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Taxonomy and Disaster: Theoretical and Applied Issues

THOMAS E. DRABEK

During the past three years I have reviewed nearly one thousand publications that assessed various aspects of human responses to disaster (Drabek 1986). While most of these were prepared by sociologists, many were published by social geographers, economists, anthropologists, psychologists, political scientists, and representatives of other soical science disciplines. My aim was to identify the key findings from each study and then to synthesize them into a series of analytical propositions. The propositions serve to summarize the major contours of the disaster research legacy.

Because I had participated in a similar effort a decade earlier (Mileti, Drabek, and Haas 1975), I had some awareness of the issues and limitations inherent in such a venture. For example, Aguirre (1976) has challenged us to think much more carefully about numerous barriers that preclude the construction of propositional inventories and other types of integrative syntheses. In contrast, Torry (1979) has urged that we not limit our inquiries by excessive adherence to disciplinary boundaries so as to preclude the conclusions provided by anthropologists. Of course, a similar argument could be made regarding the other social science disciplines. Thus, as I proceeded to cluster over two thousand empirically verified findings around 153 subtopics, my concerns intensified about defining disaster and current approaches to its study. As I wrote in 1986,

> Throughout this book I have argued that the most basic scientific problem confronting social scientists studying disaster responses is the matter

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of taxonomy. Kreps' (1985b, 1984b) work has pushed this matter further than others. I am not convinced that a single set of event-related criteria will do the job, however. Rather, researchers must carefully assess event properties across each of the disaster phases. Similarly, far greater specification must be made regarding the type of response system regardless of disaster phase. Not all social systems are the same.

Hence, I was pleased to receive Kreps's invitation to the 1986 symposium and to provide some reflections on the theoretical and applied issues implicit in the taxonomy problem. I have organized these reflections into five major topics: first, the emergent recognition of the taxonomy problem by disaster researchers; second, illustrations of and issues pertaining to the use of disaster and hazard taxonomies; third, taxonomic work in organizational research; fourth, integrations of disaster-social system taxonomies; and fifth, implications for emergency management practitioners.

Through this sequential analysis of these five themes I will develop a conception of disaster as a particular kind of social problem. The taxonomic issues inform the development of that conception.

The Increasing Recognition of the Importance of Taxonomy by Disaster Researchers

Two decades ago a taxonomy of disasters was viewed by many as esoteric or "theoretical"—in the negative sense of that term. But as I surveyed the literature of the past decade, I detected a pronounced shift. In part it stems from key points of controversy. For example, in contrast to earlier images of victim adaptation and resiliency (e.g., Fritz 1961; V. Taylor 1977; Bolin 1982; Drabek and Key 1984), some have proposed that acute and persistent mental health impacts can be traced to the horror experienced by flood victims in Buffalo Creek (e.g., Erikson 1976; Lifton and Olson 1976; Gleser, Green, and Winget 1981) or the lingering uncertainties of families residing near the nuclear power plant at Three Mile Island (e.g., Goldsteen, Schorr, and Goldsteen 1984). To reconcile these and other apparent inconsistencies, many researchers have proposed that events may differ in significant ways so as to "... make one disaster more disruptive or stressful than another" (Baum and Davidson 1985, 33; see also other chapters in Sowder 1985).

Similarly, building on field data collected following several of the so-called ghetto riots of the late 1960s, Quarantelli and Dynes (1969, 1970) explained why looting behavior—a phenomenon of conspicuous absence in "typical" disasters—emerged. (See also Warheit 1972.) So too have responses and adaptations by media organizations following such "civil

disturbances" been found to differ from those precipitated by natural disasters (Kueneman and Wright 1975, 677). Thus, while crude and lacking in precision, the literature of the late 1970s and early 1980s is repleat with statements of qualifications by type of event. The implied argument is for systematic taxonomic work.

There are additional sources of controversy that converge on problems of taxonomy. Let us briefly consider two of them. First is the issue of threshold—when should a crisis event be classified as a disaster? Several researchers have proposed threshold levels that distinguish events of varied scope and/or magnitude. For example, Foster (1976, 241–45) proposed to use life-event stress scales as an analogy to develop a measure of event magnitude using four criteria: number of fatalities, number of seriously injured, infrastructural stress, and total population affected. Similarly, Geipel (1982, 30) differentiated among sixty communities following the earthquakes (6 May and 15 September 1976) that struck the Friuli region in northeastern Italy by using five criteria (e.g., percentage of homeless at varied dates).

While elaborate, such schema reflect the earliest conceptualizations of disaster events. For example, Fritz (1961) proposed:

> Disasters, of course, differ in many ways. They differ in the degree of their *predictability*, *probability*, and *controllability*; in the nature of the precipitating agent (flood, fire, explosion, tornado, hurricane, earthquake, etc.); in their *origin* (natural, man-made); in their *speed of onset* (instantaneous, progressive); in their *scope* (focalized, diffused)*; and in their *destructive effects* on people and physical objects. Human behavior differs in relation to each of these features of disaster, and also within a given disaster by *spatial zones*, by *time periods*, by *type of involvement*, and by *prior preparation and conditioning*. The various combinations and permutations of these variables introduce numerous subtle contrasts and distinctions in human responses to disaster that cannot be covered systematically within the scope of this chapter. (Fritz 1961, 456)

The asterisk in the quotation denotes Fritz's footnote to Carr's (1932) seminal essay, which also discusses several of these designations. Thus, from Fritz's perspective—as defined in an unpublished paper that he quoted (Endleman 1952)—a disaster is:

> ∴ an event, concentrated in time and space, in which a society, or relatively self-sufficient subdivision of a society, undergoes severe danger and incurs such losses to its members and physical appurtenances that the social structure is disrupted and the fulfillment of all or some of the essential functions of the society is prevented. (Fritz 1961, 655)

While defining disasters as a subset of "collective stress situations" (i.e.,

"a collective stress occurs when many members of a social system fail to receive expected conditions of life from the system"), Barton (1969, 38) created a typology of events by using four variables: (1) scope of impact (geographical, number of people); (2) speed of onset (sudden, gradual, chronic); (3) duration of impact itself (short or long); and (4) social preparedness (low or high) (Barton 1969, 41). Thus, a plant explosion that impacted only a single organization was classified differently from a tornado that ripped through a city segment or an earthquake that damaged an entire region of a country. Natural, technological, and political circumstances were used as illustrations. While not exhaustive, Barton's classification presents disaster as a broadly defined social problem.

Of course, the more specific criteria by which events might be classified within this typology were not designated. How, for example, was scope of impact to be assessed? Hence, when Rossi and his asociates tried to bring increased quantitative rigor to disaster research, they discovered important voids (e.g., see P. Rossi et al. 1978 and Wright et al. 1979). As they tried to document the long-range impacts of tornadoes, floods, and hurricanes on counties and census tracts, they perforce struggled with the threshold problem. In part, because of the tone and style with which they initially summarized their conclusion, many researchers and practitioners reacted with concern (Drabek 1981). Based on numerous case studies, it was known that a blanket conclusion of "no-effect" was in error. Yet initially that seemed to be what Rossi and his colleagues were proposing. While the tenacity and quantitative rigor of the research team was admired, the relevance of the events they had selected for study was questioned. Simply put, were they studying disasters?

Rossi's research results point to the potential utility of impact ratios (i.e., ratio of damages to available resources) in the development of thresholds (e.g., see P. Rossi et al. 1978, 129). It should be recognized explicitly that disasters are not unform in their impacts. Some communities may have more resources than others. And microunits, like families, may have relatively little resource base in comparison to what may be lost in even a small tornado. Thus, both the losses and the resources must be reviewed in some manner, be it called "absorptive capacity" as Kreps proposed (1984b), "system vulnerability" as Pelanda (1982) proposed, or "system stress" as Warheit (1985), or earlier, Haas and I proposed (1973). I will return to this point later.

