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Sociological Theories and Disaster Studies[*]

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Sociological Theories and Disaster Studies

I am delighted and honored to give the inaugural DRC Lecture on Disaster and Risk. My thanks to the Co-Directors—past and present—for their hospitality and for making my visit possible. My remarks this afternoon are organized as follows. First, I make a distinction between disaster and risk, emphasizing how the two differ especially with regard to social time. Then I use this distinction to differentiate between earthquake disasters and the threat of earthquakes. Second, I argue that the study of the earthquake threat—as well as other forms of risk—may usefully be conducted with a sociological theory of social problems. I then briefly identify and contrast several different types of social problems theories, including social constructionist theories, and comment very briefly on how the latter overlap with constructionist theories in the sociology of knowledge, especially the sociology of science, on the one hand and with the resource mobilization theory of social movements on the other. Fourth, I describe how I recently attempted to apply these theories in an examination of the earthquake threat in the U.S. Finally, I conclude with some remarks about future theoretical directions that seem useful to pursue in disaster studies.

Risk Versus Disaster

Risk and disaster differ in significant ways, only two of which I will comment on here. First, people locate each differently with respect to time. Second, each differs with respect to how people acquire knowledge of its characteristics. Disasters involve the past. The threat that they possess is over (except for relatively quick-following "secondary" disasters such as earthquake aftershocks and tsunamis, which themselves soon become part of the past). This

means that knowledge of the details of disasters (when it occurred, where, how many were killed, what one was doing when it struck, etc.) can be obtained through observation, either direct or indirect (e.g., from others including news media). Risk, on the other hand, is about the future. The threat is yet to come. Hence, the character of events is unknown in their important details. They can only be inferred from past events that are deemed comparable. Thus, the social meaning of time is central to the distinction between disaster and risk and to the understanding of risk itself. When we study disasters, we ask people what they did. When we study risk, we ask people what they are doing in the present about an uncertain future.

Earthquake Disasters Versus The Threat of Earthquakes

Earthquake disasters are events in the past with known characteristics (Richter magnitudes, epicenters, lives lost, etc.). The threat of earthquakes exists in our ideas about the future. A fundamental question about the earthquake threat in the United States—perhaps *the* fundamental question—is: Why is there greater public concern for other threats that seemingly have less potential for future harm? If the proverbial experts are correct, we can expect the same number of people to die in a single worst-case Los Angeles earthquake as are murdered in the U.S. in a single year (about 23,000 people); the dollar loss to exceed the amount spent annually on the criminal justice system in the U.S. as a whole (about $70 billion in 1980 dollars); and the U.S. Gross Domestic Product (GDP) to drop instantly by five percent. Yet, even in California there is vastly greater public concern for crime than for the threat of earthquakes—or for all natural hazards combined, for that matter. Put differently, people's reaction to crime identifies it as a social problem, while their treatment of the earthquake threat suggests that it is not a social

problem despite the apparent similarities in the images of each. Sociologically, how can we explain this difference?

Theories of Social Problems

First of all, is this really a fair comparison? Does it make substantive and theoretical sense to compare the earthquake threat to other phenomena generally thought of as social problems such as crime? I think it does make sense. It makes sense not because crime or any other so-called social problem and earthquakes are somehow objectively problematic, but rather because behavior associated with each is comparable. The earthquake threat involves people (a) reacting to past earthquakes (b) to press for change in the present (c) in order to avoid an otherwise more negative future. Likewise, crime as a social problem also involves people (a) reacting to past crimes (b) to press for change in the present (c) in order to avoid an otherwise more negative future. Sociologically speaking, crime is a social problem not because crimes are inherently or objectively problematic but because crime is treated as problematic by certain organizations and institutions. The explanation for the non-problematic status of the earthquake threat therefore must be sought in the actions of appropriate organizations and institutions.

