Death and Resurrection in the Early Cold War

The Grand Analogy of the Disaster Researchers

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"How shall we think about the next war? What is the right way to do it?" People worried about panic. This is what Philip Wylie imagined would happen.

> The ones on the street were desperate. The streets … were already packed with cars and trucks. The sidewalks wouldn't hold the humanity that gushed from the big buildings. The people, driven by the siren, gripped now by stark terror … were trying to make progress over the vehicles. They swarmed up like ants – slid off – climbed again – some going toward the river, some toward the south, some east, some west – all merely going, for motion's sake. Thinking, escape!
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> … From the streets below came the most bloodcurdling sound Coley had ever heard or dreamed of, the sound of thousands upon thousands of people – men and women and children – in absolute panic, in total fear, in headless flight, being trampled, being squeezed to death, having ribs caved in and legs broken, screaming, trying to escape. …
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> Here it was. … Here was gigantic panic, uncontrolled and hideous. … Here was the infectious breakdown of the "average mind," the total collapse of man in the presence of that which he had not been willing to face. This was the lurid countenance of something unknown because he refused to know.
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> Here, too, Chuck could see was that other fear – the horror of a bomb survived, raised to excruciating horror by the terror of another. Get out of

> the city; it was all they could think of. Get out now while you still have unburned meat to move your unbroken bones.[1]

People worried but they also refused to think about it. The atom bomb was too scary, too immense. It was impossible to drum up interest in civil defense. Frank Fremont-Smith also thought people could not face the fact of atomic war. "This country is relatively ripe for a panic reaction," he said.

> We already have had part of the paralyzing effect on panic before us; we have almost no plans for civil defense ready to be put in operation. We have been paralyzed in fairly high quarters and elsewhere by the awfulness of we have had to contemplate in regard to the major disaster situations of a potential World War III. We haven't been willing to face this situation; maybe I am putting too much emphasis on the failure to get a plan ready for operation. However, we have had quite some time and no plan really is ready.[2]

This essay listens in on conversations among scholars. They weren't delusional or foolish people, they were sober and accomplished public servants. They didn't invent new ideas, – they weren't those kinds of thinkers. But they were the responsible ones shouldering the burden of looking ahead. They were men of their time and place, robust, anxious, reflecting the commonplaces of their time. If one could put oneself into the frame of mind and heart in which death and resurrection could be assured with the guidance of recent experience and good planning, then these conversations can tell us about what mattered in one tangle of the American social imagination in the early cold war.

What touches me is the elasticity, the fragility, the atmosphere of doomed credulousness of the framing assumption that formed the basis of their work. They gambled and hoped that one could study peacetime disasters in order to learn how Americans would behave in war. What did they think they were seeing? What did they think they could know

1 Philip Wylie: Tomorrow!, Popular Library Edition 208, New York: Rinehart 1954, pp. 264-265. Pagination is from 1956.

2 Frank Fremont-Smith: Remark, Chemical Corps, Conference on Psychological Aspects of Disasters, Army Chemical Center, Maryland, March 10, Medical Division Reports 237 (1950), pp. 15-16.

by looking at the one world, the peacetime present, and extrapolate to the other, the world of atomic, then soon after, thermonuclear war?

EXPECTATIONS ABOUT THE NEXT WAR

There was no question about life underground. The next war would couple atomic bombs to rockets. The rockets would vaporize the cities. Even the day after Hiroshima people began to the think of the possible future as "a world of troglodytes."[3] The day after Nagasaki people assured one another that atomic missiles were inevitable, "too fast to be seen, much less stopped." The war would be over "in three hours."[4] It was a self-evident truth, common sense and inevitable. Just weeks after the atomic bombings in Japan, the authors of the *Strategic Bombing Survey* shuddered, "The combination of the atomic bomb with remote-control projectiles of ocean-spanning range stands as a possibility which is awesome and frightful to contemplate."[5]

The aerial bombing of cities was a new way to fight wars. How effective were they? In 1944 President Roosevelt directed his Secretary of War to begin a comprehensive review of the physical, economic, and psychological effects of aerial bombardment in Europe. Just days after the Hiroshima and Nagasaki bombings, President Truman requested the same for the war against the Japanese.

The findings of the *United States Strategic Bombing Survey* were grimly satisfactory. The authors of the European War report marveled at the mental toughness of German civilians. Even while the economy was collapsing, people "resorted to almost every means an ingenious people could devise to avoid the attacks upon her economy and to minimize their effects. Camouflage, smoke screens, shadow plants, dispersal, underground factories, were all employed." It was astonishing.

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3 Hanson W. Baldwin,: "The Atomic Weapon: End of war against Japan hastened but destruction sows seed of hate", in: New York Times, August 7 (1945), p. 10.

4 "Gigantic Atom", *New York Times*, August 12 (1945), E1.

5 The United States Strategic Bombing Survey Summary Report (European War), United States Government Printing Office, September 30 (1945), p. 16 (USSBS).

> They showed surprising resistance to the terror and hardships of repeated air attack, to the destruction of their homes and belongings, and to the conditions under which they were reduced to live. Their morale … and their confidence in their leaders declined, but they continued to work efficiently as long as the physical means of production remained.[6]

The study of Hiroshima and Nagasaki was less buoyant. But most future disaster researchers took comfort in the fact that in areas that had not been destroyed utterly, people who had not been injured leaped into action. They did not abandon their cities. "A mass flight from the city took place as persons sought safety from the conflagration and a place for shelter and food," the report stated. But the next day, "people were streaming back by the thousands in search of relatives and friends."[7] One day after the attack on Hiroshima, electrical power was available for most of the surviving parts of the city. Two days after, railroad service was restored. Eight days after, telephone service was again available. Fourteen days after the attack, a full 80% of the transportation department's employees were back at work.[8]

Civilian morale was the problem and the question. The next war would be different. Two days before the Hiroshima bombing, the staff of the Provost Marshal General began to comb through analyses of the effects of aerial bombing on civilians in Great Britain, Germany, Japan and the United States. Their work overlapped with the interviews undertaken for the US Strategic Bombing Survey in the European theater.[9] It seemed that *everyone* in the military wanted to understand the connection between city bombing, civil defense, and the collective will to resist enemy assault in total war. And like everyone else, the authors of the Provost Marshal General report assumed surprise attack. "The

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6 USSBS 1945 Summary European War, p. 16.

7 *U. S. Strategic Bombing Survey: The Effects of the Atomic Bombings of Hiroshima and Nagasaki*, United States Government Printing Office, June 19 (1946), p. 6.

8 USSBS 1946, Effects of Atomic Bombings, p. 8.

9 The United States Strategic Bombing Survey began on November 3, 1944 and produced its report on September 30, 1945. U. S. Strategic Bombing Survey, U.S. Strategic Bombing Survey Summary Report (European War), Government Printing Office, Washington, September 30 (1945), p. ii.

next war will be a total war which may begin at any time; … the United States… will be attacked first and without warning." Civilian morale would win or lose the next war. The nation's "ability to withstand the attack depends on the thoroughness and efficiency of plans prepared by the national government … [which will enable them] to resist and survive."[10]

Civil Defense was the work of peacetime. On November 25, 1946, the Secretary of War established a Civil Defense Board in order to study the matter. The Board submitted its report on February 28, 1947.[11] On March 27, 1948, the Office of Civil Defense Planning was established in the new Office of the Secretary of Defense. It too reviewed available studies on civilian morale during the last war. It released its own report to the Secretary on October 1, 1948.[12]

DONORA

In late October 1948 a freak happening presented the Army with an ideal opportunity to examine the psychological effect of gas on ordinary citizens. A temperature inversion created the conditions for a catastrophic concentration of smokestack effluents from US Steel's Donora Zinc Works and the American Steel and Wire Plan in the town of Donora, Pennsylvania. Sulfuric acid, nitrogen dioxide, fluorine and other noxious gases were trapped close to the ground. Twenty people

10 Office of the Provost Marshal General: "Defense Against Enemy Actions Directed at Civilians", Study 3-B-1, Washington DC: US War Department General Staff 1946, Paragraph 3, exhibit N, 5; Cited in Center of Military History: „History of Strategic Air and Ballistic Missile Defense", Vol 1, 1944-1955, United States Army 2009, p. 89.

11 Office of the Secretary of Defense: "A Study of Civil Defense", Washington DC: War Department Civil Defense Board 1947, it was subsequently released to the public on February 14, 1948.

12 Office of Secretary of Defense: 1948, "Civil Defense for National Security", Washington DC: Office of Civil Defense Planning, October 1, 1948. It was released to the public on November 13, 1948.

died and more than 7000 townspeople were stricken. Smog in Donora made national news.[13]

This was interesting. A physician in the Army Chemical Corps thought the suffering of the people of Donora might offer a clue to the American mind-state. He gathered a small team and went to Pennsylvania to take a look. Local officials told him that 43% percent of the residents had sickened. What interested him were the people who had not been directly exposed to toxic fumes but experienced the same symptoms as those who had.[14]

John Wood, the Chief of the Medical Division of the Chemical Corps, contacted the National Opinion Research Corporation, NORC, at the University of Chicago. He wondered if it would be willing to undertake a field study in Donora. Within days of the accident, Wood, NORC researchers and some invited guests got together.[15]

The group concluded that a field study was no longer feasible. By the time that interviewers could be recruited and trained, there would be too much of a time lag for retrospective accounts to be reliable. They decided instead to invite NORC to consider the outlines of a

13 Associated Press: 1948, "Donora smog held near catastrophe; expert asserts slightly higher concentration would have depopulated community", in: The New York Times, December 25 (1948).

