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**DRAFT OF A
SOCIOLOGICAL DISASTER RESEARCH AGENDA
FOR THE FUTURE:
THEORETICAL, METHODOLOGICAL AND EMPIRICAL ISSUES***

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Introduction

Systematic and extensive social science disaster research has been going on for nearly five decades now. Much worthwhile work has been done. A very large number of empirical generalizations have been produced. Yet, I am troubled. In my view, the field more and more, is producing less and less, of what might be characterized as major advances in new knowledge and understanding of disaster related phenomena. Certainly one would be hard pressed to point to the production of new theories, models, explanatory schemes and/or master hypotheses about the phenomena that are notably different from what have been around for some time. In fact and worst, both relatively and absolutely, little of the literature advances explicit theories, models, explanatory schemes and/or master hypotheses of any kind. A partially completed informal content analysis of journals that publish disaster research articles in the social sciences support this contention.

We think this is because present day studies, as a whole, are not that much different from those undertaken these last forty five years. If so, what does that suggest? To us, a clear implication is that there is a great need to ask new and different research questions. We need a radical shaking up of how we ought to go about studying the social aspects of disasters. As has been said generally, major scientific advances require major rethinking, not just more studies. We must think through what we should do that is different in fundamental ways from what we are doing.

Now this paper only makes a few suggestions on issues that we should address to get the field out of its current intellectual rut. In no way is a completely new research agenda for the future presented. However, we do present examples of theoretical, methodological and empirical issues that if dealt with, would force research to be different in major ways than it is being presently conducted.

We make no claim that absolutely no one is doing any of the things we suggest--there are scholars here and there generally addressing some matters that we discuss in this paper. In fact, some earlier presentations at this meeting of the Research Committee on Disasters are in my view going in the right direction, although I will forego at this time any invidious indication of who the "right thinkers" are. However, the field as a whole is not going in the direction we think would be most profitable for it.

Our Intended Audience

We are not writing for everyone. There are two ways in which this paper is not directed at everyone interested in the disaster area. For one, our intended audience is primarily researchers. As I have written extensively elsewhere the goals and procedures of researchers and research users (i.e., practitioners) are distinctively different (Quarantelli, 1993b). Actually, a failure to understand this is currently subverting the basic research in the area. It is not that applied questions are meaningless. They are not. However, asking and even answering them can do very little to advance basic knowledge and understanding per se of disaster phenomena (for an example of very good observations by an operational official, see the appendices in Roberts, 1994). Practical concerns have never been the primary engines for scientific advances in any area⁽¹⁾. So why should disaster

studies be different? There is a need for good social engineering, but let us not confuse it with social science.

In my widely ignored Presidential address (1987) to the Research Committee on Disasters at the World Congress eight years ago, I paraphrasing Benjamin Franklin, said we needed more astronomers--or to change the metaphor--sociological researchers to study the skies, the stars, the galaxies and the universes of disasters, rather than more carpenters helping to build better lifeboats for floods, better buildings for earthquakes, or better shelters for radiation fallouts. We need more disaster researchers to look up and dream, and not look down and do. We need more theory and abstract thinking and less mucking around in practical matters and concrete details. The heart of any scientific activity is basic knowledge and curiosity driven, and not concerned with immediate outcomes or products.

Along another line, my paper is mostly directed at sociologists and not even all social scientists. Essentially we are trying to present a sociological paper for sociologists doing sociological research. In my view, the Durkheimian position is the *raison d'etre* for sociology. That is, we are to identify and explain social facts by other social facts. Our view is that at least sociologists working in the disaster area should take this disciplinary principle quite seriously. After all, we have been shown the light, and do not have the excuse of those living in intellectual darkness. We will do better disaster research by being better sociologists.

To take a sociological approach, is not to denigrate other perspectives on disaster phenomena. In fact, increasingly disaster researchers in other fields such as geography (see Mitchell, 1994), public administration (see Rosenthal, Charles and 'd Hart, 1989) and especially anthropology (see Oliver-Smith, 1994), are bringing their own disciplinary perspectives to bear on their studies. This is very encouraging and good for the field of disaster research in general. But we should take care not to be left behind in pushing a distinctively sociological perspective on disasters.

I will ignore here also the siren call for interdisciplinary scientific research. First proposed in ancient Greece, it has not come into being anywhere in any viable form for more than 2,000 years. Certainly it does not exist in the present spectrum of the sciences. In the United States, post World War II failures of social science interdisciplinary departments at Harvard, Michigan, Columbia, Yale, etc. should tell us something. Why should one expect disaster studies to be in the lead on this when interdisciplinary research is not noticeable in contemporary social sciences? (As an aside, we may note that the issue of interdisciplinary application of research findings is a different matter, seldom noted, and which badly needs an exploration that we do not address in this paper).

A slightly stronger case can be made for multidisciplinary studies, although recent and current examples of such work in areas like the family and crime, are not notable for their scientific contributions (Again, we are talking of research and not application). At least multi as over against interdisciplinary research does not completely forego the advantages of looking at phenomena from a particularly disciplinary perspective. Overall, the issue is not a matter of maintaining territorial boundaries or making a claim for the supremacy of some disciplinary, explanatory approach. Rather, it is that a disciplinary perspective allows one to see much and brings with it a depth of understanding

that is otherwise not possible. The division of labor among the sciences, social ones included, exists because it is worthwhile and valid and not just because of the historical traditions of different disciplines or their intellectual conservatism⁽²⁾.

In a way I am arguing sociologists in the disaster area at least should be doing more and better sociological work than they as a whole have done so far. Unfortunately, a great deal of what sociologists (including myself) do in the disaster area is not sociology at all--in fact, it is sometimes very difficult to identify the work in any disciplinary terms since it lacks, at least explicitly, any of the assumptions, models, theories, hypotheses, concepts, linkages to the non-disaster literature, etc. that is the corpus of present day sociology or any other social science. Some of it is good journalism, some is good social history, some is good descriptive inventorying--all worthy endeavors, more of which is desirable. But such work is not sociology in intent, execution or end result even though I use the term "sociological" in a broad way as some of my later examples will exemplify. As such, if you take sociology seriously as a scientific enterprise, this should bother you if you are a sociologist.

To recommend radical innovation in future sociological studies of disasters is not to downgrade what has been accomplished so far. As already noted, much has been learned at an empirical level. For example, the Inventory put together by Drabek (1986), the edited volume by Dynes, De Marchi and Pelanda on the contributions of sociology to disaster research (1987), and the more recent volume edited by Dynes and Tierney (1994) well summarize and document the extensive empirical knowledge and understanding that has been acquired in the last four decades. By any criteria, we know a fair amount and as demonstrated in the indicated publications, some of what is known is even clearly very sociologically valid and something upon which we should build in our future research studies.

Actually I think we could make a case that sociologists in the disaster area have accomplished relatively more in their initial 40 plus years of work than what many other specialized areas have done over a similar time period. Perhaps there is some intellectual ethnocentrism in this statement, but it stems from my belief that disaster phenomena necessarily involve all the basic dimensions and processes of social life. It is after all an old saw in common sayings and philosophical musings that crises lay bare the essence of personal and social life. This more easily allows the obtaining of knowledge since anything learned tells us something. When you start at zero, you necessarily will learn much. But overall, this possible theme on the relatively fast pace of growth of general knowledge about the human and social aspects of disasters is also a central topic for another paper.

For the immediate future, we ought to learn from our own studies on how change in the area must generally occur. Thus, one minor theme in the disaster literature is that involved organizations frequently plan for and manage disastrous occasions based on what is most convenient for them and what they traditionally do.

We should apply this principle to ourselves. Many studies are undertaken in the ways they are, because they are most convenient for us and what we traditionally do. For example, as sociologists we know how to conduct population surveys, draw appropriate samples, etc. and we do that in

conventional and traditional ways. In fact, survey research methodology is one of the best validated areas in all of the social sciences. Yet if we who are interested in disasters stood back and looked, we should realize that the convenient and familiar way may not be always the most appropriate for what we are often studying. For instance, as researchers of disaster occasions, we have empirically established that individual or solo actors are not the key players in such phenomena as evacuation and search and rescue. Such behaviors involve instead household or family units and new informal emergent groups. Unfortunately much of the research on such topics draws samples of individual actors instead of the collective entities that we already know are involved. But of course surveying family units per se or drawing a sample of informal emergent groups is not convenient and not what we are used to doing, so we keep doing disaster research in traditional ways..

Theoretical Issues

Under theoretical issues we will discuss five matters: the paradigm of disaster that is currently used, the lack of conceptual clarity in the area, the failure to take the larger social context into account, the dysfunctional assumption implicit in most present theoretical views, and the ignoring of certain relevant basic theoretical frameworks.

1. The current paradigm.

At one level, I would say our basic paradigm in the disaster area is acceptable (although not unchallengeable). The current paradigm involves a number of interrelated notions, but two of the more fundamental ones are that:

- (1) disasters are inherently social phenomena, and
- (2) that the source of disasters is rooted in the social structure or social system.

