort to conduct scientifically valid field studies in the behavioral sciences. The disaster situation itself, however, creates special or aggravated problems . . . " (p. 49). The basic tools of disaster researchers—a theory, a working hypothesis, an appropriate research design, a plan for selecting cases for study, a strategy for gathering data or recording observations, and a way to extract meaning from the materials collected—are easily recognizable as those used in all of the social sciences. Yet, issues specific to disaster research need to be addressed.

Simply put, the difference in doing research on disasters is the *context* in which it is carried out (Mileti, 1987, p. 69; Taylor, 1978, p. 276). The greater the difference between that context and the everyday world in which the rest of social science research takes place, the more unique are the challenges of disaster research (Stallings, 2002b, pp. 21–22). This means, for example, that studies conducted during the crisis time period (a term from Quarantelli, 2002a) face challenges that research carried out during the late stages of recovery or during pre-disaster mitigation and preparedness phases do not. The latter encounter only the usual difficulties found in all social science research—no more, no less. This chapter focuses on issues that arise in conducting research in settings that are usually far from the day-to-day. Admittedly it is skewed toward methods of disaster research employed by sociologists because the author's background and training are in this field.

No attempt is made in this chapter to discuss the range of methodological issues currently facing the social sciences in general or even sociology in particular. Instead, three issues have been singled out because they seem to comprise the essence of the difference, when it exists, between disaster studies of all types and "everyday" research: (1) *timing*, meaning generally *when* the process of observing or collecting data and other materials takes place in relation to

the onset of disaster; (2) *access*, referring especially to researchers' initial contacts with interview subjects, survey respondents, and holders of documents or other relevant materials; and (3) *generalizability*, or what Killian (2002 [1956]) calls the ability to draw "valid conclusions" (p. 54) from disaster studies. While only partial solutions exist for many of these issues, *triangulation*, meaning the emergence of patterns out of the findings from many studies using different methods, seems to offer the most confidence in the "validity of the conclusions drawn" (p. 49) from disaster studies. Of course, the three issues themselves are interrelated: the ability to generalize from the findings of any given piece of disaster research is directly affected by the successful resolution of timing and access issues, and access itself is affected considerably by timing issues.

Three of the most common types of disaster studies are discussed in this chapter: *field research*, often identified as the qualitative case study or simply field studies; *survey research*; and *documentary research*. In the last category I include historical research. Clearly, documentary research, like field studies and survey research, involves dealing with people rather than just written documents. Because so much of the literature is based on field studies, because many of the unique constraints on disaster research are encountered only in carrying out this type of study, and because they are most often conducted during the crisis time period, more space is devoted to this topic than to the other two.

FIELD STUDIES

The prototypical method of disaster research has been the field study.[2] The following is an illustration: A researcher or group of researchers learns of the occurrence of some disaster,[3] most often through the news media. Despite the fact that such initial information is usually far from accurate, arriving on site as soon as possible is generally seen by field researchers as key to the success of their work, so the decision to launch a study needs to be made quickly. In large-scale research operations such as the Disaster Research Center (DRC),[4] especially in its earliest days when a sizable stand-by research capability was maintained, a small (one- or two-person) "reconnaissance team" might be dispatched initially. These teams would attempt to accomplish several things. They would, as nonparticipants, observe activities at locations where important disaster-related tasks were being carried out such as in emergency operations centers (EOCs), field command posts, hospital emergency rooms, temporary shelters, and disaster relief "one-stop" centers. They also would try to informally "interview" (speak with) the people involved or at least set up appointments for later formal interviews "when things have calmed down." If the reconnaissance team judged the situation suitable for a more thorough study, then one or more subsequent field trips would be undertaken. During follow-up visits to the site, formal interviews would be conducted. At all times, researchers would be alert for any documents that they might obtain, which typically would include copies of disaster plans, emergency logs, notes or minutes of meetings, after-action reports, local newspapers, and other relevant records of any kind. After returning from the site, at least one member of the

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[2] Much of the current research of this type is carried out in the form of Quick Response studies with small grants from the Natural Hazards Center at the University of Colorado. (See http://www.colorado.edu/hazards/qr/).

[3] Exactly what events should be considered disasters for research purposes is a complex issue that will be ignored for the time being. (See Quarantelli, 1998; Perry & Quarantelli, 2005.)

[4] The Disaster Research Center was located at The Ohio State University in Columbus, Ohio, from its formation in 1963 until 1985. Since then, it has been located at the University of Delaware in Newark, Delaware.

field team would write up a preliminary report of the team's findings and conclusions based on field notes, observations, casual conversations as well as formal interviews, and documentary material collected.

Timing

Timing is paramount in disaster field studies. It is frequently discussed in field reports, but details of the decision-making process that leads up to getting to the disaster site in the timeliest manner are usually implicit. The following examples illustrate some of the considerations involved.

One of the earliest studies undertaken by the Disaster Research Center (DRC) during its first years in existence at The Ohio State University was of flooding on the Ohio River at Cincinnati (Anderson, 1965). In its brief introduction, the research note describing this study gives a glimpse of the processes involved:

> Early in March, 1964, the Ohio River Valley was subjected to very heavy rains. After several days of almost continuous downpour the major river in the valley, the Ohio, reached the flood stage of 52 feet at Cincinnati. This signaled the beginning of the worst flood in that city and in the valley in 19 years and was climaxed on March 11 when the water crested at 66.2 feet. Throughout Pennsylvania, West Virginia, Kentucky, Ohio and Indiana, thousands were left homeless as a result of flooding from the river and several of its tributaries. Red Cross officials estimated that about 110,000 persons were directly affected by the floods in the five state area. At least nine deaths were attributable to the high waters, seven in Ohio and one each in Kentucky and Indiana. Total property damage went beyond the 100 million dollar figure. (p. 1)

Researchers typically learn of disasters the way others do—through the news media, especially radio and television. *When* such reports are received relative to the onset of disaster affects the kind of study that can be fielded and to some extent the topics that can be investigated. A meandering off-shore hurricane or a slow-developing flood may enable researchers to select a probable target location and position themselves there in advance of impact (see Quarantelli, 2002a, pp. 106–107).

Initial deployment of a field team based on early news reports, whether just before or as soon as possible after onset, is complicated because the earliest estimates of casualties and damage are notoriously inaccurate (Quarantelli, 2002a, p. 107). The DRC "law" was that estimates of the number of dead varied inversely with distance from the disaster (Dynes, Haas, & Quarantelli, 1967, p. 219). Notice in the first paragraph quoted that the information received was technical: flood-stage river levels had been reached; the river crested at a certain height on a particular date; "worst flood" in a specific period of time; and so forth. In the second paragraph, estimates of the human dimensions of this disaster are described: "thousands" homeless; "110,000 persons" directly affected; at least nine deaths; property damage "beyond the 100 million dollar figure." Based on reports of such estimates, the decision to launch or not launch a field study must be made—and made quickly.

Just how tricky this decision can be is illustrated in another example. The incident was a freight train derailment and resulting toxic fire that caused widespread and prolonged evacuation in the immediate vicinity of Miamisburg, Ohio, south of Dayton. The researcher (Stallings, 1986) was interested in interorganizational relationships among emergency response agencies and was attempting to determine through news reports whether the derailment and fire would provide the degree of interorganizational complexity that would justify a 2,500-mile-trip to the accident site:

Entry into the field in the Miamisburg case was complicated by the changing nature of the incident, the nature and scale of the evacuation, and the routines of both the [researcher's] normal work week and the phases of the emergency period. First news of the incident reaching the West Coast late Tuesday evening, July 8 [1986], suggested that the accident was more or less "routine." But after the second explosion and subsequent evacuation on Wednesday, it began to appear that the situation fit the research design outlined in the Quick Response proposal, and plans were made to depart for the scene. With the evacuation estimated to last through the end of the week and with the probability that at its conclusion key representatives of disaster-response organizations would take some well-deserved time off, it appeared that the optimal time to begin gathering data would be Monday, July 14. (p. 4)

In the case of the DRC study of the Ohio River flood, the decision was made more easily:

On Tuesday, March 10, a two-member DRC team went to Cincinnati to conduct a preliminary survey of the situation. Two days were spent on this initial trip interviewing local officials and making general observations. (Anderson, 1965, p. 1)

This field team was able to arrive at the disaster "site" twenty-four hours *before* flood waters crested, that is, during the emergency or crisis time period itself. This was due both to the relatively slow-onset nature of the flood and to the proximity of these researchers to the disaster area (in 1964, the DRC was slightly more than 100 miles from the Ohio River at Cincinnati).

After the team returned to Columbus, the DRC staff decided that a follow-up trip focusing on organizational preparedness or what will be treated in this paper as an aspect of the "flood disaster subculture" would contribute to an understanding of community response to disasters. With this in mind, a three-man team returned to Cincinnati on March 15 for two additional days of study. (p. 1)

The reconnaissance team had confirmed the impression formed from news reports that this flood disaster was worthy of study, at least relative to the resources required such as the costs of travel to the site. Notice that this is always a relative decision—the nature of the disaster, in particular its magnitude, relative to the resources required to study it. In other words, a similar disaster, especially one comparable in scale, in a more distant location might be judged less worthy of study. The qualifier to this statement of course is the substantive interest of the researcher. For instance, a disaster of otherwise unremarkable characteristics may have damaged or destroyed one or more nursing homes and assisted living facilities. A researcher interested specifically in threats to the elderly in disaster might select such an event for study on substantive grounds. Generally speaking, the decision to undertake a field study of events with unique properties such as the nuclear reactor accident at Three Mile Island, Pennsylvania, or ones of obviously catastrophic magnitude such as the 2004 Asian tsunami and Hurricane Katrina are easier to make, but timing issues remain in these cases as well.