14 The Army Chemical Corps were not the only officials interested in Donora. A large multidisciplinary team from the Division of Industrial Hygiene of the US Public Health Service came out immediately. It took a year for the epidemiological analysis to be completed. Not surprisingly, there were psychosomatic illnesses. Some of the interviewees "probably exaggerated" the degree of their illness, while others, fearful of losing their jobs at US Steel, minimized their suffering. See James Townsend: "Investigation of the Smog Incident in Donora, PA., and Vicinity", in: American Journal of Public Health, 40, 2 (1950), p. 185. The Public Health Survey interviewed 1308 households which resulted in reports of about 4613 people affected by the explosion. Army Chemical Corps: Conference 1950, p. 39.

15 Army Chemical Corps: "Symposium on Psychological Research in the Chemical Corps", in: Medical Division Report #169, October 22 (1948).

study that could be used in future disasters.[16] Wood instructed NORC to develop a standing plan that would enable a crack team of "skilled observers and investigators" to scramble to the site of a disaster and begin work immediately.[17]

Wood circulated NORC's plan to federal officials as well as psychiatrists, psychologists, and sociologists for comment. NORC revised its plan in light of criticism received, and resubmitted it in late December 1949.[18] A month later, Wood convened a conference in order to discuss the way forward.

THE 1950 CONFERENCE ON THE PSYCHOLOGICAL ASPECTS OF DISASTERS

Just as the *Strategic Bombing Survey* established the reference for civil defense's belief that pitiless bombing could be endured by a resilient people, the problems posed by the participants at the Chemical Corps' conference in January 1950 gave voice to the themes of disaster research for the following decade.

NORC's *Plan for the Study of Disasters* opened with the observation that from the point of view of administrative control, the available studies were journalistic and unsystematic.[19] Nevertheless one could discern "constant elements" in the mosaic of natural, industrial, and wartime disasters. The following were "minimum elements in efficient disaster control":

- The reduction and control of panic reactions
- Organization and effective leadership

16 Enrico L. Quarantelli: "The NORC Research on the Arkansas Tornado: a fountainhead study", in: International Journal of Mass Emergencies and Disasters 6, 3 (November 1988), p. 284.

17 Chemical Corps: Conference on Psychological Aspects of Disasters, Medical Division Reports #237, Maryland: Army Chemical Center, March 10 (1950), p. 1.

18 National Opinion Research Center, University of Chicago (NORC): „A Plan for the Study of Disasters", Revised Report covering Contract No. W18-108-CM, 1211, Army Chemical Service, December 7 (1949), appearing as Appendix 1, Chemical Corps (1950), pp. 66-77.

19 Ibid., p. 65.

- The elimination of confusion by means of directions to the public as to how to proceed and the provision of adequate and authoritative information
- The securing of [public] conformity to emergency regulations
- The minimization of discomfort
- The maintenance of public morale
- Rapid reconstruction[20]

These themes rationalized the whole enterprise of disaster research in this period. Because there were constancies across these events, one could study natural disasters and industrial accidents and extrapolate from them to the next war. I call this the Grand Analogy.

On the grounds of there being "constant elements in disasters and in disaster control, empirical study of peacetime disasters will yield knowledge applicable to the understanding and control not only of peacetime disasters but also of … war." Of course they were mindful of the differences among these classes of disasters, but in defense of the principle of generalization, they pointed out that there were *also* differences among different classes of disasters as well as differences between two disasters of the same kind. The fact of difference as such was not insurmountable. Given the assumption of *constant elements*, generalization could proceed. "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, and, therefore, permit generalizations applicable to war disasters as well as to the situation studied."[21]

The NORC plan circumscribed the research problem. The following questions should frame any plan of field study:

- 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 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?

20 Ibid., pp. 66-67.

21 Ibid., p. 67.

- What types of organized effort work effectively and which do not?[22]

What followed were pages of questions which a field team could use to survey the survivors of a disaster.

The second approach also related to disaster control. This one was rooted in mental hygiene. The Director of the newly-formed National Institute of Mental Health, the psychologist Joseph Bobbitt, ran through the conventional assumptions of military psychiatry which had coalesced by the end of the war. Anxiety would arise in people who were already predisposed to neurotic reaction; anxiety could be reliably dissipated by working at tasks for which one was trained and qualified; authoritative and realistic information about the nature and extent of the threat was reassuring. The tendency to panic could be blocked by a "preventive mental hygiene program."[23] Field studies were not needed, but *opinion research* about American's attitudes towards the next war would be necessary. What he wanted to know were "the nature of the fears and anxieties experienced as a result of anticipation of attack" and "the plans of individuals in event of the outbreak of war."[24]

An academic who did not enter the field by the attraction of a government contract presented a third approach to the group. Dr. J.S. Tyhurst of McGill University in Montreal was a psychiatrist who was wary of applied research, which he called "social engineering." The study of disaster would proceed in radically different ways according to whether it was oriented towards fundamental questions or in meeting an immediate need. "The problem is whether we are to have our research goals (and our methods) defined by the progress and findings of the investigation, or whether we are to define these goals and methods by the limitations of contemporary and practical necessity."[25]

Rather than disaster control or preventative mental hygiene, Tyhurst regarded disaster as a psychological and sociological "stress situation."[26] But like the NORC researchers he too was interested in classes and kinds. While he had only begun to do field work, he was

22 Ibid., pp. 67-68.

23 Ibid., p. 14.

24 Ibid., p. 13.

25 Ibid., p. 23.

26 Ibid., p. 25.

prepared to investigate "fires, explosions, floods, collapse of buildings, earthquakes, high winds, riots, strikes, rumor development, widespread utility breakdowns, epidemics, serious accidents, train wrecks."[27]

Tyhurst was unapologetically oriented towards basic questions. "The needs of research in this field include the setting up of a frame of reference that will organize such data into a coherent and meaningful patterns." His plan was consecutive and methodical. First he would do a literature review, then a field survey. Next he would undertake an "experimental investigation," followed by an "interview-schedule and questionnaire." Finally he would review the effort in its entirety. Thus he hoped "to provide an increased structuring of the data as the investigation proceeds."[28]

What mattered were the personalities of his respondents. When he went out to the field, he held two-hour long conversations that were open-ended, similar to psychiatric interviews rather than the standardized questions ticked off by poll-takers. The first time he traveled to a disaster site, he went alone. But he recognized the usefulness of adding an informed description of the social context of the people he met. He decided to include a sociologist in his field studies.[29]

So here we have the proposals for how to think about a disaster. Only one of these was reported by someone who had actually gone out and gotten started. (Tyhurst, by the way, isolated himself from the contract disaster researchers. Enrico Quarantelli, one of the original NORC field surveyers, and later the founder of the Disaster Research Center at Ohio State University, was dismissive of Tyhurst's efforts. He sniffed, "His work, maybe because it was published in psychiatric outlets, was a dead end with no continuity."[30])

Before we turn to the invited guests, let's hear from the two men in the Chemical Corps who felt the need for field study most acutely. When a review of the research programs of the Chemical Corps was conducted in 1947, David Dill, its Scientific Director, told the assem-

27 Ibid., p. 46.

28 Ibid., p. 25.

29 Ibid., p. 26.

30 Enrico L. Quarantelli: "Earliest interest in disasters and crises, and the early social science studies of disasters, as seen in a sociology of knowledge perspective", Working paper #91, University of Delaware Disaster Research Center 2009, p. 34.

bly that the only topic missing was the psychological dimension of gas warfare. More people would become "demoralized through fright" than would die or be permanently injured. But he couldn't find any useful reports on the topic, nor could he persuade anyone to study it. It was mystifying. "No one has suggested a plan for fundamental research to be conducted under contract dealing with human emotional reactions to toxic agents." Animals were useless, obviously, and one couldn't experiment on human beings. The only way forward was to exploit accidents like Donora, "uncontrolled and unplanned situations in which toxic agents lead to large-scale death and injury."[31]

What Dill wanted to know from his guests was whether his idea of looking at industrial accidents was reasonable. "If so," he asked, "how shall research on disasters be organized? Is the NORC plan valid?" But if the idea of looking at accidents was wrong, then what else could he do? "What is the best approach to research on the psychology of fear and of panic?"[32]

His colleague, John Wood, the Chief of the Medical Division, was in favor of open-ended exploration. What he wanted to do was assemble some clever people and have them ready and poised to travel to a disaster. "We wished to depend upon their skill and experience to shape the investigation to fit the circumstances. We want to learn as much as possible about the mass psychological reactions of a population in the face of such disasters and its effects upon the normal organization and function of the community – plus as much as may be possible of the psychological reactions of the survivors most directly involved." It was a great idea. Pick the right people, tell them what matters to you, let them loose on the phenomena, and have them report back.

The official understanding of *field study* stopped him short. "We quickly ran afoul of a Federal Statute, which requires a questionnaire to be prepared in advance for a field investigation of this type and submitted to the Bureau of the Budget for approval." So he took the long way around. He hired NORC, circulated its questionnaire to consultants and colleagues for comment, requested a revision, and now stood before an assembled conference. His good idea had been mutilated. "Our plan," Wood remarked morosely, "is thus, unfortunately,

31 Chemical Corps: Conference on Psychological Aspects of Disasters, p. 3.

32 Ibid., p. 4.

made far more rigid than seems to me to be warranted, and it reduces now largely to a poll-taking affair."[33] Wood was particularly interested in the group's response to the NORC questionnaire. Was it as clumsy as it appeared to be?

