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AN ASSESSMENT OF CONFLICTING VIEWS ON  
THE CONSEQUENCES OF COMMUNITY DISASTERS  
FOR MENTAL HEALTH\*

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## Focus

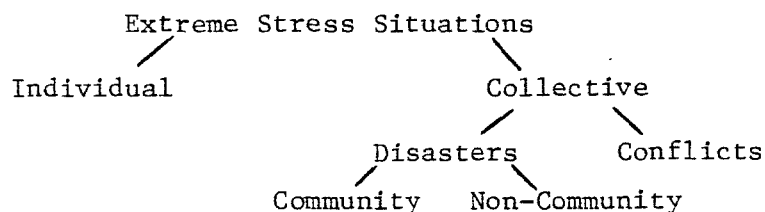
In the early 1980's, there is much controversy in the United States in connection with the opening and reopening of nuclear power plants, and the siting of or the living next to hazardous waste sites. While many issues are involved, a central one is the question of the psychological impact on the affected population. If the Three Mile Island plant in Pennsylvania were to be started up again, would there be negative mental health consequences for the area residents? Have there been deleterious mental effects as a result of the exposure to hazardous wastes in the Times Beach, Missouri situation? Public hearings, court cases, and mass media accounts associated with these and similar events openly state and speculate about both the short and long term consequences of such actual or potential stressful situations on the mental well-being of the people most directly involved.

These examples highlight a very important theoretical and practical question, namely--how well or how poorly do actual and potential victims respond to extreme stress situations? The answer to this question has crucial implications for public policy in emergency planning and management and tells us much about the basic nature of human beings and societies. What is the answer to the question? What is the quality and quantity of the psychological response to extreme stress?

We should preface our answer by noting that in this paper we are not dealing with all stress situations, individual and collective. Rather our focus is on collective stress situations, and within that category, on disasters, and within that class, on those of a community nature. Our conception of collective stress follows from Barton who sees it as a situation when "many members of a social system fail to receive expected conditions of life from the system" (1970:38), and who gives as examples of such situations: pogroms, depressions, famines, civil wars, mass purges, floods, economic declines and ghost towns, reigns of terror, earthquakes, atomic wars, deaths of heads of state, hurricanes, air raids, droughts, blackouts, deteriorating slum neighborhoods, etc. We view disasters as an imbalance in the demand-capability ratio in a consensus crisis occasion, separating disasters out as this kind of social phenomena from disasters viewed as physical agents, physical impacts, assessments of physical impacts, social disruptions from physical impacts, social constructions of crises which may not involve physical impacts, and political definitions of certain crisis situations (for a discussion of the prevailing major conceptions of disaster, see Quarantelli, 1982b). In our formulation, disasters are ad hoc irregular occasions which involve a crisis--there is relative consensus things have to be done but the wherewithall available is not enough to meet the demands in the occasion; those consensus crises which involve a community are of major interest to us.

It is important to note these matters for we shall later argue, part of the controversy which prevails in the area stems from a failure to distinguish individual and collective stress situations, and within the latter category to distinguish disasters from conflict types of happenings such as wars and revolutions, civil disturbances and riots, terrorist and hostage taking attacks, and other potential traumatic events (for a discussion of consensus versus dissensus or conflict types of crises, see Quarantelli, 1970). In addition, not all disasters disrupt communities and as such there are disasters which

are not community disasters (e.g., many transportation accidents). In graphic terms, the distinctions we are making can be depicted as follows:



What is known about the psychological responses of human beings to sudden and unexpected great stress such as would occur in a community disaster? The answer is both a little and a lot; a little in the sense that there is no agreement about the answer in the relevant research and literature; a lot in the sense that now enough has been done and said so that a systematic assessment of the two conflicting viewpoints can be made.

Some students of the problem, seemingly the statistical minority, see the mental health effects of disasters as widespread, deep, persistent, long lasting, and dysfunctional, and that the consequences are as drastic and negative and quite similar to what can be seen in other individual and collective stress situations. The apparent majority of students of the question only appear to agree that there are immediate widespread effects. Their position is that much of the reaction is surface, non-persistent, of short duration, and not behaviorally dysfunctional. They further argue that possibly unlike in other kinds of individual and collective stress situations, community disasters may often actually generate significant positive psychological effects. For purposes of stating the positions, we have stated them in Weberian ideal type terms, that is, in their purest extreme form, although in our judgement, in this particular controversy, the actual is closer to the ideal than is often the case in many scientific disputes.

We shall not only indicate the essence of the two approaches and the kinds of data they present, but we will also suggest some of the reasons for the difference in opinion. That will be the heart of our paper. However, before zeroing in on the central issue, we think it would be helpful to see the matter in a larger context, and thus, we will provide some background information on disaster studies of a social and behavioral nature. A lack of understanding of the history of disaster research can lead not only to more reinventing of the wheel, but worse, to poor and faulty scholarship including the passing on of incorrect or dubious statements, or ignoring relevant past studies and literature. Such lack of historical knowledge and poor scholarship may be more of a problem in disaster studies than in many other areas of research. This is because while the field is in its second generation, the great majority of those currently studying disasters come from other theoretical backgrounds and interests (for a partial history of the field, see the articles in Wright and Rossi, 1981). A consequence of this is much intellectual importation from outside, which is not necessarily bad but it can lead to seeing only what one brings to a study from non-indigenous sources. There are also very few scientists working in the area who have spent their professional life researching in the area, which is not the normal career/work situation for

most specialists in most research disciplines. While this more readily allows for the application of new perspectives, it is usually at the cost of not perceiving subtleties and important distinctions which any veteran worker in any area can bring to bear.

We will try to be as fair as possible in detailing the two points of view about the consequences of disasters for mental health. But we do not come to this presentation devoid of a professional judgement. Our position is that one of the existing views on the question does not fully recognize the implications of what has already been learned about behavior in community disasters, and holds partially mythological as well as incorrect ideas about disaster occasions.

### Background

There was almost no work done on the social and behavioral aspects of disasters until after World War II. Prior to that time apart from historical accounts which go as far back as the description of a plague in ancient Athens by Thucydides (1934:109-114), the corpus of the literature consisted of a handful of empirical observations (e.g., Prince, 1920; Slade, 1932, 1933; Prasad, 1935; James, 1942) and a few speculative essays (e.g., James, 1911; Carr, 1932; Kutak, 1938; Sorokin, 1942). Thus, it is not surprising that the post-war disaster studies could not be born out of this almost non-existent background. Rather research on disaster topics came into being indirectly because of a more primary interest in conflictive types of collective stress situations, although if earlier studies on the latter kind of phenomena had been seriously attended to, little might have happened.

World War II stimulated a number of studies of civilian behavior under the extreme stress of wartime bombings. Some were very systematic and social science guided, such as the immediate post-war, large scale, sample surveys, of the German and the Japanese populations which had been subjected to air attacks (see the multi volumes of the U.S. Strategic Bombing Survey, 1945-47). Semi-social science research was also undertaken on individual behavior and organizational problems in Britain during the war (e.g., see Titmus, 1950).

The results reported in these studies were considerably at variance with pre-war expectations and prevailing views on the behavior of people under extreme stress situations. For example, the research showed that the civilian populations in all the countries reacted remarkably well to wartime attacks and problems. There was not the widespread personal and social disorganization that had been predicted before the war. A few of the empirical findings were that morale remained generally high, mental disorders did not significantly increase, panicky evacuations did not occur, anti-social and criminal behavior did not markedly escalate, and suicide rates went down (much of this research is summarized in Janis, 1951).

In addition, certain little known case material, demonstrated a picture of impressive stability and resiliency on the part of very heavily stressed bombed populations in certain British cities and in Hamburg, Germany, as well as among the survivors of Hiroshima. In the latter case where more than 60 percent of the population was either killed or injured, the less than 40 percent left immediately tried to reestablish the routines of life. Little overt panic, disorganized activity, or anti-social behavior occurred. The day after the bombing, survivors from the 12 banks got together and resumed banking services,

one of the few standing steel companies resumed activities when 20 percent of the employees reported to work, the trolley lines were completely cleared and some electric service was restored. The next day there was restoration of railway services and some of the phone system. The tremendous physical destruction was not matched by any comparable social disintegration (Fritz, private communication; see also Janis, 1951).