Nevertheless, while we generally accept these notions, we do not seem to always take them as seriously as we should.

a. If we did, for example, with respect to the first, we would see all processes associated with disaster occasions as also inherently social. Thus, instead of talking about chronological time and geographic space, we should use the concepts of social time and social space in looking at the temporal and spatial aspects of disasters. I suggest our understanding, for example, of response to warnings and emergency time protective behavior as well as informal search and rescue activities would be considerably enhanced if we saw them in the framework of social time and social space.

Anyone interested in these notions can look at the ideas advanced more than a half century ago by Sorokin and Merton, and more recently the work, both theoretical and empirical, done by sociologists and social psychologists on the topic (see Gurvitch, 1964; Zerubavel, 1981; McGrath and Kelley, 1986; McGrath, 1988; Young and Schuller, 1988; Pronovost-Giles, 1989; Baker, 1993; Flaherty, 1993; Adam, 1995).

As an instance of what we might learn, there is a sociological analysis that suggests nighttime life and work has characteristics also found in frontier life, that is, social space and time at night is similar to

what exists in a frontier type community with the according manifestations of certain kinds of behavior . Actually the frontier notion that implies a degree of unstructuredness and much informal emergence is not a bad metaphor to apply to the crisis period of disasters. The general point of my example is that by using the concepts of social time and of social space we will be forced to think of disaster phenomena in somewhat different ways than we see them when using chronological time and geographical space.

As a somewhat related kind of example, Forrest (1993) used a sociology of time framework to explore how six coastal and inland communities acknowledged the first and second anniversary of Hurricane Hugo. He reports on how past events surrounding the disaster were reconstructed to have meaning and utility for the present. Our analyses of disaster recovery would be better informed if we took this general notion that it is not the passing of chronological time or the placement in geographic space that is crucial in the process, but that of social time and social space.

b. With respect to disasters being rooted in the social structure, we should note that what we "really" mean is that disasters are consequences of social change since structure is simply change analytically frozen by a scholar at a particular point in time. The notion of disasters as being inherently related to social change goes as far back as one of the very earliest theoretical articles on disasters ever written, the paper by Carr (1932), a work known by name to a fair number of disaster researchers but as far as I can tell has been unread by almost everyone (except for a few European disaster researchers with theoretical interests such as Dombrowsky, see 1995 and Gilbert, see 1995).

More important, it is of interest that most students of disasters in developing countries, these days almost automatically link disasters to the development process. The link to social change in that context is "obvious," but the great majority of disaster researchers come from developed countries so it is not that apparent to them (instead some try to make a link to "social problems that is more "obvious" in their social systems). If disasters are rooted in the social change processes of developing countries, is it not reasonable to assume they are also similarly embedded in the social dynamics of developed societies? I would urge all my colleagues interested in theoretical aspects of disasters to answer that question.

My overall point is that if we start out with a theoretical assumption that disasters are inherently rooted in social change, we will be far better able to explain, for instance, the sources and locuses of resistances to disaster mitigation measures, for instance, instead of looking at the psychological makeup or attitudes of realtors, community planners or policy makers. The social dynamics and processes of communities and societies are where we should seek answers. Unfortunately, the few who have ventured down this path, have sometime tended to reify social structure, a frequent but badly misleading approach.

At a more general and futuristic level, my feeling is that if we press the current paradigm to its fullest, we will be forced to a worthwhile paradigmatic shift. What this will involve is not totally clear to me at present. However, in earlier writings I suggested the value of incorporating into our view of disasters, the notions of genotype and phenotype as developed in the biological sciences (Quarantelli, 1987). Essentially making this distinction argues that less obvious or visible characteristics are far

more important than surface features. My prediction is that our eventual new paradigm will involve far more genotypical rather than the phenotypical features we now almost exclusively use.

2. The lack of conceptual clarity

This last point brings us to the lack of conceptual clarity in the disaster area. As some of you know, I have long argued that unless we get a better conceptual grasp of "disasters," there are going to continue to be serious problems in our data gathering and analysis. Thus, for example, much of the disagreement on the mental health effects of disasters, stems from different conceptions of "disasters" that various parties to the argument take. It has far less to do with empirical findings per se. Thus, the more there is the inclusion of conflictive types of crisis occasions in an analysis--such as war and terrorism--the more heterogeneous the social occasions looked at, the more likely negative consequences will be found. The larger and the more differentiated the social net used--and a concept in many ways is a word net--the greater the certainty that more non-positive features will be found.

We say this to highlight the point that definitional and conceptual issues are not side matters, but go to the very heart of what we will find in our studies.

As Robert Merton wrote a long time ago:

concepts constitute the definitions (or prescriptions) of what is to be observed; they are the variables between which empirical relationships are to be sought. . . it is. . . one function of conceptual clarification to make explicit the character of the data subsumed under a given concept. . . our conceptual language tends to fix our perceptions and, derivatively, our thought and behavior. The concept defines the situation, and the research worker responds accordingly . . . conceptual clarification . . . makes clear just what the research worker is doing when he deals with conceptualized data. He draws different consequences for empirical research as his conceptual apparatus changes (1945: 465, 466, 467)

Or as another sociologist has written:

concepts are categories that help to establish the origins and perimeters of activity. At best they can mirror only a part of reality. They abstract and encase representative selections from phenomena, help to organize the frame of reference, and represent description (Pittman, 1960: 34)

The concepts used are especially crucial for new scientific understandings. As Huff has written:

whether it be the discovery of oxygen, the perception of a new planet, or the positing of such constructs as the positron, the meson, or the neutrino, the history of natural science . . . repeatedly shows the central role played by concept formation. From this perspective,

theoretical innovation is heavily indebted to the postulating or "conjecturing" of novel relationships between "old facts" and new entities; stated differently, innovation is the result of discovering new ways to conceptually organize previously known but puzzling and inexplicable phenomena (Huff, 1973: 261).

Much could be said here. For instance, definitions and concepts are not matters of empirical determination. Both terms have to be independently identified apart from whatever conditions are seen as generating whatever the phenomena are. Similarly, effects or consequences of whatever the definition or concept encompasses, have to be separated out or otherwise the outcomes are true by definition or concept. Thus, in a recent volume on children and disaster, many of the formulations are such that disasters can only exist if there are widespread negative effects; this is so by definition (see, Saylor, 1993). Conceptually we need to independently keep separate the conditions for something, from the characteristics of that something, from the consequences of those characteristics. Those three aspects are often badly intermingled in much current disaster research.

However, of all the conceptual issues possible let me single out three.

a) First, we are trying to use one concept, that is, "disaster" to attempt capture too much. For instance, elsewhere, we have suggested conceptualizing "disasters" and "catastrophes" as two different phenomena since there are both qualitative and quantitative behavioral differences between the two. At least four differential dimensions are involved. In a catastrophe, most/all of the total residential community is impacted, for example, making it impossible for the homeless to go to friends and relatives who are in a similar situation. Likewise, most of the facilities and operational bases of emergency organizations are themselves impacted. Also, local officials are unable to undertake their usual work roles not only in the emergency period but also into the recovery period. Finally, most of the everyday community functions are sharply and simultaneously interrupted across-the-board. In disasters these four features do not clearly appear. In the United States, it was the presence of these features that distinguished the social effects of Hurricane Andrew from other recent hurricane impacts, and in Japan, what separated out the Great Hanshin earthquake (popularly called the Kobe one) from most other earthquake effects in that country. The two named would best be viewed as catastrophes, the others as disasters.

If my view is correct, we should stop trying to squeeze relatively heterogeneous phenomena under one label. That would improve not only our theoretical understanding of disaster phenomena, but create knowledge useful for planning and managing purposes. As an example, if victims cannot go to friends and relatives in a catastrophe as they typically do in a disaster, there are different operational implications.

b. Second, we should question why we mostly conceptualize disasters as primarily focused occasions, both in terms of time and space (in chronological and geographic terms). I grant that some of us are uneasy in not thinking of the social effects of famines and droughts, for instance, as disasters. Yet since these happenings do not occur in traditional form in developed societies, many of us can blithely ignore them in our theoretical musings, justified to an extent because the phenomena

are not in the sights of our research sites. However, it is no accident at this very meeting of the World Congress that there is another group, the Sociology of Famine one, which has an existence independent of our Research Committee on Disasters (and there is almost no link or overlap of any kind between the two groups of researchers involved; for example, I think I am the only person that is a member of both groups!) This is because those interested in "famines," come from developing countries that have to deal with the phenomena.

However, this does not explain why disaster researchers are not studying the AIDS epidemic. Not one as far as I know has ventured into the area, although an occasional scholar is willing to allude to the Black Death as a well-known historical "disaster" (something that was spread out over a continent and over decades). This begs the question of what is the theoretical justification for the exclusion of AIDS. To be certain, there is always a danger in our area of labeling all negative social effects as "disasters," an equation which enlarges the concept to almost a meaningless and useless one. Personally, I would be inclined to exclude from the concept of "disaster" all very diffused events, including traditional droughts and famines and certain kinds of epidemics. Mostly I would do this because in my view it is best to think of the concept of disaster as an occasion involving an immediate crisis or emergency. Using that kind of thinking, in some of my other writings, I have also tried to distinguish ecological problems from disasters (Quarantelli, 1995b).