Herbert Goldhamer of The RAND Corporation was skeptical about the practical results of field research. To begin with, he thought panic prevention was a dubious business. He remarked, "It is generally recognized, although sometimes forgotten in connection with specific undertakings, that knowledge of how people behave in given situations does not necessarily enable us to control this behavior." The framework of the research problem was all wrong: one should not assume that any agency could control disaster behavior. Second, one should not assume that field surveys were the right way to accumulate knowledge of disaster behavior. He recommended "taking a step back." Rather than supposing that "field survey studies of disasters will contribute to the control of disaster situations," one should instead look hard at the methodology of field study itself. One should "inquire what contributions such studies can make to an understanding of disaster behavior" itself. The next step, from field study to agency needs, was equally questionable. "How much such studies can contribute to policy depends on the nature of the findings they provide."[34]

The physician Harold Abramson offered unexpected reasons for endorsing field studies. The problem with thinking about the next war was that inevitably one rebounds to the past. "We are not dealing in future disasters with anything that we really can measure." Since "future disasters are on an entirely different psychological level," he mused, "I feel very strongly … that we must have field experience in all minor disasters irrespective of their magnitude." No doubt they were "going to be much more minor compared to the magnitude of future disasters." But in order to block the impulse to think backwards in time, it was better to concentrate on any kind of disaster behavior in the present. "I would like to emphasize that without field experience we will be wandering around in the areas of World War I and World War II."[35]

When the NORC team reviewed the candidates for field study, the sociologist Shirley Star, NORC's lead researcher, observed, "it soon

33 Ibid., pp. 1-2.

34 Ibid., p. 37.

35 Ibid., pp. 18-19.

became apparent that every disaster study is going to be less than perfect … it won't be like the one you will encounter in war." There would *never* be an acceptable analogy from peacetime disaster to war. But that did not invalidate a generalizing approach to field study. The solution was to perform multiple case studies across a range of disasters. After a dozen or two dozen analyses have been made, a body of knowledge will have accumulated. "Each one can be made with increasing refinement and we can begin to find what is general to all disasters." The basic assumption of common elements across the kinds of disasters was justifiable. One develops a corpus of work, "then we can make some guesses about what would follow in wartime when you have a very different problem."[36]

Stop talking and let's get going! The psychiatrist Calvin Drayer practically burst with impatience. "There has been a great deal of talk and a great deal of comparison of ideas but we have got to get out into the field," he insisted. "Whether it is to be done as it is proposed to do or whether we are to adopt other methods doesn't seem too important right now." The main thing was to get started as soon as possible. "The point is that the needs of the Chemical Corps call for a tangible effort fairly soon." The group shouldn't overlook "the urgency" of the situation. The problem of extrapolating from peacetime disasters to war didn't bother him. "We have to get started on some definite approach which can then be modified as we work along and begin to learn its limitations. We can assume that there will be areas in which it does not work too well, that there will be differences in circumstances of the disasters which may be studied."[37]

What was to be done? Wood was angry about the way in which his beautiful idea had been mangled by bureaucratic stupidity. In his opening remarks he confessed, "I am not at all certain that this is adequate for our purposes. It may have to be supplemented by the addition of one or more professional psychologists or psychiatrists, who are not burdened in advance with a questionnaire – who are free to develop their own theses, upon which questioning is to be based, on the spot."[38] He wasn't convinced that "this approach to our problem is likely to be the most fruitful." The night before the conference he had gone over

36 Ibid., p. 59.

37 Ibid., p. 51.

38 Ibid., p. 2.

the matter thoroughly with two guests who were skeptical of the NORC plan. "I have some doubts about the matter."[39]

Wood and Dill decided to offer multiple contracts to different research organizations. In 1951 and 1952, the Chemical Corps issued two follow-on contracts to NORC. NORC ultimately produced eight field studies, including a major 3-volume work on a catastrophic tornado in Central Arkansas. It sponsored a conference, and wrote a final summarizing report.[40] The Chemical Corps also offered a contract to the Psychiatric Institute at the University of Maryland. It called for studies of "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." While psychologist-sociologist teams were envisioned for field study, the project stumbled. It was headed by a social scientist who supervised teams that made small studies of eleven disasters. Funded for three years, the effort resulted in a four page publication.[41]

THE NATIONAL RESEARCH COUNCIL

In 1951, Wood was promoted to the Chairmanship of the Medical Research and Development Board in the Army Surgeon General's Office.

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39 Ibid., p. 2.

40 Enrico L. Quarantelli: "Disaster Studies: An analysis of the social historical factors affecting the development of research in the area", Preliminary Paper #128, Disaster Research Center, University of Delaware 1988, p. 286. For NORC studies see Shirley Star: "Conference on field studies of reactions to disasters", NORC Report No. 47, NORC, University of Chicago 1953; Eli Marks/Charles Fritz: "Human Reactions in Disaster Situations", 3 Volumes, NORC Report No. 52, NORC, University of Chicago 1954; Charles Fritz/Eli Marks: "The NORC studies of human behavior in disaster", in: Journal of Social Issues 10, 3 (1954), p. 26-41.

41 John Powell: "An Introduction to the Natural History of Disaster", Baltimore, MD., Psychiatric Institute, University of Maryland, Baltimore, unpublished ms. 1954; John Powell: "Gaps and Goals in Disaster Research", in: Journal of Social Issues 10, 3 (1954), pp. 61-65.

He discussed his ideas with his counterparts in the other services. They decided that the combined offices of the Surgeons General of the Armed Forces should develop a proposal for research in disaster studies and submit it for consideration to the National Research Council (NRC) of the National Academy of Sciences.

The NRC, an independent organization within the National Academy of Sciences, prided itself on its capacity to nurture avant-garde research. The Chairman of the Division of Anthropology and Psychology boasted, "The NRC conceives itself … as a pioneer in new ventures. It likes to get new enterprises started and let them go."[42] Disaster research seemed like the kind of project that the NRC would agree to foster. It was a home only in affiliation and residence. The Committee on Disaster Research would have to find outside funding for the duration of its tenure at the NRC.

In May 1951, Wood sent a formal request to the Executive Committee at the Division of Psychology and Anthropology for the establishment of a consultative committee on disasters.[43] He envisioned that disaster researchers would undertake the following:

- To study the psychological reactions and behavior of individuals and local populations in disaster, for the purpose of developing methods for the prevention of panic, and for minimizing emotional, psychological, and psychiatric failures.
- To study the sociological upheavals caused by major disaster, to assess its effectiveness, to discover its failures, and to devise improvements to overcome its defects.
- To study the organization of the community to cope with disasters, to assess its effectiveness, to discover its failures, and to devise improvements to overcome its defects.

42 S. Stevens: "Memorandum to the Ad Hoc Steering Committee of the Committee on Disaster Studies", Ad Hoc Steering Committee, May 2, 1952. Folder: Committee on Disaster Studies, Executive Committee Meetings. National Research Council Archives 1952 (NRC abbreviated).

43 Col. John Wood, Chair, Medical Research and Development Board, Office of the Surgeon General, Army. Letter: 5/29/51. Folder: Committee on Disaster Studies, Beginning of Program (1951). NRC.

- To study the rescue, first aid, transport, treatment and disposition of casualties in order to devise ways to improve the handling of the injured victims.
- To study the effectiveness of extra-community assistance in disaster, in order to improve plans for bringing in timely outside aid.
- To determine how the Armed Forces can best assist civilian communities in major disasters."[44]

Several months later, Wood was invited to pitch his ideas to the Division heads in person. He proposed the establishment of a research group devoted to the American response to military attack and invasion. He commented that he was impatient for results "in this emergency situation."[45] The Korean War had been underway since June 1950. The emergency was the anxieties aroused by reports of extreme psychological coercion exerted against American prisoners of war. Military personnel across the services worried that civilians might not resist the pressures of communist psychological warfare unless proper training for national mobilization and morale was immediately instituted. The Division decided to establish a Committee on Disaster Studies using Wood's original proposal as the framework for the new group.[46]

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44 Letter, Col. John Wood, Chair, Medical Research and Development Board, Office of the Surgeon General, Army, 5/29/51, quoted in *Final Report to the Surgeons General, Departments of the Army, Navy and Air Force*, Contract No. DA-49-007-MD-256. Committee on Disaster Studies, Division of Anthropology & Psychology, NAS-NRC. March 31, 1955: 1.

45 Lieutenant Colonel John Wood: Minutes, Conference on Disaster Studies, December 6, 1951:9. Folder: Committee on Disaster Studies, Beginning of Program (1951). NRC.

46 The contract was extraordinarily broad, stipulating that the new committee would "conduct a survey and study in the fields of scientific research and development applicable to problems which might result from disasters caused by enemy action, including: (a) reviewing, analyzing, and evaluating the programs of research and development therein conducted under the auspices of private or governmental organizations in the United States or abroad; (b) proposing additional fields of investigation within these general fields, and additional projects for research and development

The Executive Committee on Disaster Studies met for the first time in March 1951. Over the course of the next six months, they wrestled with the immediate conundrum: what should the group tackle first of the ambitious program articulated in Wood's proposal and the subsequent NRC contract? What were the most important problems in the field? What should be the sequence of their work? What was the nature of the group? Would it administer contracts to individual scholars? Would it perform the research itself? The minutes to the first several meetings make excruciating reading. The members of the committee were all eminent men in their fields, they were not all in the same disciplines, they held different methodological commitments. The meetings were lengthy, inconclusive brainstorming sessions.

