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CONVENTIONAL BELIEFS AND
COUNTERINTUITIVE REALITIES

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THIS PAPER DISCUSSES MAJOR MYTHS AND WIDELY HELD INCORRECT beliefs about individual and group behaviors in disaster contexts. Why can we categorize such views as invalid? Because now there has been more than half a century of systematic social science studies (and an earlier half century of less well known scattered works) that have established the actual parameters of the behavior of individuals and groups in natural and technological disaster situations (for recent summaries of the extensive research literature, see Lindell, Perry, and Prater, 2006; National Research Council, 2006; and Rodriguez, Quarantelli, and Dynes, 2006). All is not known, and serious gaps remain in knowledge about important topics, but we are at this time far beyond just educated guesses on many dimensions of the relevant behaviors.

Our focus is on six different behavioral aspects of disasters, primarily occurring around the impact time period of such crises. Stated in just a few words, we look at panic flight and at antisocial looting behavior, supposed passivity in emergencies, role conflict and abandonment, severe mental health consequences, and the locus of whatever problems surface. We present what is often assumed, believed, or stated on these matters—at least in popular discourse and to a varying extent in policy, planning, and operational circles—as over against what study and research has found.

The concept of "myths" was coined in the early 1950s by researchers who were studying the natural and technological "disasters" that

were taking place in American society at that time. These researchers were never under any illusion that these were the only kinds of collective crises that societies could suffer. This idea was reinforced in the early 1960s when there were many urban and university riots that "disaster" researchers studied even as they recognized they were along some lines qualitatively different from the earlier natural and technological disasters looked at in the field. To some researchers these became known as "conflict crises." In the decades that followed, additional notions about mega-disasters/catastrophes, as well as even newer kinds of crises, also qualitatively different, crept into the literature.

Without going into the uneven historical evolution of the thinking about different kinds or types, we need here to identify distinctive aspects of the four kinds of collective crises just noted. This is because the idea of the myths is not equally applicable across the board. Particularly important is that the idea makes sense for disasters but needs qualification for catastrophes.

A few researchers have argued for decades that there are disasters and there are catastrophes. This is not simply substituting or replacing one word with another to try to maintain the idea of disaster myths, as has been incorrectly implied (Handmer, 2007, for example). Rather, it involves an attempt to differentiate major differences between one kind of social crisis and another as the result of the impact of a destructive natural or technological agent (see Quarantelli, 2005a). The characteristics of a catastrophe in ideal-type terms are the following.

In a space-time framework, a catastrophe occurs when 1) within a relatively short time period, 2) a large but not necessarily fully contiguous area with multiple land uses and diverse communities, is 3) perceived as being subjected to very major threats to life and property, thus 4) requiring immediate responses to start restoring a routine social order.

This kind of social occasion results in:

▸ most everyday community functions and social institutions being sharply and concurrently interrupted (in contrast to this not happening in a disaster);

- many organizations, including those that are emergency oriented, either cease operating or do so in a markedly reduced manner (in contrast to a disaster where few organizations in a community deteriorate to such a degree);
- many local community officials and others are unable to undertake their usual work roles (in contrast to this happening only on a small and selective scale in the typical disaster);
- most help or aid has to come from more distant areas (in contrast to massive convergence in a disaster from the community itself and/or from nearby areas);
- the immediate and ongoing crisis is socially constructed by nonlocal mass media supplemented by cable television and Internet bloggers (in contrast to a disaster, where the greatest attention is by the local media with only incidental and brief reporting carried out by cable and national mass communication outlets); and
- high-ranking government and political officials and organizations from the national (and sometime international) level become involved (in contrast to a disaster, where there is at most limited and primarily symbolic attention given by other than local persons and community/state agencies).

Then there is the question on whether our observations and findings about myths in disasters and catastrophes are applicable to conflict-type crises (such as civil disturbances, riots, acts of terrorism, and what the National Science Foundation increasingly labels "willful" disasters) as well as newer or emerging kinds of disasters (such as the spread of SARS and massive computer system disruptions). The answer is that the extrapolation that can be made is limited. There are features of such crises that are not seen or are more important in what has been established about impact-time behaviors in natural and technological disasters and catastrophes (see Quarantelli, Lagadec, and Boin, 2006). So while we make passing observations in what follows on these different behavioral reactions, we do not systematically discuss some distinctive or unique features in conflict crises and the newer kinds of disasters.

THE SIX MYTHS

1. Panic

Perhaps the most frequently used term in connection with disasters and crises is the word "panic." However, the referent of the word is widely diverse both in popular culture and the scientific literature. There are multiple denotations and connotations for the word.

Although written four decades ago, an observation by Jordan unfortunately still is true today. As he noted: "The literature on panic research is strewn with wrecked hulks of attempts to define 'panic.' When these definitions are placed side by side, one is confronted by chaos. . . . There doesn't seem to be a common behavior accepted by all concerned; even flight behavior is excluded" (1963).

Recent extensive discussions using the word (see Orr, 2006; Mawson, 2007) continue to present diverse and heterogeneous references of the term as was done more than half a century ago (see, e.g., Strauss, 1944). In 1954 we wrote: "Almost every kind of socially disorganizing or personally disruptive type of activity has been characterized as panic. The range includes everything from psychiatric phenomena to economic phenomena (e.g., the 'panics' involved in bank runs, stock-market crashes, depressions, etc.)" (Quarantelli, 1954: 268).