However, leaving this last distinction aside, I think there is considerable murkiness in how we deal with diffuse situations like those just mentioned. It leads to our odd intellectual ignoring of such phenomena as famines. And we do ignore it for very seldom is the existing literature on famine or drought used or cited by self-defined disaster researchers. Actually it would be difficult to use because certain research findings from famines/droughts are inconsistent with a number of the empirical generalizations presumably applicable to all such occasions from the "disaster" area. In part this is because in my view famines could be meaningfully conceptualized as "social problems" (illustrated in the discussion of McCann, 1987 about the vulnerability to famine of northeastern Ethiopia, which has one of Africa's most efficient traditional agricultural systems) in ways that distinguish them from what I would prefer to call "disasters." At any rate, far more systematic work needs to be done on this whole matter of inclusion or exclusion of diffuse happenings from the category of disaster.

c. Finally, there is the perennial problem of whether or not to include conflict situations as disasters (see Stallings, 1988). Recently we published a review article that compared natural/technological disaster behavior and behavior in riots and civil disturbances in the United States (see Quarantelli, 1993a). Our comparison was of behavior at the individual, organizational and community levels in the preimpact, impact, and postimpact stages of both kinds of situations. Overall, we found that while there were some behavioral similarities, especially at the organizational level, there were far more differences, some of a rather marked nature. For example, we noted that when disasters occur, individuals react actively and with a prosocial mode; there is far more variability in riots with antisocial behavior frequently surfacing. Also, while the experience of a disaster is a memorable one, and there are differential short run effects, there does not appear to be too many lasting behavioral consequences; riots seem to leave more residues. Similarly, there is a somewhat more likelihood for organizational changes after riots than after disasters. At the community level, disasters generate massive convergence behavior; this is far less true of riots. While there are some

selective longer run outcomes and changes in communities impacted by crises, the impact is less in typical disasters than riots.

Thus, our comparative summary of a range of empirical data supports conceptualizing at least some major conflict situations in a different way than disasters. However, I do not think this issue in the long run is primarily a matter of empirical determination. The very analysis we undertook found differences in part from the very way we defined and differentiated disasters and riots. In our view, a position of some kind on the matter of theoretical exclusion and inclusion of conflict situations is better based on the basic imagery one has of what processes hold social systems together. Of course in sociology, and vastly oversimplifying, there has been the functional point of view that mostly holds that systems are held together by commonly shared values and norms, and the Marxist point of view that conflict binds a social system. Professionally, I would be willing to take a third position by borrowing from the "garbage can" model of organizations. This argues that organizations (and in our view, societies) instead of having clear and consistent goals and values operate instead from a variety of inconsistent and ill-defined preferences. Different social entities at different social levels have different and incompatible views at different times; preferences may not be known until after choices are made. In addition, different parts of the system do not know what others are doing; what happened in the past and why it happened is not clear, and the connections between the actions taken and the consequences of such actions are obscure. (see March and Olson, 1986).

My overall point here is that one can arrive at drastically different conclusions about whether consensus occasions and conflict situations should be treated within the same definitional or conceptual category. Much depends on the researcher's more basic assumption about what processes serve to integrate social systems at the community/organizational levels. Since this is an unresolved question in sociology generally, it is also probably unresolvable in the particular area of disaster studies. Yet we should at least be aware of our starting points and be consistent in our approach.

For example, I am puzzled why disaster researchers who include conflict situations as part of the disaster arena for the most part do not study civil disturbances, or why they do not take advantage of the rather substantial body of theoretical and empirical literature in the sociological specialization of collective behavior that deals with crowds and riots. Of course, from my viewpoint, as someone familiar with the collective behavior literature, I think they would find that much which is empirically known about crowds and riots would be difficult to square with what is known about behavior in natural and technological disaster situations. (Elsewhere we have noted the marked behavioral contrast in the delivery of emergency medical services in disasters and riots, see Quarantelli, 1993a).

3. The failure to take the larger social context into account

We are currently undergoing a massive transformation in social life. This has been well described in summary statements by such sociologists as Smelser (1991a) and Tiryakian (1994) and in a somewhat different way on a global scale, by Oommen (1995) in his Presidential Address at this World Congress. Massive social changes are happening in the political, economic, familial, cultural, educational and scientific areas everywhere in the world, developing as well as developed countries.

As examples, we can note the new family and household patterns that are emerging, the basic alterations occurring in the role and status of women, the move almost everywhere to a market type economy to produce goods and distribute services, the spread of at least nominal democratic patterns

of government, the growing dominance of nontraditional artistic and musical forms as well as a globalization of popular culture, the escalating employment of computers and related means for training and educating people, and the growing diffusion and expanding use of applied social science to many areas of life.

We mention these massive transformations, because as far as I can see almost no one in the disaster area is incorporating them into their research designs. This is rather odd. Among other things, clearly these changes will transform the numbers and kinds of disasters that will occur and the nature of the disaster planning and managing necessary in the future (Quarantelli, 1994a).

In part, this neglect of the larger social setting reflects the generally ahistorical approach dominant in disaster research since its beginnings. To be sure, this again this partly reflects more general sociology. While there has always been a minority point of view around, it is only relatively recently that there has been increasing acceptance as Fischer wrote earlier this year:

Sociology, like biology and geology, is a historical science. Specific historical conditions and events, as well as lawful processes, determine current life ways. Sociologists of American society ought, therefore, to know American social history; too often, we do not. But there is help. Within the last three decades or so, historians have amassed a bounty of studies on American society, culture, and behavior. Focusing upon the everyday life of the "masses" instead of the dramas of the elite, this "new history" is informed by sociologists' questions and methods. So much and such diverse research has appeared that leading practitioners now call for a synthesis (1994: 226)

The same general notion is set forth in a statement this year by the current President of the International Sociological Association, Immanuel Wallerstein. He quotes from Durkheim who in the very first issue of the Annals of Sociology looked forward to an inevitable merger of sociology and history into a single discipline. This was because:

as soon as history compares, it becomes indistinguishable from sociology [and] as long as the sociologist is a stranger who intrudes in the domain of the historian in order to help himself, so to speak, to the data that interest him, he will never do much more than skim the surface rather superficially . . . it is virtually inevitable that the sociologist will not pay attention to, or will consider as disturbing, the data most worth noticing.

Wallerstein concludes:

I personally agree with Durkheim . . . , I cannot imagine that any sociological analysis is valid without placing the data fully within their historical context (1995).

If we take all of this seriously, does it not suggest some different theoretical studies than are now being undertaken. For example, if market driven economics has or is moving to the fore everywhere, what is the implication for the disaster area of such a trend? Even confining ourselves to the United States, I would suggest that, for instance, the recent emphasis on disaster mitigation by FEMA probably reflects the macro level economic orientation that the last three national administrations have taken. Yet who in the disaster area is doing such macro level studies? (for a speculative essay on market forces and disasters see an article by a non-disaster specialist, Horwich, 1990). Whether there will be empirical support or not for my specific example, my general point is that we need to take the larger social context into account especially since it is a very dynamic one.

4. The dysfunctional assumption.

Belief in the "badness" of disasters is very widespread. A common sense notion, it is widely shared among very many disaster researchers. In fact, as said earlier, some definitions of disasters characterize a disaster primarily in negative terms (and much of the mental health literature on disasters including that used by many sociologists implicitly does so, see footnote #3). In my view this implicit assumption about "badness" is also one factor that leads some scholars to view disasters as some kind of social problem; even in the social constructionist approach to social problems, the perceived "badness" of the phenomena by claim makers is a crucial element (see Schneider, 1985; Hilgartner and Bosk, 1988).

However, my argument is that this issue is a complicated one, which needs far more attention and some rethinking. Definitions aside, the matter can partly be approached at an empirical level. And the evidence on that clearly is that there are many positive consequences of disasters at all social levels (see the work by Scanlon, 1988, and others). Disasters can and do have positive effects and we should have more systematic studies on such consequences and not leave it to newspaper accounts to document the matter (e.g., a recent New York Times article had a headline of "Winners as well as losers in the Great Flood of '93" and did a very good job of illustrating that point; see Feder, 1993).

In many cases, we do not find other than negative consequences because that is all we seek. For example, in the mental health area most studies cannot find the positive outcomes of disasters because, simply put, they do not search for them. For those who might be concerned about focusing on functional aspects of disasters, it might be noted that sociologists have already written on such topics as "the positive functions of poverty" (see Gans, 1972).

However, at another level, in my view, the question about the dysfunctionality assumption is more of a theoretical issue. It rests on the basic imagery we have of what constitutes, what is the sociological heart of a disaster.

For example, many disaster researchers assume a disaster is a traumatic event occurring to an existing social system. This conjures up an image of damage, and efforts to react to an external agent. This is an understandable view and was implicit even in my earliest writings on the subject matter. On the other hand, there are other researchers who see disasters as evolutionary manifestations of ever changing social systems. This evokes, I would suggest, a rather different image, of efforts to adapt

to internal system dynamics. The first image emphasizes the negative and reactivity, the second the positive and proactivity.

My more up to date view now is that we would do better by using the semi-Darwinian model of evolutionary change. It would force us to consider the more positive effects of disasters (all but impossible to consider in a social problem context that focuses on the negative). We would necessarily need to think about and look at both the functional and dysfunctional aspects if we see disasters as part of the evolution of social systems. In this very World Congress, a French sociologist, Tourraine, in a paper that examines the future of social movements makes roughly the same point in writing that:

Many social problems or political issues are not related with social movements; many of them, in all kinds of societies, are related with processes of societal change, especially of modernization.