Not discussed in this quoted passage are three important accomplishments of these reconnaissance teams when they are successful and why timing is so important to their success. First, they piece together an overview of what has happened during the pre-impact and crisis time periods, including identifying the principal actors (organizations, in the case of most DRC studies) and any unique aspects of the disaster. Early disaster researchers referred to the former as establishing the *Gestalt* of the overall disaster response (Dynes, Haas, & Quarantelli, 1967, p. 221; Killian, 2002 [1956], p. 69). Second, reconnaissance teams return with a list of key contacts in the groups and organizations most heavily involved in the disaster response. If a follow-up trip were deemed appropriate, appointments for interviews can then be arranged by telephone before the follow-up team departs. (It was standard procedure for one member

of DRC reconnaissance teams to be responsible for obtaining a local telephone directory, for example.)

Third, and perhaps most important for the success of follow-up field trips, contacts made by members of reconnaissance teams greatly facilitate later access to informants. Typically, many more people would have been contacted than were formally interviewed, with reconnaissance team members introducing the research project, showing their identification or credential, providing a business card, and leaving contacts with the pledge that subsequent field teams would be getting in touch with them to request a formal interview. These initial contacts usually provide subsequent field teams an "in" with key organizations, differentiating them from later-arriving researchers. They would also establish the identity of field team members as researchers rather than reporters and other representatives of the news media.

In the case of the one-person field study of the Miamisburg train derailment, all three tasks were compressed into one trip to the locale:

> After arrival on Monday morning, contact was first established with the regional emergency response agency, the Miami Valley Disaster Services Authority, headquartered in Dayton. Staff members who were not still at the site of the derailment provided an initial briefing and overview of the organizations involved. A beginning list of the names of key respondents was obtained. The remainder of the first day was spent in touring the cities affected, visiting the crash site, and generally "getting the lay of the land." The second day in the field (Tuesday, July 15) found most key respondents needing time to catch up on work that had piled up during the emergency period, so the day's principal activity consisted of making appointments for interviews during the remainder of the week. Wednesday, Thursday, and Friday were devoted entirely to interviews with key respondents having special insight into the interorganizational relationships that emerged during the emergency. (Stallings, 1986, pp. 4–5)

Also hidden in the passage from the DRC Ohio River flood report is what goes on between the return of the reconnaissance team and the fielding of the follow-up study. Discussions would have been held regarding the issues or problems to be focused on during the follow-up. Interview guides might be written specifically for such a study. In the case cited, the decision was made to concentrate on elements of a "disaster subculture" as they were reflected in the actions of key emergency response organizations. (Organizational and group representatives were interviewed because the focus of DRC's early research was on organized behavior rather than on individuals; see Dynes, 1970, pp. 1–5).

Quarantelli (2002a) makes the case for the earliest possible arrival at the disaster site:

> 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 collected that cannot be obtained through later interviewing. The social barriers that normally exist to restrict access to high-level officials and key organizations do not exist. A second reason for being on the scene early [is that it] ensures a high degree of access and cooperation. Victims are typically candid, cooperative, and willing to talk in ways far more difficult to get later. (p. 107)

It is probably fair to say that poor field research results when studies are begun only after disaster has struck. Not only does this increase problems associated with late arrival on the scene, but it also probably means that the researcher is unprepared to take advantage of the opportunity afforded by unfolding events. If the researcher is unfamiliar with the literature on previous disasters, for example, both missed opportunities and "reinventing the wheel" are likely to occur. Unless the event is so compelling as to make any sort of study preferable to no

study at all,[5] it is probably better to head to the library rather than to the disaster site. Becoming familiar with the existing literature can be used as an opportunity to create working hypotheses that will be available beforehand when the next disaster strikes, as it inevitably will. Interview guides or rudimentary questionnaires can also be designed and held ready for a future event.

Not all barriers to timely entry to disaster sites are to be found at the sites themselves. Lack of funding can be a major barrier. While there have been occasions when researchers have initiated studies without outside funding (covering the costs of field research "out of pocket," so to speak), most such instances have occurred when the researcher is already in the immediate vicinity when disaster strikes. Fortunately, it is no longer necessary for disaster researchers to begin the months-long process of writing a lengthy and complicated research proposal, submitting it to a large government agency or private foundation for review, then waiting weeks for a favorable decision and eventual receipt of funds, including money for travel and other direct expenses involved in carrying out a field study. Major funders such as the National Science Foundation have created small-grant programs that facilitate researchers' ability to begin research without the usual delays. In addition, the Natural Hazards Center administers "quick response grants" that are available to researchers both on an annual basis (that is, pre-disaster) and in the immediate aftermath of significant disasters, which do not require lengthy proposals and have relatively rapid review and decision times. The primary purpose of these quick-response grants is to provide, as the term implies, a level of support sufficient to enable field researchers to reach disaster sites in timely fashion. In return, the center expects a preliminary report on the study and its findings and makes these available online as Quick Response Reports. (See Note 2 for the link to these reports on the center's Web site.)

Another barrier to timely arrival at the scene of a disaster can arise from the Institutional Review Boards (IRBs) at the researcher's home university. Previously known as human subjects committees, IRBs exercise universities' ethical and legal responsibilities for ensuring that the rights and the physical, psychological, and emotional well-being of research subjects are protected by researchers. This involves requirements that researchers provide subjects with information about the nature of the research and its intended uses, apprising subjects that they have the right to refuse to participate in the research, and assurances about the confidentiality of any information provided by subjects including how that information will be stored, for how long, and who will have access to it. Providing IRBs with the necessary documentation to obtain official approval to carry out the research one has planned can be not only a source of irritation for researchers but also a source of delay in beginning the gathering of data. One solution is to seek, if not prior approval, then at least all the information that will be required for formal approval and to prepare in advance as much of the documentation that will be needed when the time comes. Successfully navigating the IRB process may be another reason to bypass the study of an existing disaster in favor of better planning for a study of the subsequent disasters that are sure to arise.

Access

As noted earlier, the primary reason that researchers who conduct field studies of disasters consider the ability to get to the scene in timely fashion to be so important is that timing

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[5] On the other hand, if an event is that compelling, then there is a very high probability that veteran disaster researchers, better equipped to take advantage of the situation than novices, are already in the field. The case will therefore probably not be lost to the research field as a whole.

is usually crucial for gaining access to the key people to be interviewed and for acquiring invaluable documentary materials. Both become more difficult if not impossible later in the disaster process. Researchers refer to this as the problem of ephemeral (Quarantelli, 2002, p. 107) or perishable data (Bourque, Shoaf, & Nguyen, 2002, pp. 179–183). Both terms refer to everything from the impossibility of observing personally events after the fact, to obtaining documents that were freely available during the crisis time period but are deemed too sensitive for outsiders after disaster ends, to respondents' personal recall that may be skewed by repeated retelling of their stories to a succession of interviewers.

Tierney (2002b, pp. 359–365) has written expressly on the relationships among timing, access, and generalizability in research where formal organizations are the primary units of analysis: "Virtually all organizations, both public and private, seek a favorable public image, and one means to accomplish this aim is to exercise control over information, including the kinds of information researchers seek. The need for organizational impression management is probably even more marked in disaster situations than during normal times . . . " (p. 359). More generally, each successive phase of the disaster process seems to bring about a change in the relationship between researchers and subjects. Drabek (1970, pp. 331–332) observes that, while cooperation may be adequate initially, researchers become increasing suspect as more and more outsiders arrive representing insurance, sales, welfare, and other interests.

One barrier large-scale organizations use to deflect disaster researchers is the Public Information Officer (PIO). Created to handle information requests from the news media, organizational officials frequently try to fend off requests for interviews by steering disaster researchers to PIOs and to press conferences (Tierney, 2002b, p. 360). In fact, researchers are often seen as a threat to the organization similar to that posed by reporters. Tierney notes that these informational-control mechanisms ". . . promote a 'command post' point of view [see Quarantelli, 1981] that privileges the official information-dissemination function over the perspectives represented by other elements in the disaster management network" (p. 361). By blocking access to the varied individual perspectives on the disaster within organizations, officials are better able to present a single, approved, "orthodox" perspective.

Another situation making access problematic, especially in large-scale, highly publicized disasters, is the "convergence" of researchers. Convergence has been a term used in this field to denote the movement of material, information, and especially people toward the disaster site from outside the area (Fritz & Mathewson, 1957, p. 3). It is a ubiquitous aspect of disasters, most visible in news reports of sightseers converging on the disaster site. High-visibility catastrophes not only attract veteran disaster researchers, who themselves are more numerous now than two or three decades ago, but also spawn novice disaster researchers, who might be veterans in some other research specialty but who are studying a disaster for the first time. While funding agencies and other professional associations can facilitate a certain amount of coordination among researchers and avoid at least the most blatant forms of duplication of effort, nevertheless the presence of a large number of researchers in the field can create competition among them for access to a handful of informants such as a mayor, a police chief, or other individuals who may have unique perspectives on events (Tierney, 2002b, p. 364). At the very least, respondents may feel besieged by multiple requests for formal interviews and decide to grant none. More cooperative respondents may provide rehearsed answers after being interviewed numerous times, making it difficult for later interviewers to probe effectively. Clearly, timing can be especially important in mega-disasters, with the first researchers on the scene enjoying a "competitive advantage" in access to never-to-be-repeated events, to people, and to perishable documents. Having an established reputation in this field such as that enjoyed by the Disaster Research Center, which has been conducting research on disasters continuously

since 1963, or the Natural Hazards Center, can also provide a competitive advantage in gaining access.

The issue of access to all members of disaster-relevant organizations is one aspect of a larger issue that Killian (2002 [1956]) refers to as "sampling 'points of observation' " (p. 68). In most social science research, research subjects are *respondents* who are sources of data on their personal attitudes, behavior, and characteristics. In disaster research, especially in field studies, subjects are more often treated as *informants* who describe not only their own actions but also those of people around them. Often the researcher's goal is to obtain a holistic picture of some social process or bundle of processes. Any systematic constraints on access to informants introduce an unknown amount of distortion into the picture obtained.

