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CONCEPTUALIZING DISASTERS FROM A  
SOCIOLOGICAL PERSPECTIVE

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**CONCEPTUALIZING DISASTERS FROM  
A SOCIOLOGICAL PERSPECTIVE**

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The question of a taxonomy for disaster research cannot be addressed meaningfully until some sort of answer is given to the more fundamental issue: what is a disaster? Kreps does provide one general answer in his paper. Regarding much of what he says, he clearly is right. The evidence for this conclusion comes from the fact that many of his views correspond to mine--obviously there could be no stronger confirmation that he is correct in expressing those views!

However, I do have some problems with certain of his positions. In this brief commentary, let me discuss four matters regarding which I would take a different position than that which seems to be taken by Kreps. However, what is important is not that we disagree, but rather that the differences lead in different directions in conceptualizing disasters and developing typologies or taxonomies. So these are differences which make a difference.

Now as a preface, I should note that my remarks reflect a **sociological** perspective only. In no way is this a claim that disasters can only be approached sociologically. Actually, it is the very fact that disasters can legitimately be studied from all the perspectives of any discipline that deals with human and social behavior that necessitates specifying which disciplinary point of view is being used. No one formulation can be equally valid and useful for all perspectives be it a psychological, an economical, historical, geographical, political, anthropological, etc. one. There certainly is no reason to think that somehow the study of disasters can escape the disciplinary differences which are involved in the study of any behavioral phenomenon.

Also, given the disciplinary perspective being used, certain assumptions and a certain corpus of knowledge make sense. I use sociological

assumptions and draw from the general theories and research findings of sociology. Other disciplines would have a different set both of assumptions and theories/findings. Again, there is very little in the history of efforts to develop one interdisciplinary approach to phenomena that suggests disciplinary differences will all be melded into one overall perspective. (There is a multidisciplinary approach possibility, but that involves maintaining disciplinary differences).

**1. Disasters as conceptual creations rather than as realities which need correct labels.**

My view is that concepts are intellectual exercises, word games played by scientists. As such, they are rather arbitrary linguistic jousts and essentially they involve taking names or labels to fashion the world for the researcher. In the area of research, what the research scientist is saying, is that this is the verbal screen with which I will see the "reality" that I am studying. As such, labels create the reality being studied.

A different point of view is that the world preexists and that there are things out there which preexist the activities of the scientist. As such, concepts and labels are used primarily to give a name to what is already there, already existent. Concepts follow rather than precede reality in this approach. Kreps seems to imply this second position. For example, he writes that "while we continue to debate what disasters are, and when they occur, **no one denies that they do take place.**" In the next paragraph there are remarks to the effect that "Nor is there much debate about whether the civil strife in Nigeria and more recently, Lebanon are disasters" and "aren't social calamities such as economic depressions, various forms of terrorism, and wars disasters?"

I leave aside here the purely empirical point that a number of researchers do distinguish between consensus (i.e., disasters) and dissensus (i.e., conflicts) types of mass emergencies, or that there are those who treat disasters merely as one subclass of collective stress situations among whom there are other categories within which are placed economic crises. In fact, the title and stated scope of this very journal, and the character of the ISA Research Committee which publishes it, are not supportive of Kreps' elimination of major theoretical differences that others see between "disasters" and other crisis-like phenomena. This does not make Kreps wrong and the others right, but simply illustrates that others have a rather different view of what is involved.

However, more important is the idea that somehow or other "disasters" are out there in the world, that everyone should be able to see them, and all we need to do is to place the correct label on them. From my viewpoint, they are not out there at all. I do not see all of them in the same fundamental way with the conceptual spectacles I use. Nor is it just a matter of applying the appropriate label. Social scientists using a symbolic interactionist perspective, and it is the social psychology with which I operate, are likely to view concepts as structuring phenomena rather than just as labels to apply to existing phenomena.

To many, this may appear to be the chicken or the egg dilemma. To some others, however, including myself, it is a matter of what one assumes as a starting point. To Kreps "disasters" are out there waiting to be labeled. To myself, we create the phenomena of "disasters" by the conceptual labels we use or impose. Do "groups" exist? Sociologist--or at least most of them--seem to assume so, but most other people apparently have considerable difficulty in really seeing group phenomena, as anyone who has ever taught an introductory course in sociology can attest. Are "groups" conceptual creations of sociologists or are they out there waiting to be labeled? To cite a few conceptual discards from social science history--do "instincts", "races", "the unconscious", "crowd minds", etc., exist? Similarly, do disasters as objects of scientific research have independent existence outside of the concept of the researcher? (They are of course, part of common sense concepts, but that is not the issue here since we are discussing scientific concepts).

Also, our point here goes beyond the simple minded notion that anyone can define or conceptualize anything in any way they want. Rather, if disasters are conceptual creations, there are other criteria than empirical data by which to evaluate their usefulness. Instead, important in the process of conceptualization or reconceptualization are such matters as logic, consistency with related concepts, clarity of definitions used, new questions which can be asked, and different research which might be suggested.