They also accomplished a few practical things. They established liaison relations with federal stakeholders in a number of agencies besides the Surgeons General such as the Public Health Service and the Federal Civil Defense Administration. They set up administrative offices in the NAS-NRC building in Washington. They hired staff, including an able research administrator, a sociologist named Harry Williams, to steer the project.

William's entry into the picture in early Summer 1952 signals the start of the group's real work. The first thing he does is travel around the country talking to everyone who is studying individual and group behavior in a disaster. He asks them what they think are the most important problems in the field. He also wants advice on what the NRC committee should do.

He meets Wood and asks for specific "guidance in my assignment." Wood hammered away at his unaltered conception of the problem.

> The American public has never been subject to a major catastrophe of the type suffered by the British and Japanese populations in the last war. We do not know how the American people would behave in such an event. The only way we have to study this problem of learning how they would

therein; and (c) collecting, collating, and disseminating scientific and technical information in these fields." Wood letter 5/29/51:2. Also see Minutes, First meeting, Committee on Disaster Studies, March 31, 1952. Folder: Committee on Disaster Studies, Meetings, 1952-1957. NRC.

> behave is to study disasters which do occur from time to time and see how people behave in these situations.[47]

He felt the urgency of the war intensely. "He stated that we needed to have results in the next year which will be helpful in making plans for the actions of responsible agencies in the event of a disaster. It is … an emergency kind of situation."[48]

By 1952, Woods was certain about what he wanted. There should be a direct relationship between the problems and responsibilities of the military sponsors and the questions posed by researchers. Field teams should be dispersed, trained, and put on standby so that they could travel as soon as possible to a disaster site. This was the method "on which he pins his hopes," Williams reported. "The organization of disaster study teams in various regions that will be prepared to observe the actual events which occur in a disaster situation."[49] Wood wound up by fuming about the impossibility of recruiting people into this effort. "He has been trying for four years to get something going in this area and has found a lack of interest on the part of people that he felt should be interested."[50]

This last point is not trivial. While disaster research has had a long afterlife since the 1950s, the dissolution of the NRC disaster research effort in 1962 amounted to a failure. I will return to this at the end of this essay. But let us notice here both Wood's persistence and the fact that he could not succeed in persuading others of the significance and intellectual merits of a problem he found arousing and vital.

The first order of business for the new research group was to collect books and articles, unpublished studies from federal agencies and universities, protocols of interviews with disaster survivors. The committee amassed a comprehensive bibliography. What followed was a plan of action, and a cascade of memoranda, newsletters, minutes, working papers, grant proposals, conference papers, annual reports, final reports, promises, excited meetings, frustrations, fizzles, small successes. A change of name. A new employee from NORC.

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47 Harry Williams: Report No 1: 8/4/52, Meeting with Col. John R. Wood, Surgeon General's Office, US Army (1952), p. 1. NRC.

48 Ibid., p. 2.

49 Ibid., p. 3.

50 Ibid.

CIVIL DEFENSE

From the war years, through the Korean War, all the way to the Cuban Missile Crisis, civil defense meant protecting civilian against the harms of incendiary, gas, and atomic bombardment. It encompassed programs to evacuate children, the elderly and sick to the countryside, the distribution of gas masks, the organization of neighborhood fire brigades and aircraft spotters, emergency radio communication, stocking underground bunkers in subway tunnels, basements and cellars and flimsy backyard installations, and ambitious but unrealized plans to disperse critical industries across the nation.

In 1950, the year the Federal Civil Defense Administration was founded, when working out state and regional civil defense programs that directly concerned vital sectors of the civilian economy, public morale, and the associations of civic life, agency bureaucrats could refer to the World War II and contemporary Korean wartime needs for a central command authority. Such desired centralization had provided the rationale for the establishment of the National Security Resources Board and the Office of Defense Mobilization.

But civil defense did not enjoy presidential, congressional or popular support. Its budgets were always modest. Both in its wistful visions of a fully prepared society and its puny authorized mission, civil defense operated in the space between the federal bureaucracies with administrative mandates for emergency preparedness on the one hand, and on the other with the handful of stalwart veterans of the WWII municipal civil defense organizations who volunteered for duty during the Korean War. While its command authority was restricted to weak advocacy and prototype shelter building, nevertheless civil defense bureaucrats and disaster researchers alike approached the problem of national preparedness for surprise attack with the recent experiences and impulses of total social mobilization for war.

From 1951 through 1960, civil defense had an intermittent public existence. Thousands participated in compulsory civil defense activities: urban dwellers practiced evacuation drills in the early years of the decade: 11,000 people in downtown Spokane (in April 26, 1954 in Operation Walkout); 40,000 African-American inhabitants of Mobile, Alabama (on June 14, 1954 in Operation Scat); 28,000 citizens of Bremerton, Washington; the entire population (notionally speaking) (on June 24, 1954 in Operation Rideout). The nation was exhorted to

participate in President Eisenhower's annual National Civil Defense Day, (1955-1961). Women volunteered to be plane spotters in the Ground Observer Corps during and immediately after the Korean War. Employees completed mandatory or voluntary courses in hospitals, schools, federal, state and municipal buildings. Police and firemen, doctors and nurses, Red Cross and disaster relief personnel were instructed in rudimentary civil defense training. Most children in elementary and middle school and some high schools in all the states were drilled in "duck and cover" air raid maneuvers. Civil defense officers addressed luncheons at civic associations, clubs and auxiliaries. Ordinary citizens volunteered for specialized training as block wardens and auxiliary fire and police men. Nearly everyone had seen the yellow signs marking fallout shelters which began to appear on urban and suburban buildings at the end of this period (1961), and heard, if not heeded, the tests for CONELRAD broadcast on the radio and the air raid sirens sounded weekly in most cities.

What interests me is the way in which civil defense presented a conceptual problem to the disaster researchers in general, and how the phenomenon of fallout sharpened the problem of the analogy between peacetime disasters, previous wars, industrial accidents, and atomic war.

In undertaking their first task, literature review, the range of analogous events considered by the disaster researchers was nearly encyclopedic. The first study released by Committee on Disaster Studies was a survey of the literature entitled *Human Behavior in Extreme Situations.* It itemized specific and general events such as:

- atomic and conventional bombardment
- combat stress in the Battle of Guadalcanal, and the Battle of the Bulge
- major earthquakes
- the economic depression of the 1930s
- the black death in 14th century Europe
- the epidemics of yellow fever in the United States in 1793 and 1870
- expatriation and displacement during war such as the Spanish civil war refugees and the evacuation of British cities in World War II
- chemical explosions
- floods

- famine
- fires
- invasion, occupation, and conquest such as the recent occupation of the Axis countries
- massacre and pogrom
- mine accidents and cave-ins
- concentration and prisoner-of-war internment camps
- rebellions and revolutions
- mob violence such as race and prison riots
- sieges such as in Malta and Stalingrad in World War II
- hurricanes and tornadoes
- labor strikes.[51]

How did they extrapolate from this hodge-podge to the survival of the individual, group, community, and society during and after an atomic war? Before long, the disaster researchers no longer justified their methodological assumptions. They were working in an environment in which the idea that diverse phenomena manifested "constant elements" (something which the NORC researchers felt compelled to explain and defend) was common sense.

They could nonchalantly transpose incommensurable field and historical experiences into the world of atomic war because their institutional home was a prominent champion of "behavioral science," the prevailing fashion in social science of the period. In the latter half of the 1950s, NRC members who advocated the unifying synthesis promised by behavioral science persuaded the executive committee to inaugurate an ambitious program of research. They argued that such an effort would "bring behavioral science into the staff functions of government hierarchies ... especially in connection with military plan-

51 These categories were itemized under the heading, "Specific Disasters For Which the Most Useful Descriptive Bibliographies Were Compiled," in Anthony Wallace: "*Human Behavior in Extreme Situations: A Survey of the Literature and Suggestions For Further Research"*, in: Disaster Study Number 1, Washington, DC: Committee on Disaster Studies, Division of Anthropology and Psychology, National Academy of Sciences-National Research Council 1956.

ning."[52] As a result, in 1961 the divisional location of the Disaster Research Group, Psychology and Anthropology, was formally dissolved and replaced by the broader Division of Behavioral Sciences.

In a 1959 memorandum defining the reorganization of work under the new directorate, NRC officer Clyde Kluckhohn emphasized that "particular events and their sequences are of interest to behavioral science *only to the extent that such events may be regarded as instances of general or abstract regularities*." Kluckhohn defined behavioral science in direct contrast to the interpretive concerns of humanistic social research. "Behavioral science either excludes or de-emphasizes areas of the traditional social sciences that are dominantly historical or 'philosophical.'" Whatever in the humanities or social sciences disregarded "the biological dimension in human behavior" would be set aside. That is to say, "historical, or rational, or reformist" ideas would be omitted. Instead, the bases for behavioral science were "(a) observational and/or experimental investigations of human behavior; (b) the biological and situational bases of such behavior; (c) comparative psychology insofar as this bears upon the understanding of human behavior; (d) the construction of abstract models to represent regularities in the data."[53] "Regularities in the data" guaranteed the legitimacy of the Grand Analogy.

SHELTER HABITABILITY

In March 1, 1954, a multimegaton thermonuclear weapon was detonated at the Bikini Atoll testing area. On February 15, 1955, under pressure from activist scientists and citizens' groups, the Atomic Energy Commission reluctantly issued a press release describing the phenomena of radioactive fallout.

52 R. W. Gerard: Memorandum to the Committee on Behavioral Sciences, March 24, 1959, p. 1. Folder: Committee on Behavioral Sciences, 1959. NRC.