In the same article we note that a paper on panic a few years earlier (Meerloo, 1950) cited as examples of panic: lynch mobs, suicidal epidemics, individual and collective anxieties, plundering troops, spy hysterias, military retreats and surrenders, social unrest, war, psychotic behavior, mass hysteria, animal stampedes, confused voting behavior, orgiastic feasts, the activities of war refugees, and group tensions.

From that it could be argued that the only common dimension is that whatever it is, panic is something that is bad or unfortunate from the viewpoint of human beings and their groups.

Collective panic in its various conceptualizations has been more empirically studied, especially in the earliest days of disaster research, than most realize. A keyword search using the word "panic" of the Disaster Research Center (DRC) Resource Collection produced 295 items

that included very few non-English sources. In the professional natural and technological disaster literature there are at least five major sources of empirical data, mostly on collective flight behavior.

It was an explicit partial focus of the famous National Opinion Research Center (NORC) field studies, recognized not only as the pioneer work in disaster research but also as a classic piece of research (see Quarantelli, 1988). As such, the almost complete absence of panic flight that was found provided part of the initial formulation about the existence of "disaster myths."

It was a major topic addressed by the National Academy of Sciences (NAS). Our recent perusal of the archives of all National Science Foundation disaster-related committees and groups now in the possession of the DRC found that the topic of panic was intensively pursued for several decades, and a number of informal essays and memos were written on the topic both from re-analysis of previously gathered data as well as newly gathered data.

It was consistently looked for by DRC in its early field work. Field researchers irrespective of the particular focus of the field work involved were supposed to be sensitive to hearing about any possible panic phenomena. In reality, extremely few such instances were ever reported, but that in itself supported the notion that panic flight was very rare.

It was a long focus of empirical research by fire researchers and the National Bureau of Standards (as well as related work done in England; see, e.g., Canter, 1980). Totally independent of traditional disaster research, these researchers were specifically interested in movement of people away from home and other fires. Panic flight was so rarely found that eventually the very concept of "panic behavior" was deemed useless for fire research purposes (Bryan, 1980).

More recently there have been studies of occupant behavior in buildings at times of earthquakes that imply some cases of panic flight (Alexander, 1995). Unfortunately, these studies are innocent of exposure to social science studies of panic that have long distinguished

between rational and meaningful evacuation of buildings, and wild and uncontrolled flight. Our own personal first three field studies were of an earthquake, a series of separate house explosions, and a plane crash in a very dense urban neighborhood, where the difference between organized and uncontrolled flight was obvious.

The usage definition more commonly used by students of the problem equates panic with inappropriate flight behavior away from a response to threat or danger. Although used in popular discourse for centuries, the term panic as applicable to collective flight behavior was only conceptually developed in the early 1950s by disaster researchers (Quarantelli, 1954, 1960).

Does panic emerge at times of disasters? It is important to note that it is clear that at times that some reports of panic are just social constructions by mass media outlets. There is a famous study of a 1938 radio broadcast of a supposed "invasion from Mars" (Cantril, 1940), which has been both incorrectly reported and or highlighted by others and that itself conveys an incorrect picture. For example, if carefully read, the huge majority of radio listeners in the 85 percent and higher range simply heard it as a radio show. More important, anecdotal material used in the book taken from newspaper clippings, conveys an impression of widespread panic flight, but that conclusion was not drawn from the purposive sample used. In short, the text nowhere supports the notion of widespread panic flight.

In 1973 another fictitious broadcast reported the following:

> According to reports in the mass media, a fictitious radio broadcast about a disaster at a nuclear power station in southern Sweden caused widespread panic flight among the population in the area. A telephone survey of a representative sample (n = 1,089) in combination with unstructured interviews with police and other authorities indicates that no panic flight at all did occur (Rosengren, Arvidson, and Sturesson, 1975: 303).

In the sense of wild collective flight behavior, our conclusion is that it very seldom occurs since it requires the presence of rarely present concurrent social conditions. The necessary affective/cognitive factors can be categorized in different ways but include at least the following:

▸ Perception of an immediate great threat to self and/or significant others. It is extreme fear rather than anxiety that predominates since the risk to physical survival seems clear. Fear, no matter the magnitude, in itself is not enough to generate panic despite what some users of the term mistakenly assert.
▸ Belief that escape from the threat is possible (a perception that one is trapped does not lead to panic flight; this can be seen in entombed coal miners or sailors in sunken submarines). It is hope, not hopelessness, that drives panic flight.
▸ A feeling of helplessness in otherwise dealing with the threat and particular others are not seen as being able to help. If there is a perception that movement away from the risk is possible, an orderly or organized movement or evacuation from the location usually occurs. Such flight behavior is not panic behavior—as was overwhelmingly the case among the survivors who left the towers in the 9/11 disaster.

These necessary, collective panic-generating conditions can be reinforced by the presence of two other conditions. Flight is more likely to occur among an aggregation of strangers rather than where there are many prior social ties among those present. Also, flight is more likely in social settings where there are prior cultural norms that can make such locations panic inducing (closed and confined physical spaces such as theaters and night clubs).

