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DISASTERS AND EMERGENCY MEDICAL CARE: METHODS, THEORIES AND A RESEARCH AGENDA*

Geoffrey Gibson

Johns Hopkins University and Medical Institutions, Baltimore, Maryland

INTRODUCTION

The quest for relevance has at last reached disaster research and, with its promise of Faustian compacts, threatens to overwhelm the scholarly attraction of examining a phenomenon for its own sake. There appears, for example, to be a pernicious but nevertheless widespread notion about that the behavior of emergency medical services (EMS) during disasters represents a valuable set of educative insights and evaluative indicators as to how EMS behave routinely in non-disaster situations. The argument has at first glance a spurious air of intuitive reasonableness: since medical care demands generated by a disaster are a qualitatively identical but quantitatively more intense version of routine demand for emergency medical care, the disaster response of hospitals, ambulances and public safety agencies may be taken as a useful, generalizable and valid test of their likely non-disaster response. From this basic tenet flow several secondary but prevalent myths:

– modifying an EMS system to create a more effective disaster response will necessarily and unaided also result in a more effective non-disaster response;

– disaster responses are sufficient tests of routine EMS so that day-to-day monitoring is unnecessary;

– inter- and intra-organizational relationships involving the evacuation and referral of patients, the dispatch of ambulances and general resource allocation are identical in disaster and non-disaster situations to the extent that either is predictive of the other and an organizational commitment to a specific policy change for a disaster may be taken to infer willingness to behave similarly in routine daily EMS operations.

One of the excellent contributions of the papers from the Disaster Research Center (DRC) at the Ohio State University in this issue is to carefully document and disabuse us all of the notion that the disaster response of EMS is identical or even similar to routine EMS behavior. Tierney and Taylor state:

> ... it is commonly argued by EMS professionals on all levels that everyday and disaster EMS are simply two points on the same continuum. DRC research indicates that this is not the case. Mass emergencies create demands that differ qualitatively and quantitatively, from everyday EMS demands (this issue, p. 153).

As examples are cited the convergence of press, relatives and medical personnel on hospitals in disasters and the greater importance attached

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to speed and less to overall coordination in routine EMS as compared with disaster EMS. Worth and Stroup comment that "... [although] disasters are often regarded as an extension of day-to-day EMS as a more-of-everything situation, [in fact].... disasters present circumstances which are different in more complex ways than sheer numbers of casualties" (1977: p. 168). Two important factors might be added to this list. First, the program guidelines of the Department of Health, Education and Welfare's (DHEW) Division of Emergency Medical Services explicitly state that the local EMS system is not the disaster organization. Thus, the Division requires that:

> The EMS system must have a plan to assure that the system will be capable of providing emergency medical services in the system's service area during mass casualties, national disasters or national emergencies. The EMS system is not the regional health disaster organization. It is the emergency medical organization that will work with other agencies during a disaster to provide emergency medical care. The EMS system must be linked to the local, regional and state disaster plans and participate in exercises to test disaster plans (DHEW, 1973).

As further evidence of the distinction that DHEW sees between routine EMS and disaster response as well as depressingly, of the mundane quality of Federal thought on disaster evaluation, the following criteria are those used by DHEW in assessing the adequacy of the disaster planning component of an EMS system application for Federal funds. (This component, incidentally and tellingly, receives a magnificent weight of only 100 points out of a maximum of 3000 used to prioritize an entire application.)

> 1. Does the application contain provisions for EMS during a disaster?
> 2. Does a plan exist for systematic delivery of EMS during either a natural or man-made disaster?
> 3. Is the disaster plan an appropriate modification of the EMS system's daily operational procedure?
> 4. Is there assurance that adequate non-telephonic communications will be provided?
> 5. What provision has been made for distribution of the disaster instruction manual to all system elements with periodic drills and adequate evaluation?
> 6. What assurance is there that revision will be accomplished immediately after test or drill evaluations?
> 7. Will disaster plans be tested once during the grant period?" (DHEW, 1973).