Fortunately for the later development of disaster research, the results of the wartime stress studies were neither particularly nor widely noted. In the late 1940's, various federal government agencies became concerned about how the American population might react to atomic, chemical, and other newer kinds of wartime threats. Ignorant of the findings about collective stress responses during World War II, they visualized the worst of scenarios, and asked what planning could be undertaken to prevent or minimize the personal and social disorganization they assumed would occur if the United States were directly subjected to attack. Afraid of widespread "panic," the agencies turned to social and behavioral science research for help (see Kreps, 1981 for some of the history of the earlier funded studies particularly those undertaken in 1952-1957 by the National Academy of Sciences Committee on Disaster Studies). They reasoned that perhaps it might be possible to extrapolate findings from peacetime civilian disasters to potential wartime stress settings. Studies of disasters were therefore supported in about half a dozen universities, with the major initial research effort being at the National Opinion Research Center (NORC) at the University of Chicago during 1950-1954 (which was our own first professional involvement in the area). The NORC work involved the first interdisciplinary team effort conducting field research on a variety of disasters such as earthquake in California, tornadoes in Arkansas, fires in Chicago, and a gas explosion in a Rochester, New York suburb (NORC studies are summarized in Fritz and Marks, 1954).

Very important was the initial starting point, the work undertaken, and the major conclusions independently reached by the half dozen centers of disaster studies around the country (apart from the NORC studies, other early research results are reported in Logan, Killian, and Marrs, 1950; Chapman, 1954; Powell, 1954; Demerath and Wallace, 1957; Fritz, 1961). All started with the idea that the major social and behavioral problem in disasters was the personal and social breakdown and disorganization which supposedly occurs in the face of extreme stress. The researchers, it was hoped, might be able to advance some ideas on how to reestablish personal stability and social order. It was simply taken for granted that disaster behavior deviated sharply from everyday behavior; to document that, therefore, was not a research goal. Although not always successful, all the researchers attempted to do systematic field studies: they interviewed and they observed on the scene. Even as early as 1952, the NORC team conducted a month long, random sample survey of 342 tornado impacted households which involved a field interview instrument that took over two hours to administer (see Marks et al., 1954).

Most important of all, the pioneer researchers in the disaster area, from the early 1950's to the early 1960's, reached the same general conclusion--they had made a basically incorrect assumption. There simply was not the personal and social disorganization in disasters that had been anticipated. Particularly at the individual level, behavior in the extreme stress of disasters is usually controlled rather than impulsive. Behavior generally involves the use of what

is seen as appropriate means to the perceived ends; it is generally not disorganized. Behavior is mostly adaptive, that is, it is, in general, functional rather than dysfunctional for the situation. It is not that researchers found no problems. Quite the contrary, these early studies found many, especially at the group level. For example, the mobilization of all relevant organizations at times of disasters is usually problematical; certain necessary tasks such as search and rescue and allocation of casualties to hospitals are frequently poorly done; interorganizational coordination in the post-impact period may or may not occur (Fritz, 1961).

Overall and in retrospect, it can be said that the pioneer researchers accomplished two important things. They uncovered a number of myths and misconceptions about disaster behavior, and obtained some ideas of the sources of problems affecting efficiency and effectiveness of preparations for and responses to disasters. We think it is important to emphasize this because they are relevant to an understanding of the consequences of disasters for mental health. As we shall indicate later, we think some mythological views still prevail with respect to mental health problems, and the prime source of problems in disasters is not yet fully recognized.

The pioneer researchers in the disaster area established the existence of a number of myths regarding behavior in the stress situation of a disaster, which are now widely accepted. The notion that panic behavior is a common phenomenon in disaster events is one example of an erroneous popular belief that was refuted by empirical research. Media accounts reporting instances of panic flight reactions at disaster sites have long been common. At times, entire communities have been described as fleeing from a potential site of disaster impact; however, the reality in the pre-impact period is that the vast majority of community residents can scarcely be induced to evacuate their homes, even when the possibility of damage and destruction is imminent (Quarantelli, 1954, 1957).

Disaster research in the mid 1950's and early 1960's not only uncovered myths but also indicated that the roots of many disaster problems could be found in the organized effort to prepare for, respond to, and recover from major community emergencies. Often the very organizations involved in disasters are a major factor in the disaster-associated problems which develop. This, of course, is not deliberate, but nonetheless, agencies and groups often act in such ways as to magnify or create difficulties and problems for populations threatened by or actually exposed to danger. In fact, this point is now so well accepted by experienced disaster researchers that they draw a distinction between agent-generated demands and response-generated demands (Dynes, Quarantelli, and Kreps, 1981). Disaster agents create certain demands such as search and rescue, care of injured and dead, etc.; but there are also response-generated demands. These are the needs and demands which are not created by the disaster agent itself, but by the very activities that take place when responding to the disaster occasion. These include such matters as communication, continuing assessment of the emergency situation, the mobilization of human and material resources, coordination, and control and authority. In some cases, the response-generated demands turn a minor incident into a major disaster. In other words, the response itself can be worse than anything the disaster agent itself presents.

In the Wilkes-Barre flood, about 20,000 people had to leave their homes for a long period of time because of a massive flood (we ignore the pre-impact organizational failures which did not create the disaster but almost insured that it would have maximum destructive impact). The displacement forced evacuees out of their homes and disrupted their lives in many major ways, but even worse, an incredible amount of bureaucratic inefficiency forced many of them to break neighborhood ties and live in trailers which were very poorly suited to the area. The Disaster Research Center found that for many households and individuals, greater social and psychological damage was occasioned by the "helpful" response of putting evacuees in unsuitable trailers in undesirable areas than was done by the disaster agent, the flood waters.

In another situation, the Buffalo Creek disaster, a social scientist looking at another massive relief effort said: "The end result insofar as rehousing was concerned was what might be expected if a brilliant madman set about in the most ingenious ways to maximize personal and social pathologies." (Harshbarger, 1976:276). This was said of an effort in which millions of dollars were spent over several years by many well-intentioned agencies that did not know what they were doing, or perhaps worse, that thought they knew what they were doing.

Our point in citing these examples is to indicate, as said in much of the early social science disaster research, that to focus solely on the disaster agent results in the omission of an often very important aspect of the situation, the organized attempt to respond to the occasion. Organizational response to a disaster or its threat is crucial since it provides the structure for possible individual responses. At one time, the National Weather Service often asked why people did not pay attention to their warnings about dangers. The problem was seen as residing in individuals, as one of perception of messages, and as a reflection of a passive or unresponsive attitude in the face of danger cues. However, social science studies of the warning process indicated that the problem was incorrectly viewed--the question to ask was not why people did not pay attention to warnings, but why alerting organizations did not issue messages that people could interpret as warnings. The deliverers of certain disaster-related services were the source of the problem, not the recipients of the services, the population at large. People did not passively perceive danger warnings; rather, the involved organizations simply were not communicating warning messages, a rather different view of the problem with drastically different implications of what could be done about it (McLuckie, 1970).

We certainly do not want to imply that all disaster problems can be solved by looking for the source of difficulties in organizations rather than in the affected individuals. However, the research of the 1950's and early 1960's did establish this approach as being a very fruitful way of looking at many disaster problems. Rather than blaming people for what is happening to them, there are many matters which can be better understood by looking at the social situation or social structure in which people must respond. The early disaster researchers also showed that if the "wrong" questions were asked, rather meaningless answers would be obtained. "Wrong" questions will be posed if mythological assumptions about disaster behavior are not uncovered and challenged.

Considerable research has been undertaken on human and social aspects of disasters in the last 20 years. The Disaster Research Center (DRC) alone has conducted over 250 different field studies of natural and technological disasters, most of a community nature. Dozens of social scientists around the world are looking at disasters (an International Research Committee on Disasters formed in 1983 has over 250 members in 24 countries). The development of a "critical mass" of specialists in a variety of disciplines, the institutionalization of the research area through the creation of research institutes and the establishment of professional journals and newsletters, and the beginnings of collaborative cross-societal studies, and other happenings, all attest to the flourishing of the field (see Quarantelli and Dynes, 1977; Kreps, 1984). However, they are important for the purposes of this paper, primarily in two ways.

First, all this social and behavioral research has resulted in solid understanding about some topics. We have good information about other topics. To be sure, on some matters, observations are no more than mere speculations, and with regard to a few things, nothing is really known. But, compared to just a decade or two ago, for those who would look, there is much empirically grounded knowledge to be found (compare, e.g., Fritz, 1961; Barton, 1970; Dynes, 1974; Mileti, Drabek, and Haas, 1975; Quarantelli and Dynes, 1977; Quarantelli, 1978; Wright and Rossi, 1981; Kreps, 1984; Drabek, forthcoming). Unfortunately, not everyone who has been examining the topic of mental health in disasters has taken advantage of the knowledge about disaster behavior in general and the full range of specific work in the mental health area in particular. It is a little disturbing to read supposed examinations and reviews of the literature and not find relevant references to whole bodies of research findings and theoretical formulations from the disaster area (e.g., Hocking, 1965; Kinston and Rosser, 1974; Frederick, 1980). Such lack of references attests either to poor scholarship or a deliberate avoidance of ideas that do not fit preconceptions. More important, it can only lead to a slow and painful reinvention of the wheel and a tendency to incorrectly generalize from limited incidents, or highly atypical events (e.g., Buffalo Creek).