If social problems are socially constructed as many sociologists have long argued (see, Spector and Kitsuse, 1977; Mauss, 1992) it follows that a social change approach should be more fruitful for research purposes (I should note that I think Stallings, 1995, has done a masterful analysis of why at least earthquakes have not come to be perceived as social problems in American society).

5. The ignoring of relevant basic theoretical orientations.

There are many theoretical models and frameworks in sociology. However, sociologists in the disaster area have explicitly used very few of them. Even implicitly, the range of what has been employed has been rather narrow. For example, symbolic interactionism is the social psychological approach most used (Nigg, 1994); it is even the one that has been implicitly my approach and as such probably the correct way of doing things.

Nevertheless, there are other kinds of formulations or orientations that at least might be brought to bear on certain questions, because they would seem on the surface level to be particularly relevant. As examples, let us first mention two that could be applied in studies of decision making in disasters.

a. "Attribution" theory in social psychology. Without getting technical about it, this approach is fairly simple. Essentially it says that practically everyone commits the "fundamental attribution error," that is, explaining the behavior of others on the grounds of personal disposition to behavior in particular ways across a variety of situations, rather than--as we interpret our own behavior--as a response to circumstantial and contextual pressures.

If we seriously accepted this theoretical formulation, it should influence how we might study decision making at any level in disasters. (However, we should note that attribution theory has very recently been used to examine how judgements of responsibilities for disaster consequences are made, see Hans and Nigg, 1994). It suggests that the research focus should be on what actors see as the circumstantial and contextual pressures rather than looking for some predisposing attitude or motive that moves them to action.

Of course the general point was made a long time ago by the Russian novelist, Leo Tolstoy who wrote:

One of the most widespread superstitions is that every man has his own special, definite qualities; that a man is kind, cruel, wise, stupid, energetic, apathetic, etc. Men are not like that . . . Men are like rivers; the water is the same in each, and alike in all; but every river is narrow here, is more rapid there, here slower, there broader, now clear, now cold, now dull, now warm. It is the same with men. Every man carries in himself the germs of every human quality and sometimes one manifests itself, sometimes another, and the man often becomes unlike himself, while still remaining the same man.

b. "Satisficing" theory in social organizational theory. Although the basic notion won the Noble Prize for Herbert Simon, it too states a fairly simply notion. It is that organizations instead of trying to optimize or maximize goals, settle for "good" enough or satisfying decisions. They stop at that point instead of attempting to do better.

If we would take this theoretical formulation seriously, it too could affect how we might study decision making with respect to organizational learning from disaster experiences. The limits of learning are clearly indicated by the satisficing theory, as well as why organizations are unlikely to initiate massive disaster mitigation measures.

For another example apart from decision making, let us mention diffusion studies. The diffusion of innovations and the decisions involved in their adoption has generated a substantial literature in sociology and related disciplines (see Valente, 1993a, 1993b). However, I am not aware of a single use of this theoretical framework or idea in any disaster study. Yet it would seem a particular relevant approach for studies of mitigation, especially since recent work in the area has produced a PAR score, that is, a potential for adoption score representing the likelihood that any one innovation will be adopted (see Dearing and Meyer, 1994). The spread of GIS or geographic information systems would seem an obvious candidate for diffusion studies (for an initial work on this topic, see Gatreil and Vincent, 1990). In fact, any disaster related phenomena that involve social networks could be well approached with a diffusion framework.

The overall point we are making is that we should explicitly use far more than we currently do of the more relevant theoretical orientations around. The examples we gave are just that, examples. There are many more notions around rooted in larger theoretical frameworks that could provide us guidance for the testing of important hypotheses (e.g., the notion that ignorance is not absence of knowledge but is socially structured, see Stocking and Holstein, 1993; that changes have occurred in collective self identities of different ethnic and racial groups in Western societies; and, that we are moving into a time of postbureaucratic types of social organization with more participatory decision making). One idea which would be particularly applicable to formal groups in the disaster area is the notion of organizational decay which attempts to explain how some organizations function (see Schwartz, 1990 who applies it to the US space agency). More generally, some might argue that we

could learn much by applying "chaos theory" not because the term is somewhat homologous to the word disaster, but because that theory is particularly applicable to irregular cyclical phenomena. Let us use the theoretical guidance provided by those who have preceded us in sociology.

Methodological Issues

Under methodological issues, we want to discuss the urgency of taking advantage of the computer/high tech information revolution, the possibilities of experimenting with less traditional sampling and unorthodox interviewing techniques, the need to obtain better and more systematic field observations, how we might learn from historians how to gather more diverse and better documentary data, and the possible value of borrowing methods used in nuclear war studies. Some of these points are not new. Taylor, more than 15 years ago wrote that: "work in the field of disaster studies needs some exercise of the sociological imagination in the use and development of research techniques and procedures" (1978: 276). Unfortunately, there has not been much following through on what she advocated.

It signifies something, although I am not sure exactly what, that there are only a handful of publications on general methodological issues in doing disaster research (there is, for instance, very few between Killian, 1956 and Mileti, 1987). Perhaps this is because there are no special or unique problems in undertaking disaster studies. However, I think most veteran field researchers in the area, including myself, would argue that there are both advantages and disadvantages in doing disaster studies compared to what is present in normal times. For example, it is often much easier to get into organizational headquarters and to get to higher level officials at the very height of the crisis time in a disaster than would be possible during routine times (although my French colleagues would disagree with this, but perhaps that signifies some cross-societal differences that we need to explore).

On the other hand, there is also no need to document that there are significant sampling problems if one is interested in search and rescue activities during the crisis period. If my general perspective is anywhere near correct, we need considerable more attention to methodological issues in the area.

We leave aside, in considering methodological issues, the view increasingly being expressed that positivism in sociology is in massive retreat (see Baldus, 1990; Brown, 1990), and that an emerging postmodernism approach is changing the very nature of the knowledge being obtained (see, Sassower, 1991). If this is a true reading, someone ought to consider the implication of this shift for future disaster studies. There would seem to be important consequences if we do shift from a scientific positivism that sees scientists as noninvolved "spectators," to a postmodernistic stance that views researchers as "participants" (see Toulmin, 1981)

1. Taking advantage of the computer/high tech information revolution.

It needs little documentation to note that we are at the start of a massive information/knowledge/communication revolution that is transforming the world. What differences is this making in what is, or perhaps better stated, should be the focus of our research? It seems obvious that the recent flood of technology as manifested in computers, video cameras, satellite dishes, modems, fax machines, etc. and that result in Email, virtual reality, the INTERNET, CD-ROM disks, electronic journals, etc., are creating a built environment that is fundamentally different from any the human

race has lived in up to now (see e.g., Heap, Thomas, Einon, Mason and Mackay, 1995; Jones, 1995). At the disaster research level, as I have discussed elsewhere, it is clearly creating massive changes in disaster phenomena themselves, as well as in the planning for and managing of such occasions (Quarantelli, 1994a).

However, in this paper I want primarily to point out that the computer/high tech revolution is also opening windows of previously unavailable opportunities in the gathering of, the analyses done, and writing of reports from data on disasters. This is not to say that everything that is occurring is all to the positive for research purposes, but there are some potentially very positive aspects for research purposes. Only a very few have even addressed the possibilities (see, e.g., Butler, 1994; and also n.d., but who in writing several comprehensive summary articles on the current communication and information revolution focuses mostly on the implications for disaster management rather than for disaster research).

(a) For one, we can obtain data now that previously was impossible or very difficult to collect. For instance, it is not possible to tap into in much real time organizational and interorganizational information and communication flow. As an example, recently available on the INTERNET was much of the information that the US Center of Disease Control was sending out to all state and local public health agencies during the Iowa flood. Also available were the reports about health problems such agencies were reporting back from every county in the state of Iowa. Talk about availability of primary data!

It is also currently possible to obtain via computers much of the information that FEMA in the US currently issues to citizens and the mass media. In terms of the examples given, I would say we can get a much better picture of the content of such communications than we could by interviewing the communicators themselves. To be sure, there are problems even of just making a content analysis of such data, but any data has problems. There are many electronic bulletin boards currently in existence. For example, it is now possible to tap into information put out by US federal government agencies besides FEMA, such as EPA, USGS and NOAA.

At the international level it is possible to keep very current with situation reports on disasters issued by international agencies such as the UN Department of Humanitarian Affairs. In doing this recently, it struck me how blind disaster researchers from Western societies probably are to constantly occurring major disasters elsewhere, especially in developing countries. For instance, although there was not one referent to them in the American mass media we were exposed to, there were five massive disasters in about 10 days of July 1995:

these included one in Togo where torrential rains made 75,000 people homeless,
floods in Ghana that made over 200,000 homeless,
monsoon rains in Pakistan that affected more than half a million of the population in 1,018 villages,
torrential rains in Bangladesh that affected 12 million persons and destroyed or damaged nearly one million homes, and

heavy rains in the Ukraine where a city of two million residents destroyed and incapacitated the sewer systems and heavily polluted the main sources of drinking water.

To put it as politely as possible, disaster researchers, who are mostly from developed countries, seem to work with a very limited range of disaster occasions. The universe of such happenings is being very badly sampled.