53 Memorandum on the June Meeting of the Committee on Behavioral Science. Folder: Committee on Behavioral Science, 1959. NRC.

1958 marks the inauguration of the fallout shelter program.[54] While President Eisenhower had been presented all year with blue-ribbon panels and summer studies clamoring for a massive federal fallout shelter program, he was unpersuaded. Rather than installing blast and fallout shelters in every metropolitan, suburban, and rural political district, as the federal agency and its boosters would have liked, the President declared that the national program would only disseminate public information about the physical effects of nuclear attack; survey existing underground structures, mines, subways and tunnels; fund limited engineering research to work out the degree of fallout protection offered by existing buildings; construct a handful of prototype shelters; mandate fallout shelters for all new federal buildings; and slap some signs here and there indicating the whereabouts of identified and stocked facilities.

The national policy placed the onus for shelter construction on the individual states, which in turn delegated responsibility to county and municipal governments, and the voluntary energies of private firms, civic associations, and individual citizens. Fallout shelter construction was neither federally mandated nor funded.

With the advent of thermonuclear weapons and the recognition of the phenomenon of fallout, the disaster researchers added a new modality to their Grand Analogy. In addition to field studies, it was now possible to add *laboratory simulations* to their catalogue of behavior under stress. In a 1958 memorandum urging the initiation of a laboratory research program on shelter habitability, the disaster researchers insisted, "Control over such conditions as duration of stay ..., kind of persons in the shelter, and supplies available is feasible only in the laboratory."[55]

When President Eisenhower merged two offices, the Federal Civil Defense Administration and the Office of Defense Mobilization into the Office of Civil and Defense Mobilization (OCDM) in 1958, a social science research division was established in the new agency. The

54 The OCDM announced the National Shelter Policy on May 7, 1958 and began to administer the program the following October.

55 "Memorandum on the Initiation of a Laboratory Research Program on Shelter Habitability", July 22, 1958, p. 3. Folder: Anthropology and Psychology, Disaster Research Group, Advisory Panel on Shelter Habitability, Guidelines for Research, 1959-1960. NRC.

division underwrote research on social and psychological reactions to nuclear attack and post-attack conditions; plans for evacuation and dispersion, relief and reconstruction; and above all, ways to attain widespread cooperation with civil defense preparedness programs throughout the nation.[56]

In 1958, at the request of the OCDM, the NRC established a Consultant Panel on Shelter Habitability. The problems of *habitability* seemed straightforward: shelter training, taking shelter at the time of warning, occupancy, and post-shelter survival and recovery. Habitability studies sought to determine the physiological, social, and psychological effects of nearly every dimension of shelter confinement.

An NRC memorandum outlining the research needs in shelter habitability underscored the fact that current designs exacerbated the discomfort of confinement. Design specifications were themselves a „very real danger". One had to keep in mind that:

> Space, food, ventilation, sanitation and sufficient warmth represent the barest necessities. ... Medicine, light, communications, sound and vibration control ... are other factors deemed necessary to comfort in our society. … Probably least essential to immediate problems of actual physical survival are such things as recreation, religion, and education. However, the heightened morale generated by these activities might make the difference between enduring the stress or lessening it, and giving up or leaving.[57]

OCDM wanted the disaster researchers to correlate human factors to design criteria by means of examining such problems as:

- What facilities, equipment, and supplies would be indispensable?
- What sleeping, seating, and space arrangements would be most effective?
- How well would people tolerate shelter confinement?

56 See Ralph Garrett, Director, Social Science Research Division, Office of Civil and Defense Mobilization, in George Baker, Behavioral Science and Civil Defense, Disaster Study #16, Washington DC: National Academy of Sciences-National Research Council 1962, p. 109.

57 Memorandum, 6 April 1959. Disaster Research Group Folder: Advisory Panel on Shelter Habitability, General, Division of Anthropology and Psychology, 1958-1960. NRC.

- Would they be willing to enter shelters?
- How would their willingness to remain in shelters be affected by the length of time they might be required to stay?
- What organization, management, and leadership elements of a shelter program are indispensable?
- How would shelter experience affect the occupants' ability to face the stringent demands of post-shelter survival and reconstruction after leaving the shelter?[58]

FIELD STUDY AND LABORATORY RESEARCH FOR SHELTER HABITABILITY

In 1958 two memoranda circulated among members of the NRC Shelter Advisory Committee. The first once again affirmed the legitimacy of extrapolating from field studies to future war. "Natural and manmade peacetime disasters and major accidents provide the closest approximations to 'real' attack situations." They were "natural laboratories" in which behavior under stress "can be studied most fruitfully."[59] The authors admitted that the very nature of field study of natural disasters could not apply to some dimensions of habitability. "The extrapolation of findings from 'natural' shelter situations to planned or actual wartime shelter designs is limited by the fact that the natural situations rarely provide comparable physical structures, equipment, and environmental conditions."[60]

Nevertheless, field studies of "disaster- or accident-type shelter situations" did yield data that could not be acquired in laboratory simulations. What was missing was fear. „An experiment cannot introduce the disaster stresses of overwhelming threats to life and limb, of sudden destruction of kin and intimates, of the pain and shock of serious personal injury and loss of home and possessions."[61] The uncertainty and contingency of a disaster could not be replicated in a labora-

58 G. Baker: Behavioral Science and Civil Defense, p. 114.

59 Memorandum, no date: "A Program of Field Research On Shelter Habitability", pp. 2-3. Folder: Disaster Research Group, Advisory Panel on Shelter Habitability, Guidelines for Research, 1959-1960. NRC.

60 Memo On Field Research On Shelter Habitability, p. 6.

61 Ibid., p. 7.

tory. Nor could researchers provoke authentic anxiety about the „future continuity of one's social life." Moreover, in a disaster, the people who gathered in an emergency shelter would be diverse. It was exceedingly difficult to assemble a truly heterogeneous group of research volunteers for a shelter confinement experiment. The behavior of laboratory subjects would be unnaturally similar. But real disaster victims would behave more like nuclear war survivors. The very lack of control over the environment guaranteed the richness and even the accuracy of research observations.[62]

The memorandum concluded with a catalogue of topics relating to shelter confinement that could plausibly be addressed with field studies.

- Reactions to warning: studies concerned with factors ... regarding the acceptance or rejection of air-raid sirens.
- Expectations concerning length of stay in shelter and decision about leaving.
- Effects of size of shelter group and diversity in social and cultural characteristics in determining the type of organization, utilization of skills, emergence of leaders, division of labor, conflict, and other interactional patterns.
- Effects of separation of family members and other critical uncertainties ... in determining behavior in shelter and length of shelter stay.
- Effects of death, serious injury, and illness among members of shelter group.
- Reactions to conditions of close confinement, conditions of crowding, and lack of solitude and privacy.
- Reactions to absence or shortages of food, essential facilities and supplies, personnel with required skills, and other physical deprivations.
- Reactions to isolation and loss of communication with the outside world.
- Physical and emotional condition of shelterees of time of emergence from shelter and capacity to work in the post-shelter environment.

62 Ibid., p 9.

- Types of spontaneous or emergent solutions to shelter problems -- including emergent leadership, forms of organization, division of labor, improvisation, changes in social values and norms, and degree of social cohesion.
- Effect on shelter-taking and other protective behavior of recurrent or continuing threat in the post-shelter environment.[63]

In considering the kinds of analogies which the disaster researchers had explored in the previous eight years, the memo correlated the following kinds of disasters with problems for shelter study:

- Strategic bombardment during World War II.
- Natural Disasters: Researchers focused on reactions to massive community destruction and to the total loss of property. The studies on community dynamics also considered mining accidents and chemical spills.
- Forced Confinement: Disaster literature typically included the analysis of concentration camps, prisoner-of-war camps, displaced persons camps, and civil prisons. Researchers focused on the deprivations, uncertainties, and isolation of these group experiences.
- Accidental isolation of some duration such as accounts of explorers and military personnel who were unexpectedly cut off from their group for long periods of time, and civilians stranded in their cars, restaurants or office buildings during blizzards or flash floods.
- Isolation on extended missions: these comprised studies of scientific or military groups who spent weeks or months in isolation in the polar regions, in submarines, and missile silos and bombers during alert field exercises.[64]

But what about laboratory simulations? The shelter advisory committee convened in July 1958 in order to discuss the initiation of habitability experiments. A memorandum resulting from the meeting expressed the uneasiness long felt by researchers pressed for specific policy recommendations. It began by emphasizing the limits of extrapolation. The conditions of shelter confinement were *unlike* the catastrophes

63 Ibid., p. 12.

64 Ibid., p. 13.

probed by field study: a two-week wait of confinement, the invisibility of the threat, passivity as compared to the energetic flight and work in natural disaster, and the assault against the whole, rather than a part, of society.[65]

The authors conceded that while nuclear attack "would involve many fears, expectations and uncertainties which could not ethically or practically be simulated," engineering research could be done in laboratory tests, and indeed, this was fundamental to habitability.[66] They catalogued the criteria for evaluating prototype and experimental shelters. Shelters should be evaluated for the following: their architectural features and social and psychological factors must combine to create the internees' willingness to stay for two weeks, they must support physical and mental health, and they must be cheap.[67]

Although it was not possible to simulate the unbearable anxieties of nuclear attack or the traumas of confinement such as the close proximity to sick, dying and dead people, nevertheless crowding, poor food, humidity and heat *could* be reproduced in a laboratory shelter.[68]

SYMPOSIUM ON SHELTER HABITABILITY

On February 11 and 12 in 1960, the NRC and the OCDM jointly sponsored a conference called *Symposium on Human Problems in the Utilization of Fallout Shelters*.[69] It assembled most of the beneficiaries of the NRC's grants of the previous decade.