Ideally, one would like access to organizational informants from different levels in the chain of command and from different subdivisions, the precise determination of which being dictated by the circumstances of the disaster. When officials deny researchers access to specific elements within organizations, a bias is introduced into the "data." Similarly at the individual level, if only subjects from one location within the disaster area are available to interviewers (e.g., those from the least heavily damaged areas), an incomplete picture is likely. This is the case, for example, when researchers are prevented from contacting individuals assumed to have been most traumatized by the disaster (e.g., so-called vulnerable populations such as young people).

Barriers such as these arise not only in the field. University IRBs, perhaps altruistically but also perhaps out of fear of "bad publicity" if not litigation, increasingly but indirectly impose forms of constraints on researchers' access to human subjects. Requirements vary across universities, but overall there has been increasing concern on the part of IRBs for the protection of presumed at-risk human subjects, of whom disaster victims would presumably be a prime example. Many universities require researchers to provide subjects with written documents such as those described in the previous subsection and to obtain signed consent forms before conducting interviews. Also, more precise disclosure is being required about the storage and usage of interview data. (The ever-present threat of third parties seeking to obtain copies of recorded interviews or transcripts through litigation is something that both researchers and potential subjects are usually aware of.) All of these well-intended procedures have had a noticeable effect in making respondents more wary of researchers (Tierney, 2002b, pp. 353–355).

Nevertheless, if approached in an appropriate manner and under the right set of circumstances, most people do not refuse to be interviewed by researchers. The incentives to grant interviewers' requests have been noted over the years. Some respondents are undoubtedly interested in sharing their experiences, good and bad, so that others may learn from them (Killian, 2002 [1956], pp. 71–72). Some seem to find recounting for researchers what they have experienced to be therapeutic, as many DRC field team members frequently noted. Still others may simply be flattered that they have been chosen for an interview or may desire to embellish their actions in the eyes of others.

Generalizability

By generalizability, I am referring to researchers' ability to make empirically grounded statements describing phenomena and relationships among phenomena that hold across all similar events. Generalizability may have a negative connotation to some readers (as in the criticism, "That's a generalization," implying that a statement is suspect because it cannot possibly hold

for all cases). However, generalization is the goal of all science, including the social sciences. And the limits of generalization—knowing as precisely as possible the boundary between when such statements hold and when they do not—is as important for understanding as are the statements themselves. This is one reason why the definition of "disaster" itself is such an important issue in this field (see Quarantelli, 1998; Perry & Quarantelli, 2005). Such a definition provides answers to the questions: To what types of events do the generalizations from disaster research apply? And, implicitly or explicitly, to what types do they *not* apply?

Just as timing affects access to subjects, observable acts, and documentary materials, so too does access affect generalizability. Most of the major ways in which this can occur were discussed in the preceding subsection in terms of the selection of subjects and the unique perspectives on disaster that they can provide. These are sampling considerations in the largest sense of that term. But "sampling" in disaster field studies seldom involves population listings and the probabilistic selection of cases. Nevertheless, some sampling plan is followed. Most sampling strategies used in disaster field studies produce what are commonly referred to as nonprobability *purposive samples*. These are samples dictated by the nature of events and people's participation in them. Indeed, when researchers desire the perspectives on disaster of a particular category of actors (women in key response roles, for example; see Enarson, 2000b) purposive samples are the only appropriate device. They clearly are preferable to the *accidental samples* typically used by news reporters (e.g., people who just happen to be at a particular location at the time reporters and camera crews are ready to record a series of interviews).

Before readers dismiss the findings of disaster field studies because at first blush they seem to be "unscientific" based as they are on nonrandom samples, let me hasten to point out that the goal of all sampling strategies is to create a subset (the sample) from a larger set (the universe or population) that is *representative*. The desired end result is a sample whose characteristics are as similar as possible to those of the universe as a whole or whose characteristics differ from those of the universe in ways that are *known*. Random selection of cases is a strategy employed when the researcher *does not know how* to select cases that will be representative of the universe. It is, in effect, a strategy that assumes ignorance. Chance governs selection instead of knowledge of the universe. In the disaster situation, researchers do know something about how different segments of the population-at-large were affected by and how they reacted to events. A sample can be selected purposively to reflect patterns of activity or patterns of victimization.[6] Conversely, a random sample of a disaster-stricken community would have a high probability of failing to produce a sample that includes such key actors as the mayor, the chief of police, and the emergency services coordinator. Each would have the same chance of being selected as any other member of the local population—no more, no less. Hence, purposive sampling and so-called "snow-ball" sampling (wherein informants identify still other informants to be interviewed) are more appropriate for many more types of disaster research than traditional probability sampling techniques.

Another traditional topic associated with sampling needs to be discussed in connection with the issue of the generalizability of the findings of field studies. This is the matter of *sample size*. No matter how many interviews are conducted, whether with organizational informants or individual respondents, and no matter whether the sampling strategies produce probability or nonprobability samples, the resulting field study is in reality a "sample" of one disaster drawn nonrandomly from the (hypothetical) universe of all disasters (Mileti, 1987, p. 67). Mileti's

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[6] Of course, probability and nonprobability samples in disasters are not mutually exclusive. In organizational research, for example, a purposive sample of informants may be created at the higher ranks (chief, deputy chiefs, division commanders, etc.) while a probability sample may be chosen among rank-and-file police officers.

recommendation is *replication*, specifically, adding to the sample of one field study a second study and then another and another. This is not merely an expansion of the existing literature; it is increasing the sample size, one case study at a time. Confidence in the generalizations from a single study grows as similar patterns of findings appear in subsequent studies.

Another issue involved in the generalizability of findings from disaster field studies is that of *establishing causal relationships*. Whatever specific language is used, researchers are interested in drawing conclusions about one of two things. Most often it is linking behavioral *consequences* to the temporally prior disaster event (i.e., their cause). Sometimes it is linking pre-disaster adjustments as the cause of post-disaster consequences. Regardless of the content of the specific causal hypothesis involved, the logic of cause-and-effect demands multiple cases. Confidence in causal generalizations is further enhanced if some of the cases are *non*-disasters that constitute a *control group*. Mill's (1872 [1843], pp. 451–452, 458–460) Method of Difference and Joint Method of Agreement and Difference demand, respectively, the contrasting of a disaster with a non-disaster and of multiple instances of disasters with multiple instances of non-disasters. In the case of complexes of social processes such as disasters, these requirements are untenable.

As a result, much effort in a disaster field study is expended to identify a *baseline* for purposes of comparison. Typically this baseline is a description of the unit of analysis (e.g., a community, an organization, or even individuals) *prior to* the crisis time period. Descriptions of conditions, procedures, or typical activities before disaster serve as the logical equivalent of a control group. Differences between pre-impact and post-impact patterns are inferred to have been caused by the disaster rather than by some unknown spurious factor. Obviously, such inferences are more readily acceptable when the "change" is the activation of a disaster plan, for example. They become more tenuous when the change is a higher local post-disaster unemployment rate. Did the disaster cause unemployment to rise and remain higher than before the event? Or did some macroeconomic factor produce the downturn in employment, one that would have occurred anyway, even without the disaster? There are ways of dealing with such questions statistically in the case of "social indicators" such as unemployment rates (e.g., Friesema, Caporaso, Goldstein, Lineberry, & McCleary, 1979; Wright, Rossi, Wright, & Weber-Burdin, 1979). However, in the particular instance of field studies, statistical controls, time-series models, and so forth, are not available.

The best approximation to inferring causal relationships in one-shot field studies of disasters is *triangulation*. Partly what is meant by triangulation is the accumulation—metaphorically, the piling on top of one another—of more and more field studies until the common findings that run through all or most of them stand out. These then become generalizations about disasters, "what the research literature has to say" about such events. But triangulation involves more than replication. It also encompasses the consistency of findings across different *types* of research: not just qualitative case studies, but also survey research and the analysis of documents, historical or otherwise. No individual study is without limitations, and different types of research designs are better suited for some research problems rather than others. As in any field of research, when the preponderance of evidence from a variety of different types of studies supports a particular generalization, researchers can claim with some level of confidence that they have a valid conclusion, in Killian's words.

Numerous examples supporting this contention about the efficacy of triangulation for the field of disaster research could be offered. Two will suffice. Much of the conventional wisdom about people's behavior in disasters had been dispelled, at least in the research literature, by the early 1970s. Earliest to fall by the wayside was the belief that people when confronting disaster would panic or otherwise behave irrationally (e.g., Quarantelli, 1954). More generally,

the accumulation of research using a variety of research designs destroyed a number of myths about disasters (Quarantelli & Dynes, 1972; Taylor, 1977; Wenger, Dykes, Sebok, & Neff, 1975). In addition, another series of studies of urban riots during the 1960s showed the limits of some of the generalizations about disasters. For example, looting—the mass theft of consumer goods—was so rare as to be almost nonexistent in disasters (despite the recent events in New Orleans after Hurricane Katrina) but was a significant feature of urban civil disturbances (Dynes & Quarantelli, 1968; Quarantelli & Dynes, 1970). This qualification to an accepted generalization led to further evaluation of the findings of field studies of disasters, for example, about the difference in post-disaster norms sanctioning looting in small, insular towns versus large, diverse urban areas—and presumably overwhelmingly catastrophic events such as Katrina.

SURVEY RESEARCH

Field studies typically involve a small number of researchers, often only one, who are in face-to-face contact with the people caught up in disaster and who seek rich qualitative descriptions of disaster-related structures and processes. Most often these studies have been exploratory in nature, with their major objective being the generation of hypotheses rather than hypothesis testing. In contrast, survey research in disasters has both a different type of relationship between researcher and subject and different objectives. As a result, the issues of timing, access, and generalizability take slightly different forms.