Of course, the most controversial aspect of the threshold issue crops up when the nuclear war case is added to the mix. The question becomes, "Can we generalize the results of community responses to a large tornado to the postattack environment?" Several monographs published at the Disaster Research Center included sections entitled "implications for a nuclear catastrophe" (e.g., Anderson 1968, 59–68; 1969, 55–62). Some-

what reflecting a constraint and rationale for funding, etc.
dissect the problems of generalization. They make it clear that this question is very complex. The contrast in reviews by Platt (1984) and Kreps (1984a) of Perry's book entitled *The Social Psychology of Civil Defense* (1982) is an illustration of an increased awareness of this complexity, As such it is but one of the many pieces of the disaster taxonomy puzzle.

A second element of controversy comes from two very different sources; social geographers and German sociologists. Reflecting a long-term interest in responses to flooding, Gilbert White (1945, 1975) has influenced successive generations of social geographers who have researched various aspects of natural hazards. Viewing disaster events as only one category of issues within the broader problem of uses of the natural environment, White has focused on public policy and the perceptions of hazard and risk that guide its formulation. Thus, rather than ask "How do people respond to a flood?", he urges instead that the issue be recast to "How do people seek to use flood-prone lands?", "What are the consequences of alternative use patterns?", and "What social policy options might precipitate better use of such hazard-prone environments?" (e.g., see Changnon et al. 1983, 183–217).

These questions redirect the research initiative so that disaster mitigation alternatives, such as flood insurance and land-use management and hazard perception studies, become the central thrust. This line of inquiry dovetails with those interested in risk analysis and risk management (e.g., see Burton, Kates and White 1978; Douglas and Wildasky 1983; and Short 1984). Hence, economists like Kunreuther and his colleagues (e.g.. 1978) or Milliman (1983) point out that excessive focus on events like floods and their costs preclude a more balanced view of policy options.

While many criticisms of event-focused disaster research have been made, a collection of essays edited by Hewitt (1983) highlighted this issue and its theoretical implictions. For by focusing on disasters as extreme events, most sociologists have given minimal attention to both issues of mitigation and problem definition. We have failed to ask—as Perrow (1984, 1983) pointed out so cogently in his analysis of the accident at Three Mile Island—which institutional processes place some people at greater risk than others. And as Morren (1983) noted, we have not questioned the problem definition that, in turn, structured images of disaster victims and their needs. Carrying the argument to the public policy levels, Copans (1983, 96) echoed the same message: "For the struggle against the drought and the famine begins with the struggle against those who profit from it." Thus "natural hazards" become transformed into human-caused events that are interpreted as resulting from strategies of control and domination by powerful elities. The criticisms from this wing of social geography parallel the earlier insights provided by conflict theorists like L. Coser (1956)

and Dahrendorf (1968), and more recently by advocates of "critical theory" (e.g., see Zey-Ferrell and Aiken 1981).

These views also parallel the criticisms and insights offered by several German sociologists (for a summary see Schorr 1985). The constraints of functionalism are charged to have precluded analyses of conflict, and more important, "a proper" conceptualization of disaster as a process, rather than an event. According to Schorr, Clausen, for example, proposed that "... disaster is not something abnormal and external to the system but is quite normal and can be thought of as a step in the process of social change" (Schorr 1985, 15, see also Dombrowsky 1985).

In summary, as we ponder the problem of taxonomy, I urge that we do so within this much broader context of concern (Drabek 1983b). Below I will argue that our taxonomic efforts should address these issues head-on and thereby expand the sociological research agenda. These issues include: (1) the criteria used to distinguish events; (2) the threshold problem; (3) continuities with social processes related to, and perceived risks associated with, the use of the environment and various technologies; and (4) problem definitions advocated by persons located at varied points within the social structure. Unlike the critics who imply that the disaster research legacy should be abandoned, I propose that it be expanded, both in scope and in level of abstraction so as to provide to sociology the enriched theoretical opportunities that it offers. For as I concluded at the end of my literature review exercise, disasters truly comprise strategic research sites.

From Event Characteristics to Disaster Typologies

With the above issues as context, I will now review several disaster typologies and discuss a series of issues that one confronts when a taxonomic approach is taken. Let us start by recalling the definitions and qualities offered by Fritz (1961) and Barton (1969). As noted above, Fritz (1961) identified seven event qualities (e.g., predictability, probability, controllability, etc.) and Barton (1969) used four (scope of impact, speed of onset, duration of impact, and social preparedness).

Dynes (1970, 52–55) expanded the list by identifying nine qualities: frequency, predictability, controllability, cause, speed of onset, length of possible forewarning, duration, scope of impact, and destructive potential. In contrast to these features, Schulberg (1974) emphasized adaptive capacity—a variant of Barton's social preparedness dimension. And finally, Warheit (1972, 134) has stressed the normative context: "This varying normative consensus (and dissensus) which provides the operational context for a community's emergency sub-system is perhaps the most important difference between natural disasters and civil disturbances." This

quality, as noted above, was [illegible]
why looting behavior occurred during civil
disasters.

More recently several researchers emphasized event variability as a key to unraveling inconsistencies in research findings regarding potential mental health impacts (e.g., Erikson 1976). Different contributors to a volume edited by Sowder (1985) proposed the following qualities:

1. Victim vulnerability (primary versus secondary victims such as family members or rescue personnel and demographic characteristics) and event qualities (terror and horror; duration of impact; unexpectedness; preimpact and postimpact threat; impact ratios; potential for prolonged alteration of the postdisaster environment; and cultural and symbolic aspects, e.g., level of control people feel they have over a situation) (Bolin 1985a, 3–17).
2. Disasters as stressors (duration of the stress; nature and extent of impact, including the terror and horror evoked; predictability; and controllability). Thus "technological disasters, like Three Mile Island, represent a different kind of control-related phenomenon" (Baum and Davidson 1985, 29–37, quote on p. 36).
3. "Among major dimensions or characteristics that could be singled out with respect to a population's response in a disaster, eight will be discussed. Given our conceptualization of disaster, the emphasis is on characteristics of the occasion, rather than any dimension of an agent (even if there is one, which is not always the case)." The eight dimensions are proportion of the involved population; social centrality of the affected population; length of involvement of the affected population in the crisis; rapidity of involvement by the population in crisis; predictability of involvement in a crisis; unfamiliarity of the crisis; depth of involvement of the population in the disaster; and recurrency of involvement (adapted from Quarantellli 1985, 59–63).
4. While disasters have been differentiated by cause, community preparedness levels, victim roles or specific stressors, they "also should be classified in terms of separate characteristics of the post-disaster environment." These separate characteristics are community disruption factors such as duration; effectiveness of relief efforts; trust in communication; community attitudes toward survivors; geographic spread of survivors in relation to the disaster; and social support (Lindy and Grace 1985, 139–47).

Beyond event characteristics, other researchers have tried to build on Barton's initial typology of collective stress situations to create preliminary typologies of disasters. For example, Berren, Beigel, and Ghertner (1980)

used five variables (type, duration, degree of personal impact, recurrence potential, and control). This five-dimensional matrix could be used to "locate" different events into specific taxonomic cells that may provide a basis for organization of comparative work across events. Thus, if certain group responses were found to be consistent across events within each of these taxonomic niches, the range of generalization for study findings is thereby specified.

The logic here parallels Willer's (1967, 97–115) idea of conditional universals (i.e., empirically tested relationships with the universe to which they apply specified) that I described in my earlier discussion of the taxonomic approach (Drabek 1970, 334). Such a matrix could also serve as a guide for sampling sets of subsequently occurring events. This procedure has been proposed by many researchers during the past decade under such labels as dimensional sampling (Arnold 1970), theoretical sampling (Glaser and Strauss 1967) or purposive sampling (see Drabek et al. 1982, and Wallace 1983, 418).

Addressing the threshold problem I alluded to above, and noting that the terms *disaster*, *emergency*, and *accident* are often synonymous, Britton (1985) proposed a differentiation. As there is an increase in three variables (number of people, degree of involvement, and amount of disruption), we shift from events that might best be classified as "accidents" to "emergencies," and finally to "disasters."