The second differentiating factor between disaster EMS and routine EMS has to do with defining the optimal inter-hospital distribution of patients. In most EMS systems, the routine day-to-day major problem is that patients are distributed between too many hospitals regardless of their varying capacities to render quality care while the idealized solution is seen as a regionalizing strategy of concentrating patients at a very few hospitals. In direct contrast, the problem of disaster EMS is that patients are distributed amongst too few hospitals and that the solution to be aimed at should be a more pluralistic evacuation protocol.

This last point serves to emphasize that the notion of identity between disaster EMS and routine EMS is not only a falacious belief but one that could, and perhaps already does, prove dangerous if widely believed and acted on. Thus, many EMS systems have given themselves a false sense of security as to their routine EMS capability, solely on the basis of one or two superficially effective disaster drills. Often systems harbor under an entirely inaccurate set of self-comforting beliefs as to county/city or even fire/police department routine EMS cooperation on the fragile basis of inter-agency and inter-jurisdictional agreement to a disaster plan.

In this light, then, the realization that disaster EMS response has little to tell us about routine EMS response ought not to be a depressing conclusion that belittles the significance of disaster research. Indeed, the realization can be a constructive and enlightening one for both disaster EMS and routine EMS since it removes the limitations of cross-relevance and inter-generalizability. In going their separate ways, newly liberated from each other, each may be pursued for its own sake and for what each tells us about itself. The papers in this volume are a significant step in validating

disaster EMS as a scholarly area of conceptual and programmatic significance, and in delineating the remaining tasks to be resolved before this body of knowledge can continue its substantial forward progress. The rest of this paper will be concerned with these tasks which may be listed as follows:

1. What are the most appropriate methodologies to collect data on disaster EMS?
2. What are the most insightful theories and concepts to be used in organizing data on disaster EMS?
3. What should be the research agenda for disaster EMS in the coming years?

METHODS FOR DISASTER EMS

The central methodologic problem in collecting data on the operation of EMS during disasters is quite obvious: since disasters have an irritating tendency toward unpredictability as to time and place of occurrence, it is unlikely that skilled observers (for whom detached observation is their sole function) will be present during the disaster. For the most part, then, the data are collected retrospectively from a number of sources and are inherently subject to the limitation of all retrospective as opposed to prospective data obtained from subjective as opposed to objective sources. The Disaster Research Center has, of course, been long aware of this problem and has attempted to deal with it. Neff describes the DRC data collection techniques:

> Since its inception in 1963, the Disaster Research Center has employed a qualitative methodological strategy in studying disasters. Such a strategy involves conducting in-depth, semi-structured interviews with representatives of the various agencies responding to the disaster, collecting a wide assortment of documents (disaster plans, operational critiques, and agency information brochures), and participant observation of disaster operations. All of the above data collection techniques have been employed during the course of this present EMS study. Most of the information presented in this paper is derived from approximately 40 hours of tape-recorded interviews with representatives of various police departments, fire departments, ambulance companies, and hospitals. Each of the interviews were examined for information pertinent to.... (this issue, p. 180).

In another paper, Tierney and Taylor indicate the kinds of multiple casualty and disaster situations in which data were collected:

> ... DRC has conducted field studies of 27 mass emergencies in 22 communities, spanning 14 states, the District of Columbia, and the U.S. Virgin Islands and gathered baseline data in a number of other U.S. cities. Field research has consisted of three kinds of studies: baseline or Time One studies used to gain information on EMS disaster planning and normal EMS operations in six disaster-prone U.S. communities; on-the-spot research of five pre-planned events, such as the Mardi Gras in New Orleans and the July 4 Bicentennial celebration in Washington, D.C. and Philadelphia, Pa., where the possibility of excessive casualties was anticipated; and studies of Time Two operations, or the EMS response in 22 mass casualty events... Disaster agents included four tornadoes, two floods, eight transportation accidents, one major fire, and seven explosions and toxic leaks. Often, more than one trip was made to a community by DRC personnel, so that knowledge could be gained concerning EMS operations in pre-, trans-, and post-disaster settings (this issue, p. 151).