(b) We can much more easily now locate and obtain comparative data from the same or similar occasions by other researchers. Many note that the high tech revolution is not only generating more information, but has an interactive quality to it that renders it qualitatively different from previous information/knowledge distribution systems.

For example, it is now possible to learn very quickly who else besides oneself might be undertaking studies on the same disaster. Thus, recently by using the Earthquake Information Gopher easily reached by our own PC we were able to find the Northridge Earthquake Research Directory that led to a file called the Northridge Earthquake Research Coordination Project Participants. Entering this file, it was possible to generate a listing of the names and titles of 119 researchers on the earthquake. Clearly the existence of such information allows quickly making links and tying in to networks of researchers that previously either could not have been done or would have been much more laborious to establish.

As another example, is anyone interested in learning who has worked on disaster aspects of cultural properties? Using the keyword "disaster" on the Veronica system, I was able to find again using my PC an annotated bibliography of 102 publications of work done on protecting and restoring cultural artifacts in disasters. In a similar kind of search I found listings of children's literature on floods and natural disasters, publications providing examples of disaster-related emotional problems, abstracts of TV coverage of natural disasters, and statistics on financial assistance for disaster-related schools and tourism problems, and also similar information on chemical disasters and droughts. Many similar kinds of information can be obtained through the EPIX system, that is, the Emergency Preparedness Information Exchange Gopher accessible via the Center for Policy Research on Science and Technology at Simon Fraser University in Canada (see Anderson, 1994 for a good summary of EPIX).

Sometimes it is possible to directly get written papers and reports. Some research centers such as NCEER allow direct computer access to entire texts that are available on line. Other information sources provide the information in different ways, such as the PAHO/WHO Disasters Documentation Centre in San Jose, Costa Rica, which has set up a CD-ROM library especially for professionals in the Americas.

Even if we had the technical knowledge, which we do not have, we do not have the time here for anything resembling a full discussion of all these matters we have alluded to above. Interested parties, therefore, are especially urged to look at, besides the references already provided, the 1994 November/December issue of the Stop Disasters Newsletter put out at the Osservatorio Vesuviano

in Naples, Italy for the IDNDR Secretariat, which has a number of articles on the communications/information technological revolution and its implication for the disaster area.

My point is that what would in the precomputer era would have taken months of searching and probably ending up with less information, can now be found in minutes. We have been particularly impressed by the real time nature of much of the information that currently can be found through a computer. For instance, there were 16 WWW sites, three Gopher sites, four newsgroups and one relay chat available on various computer nets that focused on the Great Hanshin earthquake in Japan, as of January 18, 1995, and many came into being within days of the disaster.

This kind of computer generated data and study sources would seem to considerably enhance, facilitate and quicken the linkages between disaster researchers interested in the same disaster or disaster topic. In fact, a very worthwhile project that might be funded by the National Science Foundation in the US should be one examining the quantitative and qualitative implications of the computer revolution for the development of critical masses of disaster researchers, their informal colleges and the professional networks involved. Earlier sociology of science studies on these topics, totally apart from the disaster area, strongly suggest that the computer revolution will significantly accelerate intellectual exchanges among disaster researchers.

Of course we would want to stress that no technological innovation can be any better than what human beings contribute by way of substantive content. For instance, in this last month while revising this paper, I have been monitoring on my PC a discussion group that has been exchanging ideas on how to define disasters and hazards. The active participants have come from Mexico, England, Canada and the United States. To put it mildly, the degree of sophistication, substantive knowledge and awareness of the existing literature, shown by the interacting parties has been extremely uneven. Nevertheless, even such a mixed intellectual exchange suggests the exciting new potential that exists for learning from others that is for the first time now possible via certain modern technologies.

c. We should improve our writing up of data, not so much in the word content as such of reports but in how some data could be displayed. By this I mean we can use better graphics, visual displays, photographs, and similar means to describe and depict our data about disasters; with imagination even more could be presented on video tapes. More than a decade ago I was very impressed in seeing in Japan a computer generated depiction of the dynamics of where victims had died in a night club fire. However, that is one of the very few times that I have seen disaster researchers go beyond words, simple graphs and tables, and occasional photographs. Actually the last was once more prominent even in sociology generally; thus in the early days of the American Journal of Sociology, from 1896 to 1916, 31 a total of 244 photographs accompanied 31 articles (Stasz, 1979).

In my view we could make substantial improvements in our description and depiction of data if we were to utilize some more recent technologies that have been developed and can be used for graphic and visual displays. Actually sociologists have written extensively on some of the older mechanical means available, such as those who have employed film records to judge the presence and extent of collective behavior (see Wohlstein and McPhail, 1979), to those who have discussed the use in

sociological studies of visual images from photography (around for nearly two decades now; see Curry and Clarke, 1978; see also, Becker, 1974; a more recent text is Ball and Smith, 1992). However, it must be admitted that sociology as a whole has made very little use of even these older audiovisual ways of presenting our data. If August Comte were to return today, however surprised he might be along certain lines, his attention would not be distracted by any exciting visual displays (about the only exceptions are some interesting photo and graphic displays that appear in introductory sociology texts). Given that, it is not surprising that disaster researchers in writing reports primarily display data using only means that preexist not only disaster studies, but also sociology as a field.

My point is not so much "to jazz up" our presentations, but that if we proceeded to use some imagination we could both better see what we are analyzing as well as conveying a much better picture to our audiences. As an example, we should make far more use of the graphic depictions that some Japanese researchers have recently employed. We do not have the capability here to reproduce the different colors that they used in the illustrations employed to give an overall picture of the dynamics of different disasters, but maybe even in black and white something of what they convey can be illustrated here. See Appendix #1 of this paper where the illustrations presented are taken from a series of them in a Japanese publication, Long Road: Toward Mitigating Earthquake Hazard (Learning from the Past) Tokyo, Japan: SEEHM, Kajima Corporation, No. 2 September 1992.

2. Experimenting with less traditional sampling frames and more unorthodox interviewing techniques.

Few researchers would question the fact that particularly in studying impact time disaster behaviors, we are usually faced with major problems of sampling and interviewing, for reasons quite familiar to any experienced field researcher. Yet, there would appear to be better ways of sampling and interviewing disaster relevant populations than what is usually done in the standard survey study.

At least, we ought to make some attempts to experiment with less traditional sampling procedures and frames.

For instance, there already exists a literature that discusses in detail a variety of existing methods, in the felicitous phrase of one article, for "sampling rare populations" (Kalton and Anderson, 1986).

Among established techniques available are screening methods, the use of disproportionate sampling, multiplicity sampling, multiple frames and snowballing. Except for very rare and isolated uses of disproportionate sampling (this oddly enough was used in the very first systematic population survey of disaster population, namely the 1954 NORC study of the Arkansas, now recognized as a classic, see Marks and Fritz, 1954), and of snowballing (used in some NORC studies and early DRC research on search and rescue and emergent groups), the other techniques are not used on any scale by disaster researchers.

Many of these techniques are not that new; for instance, household surveys with multiplicity sampling where respondents report not only on their own behavior but of other persons as well such as friends and neighbors, are at least three decades old (see Sirken, 1970). Researchers from DRC did use results from one multiplicity technique in their study of the 1985 earthquake, but honesty requires noting that this was only because the Mexican polling organization carrying out the population survey, solely on their own initiative, employed the technique. Nevertheless, from 567 randomly

selected, treated as informants who provided information on earthquake-related behavior of every member of their household. Therefore, we obtained data on the nature and extent of volunteer activity for a total of 2,965 individuals (Dynes, Quarantelli and Wenger, 1990).

In addition, I would argue that we should try using some unorthodox or little used interviewing techniques that are different from the standard procedures typically employed in both open-ended and structured survey interviewing. For instance, we have always thought although admittedly never have tried, that it might be worthwhile to do personal interviews of several persons together, in a semi-focused group setting. Also, while I would not for sociological purposes utilize Rorschach type techniques, it could be worthwhile experimenting with the use of maps, photos, diagrams, etc. in conjunction especially with open-ended personal interviews (geographers for instance have obtained some fascinating data from having respondents draw maps of their "neighborhoods" or communities).

3. Obtaining better and more systematic field observations.

For a whole variety of reasons, including the increasing legal problems of protecting the confidentiality of data, it has become more and more difficult to conduct personal interviews on almost any subject matter in North America. Until recently, sociological disaster researchers have depended heavily on face-to-face or personal interviewing (although in recent years, there has been a marked shift to greater use of survey interviewing, either by phone or mail). My view is that whether we want to or not we will have to look for other data gathering methods to complement if not to supplement our primary reliance on the interview *per se*.

For a long time I have thought that, far more might be done to develop field observation teams, especially for obtaining systematic information regarding the crisis time periods of disasters. In fact, if I were to be reincarnated and started working in the disaster area afresh, it would be an interesting challenge to try to develop such teams of observers. This would require extensive training on how to conduct systematic observations on site, and in particular how one could take advantage of and collate the multiple perspectives of different team members (although we have strong doubts about the theoretical value of the notion of "the assembling process" for collective behavior analysis as developed by Clark McPhail, 1991, there is much we can learn from the systematic coding systems he has produced for studying the process, see McPhail and Wohlstein, 1983, 1995).

