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THE EARLIEST INTEREST IN DISASTERS AND CRISES,
AND THE EARLY SOCIAL SCIENCE STUDIES
OF DISASTERS, AS SEEN IN A SOCIOLOGY
OF KNOWLEDGE PERSPECTIVE

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**THE EARLIEST INTEREST IN DISASTERS AND CRISES, AND THE EARLY
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KNOWLEDGE PERSPECTIVE (2009 version)**

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Introduction

Today, the social scientific study of disasters is a very flourishing area. There are around five dozen research centers and institutes in the world, as well as hundreds of researchers whose major professional work is focused on disasters and collective crises. These groups and scholars have conducted field studies numbering in the four figures and have written thousands of publications. The result of such activities is a body of findings, setting forth many well data-rooted propositions about individual, group, organizational and community behaviors in disasters and catastrophes. Major inventories of these findings have been set forth in monographs, books, handbooks and encyclopedias. In addition, this area of study has its own infrastructure in the form of newsletters, journals, websites and professional associations as well as regularly scheduled domestic and international meetings. Multidisciplinary research involving the non-social sciences is increasing, and within the social sciences disciplines such as management science, political science, public administration, social geography and sociology, more researchers are involved and more studies are being conducted than ever before. (See End Note # 1 for web sites that currently have much information on what is mentioned in this paragraph).

Is everything perfect? Most everyone would say, of course, no. Recently there have been critical reviews and evaluations (e.g., National Research Council, 2006; Tierney, 2007)

which have identified significant issues and problems that the field will have to deal with if the future is going to be better than the present. That granted, the present is markedly better than the past. Today, as just indicated, the field has never been more active and promising.

It might be asked, from whence has come this vibrant research-related activity? It has not been the result of a slow development over a very long period of time. Almost all of what we have just mentioned came into being only since the early 1950s. At least in terms of continuous and systematic scientific activities, the area is barely half a century old. However, disasters and crises were of major interest to human beings and their societies much prior to the last five decades. In fact, this essay initially looks at the very earliest happenings with respect to disasters and ends with the institutionalization of disaster research in the 1950s and 1960s. Thus, we start a very long time ago in prehistory, and conclude with the establishment in 1963 of the Disaster Research Center (DRC), the first social science research center anywhere in the world focused on disasters and catastrophes.

The Sociology of Knowledge Approach

So far there are only a handful of rather limited and selective descriptions of the evolution of interest in the study of disaster (End Note # 2 lists several such as Rubin, 2007 which confines itself to American society only from 1900 to 2005). There currently is no comprehensive and overall account that pulls together past and current attention to disasters, when and in what way different social science disciplines got involved, and how the topic has been approached around the world. This essay is offered as a first step towards such an all inclusive analysis and review.

This depiction could be approached in two different ways. One would be a historical account, discussing very specific persons and detailing chronological happenings. That would

emphasize the distinctive and unique actors involved (almost a Great Man theory of history). This will not be our approach. Instead we will use sociology of knowledge approach. This perspective involves a somewhat esoteric endeavor undertaken by a very small handful of sociologists. The perspective of the sociology of knowledge and science (not exactly the same, but we will treat them as equivalent for our purposes) is that "knowledge" is socially produced and validated, and that similarly "science" is always a social activity and carried out by human beings whose behavior is influenced by the norms, values and beliefs that all human beings are subject to at all times. Put more succinctly, the sociology of knowledge deals with the social origins of knowledge and science and the social influences on all substantive findings.

Research in this field has produced certain generalizations about origins and influences. Let us note three such generalizations, and illustrate how they apply to the disaster and crisis area.

First, the sociology of knowledge and science takes the position that the larger social context is at least as important, if not more so, in the development of a field of study than the internal dynamics or research findings of the field. This challenges the widespread belief or myth that science is almost all and always research-finding driven. However in accordance with the sociology of knowledge perspective, we take it for granted that the larger social context can and does encourage and discourage any development.

For example, as detailed later, the establishment of DRC owes as much if not more so to major cold war happenings such as the Soviet Union blockade of Berlin and the Cuban missile crisis in the 1960s, than it does to the initial and limited research proposal written by the three faculty members at Ohio State University (OSU), who ended up informally setting up the Center. In fact, it is clear that DRC would not have come into being at that time without the larger global

happenings just mentioned.

Second, the sociology of knowledge and science has discovered that the development of a field of study is far from being unidirectional or linear. Instead, there usually is a messy growth. There are false starts, arrested developments, resurfacing of ideas and observations earlier advanced but then forgotten, tangential dead ends, the advancement of downright wrong ideas, etc.

As illustrated later, the growth of the field of disaster research has been far from a neat, sequential, and cumulative development. For instance, certain ideas and frameworks which have been advanced in the field in the past, such as setting disaster studies into a social change framework, failed to develop in that direction despite initial attempts to do so by pre-World War II scholars. The field of disaster studies did not build on that framework, although later every now and then a link to social change is explicitly argued (e.g. Clausen, who says: “disaster is a very accelerated and radical type of social change” (1980:19).

A third sociology of knowledge and science idea is that there are only a limited number of significant observations (or findings), as well as turning points in the development of a field of study. They are best seen in retrospect, but they do make a difference.

For example, the field studies conducted at the National Opinion Research Center (NORC) at the University of Chicago between 1949 and 1954, found that human beings reacted very well to collective stress occasions (Marks and Fritz, 1954). They did not break down in panic flight nor did they react in antisocial ways. These and related observations by others led to the idea among scholars that “disaster myths” prevailed in much of the thinking about disasters and crises. Equally as important was the implication from these and other early analyses that organizations instead of individuals had to be the major focus of study if disaster and crisis

behavior was to be understood. It was the pioneering NORC work, which led to a primary emphasis in the area that still exists to this day of giving priority to field studies of organized behavior in disasters and crises (see Stallings, 2002).

The Earliest Reactions

When did the first human groups start to become aware of and react to disasters? It was very early in the course of evolution. Stories of disasters and other community level collective crises are as old as when the humans started to live in non-nomadic groups. This can be seen in numerous legends and myths, folk songs and oral traditions, and religious beliefs as well as archeological data that indicate floods and earthquakes as well as inter-group conflicts were frequent even before written accounts about such happenings appeared. As an illustration, a "great flood" story has circulated in ancient times in many different and widely separated social systems, and it is a result of independent origins rather than a diffusion process (Lang, 1985).

