PRELIMINARY PAPER #118

RESEARCH IN THE DISASTER AREA: WHAT IS BEING DONE AND WHAT SHOULD BE DONE?*

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1987

*An edited version of a talk given during the Disaster Research Workshop at the Australian Counter Disaster College, Mt. Macedon, Australia, January 27, 1987.

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RESEARCH IN THE DISASTER AREA:
WHAT IS BEING DONE AND WHAT SHOULD BE DONE

I. Introduction

Having been requested to address in my remarks, first, what interesting disaster research is being conducted worldwide and, second, what important disaster research has not yet been addressed, I will do so.

However, it is my intention to do so by providing a larger context than just simply answering the two questions.

This context involves stressing that what is needed is not simply more research, but different research, and this different research is to be achieved by the exercise of imagination and not by following in a rote fashion or mechanical procedures whether they be called the "scientific method" or something else.

Put another way, we will not get better research by just doing more research. Quantity does not automatically translate into quality. If we are to improve, we will have to do somewhat different research than has been done up to now in the disaster area.

In part, this different and better research will require imagination and creativity. There will have to be originality in the questions we pose and how we seek answers to them. The path to this is not by slavish adherence to a rigid methodology.

Let me illustrate my points about the need for different research and an openness in the way we should go about studying disaster phenomena.

Evacuation behavior has been one of the more extensively studied topics by disaster researchers. Now one of the better data grounded or empirical generalization on this topic is that "when people evacuate, they commonly do so as group members--most typically the group is a family unit" (Drabek, 1986: 114). In other words, individuals do not evacuate; it is family or household units which do. The fact that the great majority of evacuations involve collective entities rather than isolated individuals is of great significance. It is important from a practical or operational point of view that what has to be motivated to evacuate is a small group, not an individual actor. But this is also crucial for research purposes. The unit of analysis and study should be the small group involved. Unfortunately, most studies on evacuation assume individual actors and attempt to work with individual linked characteristics such as age, sex, race and other demographic and solo actor attributes which in no way capture the small group dynamics which are involved in the evacuating unit. As I wrote some years ago, if we keep doing evacuation studies as we have done in the past, the greater number of studies will contribute very little to either our theoretical or practical understanding of the phenomena. More is not enough. Something different is needed.

My example could just as well have been about the disaster behavior of organizations, communities or societies. To study just formal organizations, for example, as if they constitute the basic organizational response in disasters, is to ignore another well established empirical generalization--the emergent nature of much group behavior at the emergency time periods of disasters. Studies which continue to focus on organizations in disasters rather than the organized nature of the response, will produce more of the same and not allow us to see things differently.

Part of the reason I think we have difficulty in launching different and new studies is that very often we have become ideological prisoners. As part of the scientific research community, we have become oversocialized to something which often is called the scientific method. The general idea is that the closer the adherence to what passes for scientific methodology, the better the results will be. That may be true for relatively unimportant findings, but sociology of science and of knowledgeable scholars tells us that is not the way new and different and important scientific achievements are attained. These are scientific mythologies as well as disaster mythologies.

Let me quote from two book reviews. The first is about a book called <u>Sociologists at Work</u> (1964). More than a dozen outstanding sociologists were asked to candidly and honestly report on what they actually did in doing their best field research. I quote from a review:

> Old hands at field research will not be surprised at the reported experiences, but it will be good to force the accounts upon the young and the undiscerning. The Model of True Scientific Research is universally departed from in practice. Real research conducted by the best men of the profession, does not follow the archetype: postulation of clear hypothesis, experimental design, pre-testing, execution of standardized procedures, ordering of findings in tabular or other quantitative form, tests of validity and reliability, and strictly delineated reporting.
>
> Rather, real research is characterized by grave abuses and mistakes in every one of these routines. A piece of pure research according to the model of science is rare. (American Behavioral Scientist, January, 1965)

The review, as well as the book itself, goes on to note that the research findings and conclusions in their studies were significant and important, and essentially argue that the researchers involved achieved major results even though they violated, in almost all respects, the norms of how scientific work is to be done. In fact, both the researchers and the review are clearly taking the position they would not have found what they found if they had let themselves be constrained by ideas of "correct" research procedures instead of exercising their imagination.

Of course the reaction of some might be that it is sociological research that was discussed. What about real scientific research? Let me offer another book review quotation. James D. Watson, the co-winner, with Creek, of the Nobel Prize for the discovery of DNA--the key genetic material--wrote an autobiographical account of his research. The book, called <u>The Double Helix</u>, was reviewed by one commentator as follows:

> "What emerges from Watson's candid account of these frequently amusing, sometimes tragic and characteristically fortuitous events is a picture of the scientific enterprise that is far different from that contained in standard textbook treatments of "correct" scientific procedures or for that matter in the musings of many philosophers of science. No "careful review of the literature," no "operational definitions" of the problem, no framing of hypotheses and no specification of indicators. Not that they were consciously or systematically ignored; rather, we are brought to an appreciation of the relative triviality of those textbook injunctions when set beside the irrepressible and cyclical gropings of intent minds to solve a puzzle that has resisted solution."

Other interesting examples of how major scientific advances were made, especially in the physical sciences, by deviations from the supposedly proper research norms are given in a book by Kuhn called, <u>The Structure of Scientific Revolution</u>.

Of course what these writers are suggesting is not sloppy research or slipshod studies. Rather, they are asserting that good research results, significant findings, and major reformulations are achieved by the exercise of imagination and creativity, a willingness to do things or to look at matters in ways different from what the current research methodology tradition might be at a particular point in time.

I am not promising a Noble Prize to disaster researchers who deviate markedly from the traditional research methodology. Rather, as I said before, let us not become imprisoned by certain methods or procedures. Those, for example, who insist on standard random sampling for most questions relevant to search and rescue activities in disasters may undertake the orthodox statistical procedures, but they will miss almost anything of substantive interest because of the very non-random social chains, links and networks which are at the heart of search and rescue behavior (and which can far better be examined through non-random snowballing sampling). Here, as in many other research questions in the disaster area, the nature of the sampling should be determined by the nature of the substantive problem being studied rather than by traditional ways of doing things.