Noting that "there have been very few attempts to make systematic comparisons of human response to different types of disaster agents," Perry (1985, 14) selected a key dimension proposed by Anderson (1969)—that is, secondary impacts—and added it to the four selected by Barton (see Perry 1985, 14–21). This fivefold set of "defining characteristics" facilitated his comparison of three types of events: riverine floods, volcanoes, and nuclear power plant accidents.

Of course, in efforts like these the problem of threshold remains—that is, cut points have to be determined to distinguish among the cells. And debates about such thresholds and whether or not the variables should be conceptualized as linear or nonlinear are important to taxonomic work. Thus these works should be welcomed as signs or progress.

Following his assessment of evacuation responses for events in these three categories and brief discussion of potential short-term impacts on individuals, Perry (1985, 102–16) speculated about potential long-term impacts on individuals. Building on his previous work (Perry 1979a; Perry and Lindell 1978), he integrated the insights of others into a multivariate model comprised of fourteen propositions linking characteristics of disaster impact, social systems, and individuals (Perry 1985, 112). Thus he extended and clarified the type of mode-building contributed by Barton (1969). His model proposed, for example, that "the longer the duration of

impact, the more likely is the destruction of kin and friendship networks" (Perry 1985, 113). And in turn, "the more extensive the destruction of kin and friendship networks, the greater the probability of grief reactions" (Perry 1985, 113). Only three of the five characteristics used in the initial typology were incorported, however. Perry was not explicit about his reasons, but I infer that he sensed that speed of onset and social preparedness were less relevant at this point in the life cycle. Through such multivariate models, and there are now many of these in the literature (see Drabek 1986), disaster researchers have begun to specify more complex networks of relationships that recognize explicitly the variability in disaster events and use them as part of the predictive calculus (e.g., see Bolin 1985b; Green 1985; and Solomon 1985).

These same themes, although reflecting different variables, were proposed by Garrison (1985) in her review of the consequences of disaster-induced relocation for individuals who had been victimized by various events. She wrote, "The type stressor that occurred both prior to and during the disaster, the personal meanings of these stressors, and individual coping abilities all combine to help determine a person's ability to confront the additional demands of relocation . . ." (Garrison 1985, 57). And echoing the views of many who have described the human costs of prolonged uncertainty that sometimes characterize the disaster recovery phase, Garrison emphasized the variable of controllability. Apparently, some disaster victims have been stressed further by lingering uncertainties regarding their safety: "At least five Times Beach families actually were relocated to another contaminated site, where they were once again advised to evacuate" (p. 57).

Similarly, and building on his earlier observations regarding the importance of explicit recognition of event variability, Warheit (1985) formulated a framework that viewed disasters as stressful life events. Such events may rise from any or all of five general sources: "1) the individual's biological constitution; 2) the individual's psychological characteristics; 3) the culture; 4) the social structure, including interpersonal relationships; and 5) the geophysical environment" (Warheit 1985, 199). This framework is another example of a model-building approach to the problem of taxonomy. It permitted the formulation of twenty propositions relating qualities of events, social structures, and individuals to stress levels. For example, stress levels are increased "when an event poses high risk to the lives of individuals and/or represents a major threat to property, community structures and/or values and beliefs" (p. 204).

Typologies of Hazards—Natural and Otherwise

In sharp contrast to all of these event-oriented formulations are works fo-

cused on hazards, risks, and their management (Burton, Kates, and White 1978; Covello 1983; Burton, Fowle, and McCullough 1982; Hohenemser, Kates, and Slovic 1983; Kunreuther and Ley 1982). Therefore, beyond the integrations implicit in the views presented thus far, some researchers like Swisher (1985) have called for linkages to these research areas.

Of course, as with the term *disaster*, maybe even more so, the terms *risk*, *risk assessment*, and *hazard* are defined differently by various researchers. And many of the unsolved puzzles in disaster taxonomies have their parallels within this approach. Sidestepping this series of debates and the relative merits of alternative definitional stances, I will define hazards as "threats to humans and what they value ..." (Hohenemser, Kates, and Slovic 1983, 379). In contrast, risks are "quantitative measures of hazard consequences that can be expressed as conditional probabilities of experiencing harm" (Hohenemser, Kates, and Slovic 1983, 379). And in turn, risk analysis is comprised of three subareas: risk estimation, risk assessment, and risk management.

Risk management and risk assessment are inherently normative. Similarly, risk management may be viewed as differing from emergency management in that the latter term refers to a subset of managerial options—i.e., those related to actual, not potential, events (see Berke and Ludeke 1985, 2–4). However, as with more recent definitions of disaster, so as to include the threat to life or property (e.g., see Quarantelli 1985, 51), so too have many recognized the merits inherent in a "comprehensive" emergency management perspective (National Governors' Association 1979). I therefore propose that we conceptualize hazard management as encompassing the domain circumscribed by emergency management but extending beyond so as to include risks of a more mundane nature, such as skateboards, bicycles, and so on. Both terms ought to refer to both potential as well as actual disaster events across their full life cycles, including the processes of problem definition or recognition.

This expansion of the taxonomic task may appear to be overwhelming, and hence undesirable. But as Cvetkovich and Earle (n.d., 6) have pointed out regarding biological taxonomics, it has been estimated that there are ten million kinds of organisms in the world, of which only 15 percent have been studied scientifically. Hence, I endorse their argument for a "constructionist" approach (as opposed to "essentialism") whereby we consider classification systems as aids to thinking and communication and assume that there is no single "correct" or "best" classification system for every purpose by every potential user (see Cvetkovich and Earle n.d., 6–8). This parallels Kreps's (1984b) suggestion that researchers should try to be precise about the range of events that they include, rather than laboring over extensive listings.

In turn, it seems important to recognize that human beings with varied

experiential bases may have widely divergent views of specific hazards and the degree of risk they represent. Indeed, as Hohenemser, Kates, and Slovic (1983) have documented, the very criteria used and their relative ranking appear to vary between scientists and lay persons.

This perspective led Hohenemser, Kates, and Slovic to identify twelve hazard descriptors that reflected the life cycle or causal sequence, such as intentionality, spatial extent, concentration, persistence, recurrence, and so on. By surveying ratings of hazard perceptions ($n = 34$) across these factors for ninety-three hazards that ranged from auto crashes and chainsaw accidents to nuclear war and a harmful release of recombinant DNA, they proposed a seven-class taxonomy: (1) multiple extreme hazards such as recombinant DNA; (2) extreme hazards such as (a) intentional biocides (e.g., chain saws), (b) persistent teratogens (e.g., uranium mining), (c) rare catastrophes (e.g., liquefied natural gas explosions), (d) common killers (e.g., auto crashes), and (e) diffuse global threats (e.g., ozone depletion); and (3) hazards such as saccharin.

This analysis is only one of several that were contrasted and critiqued by Cvetkovich and Earle (n.d.). Among the others that were thought-provoking was Mileti's (1980) classification of responses to environmental extremes (i.e., purposeful adjustments, incidental adjustments, and unwitting adjustment). (See Cvetkovich and Earle n.d., 49–51). Also useful was their discussion of the classification of environmental stress conditions developed by Lazarus and Cohen (1977), who used three criteria (speed of onset, shared versus individually experienced, and duration) to form three categories (cataclysmic events; individually experienced life stresses such as divorce or loss of a job; and daily hassles). (See Cvetkovich and Earle n.d., 44–46).

These various disaster and hazard classification schemes illustrate the types of differentiations and rationales that are now rather numerous in the literature. Thus, each in their own way, the hazards and disaster research traditions contribute to an image of the human problem of social construction. And as I argue in detail below by asking the questions "what is a hazard?" and "what is a disaster?", the context of life histories of events, both real and potential, looms forward, as do the definitional processes used by various interest groups for their particular purposes. After pondering these matters for some time, I identified seven separate issues that require careful review and explicit consideration as we proceed to wrestle with the taxonomy issue.

