

University of Delaware  
Disaster Research Center

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#65

A RESPONSE TO THE DESCRIPTION AND EVALUATION OF  
THE DRC WORK IN THE DISASTER AREA\*

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\*Revised version of paper given in response to presentation to Professor Gary Kreps on "Lessons Learned from the Social Responses Disaster Studies" at the meeting, Social Science Contributions to Natural Hazards Research: An Assessment, on May 18-19, 1979, Washington, D.C.

Kreps' paper accomplishes three things: (1) it selectively presents the history of social and behavioral disaster research in the United States during the 1950s and 1960s; (2) it highlights some of the major substantive themes and orientations of that research; and (3) it assumes and advocates a philosophy of research generally and in the disaster area specifically.

Any "outsider" approaching a strand of research that has had long continuity and has produced many studies and publications over a long period of time will see patterns that may not be readily apparent to those directly participating, but will, in turn, misperceive matters as they are "known" to those who were actually involved. Thus, Kreps correctly notes the persistent and consistent attempt of the Disaster Research Center (DRC) at Ohio State University to maintain a social organizational level of description and analysis in contrast to the pre-DRC dominance of a social psychological orientation. On the other hand, there is a tendency on his part to overstate the importance of DRC research objectives as a factor in the historical development of DRC and to overlook how much was guided and directed by non-research factors stemming from DRC's very complex relationships with the sociology department and the base college of which it is a part, as well as extrauniversity circumstances.

My last two sentences, of course, merely point to what is commonplace in the sociology of science and knowledge; namely, that research is social behavior and researchers are social beings, and as such are subject to all the social factors which affect group and personal behavior. In that larger context, such matters as scientific goals and substantive research questions are but one

possible set of conditions influencing behavior and not necessarily of major importance at all times.

Thus, on all three matters --- the history, the substantive orientation and the philosophy of research involved --- which Kreps discussed, the picture he presents especially of DRC, but also of the early National Academy of Science (NAS) disaster work is, from my perspective as an "insider" in both activities, partially idealized. At least it is idealized in the following sense. The discussion is partly presented from a perspective of what might have happened if the whole undertaking had followed the current ideal norms of scientific enterprise carried out by persons acting solely in terms of their professional roles as social scientists. Along some lines, the paper by Kreps reminds me of the second chapter or the methodology section of most dissertations. As is well known, dissertation writers usually describe their research as if it proceeded in an orderly and organized linear fashion, and as if the only matter influencing what was done were the research goals and the data obtained. As anyone in academia knows, the methodology described very seldom notes the dead ends, false starts, meanderings, mistakes, etc., and most of what was actually done. Even more rarely alluded to are extraresearch factors such as personal problems, interpersonal relationships, group pressures, institutional constraints and all the other nonresearch elements which entered into what was and was not done in the data-gathering and analysis, as well as the writing of the dissertation. There is, therefore, a very strong mythological tone to most methodological accounts. The presentation by Kreps of the development of the NAS and the DRC work is nowhere near that extreme, but it does suggest a

far more exclusively professional orientation and a considerably more organized enterprise than was actually the case, and it only hints at some of the crucial extraresearch factors which were usually operative.

There are, of course, good reasons for the limits typically involved in examinations of an overall research effort. For one, it is often necessary to be tactful in any discussion. Kreps was constrained by both self- and other imposed discretions. Many of the involved parties, both individuals and organizations, are not only still around but active participants in current disaster research. Thus, I would find it difficult at this time, for example, to be completely candid in reporting the significantly varying relationships, especially of an informal nature, that prevailed between DRC and about ten different research-sponsoring agencies. In most cases, there was mutual respect and correct interaction although there were exceptions. At one extreme there was almost complete sponsor indifference to anything that was done; at the other end, conscious sponsor efforts to manipulate field data and distort research findings.

Along other lines, obtaining a fully accurate picture of what went on would have required intensive research totally beyond the mandate and resources that Kreps was given. I have been deeply involved in the birth of socio-behavioral disaster research in this country; however, I would have to ponder considerably, refresh my memory, and look up fugitive documents to make certain I was completely and correctly recalling both my own past activities and the early history of the area. In fact, in preparing for this presentation, long-forgotten details resurfaced about the development

of particular studies and the operative circumstances at the time when some data were obtained and analyzed. In one of the smaller scale DRC studies, I particularly recalled how scientific norms and values were probably the least important factors at play in the situation.