I will return to this point later, that methodology, at best, is a means to an end. It is not an end in itself. Here I merely want to stress that the better research we need will not be achieved by an unthinking adherence to the way things have been done in the past or should be done according to some ideal conception of a world that does not exist in reality. The different research needed in the disaster area requires researchers with open minds on how to go about their work. It is the end result of research that is important, not the means used to do the research. In fact, means should always be adjusted to the ends sought. Stated in another way, we should keep our research goals in mind in the disaster area and adjust our methodologies to those goals, instead of trying to force studies to follow certain rigid methodologies which might be inappropriate for what is being studied.

II. The Current Status of Research on the Human and Social Aspects of Disasters

In order to indicate where we might go, it is necessary to give a brief indication of where we are now, why this might be a good time for a new research thrust, and what are the options that might be available. My remarks will primarily be about studies of the human and social aspects of disasters. However, my suspicion is that many of my comments are relevant for most students of most disaster phenomena.

(1) The quantity and quality of what is known and unknown

It seems to be an occupational characteristic of most areas of study, for the researchers involved to bemoan the research undertaken in their area. This certainly seems to be true of many disaster researchers. In conferences and in publications and literature reviews, it can be safely predicted that it will often be stated that the existing research is both quantitatively and qualitatively poor.

It is my opinion that such a judgment depends on what is being used as a point for evaluation. Measured against some ideal standard, the work done so far in the disaster area might not be evaluated too highly. From a quantitative point of view it could be argued that we possibly have not yet reached the total of five hundred systematic social science studies of different natural disasters and technological accidents (events, not total number of studies), certainly a very low number relative to the totality of such events which have occurred, or that on many important questions we have only a handful of studies of any kind. Qualitatively, it might be questioned if there is any fully established proposition at all in the disaster area, let alone laws or fundamental models.

On the other hand, and it is my position, a much more positive evaluation of what has been achieved can be reached. Forty years ago, when systematic disaster research was starting to develop, it was possible to cite only a handful of studies in the area, using the word study rather loosely. Current lists of unstudied topics in the disaster area almost always are refinements of major questions which have at least been explored in some fashion. If the criteria used are the number of works done in the last several decades or the range of topics covered, then we obviously have come a long way.

Judgments of qualitative merit can be made using a variety of criteria. However, again, if I use as a basis of evaluation not some ideal norm, but some real world feature such as the quality of the research work in non-disaster areas of study, disaster research would not rank that badly. In terms of such criteria as intellectual sophistication, theoretical models, explanatory schemes and/or empirical generalizations, the field of disaster research does not compare badly with the studies done in some major subspecialities of the discipline with which I am most familiar, that is, sociology.

At any rate, my point is that a case can be made for both the relative, if not absolute, quantity and quality of the research in general undertaken in the disaster area, although it may not be possible to say this of very specific questions (e.g., there are only four studies on the handling of the dead). Instead of glibly accepting the downplaying of what is generally known, we should ask what evaluative criteria are being used, and, in particular, how disaster studies stack up to what has been done in non-disaster areas. Occupational self-flagelation about the poor research in a given area undoubtedly serves some psychological and social functions for the operation of professional communities, but should not be confused with a correct assessment of the situation.

(2) The post-pioneer era and the time for a different research effort

Having said that to emphasize my views about the past and recent research in the area, I do want to note that in a historical sense we are at a possible turning point in the development of the field of disaster research. Put very simply, the second generation of researchers are taking over from the pioneers who developed the area. This presents both a problem and an opportunity.

In many ways the initial researchers had an easier time than will those who are coming after them. In part, since in one sense practically nothing had been explored, almost anything the first disaster students did led almost all of them to come up with "new" findings and observations. As they increasingly ventured into unexamined topics and questions (and some, like those at the Disaster Research Center, did so as a matter of conscious decision) they increasingly structured the research perception of a variety of disaster phenomena, as well as how the phenomena should be studied. This was inevitable, and is not inherently bad or dysfunctional.

However, there is a potential problem created for the second generation of researchers. Students of the sociology of science and knowledge have noted certain typical patterns in the development of scientific subfields. The pioneers in the area open up the field, come to see certain questions as the important ones, reach many empirical generalizations, and grope towards conceptual clarifications, typologies, explanatory schemes and theoretical models. In essence, they develop what is known as a scientific paradigm--mostly an implicit way of looking at and thinking about certain phenomena. However, these same students of the sociology of science and knowledge have noted that sometime a developing field falters as a second generation of researchers take over from the pioneers. The problem appears to arise when the succeeding scientific generation roughly repeats what the earlier one had done. The field does not continue to develop since, in one sense, it has been mined out by the earlier workers.

Without trying to document this, I believe we are at this transition stage in disaster research and there could be a faltering. There will be a problem, especially if the quest for empirical generalizations continues in the same way. It is almost as if there were a principle of diminishing returns operative.

On the other hand, there is an opportunity here as we move into the past-pioneer era. The opportunity is to develop a different research thrust. The quantity and quality of what has been done up to now, might be satisfactory as we indicated earlier. But if present and future researchers try to continue in the same path, there will not be as much payoff. (A possible indication of this is that some researchers in the disaster area think that on certain topics we have enough knowledge, at least for research users, and advocate more research application in place of more research). In my view, while we should build on what we have, we need to strike out as researchers in some different ways than we have proceeded in the initial decades of disaster research.

(3) Possible future paths: methodological improvements or substantive reorientation?

There is one obvious "new" path and it is one that is frequently advocated. This is the argument that the research methodology used in the disaster area be improved. This is such a familiar position (in many non-disaster research areas as well) that I do not think I need to spell it out in any detail. The major thrust of the argument is that there is need for tighter research designs, better sampling, more use of quantitative measures, application of a variable language, and the other standard research features which perhaps can best be seen in the prototype of survey research. Whether explicitly stated or not, it is assumed that the end result will be "better" data and supposedly, therefore, better research results.

As one who had his first real life research training in a major survey organization (the National Opinion Research Center--NORC--at the University of Chicago), the thrust of the argument to advance disaster research by improving the research methodology used, is both familiar and understandable. There is no doubt that a number of the empirical generalizations about disaster behavior we have around, could become more significant if we had some of the parameters survey research results could provide us. Even at the organizational level, where survey results are generally less useful, there are many empirical generalizations which could be far better grounded if we had frequency distributions, for instance.

