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Future Directions in Disaster Research

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FUTURE DIRECTIONS IN DISASTER RESEARCH

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Papers on "future directions" are usually written by list makers and appear in a form similar to those small magazines found in motels which tell you "what do do" this week. It is always easy to tell others what to do and even easier to tell others where to go. Too, since we are discussing "future" research, there is a standard litany that future research should always be driven by theory, while ignoring the question of what drives theorists. Such a litany underscores the importance of ritual in social life. Such litanies are important in the rituals of the research community.

But beyond the standard litanies, choices and selections have to be made as to future directions, even if those suggestions will not lead to any immediate action. The selection criteria used here is a very personal one. I will suggest things that I have thought about, but not much. Thus, the aim is to reduce my puzzlement rather than to advance theory.

My comments are divided in the following ways. First, some issues relating to the conceptualization of "disaster" are raised. Perhaps a purist might object that that's not research, but it is an honorable and scholarly pursuit. Second, some suggestions will be made based on the conventional and common sense categories, indicating social time and social processes—mitigation, preparedness, response and recovery. While they are "public policy" categories, public policy drives funding and funding drives research directions, more often than "theory." Finally, there is a concluding comment on style and direction in the communication of research results. As social

scientists, we seem to have an institutionalized mind set which affects the way we structure problems and reality. Consequently, our effectiveness may be determined as much by our orientation as it is by our methods and tools. While this is a more philosophical comment, it still suggests a "direction." We turn first then to problems relating to the conceptualization of disaster.

The Concept of Disaster

It is not particularly startling to suggest that there are a number of problems with the concept of disaster. Like many other sociological terms, there are multiple meanings, some of them common sense. Over the last several decades, several manuscripts and articles have claimed to have solved the definitional problem. Most of those attempts have not resulted in any conceptual clarification.

In Organized Behavior in Disaster (Dynes, 1971), I distinguished four different meanings of the term disaster—one which related to the name of the disaster agent; a second which related to physical impact; a third which related to social impact; and a fourth which related to the evaluation process which defines the event normatively. In making those differentiations, there was the suggestion, perhaps too implicit, that those definitions vary independently--that physical impact is not perfectly correlated with social impact; that social and physical impact might not be related to the evaluation. One might argue that TMI was a disaster only in the evaluative sense, there was almost no "physical" impact and little social impact, at least from the agent itself, but considerable social impact from the evaluative process. Unfortunately, there is still a tendency to lump rather than to differentiate. Most discussions often treat all four dimensions as identical.

Agent as disaster. Many of the putative theoretical breakthroughs are based on what one can call "agent exceptionalism." Most of these approaches take "natural disasters" as somehow normative or, at least, traditional and suggest that a new agent, such as a technological one, is not only different but also more important than the effects of "natural" disaster. Many such claims are true by assertion--if you define disasters by agents, differences in agents produce differences in "disaster." We need to appreciate that some research and writing careers are built on making such tautological differentiations but, in the long run, they add little to our understanding.

Of course, some insight can come from an examination of agent characteristics, especially as they might be predictive of certain characteristics of the social impact. For example, suddenness of impact has meaning for warning, scope of impact has meaning for damage assessment, etc. This would mean that judicious cross classification efforts among various disaster agents allows observation of the "same" effects created by quite different agents.

Physical Impact as Disaster. We have, of course, developed and improved a number of measures related to physical damage. We can measure wind speed and flood stages. We have Richter and Mercalli scales to measure earth movement. We have Geiger counters and gas detectors. We can do body counts and assessment of injuries. We can calculate structural damage to buildings, roads and underground pipes. None of these advances in the assessment of physical damage will add very much to the conceptualization of disaster. The motivation for such improvements, in part, stems from the need for record keeping necessitated by government and insurance companies. There can be some interesting sociological questions as to how assessment is made, processed and settled, since these move directly toward issues of liability,

insurance, replacement and restoration. (It is my understanding, for example, that the current plight of the Small Business Administration and of a number of defaulted farm loans can be directly traced to the "liberal" use of disaster loans which encouraged expansion and greater debt. The default of these loans in turn lead to another "disaster.") In effect, while many of the issues of physical impact have important public policy issues, they have more tangential impact rather than direct consequences for the social sciences.