Second, the work of the past decades has considerably extended our detailed knowledge about how individuals, groups, organizations, communities, and societies prepare for, respond to, and recover from both community and non-community natural and technological disasters. But certain basic themes advanced by the pioneering researchers have continued to be documented over and over again in the later work. Among the major ideas running through the research and the literature are the following: human beings react much better in the extreme stress of a disaster than they are usually given credit for; reasonable rather than panicky behavior is the mode; prosocial rather than anti-social behavior predominates; activity rather than passivity is what emerges even at the height of the emergency periods of disasters. On the other hand, organizations show less adaptability and flexibility in the face of disasters than do human beings; in their efforts to mobilize resources to respond to and to recover from disasters, organizational and structural factors serve to compound communication, authority, and coordination problems, which may help to turn minor accidents into disasters, and disasters into catastrophes (for summaries of these points, see Quarantelli, 1982a). As in much of the current writings on social problems by contemporary sociologists, disaster researchers first said and continue to say that their studies show we should "stop blaming the victims," for the source of most problems is in the social environment, especially the organizational setting within which "victims" have to act.



## Beliefs About Mental Health Consequences

To say that most people equate the term disaster with intense and prolonged human suffering, anguish, and loss, is to state the obvious. Even the common sense observer would agree that the impact of a tornado, an earthquake, an explosion, or the water surging from a crumbled dam goes far beyond the immediately recognizable loss of life and the sheer physical damages and destruction associated with such events, impressive though these may be. What is even more important about a community disaster, is the disruption of life, the marked alterations of routine patterns of everyday social expectations, and day-to-day personal habits. While the physical impact of a disaster may be over in a few minutes, other consequences may extend over weeks, months, and even years. A major disaster does far more than wreck buildings and sever lifelines; it interrupts the rhythm, cycles, and very social fabric of community life.

Disasters are part of a class of collective stress situations, since they disrupt social life; they also induce psychological stress for their victims. But how do human beings respond in these collective stress situations? Can it be assumed that the social disruption occasioned by a community disaster also creates psychological disorder or malfunction among victims? The answer to this question is twofold: how people are believed to respond and how they actually respond. We will look at three belief systems. The first, will be what the public at large believes--what might be called folk wisdom and mass media conceptions. Second, we shall look at the theoretical notions in the mental health area relevant to mental health consequences of disasters--what might be called the theoretical presuppositions of mental health practitioners. Third, we will briefly look at the changing models in the social sciences which have a bearing on the question. As already noted, social scientists started out with an image which has since changed.

### Folk Wisdom and Mass Media Conceptions

Conventional wisdom has long held that human beings do not react well to large-scale stress. It is commonly believed that when people are faced with the threat or the actual occurrence of a major disaster, they disintegrate physically, mentally, and morally. They supposedly engage in bizarre, anti-social, irrational and destructive acts, such as wild and disorderly panic, looting, and other forms of criminal deviance. Popular beliefs about reaction to extreme stress situations are so grim that hysterical breakdowns and psychotic episodes are thought to be common among disaster victims in the short run, and various forms of severe psychopathology are expected to be manifest among victims in the long-run. In short, the image is essentially that disasters create or exacerbate severe forms of mental illness for their victims.

These common stereotypes of how persons respond to and are affected by disasters are not new. While there are undoubtedly many reasons for such stereotypes, one basic reason is that mass media and journalistic accounts often reinforce and support such beliefs. This can be seen in the images played up by news and magazine accounts of disasters dating as far back as the late 1800's and early 1900's. For example, in a Harper's magazine article of 1889, survivors of the Johnstown, Pennsylvania flood were described as, "crazed by their sufferings." (Dieck, 1889:139). A Saturday Evening Post account of the devastating hurricane

which hit Galveston, Texas in 1900 wrote of 500 people who went "insane almost in unison" following the disaster (Perry, 1950:117). Similarly, Harpers Weekly wrote that the 1906 San Francisco earthquake and subsequent fire brought about cases of "men gone mad." (Chard, 1906:702). While the terminology used in these articles is, of course, outdated, they do nevertheless illustrate the long history of viewing disasters as leading to severe psychopathology.

Perhaps even more important, the same general stereotypes continue to be emphasized in present mass media accounts of disasters. Following a series of major floods in 1973, Newsweek, for example, reported that once the immediate post-impact period is over, a new reaction starts to appear among victims--this one a "kind of shared psychosis that hits just about everyone affected directly or indirectly by the events." The story then goes on to assert that within a few weeks after such a catastrophe, "symptoms of emotional problems will become disturbingly obvious; the number of successful suicides rises by about a third; hospital admissions for psychiatric reasons run at double the normal rate; and the frequency of accidents skyrocket" (Newsweek, 1973:62-63). The more recent Mt. St. Helens volcanic eruption has generated press comments to the effect that "Wife battering is up. So are suicide attempts...the volcano's impact on the mental health of hundreds of thousands of residents in the area will be serious and long-lasting" (Columbus Dispatch, 1980:A-7), and "Now, They're Going Crazy" (Devlin, 1980:B1).

While numerous other examples of journalistic writings which advance similar ideas could be cited, do people in general share such beliefs? Three surveys recently empirically ascertained public beliefs about human behavior in disasters. The first, a survey in the state of Delaware found that large blocks of the population do, in fact, believe that disasters evoke extreme reactions in their victims. For example, these researchers report, among other things, that 74 percent of those surveyed agree with the statement that "immediately following the impact of a disaster, victims are in a state of shock and unable to cope with the situation by themselves." (Wenger et al., 1975). The second survey conducted in a small community in Ohio not far from Xenia only months after a tornado, produced similar findings. The attribution of problems of a mental or psychological nature to victims of disasters was widespread among the population surveyed (Blanshan, private communication). A more recent field survey in Arkansas, Wisconsin, and Mississippi also looked at beliefs of community officials as well as the general populace about psychological consequences of disasters. The findings are consistent with the previous studies. Officials as well as the public at large hold to the image that disasters produce extreme psychological and emotional reactions in their victims (Wenger, James, Faupel, 1980).

### The Mental Health Area

What do psychiatrists, psychologists, and other experts in the mental health field have to say about human response to disasters? (We leave aside those with direct systematic research experience in community disasters, since their views are discussed later.) Like the mass media and a majority of the general public, a large number of mental health professionals also assume that extreme emotional and psychopathological reactions are a typical consequence of disasters. This is true with respect to both the short run and long run. While the terminology used varies somewhat, psychiatric and psychoanalytically oriented writers often note that immediately after impact, victims can be expected to

display what is often termed the "disaster syndrome." This condition is supposedly characterized by an unrealistic absence of emotion, inhibition of activity, docility, indecisiveness, lack of responsiveness, and automatic behavior on the part of disaster victims. An often cited numerical projection of the numbers of victims likely to display such symptoms was set forth over three decades ago by Tyhurst, one of the first professionals writing on the subject. According to Tyhurst, about 10-25 percent of a disaster impacted population will show "manifestly inappropriate behavior, states of confusion, paralysing anxiety, inability to move out of bed, 'hysterical' crying and screaming, and so on" and in a post-impact stage there may be "temporary anxiety and fatigue states, psychotic episodes, recurrent catastrophic dreaming, depressive reactions" (1951:766-767). Another 75 percent at impact will be "dazed, stunned, bewildered," show "childlike dependency" or otherwise exhibit the disaster syndrome noted above. Although an examination of Tyhurst's writings show no evidentiary research basis for such statistics, the figures are cited to this day (see Kinston and Rosser, 1974:439; Edwards, 1976:944; Boyd, 1981:745-746), and sometimes are not even attributed to Tyhurst (e.g., Cohen and Ahearn, 1980:32).

While some recent writers in the professional area have backed off from ascribing negative and dysfunctional psychological reactions to victims in the immediate post-impact period, most still have a different view of the longer run. Thus, it is often claimed that during the later post-impact phases, victims are likely to exhibit reactions such as: an increase in the use of alcohol and other drugs; acute, traumatic neuroses; tormenting memories and guilt feelings over survival; and irrational hostility and scapegoating. Frederick in a recent general listing of the effects of natural disasters names as psychological symptoms: "anxiety; insomnia; depression; anorexia; psychophysiological reactions; phobias about the event; little guilt about plight of other victims; hostility; paranoid reactions toward Governmental officials and persons with fewer losses... Desire for reprisal. Aberrant characterological acts, e.g., looting, deviance, alcoholism" (1980:72) and also states that "Psychological reactions that are less frequent, but which may occur, are: disorientation, wherein confusion and memory loss develop; hysteria, wherein screaming and uncontrollable crying are apparent, or numbness develops in parts of the body not consistent with neurological structure; and psychoses, in which the individual engages in highly irrational thinking and may be temporarily out of contact with reality. Bizarre sensations may be reported, and delusions of grandeur or persecution may be apparent" (1980:74).