We also cannot ignore the fact that there is a strong tradition among methodologists, in sociology at least, to attempt to deal with research issues as if they are solely technical matters in a social void divorced from the realities of political and economic limitations and possibilities. To a degree, that kind of orientation to "means" rather than "context" has rubbed off in the Kreps' account. For example, the federal civil defense agency, under a changing set of names, was the major sponsor of disaster research in the United States in the 1950s and the 1960s. Its overall policies and internal politics did influence what types of research would be done and what kinds of research reports would be produced by disaster researchers. The agency and personnel involved were among those who did maintain a generally correct "hands off" attitude towards research and researchers, but they had a major interest in only certain kinds of matters (e.g., the immediate post-disaster period rather than the long-run recovery) and were disinterested or actively avoided other kinds of topics (e.g., disaster mitigation planning, or in what ways political considerations influenced federal level decision making with respect to responding to state and local inquiries and requests at times of mass emergencies). Any complete and balanced assessment of the DRC research thrust would have to take seriously into account the influence which sponsors had upon the substantive kinds of questions addressed, the data

gathering techniques which could be used and the kinds of data analyses and reports which would become public.

Let me turn now specifically to the history, substantive orientation and philosophy of research discussed in the paper by Kreps. As a whole, I think the paper is rather good. I do not have any major disagreements with most of what it states. It is more what it does not cover or examine that I want to address. I will note in my remarks some changes in emphases, certain qualifications and the filling in of factual and other gaps which, in my opinion, are necessary corrective and balancing additions to the paper. Also, while I will make some passing references to the NAS work, my focus will primarily be on matters pertinent to the history of DRC and the studies it undertook.

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History

In regard to the selective history of disaster research presented by Kreps, I want to clarify some matters with respect to DRC. I want to note DRC's peculiar organizational nature which has existed since its establishment, some direct and indirect links in the past between DRC and NAS, and the two major phases in the historical development of DRC.

From the paper by Kreps, one might reach the conclusion that DRC is an organization that was deliberately set up to study disasters. That is not the case at all. What happened in historical terms was the following. Two rather different research projects on disasters were initiated at Ohio State University. The projects involved three faculty members and a number of graduate students. In addition, there was a university plan to physically centralize certain research activities. The label of a center was advanced to

cover the multiple but interrelated faculty and student work, the physical location of the work and the need for both the researchers and other university elements to have a single name for the collective activities. In other words, the establishment of a disaster research center at Ohio State University followed ongoing work: sociological structure came after function.

Perhaps even more importantly, a center was never established in the sense of an organization with full-time personnel and resources of its own, including a general budget. DRC does not exist in any formal sense at either departmental, college or university level. Up to this day, almost all people working at DRC have been part-time personnel. Infrequently, there have been full-time staff members, but usually only for a year or two duration. Even the directorship, which has never been a formal university position, has always been held by faculty members with major and concurrent responsibilities for teaching and service activities in the sociology department. Funding for the Center has come almost exclusively from external grants and contracts; there have not been and are no general fundings or direct university financial support for Center activities. The specialized library operation and the DRC publication program, as well as the personnel undertaking such activities, have been funded by "bits and pieces" from budgets for specific research projects. Thus, in many ways, DRC has simply been a label covering sequential and concurrent multiple research projects on specific topics. There has never been a center with a formal structure, a regular staff of specialists or resources for general center activities.

I emphasize all of this to point out that at no time did some people get together and say "Let's create an organization to carry out disaster research". It just did not happen that way. Similarly, there is not to this day a formal group with a preplanned program of disaster research. The DRC label obscures the highly informal and unstructured nature of the whole enterprise. Statements in the paper by Kreps that DRC did this or that should be read with the understanding that the corporate existence implied by the name is more nominal than real.

The paper also seems to indicate that there are two separate traditions in the early development of socio-behavioral disaster research in this country: namely, the DRC tradition and the NAS tradition. There is an element of truth to this statement: there were two traditional paths of study. However, the two paths have always been interrelated and share a common origin. The fountainhead of disaster research in the social area in this country was the work done in 1950-1954 at the National Opinion Research Center (NORC) at the University of Chicago. Charles Fritz was the work supervisor, and I was one of the staff members of the NORC disaster research project. Together we learned basics about disasters within the same context. Fritz went on to become a very major figure in the early NAS disaster studies. I eventually helped found DRC in 1963. Due to our chosen career paths, we kept in contact in a variety of ways. Most importantly, I served on a number of NAS committees dealing with civil defense and mass emergencies of different kinds in which Fritz had a leading role. Several times, I acted as an NAS consultant on disaster matters. Thus, while most of the NAS work preceded DRC, both had a common origin by way of our experience



at NORC, as well as the contact provided by the continuing professional interaction between Fritz and me.