Perhaps of even more interest is that human communities as far back as their existence goes have often been proactive and reactive in the face of risks and threats to the lives and well-being of their members. This is contrary to what has sometime been believed and written (e.g., Quarantelli, 2000), with some early disaster scholars arguing that the association of religious beliefs with disasters and catastrophes leads to a passive attitude and inaction. This is the "Act of God" notion. This specific designation first appeared in the 14th Century. With the rise of science in Western Europe the term lost much of its explanatory value in educated circles, but it remained in common usage in part because it supported the vested interests of insurance companies. They did not have to be responsible in paying for unfortunate happenings that were totally outside of their influence and control.

However, scholars have made two observations that challenge the idea that a dominant

orientation from religion necessarily results in fatalism and consequent passivity to the occurrence of disasters and catastrophes. For one, it is noteworthy that in almost all religions, actions such as sacrifices, rituals, prayers and offerings can be used to appease and influence supernatural forces (see Stern, 2007 for the ways in which about a dozen major religions look at natural disasters). Second, assignment of a religious source for disasters does not preclude active responses. For example, in the 2004 Asian tsunami, many Indonesians while seeing the catastrophe as “sent by God either as a test of their love for him or as punishment for straying from his teachings” (Brummitt, 2007:1), nevertheless took very many pre-trans and post-impact actions to reduce the impact of the collective crisis. This is not only a modern day perspective. Hanska (2002) has documented how, in the Middle Ages in Europe, religious beliefs actually provided many ideas as how to survive and cope with natural disasters, and even how to prevent them.

It does seem that even if there is popular acceptance of the label “Acts of God” there was less fatalism and passivity even in the past than some of the pioneer disaster researchers thought was the case. It is clear that human beings have consistently acted as if they could do something about the occurrence of disasters.. Studies show that human groups from the very earliest times created a variety of informal and formal mechanisms and groups to prevent and to cope with disasters and conflict crises. A so-far-not-fully substantiated legend is that 25 centuries ago a Chinese Emperor ordered massive dredging and the building of diversion canals in an attempt to control the ever flooding Yellow River (Waterbury, 1979: 35). More undisputed evidence exists that mitigation measures were undertaken in the initial stages of the ancient Egyptian Empire. For example, the 12th Dynasty Pharaoh oversaw in the 20th Century B.C. what may have been the first substantial river control project, namely an irrigation canal and a dam with sluice gates.

And there is documented evidence that dams for flood control purposes were built at far back as 1260 B.C. in Greece (Schnitter, 1994: 8-9).

In ancient Rome, the first groups to fight fires were composed of slaves organized into bands. But when a fire in 6 A.D. burned almost a quarter of Rome, the bands were abolished and a professional Corps of Vigiles was created which had full time and trained personnel and specialized equipment. These kinds of professional groups expanded from Rome into the rest of the Empire, for example to Britain by at least the 5th century.

In ancient Greece and Armenia earthquake resistant building techniques was developed, although they had generally been forgotten by the Middle Ages (Massard-Guilbaud, Platt and Schott: 2002:31). Threats to urban areas particularly from floods and fires especially spurred mitigation efforts. As we have discussed elsewhere, starting in the 15th Century many measures, both structural and otherwise, were set up in Europe to safeguard against fires (Quarantelli, Lagadec and Boin, 2006). Dams, dikes and piles along riverbanks were built in many towns in medieval Poland (Quarantelli, 2000). The actions taken were not always successful, but the efforts undertaken show that citizens and officials in the face of everyday dangers were often not passive but proactive as well as reactive. Attempts at mitigation have waxed and waned through the centuries, but clearly organized efforts to prevent or lessen the impact of disasters is not just a recent happening. This kind of proactive approach was not invented by the US Federal Emergency Management Agency, as some social commentators seem to think.

Our general point in using these scattered examples and illustrations and in leaping centuries is to emphasize three things. First, there has always been awareness that hazards and risks for human beings and their communities could and did sometime set the stage for disasters and crises. Second, efforts were made at both the community and societal levels to try to prevent

or mitigate possible negative impacts from such happenings. And third, with only limited understanding of the physical and social components of natural disasters, all efforts to cope with them did not get very far (see Quarantelli, 2000 for more examples of efforts up to the late Middle Ages to cope with possible disasters and conflict crises). But there was a significant turning point about two centuries ago.

The Significance of the Lisbon Earthquake

When were "disasters" singled out and thought of as something that human groups could definitely do something about? Tracing the linguistic origins of the term provides a clue. The word "disaster" entered the English language either from a word in French (*desastre*) or Italian (*disastro*), the presumed original source depending on which etymological volume one consults. But there is agreement in both cases that the term is a derivation from two Latin words (*dis*, *astro*), which combined meant, roughly, formed on a star. So, in its early usage in the 16th Century, the word "disaster" had reference to unfavorable or negative effects, usually of a personal nature, resulting from a star or planet. So we have Shakespeare, in 1605 writing in the play, *King Lear*, that: "We make guilty of our disasters, the sun, the moon and stars" (Act 1, Scene 2). In time, the word came to be applied to major physical disturbances such as earthquakes or floods, or what came to be known as Acts of God. However, with the spread of more secular and non-religious ideologies, nature was increasingly substituted for the supernatural and the term natural disaster came to the fore. There are some suggestions in the literature that when changes in plate tectonics became identified with the occurrence of earthquakes that particularly accelerated the trend to thinking of such hazards as creating the possibility of natural disasters. Neither the hazard nor the disaster could be "attributed" to God or the supernatural. (We leave aside in this essay that this might also be seen as the start of a

separation in scientific circles between the concept of hazard and the concept of disaster, which however only became clearer near the end of the 20th Century, and is still today not fully recognized).

An event which happened in the late 18th Century was particularly significant in accelerating this secularist trend. In the early morning of November 1, 1775, a major earthquake impacted Lisbon, Portugal. At that time, Lisbon was probably the fourth largest city in Europe. The tsunami that followed, and the fires that broke out in various neighborhoods in the city further increased the damage and destruction. How many died is still very unclear. Estimates have ranged from 5,000 to 70,000; some educated guesses were that perhaps only 3,000 out of 20,000 buildings remained livable. By any criteria, a major catastrophe had occurred.

While the earthquake became the focus of attention and discussion in European intellectual circles (Voltaire in particular is well known for his writings on the earthquake), Dynes argues it was something else that led this to being "the first modern disaster" (2000: 10). In particular, the changes in conception of the meaning of the Lisbon earthquake occurred directly from the emergency response and early reconstruction of the city. As Dynes has written about that earthquake: a "natural" explanation for the cause of the earthquake emerged. That explanation was not related to the growing acceptance of new geologic explanations about earthquakes, although some of those theories were beginning to be generated. Nor was that explanation related to the philosophical challenges to religious authority which characterize the intellectual climate which has come to be known as the Enlightenment" (2000: 17).