The social problems literature comes in two main varieties which I will label the "objectivist" and the "subjectivist" camps. Objectivists take the objectionable or problematic condition identified with a social problem to be a given—that is, to be both objectively real and objectively harmful—and proceed to examine the causes, characteristics, and consequences of that condition as a condition. For example, viewed from the objectivist camp, crime *is* a social problem; here are data on the rates of various criminal behaviors; some of the causes of crime are...; the costs of crime to the U.S. annually are...; some things that can be done to prevent crime

are...; etc. Most standard textbooks for undergraduate social problems courses are representative of this tradition (e.g., the several editions of Merton and Nisbet's *Contemporary Social Problems*; see, for example, Merton and Nisbet 1976). Theorists working in this tradition include both structural-functionalists such as Robert Merton and (often overlooked) their traditional critics, the conflict theorists, especially Marxian theorists who see social problems as objectively real consequences of the class division inherent in modern capitalism.

Subjectivists on the other hand deny that objective conditions are either necessary or sufficient for a social problem to exist. They contend that social problems are the outcome of group activity. Problems, in other words, are *accomplishments*. Hence the term "constructionist" is used to describe this camp; in fact, this is really a more accurate label than the term "subjectivist." Constructionists contend that conditions—whatever their objective nature—must be turned into problems through active promotion (called "claims-making"). They insist on using the adjective "putative" to indicate that the objective features of conditions are sociologically irrelevant in explaining social problems.

Sociology of Knowledge and Sociology of Science

Because there is considerable overlap between theories of social problems (especially constructionist theories) and theoretical traditions in the sociology of knowledge and the sociology of science, I will say a few things about these areas. In an effort to maintain brevity, I will unsystematically lump together what otherwise should be considered separate theoretical perspectives on knowledge more broadly and science more narrowly. There are many similarities between these areas and constructionist theories of social problems. Most obviously, the term "social construction" is common to all three. Recall that Berger and Luckmann titled

their famous (1966) introduction to the sociology of knowledge *The Social Construction of Reality*. Second, all three areas can be divided into "objectivist" and "subjectivist" camps. Not coincidentally, structural-functionalists such as Talcott Parsons and Robert Merton (as well as most sociologists of science in the U.S. including Stephen Cole, Jonathan Cole, Diana Crane, and Harriet Zuckerman) fit more or less easily in the "objectivist" camp (see Zuckerman 1989; also Cole 1992). So too do most Marxian-oriented sociologists of science who see knowledge and belief as rooted in class or social structure (e.g., Mannheim 1936 [1929]). Third and perhaps most importantly, the same dissociation of ideas about nature and the objective conditions of nature is central to all three areas. This is especially true of the so-called "strict constructionist" camp in the U.K. and Europe. Work under this theoretical rubric is generally referred to as "social studies of science." Its most well-known practitioners include Karin Knorr Cetina (esp. Knorr Cetina 1979), David Bloor (see Barnes, Bloor, and Henry 1996), Steve Woolgar (see Latour and Woolgar 1986), and Bruno Latour (1987). These practitioners contend that all scientific knowledge—including, by inference, knowledge about the causes, characteristics, and consequences of disasters such as earthquakes—is socially constructed.

Constructionist jargon is now thankfully passé, but the sociological significance of this perspective is not. What it insists upon is that scientific knowledge can be explained solely by the interaction among scientists and is not driven by the nature of nature. Personally, I prefer to avoid some of the controversies to which the extreme relativism that this point of view can lead. I am comfortable with the following assumptions: that there is an objective reality "out there," but that we can never experience it directly. Objective reality is always experienced through the intervening medium of culture, meaning most generally language but including the technical vocabulary of specialized scientific disciplines. The theoretical implications of a constructionist

view of science include the following: we do not need to become seismologists or geologists in order to understand the level of public concern for earthquakes; we cannot hypothesize a direct link between "objective" features of the earth and the level of public concern for earthquakes; we must hypothesize instead about how scientific knowledge is used in order to understand the level of public concern for earthquakes.

Constructionist Theories of Social Problems

Now, let me make a few comments about the constructionist versions of social problems theory. Joel Best (1989) divides constructionist theories into three camps; I will add a fourth. There are, first of all, the strict constructionists most notably represented by John Kitsuse and his collaborators (e.g., Spector and Kitsuse 1987 [1977]; Ibarra and Kitsuse 1993). Kitsuse's earlier work equated social problems with processes of claims-making and responding to claims about putative conditions. His more recent statement equates social problems with rhetoric about putative conditions. Like strict constructionists in the sociology of knowledge and the sociology of science, this variant of social problems theory makes no assumptions about objective reality.