65 Memorandum, no date: "Memorandum on laboratory research program on shelter habitability", pp. 2-3. Folder: Disaster Research Group, Advisory Panel on Shelter Habitability, Guidelines for Research, 1959-1960. NRC.

66 Memo on lab research, p. 3.

67 Ibid., p. 5.

68 Ibid., p. 6.

69 G. W. Baker/J. H. Rohrer (ed.): Symposium on Human Problems in the utilization of Fallout Shelters, Disaster Study #12, Washington: National Academy of Sciences-National Research Council 1960. The conference was sponsored by the Office of Civil and Defense Mobilization under the direction of its Social Sciences Division.

Let us listen in on the way in which the Grand Analogy was imagined, defended, justified, explained away.

What in the world could be likened to life in a fallout shelter? Submarines? A mission station on the North Pole? We can start with a field study conducted by John Rohrer of Georgetown University Medical School who delivered a paper called "Implications for Fallout Shelter Living From Studies of Submarine Habitability and Adjustment to Polar Isolation." Rohrer prefaced his comments with the declaration that it was "difficult to extrapolate the findings on submarine habitability to fallout shelter habitability."[70] The difference lay in the nature of the hierarchical, ritualistic, well-rehearsed submarine society. But the two experiences were not incommensurable. On the basis of the anxiety experienced by highly motivated submariners upon entering into conditions of isolation within the first two days of the dive, Rohrer speculated that "the most critical period in terms of managing, minimizing panic, and maintaining control of the shelter population" would most certainly occur during the initial hours of confinement.

He enumerated the kinds of anxiety shelter inmates were likely to feel: „not knowing what to expect in shelter living, fear of radioactive contamination through contact with other people in the shelter, fear of the presence of contagious diseases, phobic fears in response to the small space and the threat of suffocation, anxieties over family, business, etc."[71] People needed diversions in order to suppress these feelings. He suggested card games, movies, hobbies, daily routines.

He reviewed the usual topics of habitability: crowded space, the necessity of communication with the outside world, humidity, ventilation, the role of hot food in sustaining morale, excessive sleeping as a refuge from boredom and fear, the importance of recreation for maintaining group cohesion and high morale, and the significance of assigning tasks to people so as to relieve anxiety. Rohrer assumed a simple correspondence between submarine confinement and shelters. He noted resemblances where he saw them, and justified his position with a statement no more elaborate than the remark, "The implication for fallout shelters is straightforward." In this, Rohrer's presentation was typical of the majority of disaster researchers.

70 Ibid., p. 26.

71 Ibid., p. 22.

The sociologist Albert Biderman was far more ambitious. Whereas most habitability researchers simply identified the interaction between environmental stresses and social relations in the shelter as *problems* and sought their remedies in optimal engineering, Biderman imagined the optimal shelter culture that could socialize the deprivations felt in confinement. His field studies were prisoners of war and concentration camps.

He began by imagining the problems that would arise for a large group confined to an inadequately stocked facility.[72] The nearest analogy to this would be a group of prisoners incarcerated in a temporary camp, "where the captor did not exert active influence on the affairs of the group. ... The shelter would be like the collection point … It would be a temporary way-station toward a dimly-known, indefinite future: a purgatory."[73]

Biderman's question was disturbing: how could one design a shelter so that people would not kill, cannibalize, violate one another, or commit suicide? "Where deprivation exists, complicated problems of sacrifice and balance arise between the biological and the sociocultural systems ... This conflict ... [has] an important bearing on the problems of social organization in shelters."[74] Would they lose the will to live? Chronic starvation could result in apathetic withdrawal amounting to catatonia. This struck a chord in his audience. Behind much of the rhetoric concerning the need for civil defense training in order to insure high morale for post-war reconstruction was the widespread belief that the American soldiers who died in Korean prisoner-of-war camps were psychologically unfit to survive the rigors of the real cold war. In other words, they were sissies. On the contrary, Biderman assured them.

> The widely publicized contention that most of the deaths of American POWs were due to a psychologically caused "give-it-up" seems to be without foundation. Avitaminosis, simple malnutrition, and the dehydration associated with … dysentery … seem to have played much greater

72 Ibid., p. 31.

73 Ibid., p. 40.

74 Ibid., p. 33.

> roles. A shelter program need not be founded on the premise that Americans have become soft or that they are softer than other nationalities.[75]

Prisoners of war typically did not give way to panic or hysteria, but instead to introversion. Clinical depression set in long after the deprivations of two weeks. Nevertheless, he admitted, depression might be a problem. Here Biderman moves directly from the camps to the shelters. "Depression," he observed, "is likely to be immediate when the privational experience followed the total destruction of the captive's sources of basic meaning and purpose; e.g. when he regarded himself as totally and irrevocably isolated from his world." This is why shelters shaped the psychology of its inmates. One wanted "frills" in order to encourage "activity, involvement, and a positively-toned mood."[76]

One had to know how to think about the matter properly. Americans needed to learn what they should expect. "The attitude that surviving a thermonuclear attack would be a 'life not worth living' is a widespread barrier to the acceptance of civil defense measures." But, Biderman insisted, suicidal apathy was not an inevitable consequence of nuclear war. The experiences of the concentration camp survivors confirmed this point. The chief problem for shelter culture lay in establishing binding social relations robust enough to check in-shelter strife. Shelter internees were likely to be alienated from one another. Prison societies usually adapted to the stresses of captivity by tolerating spiteful jokes, malice, churlishness, and provocative hostility. But survival itself could be the kernel of solidarity in a subsistence situation if it appeared that all might survive if everybody cooperated. But in subsubsistence situations, "shelter leaders must have a commitment to purposes for which they are willing to sacrifice others, and preferably, a purpose to which others are willing to be sacrificed."[77] Only group attachment to "heroic purposes" would solve the problem of legitimating the sacrifice one or more of the shelter inmates. The shelter manager had to know how to arouse the memory of a cherished cultural value of the pre-war society to which the present could be linked such

75 Ibid., p. 43.

76 Ibid., p. 34.

77 Ibid., p. 46.

as "the defense against invasion and the reconsolidation of the larger community and the nation."[78]

From his discussion, one might suppose that, quite apart from the malnutrition that weakened the resistance of the American POWs in Korea, life-sustaining morale was difficult to cultivate. Without intensive mass indoctrination, Biderman seems to have argued, shelter inmates might not survive – a point which might shake one's confidence in civil defense's social efficacy. But civil defense boosters always transposed these propositions into positive imperatives. Because national survival was at stake, intensive preattack training must be initiated immediately. This was a national security measure that could not be gainsaid, for who would dare to challenge a program designed to protect individual lives, sanity, and civilization itself?

Biderman's approach to morale was echoed by many other disaster researchers. For example, in summing up his report on a 500-person shelter study, the social psychologist Donald Michael highlighted the importance of training "pre-selected shelter managers" as well as indoctrinating the public about "the realities and obligations of shelter life."[79] He defended the compulsory nature of his ideas with reference to the austere conditions of group survival. Necessity did not tolerate democratic disorder. He admitted, "It may seem that this system is quite rigid and authoritarian," but enduring two weeks "in a small space with very limited resources" required the concentration of authority.[80]

Most disaster researchers assumed authoritarian leadership during the shelter period, and some kind of martial law in the post-attack society. But there were exceptions to this view. The sociologist Norman Hilmar of the Walter Reed Army Institute of Research described a clan-based, democratic organization for shelter-society. He proposed that the mass of shelter occupants be divided into groups of ten or twelve people. Their social space could be enclosed by partitions. "By forcing people to move into small quarters... they can group according to families, friendship, or common interest." These primary groups would not only provide emotional support, but would be sources for "social influence and control." Hilmar argued that "meaningful, ac-

78 Ibid., p. 47.

79 Ibid., pp. 191-192.

80 Ibid., p. 192.

ceptable control" over the small area of the sub-group could supplant the terror and unfocused anxiety arising from the catastrophic disruptions of nuclear war and shelter confinement. "By setting up these small living units, you can permit a modicum of self-determination and thus foster feelings of personal responsibility in a situation where the individual can see that his own actions now count. The individual thus becomes one of a face-to-face group of some twelve people, the names and identities of whom he knows, rather than one of a hundred strangers milling around in the underground." He suggested that the shelter be stocked with screens and other means of creating privacy. These would "provide some psychological anchorings in proprietary feelings about a bunk or a chunk of earth. 'This is where I live. I may not have a home someplace else, but now I have a home here and it is this square meter of space.'" The small groups could be regarded as "quasi-political districts" in which one member of each could be elected representative to the shelter government. "If we can, by architectural arrangements, break people up into meaningful social units, we have made giant strides toward the differentiation and ultimate reorganization of society. I hope this will permit shelter occupants a vestige of democratic control over their lives even in this very horrendous situation."[81]

The sociologist Charles Fritz suggested that the initial period of confinement be devoted to designing and building shelter furnishings from a pre-arranged stock of lumber in its bare interior. Ideally, the shelter could include workshop space, hand-tools, and raw materials with which to fabricate in-shelter and post-attack survival goods. Not only would this activity contain anxiety, it would also serve as a kind of homesteading, allowing for specific accommodation to the needs of the shelter population.[82]