Instead the change in conception occurred because the disaster happened in the context of the development of a centralizing and modernizing country that was Portugal at that time. The earthquake "evoked a coordinated state emergency response as well as a forward looking

comprehensive plan for reconstruction which included mitigation efforts to reduce future disaster effects." (2000: 10). While what happened in 1775 in Lisbon did not immediately and totally change all reactions to later disasters in Europe. "it can be identified as a .turning point in human history" (Dynes, 2000: 10). At least in professional and elite circles in Western Europe, later disasters were thought of as something human beings and societies could attempt to prevent and cope with in a secular and proto-scientific framework. We do not deal with in this essay the fact that in most societies there is often a gap between elite and citizen views about the nature of disasters and what can be done about them (Quarantelli, Lagadec and Boin, 2006). That to a certain extent is almost as true today as it was more than over two centuries ago.

Pre World War I Truncated Efforts to Study Disasters

Unknown even to most current researchers in the disaster area, who tend to think of studies as exclusively a post World War II happening, there actually were studies of a social science nature even before World War I (see End Note # 2 for a list of selectively written on earlier efforts). Some, including us, have previously written that Samuel Prince wrote the first social science dissertation on disasters. He studied the Halifax, Canada ship explosion in 1917 and what it did to that city. His dissertation was published in 1920 by the sociology department at Columbia University. But it turned out that it was not the first dissertation in the disaster area.

About a dozen years ago we ran across information that a Ph.D. dissertation had been written 11 years earlier by Eduard Stierlin (1909) at the University of Zurich in Switzerland. In following up, we discovered he wrote on psychological and psychiatric consequences of disasters using data from 135 persons caught in the earthquake in Messina in Italy in 1908, as well as data from 21 survivors of a mining disaster in 1906. This at present is the oldest known social science dissertation on disasters.

Research on the Messina earthquake turned out, upon examination, to be an instance of a truncated development in the evolution of disaster studies. The earthquake resulted in one of the largest catastrophes of all times in terms of fatalities. In Messina alone, probably 70,000 plus of the residents died, that being half of the city population. In nearby localities such as Reggio de Calabria another 25,000 were killed out of 150,000 residents.

After an extensive search, we were able to find that there were a number of articles published on the catastrophe in Italian professional journals. For example, there were a series of articles in a 1911 issue of the Italian Review 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. For the most part, almost all the articles focused on the negative psychological shock to the earthquake victims, although there is an interesting footnote in one paper that said many individuals reacted well but it would not be discussed in the article (there is a tendency to look for the negative in disaster studies which often is manifested even in current disaster studies).

As far as we can see, this outbreak of systematic studies never was built upon and they seemed to have disappeared from the awareness of later scholars. Maybe this was because the social sciences in Italy at that time were barely starting to develop (one of the journals we mentioned was volume 2; another was volume 5, indicating that both had just been recently established). What happened also indicates that the first large scale initial systematic thrust toward studying disasters and crises was undertaken in psychology rather than in sociology as many current researchers believe.

It should be noted that a focus on psychological shock was not peculiar to Italy. An American physician (Robertson 1907) wrote an article in the *California State Journal of*

Medicine with the title of "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 (e.g. insane asylums), the overall conclusion was that the 1906 San Francisco earthquake did not lead to use today's language to any increase in mental illness. The data used were obtained from interviews with medical personnel, as well as statistics obtained from what we would currently call mental health centers and hospitals. This study remained unmentioned in the disaster literature for decades, and to this day is still unfamiliar even to many scholars interested how in how mental health is affected by the experience of surviving a disaster or catastrophe.

Of course it is noticeable that about a century ago the studies on the Messina and the San Francisco catastrophes reached dramatically different overall conclusions on the consequences for mental health of survivors. This professional division of opinion exists to this day, with one camp arguing that a severe mental health consequence from extreme stress is a disaster myth. Recently we outlined again the more salient dimensions around which the dispute revolves (see Quarantelli, 2008), but it illustrates the length of time that something is in scientific dispute, does not necessarily lead to a quick resolution of the issue.

First Sociological Efforts to Study Disasters

In between World War I and World War II there were isolated efforts by a few sociologists to study disasters and to put them into a social change framework. Prince's dissertation (1920) actually had the title of "Catastrophe and Social Change." Although mentioned now and then in the general sociological literature, no one built on that work which was as much a theoretical as it was an empirical effort (Scanlon 1988). As it is, his dissertation was long incorrectly supposed to be the first social science dissertation on disasters, but as noted

earlier the one by Stierlin in 1909 was written almost a decade earlier. However, Prince was first one in sociology. In his honor, there is an award named after him that is given by the International Research Committee on Disasters (IRCD) for an outstanding dissertation in the area.

Later, in what was essentially a theoretical essay rather than an empirical study Carr (1932) explicitly treated disasters as part of social change. This work was not rediscovered until the 1950s and is still unknown to many current researchers. Finally, Sorokin, a very major figure in sociology and noted for his interest in social change and dynamics, wrote a tome with the title *Man and Society in Calamity* (1942). While statistics and historical data are cited, the work is primarily a theoretical statement. More important, while this volume is not unknown, no one has ever attempted to build directly on this publication of now 67 years ago.

To be fair, it should be noted that while field studies of social disorganization and social problems and other problematical phenomena were a very prominent part of sociological research in the decades between the two world wars, it did not seem to occur to these textbook writers and researchers to study or at least mention disasters. So the three scholars we just cited were not seen as that relevant for their purposes by their professional colleagues, and the idea on using social change as a framework within which to study disasters was not advanced in the literature. (We should note as elaborated in End Note # 3 that the profession of social work could have been a possible source for an increased interest in as well as information about disasters, but it too was almost totally ignored).

Expectations about Civilians in World War II

World War I created a new type of collective crisis situations, namely that for the first time in history, civilians came under direct attack by enemy forces from afar. For example, there

were German air attacks on London and other cities in Great Britain. These air raids during 1917-1918 resulted in 4,820 casualties and killed 1,413 (Titmuss, 1950:4). While no specific studies were done at the time, the general observations made of the reactions of civilians to the air attacks were rather consistent. The population reacted well with no major social breakdowns or inappropriate and antisocial behavior surfacing. However, this positive finding seems to have been totally forgotten in the ensuing years.