Implicit Issues in Disaster Typologies

1. *Who are the victims?* As A. Taylor (1983) and many others have pointed out, disasters may vary greatly in the range of impacts on "vic-

tims" of various types. Thus efforts to conceptualize the degree of impact must go beyond the obvious. Certain disasters may precipitate a large number of victims among rescue personnel, for example, who may be traumatized by rescue-and-recovery efforts. They, like impacted kin and friends, constitute a category of "hidden" victims that must be recognized in comparative work. But, as the hurricane studies by Moore and his colleagues (1963), Erikson's (1976) Buffalo Creek analysis, and Geipel's (1982) assessment of the Friuli response indicate, regional or subcultural qualities of victim populations may exacerbate or reduce the impact of certain disasters. Similarly, the national grieving precipitated by the televised horror produced when the spaceship Challenger exploded cannot be understood simply by noting the deaths of the seven crew members. Comparisons based solely on death tolls or simple damage estimates will preclude important understandings and insights.

2. *Which social systems are impacted?* As opposed to research wherein individuals are the units of analysis—be they "hidden" or obvious victims—important research questions require that organizations, municipalities, and other forms of social structure be analyzed. Little headway has been made by the disaster research community toward penetrating the cross-system interactions and implications. For example, are the family impacts different among cities with markedly different structural features? (See Quarantelli and Dynes 1985, 165.) In short, many research questions require that samples of social systems be selected.

3. *What criteria define a sample of events?* As noted above with Perry's contrasts (1983, 1985), we have just begun to see cross-event comparisons that are done more carefully. But the criteria that characterize and differentially scale events as disparate as wars, droughts, and economic dislocations have not been articulated. Thus, when we read of the difficult adjustments made by the widows of the Sunshine Mine disaster (e.g., Harvey and Bahr 1980) and note how these paralleled those confronting the uncertainties stemming from husbands who were prisoners of war (McCubbin, Olson, and Patterson 1983) or missing in action (Boss 1983), we wonder about the outer limits that define "event." Could parallel coping processes operate among families stressed by incarceration of a spouse or a divorce? Thus, the definitions of *emergency* and *disaster* and *collective stress* permit some researchers to envision theoretical payoff in comparisons that might never occur to others. The criteria that order and structure the comparisons must be articulated more carefully.

4. *What transhistorical assumptions should be made?* If cross-event comparisons are made, to what degree must analysts be sensitive to history? Indeed, I have encountered a few researchers who question the probable utility of work like Kreps's because of his "fossilized" data. They have argued that federal policy changes have been so enormous since the 1964

scene. I disagree, of [illegible] at which he is working, but we should be sensitive to the issues implicit in our trans-historical assumptions.

[illegible] the [illegible] variations. As noted [illegible] (Erikson 1976), rooted in the traditions of [illegible], did not appear to respond to the postflood stresses in ways that paralleled families in Topeka, Kansas (Drabek and Key 1984), or Xenia, Ohio (Taylor 1977). So if responses are to be generalized, what limits are appropriate? To date we have made minimal progress toward integrating the insights and approaches to comparative structural analysis whereby societies, communities, or organizations, for example, might be compared regarding event responses or approaches toward disaster mitigation. Although Mileti (1983) has contrasted Japanese and United States organizational responses to earthquake predictions, Perry and Hirose (1983) have compared two communities seeking to recover from the disruption of volcanic eruption, and Huffman (1983) has compared the evolving litigative principles and legal definition of liability in four nations (Japan, Soviet Union, China, and the United States), these instances remain exceptional rather than typical. As a result, the comparative work completed by McLuckie (1977, 1975) remains relatively isolated within the disaster research legacy. Hopefully, that will change during the next decade.

6. *Where does disaster causation fit into the taxonomy process?* Perceptions of risk appear to vary with several factors, including a dimension of choice. If placed at risk because of political decisions that precipitate war or managerial greed that produced a hazardous toxic dump, individuals respond differently at several phases in the life cycle of such events. This is in contrast to so-called natural disasters. Analysts wishing to compare events should not ignore the perceptions of causation and intentionality either as defining criteria or social processes that merit study. One need not accept the limited frameworks of Marxists or so-called conflict theorists to recognize the importance of this matter.

7. *How do mitigation activities relate to the disaster taxonomy problem?* As I noted above, German sociologists have criticized the American tradition of disaster research because it ignored conflict processes and the mechanisms whereby populations have been placed at risk. So too have many social geographers stressed the importance of studying the perceptions of hazards that have stimulated or thwarted the development of policies that might have mitigated potential disasters. Our agenda must be stretched to recognize the insights and implications provided by Hewitt and

others who have viewed sociological disaster studies as inherently limited. We have failed to ask how official definitions of disaster are proposed, legitimated, institutionalized, and changed. Nor have we asked whose interests are served by these ongoing social negotiations.

Taxonomy in Social Organization Research

What types of social organizations are there? While most researchers would accept the premise that not all social organizations are the same, there is minimal agreement regarding the criteria that differentiate them. Certainly the literature on organizations—call them formal, complex, or whatever—is replcat with typological schema. Of course full-fledged taxonomies have not yet appeared. Rather than examine groups, communities, or societies—for which typologies have been developed—let us review the organizational literture briefly in order to have a context for a series of issues. For example, many researchers have found utility in a Parsonian variant, be it a goal- (Meyer 1972; Blau and Schoenherr 1971) or product-based version of the AGIL System (e.g., Hills 1968). Others have proposed such criteria as (1) prime beneficiary (Blau and Scott 1962, 42–50), (2) control structure (Van Riper 1966, 1–12), (3) compliance structure (Etzioni 1961, 3–67), and (4) technology (Thompson 1967; Perrow 1967; Woodward 1965).

But in 1961, Haas, Hall, and Johnson (1966) began collecting data on seventy-five organizations in order to construct an empirically based taxonomy. Their efforts were imaginative but never produced an ordered structure that could be used by others for classification. While many of the structural variables they examined proved to be useful in differentiating clusters of relationships, the global taxonomy they envisioned at the outset did not emerge (e.g., see Hall, Haas, and Johnson 1967; Hall 1982).

Similarly, the more theoretical and abstract efforts of Pugh, Hickson, and Hinings (1969), who sorted fifty-two work organizations into a twelve-celled matrix ($3 \times 2 \times 2$; based on authority, structure, and control), did not produce a classification scheme that others were quick to accept.

This too was true of later schema that reflected additional dimensions like environment (Jurkovich 1974) or organizations with tasks that others had not included, like libraries (Lynch 1974). Among the most complex of these was formulated by Warriner (1979), who included ten broad categories of characteristics ranging from "origin and life history" to "cultural" and "participant." Indeed, sensing the complexity of the problem, he included an eleventh category entitled "other" that would "encompass other characters that may be suggested."

All of these efforts suffered from inadequate data bases, which reflected

insufficient sample sizes, etc.
biguities. Thus, when Warriner called for a collective effort through an American Sociological Association panel, several responded (see Warriner 1979 and Warriner, Hall and McKelvey 1981). But funding to pursue such basic research has remained difficult to secure.

McKelvey's (1975, 1979, 1982) reflections on the problem are the most thorough I have encountered and I would commend his text (1982) to any in the disaster research community who are addressing the taxonomy problem or are involved in less ambiguous comparative work. Drawing on population ecology theorists (e.g., Hannan and Freeman 1977; H. Aldrich 1979; see also McKelvey and Aldrich 1983) and concepts like natural selection that many like Weick (1969; 1981) have used within an evolutionary theory model, McKelvey (1982, 445) argues that "A set of organizations facing similar environmental pressures tend to acquire similar aspects of form and come to form a homogeneous population." Populations of organizations comprise McKelvey's units of analysis. As will be noted below, this contrasts to Kreps's (1985a, 1985b) approach or that of others, who choose a developmental perspective that is member-unit-based.