Since NORC was the breeding ground for most disaster research in the 1950s and the 1960s, its importance in establishing the research area in this country cannot be overestimated. It was the NORC experiences of Fritz and myself which influenced us in our later work in the area, for example, to emphasize field research and to maintain an openness and flexibility in field designs. From a substantive viewpoint, the NORC experience led us to focus on groups rather than individual victims as the important unit of disaster study. Anyone at NORC also learned the basic principle that many of the most centrally held beliefs about disasters by planners, operational responders and even researchers were mostly mythological. For better or for worse, the NORC experience provided both the methodological and theoretical guidance for most socio-behavioral disaster research in the United States for several decades.

A third point which never occurred to me until reading Kreps' paper concerns the implication that DRC has had substantive and methodological continuity in its research activities. But in looking back at the history of DRC, a case can be made that there has not been as total a continuity as might be thought. It is possible to see a pre-1970 phase and a post-1970 phase to the work at DRC. In his paper, Kreps is primarily talking about the pre-1970 work of DRC. He does not take the post-1970 work very much into account.

What is the difference between the two time periods? Up to about 1969 and trailing off into 1970, DRC's research interests were almost totally restricted to natural disasters and the emergency-time period. But from about 1969 to about 1973, DRC,

although operative, was not doing much in the disaster area. This was not a deliberate withdrawal from the area but rather a consequence of funding changes. For about five years, DRC, through a major grant from NIMH, primarily looked at a non-disaster category of mass emergencies, namely, civil disturbances. At that time, we partly rationalized this shift in focus as being a consequence of our interest in studying another kind of community mass emergency situation other than natural disasters. Whatever the reason, DRC was away from field studies of disasters for a relatively long time. I was curious enough myself about this matter to go back and check the records. I found, for example, that in the three years from 1970 to 1972, DRC conducted only six field studies in the disaster area. In contrast, DRC undertook 55 different field studies in the civil disturbance area. My feeling that DRC changed its work focus for about five years is confirmed by the record. It was not until about 1973 that the Center returned to working in the disaster area. In some respects, even this refocus on disasters has been different since we have extended our field work to technological disasters. Emphasis upon emergency-time period is no longer as dominant as it was in the pre-1970 era and different field procedures have been instituted.

At any rate, the point is that the DRC research tradition, whether in terms of research focus or methodological orientation, is not all of one piece when looked at through time. By focusing upon the pre-1970 period of DRC, Kreps gives little consideration to the changes of the post-1970 period. If the full time period were examined, the description and analytical evaluation might be somewhat different. At the very least, the different post-1970

DRC research activities would have to be taken into account.

### Substantive Orientation

Let me turn now to the substantive orientation of the DRC work. There tends to be two somewhat different positions as to what determines or at least should determine the focus of a research effort. In some scientific circles, there is the orthodox point of view that research ought to be guided by theoretical schemes or models. This is also sometimes voiced in the disaster area, although it has never been clear to me why this particular area should reflect such an orientation when at least some sociological research deviates sharply most of the time from such a stance.

The other point of view is that policy questions ought to dictate research efforts. This is a frequently asserted position in the disaster area. It has been reflected in some of the discussions that have gone on at this conference. Sometimes, the focus is upon the ways in which policy questions should structure research: at other times, emphasis is upon the ways in which research ought to feed back into policy issues. In the latter case, the implication is that feedback will be better if policy questions are taken into account when research starts. While there are some major differences between starting with policy or ending with policy concerns, both views assume that a direct connection exists between policy matters and research activities.

Undoubtedly there is some connection between theory and research and between policy and research. However, I believe to view the matter in such a way is rather unrealistic. My basic position

is that a great deal of what happens in a research area is dictated by extraresearch considerations. Put in other words, the substantive questions addressed in many studies stem from other than practical policy questions or theoretical positions. Likewise, what is produced may have few or no direct policy or theoretical consequences. This is as true in the disaster area as any other field of study.

On the basis of my experiences at DRC, I would say that what is examined in studies, the research findings that are produced and what is done with the work undertaken is influenced by at least three factors.