This is supported by the fact that prior to World War II, when it became increasingly clear that Britain was likely to become involved again in a war with Germany, much alarm was expressed on how civilians would react to air attacks. There was very widespread concern expressed that British citizens would react very badly to the expected attacks. A Committee of Imperial Defence reached the conclusion that the "moral effects" of air attack in a future war would be "out of all proportion greater than the physical consequences" with the "most probable cause of chaos in the community will be the moral collapse of the personnel employed in the working of the vital public services" (quoted in Titmuss, 1950: 18). It has also been written: "It seemed to have been accepted almost as a matter of course that widespread neurosis and panic would ensue". The belief that social disorder was likely led to the suggestion that to prevent "panic flight the police forces should be enlarged and a cordon thrown around London" (Titmuss, 1950: 18).

As a post World War II analysis of documents said: "In sifting the many thousands of papers, which passed through Governmental agencies during the nineteen-twenties and nineteen-thirties, it is difficult to find even a hint that this fear of a collapse was based on much else than instinctive opinion" (Titmuss, 1950: 18). But even professional opinions were not much better. For example, psychiatrists formed a committee in 1938 to consider mental health services in time

of war. Their report said that it should be expected that there would be; "3-4 million cases of acute panic, hysteria and other neurotic conditions during the first six months of air attack" (quoted in Titmuss, 1950: 20). Nothing even remotely close to what was predicted actually occurred.

It would be interesting to find out what the authorities in Germany and Japan had expected of their civilians under air attack. The views expressed above were almost certainly not peculiar to British society before World War II. In fact, some of these very same concerns have been currently expressed by some of the authorities involved, about British society as it at this time currently for terrorist attacks (personal communication, Eve Coles). And there is a familiar ring to views currently being expressed about possible future terrorist attacks in the United States (Aguirre, 2004). Research ought to be undertaken on why there seems to be this cycle of concern about civilian behavior, observations that the concern was misplaced, a forgetting of what was learned, and a later repeat of concern about how civilians would react to a new crisis. The value of doing such a study is that it would provide clues on the evolution of future disaster studies, and perhaps prevent "reinventing" the wheel again.

The Reaction of Civilians to Bombings during World War II

The pre World War II prediction by the military that civilians would be subject to direct attack in the next large scale war was more than borne out. Starting in 1939 until 1945, around the world there were thousands of air attacks that killed hundreds of thousands and physically devastated many urban areas. As is typical of the disaster area, all wartime statistics are very uncertain; for example, the massive British air attack near the end of the war on Dresden, Germany has been estimated to have killed from 25,000 to 60,000 civilians. But clearly deaths and destruction were very high in Germany and Japan.

How did bombed civilians as a whole react? On this we can go beyond speculations or anecdotes. After the war, the US Air Force decided to study the effects of its massive bombing of Germany and Japan. The focus was mostly of a technical nature such as how industrial production in both countries was affected by the numerous air raids (interestingly in Germany production kept rising until the last few months of the war). But a small part of the study was on how civilians reacted to the bombings. A Morale section was set up in what came to be known as the US Strategic Bombing Survey Study (USSBS). Psychologists primarily staffed that section, but there were also anthropologists and sociologists in the group. When these persons returned to academia after leaving military service, many became among the most prominent researchers in the social sciences in the decades of the 1950-1980s.

Of the several hundred reports produced by USSBS there were about a dozen on what the massive air raids did to German and Japanese morale. The research using survey data and documents primarily focused on attitudes and opinions, but almost incidentally some behavioral data were obtained. The general findings are very clear. The bombings had very little effect on morale. The behavioral data indicated that there was no breakdown of social order in either country. Anti-social behaviors, looting attempts, and mental health problems were not widespread and seemingly had not increased from prior the war (see reanalysis of the USSBS data in Janis, 1951). There was no turning by the bombed civilians against the government or the authorities in either society. These USSBS observations and findings were consistent with other descriptions and analyses made of wartime situations such as an internal extensive report by the Mayor's Office after a 1,000 plane attack that devastated Hamburg, Germany, and a Japanese government report on the days immediately after the atomic bombing of Hiroshima (see also Nagai, 1951). By almost any criteria that could be used, under extreme and often continuous

stress, civilians reacted very well and the groups of which they were a part continued to function in viable ways.

These general observations were reinforced by analyses done in Britain a few years after the war on how its civilians had reacted to air attacks. In a very detailed analysis in a book by Titmuss (1950) it is noted that the overall picture that comes across is that the civilian population in London and other bombed cities in Great Britain had reacted very well. There was very little if any at all by way of disorganized behaviors, maladaptive responses, dysfunctional activities, or rises in anti-social and criminal behaviors or in mental health problems. A very recent reanalysis found that the studies done after the war had reached correct conclusions. According to the authors, "dire predictions...failed to materialize...civilians proved more resilient than planners had anticipated" (Jones et al., 2004: 463).

The Military Interest

It is fairly clear that in military circles in the United States right after World War II that there was considerable concern on how American civilians would react to direct attacks. It is known that by 1948 that the military commissioned studies of the psychological effects of air war (Janis, 1951: vii). These were done at the Rand Corporation: an entity specifically created to do research for the U.S. Air Force. This was followed by a report a year later that indicated how field research could be conducted on the psychological impact of peacetime disasters (Janis, 1949). Interestingly, several of the reports specifically recommended that social science field teams, preferably coming from academia, be set up (Bennett, 1949; Janis, 1949). There are also vague hints that there might have been still other studies, classified ones, done for the Rand Corporation. Again this would be consistent with the theme running through all the public reports that interest was in extrapolating from peacetime disasters to how American civilians

would react to direct attacks, including atomic bombing and biological warfare (Rand, 1949).

It is not at all clear at the present time to what extent there was widespread awareness in the American military of such research as the USSBS studies as well as the work done in the Rand Corporation in the decade after the end of World War II. On one hand, the general conclusion of most of this research and analysis was that civilians reacted consistently well to wartime stresses. If so, it is puzzling why the basic question continued to be asked by different military organizations on how the American population would react to direct attack? On the other hand, it is possible that different military circles may have become aware that it was considered important to conduct social science studies without having much specific knowledge and understanding what this research had already found about human and group responses in crisis situations. These and other possibilities accounting for the military interest in civilian responses should be studied. No one has done that so far, and the chance for oral histories being obtained no longer exists. But it is very possible that answers could be found in military archives with even once classified reports now being available for study.

Whatever the actual knowledge in military circles, it is a fact that the first direct support of disaster field studies came from that social institution. Military officers from the Chemical Corps Medical Laboratories of the Army Chemical Center in Maryland had gone to Donora, Pennsylvania where in October 1948 a combination of chemical fumes and a temperature inversion created a concentration of sulfur dioxide. Chemical Center personnel thought it would be a good situation to study how civilians reacted to something similar to a poison gas attack. The field study found that approximately 43 percent of the population became ill and 25 persons died over a several day period.