Implicit Issues in Taxonomies of Social Organization

1. *Is there a need for an a priori theory?* Haas, Hall, and Johnson (1966) departed from the traditions of their era by going beyond both the deductive approaches to typological formulation that then prevailed and excessive reliance on intense case studies of single organizations. They argued that larger data bases comprised of information about organizations with differing tasks could be scrutinized without regard to theoretical trappings. Clusters of structural features were sought so that—by analogy—bats, whales, and humans might be grouped together to form a taxonomic niche. More recently Warriner (1979) has advocated a somewhat similar position, perhaps also in response to the excessive domination played by organizational goals or tasks as the basis for classification.

McKelvey (1982, 48) concludes that an a priori theory is essential and he proposes that the population perspective should be it (see especially pp. 450–51). As yet, however, this perspective remains just that; it falls short of providing the requisite specifications. Without drifting into the debates about the feasibility of formal theories of human systems, I suggest that the population ecology perspective serves as a powerful metaphor (e.g., see Morgan 1980; 1983a; 1983b, and Pinder and Bourgeois 1982). But clearly in its present-day versions it lacks the precision required of a formal theory. So do we need a theory of disaster? A theory of complex organizations or families? And how are the disaster-related themes to be related to those of social structure?

2. *What is an organization?* This question could be rephrased so as to include any one of the structural systems I have discussed, be it families, communities, or whatever. The point is that if any form of comparative analysis is to occur, we must delineate the object of study. Thus, when asked "to what can we generalize your findings?" we must clearly answer in terms of our units of analysis. And some researchers are asking questions wherein events are the units, whereas others are focused on individuals, organizations, and so on. In considering the matter of taxonomy, we must ask taxonomy of what? And, as the stream of work within the complex organizations area demonstrates so clearly, not all features are included by every researcher. They have differing, albeit usually implicit, views of the phenomena under study.

3. *Is a "core-species" concept required?* McKelvey attacked the definitional issue regarding organizational systems by arguing in favor of a "core-species" concept. He proposed that the primary task and workplace-management competencies be blended into a single concept that can serve this function—what he labeled "dominant competence" (McKelvey 1982, 189–95, 215–18). Seven elements comprise the "dominant competence" of organizations that parallel rather critical system concepts advanced by Miller (1978), Buckley (1967), and Katz and Kahn (1978). For example, unit operation parallels "throughputs," measurement of effectiveness corresponds with "importation of energy," control systems resemble "homeostasis," and so on. The other four elements are workplace interdependencies, adaptive systems, internal differentiation, and vertical and horizontal layering (McKelvey 1982, 193). This listing resembles the types of structural features proposed by many organizational theorists.

But McKelvey (1982, 218) proposes that a "fundamental taxonomic unit is defined as an organizational form in which the activities of technologically interdependent subsystems are pooled toward the accomplishment of the primary task." Although some "special cases" may be confronted that may require "additional species concepts," he concludes "that the dominant competence concept will apply to the mainstream of organizational forms . . ." (p. 218). As will be discussed below, Kreps (1985a, 1985b) has approached this problem through a developmental perspective, in contrast to an evolutionary orientation, and uses the concept of domain to identify member units and specify their boundaries.

4. *What criteria define organizational boundaries?* As we discovered a few years ago when we surveyed the organizational literature to ascertain what sample sizes were used, investigators vary widely in the criteria they use to establish boundaries (Drabek et al. 1982). But any who pursue the problem of taxonomy very rigorously will confront this fundamental problem. The disaster research community could be assisted by the analyses of this problem that organizational researchers have produced (e.g., Haas and Drabek 1973, 15–17; Miller, Anderton, and Conaty 1985).

5. *How are system termination and emergence to be conceptualized?* Whether or not one adopts an evolutionary or developmental perspective, both system death and birth must be reckoned with in some way. As Kreps's (1985b) work illustrates so well, certain aspects of many, if not most, disaster responses pertain to the birth of newly formed systems that dissipate within a relatively short period of time. While a multitude of research questions can be pursued that do not involve such matters, certain topics or functional problems require them. Some of these such as search and rescue (Drabek et al. 1981), body handling (Hershiser and Quarantelli 1976; Blanshan 1977), or other areas requiring interagency coordination (Drabek 1985) comprise critical areas of research relevant to disaster management and policy.

Thus we are left with a series of unresolved issues by those who have explored the problems inherent in the development of organizational taxonomies. But their work indicates the type of careful and detailed analysis that must be completed if we are to attain the promise intrinsic in the disaster research legacy.

Putting the Two Together

While at a very preliminary stage of development some disaster researchers have tried to bridge the gap. That is, they have juxtaposed the taxonomic issues related to both disaster events and forms of social organization. Three of these efforts strike me as being especially useful. Following brief comment on an evolving typology that informed numerous studies conducted by persons associated with the Disaster Research center while it was located at Ohio State University (1963–84) and the more recent innovative work of Britton (1983; 1984a; 1984b) and Kreps (1985b), I will summarize my own formulation and specify some future directions that look promising to me.

Although it appeared earlier, Dynes (1970, 138) described in detail the logic of a typology that clustered into four categories the enormous range of organized behaviors observed after disasters. Organizational tasks were categorized as either regular or nonregular. Structures used to accomplish these tasks were designated as old or new—that is, functioning before a particular disaster event or implemented after its occurrence. Crosstabulation of these two dimensions, with their respective twofold breaks, produced four analytic categories or organizational forms: (1) established (regular tasks, old structures); (2) expanding (regular, new); (3) extending (nonregular, old); and (4) emergent (nonregular, new). These distinctions proved to be fruitful for theoretical integrations (e.g., see Weller and Quarantelli 1973, 676, and Stallings 1978), advice to practitioners regarding disaster planning (e.g., see Dynes, Quarantelli, and Kreps 1972), and analyses of volunteer activities (Dynes and Quarantelli 1980).

Most recently, Quarantelli (1984a) has proposed an elaboration; he indicated that additional DRC work has confirmed earlier suggestions made by both Bardo (1978) and Stallings (1978). Each argued that the DRC typology was too limited. This modification, however, does not resolve at least three kinds of issues implicit in the above discussion, although Quarantelli has discussed several of them (1984a, 22). First is the key point made by Bardo (1978, 90) regarding planning. Many aspects of emergent structures are guided by disaster plans that represent a "latent" structure—that is, one expected under specified conditions. In my judgment this issue will be better resolved by treating planning as one of several structuring factors, rather than using it as a taxonomic criterion for grouping emergent systems. Properties of the behavior systems should be kept distinct from the qualities that may mold or explain their developmental course. In short, we must differentiate consistently between social structure and the cultural structure (Wallace 1983, 29–33, 54–132).

Second is the matter of structural complexity. Nowhere does the DRC typology differentiate among emergent systems with varying degrees of structural complexity. Of course, as Kreps's (1985b) work demonstrates so clearly, many emergent systems are comprised of individuals who are affiliated with established disaster-relevant organizations, such as police or fire department. The emergent system in which they are participating, however, can be analytically differentiated from these ongoing organizations. Frequently, however, as my research on emergent multiorganizational systems that were focused on search-and-rescue activities illustrates, such individuals are performing boundary-spanning functions so as to lace requisite resources from numerous autonomous units into a more integrated whole (Drabek 1983a). Of course, depending upon the definitions of terms, such behaviors may or may not be viewed as comprising organizations or merit being selected as units for analysis.

Third, given its origins as a tool for classifying organized behavior during the emergency period, the typology has not been applied to the other phases of disaster. Hence it has directed researchers towards the emergency phase. This implicitly discourages investigation of potential human and institutional culpability in causing many disasters or increasing risk levels for limited segments of a community population (Hewitt 1983; Perrow 1984; Drabek 1987).

Desite these ambiguities, the DRC typology has proved useful for guiding many research studies. For example, Stallings (1978) integrated several organizational features such as technology and task structure with the typology so as to specify an entire series of analytic propositions. Similarly, case materials from a November 1977 mudslide in Tuve (a suburb ten kilometers north of Gothenburg, Sweden) were ordered by Syren (1981). He discovered that the DRC typology provided a useful classification device

that specified both the ... tasks performed by the responding organ...

