memory of the answers, the solution must be in cross-checking with people of different ideologies.

This suggested procedure raises another problem: The interviewee then must be identified so that a cross check can be made, and with such identification, a legal no man's land is crossed, where data are taken from an individual and recorded without his consent. Therefore, although the procedure of cross-checking by interviewers of different ideologies is technically a legitimate methodological procedure, it has implications that are not yet controllable.

Seemingly, a pure moral position in this respect cannot be maintained if researchers still hope to get the most valid data.

Another topic subject to debate is whether more biases are generated in data where the researcher and researcher are anonymous or in data where a clear identification is made.

CHAPTER 2
METHODS FOR THE STUDY OF COLLECTIVE VIOLENCE

Charles Tilly

As I sat down to assemble these thoughts on the study of violence, the bitter news of Martin Luther King's assassination came over my desk-side radio. Word of riots, demonstrations, and looting in a dozen American cities began to break in through the music. The news gave me a hopeless feeling about the task at hand: too little, too late, too distant, too cool. That feeling of triviality and irrelevance has not quite left me. The moral worth or practical value of the research procedures discussed here will take a long time to prove themselves. Meanwhile, men destroy each other, heedless of sociologists and research centers. Still, that assassination and its riotous aftermath set an even higher value on any reliable knowledge that can be accumulated concerning violence, individual or collective.

Social scientists have framed their inquiries into collective violence in many different ways. More than in most discussions of social behavior, the central questions have been the causal ones. Why, how, and under what conditions do men join in violent acts? The question reappears in numberless disguises within the sociologies of war, revolution, conflict, collective behavior, social movements, politics, cities, crime, mental health, and deviance—let alone the corresponding psychologies, ethnographies, or even brands of economics. This fragmentation of the analysis of violence may indicate that it is so pervasive and heterogeneous a phenomenon that no single theory could contain it. Or it may mean that the perfect moment has been reached for a theoretical shakedown. I am not sure which is the case. But a review of methodological alternatives will most certainly help
clarify the theoretical alternatives. That is the point of this discussion.

Social scientists come to the study of collective violence most directly and systematically in their work on war, revolution, civil disorder, and collective behavior. The literature actually breaks into two parts. One group of studies might be called clinical. These studies examine and compare the internal structures and sequences of distinct cases of collective violence. The event or movement is the basic unit of analysis, its unfolding is the central problem, and the violence itself of secondary importance. Such studies run from essays in comparative history like Crane Brinton's Anatomy of Revolution or George Rudé's Crowd in History to contemporary studies like Norman Jackman's or S. Frank Miyamoto's treatments of protests in "relocation" centers to theoretical syntheses like Neil J. Smelser's Theory of Collective Behavior. Here are an abundance of theoretical schemes and richly documented case studies with little comparability from one study to the next, little attention to the problems of making the data comparable, and extraordinarily crude (and sparse) quantitative analysis.

The other type of study might be called epidemiological, in that it examines the incidence of different types of collective violence in terms of time, place, and people involved. From Pitirim A. Sorokin's massive compilations of the 1930's to Rudolph J. Rummel's extensive quantitative work of the 1960's, the explorer of this body of research ends up with a sense of disproportion: bad data, meager theory, ingenuous analysis. Recently, investigators like Ted Gurr have sought to redress the balance between theory and quantitative analysis. Even Gurr has had to work with dubious data. Lewis F. Richardson was one of the very few students of collective violence to express any satisfaction with the sample of events he was analyzing, and he made his sample satisfactory only by restricting his attention to the most obvious general features of "deadly quarrels." This literature offers more guidance for the manipulation of data already in hand than it does for the collection of reliable information. So far, it has produced few results of theoretical importance.

What distinguishes the study of collective violence as a methodological problem? Simply that collective violence is extraordinary, devastating, and ill-bounded. By "extraordinary" is meant sufficiently rare and tabooed that it is hard for an investigator to anticipate its occurrence, observe it objectively, or acquire full and reliable information about it after the fact. It is devastating in its threat to the very persons and social arrangements—routine recording procedures, local elites, agents of social control—best placed to accumulate an account of its development. It is ill-bounded (as compared with aberrant sexual acts, abortions, political corruption, and espionage, which share a number of features with collective violence) in terms of beginning and end, locale, participants, and basic definition. When does a riot start and end? What territory does it involve? Who takes part—only those who shoot or get shot? How many fist fights must be seen at the edge of a political rally before the rally can be called a riot? Is a civil war an instance of collective violence or a great many instances loosely linked to each other? Replying that these are purely arbitrary matters makes the point precisely: Collective violence is an exceptionally ill-bounded phenomenon.