Furthermore, the reactions are seen as long lasting. Even the absence of overt symptoms is not seen as an indication of no pathological reactions over the long run. Thus, "disasters...unleash powerful behavioral reactions and emotions which often are overwhelming...since...the loss of life's familiar benchmarks induces intense stress leading to physical and mental illness...Even when there has been no loss of human life, one can expect a predictable sequence of such behaviors as shock, guilt, anger, and grief to occur among affected persons over a six- to twelve-month time period. A disaster's victim's failure to display these normative reactions should not lead to the conclusion that all is well; instead, it should alert the caregiver that the victim potentially is employing maladaptive resolutions" (Schulberg, 1974:85). Furthermore, "extreme circumstances of traumatization--disasters, catastrophes and overwhelming social situations--effect marked changes in the people subjected to them and leaves them

with life-long problems" (Krystal, 1968:1). In fact, "there is mounting evidence that the effects of a disaster can extend over several generations. Adverse effects of significant proportions can occur in children of survivors even when born several years after a particular disaster" (Boyd, 1981:748).

The imagery is clear. Mental health theorists who have written on the topic generally believe that disasters bring about immediate, widespread, persistent, serious, long lasting, and dysfunctional psychological effects.

### The Social Science Perspective

The first social and behavioral researchers in the community disaster area worked with a model about behavior which they eventually found was inappropriate and inaccurate. There were four basic assumptions:

- 1) Disaster responses were relatively homogeneous, that is, there was a tendency to think of behavior under stress as being either/or; for instance, either people acted in an anti-social fashion or they did not;
- 2) Many of the responses were inappropriate or "bad," that is, they took the form of panic, looting, hysteria, shock or other forms of personal breakdown or disorganization;
- 3) Disaster victims primarily responded directly to the disaster agent or its immediate effects; and,
- 4) The major explanation of disaster behavior was to be sought in the social psychological dynamics of individual victims.

Implicitly more than explicitly, the early social and behavioral researchers obviously thought that there were severe and widespread psychological consequences for the victims of disasters. Contrary to statements that early researchers minimized negative psychological effects (e.g., Frederick, 1980:71), the first systematic disaster students assumed just the opposite. But, as noted, it was finally recognized that the initial assumptions were empirically incorrect and an invalid disaster behavior model was being used.

Eventually, a new model based on research findings emerged (for a recent statement, see Kreps, 1984). It assumes that disaster responses are always heterogeneous, that one should expect a variety of differentiated responses (as hypothesized more than 40 years ago by the sociologist, Sorokin, 1942). Specific behaviors can be functional or dysfunctional depending on a variety of factors, but overall disaster responses are functional and organized--even panic flight is not as impulsive, irrational, or inappropriate as was once thought (Quarantelli, 1981). The newer model recognizes that what happens after impact is as important if not more so than what occurs at time of impact--what we earlier noted as response rather than agent demands to which victims are exposed. Finally, the new view is the social contexts more than internal dynamics have to be understood since they provide the settings in which individual behavior occurs. This change of imagery is applied with respect to disaster behavior in general, but it clearly has major implications for approaching and explaining the consequences of disasters for mental health.

## The Empirical Evidence

Let us now turn from beliefs to actualities. How accurate are the widespread common sense beliefs that people react poorly to collective stress situations? How correct is the theoretical view in the mental health area that disasters are necessarily deeply traumatic events? Is there any validity to the implication of the newer social sciences model about disaster behavior in general that the specific problem of psychological well-being in disasters is more complex than appears on the surface?

What is the actual evidence? What do we have beyond isolated anecdotal examples or occasional clinical impressions? Any answer obviously involves a judgement call, but in our view there are a little over a dozen community disasters in which an effort was made to gather at least semi-systematic data on the psychological well-being and mental health-associated problems of the stressed population. We list here only studies in American society (foreign studies will be noted later). (See for a larger listing, Ahearn and Cohen, 1984.)

These disasters include the:

Big Thompson flash flood in Colorado

(Miller, 1977; Kimball, 1978; Miller et al., 1981)

Buffalo Creek dam flood

(Harshbarger, 1973; Church, 1974; Titchener, 1975; Erikson, 1976a, b, c; Newman, 1976; Rangell, 1976; Lifton and Olson, 1976; Titchener and Kapp, 1976; Gleser et al., 1978, 1981)

Los Angeles earthquake

(Blaufarb and Levine, 1972; Howard and Gordon, 1972; Koegler and Hicks, 1972)

Monticello, Indiana tornado

(Zarle, Hartsough, and Ottinger, 1974; Bowman, 1975; Hartsough, Zarle, and Ottinger, 1976)

Mt. St. Helens volcanic eruption

(Pennebaker and Newtonson, 1980; Murphy, 1981; Leik et al., 1982; Murphy, 1983)

Omaha tornado

(Kara, 1977; McIntire and Sadeghi, 1977; Bell, 1978; Bell et al., 1978; Rosenberg et al., 1980)

Rapid City flood

(Hall and Landreth, 1975; Bolin, 1976; Trainer and Bolin, 1976; Bolin and Trainer, 1978)

Rochester, Minnesota flood

(Ollendick and Hoffmann, 1982)

St. Louis area: Tornadoes and floods

(Penick et al., 1974; Penick, Powell, and Dieck, 1976; Brownstone et al., 1977; Powell and Penick, 1980)

Teton Dam collapse in Idaho

(Huerta et al., 1977; Huerta and Horton, 1978; Golec, 1980, 1983)

Three Mile Island nuclear plant accident

(Bromet, 1980; Houts et al., 1980; Bromet and Dunn, 1981; Dohrenwend et al., 1981; Kasl et al., 1981; Bromet et al., 1982; Baum et al., 1983; Warheit and Auth, 1983; Warheit, 1984)

Topeka tornado

(Drabek et al., 1973a, b; Drabek and Key, 1975; Drabek et al., 1975; Drabek and Key, 1976; Erickson et al., 1976; Sterling, Drabek, and Key, 1977; Kilijanek and Drabek, 1979; Drabek and Key, 1983)

Wichita Falls, Oklahoma tornado

(Bolin, 1979, 1982; Fairchild and White, 1982)

Wilkes-Barre flood

(Whalen, 1972; McGee, 1973; Birnbaum et al., 1973; Zusman et al., 1973; Richard, 1974; Knaus, 1975; Heffron, 1975, 1977; Poulshock and Cohen, 1975; Kafrissen et al., 1975; Melick, 1976, 1978; Cohen and Poulshock, 1976, 1977; Logue, 1978; Melick, 1978; Logue et al., 1979; Logue and Hansen, 1980; Melick and Logue, 1980; Logue, Melick, and Struening, 1981)

Xenia, Ohio tornado

(Laube, 1974; Taylor, 1976, 1977; Taylor, Ross, and Quarantelli, 1976)

The studies of these various community disasters vary in their methodological rigor. We have among them, for example, a strict probability sample of 15 percent of the total impact population in the Xenia tornado and self-selected samples of victims who sought some kind of aid in other disasters. In some of the research, extensive data were obtained from combinations of open-ended interviews, psychological scale instruments, mental health case load documents, assessments of key informants, drug usage statistics, etc.; in other studies, only two or three questionnaire items dealt with mental health matters. However, despite the variety of data gathering instruments used and the samples obtained, the quality and quantity of the data approaches respectability. Certainly, it is substantially more defensible as acceptable data for research purposes than the anecdotes, scattered clinical and field impressions, and selective observations which passed for data even just a decade ago.

There are, moreover, some relevant but less systematic data in both older and more recent studies (e.g., Moore and Friedsam, 1959 on the Dallas tornado; Langdon et al., 1964 on the Alaskan earthquake; Bates et al., 1968 on Hurricane Audrey; Laube, 1973 on Hurricane Celia; Kliman, 1975 on the Corning, New York flood; Sundell, 1975 on the Louisville tornado; Cryns, 1978 on the Buffalo blizzard; and Shippe et al., 1982 on the Phoenix flood).