Timing

The most explicit examination of these three issues is by Bourque and her colleagues (Bourque et al., 2002). They not only discuss these as well as other issues; more importantly, they evaluate them with empirical evidence from six post-disaster surveys. In evaluating timeliness, the authors first identify three potential impediments affecting the ability to collect post-disaster survey data in a timely fashion: lack of a questionnaire to administer; lack of an already drawn probability sample; and lack of funding (p. 178). The first two can be overcome relatively easily by interested researchers (for one early study, these authors had access to an unused questionnaire constructed years earlier by a colleague; pp. 162–163). Of the three, the biggest barrier is "the high cost involved in moving large numbers of qualified interviewers into an area quickly" (p. 178).

   In addressing this question of resources, Bourque et al. raise a more fundamental question: "[H]ow imperative is it [for survey researchers] to enter the area immediately?" (p. 178). They quickly point out that many of the questions about both reactions to and consequences of disasters can only be answered with data collected ". . . well after the index disaster" (p. 178). Focusing specifically on the psychological impact of disaster on individuals, they note that ". . . there is no definitive information about when, or if, excessive psychological distress—to the level of post-traumatic stress disorder—occurs" (p. 178). They further note that telephones, used for most contemporary survey research, may be inoperable or their networks overloaded in the immediate post-impact period. (Assuming that phone service is more disrupted the closer the customer is to the impact site, a survey of households selected randomly and carried out before service is fully restored will produce a sample biased toward the *least* victimized households.) Their conclusion is that any delays in carrying out surveys are not automatically

"untimely" in so far as collecting post-disaster data at the household and individual levels is concerned.

The question regarding timing in post-disaster surveys then becomes: Are some types of data "'perishable' and subject to memory decay or memory enhancement" (p. 179)? Using three waves of data collected 8, 19, and 24 months after the 1994 Northridge (California) earthquake, Bourque and her colleagues examined responses to the same set of questions included in each and found that "[W]hat is striking about the data is the extent to which all the information tends to remain constant across the three years of data collection" (p. 183). The authors conclude that "...this suggests that social information about disasters may not be as perishable as we sometimes think and that memories about a disaster remain quite stable for at least a substantial period after a disaster" (p. 183).

Because some surveys are done on an annual basis, there are a few instances in the disaster research literature in which a regularly scheduled survey was followed by the onset of disaster that in turn prompted a follow-up post-disaster survey (see Drabek & Key, 1984, esp. pp. 3–4; see also Sweet, 1998). In the following study (Van Willigen, 2001), the disaster occurred while a regularly scheduled survey already was underway:

> This research utilizes data from the Annual Eastern North Carolina Survey conducted by the Survey Research Laboratory, East Carolina University. This survey is an annual event, and questions had been included to assess the well-being of area residents before the hurricane was predicted. Telephone interviews began with a representative sample of households generated by random digit dialing. Once contact was established with the household, a random selection procedure was utilized to select an adult respondent. Seven hundred forty-two respondents had completed the survey when the interviewing was interrupted by the hurricane. A new sample was drawn, and interviews were resumed in early October, approximately two weeks after Hurricane Floyd, and continued for six weeks, through the middle of November. Four hundred nineteen respondents were successfully interviewed after the hurricane. (p. 65)

In general, it is not that timing issues are less important in survey research on disasters as compared with field studies. Rather, the topics that survey research seems best suited to investigate involve less ephemeral phenomena. As in all well-executed research, the idea is to avoid utilizing this type of research design for topics for which it is not well suited. This means, among other things, using survey techniques to collect data on individual- and household-level phenomena that may be expected to persist for a reasonable length of time. Hence, some of the issues of timeliness that are central in field studies are less troublesome in survey research, even when data on crisis time period phenomena are sought.

Access

The ability to access respondents via survey methods in studying disasters requires addressing two separate issues. One is related to the fact that nearly all contemporary post-disaster surveys, at least in the United States, are conducted by telephone. (See Bourque et al., 2002, pp. 160–162 for brief discussions of random digit dialing and computer-assisted telephone interviewing techniques.) Therefore, the distribution of telephone use within the population to be sampled, both in general and in the aftermath of disaster, is of fundamental importance. This issue is discussed in the next subsection in the context of how representative are the samples in post-disaster surveys and the extent to which this affects the generalization of survey results. The other issue discussed here is that of the responsiveness of subjects in post-disaster survey research conducted by telephone.

Bourque and her colleagues (2002) provide the best treatment of this question because they were able to bring data to bear in assessing it. The authors compared the response rates (i.e., the percentage of respondents successfully interviewed out of all those contacted) for the six post-disaster surveys ". . . with response rates obtained in other telephone surveys conducted . . . in southern California during the same calendar periods" (p. 176). They found that the two sets of response rates were on the whole similar. Slight variations among surveys could be accounted for by differences in the resources available for the surveys rather the different settings. The more resources, the greater the number of "callbacks" (repeated calls to the same household) attempted, the higher the response rate.

In the following excerpt, a researcher grapples with problems of access and how they might affect the generalizability of his findings. This 1997 survey (Farley, 1998) involved a random sample in the St. Louis, Missouri, metropolitan area and dealt with earthquake hazard awareness and preparedness in the New Madrid Seismic Zone:

> At least one call was made to a total of 983 households. Due to busy signals, no answer, or answering machines, we were unable over the course of the three evenings [of November 4, 6, and 9, 1997] to reach 412 of these households, despite some attempts at callbacks the second and third evenings. A total of 571 valid households were reached of which 250, or 44 percent, resulted in completed surveys. A total of 56 percent of households reached refused to participate in the survey. While a refusal rate this high is of some concern if a researcher is attempting to estimate population parameters, the primary objective of this survey was to make comparisons to baseline data established through surveys with similar methodologies in the past and with refusal rates that did not differ markedly from the present survey. For example, refusal rates ranged from 44 to 54 percent in the St. Louis metropolitan area in the second, third, and fourth surveys in this series. Thus, while readers should use caution in generalizing precise percentages to the population, the comparability of the samples across time-series surveys suggests that valid inferences can be made about trends over time in responses. (p. 309)

Bourque and her colleagues (2002) summarize the issue of access in post-disaster surveys: "On the basis of these comparisons, we conclude that there is no evidence that persons in households with telephones are any more reluctant to participate in a study after a disaster than they would be at any other time" (pp. 177–178).

Generalizability

The ability to draw "valid conclusions" about disasters from post-event surveys may be evaluated in light of three specific issues: the representativeness of samples surveyed via telephone, the availability of adequate control groups for inferring causal relationships, and the appropriateness of comparisons across different surveys in the aftermath of separate disasters.

Because so much of post-event survey research uses personal interviews conducted by telephone, the most important aspect regarding the representativeness of samples is the availability of telephone services in the aftermath of disaster:

> One concern that has been raised about doing surveys after a disaster is that the sample from which the data are collected is not representative of the population affected by the disaster. Two general objections are raised in this regard. First, it is suggested that telephone surveys will "miss" substantial numbers of persons who do not have telephones or access to telephones *prior* to the index disaster. Second, it is suggested that telephone surveys "miss" those who are dislocated as the result of the index disaster and, therefore, fail to get information on those most affected by the disaster. (Bourque et al., 2002, p. 173; emphasis in the original)

In response, Bourque and her colleagues argue that in urban areas (where most post-disaster survey research is conducted) "…telephone coverage is so pervasive in the U.S. and so quickly reinstated following disasters in the U.S. that the representativeness of any RDD [random digit dialing] sample … will be as good or better than any other method of data collection …" (p. 173), provided that survey researchers follow best practices common to telephone-based survey research as a whole. The authors examined the survey conducted following the Loma Prieta earthquake in the San Francisco Bay Area because its critics had charged that this survey underrepresented "…the homeless and those who occupied single-room-occupancy (SRO) hotels prior to the earthquake" (p. 174). Their careful analysis (pp. 174–176) leads them to conclude that a small proportion of the Bay Area population was indeed missed. However, researchers know *who* was missed and *to what extent* and can therefore estimate the likely effects on study results. They reiterate that telephone interviewing does provide "…a dependable overall picture of what happened to an entire community during and after a disaster" (p. 176). The proviso to this statement, of course, is that data from those populations known to be missed by telephone interviewing can only be obtained by other types of sampling (e.g., so-called "snow-ball" sampling of homeless disaster victims) and data collection methods (e.g., face-to-face interviews).

The following excerpts are typical of the narrative in research reports where the researcher discusses the representativeness of the sample drawn. Usually the characteristics of respondents in the sample are compared with some baseline for the population such as the most recent decennial Census of Population, as in this example from Farley (1998):

> Before proceeding with the analysis, I examined the demographics of the sample and compared the sample demographics to the combined demographics of the counties included in the survey according to the 1990 Census…. [T]he sample characteristics are thus close or closer [referring to the margin of error for the size of the sample] to the population characteristics in the overwhelming majority of instances…. [O]ur sample appears in general to be reasonably representative of the population except with respect to socioeconomic status, …[T]here is no reason to believe that there is any significant impact on the trends over time in earthquake awareness and preparedness, which are the main focus of this paper (p. 310).

In the section on field studies, the question of control groups necessary for inferring causal relationships was discussed. The same challenge confronts survey researchers. As before, interest is in being able to link post-disaster attitudes and behaviors to the disaster experience rather than to some other causal factors. The logic for making such an inferential connection requires comparing disaster victims with nonvictims. In technical terms, this requires some way of measuring respondents' "exposure" to the causal variable, in this case the disaster. Early disaster researchers, following the precedent of ecological models of cities that were a major part of the pioneering Chicago school of urban sociology (see Faris, 1970 [1967], pp. 51–87), developed a spatial model with concentric circles distinguishing areas in terms of the typical disaster-related activity within them (Wallace, 1956b, p. 3). Areas in the model ranged from that of total impact at its center to the outer, undamaged areas from which local and regional aid was supplied (pp. 3–6). These early researchers used this spatial model to identify population strata in disaster-stricken communities and drew probability samples from within each strata, producing an overall sample of households for comparison that were assumed to represent varying degrees of victimization (see Killian, 2002 [1956], pp. 51, 63–67).