The concurrent presence of all these conditions can lead to nonsocial behavior that in its most extreme form can lead to the dissolution of the most important social ties (such as mothers abandoning their small children) and violation of normal social norms (such as those

fleeing trampling upon one another). In the most extreme case, collective panic flight is the very opposite of organized behavior, although the flight that is directional tends to be short lived in both time and distance, and is not automatically contagious. Nevertheless, panic flights show human beings at their worst.

Fortunately, the phenomenon is rare. We have found clear-cut cases of collective panic flight in less than 100 disasters in a half century of professionally looking for the phenomena, and even in those cases usually only a small minority of those present in the situation engaged in anything resembling panic flight. Even those atypical researchers who still are quibbling over the nature of panic (e.g., Alexander, 1995, who mixes solo and collective "panic") grant that in terms of sheer frequency it is a rare occurrence.

To conclude, collective panic flight in disasters is such a rarity that it is not a major problem and has very little overall negative consequences compared with other bad effects. Also, while some current researchers continue to use the word "panic" in imaginative ways (Clarke, 2002, 2006), we personally think the term should be dropped as a social science concept. The behaviors involved can all be described and explained by other more powerful social science concepts (as Johnson, 1985 used social role). A major move in such a direction would free social scientists from the ambiguities and imprecisions of continuing to use a word drawn from popular discourse.

2. Antisocial Behavior

Is there antisocial behavior, especially looting, during and after disasters? Does Mr. Hyde take over from Dr. Jekyll? Disaster films and popular beliefs (as manifested in reluctance to evacuate because of a concern that one's possessions might be looted) assume such behavior is common.

Popular accounts of some earlier American disasters such as the Johnstown flood or the Galveston hurricane frequently allude to rapes and murders on these occasions. Such violent behaviors were almost

always attributed to members of lower-class ethnic or racial groups in the community. However, since such behaviors are seldom mentioned by anyone in recent and current disasters in American communities (except for some very dubious cases that supposedly occurred after Hurricane Katrina in New Orleans), we limit our discussion of antisocial behavior to looting phenomena (although since such attributions still are sometime made especially in catastrophes in developing societies, study of violent antisocial behaviors ought to remain on the research agenda).

The word "looting"—derived via the Hindu *lut* from the Sanskrit *lunt* meaning "to rob"—came into Western European languages to refer to the plundering undertaken by invading armies. Of interest is that since the "to the victors belong the spoils" notion prevailed, the term looting applied to the military was not uniformly viewed as indicating criminal or deviant behavior. There were even applied rules on what buildings could be entered, what could be looted, and the time frame within which the takings could occur (Green, 2006: 9). Only with the advent of international law and especially the Hague Convention of 1907 was looting in a military context universally condemned and prohibited (Green, 2006: 10).

As far as civilian disasters were concerned, negative views about looting developed much earlier. Condemnation of taking goods for one's own use is negatively viewed in Hebrew religious writings and was explicitly prohibited under Roman law (Green, 2006: 10). However, the word "looting" itself was almost never used in both popular and contemporary historical descriptions of disasters. Our examination of a number of such books on disasters in American society in the nineteenth nineteenth century found not a single use of the word "looting" although the illicit taking of goods is often mentioned.

The first systematic and continuing professional use of the word appears to have occurred in the NORC field studies of disasters from 1949 to 1954. However, although personally a member of the field teams, we have no recollection of how and why the word explicitly

came to be used. Perhaps it came out of the fact that Charles Fritz, the day-to-day operational manager of the project, had a military background and minor involvement with the US Strategic Bombing Survey (USSBS), which did look at whether antisocial and looting behaviors occurred during World War II in German and Japanese cities. The survey, reinforced by British studies of their own civilian population, concluded that looting was not a serious problem in and after massive air bombings.

There is a substantial but not massive body of empirical and theoretical literature on looting. More is available on looting behavior in civil disturbances than disasters, primarily because of scholarly interest in the disturbances of the 1960s and 1970s in American cities. However, except for an occasional study, little scholarly attention has been paid to the topic in civilian disasters and crises outside of the United States.

Probably the most sophisticated analysis of looting is a very recent article by Green (2006). He describes how looting has been viewed over the centuries, the complicated legal issues in defining the behavior and related activity, and how it tends to be dealt by the US court system and other authorities.

It appears that from a quantitative viewpoint, the DRC over a 43-year period has done the majority of empirical studies of looting. Major attention to looting phenomena has been paid by the center in terms of focused attention in particular studies where specific questions on the topic were part of field study questionnaires. Large-scale survey studies such as on the Xenia tornado and the Wilkes-Barre flood disasters generated considerable quantitative data. Looting in civil disturbances has been done over a number of years (see Dynes and Quarantelli, 1968; Tierney, 1994). Field studies by the DRC of looting in catastrophes such as Hurricane Katrina continue (Barsky, 2007).