However, in my view, while improvement in methodology is desirable, it is not the major path disaster research should follow. If we are going to have the different kind of research effort in the disaster area we earlier discussed, some substantive reorientations are far more important than tightening up the methodologies used in disaster research. To tinker with the methodology used is very unlikely to challenge the paradigm of disaster phenomena that the pioneer researchers developed. Such a challenge requires a more direct attack. It necessitates research of a more theoretical nature, which we will now discuss.

III. Theoretical Studies

Some might not consider theoretical studies as being research, but in my view one can only take that position if there is a very narrow equation of research only with data gathering of a particular limited kind. The term itself "research" implies to search again, to look at again whatever is the accepted knowledge at a particular point in time. In fact, it could be argued that theoretical studies are more important than empirical studies, for interpretations of the results of the latter are always dependent on the positions taken in the former. That is, all empirical data have either implicitly or explicitly some theoretical presuppositions which are matters more of logic, consensus, and/or tradition than they are of what some would want to call scientific facts or research results.

(1) The C framework

Let me elaborate on this a bit by talking about what I, for heuristic purposes, label as the C framework. The C framework asserts that in scientific research we are usually trying to make some statements about one or more of four possible aspects, all of which start with the letter C.

Thus, in sequential order, we usually want to say something first about the:

> conditions or circumstances which lead to certain characteristics which will have consequences as a result of the career of the phenomena.

Graphically we have:

> Conditions (C_1)---- Characteristics (C_2)----
> Consequences (C_3)---- Careers (C_4)----

Now the major scientific goals in most cases are to come up with statements about the conditions or circumstances (C_1) which generate a particular phenomena that shows certain characteristics (C_2). We are also interested in the consequences (C_3) once the phenomena is in being. Almost always our concern is with the dynamics of what occurs rather than a static depiction at any point in time, and, therefore, we often want to understand the careers (C_4) involved.

That is the logical sequential framework but, unfortunately, we can say very little about C_1, C_3, or C_4 unless we specify in some way ahead of time what it is that we are interested in studying, that is C_2. We have to be able to identify, or at least indicate, what it is we are studying before we can turn to an examination of the conditions, consequences or careers of that phenomena.

Now C_1, C_3, and C_4 are to a considerable, but not exclusive, extent matters of empirical determination. However, C_2, while it can be informed by empirical observations, is more a result, as said earlier, of logic, consensus and/or tradition. Thus, if we have an interest in disaster phenomena, in the conditions eventuating in certain consequences following certain careers, we cannot do very intelligent studies unless there is some prior agreement or acceptance of C_2 as a phenomena that has certain characteristics. Put another way, the definition or conceptualization of what is a disaster is not a matter of empirical determination in the same way as we can talk of what empirical research has found out about the conditions, consequences and careers of a variety of disaster phenomena.

Let me turn now to the matter of concepts.

(2) Conceptual clarifications

In my Presidential Address to the International Research Committee on Disasters in India last year, I said the major problem facing the disaster research area is that we are struggling with what we should be studying as a disaster. To give a partial flavor of what I was addressing, let me read the first paragraph of what I said under the label of What Should We Study: Questions and Suggestions for Researchers About the Concept of Disaster.

> There is no one in this audience who does not immediately recognize the descriptive referent of two phrases we will utter--the Challenger space shuttle accident, and the spread of the AIDS virus. However, we would venture to say with confidence that the quick recognition of what we are talking about would not be accompanied by a similar consensus that both, one or the other, or neither should be thought of or studied as a disaster. On the other hand, there is probably also no one in this audience who will not only recognize, but agree that the referents of the terms Bhopal and Chernobyl are, and should be, looked at as disasters. Why? Few of us would have trouble characterizing some aspects of the recent Mexico City earthquake or the Amaro, Columbia volcanic mud slide as a disaster. Yet many of us would hesitate to characterize in the same way the Soviet Union military and the guerrilla clashes in Afghanistan, the American air strike on Lybia, or the current war between Iran and Irak. Does the death from the famine in Ethiopia qualify as a disaster? If yes, what about those who are dying daily from cigarette smoking? Why do we and others characterize certain occurrences as disasters but deny this label for other phenomena also involving loss of life, destruction of property and/or general disruption of social life?

Later on in that talk, I raised other examples, such as indoor air pollution, terrorist attacks, contaminated wines and foods, the asbestos problem, the nuclear winter idea, land subsidence, medicinal product tampering, such as in the case of Tylenol and Excedrin, plane hijackings, toxic shock syndrome, and so on. Should all, some, or none be treated in the same category as natural disasters and technological accidents? There is no need to document the fact that those persons identifying themselves as disaster researchers disagree a great deal on what should or should not be included under the category of disaster. There is little consensus in the field.

The issue here is not a matter of semantic exercise. It is fundamental to the very existence of the area and what the field should study. There is something odd about an area of research that lacks clarity and agreement on what is its central referent--the very heart of the phenomena it presumably studies. (As an aside, I might note that actually there are several related problems here, e.g. use of a definition of disasters which does not encompass the phenomena being studied; definitions of such vague natures that phenomena that "obviously" do not belong get included; failures to recognize possible differences between definitions and concepts; conceptions of disasters that include antecedent conditions and subsequent consequences, etc.)

However, I am happy to note that the issue of what is a disaster has recently come to the fore as a central issue among researchers in the United States, Italy and West Germany. It should be stressed that the issue is not one of developing a single conception useful for all purposes--research, operational, legal and what have you. Rather, those scholars concerned with the issue are taking the position that among researchers there should be greater clarity and explicit formulation when the term disaster is used, and that hopefully in time there might develop some consensus on a conception of disaster which is most useful for disaster research purposes. If one conceives of disasters as involving a rather narrow range of phenomena associated with natural disaster agents and technological accidents, and someone else uses the same label to cover a very wide variety of happenings, there simply will be no communication or worse, miscommunication. For example, part of the controversy on the possible negative mental health effects of disasters results from the fact that those who see few such consequences are primarily talking of community type disasters resulting from natural disaster and technological accident agents, whereas the proponents of the view that there are serious negative mental health effects from "disasters" are usually conceiving of disasters as involving a very wide range of personal and collective stress situations. Similarly, those who cite looting in civil disturbances and riots as indicative of the anti-social nature of persons in disasters, are simply not talking of "disasters" as consensus type social occasions which are the referents of those who assert looting is not generated by disaster. The very conception of disaster will structure what is studied under the label and what will be observed.