Bourque and her colleagues (2002) propose a similar strategy for one particular type of disastrous event, earthquakes:

> There is no way that a researcher can establish randomized control groups in studying responses to disasters, but the existence of population-based samples does allow systematic examination of whether and how experiences and responses differ across groups within the same community who are differentially exposed to the disaster. In earthquakes, the Modified Mercalli Intensities (MMI) provide an approximation of the extent to which an area experienced shaking. Using MMI as an indicator of the extent to which respondents and their homes were "exposed" to the earthquake or the "dose" that they received, we can examine whether reports of damage, injury, and emotional distress differed with the MMI. We expect that these three variables do vary with exposure or "dose" of the earthquake that the respondent experienced. (p. 184)

Their examination of two post-earthquake surveys supports this expectation. Respondents' opinions generally followed the expected pattern (p. 187). In general, the authors conclude:

> The availability of data from probability samples where exposure to the disaster varies enables the researcher to estimate the extent to which proximity to a disaster results in different experiences, behaviors, and attitudes. While not as powerful as an experimental design for examining the impact of a disaster on communities, the use of the concept of dose-response provides a viable proxy or surrogate for a controlled experiment and allows inferences to be made about how the disaster has differentially affected households with, for example, similar household resources. (p. 188)

As was the case in field studies, so too with post-disaster surveys there are strategies for at least approximating the comparisons that form the basis of test and control groups in drawing causal inferences about the consequences of disaster for individuals, households, and communities. Another, more typical example is the following from Ollenburger and Tobin (1999):

> A detailed investigation was undertaken of flood victims in Des Moines, Iowa, in 1993; this cohort was subsequently described as the high exposure group. First, a stratified random sample of flood victims was drawn based on Census records, large-scale maps, and telephone directories and an introductory letter mailed to prospective respondents. An in-depth telephone questionnaire was then administered by trained interviewers from the Center for Family Research in Rural Mental Health at Iowa State University approximately four months after the flood. One hundred and six questionnaires were successfully completed with each interview lasting up to 40 minutes. There was a refusal rate of 15 percent. Three months later, a large-scale control survey was undertaken in Des Moines and surrounding communities. This survey, again conducted by the Center for Family Research in Rural Mental Health, incorporated many of the same items of the original survey as well as further questions concerning psychological morbidity and level of flood exposure. A total of 1,735 surveys were completed; these served as the control group. (pp. 66–67)

Finally, the relatively greater codification of survey research methods makes possible more specific comparisons across separate post-disaster surveys than is possible for multiple field studies. Differences in timing, response rates, sample sizes, sample characteristics, and even question wording can be weighed in comparing findings from one survey to another and the effects such differences may have had on them estimated. This ". . . allows researchers to compare community behavior across time, events, and locations" (Bourque et al., 2002, p. 169). Bourque and her colleagues, for example, compare household preparedness activities between northern and southern California as well as over time (pp. 169–173). Comparisons of surveys conducted at different points in time are especially useful for identifying trends. Interpreted loosely as similar to an interrupted times series, comparing the results of several surveys done before a disaster such as an earthquake with several conducted on the same population afterward can make as strong a case as possible for inferring the causal impact of disasters on attitudes and behaviors. In addition, superimposing the findings from survey research onto those from field studies, where both deal with the same phenomena, is part of the triangulation process that increases the ability of disaster researchers to draw "valid conclusions."

DOCUMENTARY RESEARCH

The term "documentary research" implies that there is some specific research design or method that distinguishes the analysis of documents from other types of disaster research. This is misleading. With the possible exception of historical research, the use of documents in the research process is usually supplementary or complementary to either field studies or the statistical analysis of secondary data. (For a caution on the use of documents, see Killian, 2002 [1956], p. 81.) As the term is used here, "documents" refers to more than the usual materials collected in disaster field studies such as organizational logs and records, local newspapers, and after-action reports. It includes all materials that exist in either written, printed, or digital form that are obtained by researchers other than through the interview or questionnaire process. I have avoided calling them secondary data as opposed to the primary data obtained via interviewing and questionnaire completion. However, the notion of secondary data does identify what all these materials have in common. They are all physical records regardless of format that were created by someone *other than* the researcher for a purpose *other than that* for which they will be used *by* the researcher.

   I have further arbitrarily divided the following subsections into three separate discussions. One deals with contemporaneous documentary materials of an ephemeral nature, meaning that if the researcher does not obtain them at the time of the disaster or very soon thereafter they are not likely to be available or even exist later on. The second deals with documents and records produced in the months and years following a disaster, typically by formal organizations and official agencies, that are usually converted into quantitative data for statistical analysis. The third deals with historical materials, the kinds of documents that over time have passed from the hands of those who created them to others, often either relatives and descendants or archives and libraries.

   Quarantelli (2002a) gives the most comprehensive description of documentary materials as the term is used in reference to field studies, noting that

> . . . in the DRC framework the term "document" was used to cover anything of a physical nature that could either be copied or obtained. 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, and 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, after-action 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. (p. 116)

While the typical use of documentary materials in field studies is descriptive, that is, to supplement the interviews and observations that are used to construct an overall picture of disaster-related structures and processes, there is nothing inherent in such materials to limit their use to qualitative description alone. Indeed, even the DRC field teams, which specialized in exploratory and qualitative research, were instructed to gather the kinds of materials that could yield quantitative indicators ". . . to identify disruption of community life" (Quarantelli, 2002a, p. 115):

> . . . DRC shall attempt to obtain *statistics, afteraction reports, and whatever other documentary data* are available from the [fifteen types of] listed organizations. The intent is to try 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. (p. 115; italics in the original)

Documents of exclusively written content also can frequently be used to produce quantitative variables for statistical analysis. This is accomplished either through a formal content analysis of entire documents or through the coding of specific variables from reports or summaries, for example. The former procedure is most often employed in analyses of news reports, especially reports in the print media (see, among many others, Dearing & Kazmierczak, 1993; Emani & Kasperson, 1996; Hiroi, Mikami, & Miyata, 1985; Nimmo, 1984; Seydlitz, Spencer, Laska, & Triche, 1991; Wenger & Friedman, 1986; Wilkins, 1986). Stallings (1979) used such a coding scheme to transform successful and unsuccessful applications for disaster aid into a dependent variable for statistical modeling.

Finally, written documents form the basis for historical research on disasters. (The most interesting and informative source is Scanlon, 2002; see, as an example of historical disaster research, Dynes, 2000b.) These include everything from vintage newspaper clippings to diaries to personal correspondence. Also included are official histories prepared by organizations immediately after disaster, records of testimony at hearings and before investigative bodies, and records and lists of all kinds. Historical disaster research does not deal exclusively with the written word, however. Even an event that occurred as much as 80 years before can yield a surprising amount of oral history, as relatives and descendants of eyewitnesses and survivors recount for researchers stories handed down to them (see Scanlon, 2002, pp. 286–288).

Timing

The issue of timing plays out differently in the case of documents used in each of the three different types of research—the exploratory field study, the explanatory quantitative analysis, and historical research. In the case of field studies where rich description is a major objective, the critical aspect is the ephemeral nature of nearly all types of documentary materials sought. The one-time-only aspect of the materials makes the issues of timing and access one and the same. That is, if researchers are not on site to capture the written material almost at the time it is created then odds are that it will be lost permanently. Consider just a few types mentioned by Quarantelli (above): graffiti scrawled on the standing walls of damaged buildings, soon to be torn down for reasons of both safety and rebuilding; messages and lists written in chalk on backboards likely to be erased as soon as room is needed for newer messages and lists; and scribbled notes taken to record telephone requests for action or instructions tossed in waste baskets as emergency operations are being wrapped up. Unless researchers are physically present at such "data collection" opportunities to capture this type of information, it will never become part of the documentary "evidence" from the disaster. Quarantelli (2002a, p. 106; see also Phillips, 2002, p. 206) has long advocated the use of video cameras in the field to facilitate the capturing of such ephemeral materials. However, cameras and other recording devices such as cellular telephones with built-in digital cameras still must be operated by people who have access to the scene at just the right moment. Further, gaining access often requires that researchers agree not to use any audio, photographic, or video recording devices.

In contrast to the fleeting opportunities to collect the kinds of documentary materials so valuable in field research, the type of documents most valuable for statistical analyses are not likely to exist for months or even years following disaster. These documents either contain or are themselves of such a nature that they can be converted into the secondary data that can be analyzed statistically. Two different types exist, and each has its own dynamic. One type contains data or information that is readily quantifiable and is produced by organizations

and agencies directly involved in disaster. The second type consists of all the data produced routinely typically by governmental entities that can be used as statistical "indicators" of either the severity or the consequences of disaster. These include such things as local building permits issued, property taxes collected, unemployment figures, live birth rates, divorce rates, suicide rates per 1,000 population, etc. Research that requires such secondary data obviously cannot be undertaken until they are available, and this could be a year if not years after the crisis time period has ended.

In the case of historical disaster research, the time after disaster when the researcher begins the search for documentary materials will determine the type of material likely to be available. If the disaster occurred within a generation or even two, then there are still likely to be survivors who can be tracked down and interviewed. Loss of memory can accompany the aging process, but as Scanlon (2002) found in his reconstruction of the 1917 Halifax munitions ship explosion: "Disasters are so dramatic that many vividly remember what happened even three-quarters of a century earlier" (p. 267). If the disaster is one that took place a century or two before, letters and memoirs of survivors as well as newspaper clippings are useful:

> One starting point both for getting an overview of what happened and for creating a list for follow-up later was newspapers. While newspapers are notorious for making errors of detail, including errors about disasters . . . , they provide a broad picture of an incident and, because they are dated, help establish a sequence of events. They also provide names of organizations involved . . . While local newspapers are important, newspapers away from the scene are also useful . . . Newspapers also give a sense of the times. (Scanlon, 2002, pp. 272–273)

If the disaster occurred more than two or three centuries earlier, then the main sources of information are likely to be written histories of the period or of the region, published autobiographical materials, and fiction from that era that uses the disaster in some way. As Scanlon (2002) notes: "Dramatic events inspire fiction, some of it autobiographical" (p. 274).