Officials and social scientists of Western countries have wrestled successfully with problems much like those involved in the study of collective violence at least once or twice over the last century. The development of uniform crime statistics is a dubious case, because even today many criminologists consider crime statistics nearly worthless as descriptions of the extent and location of criminal behavior. But the accumulated Western experience with industrial strike reporting began with a situation much like that faced by professional observers of civil disorder today.

The strikes of the 1830's and 1840's in England, France, and Germany were extraordinary, devastating, and ill-bounded. That strikes seem less so today is explained to some extent by real changes in their character and to some extent by the growth of a standard vocabulary for their description. More or less simultaneously, durable workers' organizations formed, governments established regular procedures for controlling and recognizing strikes, and a group of analysts specializing in the study of the conduct of strikes emerged. Strikes do not have an intrinsically quantitative character, but a complex set of conventions has grown up for the description of the magnitude and character of strike activity in
Western countries--enough to facilitate meaningful international comparisons. Several of these conventions can be applied with relatively little effort to collective violence. For example, the standard distinctions among measures of magnitude like man-days per striker and strikers per strike provide convenient means of quantifying the differences between big, brief riots and small, long ones. The students of industrial conflict have a number of other practical tips to offer their conferees in the business of collective violence.

One should not exaggerate the case of analogy from industrial conflict. The forms of collective violence that are most like present-day strikes in terms of legality and efficiency of recording--football, automobile racing, and perhaps subway riding--are of no interest to professional students of disorder. Strikes became easier to analyze as they lost some of their extraordinary, devastating, and ill-bounded character.

Anyone dealing with a sort of social behavior that is at once extraordinary, devastating, and ill-bounded can expect to face knotty methodological problems. First are the problems of enumeration and sampling. The combination of extraordinary character and vague boundary means that variations in definitions of relevant events, participants, and so on will significantly affect the results of the analysis. In this respect, the study of collective violence resembles the study of rape more than the study of homicide or voting. Even an omniscient investigator would discover that the particular definition he applied would deeply affect the comparisons he made among regions or social classes.

The mortal social scientist also encounters great difficulty in identifying sources that will permit an unbiased enumeration, and therefore an unbiased sample, of relevant events, participants, and so on--regardless of the definition. He finds some compensation in the tendency of such behavior (1) to attract special attention from mass media, chroniclers, and the like and (2) to encourage the appearance of well-informed individuals who specialize in its detection and control. But he must learn a great deal about their detection procedures and biases before treating their productions as sampling sources.

Second are the problems of documentation. The traces left by extraordinary, devastating, and ill-bounded social phenomena are ordinarily fragmentary, highly selective, and subject to considerable distortion, but rich. Students of such phenomena therefore confront difficulties in assembling reliable uniform descriptions of the cases sampled, especially when the "cases" are events. The same is true when the "cases" are persons, groups, movements, or locales. Most investigators use the lowest common denominator: the body of reliable information that they can bring together for almost every case. That procedure sacrifices the richness of the occasional fragment to the demands of uniformity. If there were a way of linking the well-documented cases to the larger uniform sample, however, it might be possible to retain the advantages of both kinds of data.

Some remedies for the problems of enumeration and documentation come to mind immediately: employing multiple sources for every enumeration of cases; applying several alternative definitions of cases simultaneously in order to be able to compare the effects of definitions; exploiting the records of the agents of social control while introducing some systematic correction for their biases of enumeration and observation; establishing trained observers in likely settings of collective violence and subsequently comparing their enumerations and observations with those appearing in more uniformly and widely available sources. Before rushing to remedies, however, one should think about the more general experiences of students of other extraordinary, devastating, ill-bounded social phenomena.