Such systematic observing would probably be limited to looking at certain phases of the preparedness and response phases of the crisis periods of disasters. Yet, I think we might learn more from such data gathering than relying as we do now mostly on retrospective and after the action interviewing.

As Mileti has written of disaster behavior: "what people say about behavior and how they actually behave are not the same thing" (1987:69). Moreover, such teams would have an advantage today compared to the past. In part, this is because such groups could now use videocameras and laptop computers in their data gathering, and there are texts on using computers in qualitative research (e.g., Pfaffenburger, 1988; Fielding and Lee, 1991). We should also note that with some sociological imagination it is possible to visualize a gathering of observational data concurrent with a simultaneous analysis at a central computer base that would allow a shifting of observational points to capture the dynamics of the processes being observed, such as in search and rescue efforts. Is this

too grandiose a formulation? Perhaps, but even now it is technically feasible although the social infrastructure necessary may be beyond our current willingness to attempt. However, it is the kind of research design and equipment that I think will be commonplace in the mid or late 21st Century.

One frequently raised objection to observational data is the supposed limitation of what can be done with the end product. Personally, I have never assumed, as is true in a very strict positivistic viewpoint, that observations are useful only in the preliminary stages of scientific exploration and can only generate but not test hypotheses. My view, hardly unique today in sociology (see Lofland and Lofland, 1984), is that as a form of data collection, observations can be useful: "for measuring concepts, testing hypotheses and/or constructing causal explanations" (Jorgensen, 1989:7). If that is your position, collecting and analyzing data poses no problem for scientific advances.

Also, the probability that much observational data might have to be qualitative also does not bother me. In part, this is because we as sociologists, as I see it, have never taken full advantage of the kinds of qualitative measurements that have long been available (and this applies to other than the disaster area). As examples, we might cite several writings on methodology by Paul Lazarsfeld and Allen Barton, which despite being decades old, are still highly relevant for qualitative observational data gathering and analysis. (see Barton and Lazarsfeld, 1955 reprinted in McCall and Simmons, 1969, and Lazarsfeld and Barton, 1962). For more recent discussions on the analysis of qualitative data, see the symposium edited by Blaxter, 1979, and Miles and Huberman, 1994.

Systematic observational data might be especially useful for cross-societal and cross-cultural research. Anthropologists and others have long done comparative observational studies of behavior such as are involved in "body language" and spatial distances between interacting human beings (e.g., Bull, 1983). Much has been learned. Who knows what we might learn if we made comparative examinations of some disaster behaviors in the same way?

4. Learning from historians how to gather more diverse and better documentary data.

We need to gather not only better but more diverse documentary data. I use the term document in a very broad sense going considerably beyond official reports, written records, census statistics and mass media contents. Many of those are often gathered in disaster studies. Less frequently obtained are such items as organizational minutes of meetings, informal group logs, business transaction data, etc. Seldom, moreover, does one find disaster researchers obtaining and using letters, diaries, graffiti and informal signs, bulletin board items, family albums, religious sermons, and much of what have been called nonreactive items (see the classic work on the last, Webb et al., 1981; for the use of personal documents, see Plummer, 1983; Burgess, 1984: 123-142; also, Hodder, 1994). Occasionally, there will also be a fleeting reference in studies to the "gallows humor" or jokes, or the songs that follow disastrous occasions, but no one has attempted much of a collection of them (I was once involved in an abortive effort with disaster researchers from England and Germany to hold a conference aimed at producing a research agenda for popular culture aspects of disasters; in my view, the idea could be very productive for disaster researchers). At any rate, my general point here is that we should not equate documentary data only with formal and written reports, and statistics. Our data scooping efforts ought to be broader.

We do not need to start at ground zero on this matter. One discipline has had to depend mostly on documentary data in the broad sense of the term. That of course is the field of history. Historians have been at their craft centuries longer than have sociologists. It is not inconceivable that perhaps they have learned how to gather, assess and use documentary data (the classic and still relevant work in historiography is Langlois and Seignobos, 1898; for more current references, see Pitt, 1972; Hodder, 1994; and especially Dymond, 1981, an English publication which deals with the use of local sources and would be particularly good for community studies). We ought to look at what historians can teach us about how to deal with documents of all kinds.

Even at present, some disaster researchers, using mostly historical sources, have done rather good studies of past disasters. Scanlon (1994), for example, has reconstructed in very fine detail, much of the behavior during and in the aftermath of the Halifax explosion, which occurred over 50 years ago. If nothing else, his work shows how persistence and imagination can uncover data that superficially one might think never existed in the first place much less survived the years. Perhaps others ought to try their hand at historical reconstruction of disasters. After all, many major works in sociology recognized as classics and by such sociologists as Weber, Marx, Durkheim, Thomas and Znaniecki, Tonnies, etc., are primarily historical studies based mostly on documentary data. What we have just cited also implies that a historical analysis of documents need not be confined to a single case study.

In these days of tight research budgets, it is perhaps not amiss to note that much, although not all, documentary data collecting is often very inexpensive. Much can be found in university libraries or organizational archives. There is a very good discussion about how to go about mining such sources, in an old publication by Glaser and Strauss (1967: 161-184).

Further, I would suggest that it might be worthwhile for some disaster scholar to put together a detailed statement on the methodology of gathering documentary data. All of us would benefit from such an effort. We cannot afford to continue to ignore the need for and the value of more diverse and better documentary data in our studies.

5. The possible value of methods used in nuclear war studies.

Finally, let me mention something very briefly. In some ways, it can be seen as an effort to salvage something from a considerable expenditure of societal resources.

Up to a few years ago and for about three decades, tens of millions of dollars were spent and thousands of studies were done on nuclear war effects, including those on the civilian population in the United States (and apparently from what we have been told by Russian colleagues, this also happened in the former Soviet Union). From a cursory perusal I have made of the American studies I would say that substantively such studies are probably not very useful for any purpose that I can think of, since almost all of them projected an unknown postnuclear war world (for a typical such study see, Chenault, Engler and Nordlie, 1967; for general summaries of these studies that attempt to use a social science approach to the postattack problem, see Nordlie, 1963; Lybrand and Popper, 1960).

However, in most of the research undertaken, for a fairly obvious reason, a consistent overall (nation state) social system approach was used. This is a framework only rarely employed in past or current disaster studies. So perhaps to the extent we move to undertake more macro level studies, there might be some clues on how to proceed from this war oriented research. More important, from my perspective, is that very many of these studies used elaborate and sophisticated research techniques and methodologies for social system analyses that clearly in my view would warrant and be worthwhile looking at for insight and ideas especially for macro level studies of regional disasters or catastrophes.

Empirical Issues

It is not our intent to present a laundry list of empirical studies that might be undertaken. Instead, we present a more strategic rather than tactical approach to this matter. While some specific topics are suggested, it is our intent here to stress general themes on how we ought to approach empirical research. These include a need to do more studies on disasters that cut across governmental and political boundaries, do far more in depth research on topics about which our data base is really weak, look at many important disaster phenomena of a social nature on which we have done almost no studies, examine for their significance the "deviant" cases we encounter, and look at institutional areas that have been neglected so far.

1. We should particularly do studies of disasters that cut across governmental and political boundaries.

Many disasters these days cut across international, national, regional or other formal governmental boundaries. Many good examples of this happening surface in the operation of lifeline systems, a few of which have been studied (see Tierney, 1992; Nigg, 1995). Other recent but different examples are the radiation fallout from Chernobyl that fell on many countries in Europe, and the pollution of the Rhine River that started near Basel, Switzerland and affected six nations along an 800-mile course. On a less extensive scale, even relatively small disasters such as the sinking of the ferries at Zeebrugge in Belgium, or the Estonia in the Baltic Sea, necessarily affect in a direct fashion organizations and citizens from many different societies. I think we can take for granted that such noncommunity disasters will be more frequent in the future.

In what ways and what do we study in such disasters? Just as risk analysis and preparedness for disasters that start in one locale and have consequences far away pose difficult operational problems for emergency managers, we in the disaster area also clearly will have difficulties in designing research to span such diffuse disaster occasions. Yet it is something that we increasingly will need to do. We need far more studies of those disasters whose effects are not community focused or locused, but cut across all kinds of political/governmental boundaries.

For empirical studies too, we must consider theoretical developments in sociology on this matter of the blurring of social boundaries. There are also research problems we need to attend at the other side of the spectrum, not where the disaster impacts but those groups that have some responsibilities

for responding. For example, what is the empirical research implication of those who like Skocpal (1992) argue that we should move from a "state centered" explanation of social policy to a "polity centered" model. She essentially believes that while the state itself remains important, nongovernmental organizations, institutions and movements that are politically active--for example, unions, veterans' groups and women's voluntary organizations--may also crucially affect policies about benefits for veterans and family welfare programs. What should we be studying if her position is a valid one and generalizable to the disaster area? In more general terms, what studies are suggested by the blurring of traditional domain lines and the evolution of important groups with other than formal governmental boundaries. From my viewpoint, this approach would appear particularly important for disaster mitigation research.