Access

Even though access and timing are so closely connected that this topic was introduced in the preceding subsection, there is another aspect of access to documentary materials that is independent of timing issues. This is the bias that is introduced when researchers are prevented, either intentionally or as a result of unavoidable circumstances (such as the inability to be in two places at once), from gaining access to selective kinds of documentary materials. The sources of selective access are important to identify since the consequences of selectivity affect the generalizability of findings.

In the case of field studies, the greatest potential sources of selective access to documentary type materials are emergency response organizations, especially law enforcement. Often for good reasons but sometimes for self-serving ones, official disaster response agencies frequently try to limit not only researchers' access to personnel for interviews but also to control the written documents that researchers are able to obtain. When they are successful at restricting the flow of official documents and other information to only those that are available to the public-at-large (including the news media), the picture of disaster that researchers are presented with will be biased toward what Quarantelli (1981) has aptly labeled the "command post point of view," meaning the picture of disaster that those in charge would like people to have (Tierney, 2002b, p. 361).

Among more recent developments affecting researchers' abilities to acquire documentary materials is the increasing use of cell phones and PDAs (personal digital assistants) in the field. Organizational communication and record keeping are becoming more and more decentralized as well as more and more ephemeral as a result. E-mail poses a challenge for researchers in general, let alone disaster researchers. Researchers know or suspect that e-mail relevant to disaster operations exists, but organizational, legal, and practical constraints on locating and acquiring it are formidable, if not prohibitive.

In the case of statistical analyses that use documentary materials as sources of secondary data, the problem of selective access is less overt. It is clearly possible that researchers may be denied access to existing data, particularly if those data are proprietary in nature or, more recently, deemed of national security interest. More frequently, the frustration of researchers stems from the fact that the data sought for quantitative analysis are simply not collected or, if collected, they are not held in a single location.[7] In general, the researcher is limited to those items of secondary data in which agencies are interested or are legally charged with collecting. The U.S. Census of the Population is the prototype. All of the criticisms of the Census as a source of secondary data for research purposes can be made of the sources of secondary data for disaster research as well.

An extreme case of selective access to documents is worth recalling because it illustrates how such selectivity can affect the generalizability of findings. The case is the award-winning book on the 1972 Buffalo Creek, West Virginia, dam break and flood disaster, *Everything in Its Path*, by noted Ivy League sociologist Kai Erikson (1976). Erikson, widely known primarily for his widely used book on the sociology of deviance (Erikson, 1966), had not previously been involved in any research on disasters. He was hired soon after the flood by the law firm representing plaintiffs in legal action against the coal company responsible for building and maintaining the dam that had collapsed and thus had access to legal depositions, psychiatric evaluations, and statements and letters written by the survivors to their attorneys. In addition, he conducted interviews and distributed a mailed questionnaire to all the adult plaintiffs in the action with a 90% response rate (Erikson, 1976, p. 14).

Not surprisingly, Erikson found that everyone along Buffalo Creek had been traumatized by the flood disaster, both individually and collectively (pp. 153–155). This finding, however, was at odds with 25 years of research on disasters that had accumulated by that time. A reviewer of the book, one of the pioneers of the field of disaster studies (and one of the editors of this volume), found Erikson's findings "troublesome" and requiring further explanation (Dynes, 1978b, p. 721). One possibility was that the Buffalo Creek flood disaster was so unusual that Erikson's findings were bound to be different from those of others in the field. Erikson himself might not have been aware of this since he was a newcomer (although he does cite several of the key disaster studies available at the time; see in particular Erikson, 1976, pp. 266–267). If this were the case, then the Buffalo Creek disaster would be so unusual as to have only limited generalization to other community disasters (Dynes, 1978b, p. 721). However, Dynes suggests that Erikson's unusual findings are better explained by the nature of the materials to which he had access (p. 722). The motives and

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[7] The author once sought data on local election outcomes in California counties that had previously experienced a gubernatorially declared disaster. The California Secretary of State is responsible for certifying the outcome of all local elections in the state. However, the author learned that the Secretary of State's office at that time did not physically possess the actual local election results. These instead were in the hands of the individual county clerks. To gather the results would have required contacting each of California's 58 county seats.

interests of plaintiffs in a major class-action lawsuit whose homes had been destroyed and whose lives uprooted seemingly were accepted at face value. The study results overwhelmingly reflected the point of view of individuals knowingly engaged in an adversarial legal action.

Regarding problems of access to documentary materials in historical disaster research, the most complete discussion is that in Scanlon (2002). His experience in revisiting the history of the 1917 Halifax, Nova Scotia, disaster shows the hit-and-miss, serendipitous nature of accessing historical documents:

> Historical research has limitations. Records are lost or destroyed. Some sources are dead. Others are alive but their memories dim. Persons have taken records with them and kept them in private hands. However, there are also advantages. Some records that were private or secret have become public; some persons will produce records or talk about past events, though they would not have been cooperative at the time. In addition, some statistics and comparative data will exist only because time has passed. Sophisticated methods of analysis may reveal things that were not evident years ago. (Scanlon, 2002, pp. 266–267)

Scanlon (2002) suggests as starting points newspapers, books, academic theses, official papers, reports, minutes, logs, and letters (p. 268):

> While much of this material will be found in libraries or archives, it may be far from obvious which libraries and archives are worth visiting. After that, finding material becomes even more challenging. There are private papers ranging from notes written in a scribbler to diaries to typed memos. Tracking these down means poking around in basements or vaults. It also means using unconventional techniques, making one's interests known, and following trails from family, friends, professional colleagues, or even strangers to written sources, then trying to fit the material into a pattern. (p. 268)
>
> . . . Some archives and specialized libraries are very useful, others less so; all have information. However, there are hierarchies of archives. Beginners to historical research should start where there is an interest in their topic. Major archives are more useful to persons who know what they are looking for. (p. 278)

In accessing documents, Scanlon reminds researchers that it is people—living human beings—who possess or at least know about the existence of documents of potential research interest. His tips for those new to historical disaster research are built around this simple fact:

> It is extremely important to tell everyone what you are doing, and that means *everyone*—the [hotel] desk clerk, the hotel maid, the swimming pool attendant, the parking attendant, storekeepers, service station attendants. Incidents like the Halifax explosion are the stuff of legends, and everyone is interested. By telling people about your research, you allow word of your interest to spread. . . . Publishing findings can also be important. When the author published an article about the role of the railroads, that led to an editorial in a Halifax paper, and that led to a letter to the editor. (Scanlon, 2002, pp. 283–284; emphasis in the original)

Information was also discovered by following a trail, starting with a person, ending with a record. This was done with help from family, friends, and colleagues. Doing this requires conviction that any lead is worth following—and dogged determination. (p. 289)

In summing up his experience, Scanlon (2002) observes: "Not every trail was productive. . . . [But for] every apparently unproductive trail, there were unexpected discoveries" (p. 292).

My own concluding observation is that the process of gaining access to historical documents is not as random as it might appear. Although researchers' abilities to acquire materials may involve a bit of luck in addition to the dogged determination that Scanlon (a former news reporter) describes, the initial production and subsequent retention of documentary materials, like all source materials for historical analysis, have more to do with wealth, power, and literacy. As the question of selectivity is raised in the general question of "Whose history gets told?" so, too, one might ask "Whose perspectives on the disasters of the past survive?" With the passage of time, the influence of wealth and power plays an increasingly important part in determining which documents are preserved and therefore which documents disaster researchers ultimately will be able to access.

Generalizability

The documentary materials used in disaster research should come with a warning label. Killian (2002 [1956]) described it succinctly half a century ago: "Documents constitute an important source of data in the study of disaster, but they must be used with caution" (p. 81). ". . . [T]he bias of the writers must be kept in mind" (p. 82). Before using any documents, researchers should investigate the people behind them. They should learn as much as possible about a document's creators as well as its preservers, their points of view, their interests, and their motives. This is part of the "leg work" involved in field studies. It should also be part of the statistical analysis of secondary data. A good financial adviser will know the weaknesses of any economic indicator used to evaluate an investment opportunity; a good disaster researcher—indeed, good researchers in general—should have the same knowledge of the variables used in quantitative analyses.

Historiography also addresses the origins and motives behind the documents that historians use. Scanlon (2002), not surprisingly, has thought a good deal about establishing the validity of materials in historical disaster research and offers several suggestions:

> People create records, and the same rules apply for testing validity as apply to checking personal stories. Does the material have internal consistency? Is there any corroboration? Is the account something that reasonably could have been known to the person who created the record? If it is not evident, it is important to ask, "How did you know that?" Sometimes persons will provide information both about things they did or saw *and* about what they heard. It is important to separate observations from second-hand accounts. The first are usually accurate, the second are not. While this is harder to do using documents, it is important to ask, when reading a written account, "How would that person have known that?" It is also important to ask if there is anything which suggests why the person might have been less than truthful or have had a systematic bias. Finally, one must pay some attention to when an account was recorded. As time passes, members of organizations are likely to recall better organization than actually existed. They are also likely to recall that decisions were made at a higher level than was the case. . . . Some stories are easy to verify. . . . Other accounts are credible because one meshes with another. . . . Other accounts do not mesh so easily. . . . Some material is credible because the source has no apparent or conceivable reason for bias. . . . Sometimes material is useful because it helps establish credibility of other accounts. . . . The fact that something is not credible does not make it useless. (pp. 297–299; emphasis in the original)

Although documentary and archival materials may be more often used in disaster studies currently than when Taylor (1978, p. 276) urged researchers to make greater use of them, the same cautions about their use remain nearly 30 years later.