In the same paper, Tierney and Taylor also outline the three key characteristics of the DRC research strategy:

> First, a large number of cases, exhibiting a variety of system characteristics were needed... Second, field teams had to be present on the scene as soon as possible... Third, in order to understand system organization and functioning, it was necessary to have contact with key EMS officials and operational personnel in a variety of emergency health care organizations... Observational data, obtained on-site by field personnel, was invaluable as a source of information about the reliability and validity of data obtained by other means as well as information about the reality as opposed to the ideal of EMS delivery in high demand situations (this issue, p. 152).

Against this background of existing DRC methodology in the EMS study, it is necessary to be concerned with the validity of the data thus collected, not so much because DRC methods are deficient but because validity is of primary importance in any newly emergent

area of scholarship.*

VALIDITY OF THE DATA

Are the data being collected an indicative measure of disaster response? The dependent variable of the disaster response of EMS is generally defined in these papers as the presence of such imputed properties as "coordination", "centralization", "effectiveness", forewarning of hospitals, etc. Many of the papers (the most impressive exception being the contribution of Wright) define EMS disaster response in slightly differing ways and several fail to offer a definition, in the sense of a measurable indicator, at all. In a quite extraordinary finding that would have made a less courageous or committed investigator substantially less enamoured of the validity or even importance of their explanatory variables Neff states:

> All available information indicates that no serious medical consequences resulted in any of the four situations we have just described. There is no evidence either directly or indirectly suggesting major negative effects on the quality of the medical attention and treatment that almost all disaster victims receive (this issue, p. 186).

Presumably if a set of descriptive parameters are important enough to be pursued as defining effectiveness of a disaster EMS response, they should not only be theoretically important but also substantively important in being related to vital consequences of an EMS response such as the quality or outcome of medical care rendered. And yet all that Neff's barrel-scraping search can come up with as being related to ineffective responses (defined this time as overlapping jurisdictions) is the ease of visualizing "a variety of complications in medical care and treatment had the nature of the injuries been more serious..." and "... unnecessary strained relations, perceptions that not everyone had acted correctly, and general overall feelings that something was amiss..." (this issue, p. 186).

These varying definitions of the dependent variable of effectiveness of disaster EMS response and the general absence of evidence linking ineffectiveness to such substantively important adverse outcomes as mortality and morbidity suggest prima facie problems of validity. In defining effectiveness of health services, the investigator is faced with three choices: of selecting normative standards that refer to input (the availability of resources), process (the utilization of resources) or outcome (change in health status of the patient target group) criteria (Gibson, 1977). In general, health services research over the last decade has focused more and more on outcome definitions of effectiveness as conceptually and programmatically most important and de-emphasized input and process measures as often bearing an unsubstantiated relationship to outcome. Faced with this, it is difficult to accept both conceptualization and definition of effective disaster EMS response contained in these DRC papers as self-validating and correct merely because it is stated, particularly when the various authors seem to be saying something different in each paper.

There are, of course, several types of validity and of procedures to validate measures, all of which require a degree of independent confirmation explicitly absent from most of the DRC papers. As I have indicated elsewhere:

> Reliability testing tells us only whether measures allegedly indicative of a concept... can be repeated by different observers at different times. Validation testing refers to whether the items did indeed indicate the underlying concept or variable allegedly being measured. Validity is assessed by seeing whether the scale relates closely to other measures commonly accepted as indicative of the same variable, whether the items are compatible with previous conceptualizations in the literature, and whether

*It had been my intention to discuss the reliability of the DRC data at least as reported in the five DRC research papers in this volume. However, since I understand this methodological issue is addressed in other DRC writings elsewhere, I forego any detailed discussion of the matter. However, some recommendations relevant to the reliability of data which might be gathered in any future study of the delivery of EMS in disasters are set forth in my first three recommendations at the end of the article.

the scale predicts (both prospectively and retrospectively) the outcome variables accepted as alternative indicators of the concept being measured (Gibson, 1976).