There are four aspects which those attempting a clarification of the concept of disasters have been especially pushing that I want to mention. For one, there is the argument that as researchers we need to move away from identifying disasters in terms of direct physical effects on victims, resulting in deaths, injuries and damage. Take the question of whether the Three Mile Island nuclear plant accident was a disaster. The older view argues that there were no deaths, known injuries, and very little direct property damage. The newer view, as illustrated in a statement by Paul Slovic is:

> Some events make only small ripples; others make big ones. Early theories equated the magnitude of impact to the number of people killed or injured, or to the amount of property damaged. Unfortunately, things aren't this simple. The accident at the Three Mile Island (TMI) nuclear reactor in 1979 provides a dramatic demonstration that factors besides injury, death, and property damage impose serious costs. Despite the fact that not a single person died at TMI, and few if any latent cancer fatalities are expected, no other accident in our history has produced such costly societal impacts. The accident at TMI certainly devastated the utility that owned and operated the plant. It also imposed enormous costs (estimated at 500 billion dollars by one source) on the nuclear industry and on society,

> through stricter regulation, reduced operation of reactors worldwide, greater public opposition to nuclear power, reliance on more expensive energy sources, and increased costs of reactor construction and operation. It may even have led to a more hostile view of other large scale, modern technologies, such as chemical manufacturing and genetic engineering. The point is that traditional economic and risk analyses tend to neglect these higher-order impacts, hence they greatly underestimate the costs associated with certain kinds of mishaps. (From Abstract of Paul Slovic, Ripples in a Pond: Forecasting industrial crises.)

My own view is that there will be greater research payoff in getting away from common sense notions of disasters and moving to conceptions which stress the social disruption of routine life.

A second idea being conceptually developed about disasters is that we need to recognize the threshold problem. For a long time some of us in disaster research have contended that a disaster is far more than simply a larger everyday emergency; essentially, the view was that it involved a difference in kind as well as degree. In the last few years this problem has been attacked again, including by your colleague, Neil Britten, and the formulations have become far more sophisticated. In fact, I personally have concluded that in the past we have put together under the common label of disaster, two phenomena of again qualitatively different nature essentially what I would call a disaster and a catastrophe. In my views there are noticable qualitative differences between events that disrupt the functioning of a total society or large parts of it, and one that disrupts relatively delineated localities or communities. In those terms, the United States (or Australia) has never had a catastrophe up to now, but earthquakes in Italy and Japan and other meteorological events in certain Latin American countries and some Pacific islands have been more than disasters. There were catastrophes and they pose different research questions.

A third matter regarding the conception of disasters is whether they ought to be defined as events, the traditional view in disaster research, or whether instead they ought to be defined as vulnerabilities in the social order or societal structure. In this case I think the traditional view is the more useful research conception, although I do think the term social crises occasion better conveys what we are defining than the term disaster event. Those mostly West European scholars who criticize the traditional researcher view of disasters as extreme and unexpected events are correct in their view that their conceptualization of disasters as part of the everyday social structures changes the research focus and the questions researchers would ask about disasters. Not incidentally, this newer conception of disasters allows the study of such phenomena as famines, droughts and epidemics which most of the older definitions of disasters cannot even capture under their rubric.

Finally, under attack again is the early exclusion of conflict type of phenomena such as wars, riots, terrorist attacks from under the label of disaster. Again, I personally believe that the degree of intentionality to damage others, which is a defining characteristic of conflict situations, makes them qualitatively different from consensus type crises situations which we and others classify as disasters. This is the traditional view among many disaster scholars and I think there is merit for research purposes to making the distinction. However, the matter of defining somthing or conceptualizing a phenomena in a particular way is not and cannot be settled by empirical data, for the very formulation of the phenomena determines what is included or excluded as observable phenomena.

At any rate, our point is that there is considerable dispute in the last few years with respect to the central concept of our field. The outcome from the contending intellectual forces will define the focus of our research concern. As such, nothing could be more important. Nonetheless, I should note that conceptual clarification is also underway with respect to other major concepts in the field, such as the terms victim, evacuation, hazard and natural disaster to cite but a few. Such conceptual clarification is long overdue and ought to be undertaken with respect to many other key terms that do not already have a technical meaning.

I do think it makes a major difference for research on what we define as disaster, on how we conceptualize the phenomena. It is more than possible that in the long run, as I have discussed in detail elsewhere, that the conception of disaster used for research purposes may differ rather drastically from common sense or even emergency operational definitions of the term. This should not be disturbing if it is remembered that major advances on many biological research questions were made when much of the phenomena of interest was primarily conceptualized in terms of geneotypes (a truly scientific concept) rather than phenotypes (a common sense view).

(3) Disaster typologies

Apart from concern with conceptual and definitional clarifications, there has also been a resurgence of interest in disaster typology or the whole matter of disaster taxonomy. There would be considerable research and practical usefulness if there were a meaningful typology of disasters. As Drabek has recently written, "we only have vague clues regarding a taxonomy of disaster events." (1986:1) Although the first analytical typology was offered more than a half century ago, most current efforts have not progressed much beyond simple and unrewarding distinctions, for example, between Acts of God and human-generated disasters, or natural disasters versus technological disasters. As to these distinctions, I have always felt that all disasters are natural if the opposite of that is supernatural, which, of course, is where the term came from--when Acts of God were dismissed, an abstract nature was substituted as the source to blame. Today, however, we blame men and women and, increasingly, society, with probably as equal a validity for blame for disasters as the past attributions to God and to nature.

What we need for disaster research is a typology based on general dimensions that cut across not only different disaster agents, but also the same disaster agent. As many have said, what is important is not the physical difference between an explosion or an earthquake but the fact, for example, that neither usually allow time for warning. Or, as others have stated:

> . . .a flash flood resulting from a broken dam might have more similarity to a sudden tornado than to a slowly rising Mississippi River flood (Stoddard, 1968:12)

or,

> . . .a flood in Cincinnati for which there may be two weeks warning, is simply not comparable to a flood in Denver with six hours warning, or to one in Rapid City where warnings were received as flood waters entered dwellings (Mileti, et al 1975:5)

or,

> . . .the differences between damaging events due to the same natural or manmade agent may be larger than between events initiated by a different agent (Hewitt and Burton, 1971:124)

If we could develop typologies of disasters based on combinations of meaningful dimensions of social crisis occasions, we could better grasp the commonality of sociobehavioral phenomena across various agent differences and differences within the same agent.