CONCLUSIONS AND PROSPECTS

Disaster Research Outside North American and Western Europe

In bringing this chapter to a close, it is appropriate to note some of its deficiencies as well as some of the topics likely to be major parts of similar undertakings in the future. Most obvious among the former is the applicability of the suggestions offered here to studies of disasters world wide. The bulk of research on disasters to date has been conducted by North American researchers on events in North America. Although research outside this portion of the Western Hemisphere has increased greatly in recent decades, existing discussions of the methods of disaster research, including the present one, are overwhelmingly colored by this geographic, national, and cultural skewing. A few treatments of methodological issues unique to disaster research in the developing world exist. Issues in cross-national comparative research facing nonnative researchers, for example, are discussed by Peacock (2002). Another perspective by a non-North American disaster researcher is provided by Khondker (2002) utilizing his experience in studying disasters in Bangladesh. With examinations such as these as starting points, it is time for a synthesis and an evaluation of research experience outside the developed world. Treatments from two different perspectives would be valuable—that of non-natives based upon their work in countries other than their own; and that of natives studying disasters in their own countries. For now, it must be said that the lessons to be taken from the present chapter may be less useful for researchers operating outside North America.

Some of the key issues that researchers face in conducting research on disasters in developing countries can be noted here at least. Most obvious is the choice of appropriate research design. Field studies remain a mainstay for a variety of reasons (Khondker, 2002, p. 337). Survey research is clearly possible, but the applicability of telephone-based survey techniques in most developing nations remains questionable. And documentary research confronts many of the same problems noted above in addition to those related to language differences and the need for translation skills (for non-natives) not to mention the predominance of oral rather than written traditions in many places where disasters are common.

Other issues that have been identified in the still sparse literature on methods for studying disasters in countries other than one's own include problems of ethnocentrism on the part of researchers and their lack of knowledge or appreciation of local beliefs and customs such as those associated with gender roles. The creation of multinational teams of investigators is one way that researchers have addressed such problems (Peacock, 2002, p. 237), including in such teams individuals, whether native or not, who can perform services such as securing cooperation from local leaders and conducting interviews with indigenous research subjects (Khondker, 2002, pp. 340–341). Another major set of problems is what Peacock (2002, pp. 244–247) calls "equivalence" issues. These include the reliability (i.e., the consistency of measurement from country to country) of social and economic indicators such as crime, divorce, suicides, and economic productivity. Also problematic is the comparability of seemingly similar units of analysis such as "the family." Even the comparability of events is often problematic in ways that differ from much of the current debate about what constitutes a disaster. Khondker (2002, pp. 335–337) notes, for example, that differentiating disasters from the normal state of affairs in many developing countries is sometimes difficult. How, for instance, does one distinguish between a famine and the constant state of mass poverty and malnutrition that are chronic in many parts of the world?

Despite such problems, there is virtually universal agreement that research on disasters occurring outside the developed nations is invaluable, whatever the obstacles. Peacock (2002,

pp. 239–240) for one identifies several trends that will continue to facilitate such research: the growing recognition of global issues and processes and how disasters play into these; increased funding for disaster research, especially from international organizations; a growing population of disaster researchers in countries outside North America and Western Europe; and increasing interest in and development of cross-national databases. Perhaps just as helpful, Peacock notes that, based upon his experience in studying the aftermath of Hurricane Andrew in South Florida and various disasters in Latin America, problems associated with disaster research abroad are not all that dissimilar from those encountered in doing similar research in ethnically and culturally diverse U.S. cities such as Miami (p. 236).

Ethical Issues in Disaster Research

In addition to methodological issues in studying disasters in less developed countries, another topic not dealt with explicitly here thus far, yet one also deserving of its own chapter-length treatment, is that of ethical issues in disaster research. The same generalizations with which this chapter began can be repeated for ethical issues in this field: they are unique only in degree, and their uniqueness varies directly with proximity to the crisis time period. Otherwise, the ethics of disaster research are no different from those associated with the social sciences in general. (For an introductory discussion of the latter, see Babbie, 1995, pp. 445–466.)

Ethical issues involve the *consequences* of researchers' actions during as well as after disaster and during the research process as well as after the research is formally concluded. Most prominent are questions regarding the impact of data gathering, whatever form it may take, on the lives of research subjects. The standard for researchers (and for university IRBs) is summed up in the phrase, "Do no harm" (Babbie, 1995, pp. 449–450). But during the crisis time period, researchers are intruding into people's lives at one of the worst possible moments. What special responsibilities do they have when interviewing people who may have lost all their worldly possessions and maybe even loved ones as well? What special responsibilities do they have when observing and perhaps occasionally conversing with disaster responders during the crisis time period?

Disaster researchers commonly justify both forms of intrusion as necessary for accumulating the knowledge that can be used to reduce suffering and improve response in future disasters. Yet the price of this knowledge accumulation—physical, psychological, emotional—is born disproportionately by subjects, not by researchers.[8] How hard should researchers push for an interview with a disaster victim, for example? Should they avoid asking certain questions when they suspect that those questions will be especially difficult to answer, given what they already know about what the subject has experienced? Should they persist to complete an interview with a subject who becomes highly emotional, or should they terminate the interview? Should they try to complete the interview at a later time or simply leave the subject alone? Should researchers refuse to perform an important emergency-related task if asked to do so by response personnel, even if no one else is available to do it?

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[8] This is not to ignore the price that researchers themselves pay, especially those directly exposed to the human carnage and physical destruction they witness in the immediate aftermath of disaster impact. The crucial distinction is that researchers by and large have voluntarily chosen to expose themselves to the products of calamity while the disaster victims who are their subjects and informants have not. While the former are likely to return to a "normal" existence in a matter of days, the latter will live with the consequences of their victimization for a much longer time, in many cases for the rest of their lives.

Other ethical issues arise once researchers are in possession of the data, information, and documents that they have sought. One is the issue of confidentiality. After assuring subjects that any comments they make will be held in strictest confidence, can researchers actually live up to this pledge? For example, it is common in writing up results to identify victims only by a few descriptors that loosely describe their social status (such as "a 29-year-old single mother of four whose home was destroyed"). But how does one disguise the identify of and thus preserve the confidentiality of remarks made by a mayor or a police chief when the data being presented involve their role performance during the crisis?[9] Furthermore, U.S. courts do not recognize data obtained by researchers from their subjects as a form of "privileged communication," as is the case with priests and attorneys (Babbie, 1995, p. 451). What promises can researchers make to potential interviewees about what will become of their remarks and about who will and who will not be able to obtain access to them later on?

Most discussions of the ethics of research assume that researchers seek to maintain a neutral or impartial stance, at least insofar as their role as researchers is concerned. Although no one presumably favors more and worse disasters (with the possible exception of American humorist and author George Carlin; see, e.g., Carlin, 2001), researchers face an ethical dilemma, often without being aware of it. This is the temptation to "take sides." (For an excellent general discussion, see the essay by Becker, 1967.) This dilemma is not unique to disaster research; it clearly exists, for example, in studies of what are conventionally described as social problems and deviant behavior. Although this issue is too complex to discuss fully here, it is worth mentioning if only briefly because of the potential consequences for the quality of disaster research.

The plight of disaster victims is compelling. The sometimes heartbreaking stories they tell often, implicitly or explicitly, suggest failures and missteps on the part of disaster-relevant organizations as well as predatory practices by local, national, and international officials and organizations. The temptation is strong to let one's view of catastrophe be defined by the stories of sympathetic victims in opposition to those of unsympathetic bureaucrats. This is further encouraged by the recent focus on vulnerable populations in disaster (especially women, the elderly, the poor, and children). However, taking sides does not further the objective of disaster research which, like all social science research, is to make sense out of the world around us by identifying its patterns, its regularities, and its continuities. To accomplish this objective requires that we treat our subjects' words as "data" to be understood, not as objectively real recreations of reality. This applies to all subjects and potential subjects, no matter where they are located in the social hierarchy (what Becker, 1967, p. 241, aptly calls the "hierarchy of credibility") or what their particular circumstances are in disaster.

The ethical position taken here—though it is by no means universally shared—is that both knowledge about disasters and the cause of reducing the human suffering caused by them are better served by adopting a stance of detachment rather than involvement (these terms borrowed from Elias, 1987) in one's research. It is from such a detached vantage point (from the "sidelines," as it were) that the clearest overall picture can be obtained.

To say that detachment is the more appropriate stance in one's role as researcher is not to say that involvement is inappropriate in one's personal life (or in other aspects of one's professional life). It has long been assumed that researchers' personal values are bound up in their selection of topics for inquiry. Max Weber (1949), for example, argued that not only are research problems in the social sciences driven by practical problems but further that

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[9] The fact that mayors and police chiefs are public figures provides researchers with some legal protection under current U.S. libel laws, but harmful consequences can befall such individuals nevertheless.

practical problems arise from researchers' personal values (p. 61). He further argued that scientific objectivity was not the same as moral indifference (p. 60). But the methods of social research, whether applied to the study of disasters or to any other topic, will not yield empirical results that will indicate the "right" course of action. Again, the classic statement is by Weber (1949):

> An empirical science cannot tell anyone what he [*sic*] *should* do—but rather what he *can* do—and under certain circumstances—what he wishes to do. (p. 54; emphasis in the original)

This same position was restated 80 years later by Gusfield (1984, p. 48). He argued that social science research can neither solve public problems nor resolve moral and political conflicts. Rather, it widens our understanding of alternatives from which to make choices and interpret events. Those choices, when they involve recommendations about what should be done about disasters, can be made only by appending one's personal values and beliefs to the findings from research. So long as one makes this clear—to one's self as well as to others—the line between involvement and detachment can be maintained.

Ethical issues in disaster research seem especially salient in events that become politically contentious. Accusations, suspicions, and finger-pointing make the researcher more visible to everyone involved, both during data-gathering and afterward. Indeed, the period after interviews have been conducted, the data collected, and documents obtained may present researchers with the most serious ethical dilemmas. Chief among them is the preservation of confidentiality assurances made to research subjects. Tierney (2002b, pp. 356–357) describes the experience of several researchers who were confronted with lawsuits brought by Exxon as a result of their fieldwork following the *Exxon Valdez* oil spill in Alaska. She also discusses the increasing use of SLAPP suits ("strategic lawsuits against public participation") brought by litigants hoping "…to intimidate, silence, and financially burden their critics" (p. 357). At the center of such legal skirmishes is access to researchers' data, including the identity of subjects and respondents. Researchers disagree on whether it is currently possible to protect the confidentiality of subjects and even whether it is ethical to offer such assurances in the first place, given the current legal climate. One can imagine the ethical as well as practical issues that will arise surrounding the research on Hurricane Katrina in view of all the political recriminations that have occurred as this disaster unfolds.