Wright's is the only paper making a serious attempt at face validation of his concepts/measures of EMS response effectiveness by deductively deriving them from the theoretical models of others. Formal procedures do exist and could be modified for research use. A starting point would be to take those communities exhibiting disaster responses generally regarded by programmatic experts as effective and compare them with communities with disaster responses regarded as ineffective to identify those characteristics which differentiate the two sets of communities. These characteristics would then be submitted to an expert panel so that these characteristics can be screened to eliminate conceptually unreasonable indicators and to differentially weight the remaining items according to whether their presence denotes effectiveness or ineffectiveness. This is, of course, the procedures one goes through in constructing and testing a scale and, in fact, it is impossible to conceive of a more important or immediate methodologic task facing disaster research than establishing by these means a scale of EMS disaster response effectiveness. With such a scale, a remaining and more important convergent validation could be attempted by determining the correlation with other alternative measures of effective response. Although there may be alternative measures other than adverse medical outcomes, it is hard to accept the conceptual and programmatic credibility of a measure that bears no relationship to outcome. The past decade of health services research is littered with the reputations of investigators who believed falsely that effectiveness could be measured without reference to outcome and solely in terms of compliance with theoretical esoterica of the investigator's own formulation without any objective validation. Disaster research has an opportunity to avoid such errors in judgement and should seize it.

Until these validating procedures have been undertaken, disaster researchers ought not to pursue the tautalogic naïveté that ineffective disaster response is denoted only by an absence of coordination and centralization and that such an absence can be assumed rather than shown to be associated with actual adverse consequences. Organizational analysis achieved its eminence by showing that organizational properties were related to and causal of substantively real and important consequences rather than merely of other organizational characteristics. It continues, I suspect, to be a fragile and conditional matter. The ultimate validation is, of course, the ability to accurately predict the nature of an EMS disaster response prospectively and to do so in a variety of settings. Such an activity is infinitely more impressive than the Monday-morning quarterbacking we all engage in by claiming to predict something that has already happened. Despite the nature of disasters, they are sufficiently frequent in occurrence unfortunately, to allow the testing of predictions and DRC could well be spending some time in courageously giving "hostages to fortune" by publishing predictions about the nature of EMS disaster responses in identified communities. If Jeanne Dixon can do as much, ought scholarly data to be as far behind? Until this happens, it will always be difficult to tell whether the data being collected are valid indicators of EMS disaster response rather than some other overlapping, parallel or even independent construct.

THEORIES FOR DISASTER EMS

The major distinguishing characteristic between generalizable higher-order propositions that allow us to predict the unknown future from the known past on the one hand and pedestrian or parochial case-by-case description on the other is the former's firm anchorage in theory or at least within an explicit conceptual framework. Such a framework sensitizes

the investigator to certain concepts, processes and variables and blocks out others and should, therefore, be chosen explicitly and voluntarily rather than assumed by default. Tierney and Taylor identify DRC's theoretical focus as follows:

> DRC researchers focused upon EMS as an open system response. According to this view, all organizations involved in the various phases of patient care in disasters are seen as interfacing and acting in a more or less integrated fashion to deliver services. System imagery was used in this research for several reasons. First, this parallels the approach used by DRC in the study of other aspects of community disaster response for example, the reaction of the group of organizations collectively termed the "political system". Thus, the EMS system was viewed in this research as one of a set of community systems which could become active in the pre-, trans-, and post-disaster settings. Another advantage of the adoption of the concept of system is that its use requires that attention be paid to the environment or context in which a group of organizations operates. EMS delivery in mass emergencies is seen as being affected by both internal system factors and factors external to the system, including other community systems. Finally, the concept "system" was employed because of its wide currency in the health care disciplines. Indeed, system imagery is becoming increasingly common in many areas of scientific study, from biological science to social science (this issue, p. 151).

It seems that in fact there is much less to this passage than meets the eye. Despite a claim of a theoretic focus, the Tierney and Taylor paper and others (again with the exception of Wright) have a distinctive atheoretical appearance. Instead of a theory being used to derive hypotheses deductively which may then be said to test that theory, the few hypotheses in evidence seem to be inductive and intellectually parentless. Concepts scamper after measures throughout the five research articles in this issue but never quite catch up, while measures abound with no explicit conceptual basis.