Since our focus is on community type disasters, work done on disasters that were not somehow disruptive of on-going community life, have not been listed. Thus, we exclude, for example, studies made of psychological reactions to ship disasters (e.g., Friedman and Linn, 1957; Leopold and Dillon, 1963; Hoiberg and McCaughey, 1981). Fires which involve massive casualties (e.g., the Cocoanut Grove night club, see Adler, 1943, 1945; Cobb and Lindemann, 1943) are somewhat more marginal instances, but even the very well-studied Beverly Hills Supper Club fire (e.g., Lindy et al., 1981 and Green et al., 1983) would be difficult to characterize as a community disaster.

In addition to this work which has focused directly on victims, there has also been systematic study of the deliverers of mental health services in community disasters. For example, DRC recently examined those communities which had obtained a federal declaration of a disaster, and/or made efforts to obtain federal funds for crisis counseling and other mental health services. The study looked at how local, state, regional, and federal mental health practitioners and involved officials reach decisions on the consequences of specific disasters for mental health. Why were services provided in some disasters and not others? What did local mental health practitioners assess as disaster-related mental health needs? What consensus, if any, was there among the various agencies involved on the kinds of services which should be provided?

What types of disaster related mental health problems did practitioners actually see? These and similar questions gave us answers about the delivery of mental health services in community disasters which are a counterpart to the findings and observations obtained in the research on the receivers of the services, or the disaster victims. Now we have data on both the providers and the receivers of mental health services (see Baisden and Quarantelli, 1981).

### The Two Positions

All the works cited in the last several pages constitute the data base we examined in making a comparison between the two major positions on the psychological effects of community disasters. We will discuss in general and overall terms the essence of the two positions. Rather than dealing with specific studies or particular writers we will state the relative consensus among one set of practitioners and researchers, as opposed to the relative consensus of another set of similar persons, and offer only a very few selected examples.

The first position we will talk about holds that since disasters are highly stressful, they are traumatic life events. These events are seen as producing among those exposed to them, very pervasive, deeply internalized, and essentially negative psychological effects. Disaster victims are viewed primarily as attempting to cope with the meaning of the trauma of the disaster impact. For purposes of having a convenient short hand label, we will talk about this as the individual trauma approach.

The second approach we will talk about basically holds that community disasters have differential rather than-across-the-board effects. Some of the effects are positive as well as negative; many of the latter are relatively surface and short in duration. The varying problems of victims are more closely related to the post-impact organized response than they are to the disaster impact itself. Again for want of a better term, we will talk about this as the social sponge approach. (A sponge is an elastic porous mass of interlacing fibers; it will maintain its basic structure under all kinds of pressures and return quickly to its usual state; it can also absorb very large amounts of all kinds of liquids and other materials, and even though wet, will not lose its overall toughness. If the sponge is considered analogous to a community, and the interlacing fibers as similar to the social fabric within which all persons are embedded, something else is also suggested which is important in this approach.)

In further examining and illustrating these two approaches, we will primarily use some of the research and literature from the Xenia, Ohio tornado disaster and the Buffalo Creek, West Virginia dam disaster, since they have been among the most intensively studied insofar as the consequences for mental health are concerned.

One difference in the two approaches can be found with respect to the pervasiveness and nature of the psychological consequences. The individual trauma approach essentially argues that everyone is negatively affected. Thus, "the psychological impact of the disaster has been so extensive that no one in Buffalo Creek has been unaffected. The overwhelming evidence is that everyone exposed to the Buffalo Creek disaster has experienced some or all of the following manifestations of the general constellation of the "survivor" syndrome and goes on to note these as being--death anxiety and permanent inner terror, guilt over having survived,

psychic numbing and depression, impaired human relationships, and inability to find an explanation for what had happened. "Our observations were all too consistent with a body of recent experience with 'massive psychic trauma' of war, revolution, concentration camps, and severe disasters--psychiatrists have regularly observed that psychological impairment can result in virtually anyone, independently of estimates of predisposition" (Lifton and Olson, 1976: 1, 15).

Two other researchers, Titchener and Kapp, state that "disabling psychiatric symptoms such as anxiety, depression, changes in character and lifestyle... were evident more than two years after the disaster in better than 90 percent of our respondents." Psychoanalytically oriented interviews and analyses of dreams more than two years after the event it is claimed, allowed recall of almost universal instances of "nightmares" and "obsessions and phobias about water, wind, rain, and other reminders of the remotest possibility that the disaster could recur" (1975:1, 4). These and other reactions "were at the traumatic level and for so long that we must compare these syndromes, at least in structure and form, if not in content, to psychoses" (Titchener, 1975:12).

In contrast, in the social sponge approach, the differential and not necessarily negative aftereffects of disasters are emphasized. To quote from a report on the research carried out in Xenia for 18 months after the disaster:

The study found that there was an extremely low rate of severe mental illness, if any at all, as a consequence of the tornado. On the contrary, it concluded that a large percentage of the people had extremely positive reactions to the disaster. Eighty-four percent of the people claimed that their experiences had shown them they could handle crises better than they thought; and 69 percent reported that they felt they had met a great challenge and were better off for it. ...Changes in the quality of social relationships are often thought to be related to changes in emotional well-being. Yet only two percent of the population admitted to worsening relationships with close friends and family after the tornado. Instead 27 percent claimed that such relationships had improved. Similarly, a mere three percent found their marital relationship less satisfying since the tornado, while 28 percent reported them to be, in fact, more satisfying. (DRC, 1976)

Similar positive results have been reported elsewhere. A study of the Topeka tornado done three years after the event was able to match victim families and nonvictim families for which pretornado data existed. It found that victim families rated their marriages as happier than before the tornado and as happier than nonvictim families. Also, victim couples went out together more often after the tornado than before (Drabek and Key, 1983). Clearly, the individual trauma and the social sponge approaches are not reaching the same conclusions about the psychological outcomes of disasters, whether it is in terms of how people feel, relate to others, or evaluate their experiences.

There is also a difference along another line. Essentially, the individual trauma approach argues that the post-disaster negative reactions are neither superficial nor transitory. Again, to quote from a Buffalo Creek analysis:



We can say it (i.e., the flood) brought about an extraordinary number of psychiatric disturbances, and that even those in the very small minority without formal psychiatric diagnoses...tended to experience significant degrees of psychological suffering and conflict...Without denying the existence of significant variation in psychological vulnerability, we have been far more impressed (as have other observers) by the degree to which the massive character of the trauma subsumed individual differences and produced strikingly consistent forms of impairment. We have also been impressed by the persistence of these expressions of psychological impairment, which in many cases increased rather than diminished over time. (Lifton and Olson, 1976:15).

It is not surprising to find elsewhere the statement:

There is, in fact, mounting evidence that the effects of disaster can extend over generations, and that adverse effects of significant proportion can occur in children of survivors, even after the children are born some years after a particular disaster...at Buffalo Creek one can certainly observe many families to be a "collection of severely disturbed and traumatized individuals," who could well transmit various disaster-related conflicts to subsequent generations. (Lifton and Olson, 1976:14-15).

Apart from the Buffalo Creek research, other studies, while not taking as extreme a position, as just stated, also suggest long term effects. A study of 562 women made five years after the Wilkes-Barre flood, assessed depression and anxiety states through self-rating scales and other measures. One of its conclusions was that for all seven variables related to long term mental health, the results consistently showed the direct flood victims had more symptoms than did non-flood victims although differences in long run physical health problems stood out more sharply (Logue, 1978).

In contrast, another study of the Wilkes-Barre flood done three years after the event matched a flood and a non-flood control group, used the Gurin Symptom checklist, and found "both groups obtained high scores, indicative of positive mental health. The flood-group mean score was 71.0 out of a possible 80 points, while that of the non-flood group was 72.2." not a statistically significant difference (Melick, 1978:338).

The social sponge approach consistently points to lack of severity, duration, and dysfunctionality in the data they have obtained. Again we cite a report using Xenia data:

A year and a half later...only three percent of the population reported feeling at any time after the disaster that they might have a nervous breakdown. The proportion of those who did have such a fear and who reported that their symptoms actually interfered with routine social activities was insignificant. Only one percent of the population had considered suicide at any time after the tornado; only three percent reported any increase in drinking whereas seven percent of Xenians claimed they consumed less alcohol. There was a slight decrease in the percentage of the population

who reported using tranquilizers, falling from 20 percent to 16 percent one year later, as did the use of any kind of service from any of the local mental health agencies which fell from 10 percent to 5 percent. (DRC, 1976).