**2. Use only social features to characterize disasters instead of including physical features as well.**

A Second problem I have with Kreps' formulation is that in my view he unfortunately mixes social and physical features in defining disasters. For example, he writes "what we need is some consensus on a definition

of disasters as a **social, physical, and temporal event.**" (italics added). Or "the most visible characteristics of these events are that they do **physical and social harm**" (italics added). Also, "I propose that social scientists distinguish actual or potential disasters in terms of physical and temporal dimensions of their impacts."

Now one does not have to be a doctrinaire follower of Emile Durkheim to argue that social facts should be explained by social facts. Put another way, my only interest as a sociologist is in dealing with social phenomena. It follows that in seeking to explain phenomena, it is necessary that both the explananda--that which needs to be explained (which are the characteristics of disasters)--and the explanantia--the conditions leading to the characteristics of disaster, must be both identified in sociological terms.

To the extent that Kreps talks of physical phenomena in terms of physical impact, he is not talking of social phenomena. His discussion of impact ratio is similarly flawed. In most respects, this is the ratio between the damages occasioned by the disaster agent and the remaining resources. Kreps does note correctly that the denominator in the ratio is crucial since it could vary considerably depending on the level of analysis involved. But again the ratio is couched in physical terms. That misses the fact that the social perceptions involved of the losses may be far more important.

Perhaps we can best see the problem involved by looking at another area of sociology. In the subfield of collective behavior and social movements, the concept of "relative deprivation" has been developed to capture the idea that what people perceive as their deprivations are what governs their behavior. It is not the absolute deprivations as measured in so-called objective terms--akin to the physical dimensions or impacts of disasters--or as seen by outside observers such as sociologists. A concept of relative impact ratio would parallel the earlier conceptual development of relative deprivation and would be more consistent with a genuine sociological approach to the matter. The relative deprivation concept has some inherent difficulties and probably is not a good conceptual role model. But the logic underlying its development is clear, and it is that logic we are interested in noting here.

We should also note that to the extent Kreps discusses the temporal dimension, he is clearly talking of chronological time. A long time ago,

Sorokin, and a little later, Merton, pointed out that sociologists, if they were to be true to their discipline, should use social time rather than chronological or physical time in their descriptions and analyses. To the extent we use the dimension of time in the disaster area, in my view, we should use social time (a topic which, fortunately, has seen a resurgence of interest in sociology).

In fairness, we should indicate that Kreps at several points expresses an intellectual uneasiness with his conception of physical impact and chronological time. In fact, at one point he writes: "immediate emergency domains such a damage control or warning issuance and dissemination, in effect, translate these physical and temporal features as the response of social units." I think this is a move in the right direction--social responses of social units are sociological phenomena in a way that is not true of physical dimensions or impact or chronological time. A similar leaning in the right direction is indicated at another point where Kreps after saying that disasters "strike suddenly" notes "or at least are ultimately represented as acute."

From my perspective, sociological disaster researchers should ideally not talk of physical or temporal dimensions as such. These features are, of course, at a common sense level and are easy to observe and use. But this is the same wrong argument for using standard census and demographic variables in survey research, although many sociologists recognize that the variables as generally used are not factors of a genuinely social nature. However, difficult or not, we should conceive of disasters for sociological purposes only in social terms.

### **3. Characteristics of disasters should not be equated or confused with conditions for and consequences of disasters.**

Kreps explicitly accepts an earlier admonition of ours "that any conception of disasters must distinguish its defining characteristics from its antecedents or consequences". In that statement we argued that we in the disaster area should stop confusing or equating antecedent conditions and subsequent consequences with the characteristics of a disaster. That which produces something--antecedent conditions, and that which is produced from that something--the subsequent consequences, is different from the intermediate category--the characteristics in this particular instance of disasters. Most of the conceptual discussions

about disasters badly confuse the three Cs, the conditions for, the characteristics of, and the consequences from disasters.

Now Kreps not only explicitly accepts the necessity of differentiating and keeping the 3Cs separate, but also recognizes the tautological trap that lurks in approaching the matter. In fact, Kreps later notes that disasters may be seen as independent or dependent with respect to social structure. This is another way of saying that certain antecedent social conditions will occasion disasters, and that disasters will have social consequences.

However, Kreps is partly inconsistent as well as unclear when he elaborates his position. For example, at one point disasters are defined as "nonroutine events...The key defining characteristics of such events are (1) length of forewarning, (2) magnitude of impact (3) scope of impact, and (4) duration of impact." I have no trouble with the specification of a nonroutine event. But as I see it, the other defining features particularly in the way they are detailed are either antecedent conditions or subsequent consequences. For instance, forewarning is less a feature of disaster agents per se than it is a function of the preventive, regulatory, monitoring and warning systems that societies and communities have, and as such is part of the social structure. (In fact, at one point Kreps partly concedes that "the warning period is a function of the effectiveness of warning systems"--why not concede that it is all social phenomena?) If so, then length of forewarning is part of the antecedent conditions and not a characteristic of disasters. A similar kind of analysis would in my view likewise indicate that the three impact-related characteristics are subsequent consequences of disasters and not distinguishing features of the phenomena.