One is what I would call organizational needs and constraints. This applies both to research sponsors and researchers. There are many reasons why sponsors fund disaster research. To obtain research findings on policy implications may be a reason, but in my experience this is very seldom a major consideration. (I do not say research does not have policy implications, for it always does, but rather that this is seldom a major reason for research sponsorship.) Sometimes research is funded because it is seen as contributing to agency or group survival; in some cases, it involves an attempt to win intra- or extraorganizational legitimacy or prestige. In still other cases, research is supported because it is believed to be consistent with already prevailing organizational policies. There are even occasions when funds are given for studies because some administrator is genuinely interested in learning something that may be useful for his or her operation! My point, of course, is that multiple factors are operative, all legitimate in their own way. What research topics will be examined

are affected by these factors. To pretend, however, that most research is directly supported because of policy questions or implications is, in my view, rather naive.

I do not know what has been behind the rather substantial disaster research support DRC has enjoyed for over fifteen years. Others, such as Jim Kerr, can better explain the intent and reasons behind their research support of the Center. I would be surprised, however, if our research funding as a whole was not generated by all the factors I mentioned, as well as others that anyone knowledgeable of governmental bureaucracies can easily visualize.

The situation is somewhat the same on the other side, that of the researcher. Research groups also have their own varying organizational needs and constraints which will influence their substantive questions and methodological usages. Let me use two examples to illustrate this point.

DRC, in terms of its unofficial structural position in the university, was and is informally a part of the sociology department. Very early in its existence, an implicit decision was made that we were going to use DRC to give sociology graduate students training in large-scale field research involving intensive interviewing and participant observation. The Center, in other words, was to be used for graduate student training. Thus, from the very beginning, DRC was committed to using primarily part-time staff members without specialized skills who would be available only for relatively short periods of time before they left upon graduation. DRC, therefore, never attempted to move in the direction of having career-oriented, full-time staff.

I suggest that with non-career, non-specialized and part-time personnel, there are major implications for the kinds of research which can or cannot be conducted. For instance, since DRC was part of the sociology department and training sociology students, it was all but impossible for us to undertake interdisciplinary research. Structurally, in other words, it was very difficult for DRC to have on its staff either graduate students or faculty members from departments other than sociology. Even if we had wanted to, we were effectively barred from conducting almost any studies outside the realm of general sociology.

To play my main theme again, research is never just research--it is social behavior in a social context with all that implies. In a sense we all know this, but it might be wise to keep the point constantly in mind. Research values and the scientific subculture are only part of what is involved. Any assessment of the substantive or methodological focus of a research effort will be more sophisticated and closer to reality of the situation if it assumes that there are always many extraresearch factors operative. Certainly, the organizational needs and constraints of both sponsors and researchers that I have alluded to and simply illustrated influenced what DRC substantively dealt with in its studies. By now, the sociology of knowledge and science should have taught the importance of non-research factors, and we would be better off expressing the ideology of science less and more readily applying its observations to the very act of scientific research itself.

Another factor that affects research are the theoretical and methodological predilections of the people leading the studies undertaken. I doubt anyone would challenge such a statement.

Certainly, such predilections have been operative in the disaster area and are manifested in both the topics examined and the way research was undertaken at DRC.

However, there can be and often are two rather different positions on this matter. There are those, and we have had them through the ages, who come along and claim that only their methodology or their theory is the acceptable one. Thus, only ethnomethodology or structural functional analysis, for example, is deemed worthy of use. I think people making such claims have a problem, but it is not my function here to deal with professional or scientific delusions. The other position, of course, is that, given the current status of social and behavioral sciences, there can be honest differences of opinion as to appropriate theories and methodologies. Thus, it is seen as not possible at this point in time to rule out the possible value of different theories and methods currently advocated. We find both positions--the only-one approach and the openness-to-all approaches--expressed in the disaster area.

DRC has always strongly adhered to the last position. We have never advocated only a particular theory or a specific methodology. We have never assumed, for example, that only symbolic interactionism or quantitative surveys were the only or even the major ways in which the disaster area necessarily had to be approached. In fact, we have gone further and have assumed that there are not only different ways, but that using different theories and methodologies will lead to substantively different findings and results.

For example, some researchers have a great deal of confidence in attitude surveys. This is fine, except in terms of my biases;

I do not see much connection between attitudes and behaviors. I came out of the University of Chicago trained as a social psychologist, but one of the reasons I became disillusioned with social psychology was because of its obsession with the study of attitudes and opinions. I could not see that much of what I was doing, including my disaster work, indicated much of a meaningful relationship between attitudes and behaviors. Therefore, for example, I became much more interested in looking at behavioral activities rather than verbal utterances.