Social Impact as Disaster. One can argue that this dimension is central to the social sciences and, thus, constitutes the proper study of disaster. Social impact is only imperfectly related to physical impact. A sociological definition should be able to measure "social effects." A death should be translated into "fractured" social relationships; an injury needs to be translated into lost work time and income. Building damage can be translated into the reallocation and substitution of resources. Impact "might" be measured by increased social "disorganization" or at least in terms of . changed and deferred expectations.

While I do not intend here to identify clear directions, Alan Barton (1969) was on the right track a number of years ago when he talked about "collective stress" situations. His "classification" was to try to bring "disasters" into a more inclusive typology which implicitly reflected social impact. So he included such phenomena as war, depression, and drought to fill cells. He also saw some communalities among what others see as disparate in that he was moving in the directions of identifying "social impact." He suggested that collective stress occurs when members of a social system fail to receive expected conditions of life from the system. (Barton, p. 38)

That sounds "good" until we try to imagine what it means and particularly how one might measure "collective stress." What are "expected" conditions of life? Posed in that way, it suggests, in part, the notion of "functional prerequisites" and, on the other hand, some notion of psychological well being. Are people supposed to have predictability and stability in life, the lack of pain and injury, the absence of fear and anxiety, and the fulfillment of aspirations and expectations? If so, then, collective stress is not only anti-utopian but contrary to social reality.

Too, there is the problem of levels of analysis. I do not believe that "collective stress" can be inferred from aggregating attitudes, perceptions, opinions, or "behavioral tendencies." Individual attitudinal data can always be manipulated to give them a sense of reliability and validity they do not deserve.

A shift to "group and organizational level" analysis provides a better approximation of reality but a greater set of problems to seek easily identifiable indicators. Should changes in norms be used? If so, how do we measure? Should we use changes on behavior; the displacement of goals; the cessation of normal routines; the reorientation of expectations? What difference does it make if those changes are momentary rather than long term? What happens when those changes are segmental rather than total? Is a starving tribe in Ethiopia collective stress for Ethiopia or for just the tribe? The problem of identification of the social system experiencing "collective" stress is often not easily identified. For example, was the "location" of the latest Tylenol "scare" in Long Island or someplace else, or everywhere? Is terrorism a Western European problem, a Middle Eastern problem, or a global problem? What makes it global?

Barton took three dimensions--scope of impact, speed of onset, and duration of impact--as "causes" of collective stress and suggested that collective stress involved large, unfavorable changes in the external environment such as natural disasters or internal social disorganization--such as economic and political breakdowns. All of this has a good deal of face validity, but it does not address the question directly of what is the "social impact." Until we can make some progress in defining that, we will have to use agent characteristics or physical damage as very imperfect surrogates for the sociological conditions we infer.

It is unlikely that we will be able to make rapid progress on this. Part of the problem may be our lack of sociological imagination as well as our rather persistent difficulty in developing measures which are not individually and psychologically based. Even if we did, we would have difficulty in convincing others that such measures are more important than body counts or the quantity of rubble. But the point should be reiterated that we need to be able to measure "social" impact and not be content with very, very imperfect surrogates. After making that plea, we need to consider the fourth definition of disaster which, in many ways, suggests that "social" impact may be irrelevant.

Evaluation as Disaster. Regardless of the eventual outcome of the conceptualization of social impact, it is quite likely that public attention and, consequently, political reaction will be directed to those events which are defined so by the media, especially the national and international media. This suggests that we do not need to study events but only the definers of reality. Three recent examples might point to some of the issues and dimensions.