The overall conclusions from all the empirical research can be summarized as follows. As we wrote recently (Quarantelli, 2007), looting of any kind is unusual in the typical natural and technological

disasters that afflict modern, Western-type societies. But the picture is rather mixed in other kinds of social systems, with looting seemingly occurring more often than not in developing systems (in 2007 there was massive looting in catastrophes in Peru and Columbia). There is also a distinctive pattern to the rare looting that occurs in disasters that is different from what emerges in civil disturbances. There are atypical instances of mass looting that only emerge if a complex set of prior social conditions exist, namely what is seen in a catastrophe rather than just a usual kind of disaster (to the necessary conditions other sufficient conditions are also needed).

Given what many people think they know about Hurricane Katrina and New Orleans, it may not be amiss to discuss that occasion from a research perspective. To some it appeared that the apparent "looting"' behavior that emerged contradicted the notion that looting in disasters was a myth. Actually, anyone who had ever systematically looked at the research literature knew that it never said that looting never happened. The basic proposition advanced from the earliest studies half a century ago was that it was very rare and in many cases almost nonexistent in American-type communities except for some souvenir hunting. That researchers had from the start believed that looting could happen is supported by the well-documented fact that scholars more than three decades ago identified and discussed the four characteristic patterns of looting in natural and technological disasters. We said that when looting did occur, it was socially and overtly condemned by others experiencing the disaster; it was covertly done, undertaken mostly by isolated individuals or pairs, with the objects looted being a matter of chance or opportunity. In contrast, the same researchers noted that in the often massive looting in civil disturbances was socially supported, undertaken overtly by small groups (including family units), socially approved, with the looting taking place at targeted sites. These two distinctive patterns were set forth in numerous publications, but apparently they were never read or if read were badly misread by some who challenge the looting myth.

Hurricane Katrina created a catastrophe in the New Orleans area but a much better case of mass looting occurred in St. Croix in the US Virgin Islands after Hurricane Hugo, which made the former situation look like a picnic (we have earlier reported the following in Quarantelli, 2007). We undertook three extensive field studies including a systematic quantitative survey of all businesses in the four shopping centers or malls on the island. The looting was by any criteria massive. Not only were all consumer goods in sight stolen, but electrical and wall fixtures as well as carpets were also stripped. The biggest malls with over 150 shops as well as two others were swept clean with less than 10 percent of the businesses reporting they were not totally looted.

The looting was initiated by preexisting juvenile gangs of delinquent youths (as was the case in the 1977 New York City blackout) who targeted stores with large amounts of consumer goods such as television stores (but not food supermarkets). The second stage was when initial nonparticipants who did not have everyday criminal lifestyles began taking goods also from other locations such as hardware stores. Finally, a much larger number of people joined in targeting stores with basic necessities (groceries, for example) who generally did not loot items taken by the first two sets of looters. Overall, the looting pattern was that which earlier researchers had found in civil disturbances as discussed previously. (This is a puzzling finding, for which no explanation has been offered by anyone to this date.)

However, widespread rumors to the contrary, we were not able to find a single authenticated case of looting of private residences, schools, hotels, the four banks on the island, the one industrial complex with valuable equipment, or any of the resort hotels. The looters used no physical force, and, at worst, made only unfulfilled verbal threats.

Our explanation of this very atypical occasion of mass looting is that the necessary condition present was simply that it was a catastrophe rather than just a disaster. However, not all catastrophes lead to looting. A recent book using historical data on the 1886 Charleston,

South Carolina earthquake documents that it was clearly a catastrophe, with two-thirds of the population becoming homeless but "no reports of looting and only scattered accounts of thievery" (Cote, 2006: 201).

We would argue that in addition to the necessary conditions there were additional sufficient conditions that tipped the balance toward a major outbreak of looting. As in St. Croix as well as New Orleans, we have identified three such factors. They are a pre-impact concentration of disadvantaged people subject to everyday perceptions of vast differences in lifestyle; a subculture tolerant of minor stealing along with everyday organized youth gangs involved in serious crime such as drug dealing; and a local police force that was inefficient and corrupt (both in New Orleans and St. Croix police officers openly engaged in the looting, something incidentally not found in civil disturbances).

Whatever the extent of the looting, it always pales in significance to the widespread altruism that leads to free and massive giving and sharing of goods and services. Even in St. Croix and New Orleans, the pro-social help given to others swamped the antisocial behavior that did occur. There was no comparison.

Finally, we should note that after the immediate emergency time period, in the days that follow, American police statistics usually indicate that there is an actual decrease in reported criminal behavior such as murder and theft. Traffic and related violations also tend to be below pre-impact levels, although much of that can be attributed to suspension or the ignoring by the police of such legal norms. Eventually, however, all standard criminal behavior rates return to what they were at the time of the impact of the disaster/catastrophe, with future increases following whatever pre-impact trend lines existed.

Yet it has become increasing clear to researchers that there is considerable illegal behavior in the recovery and mitigation phases of American disasters. There is a large amount of "white collar" crime by middle-class participants who turn in false insurance claims or other-

wise obtain postdisaster relief aid which they know they are not enti-
tled to receive. Then there are major criminal acts during mitigation
activities involving some from the private sector and local govern-
ment officials. The violations of building codes and zoning regula-
tions are often carried out by network linkages involving, among
others, construction and building companies, elements of the real
estate sector, and government inspectors. This kind of criminal behav-
ior almost ensures that disasters and catastrophes will be worse than
they would otherwise be by way of physical destruction and human
casualties. Overall, the frequency and significance of disaster/catas-
trophe-related white collar and business crimes dwarfs by almost any
criteria even the worst of those rare mass looting occasions in catas-
trophes.