Most recently, Britton (1983; 1984a; 1984b) used it as a jumping off point to assess emergency response effectiveness among Australian organizations. Among a collection of disaster-relevant organizations, he examined nine qualities: the legislative base, the power base; resource allocation; organizational autonomy; organizational domain; recruitment of senior staff; and organizational legitimation (1984b, 214). Using these criteria, and recognizing the inherently interorganizational quality of emergency responses (Drabek 1985), he tried to assess two more abstract dimensions: potential to influence direction of organizational network and ability to determine organizational role. Crosstabulation produced four organizational types: (1) cardinal organizations (high, high); (2) conditional organizations (low on influence and high on ability to determine role); (3) controlled organizations (high on influence and low on role determination); and (4) constrained organizations (low, low). (See Britton 1984a, 136.)

Similar to Mayhew (1980, 1981) or other structurally oriented theorists, Britton was able to pinpoint system features that had greatly impacted the relative effectiveness of certain agencies, especially the state emergency services offices during the widespread wildfires that occurred in Tasmania (February 1982) and Victoria (February 1983). Responders within "constrained" organizations were unable to perform effectively. The barriers were not individual defects traceable to training or commitment, however. They were rooted in the structural qualities that defined their niche within the emergent interorganizational response network.

In contrast to these organizationally based approaches to emergent post-disaster responses systems, Kreps (1987, 1985b) has gone back to the basics. By asking obvious questions and finding that the answers are not there, he has challenged us to identify and examine what compises the core of many disaster responses. He has focused on the emergent, cross-agency, short-lived interactional systems that would be observed if one were perched in a helicopter above a tornado-ravaged community. In short, he has asked "What is a response?", "What is an organization?", and "What is a social unit?" (Kreps 1985b, 97). Using four elements of organization (domains, tasks, resources, and activities) to conceptualize sixty-four forms of association, Kreps has brought us from Simmel (1955) to Collins (1981a), Warriner (1979), and McKelvey (1982). He writes, "What has been developed in this research amounts to a 'core species' notion of social structure (the four elements), a derived taxonomy of structural forms (the 64 forms of association), and a distinction between organization (24 forms) and other things structural (40 forms)" (Kreps 1985b, 14).

By conceptualizing disaster events as social catalysts, Kreps made these

emergent systems or organizational forms his units of analysis. He thereby recognized and made explicit the distinction between the rationality of the actors and the degree of rationality that characterized the process or temporal sequence whereby the unit emerged. Once freed from the trappings of an overly rationalistic interpretation of the reported behavior—yet not impuning the motives or competencies of the individuals whose behavioral actions constituted these micro units of disaster response—he was able to identify populations of emergent systems. Once defined, he tried to describe the life histories of member units, including their origins and maintainence and death processes. And of extreme importance for practitioners and policy-makers, he added an important developmental counterpart to evolutionary theorists like Weick (1981), who had noted earlier that the processes of organizing do not always show the sequential patterning implicit in rationalistic interpretations. Paralleling organizational theorists who have repeatedly pointed it out, goal statements or conceptions of domain may be created to defend already occupied turf and previously made organizational decisions, rather than being the impetus for either (Drabek and Chapman 1973; Drabek and Haas 1974).

Kreps (1985b) did not find all twenty-four forms in equal numbers; the most rationalistic pattern (*D-T-R-A*) comprised only 39 percent of whose birth was recorded within the archival records from the fifteen disasters studied (i.e., 167 out of 423). This analytic work should prove to be as insightful—both theoretically and administratively—as did the earlier qualitative descriptive analyses of such emergent systems like Zurcher's (1968) classic portrayal of "monsterman" and the other members of the short-lived work crew that was born following the 1966 Topeka tornado.

Three Additional Directions

Finally, I want to introduce three separate approaches to this "putting-together" problem that I have explored. First, and I think consistent with aspects of Wallace's (1983, 182–84) discussions regarding system complexity, I have proposed that the DRC typology be nested within a more complex analytic matrix so as to define an elementary universe of systems (Drabek 1987). For example, when I completed a review of several dozen studies, however, I was pleased that some of them dealt with disaster phases beyond the emergency period. Several had to be sorted out by differing referent systems, however. That is, criteria had to be articulated that differentiated between structural adaptations in ongoing and continuing systems; newly created structures that emerged within such systems; emergent systems that appeared to span across the boundaries of several previously established systems; and newly created systems that emerged outside of and independent of any ongoing structure.

I found it helpful to summarize research results by classifying the studies into a typology wherein the previously discussed DRC scheme was nested within each of the analytic cells produced when three dimensions were crosstabulated: (1) disaster phase, (2) permanence, and (3) structural complexity (Drabek 1987).

This typology reflects the second direction in thinking that I want to introduce. I always found it helpful to recall that my introduction to this literature was Fritz's summary that appeared in the social problem text edited by Merton and Nisbet (1961). As so, over the years, despite a focus on very specialized topics, I have believed that one form of integration we should seek would be within theories of social problems (Drabek 1981).

Problem identification—be the problem posttornado response or concerns about a long-term drought—is an initial point of departure. Clarke noted in his critique of psychologically oriented research on risk assessment that

> . . . it is difficult to interpret the meaning of the data. They can be legitimately interpreted to indicate that the social problem of risk assessment is one of inadequate information and undue influence on the public from the mass media. Charles Perrow (1984), on the other hand, interprets these same data as evidence that the public accurately, albeit unwittingly, assesses the risks of technologies with catastrophic potential. (Clarke 1985, 5)

And advancing the need for a social system perspective—as I have done for family responses to disaster warnings (1969) and search-and-rescue activities (1985)—Clarke underscored the range of issues that interface with those stemming from more easily delineated "events." More easily delineated, that is, until we ask, say from Hewitt's perspective, "when does a flood become a flood?" Thus, as Kreps's work illustrates, there are emergent systems that are born and typically die within a relatively short time span. And these do comprise legitimate objects of study; they are appropriate units of analysis. But they are not the only ones that merit our attention. When a social problems perspective is taken, our research agenda becomes stretched and we begin to recast rather time-bound disaster events into a broader context, a context of ongoing social processes whereby some individuals enter into locations of greater risk—at times knowingly and voluntarily, at times unwittingly.

In the recovery period, this social problems perspective enriches the insights provided by Cuthbertson and Nigg (1984). They proposed that certain aspects of postdisaster therapeutic communities may be altered significantly when events are defined as technological in origin, as opposed to attributed to nonhuman causes. Hence, as with long-term disaster impacts (e.g., Erikson 1976), the definition of the disaster event by those victimized, or potentially victimized, may affect the initiation, speed, and

form of emergence. For as is known from analyses of other social problems, discontent does not lead inherently to action.

Such social problem processes become clarified as we peer into the structural mechanisms that radicalized Lois Gibbs—the heroine, at least by the values of some—when she asked if the residents of Love Canal were in danger because of a series of decisions made by executives in several organizations over a long period of time (Levine 1982).

As R. Turner (1964) and later Walsh (1981) have emphasized, the processes whereby the inertia of inaction results in action constitutes the initial theoretical problem in the study of emergent systems. This can be pursued initially through case studies. Examples include Neal's (1984) analysis of an emergent citizen group that arose because of heightened awareness and concern about the potential health effects of air pollution stemming from the burning of polychlorinated biphenyls (PCBs). Similarly, Walsh (1981) and Walsh and Warland (1983) have dissected the processes that formed social collectives to resist future use of the nuclear power plants at Three Mile Island.

When this work is juxtaposed with that of Soderstrom and his colleagues (1984) we can see an appropriate role for social analysts. That is, we ought to sample the entire range of such emergent systems, not just select those reflecting a particular political ideology. There are, as Soderstrom and his colleagues (1984) documented, organized units that favored the restart of the undamaged reactor at Three Mile Island.