Proponents of the individual trauma approach, of course, argue that self reports cannot be trusted. However, one analysis done in Xenia found that independent behavioral indicators supported what victims had self reported. Consistent with interview remarks, there was no overall change in the marriage and divorce rates after the tornado. Agencies that provided treatment and hospitalization for serious psychiatric problems actually reported a decline in demand for their services. For example, the state hospital facility most likely to be used reported a 30 percent drop in admissions in the year following the tornado. Similar declines in demands for services was reported in other area organizations specializing in long-run clinical treatment through the use of psychotherapy, drugs, or hospitalization. There was a significant drop in liquor sales in the two state monopoly stores in the Xenia area in the six-twelve month period after the tornado. (DRC, 1976; Taylor, 1977).

In another study conducted 18 months after the Rapid City flash flood, it was found that no significant increases occurred in the number of attempted or actual suicides or single car accidents (often considered suicide attempts); the rate of juvenile delinquency; the number of citations for driving while intoxicated; the number of automobile accidents; rates of scarlet fever, strep throat, and hepatitis; the number of prescriptions written for tranquilizers; and the utilization of community mental health center services (Hall and Landreth, 1975). Again, we see the individual trauma and the social sponge approaches reaching different conclusions about the severity, duration, and dysfunctionality of the mental health consequences of disasters.

The social sponge position does acknowledge that many disaster victims do exhibit a variety of transient emotional symptoms. In the Xenia disaster, one study over an 18 month period, reported the following behavioral and psychological symptoms which might be indicators of mental health difficulties:

- 56% of those surveyed reported feeling depressed or low on occasion;
- 50% admitted being more nervous or excited some time after the tornado;
- 27% reported sleeping problems at times;
- 25% reported headaches; and
- 19% indicated some loss of appetite.

At a more behavioral level, 14 percent of those surveyed said they missed five or more days of work because of an emotional or mental health problem. There were also significant increases in the number of visits to the emergency room and outpatient clinic of the local hospital as well as in incidents involved traffic violations and juvenile delinquency. However, in order to put this in a proper context, we should note that when the victims were asked how they felt emotionally or mentally after the tornado, 58 percent said they felt good or excellent, 33 percent said fair, and only 9 percent said their emotional or mental health was poor or very bad. Behaviorally, there were significant decreases in deaths due to heart, vascular and respiratory diseases; actual number of offenses reported to the police; and in drug-and-alcohol-related case contacts by the local crisis center. There were no changes in suicide rates, overall death rates, domestic trouble calls to the police, or reports of child abuse (DRC, 1976).

Actually the results presented above are not that different from what was found by the very first systematic victim population survey ever done in the disaster area--an almost ideal study, contrary to its misrepresentation in a poor survey of the literature by Kinston and Rosser (1974:440). Data from a number of communities, ranging from heavily impacted to near miss by a series of tornadoes, found that among those surveyed:

- 49% reported nervousness, excitability, and hypersensitivity;
- 46% sleeplessness or poor sleep;
- 37% inability to concentrate;
- 29% loss of appetite;
- 19% headaches; and
- 18% anxiety dreams and nightmares (Marks et al., 1954).

But what has been singled out in later studies in the social sponge approach is that the source of much such common and widespread, although relatively unimportant behavioral difficulties, is the social setting in which post-disaster relief and recovery services are obtained. Efforts to obtain services frequently generate anger, concern, worry, and anxiety; and are what some have said are "secondary disasters" which are "produced by the socially organized response and in particular inequities in the distribution process" (Golec, 1980:30). This is also illustrated in a Rapid City disaster study which concluded that while the flood did not engender a major community mental health crisis, it did result in an increase in stress for non-affluent victims. Group life in government-sponsored mobile home parks set up after the disaster was a source of stress and was probably detrimental to the psychological well-being of residents since this way of life tended to destroy their natural helping networks. It was less the impact of the disaster itself which affected victims' psychological well-being, but more the long-term impact of inefficient and ineffective federal relief efforts which accounted for the stress manifested by the victims (Hall and Landreth, 1975). In the same vein is Logue's conclusion that various stressful experiences in the recovery period following the Wilkes-Barre flood were better prediction of mental health status as measured five years after the events than the actual disaster impact (1980). Even one of the more prominent writers on the Buffalo Creek disaster, Erikson (1976), although he is the only one, seems to accept the idea that a "second disaster" by way of the post-impact relief efforts may have contributed substantially to the negative consequences for the victims.

#### Accounting for the Two Positions

Why are there two positions? What accounts for the differences in views? A few writers have addressed the question of inconsistencies in the findings about the psychological effects of community disasters, but the more comprehensive have emphasized the absence of methodological rigor (Perry, 1979; Logue, Melick, and Hansen, 1981; Green, 1982) in the research undertaken. This is a valid criticism of much of the work, but does not really answer why there are two approaches which report grossly different empirical findings. We think more basic factors are involved, of which we will discuss six possibilities.

1. First, it is possible that the different researchers and analysts are observing actual differences in the mental well-being of the victims they have studied. Since there has been very little overlap in the specific disasters studied by advocates of the two views, this is a hypothetical possibility. For

this reason, if there were actual differences in what occurred in the events, it is possible that different observations could be made and different conclusions reached. Until a number of the same disasters are systematically examined from the two different perspectives, this has to be considered a hypothetical possibility, but it seems a very unlikely explanation, statistically unbelievable.

Actually two events, the Wilkes-Barre flood and the Three Mile Island nuclear accident have been studied by researchers with different perspectives and inconsistent findings have been reported. Earlier we noted an inconsistency in research findings on long run effects of the flood. A very thorough review of almost all the research undertaken on the psychological consequences of Three Mile Island also concluded that research findings were inconsistent (Warheit and Auth, 1983) although more valid data seemed to indicate that such effects as surfaced were of a subclinical type, short-lived, and self-remitting, and "there are no scientific data which support the belief that the accident produced measurable levels of gross psychopathology" (Warheit, 1983:7). Leaving the qualifiers aside, just the inconsistency of research results from the same disaster, would seem to be more damaging to the individual trauma than to the social sponge approach, but sets of data from but two disasters, can hardly be conclusive.

2. A second possible explanation for the different results in the two approaches could be found in what is taken as acceptable data and appropriate data gathering designs. The individual trauma position does lean in the direction of using self-selected or otherwise suspect samples--the Buffalo Creek data consists mainly of material obtained in connection with some survivors pressing a law suit (see references listed earlier). The legal depositions gathered were not intended to be objective statements. Clinical impressions and what to many seem isolated anecdotal examples are also often used in the individual trauma approach. In contrast, the social sponge position, while hardly a model of ideal scientific work, leans in the direction of population surveys, drawing as close as possible to a random sample, and using standardized scales or quantitative measures such as statistics on drug prescriptions, automobile accidents, divorce rates, agency case load figures, police and court records, etc. There are, of course, methodological questions which can be raised about these data gathering procedures--needs assessment surveys are not epidemiological surveys, organizational records are not necessarily objective, etc. but, nonetheless, the logic is clear. Thus, it could be argued that the two approaches generate different results, because they use different means for obtaining data.

However, even those of us who normally are very skeptical of statistics per se, cannot ignore that the quantitative kinds of data noted above are almost always at variance with what is assumed in the individual trauma approach. It is impressive to note the consistency and directionality in data such as liquor sale records, suicide rates, family disturbance calls, etc. which are initially amassed for reasons totally independent of attempting to measure adverse psychological effects. Nonetheless, it might be possible to explain away such data (e.g., by arguing that they are not refined enough measures to capture subtle psychodynamics or that professional expertise is required for assessment, e.g., Frederick, 1977: 382); so different data gathering techniques have to remain as a hypothetical explanation for the different research findings.

3. Third and closely related to the matter just discussed is the question of the interpretation of data. Many of the theorists and researchers taking the

individual trauma approach have some kind of general psychoanalytical intellectual background. There is a tendency, therefore, to look behind the overt and the manifest. In the context of the topic we are discussing, this can lead to opposite interpretations of what at one level is the same piece of data. For example, if disaster victims assert they have no problems or state that they are happier in the post-impact period than in the pre-impact period, the assertions can be accepted at face value if not otherwise contradicted or logically suspect. But the assertions can also be taken as evidence of just the opposite, as evidence of the "denial" of the reality of the situation. The quotation we gave earlier on page 10 from Schulberg well illustrates this point of view of those with an individual trauma approach.