3. Passivity in Emergencies

Clearly, panic and looting are active responses. But the third myth
concerns almost the opposite response: that is, the notion that survi-
vors of disasters are stunned into inaction or passivity. The initial shock
of undergoing the impact of a disaster supposedly makes individuals
dazed and unable to function or react to the situation.

While media accounts often allude to survivors being in a state of
shock, the notion of passivity as part of a disaster response comes out
of a theoretical essay half a century ago by an anthropologist, Wallace
(1954). In fact, he used the term "disaster syndrome" to designate the
phenomena he said was often present in the early stage of impact. The
term "disaster syndrome" was used to characterize the supposedly
dazed, disoriented, shocked, and apathetic characteristics that disas-
ter victims showed. It is worthy of note that another description and
analysis of the same disaster studied by Wallace, the Worcester tornado
(Rosow, 1977) conveys as we read it a rather different account of the
responses of impacted individuals and groups.

The term offered (also favored by Wolfenstein, 1957) failed
to be accepted by the other earliest disaster researchers since they

saw just the opposite of what Wallace projected (see Aauf der Heide, 2004). Instead of passivity and inaction, they documented over and over again that survivors usually quickly moved to do what could be done in the situation. A good example of this is that by far the bulk of search and rescue activities, digging into the debris, and heading the injured toward medical treatment is overwhelmingly carried out by survivors looking for their family members, neighbors, coworkers, or those known to have been around the pre-impact physical location of survivors (see Denver, Perez, and Aguirre, 2007). These are truly the first responders in disasters, both in terms of time and the numbers of bodies found. Even the very earliest disaster studies found that in the first half hour after impact, usually about a third of survivors searched for missing persons, with about 10 percent taking an active role in rescue (Fritz, 1961: 7). But such activities are seldom reported by the mass media, which understandably instead focuses on the formal search and rescue efforts of emergency organizations (which are only significant if specialized knowledge or equipment are needed—as may be the case if massive piles of debris need to be searched, although, as the World Trade Center site showed after 9/11, the search usually becomes one of finding bodies rather than living injured).

What students of disasters have consistently discovered now for decades is what the earliest researchers reported, namely, a great deal of emergent behavior by survivors. So the research observations and findings have focused on that emergence rather than about the lack of passivity or inaction (or the disaster syndrome). Thus, unlike in the case of panic or looting, where studies have specifically focused on those phenomena, in the case here the myth of passivity is so accepted that, except for the earliest days of study, scientific work has centered on the emergent behavior in disasters.

Two aspects of that emergence should be noted. It is of a collective nature, not in the sense of any overall organization or coordination but in the sense of myriad small informal groupings and networks that are unaware of what others are doing. The behavior is adaptive in that

it is functional for the situation, arising because there are immediate problems that need "solving."

This can be further documented in what happened in the New Orleans area during Hurricane Katrina. What we earlier characterized as what is seen in a catastrophe clearly was present in the multiple communities in and around New Orleans. The overall response to that was not the disorganization, social chaos, and dysfunctionality that was the staple reporting by the national mass media. To be sure, certain government agencies for all practical purposes ceased functioning. However, what emerged on a massive scale were smaller, informal entities and network linkages, sometime but not always anchored in pre-impact known groups. Researchers were able to find and study this emergent phenomenon. For example, using field gathered data, DRC field teams were able to research what emerged in neighborhoods, among both informal and rescue teams, in hospitals, and in hotels (see Rodriguez, Trainor, and Quarantelli, 2006). There were literally hundreds of such new groupings in the New Orleans area, including many studied by others (especially in the religious sector), both from within and outside the large impacted area. Written popular accounts of participants in other places such as the city hall and a major jail provide further indications of how widespread pro-social emergent behaviors were in the area (see Forman, 2007, and Inglese, 2007).

Their pro-social and very functional behavior dwarfed on a very large scale the antisocial behavior that also emerged. Improvisation and innovation took place because the everyday traditional routines could not be used or were ineffective in dealing with the problems that had to be addressed. Of course, not all that was created was perfect; there was at times a degree of inefficiency in what was done. However, what came into being not only prevented the New Orleans area from a collapse into total social disorganization, but little by little provided at least semi-solutions for many immediate and intermediate problems that required attention. A decentralized response, as was true of the

emergent groups we have discussed, is almost a necessary consequence of a catastrophe.

4. Role Conflict and Role Abandonment

Are participants in a disaster likely to favor familial responsibilities over those that are work related? This question is sometimes posed by emergency managers and crisis planners (as well as by some ideologically driven political activists, such as in anti-nuclear groups). In general, the view of such people is that family will be favored over other responsibilities. A recent online article by medical personnel with the title: "Will the Healthcare Workers Go to Work During Disasters?" suggests that, especially in a crisis such as an avian flu epidemic, perhaps a majority of personnel might not go to work and would instead stay at home with immediate family members (see www.medicalnewstoday.com/printer-friendlynews.php?newsid=70828).