This brings to mind a remark made at this conference yesterday regarding a supposed lack of changing attitudes with respect to disaster planning in this country. Now I do not deny survey studies may find little changes in such attitudes. On the other hand, I am sure that if one did a pre-1970 and a post-1970 study at the community level and used behavioral measures, one would find changes in disaster preparedness. DRC did such a study a few years ago looking at 12 communities around the country. We found rather important changes in disaster preparedness planning in the last decade or so. There are, for instance, far more emergency operating centers (EOCs) after 1970 than there were prior to that date. There are far more written disaster plans at the community level than ever before. There have been considerably more risk assessment analyses made in recent years compared with earlier. Studies of behavioral measures show differences that attitudinal surveys do not, along some lines at least.

What I am trying to point out is that you can go out and conduct studies of attitudes and opinions, and you will get certain results. There is no doubt of that. But you can also go out and



do a behavioral assessment and come out with a different set of findings, as in the example just cited above. The important point here is that if you conduct studies in different ways, with different theoretical assumptions and with different methods, the end result will be different substantive questions and findings. This has always been a basic assumption at DRC, and our work should be read with that in mind.

Let me pursue this point a little further. Recently at DRC, we have been doing content analyses of community disaster plans. We have found that in the last few years, insofar as disaster preparedness planning is concerned, two new components are consistently being added to plans. One has to do with providing disaster-related mental health services, and the other has to do with handling chemical hazards. Ugo Morelli said yesterday that something was going on in Boston, and he was worried about the fact that there did not seem to be a correspondence between his--if you will--behavioral observations and what was reported in the Wright/Rossi studies. I do not think there is any incompatibility here. I really would not question that the Wright/Rossi study found little attitudinal support of disaster planning, etc. On the other hand, I would say that if some other researcher did a study in a different fashion, rather different kinds of substantive results might be found. In terms of the particular substantive question being used as an example here, it may very well be true that official attitudes have not been found to be supportive of community disaster planning. But it is also probable that observable changes can be found to be going on with respect to such planning, the kind of thing Morelli observed. The issue from my point of view is how the study

to obtain findings was conducted--in one case you will get one kind of conclusion, in another case you will get other conclusions with respect to what is going on in regard to community disaster planning. The seeming contradiction does not stem from the world of action, but results from the theoretical and methodological assumptions made by the social and behavioral scientists doing studies.

There is much of this in the disaster area and its substantive questions and its research findings. Different findings are often simply reflecting the theoretical and methodological predilections of the people involved. One can assume that one's own methodology is the only way of gathering data or that one's own theory is the only way of analyzing data, and anything else is of little or no value. My own view, and it is reflected in the substantive DRC findings, is that if we use different theories and methods, we are going to come up with both different questions and findings, all with value in their own way.

I think also that in discussing substantive conclusions, we sometimes ignore the fact that both researchers and research organizations almost always have different audiences for their products. All of us have multiple audiences for what we report. Sometimes we address one, sometimes another, and what we say or write will differ as we change our targets. Usually changes in presentations are made because we assume there is variation in the knowledge of our audiences. Care, therefore, must be taken in the disaster area in assessing statements made for different audiences, very few of which are research oriented.

Let me put this in very concrete terms, and it has reference to something that was said yesterday. Any experienced disaster researcher--experienced in the sense of knowing what is in the research literature since the 1950s--knows that disaster experience per se is not crucial or very important in itself in changing behavioral perceptions about disasters or initiating post-disaster organizational changes. The early 1950-1954 NORC studies reached that conclusion regarding disaster victims. I remember sitting around NORC, trying to relate disaster experiences to something or other, and concluding there was no such connection. In 1968, DRC conducted a study of disaster-related organizational change or learning. We reached a similar conclusion about experience and organizational change. This followed a Bill Anderson study in 1966 of organizational learning after the Alaskan earthquake which also found experience evoked little change. In the mid-1970s, DRC did a 12-city study about changes in organizations at different time periods up to five years after disasters. The same conclusion was reached again--disaster experience in itself is not a very important factor in change. Similarly, the Wright/Rossi study found that disaster experience was not very crucial at the community level. I am glad they did the study, and it was worth doing, for they established at the community level what was previously known to be true at the individual and organizational levels. Experience in disaster by itself does not appear to have too many direct consequences for changes from predisaster states. It is interesting to note that this contrasts with studies, including one conducted by Gary Kreps, which showed that civil disturbance experience, when compared with disaster experience, did

result in some kinds of organizational changes.

Disaster experiences, by themselves, do not lead to changes. Is this a counterintuitive finding as someone said yesterday of the Wright/Rossi study? Certainly it is not counterintuitive among disaster researchers. It may, however, be counterintuitive elsewhere, given the knowledge of other audiences to whom the lack of importance of experience is presented for the first time. In other words, you have to assess statements about disaster experience in terms of the knowledge of the audiences to whom the statement is addressed. Disaster researchers are not going to be very impressed with such a finding, but for other kinds of audiences, this may be a revelation of the first order and worthwhile communicating to them. It depends on whom you are talking to and what they already know.