Mental illness, heresy, natural disaster, and drug use offer good analogies to collective violence in these respects. The difficulties with which social scientists have wrestled in developing systematic accounts of all these extraordinary, devastating, and ill-bounded phenomena have a good deal in common. Students of mental illness have leaned toward epidemiological models and have thus made detection and diagnosis crucial methodological problems. Students of heresy, including political heresy, have vacillated among clinical studies, historical-diffusionist analyses, epidemiology, and attempts to assimilate the problem to more conventional forms of participation and affiliation. Students of disaster have emphasized the investigation of communications (and
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These observations on methodological choices may be brought together with the earlier conclusions concerning methodological difficulties inherent in the study of collective violence. Three main difficulties were discovered:

1. The sensitivity of the results of the analysis to variations in basic definitions

2. The near impossibility of making an unbiased enumeration of cases, even with a workable definition

3. The fragmentary character of the documentation normally available concerning the full set of relevant cases, which is linked to the sacrifice of richness that follows the restriction of the analysis to those data available in comparable form for most cases

Clearly, these difficulties beset epidemiological analyses more seriously than clinical analyses. Yet to the extent that general patterns are to be inferred from the latter, they become difficulties of clinical analyses as well. The more complex and heterogeneous the phenomenon of collective violence turns out to be, the more likely that numerous comparable cases will be needed in order to infer general patterns. Regardless of which major style of analysis is initially used, effective solutions eventually will have to be developed to all four problems.

Some other variables affect the methods used to investigate collective violence:

1. Unit of analysis

2. Materials available

3. Theory behind the research

Together with the basic style of analysis, these three variables form an interdependent system.

At one time or another, analysts of collective violence have concentrated on all of the following units: events, movements, localities, individuals, and populations.
The Lieberson-Silverman comparison of riot with nonriot cities in the United States, for example, took the locality as its unit of analysis; Stanley Lieberson and Arnold Silverman made no attempt to discover what kinds of individuals take part in riots or why. For the purpose of analyzing precipitants of riots, however, they shifted to the event as the basic unit. Several of the analyses of participation in the Watts riot of 1965, on the other hand, concentrated on the characteristics of individuals apprehended for taking part.

At first glance, the above list might appear to mean that students of collective violence have dealt with every unit they could have dealt with. But the list has one telling lacuna. Analysts of collective violence have rarely, if ever, taken social structures (roles, organizations, networks, families, and so on) as their basic units. The movement, the community, and the social class are about as close as they have come. Most likely this neglect grows from the implicit assumption of a sharp disjunction between collective violence and organized social life.

In any case, the choice among these units deeply affects the character of the investigation. Singling out the locality, for example, commits the investigator to the assumption that local conditions as such somehow affect the character and likelihood of collective violence; this, in turn, makes the bounding of localities and the specification of the relationship between individual and locality into pressing issues of theory and practice. Focusing on the movement (as in the frequent comparison of the American civil rights movement with African nationalism or with nineteenth-century working-class movements) immediately creates the problem of stating the limits and components of such an ill-defined aggregate. And dealing with individuals raises the question of whether or not the relevant effects of relationships and milieu can be adequately represented as individual attributes.

The choice of unit, then, determines to some extent what methodological problems will be most salient. It also determines, or depends on, the choice of theory and data. One of the chief difficulties in recent international comparisons of collective violence is the loose fit among units of analysis, theories, and data. Although the investigators have taken nation-states for comparison, some model of frustration-aggression or deprivation theory that deals with individuals, not nation-states, has commonly guided their thinking. The investigator must, therefore, stretch both theory and method to bring them together.

Of course, in this chaotic field there are often reasons to grab both theory and method wherever they can be found. Sound data on collective violence are hard to come by; their availability may be the most compelling reasons for doing the research a certain way. No taxonomy of data on collective violence yet worked out has much to recommend it. Yet some major characteristics of the available data clearly affect the appropriateness of one method of analysis or another. The following seem to be questions:

1. Do the data mainly describe events of collective violence or units involved in collective violence?
2. Are the descriptions contemporaneous or retrospective?
3. Is the source chiefly recording his (its) own behavior or the behavior of others?
4. Are the data routine by-products of social action or responses elicited by the investigator?
5. Are the accounts of behavior relatively continuous or discontinuous?

With the aid of such questions, one can begin to process data methodically. For example, the first question suggests the following inventory of sources.