Overwhelmingly, disaster researchers consider the point of view just expressed as a "disaster myth." As a recent review of this topic stated: "Belief in this myth by the public and even government officials continues and has been reinforced through popular culture and erroneous reporting by the mass media" (Kushma, 2007: 4). However, while we agree with this statement, there sometime has been a failure to note an important distinction between role conflict and role abandonment. The quoted author, to her credit, does differentiate between the two and stresses that conflict does not generate abandonment.

The topic has been researched since the earliest days of disaster studies, more than a half century ago (Killian, 1952), with empirical work (such as Marks and Fritz, 1954; White, 1962; Mileti, 1985) predominating over more theoretical analyses (such as Barton, 1963; Quarantelli, 1978; Friedman, 1986). While the body of relevant systematic literature available is not large (we estimate several dozen empirical studies at most), much of it is of a quantitative nature. This probably results from the fact that role abandonment is a behavior that can easily be observed and measured compared to most other disaster phenomena.

The basic themes in the literature are fairly clear cut. Many personnel from emergency and response organizations such as the police and hospitals are subject to considerable role conflict that generates psychological strain and stress. They consciously feel a concern about ensuring the safety of their family and significant others, and yet also feel they have professional responsibilities to carry out their work. Various adaptive and coping mechanisms come into play (such as asking others to check on family members). On the other hand, despite intense role conflict at times, it has been difficult for researchers to find clear-cut authentic cases of role abandonment. In a review of several dozen disasters where more than 500 organizational officials were interviewed, DRC found only a handful of marginal instances of possible role abandonment. Other studies by the National Academy of Sciences (Fritz, 1987, in a major unpublished but excellent review that only surfaced this year), and Meda White, (1962) as well as Mileti (1985) and Rogers (1986) have all supported the findings of the DRC work.

So our concluding proposition is that role conflict is common but role abandonment from that is rare. However, two additional comments need to be made on the topic.

First, the observations and findings made are applicable at present only to American society. There are occasional anecdotal examples described in the literature on disasters elsewhere, but as far as we know, there has not been a single systematic study on the topic in that body of work. Our guess is that a role conflict/role abandonment linkage is so rare elsewhere that it simply has not caught the attention of non-American researchers even though some are extremely conversant with the American literature.

Second, recently some have mentioned that during Hurricane Katrina, 240 of 1,450 officers on the New Orleans police force apparently never reported for work, and later 51 officers were fired for "abandoning their posts" (Kushma, 2007). There is no documented case in all American disasters and catastrophes of any similar kind and scale of role abandonment (however, the behavior of the St. Croix police force

during Hurricane Hugo came close to what happened in New Orleans). Clearly, such mass abandonment should be studied and explained, but it may have little to do with being a consequence of role conflict. There is considerable evidence that the New Orleans department was highly dysfunctional long before Katrina, and could not be depended on to carry out its responsibilities in any professional way. In other words, the linkage in Katrina is not between role conflict and role abandonment, but instead between pre-impact major structural flaws and mass work abandonment around impact times. (This might be a research route to follow in the case of developing societies where there have been at least persuasive anecdotal stories of mass role abandonment by government officials of all kinds, especially in catastrophes).

5. Sudden and Widespread Mental Health Breakdowns

Are major mental health problems likely to result from the extreme stress individuals can be subjected to in disasters? Our view is that this is not the case, at least in the sense of being a frequent and significant problem. We should quickly add that this is not the viewpoint of most of those who have professionally addressed the topic in some way or other. So we will be discussing something extremely controversial (see Tierney, 2000), with two widely held but two rather different scientific views about the topic (which is not true of the previous four topics, where there is generally only one major professional social science position).

It would take us too far out of the way to try here a systematic evaluation of the two approaches. Instead, we will note three general points. First, there haves been two points of view for at least a century. Second, the differences stem as we have previously discussed from a variety of factors that seem irreconcilable (Quarantelli, 1985). Third, it makes a practical difference as to which position is the more valid. We elaborate on these points in the following.

Interest in the topic is hardly new. Attention even predates the systematic development of social science disaster studies after World

War II. For example, the Messina, Italy, 1908 earthquake, which killed over 100,000 people, making it one of the largest catastrophes ever, provoked a series of studies on the psychological consequences for victims or, in contemporary terminology, their mental health status. Since we discovered that research only a couple of years ago and there had been no references in the cited literature up to then, we searched and found what had been published, especially in Italian professional journals.

There were a series of articles in a 1911 issue of the *Italian Rreview of Applied Psychology*. Other papers appeared in the *Italian Review of Neuropathology, Psychiatry and Electrotherapy* in 1909 as well in the same year in the *Archives of Criminal Anthropology, Psychiatry, Legal Medicine and Related Sciences*. Almost exclusively, the journal papers focused on what would today be called the negative mental health consequences of the disaster, although there is an interesting footnote in one article that said many individuals reacted well, but that would not be addressed in the article. As far as we can see, this outbreak of studies was never built upon and they seemed to have completely disappeared from the awareness of later scholars until we found them recently.