In our view, the scholars working on the typology or taxonomy issue are addressing an important problem. Unfortunately, as we see it, all the typologies advanced and all the dimensions proposed for typological comparisons are very seriously flawed. Often they do not start out with a clear conception of disasters or mix together dimensions that are not on the same level or plane.

In another paper, I tentatively indicated some dimensions which might be used to develop a typology of disasters. Among the dimensions proposed were: (1) the proportion of the total population involved, (2) the social centrality of the affected population, (3) the length of involvement of the affected population in the crisis, (4) the rapidity of involvement by the population in the crises, (5) the predictability of involvement in a crisis, (6) the unfamiliarity of the crisis, (7) the depth of involvement of the population in the disaster, and (8) the recurrency of involvement. Basically we emphasized dimensions of the social crisis occasion rather than any dimension of any agent (since in some cases there is no identifiable disaster agent, such as in famines). I would not defend to the death that the specific dimensions discussed are the crucial ones for research purposes. But I think they do illustrate the kind of direction we need to proceed if we are going to make any headway in developing a disaster typology.

Let me mention two other related ideas. We need at least two different sets of typologies. One is needed to place disasters, however, we end up conceptualizing them within some larger framework of non-routine situations. Allan Barton, more than 15 years ago, attempted to place disasters within a larger framework of collective stress situations. The effort was a worthwhile one and should be looked at by anyone interested in the problem. Regretably, no one has followed up his effort.

The second set of typologies needed would be to handle what we earlier called the threshold problems. A long time ago at the Disaster Research Center, we advanced a four-fold typology which implicitly drew a distinction between accident, emergency, disaster and catastrophe by taking into account organizational involvement. If only established organizations get involved there is an accident. If latent emergency organizations such as the Red Cross and public utilities get involved, we have an emergency. If, in addition to expanding, there is also the involvement of extending organizations, we have a disaster. A catastrophe is indicated by the emergence of new groups also.

Again we would not defend this formulation at all costs, but the notion of using sequential organizational and group involvement in response might be re-examined for how well it handles the threshold problem and the need for a typology of what today is often all lumped under the category of disaster. One merit of this kind of formulation is that it uses part of the social response pattern at the community level and not non-social features such as casualties or material destruction.

Whether it is a typology similar to what we have discussed or some other taxonomic scheme, we need to give priority, for research purposes, to this problem. We need to distinguish between different types of disasters and we need this soon. Again as in the instance of conceptual clarification, much of the work required will be of a logical or analytical nature. Empirical data can only be used to illustrate the types. That is not as true of the next kinds of theoretical studies we need.

(4) Explanatory models

As implied earlier, we need explanatory models for the basic purpose of most scientific research is to explain or account for the phenomena being studied. I do not intend here to discuss specific explanatory models or to suggest new ones. Contrary to what is sometimes said, we have some such models around with respect to such topics as the diffusion and effectiveness of warning messages, the emergence of new systems for mental health service delivery after disasters, and the conditions that create the kinds of community preparations for and responses to acute chemical emergencies that have been observed. There ought to be more such kinds of middle range explanatory models. Tom Drabek recently suggested four major candidates for model building, namely: (1) family evacuation behavior; (2) post-event family adjustments; (3) hazard insurance purchases; and (4) community adoption of hazard mitigation. What I want to stress, and I think I see more of it occurring in disaster research, is how such model creation might be facilitated.

Current disaster research has in the main come out of three general intellectual and disciplinary streams. The area of disaster study was initially created out of an interest by social scientists, mostly sociologists, in natural and technological disasters. This primarily sociologically oriented research stream first focused on the behavior of individual victims and gradually shifted to a focus on organizational and community behavior in the preparatory and response stages especially of local community disasters. This stream was early augmented by the work done by social geographers primarily concerned with natural hazards. Much of this research initially focused on the perception of hazards and land use choice. Later it turned also to questions of how the research results could be used. The last stream, and it is a recent one, has come from researchers interested in risk assessment and risk management. With a focus especially on manmade or technological hazards, interest has been on how societal decisions are reached concerning the relative safety of nuclear plants, food additives, medicines and so on. Research by those in this stream seems to have been considerably intensified by major disasters in the nuclear plant and hazardous chemical areas, as well as product tampering and contamination.

I mention these three major streams (and there are some others of more limited scope, such as recently out of the public administration area) simply to make the point that disaster research does not need to create from nothing its explanatory models. The disciplines involved in the three streams have scores of models about behavior at all levels. For example, there really is very little with respect to organizational behavior in disasters for which there does not already exist theoretical frameworks and models in sociology, political science and public administration which attempt to explain organizational behavior in general. The models are already in being. Independent of any disaster research, sociologists have models about the limits of organizational rationality. Thus, when disaster researchers found these limitations in disaster relevant organizations, there should have been no reason to develop a model to account for the finding. I say "should have been" because often those working in the disaster area have failed to take advantage of the already existing formulations in their own or other social and behavioral science disciplines. The situation in this respect has somewhat improved in recent time. For example, a volume in the process of being published documents how the field of sociology contributed to disaster research.

My general point, of course, is that disaster researchers should make more explicit use of models already available in different disciplines. This does not mean that the intellectual flow is, or should be, one way. In fact, analysis have been made of how disaster research, for instance, has provided important intellectual contributions or feedback to the field of collective behavior, a speciality area within sociology. Our point here is that disaster researchers should take advantage of what is already in being, and ought to avoid, as they sometimes do, giving the impression that the particular research problem or question they are addressing has not been at least generally looked at already. After all, when all is said and done, disaster behavior is human and social behavior, and the latter has been the focus of attention of major disciplines for decades. At the empirical research level, the wheel is constantly being rediscovered by disaster researchers who fail to adequately review the literature. There is even less excuse for reinventing theoretical wheels since almost all disaster researchers have been trained professionally in some social or behavioral science discipline.