Second, there are the contextual constructionists. These are undoubtedly the majority of social problems theorists and are best exemplified by Joel Best himself (esp. Best 1990), Herbert Blumer (1971), and Joseph Gusfield (1981) and, in the area of risk, by Gene Rosa's (1993) "reconstructed realism." Contextual constructionists retain the focus on processes of claims-making and responding. However, they are willing to make some assumptions about objective reality especially in order to explain why some claims are easier to promote than others.

Third, there are the "debunkers." These are social problems theorists who assume knowledge of objective reality in order to use that "knowledge" to evaluate claims being made

about conditions. Thus they are able to lend support to claims that are consistent with "knowledge" and to discredit, deny, or debunk those claims that are not. As Best points, debunkers are not really constructionists. Rather, they belong in the "objectivist" camp.

Fourth, there is Armand Mauss (1975). I find it hard to place friend Armand in any of the above categories of constructionist theories. He equates social problems with social movements. (Recall that the title of his 1975 book was *Social Problems as Social Movements*.) Thus, Mauss has much in common with Kitsuse. On the other hand, he accepts the descriptions of objective reality like the contextualists. However, he seems to part company with both Kitsuse and the contextualists over the products of claims-making activity. Whereas (the early) Kitsuse sees claims-making as evolving through a series of four stages—that is, as having a "natural history"—which may or may not have specific outcomes, Mauss holds that the problem *is* the process and gives the outcomes of the process little attention.

Without taking sides in the sometimes emotional debates among adherents to these theoretical approaches, I offer two conclusions. First, the social problem status of a given issue is a function of what members of organizations and representatives of institutions say and do about some condition, not of the objective features of the condition itself (whatever they may be). Second, the consequences of what they say and do is simultaneously facilitated and constrained by the characteristics of the organizations in which they participate. A constructionist theory of social problems (especially early statements of the strict constructionist version) and the resource mobilization theory of social movements (which deals with the organizational characteristics of claims-making while downplaying the nature and validity of grievances) provide useful tools for examining threats whose future dimensions can only be imperfectly inferred from the past—such as, for example, the threat of earthquakes.

Examining The Earthquake Threat: An Illustration

I must admit that, in talking about earthquakes, I really would like to talk about natural disasters in general. However, the amount of work required to do this would be prohibitive. Therefore, I cannot claim generalizability beyond the earthquake threat. I can only suggest the tools for doing so. I examine the content of claims (i.e., what is the message?), how claims are advanced or promoted, who carries out claims-making using what organizational forms, and with what overall effect. I will summarize my conclusions about each in very brief fashion.

First, the content of claims includes both a description of the "causes" of the problem and proposals for its "solution." Earthquakes are caused by impersonal forces. Most commonly cited are sudden fault movements in areas with high population density produced by developmental pressures. They are thus discussed using a rhetoric of nature. Human agents are largely invisible as causes of or contributors to the earthquake threat. Solutions include research to better understand these natural forces, better local preparedness, and mitigation to reduce expenditures by the federal government for disaster relief.

The nature of the earthquake "problem" has shifted subtly over time. Such shifts are correlated not with changes in the earth but with the emergence and influence of various professions and scientific disciplines. The oldest and most prevalent typification of the problem has been life-loss. In the 1970s psychological harm was "discovered" by an emerging mental health movement. The ascendancy of microeconomics, policy analysts, and benefit-cost analysis has produced the most recent typification—earthquakes as an economic threat.

Claims-making consists primarily of participation on expert panels and committees (e.g., those of the National Academy of Science and the National Research Council), giving testimony

at Congressional hearings, and appearance in television news (especially science stories about earthquake forecasts and predictions). Also prominent have been public television broadcasts such as *NOVA* and other special presentations dealing with science literacy. This claims-making process perpetuates the rhetoric of nature in thinking about the problem.