It should be noted that a focus on mental health effects of disasters (to use present-day terminology) was not peculiar to Italy. An American physician (Robertson in 1907) produced a paper entitled "earthquake shock considered as an etiological factor in the production of mental and nervous diseases." The findings reported are fascinating and relevant to this day. Although somewhat archaic language is used (for example, "insane asylums") the overall conclusion was that the 1906 San Francisco earthquake did not lead, to use today's words, to any increase in mental illness. The data used were interviews with medical personnel and the statistics obtained from what we would currently call mental health centers and hospitals. This study remained uncited in the disaster literature for nearly 100 years, and to this day is still unfamiliar even to those scholars interested in the mental health consequences of disasters.

Thus, the view that disasters create severe and widespread mental health problems as is generally the view set forth in the Messina studies, and the empirically based observation that the San Francisco earthquake did not lead to any significant increase in mental health problems, reflects the current division of professional opinion about the topic. Although finer distinctions have been made (including by this researcher and others—for example, Warheit, 1988; Tierney, 2000), generally speaking there are two camps. One argues that disasters result in widespread and severe mental health problems, that the negative effects are long lasting, dysfunctional for everyday behavior, and in some instances endure for a lifetime. For purposes of labeling, we will call this camp the "trauma of victims" approach. The other position is that mental health problems are comparatively rare and mild as well as being transient. lasting only weeks or maybe a month or two beyond the time of the disaster. This view we will call the problems of living approach.

Second, few challenge that undergoing a disaster or catastrophe will in varying degrees be stressful. The disagreement is on what are the psychosocial consequences of such an experience. In worst case scenarios advanced by the trauma of victims approach, where current emphasis is on post-traumatic stress disorder (PTSD)—in the old days it was called "going insane"—everyone, including people who just read or see in the mass media pictures of catastrophic disasters, is thought of as being potentially negatively affected. The "victims" (note the connotations of that very word) require psychological counseling and briefings.

The problems of living approach argues that while there are immediate widespread effects, much of the reaction is surface, non-persistent, not behaviorally dysfunctional, and that there can be significant positive psychological effects among survivors of disasters. Also the psychosocial difficulties that emerge for survivors are primarily the result of incompetent and ineffective organizations that require structural changes.

This paper is not the place to try and settle the differences between the two approaches. But we can discuss what might account for the differences in them. We have addressed this matter two decades ago (Quarantelli, 1985), discussing nine different factors that may account for the positions. Obviously we believe that overall the nine factors are more supportive of the problem of living approach. They are:

1. Given little overlap in studying the same disasters, it is possible that different researchers and analysts are observing *actual* differences in the mental well-being of those involved in various disasters.
2. What is taken as acceptable data and appropriate data gathering design varies considerably.
3. Data varies in interpretation on the basis of larger theoretical frameworks.
4. Different professional objectives and ideologies are involved leading to vested interests.
5. Differences exist in conceptions of what constitutes disasters.
6. There are differences in the basic models used to approach disaster phenomena.
7. There often is a difference in the length of the time frame used.
8. Overt behavior is considered more important than subjective feeling states in one of the approaches. And,
9. Dysfunctionality of behavior is given different weights in evaluation.

Third, a statement that we wrote 20 years ago still rings true to us:

If the individual trauma approach is essentially the correct one, we should be extending crisis intervention programs, preparing 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.

On the other hand, if the problem in living perspective is a more valid one, a different strategy and use of resources should be institutionalized. We should be reorganizing the federal, state, and local disaster bureaucracies, giving in-service training to providers and deliverers of services, and generally gearing up to handle a social problem which is mainly the result of organizational inefficiency and ineffectiveness relatively independent of disaster agents" (Quarantelli, 1985: 206).

Finally, it is not amiss to note that few researchers have looked for positive mental health resulting from the experiencing of disasters. Only in the last few years, apart from a few lonely disaster researchers who suggested it as a viable hypothesis long ago, has consideration been given to the possibility that the experience of a disaster could be positive for mental health.

This is not peculiar to the mental health area. There is a strong assumption that since disasters by usual definitions are something that is bad, the notion that there can be positive or good aspects of such occasions tends to be ignored. An unusual exception is Scanlon (1988), who in an excellent article describes and analyzes a variety of ways in which disasters can be positive or functional for individuals, groups, communities, and societies. More such outside-of- the-box scholarship is badly needed. The negative aspects of disasters should be empirically determined and must not be simply a priori matters of definition or assumption.

6. The locus of problems

Our sixth point is somewhat different from the previous five. For one, the point here cuts across the previous five topics discussed. It has to do with the implicit or stated major locus of the problems that we have examined. In short, where is the major source of the problematical aspects of crises? Overstated and to be qualified later, the general belief is that in disasters and especially in catastrophe, the major source of problems and difficulties are the individuals involved whose dysfunc-

tional behaviors can be dealt with only by highly centralized, top-down organizations that can impose command and control procedures. It is believed that such kinds of organizations are the only ones able to rise to the occasion of a crisis that usually involves nonfunctioning civilians and individuals.

The research evidence indicates just the opposite. It is the human beings and their informal groupings and linkages that typically rise to the daunting challenges that disasters pose. Formal, highly hierarchical, structured and bureaucratic organizations, whether pre-impact planned or post-impact imposed, are both the source and locus of most problems in community crises.