The ways in which findings are reported are also influenced by different audiences. Let me use myself as an example. When I talk to different people about research findings in the disaster area, I proceed as I do at the university when teaching students. I do not say exactly the same things to first year students as I do to graduate students. It is not that I tell them different things, but I couch statements in different ways, assuming differential knowledge at different levels. The same is true with respect to my presenting findings from the disaster area. For example, I have been stated as much as anyone else that panic and looting do not occur and are not problems in disasters. You can take any number of my statements, both oral and written, and see that I say panic and looting are nonexistent disaster problems. If the context in which these statements are made is examined, it

will be found that disaster-inexperienced audiences were primarily being addressed. The empirically truer statement that "panic and/or looting occurs only in very limited kinds of circumstances" will mostly be found in my comments directed to audiences of experienced disaster researchers.

My point is that in trying to work against a stereotype of panic and looting, I frequently make the flat statement that panic and looting do not occur in disasters. From my view, this is the best way of challenging existing beliefs and getting certain kinds of audiences to think differently. With more knowledgeable audiences, my statements will be more qualified, more truly reflecting the empirical data. Under certain circumstances, I oversimplify. In other instances, I communicate a little differently. It is not that I say different things, but I essentially use a different strategy to communicate to audiences with different knowledge.

When looking at the DRC production of several hundred books, articles and talks, it is important to keep in mind they are not aimed at only one kind of audience, certainly not just a research-oriented audience. We have always assumed that the work of the Center was of interest to multiple audiences. Writings and speeches address different levels and are couched indifferently depending on whom we are trying to communicate with in the given instance. To take the corpus of DRC work as represented in its literature as a consistent whole assumes a homogeneity which is not there and overlooks products which are deliberately heterogeneous. What we have produced for operational personnel, for instance, is often rather different from what we have prepared specifically for, say, sociologists. This, by the way, is not said as an apology but

actually as a recommended strategy for academicians. Too often, researchers in particular write only for others like themselves and then wonder why they do not seem to be communicating to the rest of the world. A slight oversimplification in an initial communication is not too high a price to pay in order to reach people who can actually do something with your findings which otherwise might be totally ignored. At any rate, I suggest DRC is a classic case of an organization addressing multiple audiences, ranging from citizens at-large to technical specialists.

Kreps in his paper was applying certain evaluative standards to the DRC work with which I have absolutely no disagreement if we assume particular kinds of audiences. But the standards cannot be applied across the board, for there have been different audiences with which DRC has tried to communicate from its very beginning. I think, by the way, the disaster literature as a whole should be treated in the same differentiated way. I have, at times, had the impression we keep trying to treat as one consistent piece what is really a set of deliberately disparate elements. Something written to convince a research sponsor that they are concerned with the wrong question in the first place needs to be assessed differently from an article attempting to show how a sociological specialty area ought to be restructured on the basis of empirical findings in the disaster area.

Someone around this conference said that recommendations should be based on hard data. That is a fashionable cliché, but a questionable assertion, for it depends both on what is being recommended and to whom the recommendation is being directed. At times, disaster researchers may try to have policy makers in the disaster

area adopt a different perspective about certain aspects of disasters. In other instances, disaster researchers may want to raise questions about preparedness planning in the minds of disaster planners. To suggest a different perspective, to raise questions and to indicate alternatives which researchers may recommend need not at all require hard data. Some communications about some empirical data should involve hard data. But I would suggest those kinds of recommendations are the least important a researcher can make. There is also the danger that such a posture will result in the researcher acting merely as a technician manipulating empirical data instead of a scientist doing research. As the very name of research indicates, a scientist should be searching again for something different, be it a perspective, question, etc.

I want to conclude my remarks on substantive matters by touching on two specific points mentioned by Kreps.

In my judgment, contrary to what he says, I think DRC has been far more successful in producing a realistic conception of pre- and postemergency disaster environments than it has in getting people to develop a disaster perspective. If Kreps is right in that we have communicated a disaster perspective, I would be most happy since that has been one of my personal, professional goals. But I really do not sense we have accomplished anything like that. My impression is that we have been far more successful in continuing to clarify misconceptions and undermining myths about disaster behavior. The DRC mental health studies on both providers and receivers of services illustrate the point well. They have generated epidemiological studies to refute our assertions about myths of mental illness but have not led to changes in mental health

delivery service systems.