The formal definition used by Kreps also becomes unclear because later it is said that the "underlying properties" of disasters are: "events, impacts, social units, and responses". (This certainly mixes the 3Cs not to mention that all four properties are said to have physical features). I do not see how these relate to the four definitional characteristics enumerated earlier--namely forewarning, magnitude, scope, and duration especially when the last three are features of impact. (We have already indicated our difficulties with this set). The effort to elaborate the distinguishing features of disasters is both necessary and laudable. But we do not see that the attempted elaboration succeeds. The result is both inconsistent and unclear.

**4. Disasters are better seen as part of social change dynamics than as nonroutine social problems.**

At several places Kreps refers to disasters as "nonroutine social problems" (italics removed). Or it is said that "most social scientists are referring to social and physical effects of or problems caused for social systems by nonroutine events". Later, looking at disasters from the viewpoint of applied emergency managers, he indicates that these are officials "who must deal with disasters as social problems." In these references social problems are not defined so Kreps near the end of the paper asked: "in what sense is disaster a social problem? But no answer is really provided in three points that are then made. It is said that "there are no categorical thresholds of disasters because impact ratios are strictly determined by what is considered to be the impacted social unit." This is probably true but it hardly defines social problems. It is then said that there is never going to be an exhaustive list of hazards facing the human race. Perhaps true, but neither is this a definition of social problems. It is then said that the boundaries of the field include events which have effects which are felt at all societal levels. Certainly true, but this still gives no definitional clue to what is a social problem. Nevertheless, following the making of these three points, Kreps concludes; "at least in their immediate aftermath, and sometimes for a considerable period of time thereafter, these events are social problems." This, we would argue, is simply no more than stating that social problems are social problems. What is a social problem is left an unanswered question.

In our view, part of the problem is that Kreps is operating with common sense notions of social problems--namely, something happens that disturbs people. However, the considerable theoretical and research sociological literature on social problems advance far more sophisticated definitions. It is perhaps because of this that disaster phenomena, while discussed in introductory and in collective behavior textbooks, are very seldom even alluded to in current social problems texts or theoretical discussions. (Fritz's famous chapter two decades ago on disasters, was dropped we understand in a second edition of a well regarded social problems text because the sociology teachers using it regarded disasters as an inappropriate topic for such a text). Put another way, except for a few disaster researchers, other sociologists apparently do not see disasters as social problems.

But the major objection to a social problem characterization we think is of a more fundamental nature. I think we should conceptualize disasters as part of **social change dynamics** rather than **social problems**. The latter conception tends to emphasize dysfunctional aspects. But it should be a matter of empirical determination, not definition, as to what consequences, if any, are negative. In actual fact, there has been enough disaster research to indicate that there are always winners in and functional results of disasters; they are not always bad in any sense of the term. Furthermore, a social change context not only allows for positive consequences, but more important, sets disasters within the social dynamics of social life. As such, they can be seen as an integral part of what usually goes on in the social structure, rather than as an external intrusion from the outside (a point Carr made in 1932 and more recently by those who argue disasters are manifestations of structural vulnerabilities). A social change emphasis also avoids the extreme relativism and the ideological biases inherent in any social problem approach, along with accepting elite views of what constitutes problems (that researchers sometime act as surrogates for political and economic elites may partly disguise but does not circumvent the issue).

Before concluding, it is necessary to observe that the four lines of criticism we presented are probably more indicative of difficulties in the field of sociology generally, than something unique to the disaster area or specific to one writer, Kreps. Sociologists in all area often assume that there is a world out there to which correct labels need to be applied. Many sociologists use nonsocial factors in characterizing and explaining what in one sense they think of as social phenomena. Many sociologists confuse and equate conditions, characteristics and consequences in their discussions of phenomena. Sociologists of all persuasions have been more inclined to look at social phenomena in social problems rather than social change terms. (Although American sociology far more than European sociology is open to this change reflected in some of the criticisms made in Europe by Pelanda, Dombrowsky, and Hewitt, among others, of the disaster studies conducted in the United States).

In conclusion, we should note that our ability to raise some questions about Kreps' formulation only is possible because he has been one of the extremely few scholars in the area who has attempted to address the question of what is a disaster. Not only in this paper, but elsewhere, he had tackled what I consider the most important issue which needs to be dealt with in the area; we need to clarify the central focus of our field of

study, namely, disasters. Kreps almost alone has systematically and at length advanced an answer. That is to his credit, and when the history of field is written in the future he may very well be singled out as one of the very few who in the first 35 years of disaster studies tried to provide the rest of us with some crucial intellectual guidance.