The basic theme we express here—that human beings do well and crisis relevant organizations do poorly—was noted long ago by the earliest disaster researchers. It is not a new idea. In fact, the strong tendency of much social science research to give priority to studying organizations (as is true of much DRC research), stems from the point made. However, for various reasons, the basic observation was not explicitly named as one of the traditional disaster myths, although the label of myth for the phenomena has recently begun to be used (see Tierney, 2003).

Both in popular discourse and the position of funding agencies, the answer is usually fairly clear. It is the individuals or people in the situation. The US military's support of the pioneering studies of disasters stemmed from a belief that the American civilian population might collapse in the face of atomic attack. This can be seen in the questions posed by the funding agency (OCD) that provided the initial support that led to the establishment of the DRC (see Quarantelli, 2005):

> Which elements in a disaster are most frightening or disrupting to people and how can these threats be met?
> What techniques are effective in reducing or controlling fear?

What types of people are susceptible to panic and what types can be counted on for leadership in an emergency? What aggressions and resentments are likely to emerge among victims of a disaster and how can these be prevented from disrupting the work of disaster control?

What types of organized efforts work effectively and which do not (that is, in terms of individual leadership, not organizational entities).

The overall theme is one that there would be a need for the "reduction of panic reactions" and the need for social control would be achieved by "securing of conformity to emergency regulations." As a veteran disaster researcher has noted, the assumption is that a crisis will have a disorganizing effect upon individuals: "they panic; they freeze; they become anti-social; they become traumatized; they become self centered: and thus they cannot be counted on for selfless action" (Dynes, 1994: 146). That was the past but it is also the present. As other veteran disaster researchers have observed, many present-day approaches to terrorism and epidemics are similar in their assumptions about how individuals react in crises (Tierney, 2000).

The command and control model, dominant in American society, has been heavily criticized by almost all disaster researchers for half a century as an inappropriate system for dealing with disasters and catastrophes (see, for example, reflecting the consensus of critics, Clarke, 2002; Dynes, 2007; Tierney, 2003). For our purposes, we want to narrowly focus only on the assumptions that the model makes about the behavior of individuals. It is almost taken for granted that disaster victims will react in highly individualistic fashion, will be competitive with others, will be self-centered, make irrational decisions, will be a danger to one another; all this indicating a total collapse of the civil order. Thus, this model assumes that authorities from above need to step in to ensure order. However, as we have indicated, the assumptions made are fundamentally incorrect.

Another unfortunate consequence of making wrong assumptions is that it draws attention away from the fact that responding organizations are usually the major source and locus of most of the problems that surface in disasters and certainly catastrophes. That is what research shows. The myth is that it is the victims, the individuals caught in such crises, who are the source and locus of the problems.

Let us now add some qualifications to our general theme here. Generally speaking, individuals and smaller groupings are unresponsive and uninterested in disasters during normal times. Survey after survey has documented that few Americans (except some living in disaster subcultures) prepare for their possible involvement in a disaster and are not much interested in doing so (Heath, 2007). However, the situation changes drastically at the time of the impact of a disaster, where almost exclusive focus is on what is happening and myriad options on what to do are consciously considered especially in intense interactions with others.

Not all organizations use command and control structures. Religiously oriented groups, for example, in terms of their belief systems, usually have flat structures with little by the way of command roles. Such groupings and networks were prominent in Hurricane Katrina, where the governmental control and command organizations were frequently ineffective and sometimes close to paralysis in the catastrophe.

OTHER POSSIBLE MYTHS

Our essay does not address all possible disaster/catastrophe mythologies. Is mitigation the best way of preparing societies and communities for disasters? A case can and has been made that creating better resilience would be a much better and far less costly path to develop (Douglas and Wildavsky, 1982). Who should be "in charge" at times of crises? As critics have noted, the very question itself makes a highly dubious assumption that some official or organization should and particularly could be in charge in the diffused and decentralized social setting that is typical at the height of a disaster or mega-disaster. Do

catastrophes really create the opportunity for major organizational, community, and societal changes? There is some evidence suggesting that at best and only under certain supportive conditions can there be some incremental changes in line with pre-impact trends. Are individuals and groups likely to engage in preparing for disasters if they are aware of disaster-inducing related threats and risks? This is often assumed in disaster educational or information campaigns and in public policies, the notion being that knowledge and information will lead to relevant actions.

Finally, there is a tendency to overstate the collapse of any social system. There are concrete and studied historical occasions that by any criteria were extreme catastrophes. There are documented data on the human and group reactions in Hiroshima after the atomic bombing. Out of a population of 245,000, at least 75,000 were immediately killed and another 75,000 were injured. Around the point where the bomb actually hit, there was total physical devastation for miles around. Yet, within minutes, survivors engaged in search and rescue, helped one another in whatever ways they could, and withdrew in controlled flight from burning areas. Within a day, apart from the planning undertaken by the government and military organizations that partly survived, other groups partially restored electric power to some areas, a steel company with 20 percent of workers attending began operations again, employees of the 12 banks in Hiroshima assembled in the Hiroshima branch in the city and began making payments, and trolley lines leading into the city were completely cleared with partial traffic restored the following day. Given the current status of a modern and vibrant city of Hiroshima, does this overall picture suggest another myth?

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