My second point has to do with the statement by Kreps that DRC has attempted to develop typologies of disaster responses, as well as a sociological theory of disaster behavior. He may be giving us more credit than we should have, for what we have accomplished on these two matters has been far short of our aim. We have struggled with different kinds of typologies, but the end results have not been that satisfactory or useful. In fact, a very important typological question was approached in the early days of DRC and then, unfortunately, dropped. This was the question of what type of mass emergency or crisis a disaster was and the different subtypes in which a disaster could manifest itself. The failure to address the conceptual problem of what is a disaster and the various subtypes into which it can be subdivided is, in my view, a major weakness of the whole disaster field. We have handicapped ourselves tremendously by blithely ignoring the nature of the very focus of the field.

DRC has also not been very successful in developing a sociological theory of disaster behavior. Kreps is right in that we have tried to operate at the sociological level, and we particularly have attempted to relate work in the disaster area to collective behavior theory and organizational theory in sociology. While the end results have been far more accepted and used than our attempts to develop typologies, we are still far from anything which could be fairly characterized as a sociological theory of disaster response. Most of what we have done is to move in that direction and to insist on the sociological perspective, but Kreps is on-target with his statement that we have not yet produced



a sociological theory of the phenomena. In fact, at least outside sociology, we have not yet adequately communicated the sociological perspective on disasters; that is, the notion of the inter-relatedness of social phenomena, the latent nature of much social activity, the notion of unforeseen consequences, the idea of social emergence, etc.

### Philosophy of Research

In taking up some philosophical issues with respect to the DRC research, I want to talk about three things. None of them are explicitly discussed in the paper by Kreps. I think at least two of them are implicit in his remarks, but whether this is the case or not, they are matters raised elsewhere by others who have evaluated, usually implicitly, the DRC work.

One issue has to do with the generally qualitative research work of DRC. In actual fact, DRC has undertaken far more quantitative work than is realized, including two of the largest disaster population field surveys ever conducted. Our survey in the Xenia tornado involving a strictly random sample of 600 households, obtained more in-depth data from face-to-face interviews averaging several hours than all but a small handful of other field studies ever done by anyone in the disaster area. The vast majority of the several dozen Ph.D. dissertations using DRC data are mostly quantitative studies. In its early days, the Center actually conducted highly controlled laboratory experiments, including the fine work produced by Tom Drabek.

Perhaps others' lack of knowledge of DRC's quantitative data stems from the fact that much of this data has never been put into

reports for general public circulation. In part, this is because a number of the analyses undertaken simply confirmed what was already generally known. For example, DRC examined in two major random sample population surveys the extent and nature of looting behavior in the Wilkes-Barre flood and in the Xenia tornado. The detailed statistical analyses reinforced earlier impressions because it found that, in general, looting was a very minor problem at best in these two American disasters. Since I am not particularly impressed by numbers, I have not thought it especially necessary to add this quantified confirmation of what I and others have long stated is among the "myths" of disaster behavior. But our failure to publicize such data and findings undoubtedly has led to the non-quantitative image of DRC which prevails among many in the disaster area.

Nevertheless, despite general misperceptions of the range of the DRC data-gathering activities, it is true that our work is more qualitative than quantitative. This does reflect certain philosophical assumptions which are given expression in the particular methodological techniques we have used and in the theoretical questions we have asked. Thus, some of what we have examined and found simply reflects certain methodological biases. The processes of the development of emergent groups, a prime focus of DRC research, for example, have to be studied in a non-random, non-survey fashion. Similarly, my own interest in panic behavior in stress situations reflects my assumption that such a phenomenon is very worthy of study because it represents an extreme case of human behavior and not because it is a real practical policy issue or a crucial question in any current dominant sociological or social

psychological theory. In fact, none of the standard theories either are interested in or account for panic phenomena. What has been and is being studied at DRC and how it is studied undoubtedly expressed intellectual preferences of what is specifically worthy to study in particular ways. What others may study in different ways, in my view, simply reflects different intellectual biases and preferences. Given the current disarray and controversies regarding all theoretical and methodological issues in the social sciences and particularly sociology, I am not at all impressed with claims for or arguments that only one research path in the disaster area is the ideal one, or that there is a prototype all researchers should follow now or in the near future.

However, to get back to the main point, to assume only one path or multiple paths does make a difference in what is studied and the acceptability of research findings. The substantive results of the DRC studies in the disaster area in the 1960s followed from the general multiple path orientation of the Center and its particular theoretical and methodological biases and preferences. As such, evaluation of the work will differ depending on what criteria of judgment are used, those involved in the orientation of the Center or those involved in a different orientation.