Researchers taking a social sponge approach are not naive; in fact, as a whole they have considerably more field experience in studying a wider range of disaster occasions than do those using an individual trauma approach. But they are willing to accept data at face value if it is consistent with other data. Furthermore, those with a social fabric approach, consider overt behaviors more important than mental states. For example, in a 15 percent statistically random sample of the population studied six months, and in a panel-like fashion 18 months after a torando in Xenia, DRC found that in terms of scale scores on psychological well-being, those surveyed showed signs they had been affected by the tornado experience. Their scores were higher than a non-disaster control group, the scores generally remained as high in the 18th month as they were in the sixth month, and those who had suffered the most (loss of homes, etc.), had the highest scores. On the other hand, on almost all measures of a behavioral nature whether reported by the surveyed population, the various community mental agencies, other community organizations, and also as reflected in a variety of statistics indicating personal and social problems, the post-impact figures were the same or below comparable pre-impact figures. In the unpublished analysis, DRC accepted this as evidence that the tornado had had little significant negative effects on the mental health of the affected population. What is crucial from the social fabric point of view is the lack of behavioral dysfunctionality; the individual trauma approach lays greater importance on the existence of psychological states. To the extent one approach gives greater weight to behaviors and the other to psychic states, it is very possible inconsistent research results will be reported.

4. A fourth possibility which could account for the differences in the two approaches is that different professional objectives and ideologies are often involved. Many of those using the individual trauma approach are mental health practitioners interested in giving treatment to victims. Most of those operating with the social sponge approach tend to be researchers more concerned with reaching an understanding and explanation of the phenomena they study and to them, frequencies of a phenomena are often of some importance, but if the figures are very low the observation may be of little descriptive or analytical value. Let us take a purely hypothetical case and say that only one percent of a population suffered something or other, making that phenomena probably of no importance for statistical research purposes. To a mental health practitioner, the one percent which may translate into 200 human beings, could be very important from a professional and humanitarian perspective. There is little doubt in our mind that at times, at least, those working within either one of the two approaches we have been contrasting, do tend to see "findings" in a very different light, given their professional objectives.

Also, as a number of writers have pointed out, there is a "mental health" industry which has a vested interest in finding certain phenomena (e.g., see Margaro et al., 1978). It is not surprising, therefore, when the psychotherapeutic establishment got involved in disaster studies about a decade ago, its professional ideology would lead to expectations of certain phenomena in such situations, which could be claimed as its province. Social science research is, of course, also an industry; some have in fact talked of the "disaster research" industry (Wright and Rossi, 1981) and it too has vested interests and professional ideologies. However, many of the researchers within the social sponge approach would argue that at one level, it does not matter to their "industry" if there are or are not adverse psychological effects from disasters--either way there is phenomena to be studied, whereas if there are not negative consequences there is much less justification for the psychotherapeutic establishment involvement with the area. At a deeper level, those in the individual trauma approach, especially with an orthodox psychoanalytical background, could probably argue, as we once orally heard, that the "denial" of adverse effects is itself a form of psychological defense on the part of researchers. In any case, different professional ideologies probably account for some of the differences in the two approaches.

5. A fifth possibility for the differences in the two approaches may stem from differences in conceptions of disasters. This could be true in at least three different ways.

For one, the individual trauma approach tends to include within the general category of disaster, the full range of individual and collective stress situations. Thus, such diverse phenomena as the Holocaust, shipwrecks, air raids, famines, mass kidnappings, plane crashes, concentration camp situations, military combat service, etc. are treated as if the same generic phenomena and into which is added natural and technological disasters (e.g., Kinston and Rosser, 1974). However, as noted at the start of this paper, many researchers see the need to separate out the latter from all other stress situations. The use of a very heterogeneous class, among other things, allows the picking of extreme and atypical cases. But since definitions and conceptions are to a considerable extent arbitrary matters, if the initial starting points are so widely divergent, the two approaches will find different phenomena.

There is also the fact that those taking the individual trauma approach usually do not distinguish between natural and technological disasters which disrupt community life and those which do not (e.g., most transportation accidents, fires and explosions, structural collapses), although a few recently have started to inch toward such a distinction (e.g., Green, 1982). The importance of the community context to many is that it provides what we have called the social sponge which can absorb much of the impact. Others have talked of the "altruistic community" (Barton, 1970), and especially of the "therapeutic community" (see especially Fritz, 1961, but compare Bates et al., 1963), and the latter might be a better term to use than social sponge, except that to us it implies some psychological damage has been done which is therapeutically repaired. In many cases, we think, the community or social fabric may actually absorb or prevent even the initial negative effects. In the first ever systematic disaster study, Prince hints at this when he notes because so many people were involved in the Halifax disaster, the blow was softened for the individual because many others shared in the suffering (1920). At any rate, the social sponge approach sees the community context as crucial, whereas it is at best a secondary consideration in much of the individual trauma approach.

Finally, there is a strong tendency in the individual trauma approach to see "a disaster" as an external agent (e.g., Berren, Beigel, and Ghertner, 1980) which necessarily negatively affects what it impacts. As Golec (1980) has astutely observed, the very term "victim" connotes an adverse consequence. What to many recent disaster researchers should be taken as a matter of empirical determination, is in the individual trauma approach taken as given. Some of those with a social sponge orientation have in fact argued for positive consequences of disasters. As Fritz said in a statement long ago: "This paper will focus central attention on these beneficent results of disaster...It is written in the belief that this focus is presently needed as an antidote to the overworked metaphors of pathology" (1961:2). It is difficult to see anyone using the individual trauma approach even thinking of making such a statement. In fact, so strong is the focus on negative aspects, that all observations of adverse psychological symptoms are taken as disaster generated when obviously some are carried over from the pre-impact situation. In contrast, the social sponge approach argues that the psychological consequences of disasters are "a function of a variety of factors, among which disaster impact is only one" (Perry and Lindell, 1978:105).

Overall, different research results will be obtained depending on the range of phenomena subsumed under disasters, whether the community context of disasters is considered, and if disasters are visualized as having possible positive effects.

6. Sixth, some of the differences in the two approaches stem in part, we think, from different basic models being used to approach disaster phenomena. For lack of a better term, there is what we will call a "medical" view of the world which implies among other things that there is an objective reality out there responsible for pathologies of various kinds. On the other hand, there is what might be called the "social problem" view of the world which assumes among other things that difficulties are primarily the result of definitional processes having no independent existence outside the actions of individuals and groups (Mauss, 1975). In the more extreme versions, adherents of this view argue that all disasters are politically defined events and in fact have no existence outside political definitions (Brown and Goldin, 1973). It would take us too far afield to explore the very interesting implications of this view. Let it suffice to say that the medical model and the social problem model of disaster behavior, when applied to the consequence of disasters for mental health, will not lead in the same direction.

The individual trauma approach tends to assume a medical model. The social sponge approach, instead, tends to assume a social problem model. How the phenomena to be studied is conceived, what is deemed important, and what should be done about it varies according to the basic model.

The basic contract in the two approaches is well set forth by a researcher who did a three year longitudinal study on psychological aftermaths of the Teton Dam flood disaster. She writes:

Basically, the assumptive framework on which the medical model rests leads to an interpretation which obscures the contradictions and essential features of the disaster experience and process of recovery. The medicalization of social phenomena...has two basic shortcomings which...obscure the important features of

social events. The adoption of the medical metaphor to explain social conduct over-psychologizes and depoliticizes social phenomena...

She further notes:

By ignoring the social context and by focusing on the causal primary of disaster impact, the medical metaphor leads to a misunderstanding about the nature of at least some of the post-disaster problems which have important consequences for disaster victims. It also fails to recognize, therefore, that the most efficacious solution to some disaster problems may reside in changes in public policy and in intervention aimed at changing aspects of the social structure. (Golec, 1980:162-163).

This view of matters is in striking contrast to that taken in a follow-up of the Buffalo Creek disaster. It is claimed that there was overwhelming evidence of psychopathology among the victims, and that 30 percent of the sample continued to suffer debilitating symptoms five years after the event. The explanatory factors offered are the extent of threat to life, degree of bereavement, prolongation of physical suffering, extent of displacement or life changes, proportion of the community affected, and the human occasioning of the disaster--primarily factors associated directly with the disaster agent and with little acknowledgement of the social context involved (Gleser, Green, and Winget, 1981). To the extent that researchers such as these use primarily a variant of the medical view of the phenomena, they will see different things, than do those who use a social problem model. The findings, to a considerable extent, are dictated by the implicit models, so the question becomes which is the better model, not what are the more valid research results?

We might note, contrary to what might be implied by the medical-social problem contrast, this is not totally a division between mental health practitioners and social scientists. For example, one of the earlier disaster researchers, the sociologist, Moore reported that "disasters lead to more long-run emotional stress than is commonly supposed" (Moore and Friedsam, 1959:139; see also, Moore, 1958). On the other hand, the DRC studies of providers of mental health services in disasters which we mentioned earlier found many deliverers or mental health services, especially those imbued with a community mental health ideology, often take a social problem approach to disaster phenomena. Many take the view that there are primarily "problems in living" in the aftermaths of disasters and not much in the way of mental illness or problems. Obviously, they draw their ideas from Szasz (1961, 1970) and others who have argued about the myth of mental illness.