A. Media Coverage of the Ethiopian "Famine." Several years ago, an itinerant BBC film crew was sent on open assignment to Africa to cover certain aspects of a civil war in Ethiopia. Since the conditions for coverage was not optimal and the material for the story was not particularly visual, they spent considerable time filming certain nomadic groups who had been displaced by the war. These groups, living for years on the edge of malnutrition, presented vivid and dramatic footage, which was shown first in the United Kingdom and later in the U.S. Sensing an extension of the story, reporters went to government and private relief officials to ask what they were doing to "solve" that situation. Such an action prompted considerable international activity. Coincidentally, an underemployed Irish rock singer decided he needed to do something, and his idea was then picked up by rock promoters to stage Live Aid--a masterful appeal for other rock stars who had done well to now be able to do good. The results and consequences of this "accidental" filming has lead to the establishment of a relief prototype now copied in Farm Aid and in AiD/AID. It has elevated Geldof to the equivalent moral stature of Mother Teresa as a candidate for the Nobel Prize. All of these cultural consequences came about when a chronic social condition was defined as a "disaster" by the media.

B. Media Coverage of Hurricane Gloria. This last fall, Hurricane Gloria moved up the East Coast of the U.S. rather slowly. This allowed south to north media coverage where successive local reports could be fed into national coverage. The information on the tracking of the storm by the National Hurricane Center became a constant part of every local and national

news show. Neil Frank was on TV day and night providing the latest information. The much-reduced storm, of course, finally moved across Long Island and into parts of New England.

There are two comments I wish to make on this event. First, and with little evidence other than my own observations and conversations, there seemed to be a disappointment that, with all this talk, there was such little action. The actual storm was anticlimactic compared with the coverage and rather than a sense of relief at the lack of effect, there was a sense of disappointment. A second derivative from this might be revealed in the future. While media coverage is essential in the warning process, will future storms be treated by those who need information as potential hype rather than valid knowledge?

C. Media Coverage of the Soviet Nuclear Accident. The most recent example of the definitional process has merged several quite disparate "motivations"--such as network competition, anti-Soviet feelings, anti-nuclear feeling, Ukrainian nationalism, technological competition among companies and nations, etc. In the "absence" of facts, there was the general assumption made that, whatever happened in the Soviet Union, it was a "big one" and that any information provided by the Soviets would be a coverup because their information is really disinformation. Given this distrust of information, but with the assumption that the event and the story was a big one, air time was filled with international experts--many of whom were properly cautious-- while others were devising scenarios emphasizing the stupidity of the Russian system and the calamity of nuclear power. In the absence of "real" information, coverage was not reduced. Consequently, the

extensive coverage added to the existing uncertainty. The point is, perhaps, that the event had less to do with "actual" uncertainty than with the uncertainty portrayed by the media.

The specific illustrations used here are transitory. The major point and the research questions concern the media's role in the definition of the situation. To a certain extent, it is only a variant of the more general issue of the effect of the media, but disasters have their own uniqueness. For example, do media outlets cover and process "news" in disasters in different ways? Is the visual media especially important for the definition of disasters? How does new media technology affect the way in which stories are covered? What is a disaster story? These are more than incidental research questions since the suggestion here is that the media is at the center of the evaluation process. It is able to "control" reactions and responses to events which they, themselves, have "created." The factual basis of those events is perhaps less important for social action than is the media creation.

These are only some of the problems which plague the definition of disaster. Many of the problems are reduced when research continuity is established as, for example, by the Disaster Research Center by focusing on a narrow range of disaster agents and when the "locus" of impact is delineated in terms of a specific community. Difficulties increase when you move across quite diverse agents and when the definition of impact can include deferred damage, such as radiation or genetic change. Difficulties increase when that "impact" can extend beyond several communities into broad national and international consequences which are quite diffuse. And many of these points of differentiation are not the ones which the media uses to select for coverage which directs public attention. Why do certain events get attention

and not others? For example, a major nursing home fire in Ohio is almost totally unknown since it occurred the weekend President Kennedy was shot and the Peshtigo forest fires have been kicked off the pages of history by Mrs. O'Leary's cow. Aside from these issues, there is a continuing need to conceptualize a range of situations and behaviors which produce "collective stress." In order to do that we need to continue to understand those situations and to identify what we mean by collective stress. It was never my intent here to suggest that we need to solve those conceptual issues prior to future research. Conceptualization should never be a barrier, only a problem.