Puzzling to the survey personnel was that some inhabitants of the area who had not been

directly exposed showed the same kind of health symptoms as had victims who had been directly exposed. Seeking an explanation of this observation, the Chemical Center in 1949 approached NORC to do a retrospective field study of the Donora episode, and suggested studying psychological factors. In joint discussions, the retrospective study was rejected since NORC argued that any such fieldwork would have been too far after the occurrence of the episode to draw valid conclusions. Undoubtedly this was a reason the NORC did not want to do the study, but there are grounds to think that there was also a desire to do more than a one-time field study. (Information about the relationship between NORC and the Chemical Center including all the quotations cited below are from the partial and not inventoried archives of NORC on the project, stored in the DRC archival collections).

We have been unable to find any specific information on why NORC was the particular social science organization that was approached by the Chemical Center. There is no evidence that either party had had any prior contact with one another (or NORC had never done any prior study on anything involving disasters or crises). In the minutes of the conference, the lead Colonel talks in very general terms about the “historical development of interest of federal military agencies in civilian disaster research” (NORC, 1952: 4). Apparently numerous but unnamed consultants were contacted with the result being “our contact with the National Opinion Research Center” (NORC, 1952: 4). However, the Colonel again and again in his remarks stresses that the American military and the population were going to be faced with a new crisis situation in the post World War II setting.

The NORC research proposal that was later accepted stated that: "it is felt that empirical study of peacetime disasters will yield knowledge applicable to the understanding and control of peacetime disasters, but also of those which may be anticipated in the event of another war." A

later statement suggests that the latter was the major goal that: "careful selection of the natural or industrial disasters to be studied can furnish an approximation of the conditions to be expected in a war disaster." It is acknowledged that there are certain differences between wartime crises and peacetime disasters, but the proposal comes back a number of times to the idea that one could learn about the probable wartime behavior of American civilians from studying how they responded to peacetime natural and industrial disasters.

That primary interest was in the wartime implications is clear from two other aspects of the proposal. One is the emphasis on social control. The other is the notion that the basic problems in disasters are to be found in the reactions of those impacted by danger, loss and deprivation. Thus, it is said that there is a need for the "reduction and control of panic reactions," that minimum elements in effective disaster control include "the securing of conformity to emergency regulations," and that morale is "the key to disaster control; without it the cooperation and conformity needed from the public will not be forthcoming." Likewise, the research design focused on individual victims and the field instruments to be developed were aimed at answering these general questions:

1. Which elements in a disaster are most frightening or disrupting to people and how can these threats be met?
2. What techniques are effective in reducing or controlling fear?
3. What types of people are susceptible to panic and what types can be counted on for leadership in an emergency?
4. What aggressions and resentments are likely to emerge among victims of a disaster and how can these be preventing from disrupting the work of disaster control?
5. What types of organized work efforts effectively and which do not?

This last question was conceived primarily in terms of "good disaster leadership" rather than organizational entities. Some informal interviewing of community leaders was projected, but that was for the purpose of uncovering: "more expert and informal account of the disaster and description and analysis of public reactions to it, and of the adequacy of control measures, all of which information will be general value in interpreting and evaluating the popular reactions uncovered in the systematic interviewing."

As one who was involved in the NORC work almost from its inception, we can attest that the actual field work on the surface proceeded along some lines as proposed. An effort was made to study peacetime disasters that appeared to have the closest parallel to a wartime situation, that is, a population subjected to some kind of sudden and widespread attack. The best research case scenario visualized, that never materialized, would have been a major earthquake in an urban area. An initial assumption, even by NORC, was that disaster problems were primarily of a social psychological nature and that they resulted from the internal states of the victims.

However, it is necessary to note that something that was not part of the proposal became a very important factor that eventually had very significant consequences. The graduate students initially hired to do everyday activities and the field work came from a variety of social science disciplines such as psychology, anthropology and sociology. That was consistent with a loose notion around that the research would benefit from having a multidisciplinary team. However, for complicated reason (some of which are discussed elsewhere, see Quarantelli, 1987, 1994), the sociologists involved (including the everyday supervisor of the project, Charles Fritz) eventually became the key actors in the research process leading to subtle changes in emphases, the questions asked in the field, the participant observations made, and especially how the

collected data were analyzed.

In addition, not only were the sociologists intellectually dominant in what was actually done, but also as it turned out, most were particularly interested in the sub specialization of collective behavior. This sociological specialization studies crowd behavior, social movements and other dynamic aspect of social life. It also assumes that at the heart of much collective behavior is emergent behavior; that is, the appearance of new behaviors and groups is to be expected in many situations. In time, the actual research design used, the data analyses undertaken and the publications produced, came to reflect heavily this particular sociological orientation (see Quarantelli, 1987 for listing of such publications).

Of course what the NORC team found in its field work in eight disasters provided the interview, observational and documentary data that was analyzed within this particular sociological framework. There were three aspects of the data gathering, which are worthwhile noting. The heart of the data came from a very systematic population survey of 342 respondents (out of strict probability sample of 362) in several towns and villages in northeast Arkansas hit by tornadoes in March 1952. A number of these in person interviews lasted up to two hours. If nothing else, that particular endeavor showed conclusively that it was possible to conduct very systematic field surveys among impacted populations soon after a disaster.

Second, the analysis of the survey data showed in a quantitative way that many widely held beliefs about how people behaved in crisis or emergency time periods were simply put, wrong. Those caught in disasters reacted very well; they did not panic or engage in anti-social behavior; they were not stunned into inaction--instead they immediately helped others around them. These data along with field observations made in the other seven disasters NORC studied, led to the coining of the term "disaster myths."

Finally, while the study of community officials had been thought of originally as a secondary effort, the more NORC worked in the field the more it became obvious that organizations had real problems in disasters. In fact, in the large Arkansas study, we were pulled off interviewing individual victims and reassigned to interviewing organizational officials. While the value and need to shift from individuals to organizations in future research on disasters is not reflected in the final unpublished NORC report (Marks and Fritz, 1954) that was probably the major lesson learned by those who had worked in the field, and was considered an even more significant observation than the findings about disaster myths.