Although we will not specifically address here the consequences for empirical work, there is something happening to all scientific activity generally, which someone ought to examine for its implication for studies in the disaster area. This is what has been called the internationalization of social science research and knowledge (see, Smelser, 1991b). Just as organizational and political boundaries are becoming vaguer and less consistent with formal legal boundaries, so too is science becoming less rooted in particular nations or social systems. I find it difficult to believe that our empirical work in the disaster area is not being affected by this internationalization process. My surface reaction is that overall the effects will be positive for research studies (and elsewhere I have suggested it will lead to improvement in disaster theory, see Quarantelli, 1995a). Yet I feel that unless we do a better job of addressing problems in doing cross-national, cross-societal and cross-cultural studies (and the three are different), and ask how we can develop transnational teams of researchers, we will not progress very far along this line (although sometime ago I once wrote one of the few articles yet available on this topic, it is now clear to me that my approach was rather superficial (see Quarantelli, 1979). Also, the more we moved toward cross-studies of any kind, the more we will discover that researchers elsewhere have frequently done more work than many of us realize (To North American and European disaster researchers, I would call attention to the recently produced Annotated Inventory of the social science research literature on disasters in the former Soviet Union and Contemporary Russia, which list over 100 publications; see Quarantelli and Mozgovaya, 1994).

2. In depth studies are needed of many topics about which the data base is very weak.

Some sociologists have said that we do not do a good job in sociology generally in the accumulation of knowledge. As Gans has written: "even the normal science that is conducted while paradigms remain dominant is not cumulative, at least in sociology, for empirical researchers regularly carry out research that repeats findings already reported by earlier researchers (1992: 701). There is certainly a degree of that in sociological disaster research, although I will forego giving anecdotal examples of failures to recognize that something more recently "found" had been consistently reported by much earlier researchers (elsewhere, Quarantelli, 1988, we indicate how the famous NORC study of the Arkansas tornado in 1952 sets forth very many propositions about behavioral responses in disasters, some of which are sometime advanced as "new findings" in current research).

However, I think our problem on this matter is slightly different. There are many topics in the disaster area that we think we know a lot about but for which the data base is really very weak. For instance, probably all disaster researchers believe that there is considerable convergence of people, goods and information to a disaster site after impact. I personally have no doubt about it. Nevertheless, the empirical data base on this is remarkably weak. The pioneering disaster researchers such as Fritz and Moore made major attempts to empirically document the phenomena (see Fritz and Mathewson, 1957; Moore, 1958). However, after that initial work, the notion that convergence existed was taken for granted and never reexamined in any systematic way.

Only very recently did Scanlon (1992) revisit the topic. While his findings did not basically challenge most of the widely held ideas about convergence, he did considerably refine, better detail, and made further important distinctions about the phenomena. However, my point here is that several decades went by between the initial work on convergence and the much later restudy by Scanlon; that should not have happened. If US Today can obtain numbers on certain kinds of convergence in the Loma Prieta earthquake (see, Stone and Castaneda, 1990) or a British newspaper can report that after the Zeebrugge ferry disaster, the Kent County police in England received 1.4 million phone calls, why cannot sociologists do better and more systematic studies? Or is the more important question that should be asked: why do we accept "conclusions" in our area when even a superficial search would uncover the weak data base?

As another example, what do we know about crime in connection with disasters? I do think we are certain that looting does not generally occur or that violent antisocial behavior is very rare at least in developed societies (Quarantelli, 1994b). But there are many anecdotal observations that white collar crime is widespread in disasters. Here again journalists have been ahead of us. For instance, while the official death toll for the Northridge, California earthquake was 58, the state received 374 requests for grants to pay funeral expenses for quake victims (Simon, 1994)! Also, not too long ago there was an investigative report in the New York Times about massive fraud in agricultural disaster aid programs in the United States (Frantz, 1994:1), and there have been consistent rumors about malpractices in international disaster relief and recovery programs. As far as I know, researchers have not look at all at such criminal behavior.

Here and there, a few researchers have built upon and extended previous work. My own studies of panic flight go back as far as my Master's Thesis on the subject matter (Quarantelli, 1954, 1957). My early writings in the area can to be generally accepted as canon on the topic (Nigg and Perry, 1988). However, Norman Johnson and his colleagues a few years revisited the topic and employing a far more sophisticated approach than I had used, substantially advanced our knowledge of panic behavior in disasters (see Johnson, 1987, 1988; Johnston and Johnson, 1989; Johnson, Fineberg and Johnston, 1994). Equally as important, they grounded their conclusions in a solid body of data. We need many more similar in-depth studies of widely believed findings about disaster phenomena that have a weak empirical base. A list of such topics could probably be developed through a perusal of the Inventory of Sociological Findings on disasters compiled by Drabek (1986), or some of the chapters in the Sociology of Disasters volume (Dynes, De Marchi and Pelanda, 1987) or those in the recent tome by Dynes and Tierney (1994).

3. Research should be initiated on disaster phenomena that have been only little or not studied.

There are many aspects about disaster behavior where we have extremely little or no knowledge at all. In fact, unlike the empirically weak but nonetheless widely held beliefs that we discussed above, we are talking here of where such beliefs do not even exist. Can any disaster researcher cite any widely held beliefs, for example, about disaster associated death, sex, humor, aspects? (To be sure there are problems of identifying something as humor such as the current saying circulating in the Los Angeles area, namely, "downtown's a great place to work but it has it's faults"). Their absence can probably be attributed to the fact that a search of the Disaster Research Center library holdings uncovered less than a half dozen publications on all these three topics together.

In a few areas, such as gender, a start has been made (see Neal and Phillips, 1990). With respect to this it is perhaps a commentary of some kind also that the role of women seems to show up more, at least anecdotally, in studies of developing countries. Related to this, and probably not coincidental, Western disaster researchers have paid very little attention to disasters in rural areas (exceptions are Green, 1984; Perspectives on Earthquakes in Rural Areas, 1994). Apart from the people involved, it could be argued that agricultural losses in crops and animals, and damages to soils and topography cannot be rebuilt in the same way as buildings. Yet, if we are going to establish that there are human universals in the disaster area as has been argued for other aspects of human behavior (see Brown, 1991), we clearly need systematic studies of rural populations and communities in disasters, because at least along some lines, there are clearly lifestyle differences between rural and urban areas.

Also, there are certain disaster associated activities that have assumed almost the characteristic of fads, but have been very little studied. As examples we might cite, the Incident Command System especially being pushed in the fire community (for some minor questioning of the system see Stoffel, 1994) and the supposed mental health effects of disasters on children (see Aptekar and Boore, 1990; Green, 1994). Even the current emphasis on mitigation, begs the question. Do we really know, for instance, that mitigation is the best strategy ---where is the evidence that a focus on mitigation will have the greatest payoff (for a contrary point of view, see Douglas and Wildavsky, 1992, who argue there is greater value in developing societal resilience to better cope with environmental adversities).

Again, it would be worthwhile for someone to compile a master list of topics that have been only little if at all studied. Of course, such a listing requires at least some implicit theoretical notions about what is and is not important for research purposes. I personally, for example, think we need to start intensively looking at the international and national levels of disaster phenomena (about which Drabek, 1986, reported we knew very little a decade ago). From our perspective, a case could be made that it is important for a whole variety of reasons to start trying to understand, for example, the decision making involved in international disaster relief, or the social norms and cultural values that come into being on giving priority to mitigation measures in a given social system. Others might pick different questions to study. Yet irregardless of what criteria might be used, I have no doubt a substantial list of topics could be produced.

4. "Deviant" cases should be examined for their significance.

I particularly think we need extensive studies of findings that are "deviant," that is, seeming exceptional results that do not fit in with generally accepted research findings. Examinations of such instances could force a major rethinking of accepted disaster generalizations. Of course, one has to encounter and recognize a "deviant" case in the first place, in order to be challenged.

For illustrative purposes, let me give a personal example. I have been one of many who during the years has contributed to the generalization that looting is very rare in disasters, at least in developed societies (this last qualification is not always made by everyone). It is also often said that such looting as does occur is, minor, done by individuals, covertly undertaken, socially disapproved, and opportunistic in nature. When Hurricane Hugo hit St. Croix in the U.S. Virgin Islands, there were immediate news reports about widespread looting on the island. Our initial reaction was to dismiss such accounts as typically incorrect reports (see a discussion of these kinds of reports in Wenger and Quarantelli, 1989). However, something just did not seem "right" about the St. Croix situation. To cut a long story short, I ended up making three different trips to the island, doing both intensive interviewing of residents and officials, and a survey of all business in the four shopping centers on St. Croix.

Our field data proved very surprising. There had been massive looting after Hurricane Hugo, not to the extent news reports indicated, but nevertheless very extensive whether measured by places and/or amount of items looted. This conclusion was not in line with other consistent findings about looting in disasters in the United States (going as far back as Fritz, 1961). However, that was not the most surprising finding. The looting that occurred in St. Croix was major, done by groups, overtly undertaken, socially approved, and situational in nature. Most of you will recognize that these features are not only the opposite of those typically found in disasters, but even more important, are the characteristics of looting behavior in riots and civil disturbances! (for looting in the latter situations, see Dynes and Quarantelli, 1968; Quarantelli and Dynes, 1969, 1970).