A. Mainly describing events of collective violence:

1. Reports of control agents (police, social workers, political officials)
2. Reports of specialist observers, particularly via mass media
3. Testimonies of participants elicited by control agents (interrogations and court proceedings)
Mainly describing units involved in collective violence:

4. Proceedings of formal organizations (city councils, police departments, welfare offices, political associations)

5. By-products of control activities (police blotters, social agency case records, hospital admission reports, death certificates)

6. Periodic social accounts (censuses, unemployment statistics, city directories, opinion surveys)

Combining accounts of collective violence with descriptions of units involved:

7. Personal documents (diaries, memoirs, letters, chronicles)

8. Systematic surveys of participants and nonparticipants

9. Reports of instructed field observers

If one decides to work chiefly with sources of Type A or B, he sets for himself the hard task of linking sources and/or units. The problem of linking diverse sources of information about the same units arises when one set of sources enumerates the units involved in an action and other sources contain information concerning the characteristics of those units. Sometimes, for example, a researcher has a simple list of persons killed, wounded, and arrested in the course of a disturbance. To make much use of the list, he needs to identify some of the attributes of the persons on it and to attach them to the populations from which they came. Census files, parish registers, city directories, and the like often contain the necessary information. But when the numbers on both sides are large and duplications of names or characteristics frequent, the mechanics of the task become formidable. Nevertheless, the researcher faced with such a problem can find some good leads in the work of demographers (who have often had to bring together information on widely spaced births, deaths, or marriages as these occurred with respect to the same individual or family), urban geographers (who have at times constructed continuous accounts of land use, value, occupancy, and so on for particular plots, blocks, or census tracts), and collective biographers (who have faced the task of assembling comparable life histories for large sets of legislators, aristocrats, or bureaucrats).

Investigators have more experience with the linking of units. In the study of collective violence, the problem arises most often when the sources describing a disturbance or identifying its participants deal with units different from the units dealt with by sources providing information about the background of the disturbance. Frequently, for example, an investigator knows the geography of a disturbance in considerable detail and has good reason to think that the residents of the areas most heavily involved were themselves the chief participants in the disturbance, but his information on the characteristics of areas within the community pertains to larger units like wards or tracts. For some purposes he will have to link the small areas to the larger one by some device such as estimating the proportion of the ward or tract affected by the disturbance.

The investigator faces a similar problem when he has detailed occupational identifications of persons arrested for taking part in a riot, on the one hand, and information concerning the wage levels, unemployment rates, or educational levels of major occupational categories, on the other. Again, he will commonly assign the fine occupations to major categories and undertake some sort of calculation of rates of participation.

The troubles begin when the smaller units do not fit neatly into the larger ones (for example, fine data concerning occupation vs. crude data concerning industry, data concerning the ethnic composition of gangs taking part in street fights vs. demographic information concerning areas differing in ethnic composition, and so on). No general remedies are available for such troubles. They have so regularly bedeviled urban sociologists, geographers, analysts of occupational change, and students of public health, however, that the work of specialists in these fields can be illuminating.

Type C sources—those combining accounts of collective violence with descriptions of units involved—do not present the same problems of linkage. Nevertheless, they have their
own costs. They rarely appear in archives. Those that do appear (mainly in the forms of memoirs, letters, and other personal documents) are generally unrepresentative of all such records ever produced. Organizations do not routinely generate them. In order to assure their production, a researcher ordinarily needs abundant resources, extensive organization, and considerable prescience. Through field observation and survey procedures, he can assemble such sources only for the present, the future, and the very recent past. To be properly done, the use of personal documents requires great effort and complicated sampling procedures. Type C sources have important advantages in principle, but we must weigh their usually high costs against the alternatives: combinations of Type A and Type B sources via efficient systems of linkage.

The theories behind research into collective violence can be inventoried only in the crudest way. Although the sociologists, economists, political scientists, and psychologists who deal with collective violence lean heavily toward ultimately individualistic explanations, the units about which they theorize are frequently classes, communities, societies, or other groupings larger than the individual. In fact, the fascinating international comparisons conducted by Ivo and Rosalind Feirabend, Bruce M. Russett, Raymond Tanter, Ted Gurr, J. David Singer, and others pose the very delicate issue of applying notions drawn from individual psychology, such as frustration-aggression hypotheses, to the analysis of entire countries. In any case, the central units of available theories of collective violence vary considerably, and these variations have widespread methodological implications.

The time spans of available theories also vary. At the one extreme, extensive theories like those of Barrington Moore or Clark Kerr have related collective violence to very long processes of social change: industrialization, the emergence of new classes, the growth of empires. Only lengthy comparisons of whole societies will provide meaningful tests of those theories. At the other extreme, theories concerning the natural histories of collective outbursts often require only the observation of the time covered by the disturbance itself, and theories concerning the structural conditions inducing collective violence sometimes require barely any time at all for their operation. In the latter cases, observations of short time periods, or even cross-sectional comparisons at a single point in time, will meet the needs of the investigator.