Fallout from 9/11

It is commonplace to say that "everything changed" in the United States on September 11, 2001. In many ways—perhaps most—it is not true, however. The changes affecting disaster research in the second half of the first decade of the 21st century, regardless of type of method, were underway long before that fateful day. Some changes, to be sure, were accelerated by the events of 9/11. Over and above these there have been changes directly attributable to the events of 9/11 that have made all forms of disaster research more difficult.

Most notable among the accelerated trends in the aftermath of 9/11 are the increasingly detailed requirements of university IRBs. The presumption is that disaster victims, similar to the survivors of 9/11, have experienced such emotional trauma as to make them fragile and in need of special protection from researchers. Assurances that project results will have practical benefits, written descriptions of the project including clear indications that participation in the study is voluntary, written and signed consent forms, and detailed descriptions about how the resulting information will be handled and stored as well as to whom it will (and will not) be

available are increasingly required of principal investigators before they can begin collecting data. Without prior approval, timing can be affected. If the effect of such ponderous-appearing preliminaries is to reduce the probability that subjects will agree to be interviewed, then access and ultimately generalizability will be affected.

Organizational self-protection, or its more extreme version, self-preservation, is another pre-9/11 trend that seems to have intensified. The increasing tendency of organizations to manage their public images, including the image that they present to researchers, was noted above (Tierney, 2002b, pp. 359–362). There is a sense among some field researchers that potential subjects' fear of retaliation from their superiors has increased in recent years. To the extent that such concerns actually do inhibit organizational "lower-participants" (from Etzioni, 1964) from agreeing to be interviewed, the more researchers must guard against the "command-post" point of view typically presented by organizational "higher-ups."

One trend seems to have been slowed by the events of 9/11. This is the trend away from field studies of the crisis time period of disasters and catastrophes in favor of topics less temporally connected to impact such as mitigation and recovery. In part this may be attributable to the costs of maintaining so-called standby field teams such as those employed by the DRC for many years (see Quarantelli, 2002a, especially pp. 101–106; for other discussions, see Scanlon & Taylor, 1977; Biderman, 1966). Such a standby capability requires continuity of funding at a certain level in order to train and retain a cohort of graduate research assistants (GRAs) who turn over completely every 4 or 5 years. Too, the priorities of major funding agencies such as the National Science Foundation have changed over time. Giving higher priority in proposal evaluation to multidisciplinary teams of researchers, for example, favors participation by individual disaster researchers over standby teams. The downside of such multidisciplinary efforts is that the specialized interests of social scientists become a lesser priority among those of physical scientists and engineers, or at worst are only afterthoughts. Other funding priority shifts such as greater interest in mitigation and the (usually unspoken) belief that enough is known about the crisis time period further reduce the willingness to fund standby teams and the kind of field studies described earlier in this chapter.

The most obvious and most pervasive effect of 9/11 on disaster research, not surprisingly, has been the greatly increased concern for security on the part of many key emergency response organizations. Security concerns have made all three types of research more difficult. Access to EOCs, where researchers can observe disaster operations first-hand, make initial contacts for later interviews, and collect the most ephemeral of the ephemeral documents, has become more difficult. Obtaining formal interviews in security-conscious organizations is also more difficult. Prior working relationships and a national (and international) reputation for studying disasters can be decisive in overcoming such obstacles in a timely manner. Survey research has become more problematic as well, although this seems to be more the case when organizations are involved, either as objects of study or as sponsors and endorsers than with general surveys of population samples. And document acquisition, not surprisingly given the considerations noted previously, is noticeably more difficult where emergency response organizations are involved. Paraphrasing one veteran disaster researcher, the current climate is one in which "testosterone is everywhere."

Social and Technological Change and the Methods of Disaster Research

The new century has seen a carryover from the previous one of various social changes and trends affecting disaster research. Among the most significant is the continuing growth in the

number of social scientists conducting research on disasters and hazards in countries outside Western Europe and North America. Transnational groups such as the International Research Committee on Disasters (Research Committee 39 of the International Sociological Association), LA RED (a multidisciplinary network of disaster specialists in Latin America and the Caribbean), the Disaster & Social Crisis Research Network (a multidisciplinary group interested in the development of disaster-resilient communities in Europe), and more specialized groups such as the Gender and Disaster Network not only serve as links among researchers in different countries but also help to nurture those new to the field. Although the cohort of academics engaged in disaster research on a consistent basis in the United States has not expanded as rapidly as that in other parts of the globe, nevertheless it has seen the training and emergence of a second and now a third "generation" of disaster researchers. Also relevant to disaster research in the United States has been the growing number of undergraduate and graduate degree programs in disaster management that are producing a new cohort of professionally-trained emergency managers (see, e.g., Neal, 2000). These individuals by and large have a better understanding of and appreciation for the value of social science research than many of their colleagues. To the extent that they are able to open doors for researchers, they make the task of studying disasters easier.

New technologies are also affecting disaster research, making the work easier and researchers more efficient in many ways but posing new challenges as well. On the plus side, a variety of portable electronic devices are especially useful in field work, at least when conditions permit. Chief among these are cellular telephones. Cell phones make the coordination among field team members easier and expedite relationships with potential subjects. Along with PDAs and BlackBerries having e-mail, Internet, and Global Position System (GPS) capabilities, they facilitate better, more instantaneous relationships between field teams and their home bases. Cell phones with digital cameras also make it easier to photograph scenes inside EOCs and at other locations that otherwise would take many words to describe. They also can be used to capture ephemeral "documents" such graffiti and erasable scribbling on blackboards. At the same time, camera phones may cause difficulties with organizational officials concerned about privacy, security, and litigation. They also pose ethical questions related to privacy, voluntary participation, and right of refusal such as whether it is acceptable to use them without first seeking permission.

Digital audio recorders in one sense are no different from their more cumbersome ancestors which have been used to record interviews in the field for more than half a century. However, uploaded to a computer with voice recognition software, it is possible to transcribe interviews without having someone listen to the recording and key in everything that was said (although the process of conversion to text remains less than seamless). Voice recognition software and a laptop computer allow field researchers to dictate their handwritten notes, observations, and anything else so that it can be printed out or even e-mailed back to the home base immediately. And camcorders, while also similar to other regular cameras, movie cameras, and video recorders, make it possible to store, transmit, and process pictorial data more quickly and more efficiently. However, the same issues surround their use in the field as with these older devices. In addition to privacy concerns, there is the additional concern that the use of camcorders and other photographic devices might alter in some way the behavior of subjects being recorded.

New technologies in the hands of research subjects offer the possibility of new types and sources of data on disaster and its aftermath. Most obvious are Internet Web sites. These range from existing sites maintained by news organizations that post articles and photos pertaining to the disaster to the sites of emergency and relief organizations to entirely new sites created in

the aftermath of a disaster. Blogs (i.e., Web logs), the diary-like personal accounts written by individuals that have proliferated on the Internet, are a potentially rich source of data. So, too, are message boards, newsgroups, and Internet Relay Chat (IRC, or chat, for short). Researchers interested in the popular culture of disasters (see Eyre, Wachtendorf, & Webb, 2000) have an exciting future ahead of them as they mine these new data sources.

A note of caution must be sounded regarding such sources, however. Although the availability of digital documents, particularly official documents on government agency Web sites, can be quite useful, the availability of online documents remains hit-and-miss, raising questions about why some documents are available and others are not. Blogs, message boards, newsgroups, and chat are used by a select segment of the population. Who these users are, what viewpoints they bring to the Internet, and what interests and motives they have should be the subject of separate research. It can be assumed, for example, that such participants are not only among a society's more literate members but also are among its most "technology literate" as well. What implications do these twin characteristics have for the ability to generalize from such data sources?

Other drawbacks of these new technologies have implications for disaster research as well as social science research more generally. Chief among these is increasing cell phone use and its impact on survey research. Apart from issues such as the availability of cellular telephone service in the aftermath of disaster, a new concern is the *disconnect* between cell phone users and their geographic locations. Sampling techniques for telephone-based surveys attempt to compare disaster victims with nonvictims selected randomly with area codes and prefixes identified as high damage and low or no damage areas, respectively. However, cell phone users (and those using VOIP [Voice over Internet Protocol], a computer-based telephone technology) are no longer necessarily located in the same geographic area as their landline-using counterparts. Reaching subjects who use cell phone raises questions about violating the U.S. Telephone Consumer Protection Act (47 U.S.C. 227), about how to compensate subjects for their cell phone usage, about the safety of interviewing subjects without knowing what they are doing while being interviewed (e.g., driving), and about the types of questions that can appropriately be asked if subjects are responding in public places where others are able to hear their replies.

There is also a downside to these new digital media as sources of data on disasters. With cell phones and other hand-held devices capable of sending and receiving text messages, data on interpersonal and interorganizational communications become increasingly more ephemeral in absence of written communication logs. So, too, does e-mail correspondence for those not included on distribution lists. Acquiring copies of such correspondence after the fact may be exceedingly difficult for practical as well as legal reasons. Other examples could easily be cited.

The overall conclusion would seem to be that these new technologies represent a net benefit for disaster researchers, but at the same time they exacerbate existing challenges and create a few new ones as well. Another conclusion is that there needs to be some research on disaster research itself. Shared experiences need to be captured and organized, and a set of "best practices" or at least recommendations formulated. This is especially true for experiences of researchers operating outside North America and Western Europe and of those working with these newer digital media and technologies. This chapter, if it achieves its objective, can serve as a starting point.