A fully elaborated theory with interlocking propositions that explain the phenomenon at hand is expecting too much. There is, I suspect, a middle position between that and the present papers and it would consist of a theoretical typology that describes systematically and theoretically the observed responses. If police/fire departments or state/county/city jurisdictions cannot be related to some conceptually important differences then they ought probably not to be mentioned in the first place. Without a theoretic anchorage, such descriptions are best left by scholars to reporters who do it rather better and less self-consciously. Even when the open systems approach has been used in the DRC papers it has not been used enough or with sufficient discrimination. Open systems theory does not merely posit that organizations "interface" but specifies the alternate processes of instrumental vs. expressive functioning or vertical vs. horizontal integration and delineates such key and relevant properties as homeostasis, adaptation, boundary maintenance, tension reduction, etc. These concepts and processes are not only deductively important components of open systems theory but offer the prospect of inductive insights in describing and explaining disaster EMS responses. Similarly, the "concept of system" does not merely "require that attention be paid to the environment or context..." but delineates the procedures whereby matter-energy is brought from the environment through ingesters and input transducers across boundaries into systems, as I have attempted to show in "The Social System of Emergency Medical Care" (Gibson, 1973).

All of this is not to say that open systems theory is the only conceptual perspective or even the best, although DRC papers as if it were one or the other and probably the former. Indeed, the DRC papers at least as represented in this volume, give no indication of having reviewed available theories and chosen knowingly or even willingly among them. The point has already been made that systems theory assumes relative consensus and perhaps even creates it when it is not really there and that it does not sufficiently sensitize researchers to conflicting interests between agencies and facilities nor to the at least conceivable chance

that an effective disaster response could come from pluralism rather than centralism and from dissensus rather than consensus. It is important therefore that disaster researchers be more self-conscious in their choice of theoretic frameworks and more aware of the insights and constraints inherent in each. Thus, conflict theory or at least exchange theory would, in my judgement, be more attuned to the competition between institutions for resources and mandates and to the adaptive readjustments each makes to the other. More specifically, social science has done remarkably well in explaining the relationship between interacting roles in institutions by means of role theory, and the set of concepts generated by this perspective offers much in achieving an appropriate balance between insights and constraints. Similarly, signal detection theory and value added models are likely to be of assistance in deriving concepts and in integrating findings within a more abstract and, therefore, more generalizable formulation.

Indeed, this two-way function of theoretical framework deserves particular emphasis in disaster research. The pressure for relevance and programmatic advice to EMS systems and disaster agencies is building up almost insurmountable pressures against theoretic formulation. Thus, many EMS disaster studies are entirely atheoretical in the mistaken belief that theory prevents rather than enhances practice while most studies use theory either to formulate the problem or to express the solution but not to do both simultaneously. The case needs to be made soon and more convincingly than in the past that atheoretical data dredging is not only bad scholarship but bad program assistance to disaster agencies. The best way to make that case is, of course, to show how a theoretical framework can be used:

(1) to systematize existing substantive literature so that knowledge can be accumulative and educative as to needed future studies,

(2) to derive concepts of important theoretic insight as components of a systematic descriptive typology,

(3) to deduce hypotheses of relationship between concepts in a way that is both measurable with operational indicators and a logical test of the framework from which they are derived,

(4) to modify the original framework to take into account the proven and disproven hypotheses from the study setting,

(5) to suggest future settings, and elucidating hypotheses for future research, and

(6) to develop a propositional inventory and therefore an explanatory model that is internally consistent, parsimonious, and even elegant in explaining much behavior in many settings, from relatively few variables or processes.