Not unlike what happens when we add drought to the scope of disaster events to be classified, this social problems perspective forces us to recognize a broader series of processes that did not just start shortly before the first victims were warned. But the defining criteria that may best guide comparison—the walls of our taxonomic cells, so to speak—may not be constant across all of the life cycle phases. Also, a single set of criteria may not be useful for all comparative work, especially among studies focused on different points in the life history of disasters. Yet if we are to bound the range of generalization for any of the principles that may emerge from our research, we must have a classification scheme. For as Wallace (1983, 394) stated, "We define not a single, unique, event but a type or class of event—a class that includes an unknown number of individual, unique, instances."

When we turn to this phase from a social problems perspective, however, we see another type of linkage that allows us to transcend a narrow "disaster research" constraint. The survey of attitudes toward mitigation alternatives that P. Rossi and his colleagues (1982) completed is a good example. They documented that public policy options are viewed differently: "We find elities representing the real estate and land development sectors to be consistently most opposed to nonstructural approaches and to strongly favor more traditional policies" (p. 13). And more important,

policy preferences among local officials are constrained by their perception of the positions held by community elites.

While different in focus, Frey's (1983) work also illustrates the point well. He shifted abstraction levels and juxtaposed the propensity of communities to adopt policy approaches to two different types of social problems, the National Flood Insurance Program versus the War on Poverty. His sample of fifty cities supported his interpretation that "if there is a match between attributes of a policy and those of a collectivity, then the probability increases that the policy will be adopted by the collectivity" (Frey 1983, 59).

Similarly, our case study of Missouri revealed a sequence of events wherein a federal agency tried to get the city of Saint Louis to adopt a more stringent policy regarding building codes in seismic zones (Drabek, Mushkatel, and Kilijanek 1983, 93–105). The failure of the federal agency to have its proposed policy implemented was consistent with the hypothesis proposed by Olson and Nilson (1982) in which they linked policy type to types of politics:

> ... different types of policy (distributive, constituent, regulative, redistributive) have different types of politics (participatory, specialist, pluralist, elitist), and therefore, there will be different political strategies appropriate to each type of policy proposal. (Olson and Nilson 1982, 89)

In short, this social problems orientation challenges us to examine parallel social mechanisms that may operate across mixes of policy formulation and implementation processes for both disaster and nondisaster-related phenomena.

A third approach that brings a complementary range of research questions that I believe will prove to be useful is the stress-strain perspective. Recall that this orientation was noted early in my discussion of impact ratios that P. Rossi and his colleagues (1978) used in addressing the threshold problem. The stress-strain perspective permits a more general and complex approach than what is implied in the notion of impact ratios, however. I began developing aspects of this while a graduate student (e.g., Drabek 1965) and have elaborated on the themes over the years (Haas and Drabek 1970, 1973; Drabek et al. 1981). Most recently, Warheit (1985) advanced a series of propositions and overall conceptual framework that added much precision to this perspective.

Furthermore, it is worth noting Quarantelli's (1985, 43–52) conclusions after he reviewed several conceptualizations of disaster; "the definition of disasters as crisis occasions in which demands exceed capabilities seems to this author the most useful conceptualization presently available" (p. 51). Hence, he proposed that this definition "emphasizes behavioral response rather than whatever may generate that response" and

> provides some narrowing limits by its focus on a social occasion of a consensus nature. It thus excludes conflict situations, be they the result of war, terrorism, civil disorders, or other specifically human-generated and -maintained situations. Further, the focus on a crisis occasion precludes equating disasters with "collective stress situations." Since the latter could be quite appropriate descriptions of, say, some metropolitan hospital emergency rooms on a Friday night or some football teams on a Saturday afternoon, it is important to distinguish them from occasions likely to be termed "disasters." (Quarantelli 1985, 51)

As is implicit in my comments above, my inclination is to go a somewhat different route. But this is because I am less interested in a particular definition of *disaster* than in a broad framework wherein research might be designed, executed, and used, with the limits of generalization specified more adequately than we have done to date. Thus I suspect that the disaster phase specification, both within and across phases, will serve our purposes. We then should seek to identify, and test for their relative impact, qualities of demand. These may be actual or threatened. Risk expectations act to translate into more specific demand expectations for social units whose members are seeking to legitimate differing domains.

These criteria may be both general, to define universes of events for comparative purposes, and unit-specific, so as to identify populations of systems for which they may be relevant. Additionally, both general structural qualities (e.g., size, administrative control structure) and more precise demand-relevant capabilities must be identified for the social systems under study, be they communities, organizations, families, and so on. And finally, a series of responses or differential patterns of coping may be identified that may be associated with variable levels of probability as to their emergence.

For given populations of events—including the more vaguely defined potential hazards like toxic dumps—we ought to seek to identify differential patterns of coping among structural systems that share common taxonomic criteria. And two criteria that may be especially useful are the stress level and distribution of strain at varied points in time.

In this way, the more commonsensical and narrower notion of *disaster* defines a short-term historic episode for particular social systems and serves as a breeding ground for a limited set of emergent ones, most of which will be equally short-lived. From this commonsense perspective, the term *disaster* refers to events that stress community level (or more complex) systems, or significant portions of them. But this stress-strain framework provides a mechanism for a much broader set of integrations. Disasters, as such, then become but one class of events, across the whole gamut of potential ways in which social units of varying degrees of complexity may experience intensified demand/capacity imbalances.

Implications for Emergency Management Practitioners

To most people with operational responsibilities, the above remarks may have little utility. When one is confronted with the day-to-day turf defenses, budget assaults, report-filing, and other activities that are required of local or state emergency management directors, notions of "core species" or "demand/capacity relationship" seem very esoteric. But we know they are not. Yet how can we articulate the necessity of research on these issues in order to enlist the cooperation and support of emergency managers? What are the payoffs for those who must respond today? Let me briefly point to five areas of contribution.

1. *Generalization—error avoidance and attack.* As Petak (1984) and I (1985) have documented, emergency management is a rapidly emerging profession. And as Moore (1970), Dingwall and Lewis (1983), and others who have studied the evolution of various occupational groups undergoing such a transformation would point out, the codification of a knowledge base is a critical activity. Currently, many practitioners are digging into research studies provided by researchers across a wide array of disciplines. Many have learned of computerized information retrieval systems such as those Rogers and Nehnevajsa (1984) have created or the specialized libraries that exist at the University of Delaware or the University of Colorado.

Whenever and wherever possible, we must help them to be sensitive to and better understand the issue of generalizability. For while our knowledge base has expanded exponentially during the past decade, the range of generalization appropriate for most conclusions has not been specified. We must assist the members of this emerging profession to understand how to avoid the error of overgeneralization. And we can help them further by giving them a basis for attack when they encounter others who have done just that—be they staff, politicians, or citizens.

2. *Disaster—a low-priority, nonroutine social problem.* As emerging professionals, emergency managers can be helped to develop a sense of perspective and basis for interpretation by expanding upon a social problems framework. When researchers like P. Rossi and his colleagues (1982) point out that state and local decision-makers are more concerned with pornography than they are with natural hazards, the issues of problem identification should be brought forward both for understanding and professional intervention. Similarly, the crosscutting tensions among various sets of stake holders ranging from developers to powerful manufacturing firms and utilities to emergent citizens groups frightened by the presence of a toxic dump should become part of a capacity to conceptualize communities and societies as arenas of conflict (Drabek 1984; Hamilton 1985).

Federal policy shifts do impact state and local programs, although

various buffering mechanisms can neutralize the impacts (Mushkatel and Weschler 1985; Drabek, Mushkatel, and Kilijanek 1983). But as the example of crisis-relocation planning (CRP) illustrated so well, local programs can become defined as instruments of war. Those local directors, who cannot conceptualize the broader structural patterns that are operating, may be caught with little understanding of the potential consequences for their bases of support. The taxonomic problem forces the broadest assessment of the social dimensions of the threat of nuclear war, not just as a potential "event" to which other findings may or may not generalize, but as a social problem reflecting parallel processes that characterize other "national crises," be them crime, unemployment, or family instability.