(5) Codifications and propositional inventories

A possible reason, although not an excuse, for rediscovering the wheel is that the disaster literature is widely scattered and often in obscure or non-mainstream sources. However, there have been both general and particular topic generalization efforts. They take the form of both codifications and propositional inventories. A most recent example is the volume produced by Tom Drabek entitled, Human System Responses to Disasters: An Inventory of Sociological Findings. It draws from nearly 1,000 different research publications. As its subtitle indicates, it does not pretend to summarize everything, but it is undoubtedly by far the most systematic and extensive codification effort made by anyone in the disaster area (although your own Prof. Blong has done a somewhat similar effort on almost anything ever written from a scientific viewpoint on volcanoes).

Apart from noting the existence of inventories, I would like to make one other simple point. For a variety of understandable reasons, the works produced generally report only research done by American researchers in the United States. A sort of cross-societal testing of the produced inventories could be done in non-American societies which have a disaster literature of their own. We desperately need to find out in disaster research how universal and how cultural bound our research results are.

Thus, to summarize, we have indicated a number of matters in which interesting theoretical work is presently going on, namely conceptual clarifications, the development of typologies, the setting forth of explanatory models, and the putting together of codifications and propositional inventories. I have suggested what might be particularly worthwhile doing. Although my view is that this kind of theoretical work should have the highest priority among disaster researchers, I recognize that many have more of an interest in empirical data-gathering--not central to the theoretical studies mentioned--so let me turn to talking about empirical research on disaster phenomena.

IV. Empirical Studies

(1) The agent specific versus the generic approach

A major decision in doing empirical research is whether the study should be agent specific or more generic. That is, research could be on, for example, people's purchase of flood insurance, on community emergency preparations for forest or brush fires, on the provision of welfare relief after a major chemical disaster, or on the problems involved in the reconstruction of housing after an earthquake. The research, in other words, looks at whatever disaster phenomena is being examined in the context of a specific or particular disaster agent such as cyclones, dangerous radioactive material, tornados or toxic chemicals. In contrast, research might be conducted on the phenomena in a generic sense apart from any specific disaster agent. Thus, studies could be done on the post-impact delivery of emergency medical services on the interorganizational problems in planning for disasters, on the long run psychological effects of undergoing disasters, or on public beliefs in their possible exposure to disasters.

Disaster researchers have gone both ways--some do primarily agent specific research, others, probably the majority, do generic research. However, if the situation is looked at more closely, what surfaces is interesting. The great majority of disaster researchers in the social and behavioral sciences clearly seem to believe that a generic approach is both more valued and more useful. Thus, for instance, it is argued that the warning process is fundamentally the same and it does not matter if the agent is a cyclone, flood, dangerous chemical or a fire. Similarly, for example, organizational problems in responding are essentially the same for all agents, be they those involving an explosion, a destructive wind, an earthquake, or a cloudburst. More generally, the argument for a generic approach to disasters is one well set forth by Tierney. She once wrote,

> . . .regardless of the characteristics of a particular disaster agent and the specific demands generated by it, the same kinds of community response related tasks are necessary in (all) kinds of disasters and for all disaster phases. In any community, for example, the assessment of hazards and the aggregation of disaster-relevant resources are necessary, regardless of the specific hazards and resources in question. Similarly, post-impact communication and decision-making procedures must be planned for and activated in <u>any</u> community crisis.
>
> To draw an analogy, a battle on land is fought with different weapons, material, personnel, and support systems, than those used in sea battles, but, nevertheless, the general overall battle requirements are the same for both. In both cases, intelligence about enemy strength and movements must be gathered, resources must be collected, trained personnel must be led effectively, and so on. The same is true of disaster planning; although disaster agents and the human and material resources needed to respond to them may vary, the same generic kinds of activities must be performed in the predisaster, preimpact, response, and recovery periods, regardless of the specific threat (1980:18-19).

While the view that both disaster planning and disaster research should be generic is widespread, and one I totally agree with, it should be noted that a generic or all hazards approach creates difficulties and problems for research of an inter- or multi-disciplinary nature that involves other than social and behavioral scientists. This is because physical and biological scientists and engineers, for the most part, are and have to be agent specific in their disaster research. A seismologist, for example, knows about earthquakes, but is very unlikely to know anything about floods, tornados, volcanos, toxic chemicals, fires, and so on which are, individually, the separate province of some specialist. The knowledge required and the research undertaken will necessarily vary drastically depending on the geo-physical, meteorological, chemical, nuclear, etc. agent involved. While this does not argue against multi-disciplinary disaster research teams, it does indicate an issue which needs to be considered when they are planned or established.

If we leave aside the agent specific versus the generic approach issue, we could divide up the range of actual and possible empirical studies in a variety of different ways. A standard distinction is to divide the research into four major categories in terms of the questions being asked about different phases or temporal dimensions of the life cycles of disasters. Thus, studies can be done about the activities involved in (1) preparing for disasters, (2) responding to disasters, (3) recovering from disasters, and (4) preventing disasters. While I will use this four time/phases distinction myself, and it is widely used by disaster researchers around the world, I think there are some serious problems from a research point of view with the formulation. It is drawn from the perspective of operational personnel, indicates linear time is a very crucial variable, and implies there are major substantive differences between the four phases. (Those who graphically depict the four phases in a circle in my view set forth a better picture of what is involved.) These are not necessarily important or valid views for anyone doing disaster research. A division of studies along lines more useful for disaster research and analysis would be desirable (e.g. in terms of a new disaster typology).

Another point to note, and it is partly derived from disaster research, is that the prevention or mitigation phase is listed last. In a more logical formulation it might appear that it ought to be ranked first; prevention of disasters to many would seem to have higher priority than reacting to their occurences. However, research has shown that disaster mitigation has low priority in the real world, is not always totally defendable even in cost benefit terms, cannot always easily be distinguished from preparedness measures, and tends to lead to viewing disasters as simply another social problem of societies. While much more could be said on all these matters, we will leave them with the following related statement. There is the beginning of the idea in the disaster research area that mitigation is not necessarily a self evident, high priority goal. Few would question the need for research on disaster prevention. Some research might be conducted on the advantages of generally foregoing mitigation in favor of the preparedness, response and recovery activities necessary if a disaster occurs, a stance implicitly taken by many individuals, organizations and communities.

(2) On preparedness/protection

Let me now mention five topics or questions with respect to preparedness and protection that either are in the forefront of current disaster research and/or that, in my opinion, should be.