What we found was looting which is typical of riots appearing in a disaster occasion. As might be expected, we have pondered this finding and its implications considerably. It would be nice to be able to report that I have intellectually straightened everything out, but I am still considering "what it all means." Yet, it is for that very reason that I think we need to examine all "deviant" cases intensively. It will force disaster researchers to at least reconsider what assumptions they are making and the validity of the data on which they base their conclusions. Although I will not have time to discuss the matter here, it is my belief that "deviant" cases are more likely to appear in cross-national, cross-societal and/or cross-cultural research, giving us another reason why we should do more and more such comparative work (for how few studies are actually of a truly comparative nature, see Dynes and Drabek, 1994).

5. Examination is needed of institutional areas that have been so far neglected.

We have very unevenly studied different institutional areas. For example, we are beginning to learn something about business recovery in the United States (see, French, Ewing and Isaacson, 1984; Dahlhamer, 1994; Tierney , 1994; Dahlhamer and D'Souza, 1995; Nigg, 1995). Yet we do not have even a descriptive picture, for instance, of the fast food outlets who provide much emergency

assistance or how large corporate entities directly and indirectly respond to disasters. Yet, newspaper stories about Hurricane Andrew reported that Exxon donated \$300,000 to the Red Cross, Chevron gave \$200,000, Home Depot sold plywood, shingles, roofing paper and sheathing at cost; Beech-Nut Nutrition donated 1,000 cases of baby food, Kmart donated 800 cartons of diapers, Campbell Soup sent 500,000 cans of food and General Motors created a matching contribution fund for its employees (Folk, 1992: 6B). In some instances, preparedness actions are taken. For instance, Cellular One, the leading cellular telephone company in the Bay Area of San Francisco recently obtained a commitment from Motorola and AT & T, cellular phone manufacturers, for 4,000 cellular phones which would be distributed to Bay Area emergency response organizations in case of a major disaster in the region. Yet, the what, why and who about such activities are totally unknown territory to disaster researchers. Given this, it is not surprising too that there does not appear to be a single study about the role of labor unions in disasters.

There are also other institutional areas barely touched by disaster researchers. For example, what do we know of religion and disasters? Extremely little I would say. There can be no excuse here that religious behavior is unknown territory generally, since sociologists have done much work in the area outside of disasters and there has been a recent resurgence of the field (see, e.g., Lege and Kellstedt, 1993). It is also very probable that praying is almost certainly one of the most frequently used coping mechanisms for dealing with disastrous occasions. Why has it not been studied? Maybe it says something about the sociologists doing disaster research. Again, we find newspapers doing a better job of tapping such reactions. A New York Times article on a 1994 flood in Georgia with the subtitle; "People who have lost all find a message of solace in religion." (Applebone, 1994: 14), offered some very interesting observations.

How should we proceed? Taylor (1978) once wrote a chapter on future directions for study. In it, she examined and suggested research into seven institutional areas---political, economic, familial, religious, health, social welfare, mass media (business is partly treated under economic institutions). Again, it might be worthwhile for someone to look at what she said, and to make some sort of systematic assessment of what is still lacking in the 16 years since that research agenda on institutional areas was set forth.

A Few Conclusions and Recommendations

What can we generally conclude from the sweeping survey we have just made? If our observations are valid, what can we do to improve disaster research? Let us suggest six general points.

First, we grant that the field will survive, but argue that it will not thrive without significant changes. For many reasons, sociological studies of disasters will continue even if nothing is done to improve the quality of the studies undertaken. However, in my view, the research results will be of less and less value and validity.

Second, for changes to occur, if again we take seriously what we know as sociologists, there is a need to provide:

- (a) professional incentives;

(b) structural inducements.

We discuss some below.

Third, there are things that the Research Committee can do, given that such an infrastructure is already in place (I leave aside here that the Committee itself probably will have to significantly reorganize itself, but most of you know that issue has already been raised elsewhere).

It should encourage:

(a) Additional special topical issues of its journal, The International Journal of Mass Emergencies and Disasters. Studies in the sociology of knowledge strongly indicate that the research agenda for a field is heavily influenced by the major journals in an area. If so, the editors of the journal have a ready means for influencing the development of research in certain directions rather than others.

(b) The production of other volumes sponsored by the Research Committee. The one edited by Dynes, De Marchi and Pelanda has been the only one up to now. Perhaps thought might be given to the production of a biquarterly (every four years) which might be called Review of Research. To be sure, commercial publishers for such volumes will be difficult to find. However, possibly the various research centers that have publication programs might be willing to take turns in putting out such products, or the Research Committee might partly subsidize their publication. Also, given what we said earlier about the computer/high tech revolution, perhaps there might be some possibilities for turning out such volumes via desktop publishing.

(c) The establishment of symbolic prizes for innovation or innovative works. Although we now have the Charles E. Fritz award for a distinguished career in disaster research, we still do not have any awards that explicitly reward new work. In my view, we ought to have an award for innovative theoretical work.

and

(d) An increase of membership in our Committee from as many different social systems and disciplines as possible. This suggestion goes far beyond the need to have enough members for the financial viability of the Research Committee. Our intent here is to facilitate the bringing in of new perspectives on disaster research. There is always a need for new "blood" in the field who can see things differently (it was not until I visited Russia that I saw something I had not seen before, by the question a Russian colleague asked, namely, how valid are current research findings that assume a stable social environment for rather different settings such as the former Soviet Union where instability in the environment reigns and is part of everyday life. In retrospect this is an "obvious" question but had not occurred to me before). In fact, I think there is an urgent need to bring in the views, social science as well as social-cultural ones, which are different from the Western ones that prevail. This in no way should be taken as an attack on Western oriented scientific work, as some recent statements in the field seem to imply (see, for example, Hewitt, 1995). But the field of science, including sociology, is moving toward an internationalization, and as Smelser, 1991b, in a

very insight article notes, there are both pluses and minuses to such a trend. One of the advantages are the new perspectives that can be brought to bear, something we do need in the disaster area.

Fourth, and more generally:

(a) We need to increase the number of good recruits into the field (particularly those who will write Ph.D.s on a disaster topic). This will provide the area with a larger base that in turn is likely to allow for the appearance of more new ideas. Perhaps the providing by the Research Committee of an award for younger researchers would be helpful among graduate students in both giving more visibility and legitimacy to sociological disaster research.

(b) Researchers should serve, whenever possible, on multidisciplinary, advisory boards and committees that in some way are involved with the area of disasters. Sometime the presence of a disaster researcher does allow significant input into published reports or recommendations. For example, I think my past membership on the US National Academy of Sciences Board on Natural Disasters (BOND) allowed me to influence somewhat what was officially reported about the actual and possible roles of the social sciences in disaster studies, and to counterbalance the notions that most solutions to problems were to be sought in engineering or the natural sciences.

(c) College and university teachers should use examples from the disaster area in their regular courses. Among other things, as I found out myself, this sometime forces thinking through certain ideas derived from research. For example, efforts to explain emergent phenomena in disasters in a collective behavior course I taught led me at one time to realize that I was failing to distinguish emergent groups from emergent behaviors and led to a new research project and publications (see Quarantelli, 1984).

(d) We should try to recruit into disaster sociology, as many professionals and social scientists as possible from outside the discipline. This has nothing to do with simply getting greater number of disaster researchers. It has to do with the fact that there is considerable evidence from the sociology of knowledge area, that the greatest innovations in a field are far more likely to be produced by recruited outsiders or peripheral-to-the-field scholars, rather than mainstream figures. Put another way, intellectual orphans are more likely to have new and unorthodox worthwhile ideas since they are unlikely to know better!

Fifth, we should note that most of these advocated changes do not require new or additional funding. The argument that more money is needed is a perennial but, in my view, not always a valid excuse for failure to take new actions. Financial resources are indeed sometime crucial, but not always. Often more important is the willingness of a few scholars and researchers to take the lead and make some investment of time and effort in helping to improve an area. This would seem to be a minimum responsibility of a professional. In fact, we could not produce our journal and our newsletter, and have a Research Committee, if some members did not volunteer their skills, time and effort.

Sixth, and finally, what about having a session four years from now at the next World Congress, which would focus solely on new ideas in sociological disaster research? Since

nature abhors a vacuum, researchers will surely attempt to produce material for such an undertaking. It is worth a try.

FOOTNOTES

1. However, it should be noted that some sociologists have attempted to make a general case for the possible contributions of applied social research to more basic or theoretical issues. One of the better such statement is in the Presidential Address to the ASA by Peter Rossi. However, it is not very convincing to me given that he grants that there is the counter argument that the: "bulk of applied social research is of poor quality and hardly likely to contribute even to the discipline, let alone to the solution of social problems. There is some truth to this argument: Much of applied social research is best left in the fugitive Xerox reports in which they were issued" (1980: 891).
2. For those interested in linking different disciplines, there is something called interfield theories which deal with the problems of bridging two fields of science. There is a good discussion of this approach in Darden and Maull, 1977.
3. The American Psychiatric Association definition of a traumatic event, which includes disasters, is that it is one: that is outside the range of usual human experience and that would be markedly distressing to almost any one." Inhabitants of many areas of Bangladesh would find the first phrase interesting; we probably badly underestimate how many of our concepts and ideas in the disaster area are not fully reflective of the full range of human experience around the world. The second phrase seems to include a prejudgment instead of making the statement a matter of empirical determination.

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