To date, however, sociologists have rarely examined the nuclear threat and only recently have we seen "a call for sociological theory and research" (Kramer and Marullo 1985, 277). It is telling that a survey of three mainline sociology journals—*American Sociological Review*, *American Journal of Sociology*, and *Social Forces*—uncovered only six articles out of 6,500 that dealt with nuclear weapons in any way. And only fifteen out of 1,000 published in *Social Problems* dealt with these themes (Kramer and Marullo 1985, 283).

Of course, not all researchers who have conducted disaster studies would appreciate the conclusion that even more disciplined analysts like Kramer and Marullo presented, such as "research on survivors of disasters, for example, demonstrates the debilitating collective depression, the loss of community identity, and the shattering of social fabric . . ." (Kramer and Marullo 1985, 285). Thus, as with risk analyses and mental health impacts, the nuclear war example is fraught with theoretical ambiguities that make it easier to politicize. While that process is to be expected among all, including the social science research community, it is a risk and challenge that must be accepted and dealt with more effectively.

Finally, as Wolensky (1984, 214) pointed out so effectively, local governments may be criticized sharply during the postdisaster period, especially if the event had not been anticipated. This does not excuse inadequate planning, but the fact remains that local governments in most communities are not organized to respond to large-scale nonroutine events. As Wright and Rossi (1981b) put it, there is a "relativity of level," and most local governments will continue for several years without a disaster. Indeed, decades may zip by before disaster hits. Thus, trying to maintain a constituency and the bases for generating it can be recast into a broader social problems framework. (See Kreps 1985b, 232–37.)

3. *Improvisation—why try to plan?* Quarantelli (1984b), Dynes (1983), Kreps (1985b), and I (1985) have critiqued the bureaucratic planning model that remains the primary tool for many local communities. As nonroutine problems, even those disasters that can be anticipated with some

degree of regularity and predictability, such as certain types of flooding and hurricanes, require a "human resources" approach to planning. That is, preexisting patterns of authority preclude an emergent decision-making authority that is highly centralized. Multiagency responses constitute the core of the planning problem, just as they constitute the core of a community's adaptation to the threat of disaster. These responses require coordination through long-term team-building activities such as simulation exercises, not managerial strategies routed in autocratic or dictatorial images of "command and control." Or as Bosworth and Kreps (1986, 712) put it:

> This finding supports the idea that preparedness has dual value. First, it increases clarity about what to do early in the process. Second, it is a resource for flexibility as organization unfolds. In the latter sense, preparedness operates as tacit cultural advantage, in a manner similar to disaster experience. In Weberian terms, perhaps preparedness supports the requirements of both formal and substantive rationality.

Taxonomies of events can help practitioners to better identify the varied populations of simulation exercises they need to select. These taxonomies and exercises should reflect increased degrees of demand complexity so as to tax, but not overwhelm, their institutional response capacity—one that can only be built over time. Always, I suspect, there will be a need to improvise—no disaster can be anticipated totally in all of its demand components. But as team-building exercises increase the interagency response capacity, simulations can be constructed that reflect more extreme events. These can provide rehearsal for more imporvised responses. Critiques and self-study can precipitate the design of the institutionalized structures that will promote, rather than retard, the improvisations that will be required should events with certain demand characteristics actually occur. (See Kreps 1985b, 220–24.)

4. *Functional variation—toward a taxonomy of domains.* In contrast to the degree of routinization, or predictability and regularity, within a universe of events, functional qualities need to be differentiated through future taxonomic work. Clarification, or I suspect creation in some cases, is required regarding the identification of the "appropriate" cluster of agencies that will respond to a toxic spill in a railroad yard versus a terrorist act in a local airport versus an airplane crash in a densely populated sector of a city versus an earthquake versus a chemically contaminated schoolyard built above a former dump. Kreps's (1985a, 58) listing of twenty-nine disaster domains that were performed by the 423 member units he analyzed is an excellent illustration of the type of elaboration needed. Taxonomy work could better inform the construction of planning annexes. These annexes are being developed in many communities todays as the policy of Inte-

grated Emergency Management Systems (IEMS) is implemented (McLoughlin 1985). Unfortunately we have not provided the necessary administrative tools, nor have we tackled adequately the conceptual problems inherent in the task.

Similarly we have not designed an adequate "Disaster Magnitude Scale," although the demands analysis strategy that was implicit in my comments above contains the seeds of the idea. Many local managers I have talked to are trying to do this—always in an off-hour on Saturday morning, it seems. They need help in trying to conceptualize the range of capacities that will be needed for responses of varied configurations. This is true for specific functional tasks like evacuation or warning, as well as overall coordination of the immediate postimpact response.

5. *Administrative versus substantive rationality—a dialectical relationship?* After backing away from his "*D-R-A-T*" taxonomy, Kreps (1985b, 189–205, 224–25) has wrestled with a more abstract interpretation. His data patterns appear to reinforce a dialectic that may prove to be very useful to those struggling within the emerging profession of emergency management. With respect to response planning by local communities, as well as the actual responses to various events, Kreps's data show that there are many ways to skin a cat, so to speak. Or more formally, "we have learned that there are alternative but perhaps not unlimited ways to meet the demands of disaster" (Kreps 1985b, 204).

By juxtaposing "substantive" (i.e., "things get done more effectively in the sense that the unit is more adaptive to changing demands," Kreps 1985b, 224) and "administrative" rationality (i.e., "ensures that things get done more efficiently in the sense that activities are more predictable and occur more rapidly," Kreps 1985b, 224), Kreps helps us reflect on his data patterns and their broader meaning for taxonomy and the problem of response effectiveness. He concludes that both ought be desired goals, both are needed. Yet he also says, "the more severe the disaster, the more important does their balancing become" (Kreps 1985b, 225).

Space precludes detailed examination of this thesis, one that I find extremely important both theoretically and practically. But I want to propose that the implicit issue carries us one more step. That is, we ought to examine the effectiveness-efficiency issue beyond the confines of responses to events. It needs to be placed into a broader theory of disasters that is rooted in a social problems context. Disasters are but one source of societal stress among a host of others that threaten the quality of life in any society. And resource allocations toward improved capacities for disaster event responses may be less rational—depending upon the structural location wherein the assessment is made, of course—than investments in mitigation actions of various sorts or stress reduction interventions that are directed toward totally different social problems. Yet, analogous to those who wish

to accept the risks of rock climbing or crosscountry skiing, the joys of the beach may render hurricane-based zoning proposals asinine.

In short, as with the issue of safety or the analysis of other social problems, we return to the normative. But through more careful work on the taxonomic problem we can assist emergency management professionals to better grasp the fact of the value judgments they must make. And equally important, they can better understand why it is that these judgments will conflict with those made by other managers that are examining the rates of exchange in different currencies from different platforms in the social structure.

Epilogue

I have argued that the term *disaster* is best conceptualized not as a single entity. In the past we have been like physicians talking about disease; much greater levels of specificity are required. What may best serve the immediate needs of practitioners—and researchers too—are a variety of fairly simple taxonomies keyed to specific problems. But even these must reflect abstract social characteristics for both event qualities and the referent system. As we cut across the phases or life cycle of disasters—as we must—the relevent criteria may vary in content and degree of importance. By placing disaster responses in the broadest sense of that term so as to include mitigation activities, we can conceptualize the field of study as a special type of social problem. We may thereby contribute to but simultaneously critique the logics used by those engaged in the new profession of emergency management (Petak 1984).

Of course, any of us may wish to propose particular normative solutions. My plea is that when we do so, we not try to hide or disguise our actions, especially from ourselves. Disciplined taxonomically oriented analysis may provide an important safeguard from such errors in method. Furthermore, such analyses will better enable us to integrate and cumulate the works of colleagues. Only in this way can we make good on the promise and potential that resides within the social sciences.