It is more likely, of course, that future research will be conditioned by public policy issues than it will be by the needs to develop some coherent set of concepts. Given that assumption, several topics are suggested, ordered under the familiar rubric of mitigation, preparedness, response and recovery, which might be pertinent in the next several years. These are topics which are timely, rather than enduring.

Mitigation. I have the feeling that policy interest in mitigation is waning. This is due perhaps to the fact that the "costs" are seen as being too high for the possible future benefits. That is particularly true when the direct costs might be absorbed by someone else. In other words, efforts to mitigate threats require some commitment to deferred gratification which is really deferred when you are talking about a 100 year flood. The assumption is made that unfortunate results will somehow be compensated by others in the distant future. The logic here is why pay now when someone else will pay later. Economic and development interests are almost always stronger than safety considerations. It might be useful to study this losing battle although critical cases will be separated in time and space.

Related to this, of course, is the whole question of public conceptions of what are acceptable risks. This topic is certainly not new but neither is it static. Those definitional processes are going on in a variety of communities in which new social movements emerge, coalesce, and develop national networks. Sociologists only have to mobilize their own resources to take advantage of this.

One traditional form of mitigation has been the relocation of population out of risk areas and there has been a long history of dam relocations in American society, many relating to the development of TVA. The extent and the time span involved offer many different opportunities. The same process is evident in other countries, India and the Soviet Union, for example, and would offer interesting comparative insights into forms of government actions and their consequences.

It would seem that one of the "lessons" of the Mexico City Earthquake is that it will be expensive to provide seismic resistant building structures in growing cities in developing countries where most of the growing cities are now located. To insist on such standards would inhibit the development process even more so. It is quite possible that a selective mitigation policy might have to be formulated, not necessarily on building materials, but on building usage. Such a policy would have very interesting social consequences as well as important political reactions.

Preparedness. Certainly, one of the more significant trends in American society has been the move to develop an integrated Emergency Management System (IEMS). That movement, which is based on the results of much previous disaster research, is still fragile since it runs counter to several centrifugal forces. One of these forces is the tendency for occupational and agency specialization as well as agent specialization at the same time when

emergency management moves in the direction of being jacks of all trades and jack of all agents. Too, since the idea of an integrated EMS is not well institutionalized in planning offices across the U.S. it is more often seen only in terms of conventional accident and safety needs. These are significant counter forces and it might be fruitful to study successful cases of institutionalization.

Along with the institutionalization of IEMS, there is another trend which needs careful monitoring as well as careful research. It can best be labeled the "politicalization of emergency preparedness." This politicalization process has had two primary sources linked to a common "agent." The first source has been political opposition to traditional civil defense activities. This opposition has been rather latent but on occasion results in attempts to discredit civil defense activities. Civil defense becomes a target since traditionally its mission has been associated with protective action in nuclear wars. At periodic times, anti-nuclear movements have attacked Civil Defense to note symbolically their perception of the futility to minimize the consequence of nuclear war. This means that communities who engage in emergency preparedness are defined as encouraging nuclear war. (It also means that those who have studied ways of reducing the consequences of threats, for example in studying evacuation, can be "charged" for creating an "illusion" that people can survive. See, for example, Leaning and Keys, 1984.)

Some of these same themes, and perhaps some of the same people, have been active in the argument that any type of emergency preparedness is futile in relation to the threats which emerge from the location of nuclear power

stations. Since in most communities, emergency planning is a low priority item, any viable political opposition to such efforts can be interpreted as sufficient reason not to initiate or not to implement such planning.

In effect then, one of the consequences of social movements directed toward nuclear war and nuclear power has been to undermine emergency preparedness. If one understands the underlying concept of IEMS, this also means that emergency preparedness for the full range of hazards is also undermined. Unintended consequences, however, are still consequential.