Anselm Strauss and others have for some time been pointing out that qualitative research comes out poorly when judged according to the criteria of quantitative research, but if criteria of qualitative work are used to assess quantitative research, the latter comes out rather badly in terms of its scientific worth. Without fully accepting the conclusion reached, I do think the point raised is not totally irrelevant along some lines when

looking at the DRC work. Questions which may sometimes be raised about the non-randomness of samples, the absence of frequency distributions, the non-quantitative indicators, etc. in much of the Center's work simply miss the mark. In the qualitative kinds of studies which we were interested in doing, other matters of more scientific worth and value were being taken into account in the questions we posed and the ways in which we obtained data. Judged by the standards of qualitative work, this DRC work is not without some merit. However, it is understandable that some critics of the DRC work do not understand this since even some of the graduate students who worked for the Center were so brainwashed by the methodological ideology dominant in the 1950s and 1960s in sociology methodology that they also fell into the snare of thinking that quantitative criteria were meaningfully applied to qualitative work. As the orthodoxy of that approach has waned in the last decade, it is noticeable that the post-1970 students at the Center seem less concerned than the earlier ones about adhering to a dogma that there is only one scientifically acceptable kind of data. Most seem to agree with the economist Kenneth Boulding who said, "Perhaps the greatest superstition in the world today is numerology--the belief that somehow numerical information is always superior to qualitative, structural and topological information. The plain truth is that numbers, for the most part, are a figment of the human imagination" (from "In Praise of Inefficiency," AGB Reports, January-February 1978).

Somewhat more briefly, let me touch on two other philosophical points about scientific research, especially as they are pertinent to the DRC studies. There is a substantial controversy in

sociology about whether research should give priority to theory and hypothesis development or to theory and hypothesis testing. This is no place to explain or detail these two views, but they are reflected in the research done in the disaster area. The DRC work generally stands with those who argue that it is premature to attempt to test theories and hypotheses when we often do not even have the most simple of descriptive knowledge of much disaster phenomena. In-depth studies, in my view, are more meaningful when there exists a comprehensive general overview of the generic phenomena being examined. The DRC work has, therefore, aimed at breadth rather than in-depth coverage of the disaster area.

This is related to another point. It again relates to differences of opinion on whether a field of research should concentrate on particular topics or keep developing new topics. Our general position was that in an unexplored area such as disaster research, it was far more important that new questions be raised, that new topics be explored and that generally a socio-behavioral perspective be brought to bear on as many aspects of disaster phenomena as was possible. In fact, given that for some years DRC stood almost alone on conducting socio-behavioral research in this country, we felt a professional responsibility to try to keep disaster research ever expanding into new topics and questions. Among the disaster topics which DRC has opened up for study are the following: the handling of the dead; the role of financial institutions in disasters; the delivery of emergency medical services in mass casualty-producing situations; the operation of the mass media in community crises; the role of religion and religious institutions in times of disasters; the provision

of mental health services in mass emergencies; legal problems in community emergencies; the diffusion of knowledge about disaster planning among emergency organizations; socio-behavioral preparations for and responses to acute chemical hazards; similarities and differences between civil disturbances and disasters; post-impact community conflict and cooperation; cross-cultural differences in national-level responses to catastrophes; and the operation of EOCs, hospitals, departments of public works, schools and the military in disasters. It has been a definite policy of DRC to move continually into new questions and issues, leaving the more detailed and in-depth work for other researchers to undertake. As a not unimportant aside, I might say that I find such exploratory and pioneering work far more challenging and interesting than studies of nonvirgin disaster matters and issues. It was exciting to learn in our field studies of warning during the Palm Sunday tornadoes of 1965 that failure in interorganizational communications was the crucial factor involved in the transmission of warnings rather than failure of the population at-large to heed the warnings. Maybe the University of Minnesota people presently focusing in great depth and detail on the interorganizational links in the warning chain are having the same kind of fun we had in doing our work, but I doubt it!

Let me conclude my remarks with a more general observation. In another setting and for other purposes, I recently wrote that research in the disaster area was geometrically rather than arithmetically increasing. I cited that as an indication of the impressive quantitative growth of the area. The efforts at this conference to assess earlier disaster research is even a better

sign. It indicates that the field has developed quantitatively enough so that systematical attention can now be turned to a qualitative assessment of what it is producing. If my reading of this is correct, this conference may have been a more significant meeting than was planned or envisioned.