This requires substantial changes in the way we conduct studies in disaster research. We should subscribe to the self-denying ordinance that a disaster response will not be studied merely because it happened and is available. A disaster response should be examined only if the setting, for which hypotheses have already been derived, is theoretically important. In part, this requires less opportunism than at present and a more explicit sampling strategy than is apparent to date. Thus, a framework will delineate parameters thought to be influential in determining disaster EMS response (centralization, frequent disaster drills) and an a priori sampling matrix should be developed to specify strata or quotas or settings which systematically vary the properties under examination. And for each type of setting (centralized, frequent drills vs. centralized infrequent drills) hypotheses should have been developed. And if a disaster occurs in a setting that is not part of an a priori nominated quota of interest, disaster researchers ought to have the theoretical self-discipline to leave that disaster alone. Even greater professional self-discipline will be required in collecting only data necessary as indicators of a significant concept and not data dredging for its own

sake. We really ought to start making a distinction between data and mountains and realize that it is only the latter that are pursued merely because they are there and available. I rather suspect that explanatory models would be better developed even with less data from fewer disasters than at present if investigators had been and were more selective in their data collection strategies and better anchored in conceptually based measures. It is a peculiar irony that such a selective focus is well-nigh impossible with open-ended interviews, random probes and multiple interviewers. It also should be added, even at the risk of institutional bankruptcy for several of us, that a variable does not automatically become conceptually significant merely because a disaster agency or, heaven forbid, a research funding agency identifies it as of interest.

SUMMARY AND CONCLUSIONS: A RESEARCH AGENDA FOR EMS AND DISASTERS

The realization that the response of EMS in disaster situations may significantly vary from the EMS response in routine non-disaster situations, it has been argued, should be seen as a liberating opportunity that allows disaster research to be pursued with undiluted and single-minded scholarship. This opportunity also brings with it a challenging obligation to develop and apply appropriate methods and theories, and the DRC papers in this issue have been used as the basis for a critical and hopefully constructive assessment. It should be emphasized that the DRC papers have been used for this purpose not so much because they typify deficiencies but rather because they represent the best available. Under the general rubric of methods and theories the following developmental activities have been identified as needed and they might reasonably be regarded as a research agenda.

(1) much greater and explicit concern should be paid to data reliability both in verifying information secured on interview and in presenting sufficient evidence in published versions so that other investigators can independently assess the evidence and the reasonableness of the conclusions drawn from it. In addition, explicit and formal decision rules must be developed a priori as to what shall constitute evidence and corroboration of it.

(2) the rate and types of conflicting testimony are likely to be of methodologic and substantive significance and should be pursued systematically within the context of stratification theory and the sociology of knowledge. Disaster researchers have an obligation to identify in advance which positions will need to be interviewed in order to secure a comprehensive, verified picture of disaster events and to indicate and justify the actual response rate secured. Interviews should be conducted at varying levels of an agency hierarchy and a series of hypotheses tested on between-vs. within-agency differences in perceptions.

(3) Systematic comparisons should be undertaken as to the effect of the data collection method upon the findings and the extent to which closed vs. open-ended interviews with or without random probes and other sensitizing and/or reinforcing interviewer feedback significantly influences the amount, nature and accuracy of data secured.

(4) As a matter of high and immediate priority, developmental effort should be invested in constructing and validating a scale of effectiveness of disaster EMS response. In addition, data validating investigations should be mounted to independently validate through panels and other means the face-, criteria-, convergent-, and predictive validity of relevant data items and measures. Different measures of response effectiveness should be developed and compared for feasibility, construct validity, and ease of measurement. In addition, disaster researchers concerned with EMS would prob-

ably benefit from much greater clinical advice and awareness which would and needs to serve as an antidote to the present rather naive and utopian optimism about the unbounded efficacy of medical care in disasters.

(5) A variety of theories, models and conceptual frameworks should be identified and assessed for their utility in EMS disaster research. Hypotheses should be deductively derived in a way that allows a test of a theory and projectively published for later testing when data becomes available. Retrospective hypothesis testing ought to receive lower priority as should data collection of items lacking conceptual significance.

(6) Disaster research ought to exercise more self-restraint in developing typologies from existing data and reanalyzing past disaster information to develop, test and interrelate inventories of propositions rather than observing disasters not likely to involve theoretically significant settings or conceptually based items. Collecting less but more significant data on fewer but better selected disasters offers the prospect of greater scholarly gain than the present somewhat random walks.

The acceptance of these research tasks and their adequate funding is an issue of substantial importance in view of the theoretic and programmatic significance of EMS and disaster behavior. Too much is at stake for it to be further delayed; too little has been accomplished to date for the matter to be regarded as even half resolved.

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