Response. One further consequence of the politicalization of emergency preparedness has been a move toward the trivialization of planning. It is not clear whether this is the result of deliberate political strategy or simply the results of planning efforts which anticipates every contingency an opposing lawyer can raise during an administrative hearing. This results in a detailed emergency plan which lawyers will soon forget and responders will never remember. It has moved emergency planning in the direction of rigidity and bureaucratization with the implications of a strong center of authority which can only be organized along para-military lines. Such a move toward detail induces a rigidity when adaptability is needed, a standardization when flexibility is needed, and a single authority when coordination is needed. It is quite likely, of course, that such detailed planning will be ignored when an actual response is necessary so the research question might be, "Is bad planning worse than no planning?" One might argue that, with no planning, at least the responders might accidentally discover an adequate solution. While one might argue that trends toward the elaboration of details characterizes only communities within emergency planning zones for nuclear plants, however, with 101 nuclear plants in operational or

construction stages, this still would affects a considerable number of communities. In addition, the current attention to nuclear plant planning provides an inadvertent model for all emergency planning.

There are still many unanswered research questions on the efficiency and effectiveness of disaster-involved organizations. There are still many questions about the emergence of structures in disaster. There are a series of new questions about the relation of public and private groups, especially in view of the move toward privatization and deregulation which the Federal government has initiated in recent years. There are still a number of critical questions about the role of the media in its dual function of providing information and news about disasters.

There are many questions which go back to the definitional issues raised earlier. Some of those are avoided if one takes the community as the locus of impact and "immediate" effects are the major criteria. But how do we measure, for example, the long-term effects of radiation of those persons who were subjected to fall out in parts of Utah in the 1950s, people who were transient, or people who in the ensuing years have moved? How do we measure the effects of Legionnaire's disease on those who were in a student union or a hotel at a particular time? These were impacts, but on what community? How does one trace out the impact of communities far-distant? For example, when the Quebec bridge collapsed in 1907, 72 were killed, but 35 of those were from a small Mohawk reservation in New York and constituted almost the entire cohort of younger adult males. Of course, all of the questions are rooted in tracing long-term effects, and they have all of the inherent problems of doing research without base-line data or matching groups, as well as all of the problems of longitudinal research which requires the continuity of funding.

Recovery. Recovery, of course, is dependent on effects. And effects are often difficult to determine, especially long-term ones. It is possible that, in the future, the disaster area might be characterized by an increasing number of law suits, perhaps class action suits, attempting to adjudicate "injury." At least part of the determination of "injury" will depend on research findings, thus, there should be considerable impetus for research along these lines. Such legal action might also be encouraged by the increasing withdrawal of the Federal government from disaster assistance. It is quite likely that now there is significant misunderstanding on the part of "potential" disaster victims about their own liability, the liability of private insurers, and the responsibilities of various levels of government. In effect, currently there is an expectation on the part of the "public" that most disaster losses are not "personal", but would be "insured," especially as a matter of last resort. Given such widespread expectation, this is likely to be translated into political pressure on local government which, in turn, will be translated into pressure on "higher" levels of government. The long-standing tradition of federal "assistance" in the event of the lack of resources of lower levels of government provides a standing rationale for efforts for new legislation. The past history of disaster legislation is a testament to the political pressures of the moment. There is no reason why that will not continue in the future.

Future Research in Disaster

It may be useful to make the point that all research in disaster is not necessarily disaster research. It is necessary to reiterate that disasters are the quintessential social situation. Most social processes, which in other contexts take much longer to develop, can be observed in their

entirety. Cultural facades which disguise and distort social processes are often stripped away, at least temporarily. This does not imply a descent toward some biological or "primitive" base, but only suggests that the complexity of the event allows observations relevant to a variety of research questions important to the discipline. My own initial interest in the topic was a by-product of the study of organizations. My impression then was that the prevailing conceptualization, then primarily dependent on notions of bureaucracy, was an inadequate theoretical base for organizational theory.