How well did the NORC work meet the expectations of those funding the research? While sponsor interest was in extrapolating from civilian disasters to wartime situations, the social science and eventually the sociological background of the researchers had a major and decisive (but rather different) influence in what the involved researchers did and thought about disasters. In fact, very little if anything was produced that directly met the wartime interest of the research sponsors. Part of this stemmed from the fact that the researchers involved saw themselves as social scientists with primarily a basic rather than applied science goal. They were learning much about how persons and groups reacted to extreme stress, something that they knew had not been systematically studied before, and that was very rewarding especially to those interested in collective behavior. Thus, generating general propositions about human, group and community behavior in disasters and crises had very high priority; doing something practical by way of developing planning and managing principles with what was found, did not loom large. Part of what happened resulted from the strong feeling of many of the researchers that it did not make sense to try to extrapolate from even the most extreme of civilian catastrophic occasions to, at that time, an atomic war situation. Surface and lip service was at times paid to the military

interest in extrapolations, but that did not reflect the true beliefs of the researchers. In fact, it was clear that important aspects of what was being learned in field studies could not be extrapolated; just the opposite was true (e.g. the usual convergence of persons and groups to impact sites that was crucial for relief and recovery purposes in peacetime, was a process that simply could not occur in a major atomic attack).

Besides the NORC work, the military in the same time period supported two other organized efforts to study disasters. The Army Chemical Corps also supported research at the Psychiatric Institute at the University of Maryland. The contract for the award said it was: "To study the psychological reactions and behaviors of individuals and local populations in disaster, for the purposes of developing methods for the prevention of panic, and for minimizing emotional and psychological failures". Under a heading of Suggested Areas of Psychological Investigations were listed: "Herd Reaction, Panic, Emergence of Leaders and Recommendations for Guidance and Control of Masses." It is clear that the findings were to be applied to a wartime civilian context. A projected interdisciplinary staff was never assembled, and the project never had more than two regular part time staff members. Limited field studies were undertaken of eleven different episodes in 1951 to 1953. The final report on the project was about the only publication to result directly from the work done (Powell, 1954). Produced only in mimeographed form, it is not surprising that it is unknown even to veteran disaster researchers and its discussion of psychological variables in disasters seems to have not been noticed by anyone.

In addition there was a team put together at the University of Oklahoma, which was funded by the Operations Research Office at John Hopkins University (the office was initially set up to do research for the US Navy). In the words of the two sociologists who headed it, the team

was: "to undertake studies of human behavior during disasters in order to determine whether typical behavior patterns developed under stress. If such patterns can be established, it might be possible to predict troop behavior under the extreme pressure for disorganization resulting from atomic attack" (Logan and Killian, 1952). There was no interest in civilian disasters per se. They were only looked at in terms of what could be learned about how military units might react (this is why a number of the team reports were classified documents for years). Actually the Oklahoma team, paralleling what happened at NORC, learned it was possible to do field research on peacetime disasters and found that individuals reacted fairly well but organizations had problems (Logan, Killian and Marrs, 1952). By chance, team members from NORC and Oklahoma met in the field at a plane crash in a residential area in New Jersey, but there was never any follow up on that contact. (For more information on the Oklahoma work done from 1950 to 1952, see Quarantelli, 1987).

In our view, pending anything that might be found in future studies of archives and reports that might be unclassified, it is not possible to say at this time that the military got much of value for themselves from the just indicated studies. But this was not the only research supported by the US military. We turn now to another effort the military also started and that did have very major payoff at least for the development of social science research on disasters and related crises.

The National Academy of Sciences Committee

In 1952, the Surgeon Generals of the Army, Navy and Air Force Medical Services requested the National Academy of Sciences (NAS) to undertake a program of disaster studies, specifically "problems that might result from disasters caused by enemy action" (Kreps, 1981: 94). They suggested a national program to advise, stimulate, coordinate and collate the results of

research on a broad inter-disciplinary basis (Williams, 1954: 6). A Committee on Disaster Studies (CDS) was established, even though some committee members thought multidisciplinary research was counterproductive (e.g., Janis 1954). The Surgeon Generals were the major source of funding for the CDS from 1952 until 1955. From then until 1963 when the CDS was dissolved, support came from the Ford Foundation, the National Institute of Mental Health, the Department of Health, Education and Welfare, and the Federal Civil Defense Administration. Only a total of \$900,000 of funds was provided over the 11 year period (Kreps 1981: 95).

While the national program mentioned above was the stated intent, there were significant deviations along three lines. Just as in the NORC work, sociologists took operative control of the everyday work of the CDS (or in a later name change, the Disaster Research Group). So despite the fact that only three members of the executive council that oversaw the CDS were sociologists, the day--to-day activities did not go much in a multidisciplinary or non-sociological direction. For example, of the 19 major disaster publications produced by the committee, thirteen were authored or co-authored by sociologists and three others by anthropologists (Quarantelli, 1994: 27).

The second way in which the CDS went in a direction apparently not visualized in the statement that set it up, was that it not only supported the research of others but also conducted some of its own field research. This kind of activity was very unusual for any NAS committee to undertake. It also may explain why the CDS name was changed to the Disaster Research Group. Important in the research done was that the two key staff members Harry Williams (the technical director) and Charles Fritz (his assistant) had prior field experience in studying disasters. Thus, it is not surprising that field research was the preferred methodology and organized behavior the major social phenomena that was studied (this is supported by oral history interviews we

conducted with both researchers). It does not take much imagination to see how the area of disaster studies might have gone in a different direction later if, say psychologists, interested in doing survey studies of attitudes of individual victims had become the prime players in a CDS.

The third way in which the CDS deviated from the original statement that set it up, was that practical applications proposed of the research findings seemed at times almost an after thought. To be sure some CDS publications made recommendations, for example, on the best kind of fallout shelters that might be used in a nuclear war. But as someone who served as a CDS staff member has written: "Notwithstanding the practical rationale tied to its early funding, it is clear that the committee, staff, and other interested persons maintained a basic research orientation from the outset" (Kreps, 1981: 97).

Perhaps another unintended consequence of the CDS is that it provided both direct and indirect support for the study of disasters. It encouraged, sometimes by direct funding, individual researchers such as Harry Moore (1956) and others to continue undertaking research on disasters, although no organization per se was ever set up by these researchers. It also encouraged others such as us that interest in disasters was a valid one for social scientists. Overall, the CDS work provided an intellectual bridge between the cessation of the NORC work and the establishment of DRC.

The Establishment of DRC

The establishment of DRC did not result from the unfolding of some master plan to set up a Center to study disaster and crises. That was far from what happened. After graduating from the University of Chicago in 1959, we obtained an assistant professor position in the Sociology Department at Ohio State University (OSU) to teach courses in social psychology as well as collective behavior and social movements. Our initial research at OSU was a continuation of his

doctoral dissertation topic, the professionalization of dental students. We did maintain our interest in disasters by continuing contact with the National Academy of Sciences committee on disasters (for more details, see the interview about our professional career in O'Leary, 2004).