(A) The more that local community disaster planning has been studied, the more it has been realized that such a situation reflects local conditions and is actually functional. An important implication is that efforts to impose one model from above or to try to force a standardized format across-the-board is an inappropriate policy strategy. The qualification is that this is probably directly related to the degree of centralization of the central government, with the less the centralization the more likely and the more functional variation in local community disaster planning. This might be an interesting thesis to test out in Australia because it is almost at the opposite pole of Japan with the United States falling in the middle of the two.

(B) Belief in the disaster myths seem widespread both among the public at large and community officials. American and Australian studies support this generalization. Assuming its validity, however, raises a question in my mind why the belief does not translate into more active public support for disaster planning; believing in the myths would seem to suggest people ought to be strongly in favor of planning for disasters. While disaster planning is seen as a legitimate undertaking, it is neither salient nor of high priority in the thinking of citizens and officials. Research might better establish why this is the case.

(C) A "command and control" image as the ideal planning and response model seems widespread around the world, especially in governmental circles and many emergency organizations. Disaster research has clearly shown that this is not an appropriate model or prototype, and I do not think this finding needs much more documentation. We do need studies of why the image is so prevalent and what factors would discourage its continuing existence.

(D) A growing body of research has increasingly supported the idea that planning is only one element that feeds into managing a disaster, and that there are principles of disaster management different from disaster planning. We need more studies about the difference between the two and particularly the development of a set of principles of disaster management, equivalent to the principles of disaster planning which have been derived from numerous disaster studies.

(E) Some of the more recent studies in the disaster area have emphasized the need to build into disaster planning exercise and training, and the skill to improvise since coping with the emergency period of time requires coping or adjustive capabilities. Too much planning, exercise and training instead imply fixed ways of responding. We need research on how to build improvisation into the whole process; perhaps we can learn something from toy makers who these days try to build toys which teach children how to think.

(F) Finally, in very general terms, we know very little about disaster planning in the private sector although, obviously, it could be very important. Research on this topic is necessary.

(3) On response/management

(A) A topic which has increasingly been studied is the non-victim, that is such persons as emergency organization first responders, volunteers, relief workers, etc. The questions which are being addressed range from motivation for involvement, through on-the-scene role playing, to long run psychological consequences. There is much here that is worthwhile learning.

(B) Another major research thrust, in which some collaborative cross-societal studies have already been done, has been on the operation of the community mass media system in disasters. A picture is emerging of the system activities as a whole, the differential response of different media, and the relationship between everyday news reporting and the reporting of disaster news. There exists research designs which could be applied anywhere, and it is one topic on which comparative societal studies can be done.

(C) Although some work is being done, we need to accelerate our studies of the key local emergency organizations in disasters, and particularly their interorganizational relationships. We need a clearer picture of police and fire departments, the public utilities, the more important relief agencies and the major governmental units. The research needs to be of an intra, inter and supra organizational nature.

(D) The study of the delivery of emergency medical services (EMS) in disasters is about to take off. There is a need to clarify the qualitative differences between everyday EMS and disaster EMS, and the relationship between the social organizational and the more medical components of the system. This is a topic on which multi-disciplinary field teams might be able to work together with relative ease.

(E) Research on emergent groups of all kinds has and is being conducted. We now know much more about the nature of such groups and the factors which generate them. However, we still lack an overall picture of the phenomena.

(F) The operation of political factors in the decision-making of emergency time periods requires systematic study. Because research of this kind is especially sensitive, it probably has not been studied as much as it should have been, but the topic would appear to be very important.

(4) On recovery/reconstruction

(A) Despite some research, our picture of the general recovery process, especially reconstruction, is not that clear. There could be studies on this at different levels--local, state or regional, and national. Differential local outcomes from the same disaster has been reported from Italy and Latin America, suggesting a possible initial research question and design.

(B) All kinds of assertions are made about the response of the elderly to disasters, but there is very little research evidence of any kind. However, in my view, any study would be better organized around different life styles rather than chronological age levels, the latter being a rather pedestrian approach.

(C) Some research on the consequences of disasters for families indicates the outcome is affected in complex ways. The positive outcome of undergoing a disaster has been given some attention. Studies already done indicate the need to take different forms or styles of family life into account.

(D) Social class differences are very influential in all aspects of life in all societies. However, this is clearly one of the underdeveloped disaster research areas. There are almost no studies on the topic. While the phenomena could be looked at in any of the time phases of disasters, we think there might be more payoff in research on the recovery phase.

(E) After disasters, there usually is much talk of organizational and community changes as a result of the event. There has been work done on the topic, but the conclusions reached are far from definitive. There is a need to more clearly document in what ways disasters do or do not contribute to social change.

(F) Some scattered research suggests that when everything is considered, economic losses in disasters are not as high as they initially appear. Further examination of this idea would seem warranted.

(5) On mitigation/prevention

(A) There have been numerous hazard perception studies of the general population in many places around the world. Perhaps more informative work on this topic could be generated by focusing more on the community power structure itself. Furthermore, these studies could attempt to ascertain how direct or indirect disaster experiences may or may not influence hazard or risk perception.

(B) The adoption of hazard mitigation measures has also been fairly continuously studied with some interesting results. They suggest complex relationships between interest groups, risk perception and other factors. There is much here on which future research could test and build upon.

(C) Using public education and the school system to impact information and knowledge about disasters has long and widely been advocated. In fact, we know little about what information is best imparted, the most effective ways to transmit such information, and which audiences are most receptive. We need such research data because, generally, similar non-disaster educational campaigns do not have much of a success record.

(D) On the basis of some studies we did of disaster-oriented emergent citizen groups, I would suggest that research of the factors influencing hazard adoption and disaster preparedness measures at the neighborhood level might be worthwhile. The importance of pre-existing social ties and links ought to be especially explored.

(E) There has been a general drift away from structural to non-structural mitigation or prevention activities. While I think this is a laudatory trend for a variety of reasons, it is not clear why it has occurred. Research leading to an understanding of the factors involved would be of considerable usefulness.

(F) The adoption of hazard insurance has received some attention, but how various factors influence awareness and purchase had not yet been systematically established. A seminar held at the Center for Disaster Studies at James Cook University a few years ago advanced many ideas worth pursuing in a research effort.