Subsequent research on disasters has provided the opportunity to look at such general problems as organizational change and organizational functioning. It has moved the organizational and collective traditions closer together to the benefit of both. It has provided considerable insight into the tradition of community research, a long-standing interest in sociology. In studying community organization and community change, it has provided a base to understand social life as process rather than as static analytic categories. It has suggested that earlier conceptions of community power were too status based and thus too artificial. It has provided significant impetus on the phenomena of "emergence" which is one manifestation of the creative aspects of social life which sociologists have in the past ignored as an aberration or as transitory. It has provided new ways of understanding interorganizational relationships. It has also provided significant insights into the linkage between the family and the larger society as well as on the enactment and creation of new roles. It has provided considerable information on decision making, at the individual, family, organizational, and community levels. On the more social psychological level, it has increased our understanding as to how people

listen to others and to the media; how people process information and translate it into action; how people reduce uncertainty and gain mastery; how people deal with fear without being immobilized.

In effect, the argument here is perhaps an ontological one—that disasters are "more real" than most other social reality. Even if that overstates the case, disasters do provide significant opportunity to test more general sociological theories. If such theories do apply in that social context, they will not have sufficient generalizability in understanding other social reality. Disasters are not atypical and aberrant social events, but they reveal the social psychological and structural elements basic to social life.

A Final Caveat

Let me, in closing, point out a potential problem for the future of disaster research. That problem lies at the interface of our disciplinary stance, our public face, and our future research role. The base of the "problem" is in the fact that research in disaster is likely to be implemented in public policy. That possibility is seldom problematic since much (most) sociological work is irrelevant or, at least, tangential to public policy.

In the first place, while sociologists study social structure, they often see it as being problematic. Structure is said to devalue people, corrupt the value of labor, distort personality, create anomie, etc. Those disciplinary themes suggest that "society" somehow is the problem. Even when we construct utopias, we explain why "societies" corrupt them. In effect, many of our theories and our interpretations perhaps inadvertently convey a

"hostility" toward our subject matter, a hostility seldom found in other fields of knowledge. Even herpetologists speak with more affection of their subject matter.

When we take that disciplinary mind set and apply it to public policy issues, we usually posit some social malfunction as the cause of the "problem." This is particularly acute when you do research on disasters, which is synonymous in the mind of the public and of public officials with problems. (The media also defines sociologists as doomsayers and critics. When it wishes for that type of voice, the media call on their local sociologist as their "objective" voice of doom. And, of course, some of us relish that role and enact it with enthusiasm. Our pleasure should be tempered.)

If we subject a practical research area to our disciplinary view that "society" is somehow the "cause" of all problems, our research findings will be cast in "critical" tones, calling for some "restructuring." While those changes may seem to us to be social innovations, they are more likely to be implemented in ways and in forms which distort our original intent. For example, I have been rather aghast at seeing basic ideas taken from the President's Commission on TMI intended to strengthen emergency preparation become trivialized by both proponents and opponents of nuclear power and, as a consequence, translated into increasingly bureaucratic form. This suggests that ideas, even ideas drawn from research, have consequences. We know that intellectually, but perhaps we cannot separate our disciplinary and research roles as easily as textbooks imply. This raises a whole series of questions about our responsibility beyond the formulation of concepts and research questions.

Finally, it may be that the best directions for future research cannot be found in lists of problems. Such lists may be dead-ends or may only lead into those vast intellectual wastelands where many of us are permanent residents. It may be that the best direction for the future is keep a skeptical attitude toward our topic and toward our own work. Leszak Kolakowski, in his discussion of Marxism, made a useful distinction between two types of philosophies--that of the "priest" who perpetuates the absolute and the "jester" who acts as a constant antagonist. The jester "exposes as doubtful what seems most unshakable, reveals the contradictions in what appears obvious and incontrovertible, derides common sense, and reads sense into the absurd." Perhaps that posture is the only clear direction for research in the future.

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