He imagined the fallout shelter to be the womb of the post-attack society. "People in shelters will be anxiously oriented toward the future, and the more realistic and meaningful the fit between the shelter activity and the future needs of the society, the greater the likelihood will be of channeling this anxiety into socially useful form." The right way to think about habitability was "as a period of reorientation and training for the hard realities of post-shelter life." By regarding shelter

81 Ibid., pp. 121-123.

82 Ibid., p. 149.

life not merely as a confinement to be endured but as "training," one can begin to imagine what might be needed for survival in the post-war world. The world inside the shelter should be arranged so that it could foster "the physical skills needed for post-shelter survival and psychological perspectives that will minimize the trauma of post-shelter perceptions."[83]

Edward Murray's paper, "Adjustment to Environmental Stress in Fallout Shelters," offered the most comprehensive picture of the laboratory studies undertaken for the OCDM. He enumerated the conditions that would contribute to inmate anxiety. Overcrowding or length of stay would produce "decreased efficiency, depression, and irritability." The temperature and humidity of the shelter would be nearly intolerable. Food would have to be rationed even though it was well known that the type and quality of food effects morale. "Acceptance of various kinds of emergency rations is related to personality," he observed. "Immature and maladjusted individuals may reject emergency rations." An inadequate water supply would result in disease and death. The quality and adequacy of the air supply of the shelter was also critical. "People who are subjected to other stresses, such as anxiety, disease, and hunger, tend to be more susceptible to the effects of oxygen deprivation." Fetid air, heat, humidity and crowding would produce a miasma of body odor. Adequate ventilation should be an engineering priority. One should also design for acoustical dampening, for crowded confined spaces were apt to be noisy. Similarly, engineers should pay attention to the quality of shelter light, too bright or too dim could torment people. The combination of these various factors could trigger insomnia, fatigue, irritability, depression, and aggression.

> Extreme crowding might increase the temperature and humidity. This would lead to sweating and, consequently, thirst, weakness, and irritability. The heat plus hunger might lead to nausea and this might produce vomiting in pregnant women and ill persons. This would add to the general level of tension. The tension, noise, and crowding would lead to fatigue and sleep loss and possible peculiar subjective experiences. Special populations, including the very young and the very old, the physically and mentally ill, would further complicate the picture. In addition, anxie-

83 Ibid., p. 150.

> ty about eventual survival and the fate of loved ones would interact with the environmental stresses."[84]

Many conference participants believed that in light of the fatal consequences of design decisions, laboratory studies examining these factors were essential. For example, Michael premised his large shelter occupancy study on the idea that the environment would structure, if not determine, inmate behavior. "These designs are ... expressions of management." He proceeded from the notion that "behavior drives from pre-conditions and can only eventuate in those forms that are, in fact, possible in the given physical environment."[85]

But others set aside the engineering problems and raised ethical and methodological questions concerning the realistic provocation of the anxiety that would correspond to atomic attack. James Altman commented, "Consider the ethical restraints to making a person believe that he may have been ... exposed to an overdose of radiation, that his family and closest friend may have been killed, that the total fabric of his society is in jeopardy. Yet these elements are required for realistic psychological simulation."[86]

The author of one of the earliest and most influential psychological studies of strategic bombardment was quite outspoken about the need for experimental studies of nuclear war.[87] What interested Irving Janis was the deliberate induction of anxiety about the war. He wanted to produce a civilian counterpart to the battle induction techniques of the last war, it was "emotional inoculation" for the nation.

He envisioned an alternative present in which civil defense would be mandatory and citizens would listen to lectures about the austerities and discomforts of shelter confinement. The description would be graphic, detailed, and disturbing (but not overwhelmingly so.)[88] By arousing the right amount of anxiety in the peacetime present, Americans would have ample time to master their fears so that they could tolerate the actual stresses of nuclear attack. The technique of emo-

84 Ibid., pp. 67-71.

85 Ibid., pp. 181-182.

86 Ibid., p. 165.

87 Irving Janis: Air War and Emotional Stress. Psychological studies of bombing and civilian defense, McGraw Hill 1951.

88 Symposium, p. 127.

tional inoculation assumed that by eliciting "a psychological 'working through'" by means of fantasized rehearsal, the subject will develop strategies for attaining self-control during the disaster.[89] Therefore, he suggested, civil defense officials should deliberately excite worry about the conditions within the shelter and outside; dread associated with uncertain threat; even annoyance resulting from social and political domination. They must also be reassuring. The fantasy rehearsal of civil defense training and indoctrination must lead someone to "vividly to imagine himself as a survivor in the future danger situation."[90]

It was for the sake of designing a program of mass emotional inoculation that Janis approved the ethics of laboratory simulations of shelter confinement. "It seems to me that it is a worthwhile research enterprise to try to do so, in view of the potential value of developing effective techniques of psychological preparation." The main thing was to "simulate as closely as possible the actual shelter confinement situation." This could probably be done. "I feel it is quite feasible to duplicate most of the essential psychological features of a wartime shelter confinement situation in an experimental [setting]."[91]

The sociologist T. W. Milburn also saw the wisdom of inspiring a public, collective fantasy of nuclear attack, shelter life, and post-war recovery. "Persons in crises should be actively seeking to master the environment that they will face. They should formulate values, goals and purposes, and expectations that lead to action. The existence of values and their internalization should serve to insulate them from the traumata of the real world."[92] The idea of imagining oneself in inside a shelter or wandering about the irradiated post-attack world attracted everyone's comment. The psychologist Dwight Chapman approached it in terms of role-conception. Americans had not yet imagined themselves as being a nation that could survive a nuclear war. He observed, "I cannot see that we have anything yet in our society that represents, for example, a literature providing a Horatio Alger who against great odds and by his own ingenuity worked himself up to be a civil defense warden or a manager of a shelter. Certainly we do not have any book

89 Ibid., p. 125.

90 Ibid., p. 127.

91 Ibid., p. 129.

92 Ibid., p. 219.

entitled *The Rover Boys in Macy's Sub-Basement*."[93] The Americans who did not fight in World War II and the Korean War did not imagine themselves as war-survivors. "It is a very heroic role. … I am not sure it exists in America. "[94]

Charles Fritz's ideas were certainly among the boldest and most ambitious of the disaster researchers. Not only had he been a full-time research associate of the NRC, he could claim the authority of having been one of the original scholars working on disaster sociology even prior to the establishment of the committee.[95] He introduced himself by stating that his ideas were supported by generalizations drawn from the entire corpus of 140 disaster field studies, the *Strategic Bombing Survey*, OCDM urban evacuation drills, and commissioned public opinion surveys.[96] On the basis of having studied everything possible to read or see, Fritz delivered his creed about the human future. "If disaster studies have taught us nothing else," he declared, "they have taught us that man is a highly adaptive social animal when he is directly confronted with clear challenges to his continued existence. He has survived every conceivable form of danger and horror in the past and, short of total annihilation, he will continue to do so in the future."[97] Prudential adaptation to the nuclear-armed world was the only way civilization would survive.

The current shelter policy with its piddling reliance on "individually-motivated self-protective action" was laughable. It was bound to fail because most people did nothing to prepare themselves to respond to a mortal danger until it was upon them. This is what he learned from his years of field study. The people who ignored or minimized disaster warnings had to improvise their means for escape and survival. But the nation could not improvise its continuity after an attack.

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93 Ibid., p. 224.

94 Ibid.

95 Fritz had been a commander of the photography division of the US Strategic Bombing Survey in Germany in 1947. Afterwards, he studied sociology at the University of Chicago. In 1951, he became the lead researcher in NORC's disaster field studies. In the mid-1950s he was hired by the NRC to be a fulltime researcher.

96 Symposium, p. 140.

97 Ibid., pp. 140-141.

"Not individuals, not communities, not states, not regions, but the total social system" would have to be reconstituted in the aftermath of nuclear war.[98] Therefore planning for national survival must not only be administered by the federal government, it must be imagined as the second founding of the nation. If American society was to survive, then the full dimensions of nuclear war must be made clear to the American people. "It should be obvious that we are talking about a major planned and directed effort aimed at societal survival. ...We are concerned not only with matters of biological survival ... but with problem of order ..., with meaning..., and with motivation." Civil defense simply couldn't be the voluntary impulse of a handful of people. The federal policy was recklessly wrong, miserably wrong. "If we continue to think of society as simply a collection of individuals utilizing resources, we will have lost our only hope of attacking the problem of preparation in any socially realistic way."[99]

A detailed appraisal of post-attack needs was the only way to determine the actual requirements of a national shelter program. This would be much more realistic than the agonized post-attack deliberations of town councils and village mayors. Fritz commented bluntly, "the traditional structure of American government ...is [not] capable of handling the needs posed by a severe attack on the nation." There was an absolute disjuncture between the political and social structure of peacetime and the post-attack world. During peacetime, civil defense functions were subject to competing sources of power, influence, and guidance.