In 1962 we were approached by two colleagues in the Sociology Department at the University, Russell Dynes and Eugene Haas, who were aware of our earlier studies in the disaster area. They had started to put together a research proposal under the title of "Organizations under stress" in which they were going to indicate that they wanted to do a study on that topic. We agreed to join them and the proposal eventually suggested field research on organizations involved in disasters, as well as possible simulated laboratory work on organizational members under stress conditions. The last idea resulted from the fact that in the early 1960s universities were creating laboratories on their campuses with one-way mirrors and photography and recording machinery that could be used to study volunteer participants in such laboratory settings. While OSU did not have an actual laboratory of that kind in place, there had been some general talk that it would be open to creating such a facility.

An initial effort was made to find internal university funding for the proposal, but the most likely source for that, the Mershon Center, rejected the proposal as not relevant to national security, ironically its major concern at that time. A revised proposal was then sent to the National Science Foundation in Washington D.C. It asked for roughly \$80,000 for research (including both field work and laboratory simulation studies) that would be done over a year and a half time period. No mention was made of establishing a Disaster Research Center because that had never been mentioned much less discussed among the three proposal writers.

The National Science Foundation, for reasons unknown to this day, turned down the proposal but before the OSU writers of the proposal learned that fact, a telephone call was

received from an official in the Office of Civil Defense (OCD) that their agency had obtained a copy of the proposal and wanted to discuss it in person in a meeting in Washington. The official also indicated that a representative of the Air Force Office of Scientific Research (AFOSR) would also be present at the proposed meeting. To this day it has never been totally clear how the formal proposal to NSF got informally passed on to the OCD and the AFOSR (something that the NSF did not normally do), but apparently Fritz who had left the NAS by that time had been given a copy of the proposal and brokered the meeting.

Why had OCD and the AFOSR become interested in the proposal from OSU? Simply put it was the ongoing cold war between the Soviet Union and the United States. This had led to several major confrontations between the two countries, one being the Berlin blockade and the other being the Cuban missile crisis. Both sharply accelerated concern over how American society would react to an atomic attack on the country. From oral histories obtained later from key officials involved, it is obvious that there was a strong belief that the reaction would not be a good one, that there would be widespread "panic" and a breakdown of the social order. As a consequence OCD, whose major mission was the civil protection of the American population, received massive increases in the millions in its funding (\$207 millions just in 1961 according to Blanchard, 2004). So there was a convergence in the middle of 1963 between major OCD concern over civilian behavior in a new war time situation, major governmental increases in funding, and the informal appearance of the proposal from OSU. The OCD and AFOSR officials saw the proposal as something that would meet their interests if details could be worked out at a meeting. It may have been fortunate that they either did not know or did not believe the earlier USSBS studies!

Specific details of what happened at the meeting can be found in the DRC archives, but

important for the purpose of this essay were the following decisions. OCD said that it was interested in the fieldwork on organizations in disasters that was set forth in the proposal, but it would prefer that a contract be drawn up for work over a five year period at roughly \$200,000 a year (this proposed funding for our one project was more money than the CDS had totally obtained in its 11 years of existence!). If the OSU researchers agreed to that (we had no problem with accepting a million dollar budget), a revised proposal would have to be written, including field studies outside of the United States. At the meeting, agreement in principle was reached with respect to the larger and longer work effort proposed by OCD with an understanding that the details would be worked out in later negotiations. The AFOSR said it could not offer immediate funding, but indicated that it was very much interested in the laboratory simulation study proposed and would be willing to provide a grant for such work, provided that the University had the laboratory facility mentioned in the OSU proposal.

Again specific details of what went on at the University can be found in the DRC archives, but the following were among the most important decisions and actions that were taken. The OSU researchers rewrote the proposal in line with some OCD suggestions. Formal work started in August 1963. The University upon being told of the AFOSR interest in supporting laboratory research quickly agreed to accelerate building such a facility. The laboratory was ready by early 1964 when a grant was obtained from AFOSR.

Because it was obvious that there would be two concurrent research projects, the OSU faculty members thought in the early summer of 1963 it would be appropriate to have a collective name to embrace such activities. The name "Disaster Research Center" (DRC) was chosen after checking with the University to see if it had any problems with such a label. It did not because as far as the University was concerned, that was an informal designation and did not

constitute the setting up of a new formal administrative unit on the campus. This proved a great blessing in disguise because it allowed DRC considerable independence in doing whatever it wanted to do; for administrative paperwork purposes it operated through the sociology department but that also left DRC with a very large degree of autonomy. For example, the three OSU members of the sociology department also designated themselves as Directors of the Center, a step usually not a prerogative of university faculty members anywhere.

As is obvious, there was no master plan to establish a DRC. The initial proposal that the OSU researchers advanced was substantially changed by what OCD suggested and was willing to support. Similarly the DRC ended up with a laboratory facility that had not been originally envisioned but fitted into what the AFOSR was interested in supporting. The initial thrust of DRC is well indicated in the revised proposal that was worked out with OCD in January 1964. To quote the proposal it said the Center would have five major objectives:

1. To collate and synthesize findings obtained in prior studies of organizational behavior under stress.
2. To examine, both by field work and other means, pre-crisis organizational structures and procedures for meeting stress.
3. To establish a field research team to engage in immediate and follow-up studies of the operation of organizations in community disaster settings, both domestic and foreign.
4. To develop, in coordination with a concurrent project, a program for field experiments and laboratory simulation studies of organizational behavior under stress.
5. To produce a series of publications on the basis of these four objectives, with special emphasis on recommendations concerning the effective emergency operations of organizations and other matters pertinent to civil defense planners.

The first three goals were exactly phrased along the lines of what DRC wanted. Goal # 4 was never agreed to by AFSOR; in fact there is reason to think they may never have been consciously aware of its existence. Goal # 5 was what OCD wanted.

The emphasis on organizations and doing fieldwork was consciously drawn from the NORC model. In time, DRC expanded its unit of analysis particularly to communities, although organizations have always been the prime focus. The field work methodology has also continued to be the dominant one, but in time was expanded and made more systematic (for details of how field teams were trained and what was actually done in the field see the extensive discussion in Quarantelli, 2002), Later other methodologies from large scale population surveys to content analyses of documents to participant observations of focus groups, for example, were added. This again mirrored what NORC had done. A final parallel to the NORC work was that the requested extrapolation in its early days from civilian disasters to a wartime setting was also strongly downplayed at DRC (it would be worthwhile to examine why and how this was done, which currently is only partly hinted at in some oral histories).