As this description of the claims-making process implies, promoters of the earthquake threat have been earth scientists in the academy and government agencies, engineers (same organizational bases), and mid-level federal bureaucrats (primarily in the USGS, NSF, and FEMA). I refer to this as the "earthquake establishment" made up of technocrats and bureaucrats. In a word, advocates of the earthquake threat are "insiders" rather than "outsiders," unlike most other problem-promoting groups. The structure of this earthquake safety movement consists of a loose but stable network sustained through workshops, panels, committees, and conferences largely funded by the federal government. Its resources consist primarily in its institutional base (funded positions in the academy and the federal government) and in the esteem of its elite (i.e., academic and government scientists). The movement's primary strategy consists of promoting voluntary change in both other governmental entities and households. Its tactics include providing scientific and technical information for disaster preparedness and planning and promoting mitigation. Overall, claims-making on behalf of the earthquake threat is an *apolitical* process.

Viewed from the perspective of constructionist theory, the earthquake threat reveals processes that are comparable to social problems activity but no *product* akin to social problems. The comparison with other social-problem topics suggests the reasons for this. Members of the earthquake establishment have access to decision-makers, but they themselves have no power. The threat has official recognition but low priority. Whatever public-policy successes the

movement has enjoyed have been accomplished without grass-roots support. In other words, earthquake safety insiders have no external power base. Not surprisingly, the outcome of their claims-making activities to date has been to produce what I call a *partially-constructed* social problem.

Future Theoretical Directions

I consider this examination of the earthquake threat from the perspective of social problems theory to be a meso-level analysis at best. Focused on group structures and processes, it is not really macro-level. I believe it is an important theoretical task for sociology to place risk and disaster in a larger context, at the societal level. An invitation from Henry Quarantelli to contribute a chapter in an anthology on "What is a Disaster?" (coupled with a sabbatical leave) provided me with the opportunity to begin to explore how this might be accomplished.

The essence of disasters is disruption, it seems to me. Disasters involve physical destruction that disrupts the routines of everyday life (micro-level) and therefore simultaneously the social structures produced and reproduced by those routines (macro-level). Rather than asking whether or not some type of event is a disaster, it seems more important to ask: Are there events other than disasters that also disrupt routines and structures? For both theoretical and practical reasons, I add a further requirement. Such disruptions must be sufficiently consequential to prompt a response by elements of the state. This has led to the development of a series of propositions about the process of routinization. By routinization I refer to the creation and recreation of routines and structures whose characteristics make them vulnerable to certain kinds of disruptions (as well as protected from other kinds of disruptions) for which

institutionalized exception routines emerge. I will share a few initial conclusions with you this afternoon.

First, I find the nation-state to be a useful analytical concept, but—surprisingly, at least to me— the term "society" is not. By state, I mean an entity with a monopolistic and legitimated claim to the use of force within a given territory and with the right to raise revenue, largely through taxation, for so doing. Most of what initially looks like "society" is better thought of as culture (language, beliefs and values, a shared sense of membership in a society, etc.). I have concluded that the institutional level is as "macro" as I can get. That is, I can recognize routines and structures that can be labeled the polity, economy, science-technology, family, religion, etc., existing within and through a nation-state, but I have difficulty identifying a readily observable entity to call "society."

In any case, this level of analysis allows me to give a precise definition to the term "crisis." Crises are disruptions (or threats of disruption that are taken seriously) to the routines of at least two societal institutions, one of which is—at least in modern societies—the polity. In modern states, most frequent are crises related to the polity-economy nexus (political economic crises and threatened crises). There are numerous examples of each, but in the early spring of 1997 the two most visible examples are the post-pyramid scheme crisis in Albania and the impending reversion of Hong Kong to the People's Republic of China. The political economic nature of most contemporary crises comes as no surprise given that the economy and the polity are the two dominant institutions in modern nation-states.