The nation's shelter policy should be determined by comparing post-attack needs with present capabilities and *altering peacetime organizational structures* in order to accommodate anticipated post-attack realities. Among other recommendations he itemized, Fritz argued that one of the first things that should be done was assigning citizens to designated community shelters. These should be decided according to a plan to engineer a coherent society within each space, being careful to mix its population and insure the presence of complementary skills. "The aim in each case would be to replicate as closely as possible a total, self-sufficient community."[100] It was a life-boat

98 Ibid., pp. 142-143.

99 Ibid., p. 141.

100 Ibid., p. 147.

utopia. Fritz admitted it. He shrugged, "I am not concerned about practical matters of implementation. The model that I am aiming for … incorporates an ideal recognition of the human and social factors involved."[101]

FAILURE

The NRC disaster researchers exhausted themselves in their attempts to excite academic interest in their topic. They repeatedly promised university-based scholars that the projects they funded would surely produce insights significant to basic disciplinary matters. The sociologist Lewis Killian excitedly declared that the study of a disaster presented a rich "microcosm" for the study of human behavior. "Rarely are so many individuals simultaneously subject to such severe stress as in a large disaster. Unparalleled opportunities for the study of perception, fear, role behavior, leadership ... are offered." Like Wood, like Fritz and Janis, Killian was enthralled by the topic and couldn't understand why he couldn't sell the idea to others. "It is strange," he sighed, "that so little scientific interest in such events has been manifested by students of human behavior."[102]

From the beginning of the NRC program in 1951 to its demise in 1962, the disaster researchers spent hours analyzing strategies for recruiting prestigious scholars to their cause. They forlornly wondered why people in sociology, political science, anthropology, psychology, and communications did not find their work relevant or significant. Fritz thought he understood the general indifference to civil defense. Preparation for war had to do with a possible future, something "painful to contemplate," on behalf of which "there are no present societal rewards for the ... sacrifices involved in making preparations." The whole thing was too uncertain, too foggy, too unreal. There was no way to test the effectiveness of expensive measures, no way to pay for a huge program given the organization of the economy, and there

101 Ibid., p. 140.

102 Memorandum: Lewis Killian to Harry Williams, August 20, 1953. Folder: Anthropology & Psychology, Disaster Research Group, Studies: Man and Society in Disaster, 1953-1961. NRC.

would always be time someday to make a decision about something that may or may not happen.[103]

Was it apathy, indifference, or anxious repression that kept other scholars away? In 1958, Harry Williams sent a passionate plea to his grant officer in the OCD to recognize the position in which the disaster researchers found themselves. Thermonuclear war was "almost unimaginable and the known ways of coping with it sometimes seem tragically limited in comparison with the situation. … [It was] something new in the history of the United States and of mankind."[104] His group has scoured "existing knowledge and current ideas," but civil defense must also "seek new ideas, new insights, new inventions, new ways of understanding and dealing with a situation which is new in the history of the race." In order to advance the cause OCD must recognize the difference between basic research and applied studies. "Applied research, designed and negotiated to provide specific answers to specific questions is by its very nature limited in producing new insights and ideas." "Especially in a field like this one," Williams insisted, people needed to be able to follow whatever thread seemed important without fear of oversight or suspicion. Disaster research would mature only if financial, administrative, and the right kind of grant support were available. "They must become curious, they must be challenged, they must want to know. Then, they must have a freedom to exercise their curiosities, to answer the questions that plague them." The funder should therefore not inhibit "the free exercise of creative minds." If "grants which … enable [scholars] to pursue ideas and curiosities of their own" were known to be available, then conditions would be ripe for "unforeseen solutions." Williams declared, "Human beings are able to comprehend unimaginable situations and solve insolvable problems under the right circumstances."[105]

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103 Symposium, p. 142. Also see J. Nunnally: "Public apathy toward civil defense: a case of anxiety", in: G. W. Baker/L. S. Cottrell, Behavioral Science and Civil Defense, Washington: National Academy of Sciences-National Research Council 1962.

104 Memorandum: From Harry Williams To Ralph Garrett, Director, Social Science Office, OCDM. September 12, 1958. Subject: A Program of Grants for Research and Academic Studies on Non-Military Defenses and Survival in Thermonuclear War: 1. NRC.

105 Memorandum: From H. Williams to R. Garrett, 9/12/58, pp. 1-2.

The one thing missing in this proposal to recruit outsiders was the recognition that the Grand Analogy was precarious. Despite the disaster researchers' obsessive self-scrutiny and dialogue concerning the foundation of its work, from the beginning to the end of the NRC program, extrapolation from field study and simulation to war was approximate. At one time or another, all of the disaster researchers uneasily conceded the difficulty, if they did not confess, the impossibility, of transposing the particulars from the certain to the uncertain event. However extensive the net of associations of all possible disaster analogies could be to war, by 1960 the disaster researchers habitually temporized their recommendations with the concession of a weakly founded semblance between their field and laboratory studies to such problems as recognizing warning sirens properly, taking shelter quickly, surviving the environmental, social and psychological stresses of community shelter confinement, and leaving the shelter at an appropriate time and with good morale. Adaptive behavior to war could not be assured.

This wasn't an awful secret but an ordinary part of their work. It was understood that they moved by way of analogy, and that sometimes it couldn't be done. In fact, Williams wrote a letter to the same OCDM grant officer about a trip he had taken to the Naval Submarine Base in New London CT. He asked the officers whether they thought submarine experience was applicable to habitability. He stated flatly that too many differences "make direct extrapolation of information from the submarine situation to the shelter situation impossible." He concluded that "we should not expect to find a great deal of information which will be immediately and directly useable in understanding and planning for human habitation of shelters, except perhaps for engineering and physiological questions of environmental control."[106] Fritz was also not shy about making a similar remark. He wrote in a letter, "I seriously question the validity of most of the extrapolations made from the radar site study findings to post-attack shelter situations. …There appears to be very little that is analogous in these two

106 Letter: October 20, Harry B. Williams, Technical Director, Disaster Research Group, to Ralph Garrett, Director, Social Science Office, OCDM 1958, pp. 1-2. NRC.

situations."[107] The simple fact of extrapolation from the known to the unknown was hardly an ethical scandal. It was no longer even a methodological problem.

But the disaster researchers couldn't shrug off criticism from others about the validity of the Grand Analogy. The novelist and social critic Philip Wylie was incensed by it. He reviewed a handful of disaster research reports for *The Bulletin of Atomic Scientists*. He wrote, "Pragmatic investigations into familiar calamities, whether fire, flood, explosion, hurricane, epidemic, earthquake or church social food poisoning will not furnish dependable data for adducing human behavior under totally unfamiliar nationwide atomic onslaught." The next war simply could not compare to them. It would be "entirely novel, different by an order of magnitude of tens of thousands, and imposed upon a population which has already repressed and misconstrued information about atomic weapons."[108]

The disaster researchers believed that their work was realistic, honorable, ethical, hopeful and important. They were furious. They retorted in a later issue of the *Bulletin* that Wylie seemed "unwilling to admit in principle" that there might be *some* value in comparing collective behavior in a tornado or explosion to war. One could not refuse to consider civil defense because atomic war was "too horrible to think about." The nation was confronted by an either-or proposition. Either "Americans … throw up their hands in pious dismay at the prospect of atomic war and refuse to make academic studies or …[they] protect themselves with the best possible civil defense." Civil defense could possibly protect millions of people. "As scientists and as citizens, we feel that we *must* think about it, and that the best way of thinking about it is to formulate concepts, and to collect data, systematically."[109]

107 Memorandum: Charles Fritz to Harry Williams, May 22, 1959, Disaster Research Group. NRC 1959.

108 Philip Wylie: "When Disaster Strikes", in: Bulletin of the Atomic Scientists 12, 10 (December 1956), p. 377.

109 Lewis Killian/Anthony Wallace/Harry Williams: "Three Disaster Studies; reply to Philip Wylie", in: The Bulletin of the Atomic Scientists 13, 4 (1957), pp. 145-146.

FADE AWAY AND REPRISAL

While cold war interests conditioned virtually every aspect of their research, from the close consultation necessary for ongoing federal funding to the particulars of experimental design, nevertheless I resist the idea that the major thesis of the disaster researchers ought to be regarded as scientific heresy, epistemologically marginal to the undistorted best science of the day. The critics who objected to the optimistic findings of disaster research clearly had in mind the ideal of a dispassionate science against which anything having to do with official civil defense must be methodologically flawed, its conclusions biased by federal patronage, and its norms unscientifically motivated by politics.

But I suppose otherwise: disaster research was unexceptionably mainstream, rather than marginal, in design, technique, execution, and interpretation. The decade of work performed by the NRC researchers drew upon the fashions of the day such as behavioral science, operations research and systems analysis, communications theory, social psychology, and the physiology of stress. If I had to pinpoint its ideological core, I would look past its conclusions to the miscellany of well-established ideas on the basis of which they grounded their efforts, and from whose own methodological and substantive assumptions they formulated what they believed were arguably coherent extrapolations.

As meager as it was, the federal shelter program was eviscerated in 1964. The public concluded that the fallout shelter policy was scientifically erroneous and politically objectionable. While the Office of Civil Defense remained in place throughout the 1960s and maintained its research division, its long-term commitments to shelter habitability studies began to drop away. Habitability's problems were largely discredited.

Disaster research did not disappear altogether, but it disappeared from view. A handful of academic sociologists developed a niche specialty, but most disaster researchers found a congenial home in the applied problems of federal and state emergency management.[110] After

110 The NRC committee dissolved in 1962, but Charles Fritz stuck around. He found work in the following decades directing various committees at the NRC studying emergency response and natural disasters in a frame-

9/11, the elision of natural disaster with war resurfaced with excited flurries of concern about the new emergency. It would not be hard to make out a renewal of the Grand Analogy in the Department of Homeland Security's simulations for surprise terror-attack.[111]

work wholly untethered to civil dense. Enrico Quarantelli, (who had worked, as a graduate student on the NORC field studies,) founded the Disaster Research Center at Ohio State University in 1963. He moved it, along with its immense archive, to the University of Delaware in 1985.

111 See Sharon Ghamari-Tabrizi: "Lethal Fantasies", in: The Bulletin of the Atomic Scientists 62, 1 (2006), pp. 20-22.