This reference to the myth of mental illness allows us to make a transition to what we might call a sociology of knowledge explanation about the intellectual careers of the two approaches. Put very simply, in fact oversimplified, the argument here is that those taking the individual trauma approach are following the same incorrect path that the early disaster researchers followed in studying disaster behavior generally. There is little question that individual trauma adherents are very recent students in the area, almost all having become involved in the area at best only in the last decade. They tend to think of mental health responses in disasters in either/or terms, failing to see that disaster responses are always heterogeneous and differentiated. There is also an assumption that



disasters are necessarily bad in their consequences. They do not recognize this as an empirical matter not to be taken as given. Actually the range of differentiated behavior can be functional, dysfunctional, or a combination of both. The individual trauma approach still assumes that disaster victims respond primarily to the disaster agent or its immediate effect and have not yet fully recognized that the post-impact setting created by the response demands on organizations, is far more important in providing a stressful setting for victims. Explanations are sought primarily in terms of the inner psychological dynamics of victims rather than the social context in which they operate, as if the former could be independent of the latter. In fact, this approach has never addressed the fact that since psychiatric epidemiological research indicates that "normally" about 15% to 20% of the general population have mental health problems significant enough to warrant some kind of professional intervention (President's Commission on Mental Health, 1978), any post-disaster study would have to find rates clearly exceeding such figures to make a case that the disaster created the mental health problems found.

In our judgement, the individual trauma approach is still at the mythological stage that most of the social and behavioral disaster researchers were at about two decades ago. It operates with myths about homogeneity, dysfunctionality, disaster agent responses, and individual foci that have now been abandoned in most other research in the disaster area. This "lag" among those taking the individual trauma approach partly results from their failing to take advantage of what is known about disaster behavior in general. That as late as 1980, one of the leading proponents of the individual trauma approach could write, "there have been numerous instances of looting, with a breakdown in community cohesiveness and cooperation after the initial stages of the disaster" (Frederick, 1980:xiv-xv), is to fly in the face of all the research findings over a 30 year period on this matter (see review articles cited earlier). A much earlier writer who imposed theoretical dogma on empirical data can easily be excused because she wrote at a time when most researchers still had certain mythological assumptions (Wolfenstein, 1957, who nonetheless raised some excellent questions), but more recent theoretical statements have no such excuse.

If we were to speculate about the future, we would forecast that the individual trauma approach will become part of the history of the development of the field of disaster research. In fact, there are signs that we are entering a transition stage from mythology to reality with respect to the psychological effects of disaster occasions. We have in mind some studies of special populations which might be thought to be more vulnerable to extreme stress than others, and some research which has been done outside of the United States with respect to disaster-related mental health problems.

The elderly and children are thought by many to be especially vulnerable to extreme stress (Cohen and Ahearn, 1980:4). But in a yet unpublished review of relevant disaster studies, Fairchild recently concluded that: "one point of agreement among the findings to date is that few if any long-term physical or mental health problems exist for the elderly" (1984:12). Two of the most systematic studies (Cohen and Poulshock, 1977 on the Wilkes-Barre flood; Bell et al., on the Omaha tornado), in fact, argue that the aged coped better and made better adjustments than similarly affected younger age groupings in the victim population. Other research is similarly non-supportive of the notion that the post-impact mental health responses of the aged are likely to be worse than those of other age categories (Huerta and Horton, 1978; Kilijaneck and Drabek, 1979; Ollendick and Hoffmann, 1982). Systematic work on children is sparser (e.g., Howard and Gordon, 1972), but unpublished research seems to indicate children are more likely to reflect the reactions of their parents than anything else; not a very new speculation since it was suggested

several decades ago (e.g., Bloch, Silber, and Perry, 1956). At any rate, there is little systematic research in the literature, on the reaction of special populations which supports the individual trauma approach. It is probable that we have similar misconceptions and myths about the general vulnerability of special categories of the population much like we have had about other aspects of disaster behavior.

Studies about mental health problems in disasters outside of the United States are also appearing and are roughly similar in what they report to the research work on American community disasters. The most extensive studies have recently been carried on in Australia, Italy, and Nicaragua. There are some inconsistent findings from researcher to researcher. Overall, however, there is little finding of major psychopathology or psychoses in immediate post-impact periods (however, cf. Bennet, 1970; Benin, 1981), and little severe dysfunctionality in the long run; but, many milder psychological disturbances in the short run. For instance, "there was rarity of severe psychotic reactions and panic" in the Skopje, Yugoslavia earthquake (Popovic and Petrovic, 1964:1170), and a significant drop off in numbers of new admissions for mental disorders in the hospital system after the southern Italian earthquake (Greco et al., 1981:501). In a study of all admissions between 1969-1976 to Nicaragua's only psychiatric hospital, "the actual increase is not different from the trend that would have been expected had there been no earthquake" (Ahearn, 1981:24), and even neurotic reactions were of short duration in a massive chemical disaster in Germany (Kroiss, 1925). In both floods in Brisbane, Australia (Abrahams et al., 1976) and in the Friuli earthquake in Italy (Cattarinussi and Pelanda, 1981), researchers found widespread but moderate psychological effects in the aftermath of the disasters. Thus, generally, there are few supportive findings for the individual trauma approach in the non-American studies.

In an Australian study of the aftermaths of Cyclone Tracy which devastated the city of Darwin, an attempt was made to measure long term effects. Perhaps the most significant findings was that victims who never left the devastated city showed a better adjustment than returning evacuees, and evacuees who had not yet returned to Darwin exhibited the most psychological problems (Milne, 1977). Continuous living in the midst of almost total destruction did not seem to have the impact that the individual trauma approach would seem to imply. On the other hand, the differences seemed to be accounted for not by what happened at the time of impact, but by the nature of the post-impact social relationships and social settings of the cyclone victims.

Let us conclude by saying the following: From our point of view, there is a basic policy question involved in all of this. Is there a disagreement here between two approaches to the consequences of disasters for mental health which has some practical significance? Is this a difference that makes a difference? Or, is this merely an academic or intellectual exercise which might be of theoretical interest and excite researchers, without having any meaningful implications in the everyday world of policy and operations?

We think there is a meaningful difference here: an important one in terms of disaster planning and response and for both deliverers and receivers of disaster-related services. If the individual trauma approach is essentially the correct one, we should be extending crisis intervention program, preparing for outreach services for victims, and generally gearing up to handle the psychic trauma of

those who have to adjust to the impact of a disaster agent. If the social sponge approach is the more valid one, a different strategy and use of resources is indicated. We should be reorganizing the federal, state, and local disaster bureaucracies; we should be giving in-service training to providers and deliverers of services; and generally we should be gearing up to handle a social problem which is mainly the result of organizational inefficiency and ineffectiveness relatively independent of disaster agents. The individual trauma approach primarily assumes the post-disaster period as the time in which most things can be done and considers the individual, or at most the family, as the basic unit to be worked with. The social sponge approach primarily looks at the pre-disaster period as the time in which most things can be done, and considers the group or organization as the basic unit to be worked with. There are differences here which are major.

In an ideal situation with unlimited resources, we suppose all approaches could conceivably be used. But in a real world with finite resources, a more specific stance should be taken. Even if practitioners and scientists want to avoid the issue, the current social climate and trends will not allow them to do so. This is an age where greater accountability is increasingly being asked of practitioners and scientists. It is a period of time when the public at large is demanding greater input into what is done for and to them as consumers, as users, or in the context in which we are talking, as disaster victims. We think that the greater accountability and the greater participation being demanded is a very healthy thing. This means, among other things, that eventually those operating as practitioners and researchers in disasters will have to take a more definite stand on the consequences of disasters for mental health.

We have presented our views on a particular controversy in the disaster research area. We believe the views we have stated. However, there is much that puzzles us, and we are far from certain how much any of us really understand about the nature of disasters, the nature of mental health, and the relationship between the two. The latter particularly becomes problematical to us when we encounter observations such as the following. In the San Fernando earthquake of 1971, the Olive View Mental Health Center in a hospital complex was very damaged with the two story building holding 50 patients in locked wards collapsing into a one story structure. The 131 psychiatric patients responded "very well;" they "seemed to react during the disaster with a great deal of stability,...attempting to help each other." In one case:

One patient had been hospitalized a few hours before the earthquake, and was so hyperactive and uncontrollable that he was placed in restraints--a practice used only in extreme situations. At the time of the earthquake, a nurse disengaged his restraints, and told him that an 18-year old catatonic girl could not get out of the building without help, and it was up to him to direct her. He escorted her safely out, remained coherent for a few hours, then regressed (Stein, 1974:40-41).

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