So far, this line of thinking has led me to three otherwise disparate-seeming projects. One is a rethinking the Northridge earthquake (Stallings 1995b) as a "legitimation crisis" (Habermas 1973, 1975 [1973], 1987 [1981], pp. 153-197 and 301-403). Another is the crash of a

charter airliner in the Atlantic Ocean (Stallings 1997), part of larger collaborative work on aviation safety. The third is an examination of the controversy surrounding monetary and non-monetary gold deposited in Swiss banks by the German National Socialist government during the period 1933-1945.

This line of inquiry and these empirical examinations related to it lead to two recommendations for future studies. We need comparative studies of nation-states in disasters. Taylor (1978) and many others have long called for the same type of studies. Two these recommendations, I add two specific suggestions. First, we need studies of disasters in nation-states with institutional configurations that differ from those of developed industrial-urban countries. To achieve this, we may need to carry out studies of historical disasters. The Lisbon earthquake of 1755 comes immediately to mind as an event that may be profitably studies. Second, we need comparative studies of crises *other than* disasters. This Swiss banking controversy is a good example. Such studies can disclose the nature of exception routines in a variety of institutional areas.

Before concluding, I would like to mention some writers whose work seems especially helpful in thinking about disasters, risk, and crises at the institutional level. I will indicate briefly what I think is useful in their work. First, Ulrich Beck's (1992) widely-read critique of modernity directly links various forms of threats, especially environmental threats, to the asymmetry between economy and polity in late capitalist states. Nicklas Luhmann's theoretical explorations on social systems (1985, 1995) are an untapped resource for disaster researchers. I find especially useful his assumption that reducing uncertainty in the environment is the basic problem facing all social systems, his propositions about time including ideas of non-simultaneity and time-binding, that is, of linking present and future and therefore cause with

effect. More familiar to disaster researchers is his long essay on risk (1993) emphasizing the separate vocabularies of danger and risk. For Luhmann, societies which develop vocabularies of risk come to think of more and more potential future harms as causally linked to prior human decisions. Those societies which possess only vocabularies of danger, on the other hand, think of misfortune—of which natural disasters are one type—as something attributed to an impersonal "environment" of either natural or supernatural forces. How vocabularies evolve from danger to risk and the implications of this evolution for the way in which social systems deal with uncertainty are central to his analysis of contemporary threats.

Anthony Giddens' theoretical apparatus, built around his concept of structuration (1979, 1984), provides several useful tools for disaster research. These include the linking of micro- and macro-levels and the treatment of time-space distanciation. He also provides a typology of societies based upon institutional differentiation and dominance that can be used to design the sort of comparative and historical studies of disasters mentioned above. More recently, Giddens has written specifically on the correlates of post-modernism (1990, 1991). These essays further elaborate his ideas on the assumption of the need for ontological security and how that need is tied to trust in anonymous institutions. Disasters and threats undermine that trust, with the potential for the de-legitimation of dominant institutions.

Not surprisingly, Max Weber (1978, 1976, 1950) continues to be a source for useful concepts and propositions. I find especially relevant his various treatments of rationality (including rational calculation), state formation, and legitimation. Weber also provides both a theoretical and an empirical rationale for the relative autonomy of the state, a point of view currently used in state-centered views of policy-making (e.g., Skocpol 1979, 1994) and Marxian state manager theorists (e.g., Block 1981).

Finally, the work of the late Norbert Elias (1978, 1994) is relevant in two senses. First, Elias builds a theoretical framework around the concept of "figuration" that in many ways parallels Giddens' theory of structuration without Giddens often opaque vocabulary. Second, Elias' seminal social history of the emergence of Western European civilizations contains not only a theory of state-formation but also is an exemplar for comparative and historical studies that is more inspiring than Weber's examination of similar topics.

A Concluding Remark

Let me conclude this afternoon by noting that this is a homecoming of sorts for me. It is not literally a homecoming, because this is my first visit to the DRC since it has been located here at the University of Delaware. Rather, it is an intellectual homecoming. Having left DRC 26 years ago, it occurs to me that I have returned to the fundamental theoretical question with which the center was concerned while I was there—the relations among institutionalized, institutionalizing, and non-institutionalized patterns of behavior. I discovered in preparing my remarks that I am working on my own answer to this question. I thank you very much for the opportunity to come here and share my thoughts on the subject with you this afternoon.

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