V. Research Application Studies

The work that most disaster researchers undertake is usually not immediately useable. For reasons I indicated in my last address here, this in itself is not necessarily bad. Researchers have enough to do simply to come up with research findings. Therefore, as we suggested in that previous talk, there ought to be others providing a bridging role between researchers and research users. The responsibility for making the translation of research should be in persons playing the bridging role.

However, all this is easier said than done. We need research on how research can be applied. There are really very few studies on this in the disaster area. Here I merely want to point out three problems which need to be addressed.

(1) The limits of the operational perspective

Many potential research users come out of operational areas in emergency organizations. From their perspective, they frequently have specific questions about how they might improve their planning and/or response for disasters. Their questions are almost always rooted in a very real world.

However, it does not necessarily follow that their questions are the most important or relevant ones to ask. Like any organizational encumbent, the perspective will tend to be rather narrowly focused and usually not very sensitive to a larger framework. Researchers often, although not always, can bring a broader point of view to bear and can see that the operational questions being asked would not be the most useful ones or that would have the greatest payoff.

We need to learn how to transform narrow operational questions into broader questions, perhaps slicing the world in a different way. We also need to learn how to show that a broader perspective can be more useful in the long run. This cannot be done by intuition or common sense. There is the need for systematic studies that will indicate how the bridging roles we suggested as necessary could reformulate questions and enlarge perspectives. Put in other words, we need research that will tell us how the bridging role can be effectively played.

(2) The problem of knowledge diffusion and transfer

At another level, in order to increase research applicability, we need to know what kind of institutional arrangements are needed. As a simple illustration, this College, this kind of conference, are, of course, the kinds of structures that undoubtedly contribute not only to the improvement of disaster research in Australia, but also to the diffusion of information and knowledge to disaster research users. Generally, we ought to have a much more systematic picture of the best mechanisms and structures that can be developed.

(3) The heterogenity of research use and research users

Finally, as I also mentioned in my last talk here, it is very important to keep in mind that disaster research can be used in a variety of rather different ways, and that there is considerable variation in the goals and needs of research users. I gave some illustrations last time both of the heterogeneity of research use and of a similar one on research users. However, here too we need a much clearer picture of what is involved. The research on this is very limited. Studies could throw some light on the blurry picture we now have of this matter.

VI. Changes in the Future and Research Needs

(1) The future is not the past repeated

One of the empirical generalizations from disaster research is that community disaster planners and emergency operational personnel tend to plan on the basis of the last major experience they have had in their locality. The last disaster is taken as the basis for planning. Seldom are preparations made in terms of more or different future disasters which might impact the community. Researchers fault planners for looking backward rather than forward.

Unfortunately, to a considerable extent, the same charge can correctly be made about disaster researchers. They tend to take past disasters as prototypes for which to plan studies of future disasters. In our view, they must instead attempt to project how disasters in the future might differ, quantitatively and/or qualitatively and plan their research accordingly.

Researchers ought to be ahead of what will happen rather than running to catch up with what has occurred. Such a projection of future disaster possibilities is in fact one of the way disaster studies can contribute to public policy and emergency planning. Even with our present state of knowledge, it should be relatively easy to indicate to interested parties why it is inevitable we will have more and worst disasters and how it is equally certain we will have newer kinds of disasters in the future. Hopefully, we can be seen as realistic forecasters rather than scientific Cassandras.

(2) Several important trends for future disasters

Since I do not want to preclude you systematically exercising your scientific imagination, I will only list a few examples of current trends that are telling us something about future disasters.

We have moved into a world where the risks and hazards can increasingly impact far in time and space from their original source. The chemical contamination of the Rhine River in Switzerland in time affected all the countries in which it flowed. The nuclear plant accident in Chernobyl with its effects within the Soviet Union and Western Europe that may last for generations is, of course, the kind of future disaster some of us in the disaster area pointed out would occur long before it actually happened (I publically said it before the Three Mile Island incident, and restated it afterwards saying that in a second case we might not be as lucky). Even more illustrative of the kinds of future disasters with distant impacts is the PCB or pesticide contamination which got into the ecological food chain in Michigan in the United States where we now have daughters of mothers who had lived in the area exhibiting signs of exposure; literally, a second generation has been directly affected.

As another example, we might note our increasing dependency on computers. The range of implications of this for those interested in disasters has not been thought through. For instance, computers along some lines will allow better monitoring for potential risks and hazards; but at the same time they almost insure an information overload on those using them creating the classic dilemma of what is the most relevant information for those using them in disaster warning systems. Also, many computers are parts of interactive systems and networks which means that there is the potential that relatively minor trouble at one point may have major chain reaction consequences elsewhere. Apparently the malfunctioning of one or two key computers in major California banks could have serious repercussions for the international banking system within several hours.

As a last example, we might note that we are moving into a biotech revolution, particularly with respect to recombinant DNA (genetic engineering) which will allow scientists to mix and match the genetic blueprints of bacteria, animals and plants. In one sense, we are at the threshold of being able to custom design living organisms. As new plants, engineered animals, and eventually genetically manipulated human beings are created, the probability that something will go wrong is certain. One does not have to project a Dr. Frankenstein scenario to forecast that organisms will be created which will have major and, perhaps initially, undefendable negative effects on human beings and societies.

The future disasters I have indicated do not, of course, exhaust the possibilities and probabilities (e.g., it will be interesting when a terrorist group will get hold of some nuclear or radioactive material). My examples are to make the point that we are not talking about science fiction speculations. There are all kinds of new disaster potentials which are here, and which the disaster research community should start to address. It may seem odd to talk of disasters coming out of high technology developments when we still have famines and plagues in the world, but that is the reality of the situation.

Let me conclude my remarks by saying the following, and it is of a rather different nature than what has come before. I have heard Australian researchers bemoan the state of disaster research in this country--a perennial occupational negative self assessment of most researchers in most places as I suggested earlier. But as an outsider, what strikes me most about the Australian disaster research scene is that it is one of the handful of countries around the world where there is a critical mass of disaster researchers, where there are some institutional structures in place supportive of a research effort (such as the activities of this College), and where there is the start of a body of studies rooted in the local scene (i.e. Australia). As such, it seems to me that you collectively have the potential for a major takeoff in disaster research. I would urge you to turn that potential into an actuality. The short run benefits may be mostly for Australia, but in the long run we, everywhere, will benefit.