What has been just described in the last few pages has been a very general description of how DRC got established. Many more specific details are in the various cited sources above. But still others currently exist only in the DRC archives and some oral histories that have only been barely looked at by researchers working with sociology of knowledge and science approach. Also, the later phases of the DRC operations have not been discussed in this essay, something that someone else should examine. Finally, while a case can be made that DRC was the major player in the establishment of social science disaster research, it was not the only group involved.

So to keep things in perspective, it should be noted that in the several years around when

DRC was established, there were more limited efforts made in Canada, France and Japan to undertake organized and systematic research on disasters. (A rather separate development of research on hazards is discussed in End Note # 4). Funding from the Canadian Defence Research Board supported the attempt in Canada. But the key researcher involved, J. Tyhurst, a psychiatrist, did not seem to attempt to get others involved. His work, maybe because it was published in psychiatric outlets (1951, 1955, 1957a, 1957b), was a dead end with no continuity; Joseph Scanlon only resumed systematic social science disaster research in Canada much later. In France a military psychologist, Charles Chandessais became interested mostly in panic behavior among civilians (1966a, 1966b) with his work being done for the Fire Department in Paris, a subunit within the French army. There was contact with DRC and even a joint conference. But after Chandessias died, the small Center he had established simply ceased operations and there were no successors. Likewise, a marketing psychologist Kitao Abe (1972) in Japan became interested in panic behavior and did field work on the phenomena and later on rumors. But while later Japanese researchers on disasters were aware of his work and had contact with him, they also did not build directly on his studies and no organized and systematic research effort came into being at that time in Japan (see Okabe and Hirose, 1985).

In short, in contrast to what happened in the United States, these three other efforts were all aborted ones. So while there was something in the social climate to stimulate efforts to study disasters in four different countries around the world, only in the United States was the social setting very supportive to getting research in the area institutionalized especially by the formation of DRC.

A Concluding Observation

Early in this essay we indicated that the development of disaster research followed a very

erratic path. At various later points we noted uncertainty about what had or had not happened. However, both the erratic path and uncertainties may reflect more the reality of what was going on than a flawed methodology or limited research effort. We want to illustrate this by noting part of the content from an original letter written in 1976 by a very high official in the National Academy of Sciences to Charles Fritz also in the NAS. The direct quotes are as follows:

I have looked over the proposal on “Rapid Response”...I don’t fully visualize the scenario of the “quick response” investigation, particularly in the pre-impact phase. How can the investigative team be alerted and got to the right place in time? It is difficult for me to see how you can select a site and collect the sort of data outlined...Officials are going to be so preoccupied with their own immediate problems that I cannot imagine their talking to researchers in advance of a known emergency. How can the monitoring system actually be put in place in the face of imminent disaster? It seems to me that the proposal is deficient in thinking through the simple logistics of systematic in the context of an emergency situation with each of the classes of emergency you are considering. (letter from the head of the Commission on Sociotechnical Systems in NAS to Fritz; the original letter was found in the Fritz archives located in the Special Collections in the DRC Resource Collection).

This letter was written after the NORC study, the CDS own studies and the DRC studies that collectively had undertaken much field work before, during and after major disasters and catastrophes over a 27 year period. Apart from this being an extreme example of the right hand of CDS not knowing what the left hand of CDS had been doing, this shows an astounding lack of knowledge by a key decision maker about something he was evaluating. By the time of this 1976 letter, DRC alone had undertaken 307 field studies!

END NOTES

1. There is no one source that can be used to document all the current activities in the research in the social science study of disasters. However, there are four web sites, which can provide very useful and very up to date information. The web site run by the University of Colorado Natural Hazards Research and Applications Information Center is at

www.colorado.edu/hazards. The DRC web site at the University of Delaware is at www.udel.edu/DRC. The Texas X & M web site (archone.tamu.edu/hrrc/overview/index.html) of the Hazard Reduction and Recovery Center is at Texas A &M University. For information about the European scene in see the web site of the Disaster and Social Crisis Research Network at www.erc.gr/Englishld&scrn/index.htm. For a discussion of the development of emergency management see Norton, 1979; Blanchard, 1984; Rubin, 2007.

2. For a sampling of different but limited historical accounts over the years see: Fritz, 1961; Barton, 1969; Stoddard, 1968; Quarantelli, 1978; Kreps, 1984; Tierney, 2002; Blanchard, 2004.

3. There is an early disaster data source that has been almost totally neglected by disaster researchers. This is the field of social work, which very early in the 20th Century had close ties and partly overlapped with the emergence of sociology. Upon checking we have found that even before 1900, there was one yearly publication called the Proceedings of the National Conference of Charities and Correction, as well as a journal called Charities and the Commons. Both publications have many mostly descriptive accounts of the participation and involvement of social workers in American disasters of all kinds. Papers carry such titles as “Experiences in San Francisco,” “Flood in Pittsburgh,” “Work of the citizens in New Orleans,” etc. A cursory review demonstrated to us there are many useful observations made, that if known to the early disaster research pioneers, would have been very helpful in developing research designs for their field work. Some of the social work accounts attempted to advance generalizations (e.g. “disaster relief and its problems” (Bicknell, 1909) or “relief work in its social bearings” (Smith, 1906). It might be interesting if a comparison was made of the general observations made by the social workers with the findings of the pioneer disaster researchers. Was the “wheel invented again”—

at least partly?

4. We have not discussed in this paper the development in geography of a sustained interest in natural hazards. Many geographers such as Mitchell (1990) see disaster research and hazard research as starting in different ways, as having different foci, and until recently as having relatively little communication and contact with each other.

Without doubt the initial major figure and pioneer in hazards studies was Gilbert White who wrote a dissertation on human adjustments to floods in 1942 but only published it in 1945 (White, 1973; for his autobiography see White, 2002). In 1976 he became the director of the Natural Hazards Research and Applications Information Center at the University of Colorado, being joined in establishing it by Eugene who had left DRC to go to Colorado. Any analysis of how the social sciences as a whole got involved in disaster and hazard studies, of necessity, will have to take into account what evolved in geography, and how the larger social context affected what happened. There is reason to believe different social factors were operative in the development of disaster research compared to hazard research. For example, the military interest in disasters for reasons indicated in this essay was far less operative in the case of those interested in natural hazards. An intensive sociology of knowledge and science study of the two different paths that led to hazard and to disaster research would tell us much that at present is unknown.

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