Finally, the central arguments vary. In recent years, four main lines of explanation for collective violence have arisen, singly and in combination: theories of deprivation, social control, power, and aspiration. Deprivation theories trace collective violence to hardships endured by the participants themselves; that deprivation may, of course, be relative to what they have experienced before or to what others in the same society have. Social control theories either emphasize the detachment of individuals from traditional restraints on aggression or protest through mobility, changes in community structure, and so on, or they hypothesize about the decline of effectiveness of elites or specialists in repression; both of these presumably make collective violence easier. Power theories tend to treat collective violence as a part of a more general process of interest-group contention; aspiration theories attribute the rise of collective violence to the spread of new goals, and thus of new dissatisfactions, through some sector of a society. To be sure, most interesting formulations combine two or three of these. Yet the available formulations vary enormously in emphasis; they go from Crane Brinton's stress on the breakdown of social control to James C. Davies' postulated combination of rising aspirations with sudden, short-run deprivation.

Deprivation and aspiration theories easily permit the selection of individuals as the units of analysis. Control and power theories direct attention to larger social units. On the whole, deprivation and aspiration theories present fewer problems of measurement than control and power theories do; changes in the character and effectiveness of social control elude indexing, and estimating the intensity of the struggle for power independently of the extent of collective violence turns out to be hard work indeed. Stated another way, power and aspiration theories direct the researcher's attention to the attitudes, intentions, ideologies, and demands of the people involved in collective violence, and therefore to the collection of personal utterances. Deprivation and control theories turn his attention to the material and social conditions in which they live. Thus varying theories themselves call for varying units, materials, and research procedures.
Such a conclusion makes general rules for the conduct of research on collective violence difficult to establish. Nonetheless it permits a bit more insight concerning the three difficulties inherent in the study of collective violence: the problem of basic definition, the problem of biased enumeration, and the problem of fragmentary documentation. Two more difficulties prevalent in studies of collective violence can now be added: the problem of multiple units of analysis, and the problem of attaching violent action to its context. Much of the technical conversation among students of collective violence over the next few years probably will roam over these five topics. The statements made here will do no more than open the conversation.

THE PROBLEM OF BASIC DEFINITION

The difficulty here is that the definitions applied to collective violence and participation in it strongly affect the results of analysis of collective violence. In this field all definitions have a good measure of arbitrariness to them. In my own work on political disturbances in Europe, the basic definition of the events to be considered is: "An instance of mutual and collective coercion within an autonomous political system which includes violence to persons or property and threatens the existing control over the organized means of coercion within the system."

Appendix A contains a detailed description of the application of this definition to the case of France. Its chief message is that everything depends on the further operations one devises to make the definition work. It was decided, for example, to focus on events in which at least one "formation" (group acting together in the course of the disturbance) had at least fifty members. That numerical minimum became the practical grounds for assuming some threat to the "existing control over the organized means of coercion." Then it turned out that the smaller the disturbance, the less often information was available concerning the number of participants. The problem would have been much more acute with a minimum of fifteen or twenty in a formation, but it was acute enough to make necessary the establishment of a set of key words like "multitude," "revolt," and so on, whose use by reporters would, in the absence of numerical information, qualify the event for inclusion. This procedure was risky and debatable. In order to identify some of the risks, a further procedure had to be adopted: "Near misses" were coded and compared systematically with events that did enter the sample.

Many related difficulties were encountered in deciding how to bound disturbances with regard to place, time, and people involved. (Appendix A also outlines some of the ways of dealing with such difficulties.) One might draw the following general lesson from the experience described here: Because in the study of collective violence the basic definitions are at once so arbitrary and so crucial, the best procedure is to work with several alternative definitions of the events or other units to be considered at the same time and systematically to compare the effects of varying definitions on the analysis.

Few things illustrate these difficulties better than the attempt to isolate a set of events called racial disorders. First, what is a disorder, whether racial or not? How many people are involved, where, and with what kind of action? Second, what makes the disorder "racial"? If the identity of the participants is the index of "race," will disorders involving Indians alone count? If not, will all "multiracial" disorders count, including those with two races fighting on the same side? Must the disorder be black against white? What about black against white property? These obvious questions facilitate the making of some judgment of the intentions of the participants. But intentions are hard to read in the best of circumstances. The experience in coding European political disturbances described above indicates that judgments of the objectives of various participants are among the least reliable of all judgments. To make the constitution of the basic sample depend on that sort of judgment is to court disaster. Therefore, whatever information can be obtained concerning intentions, ideologies, and sentiments should be recorded and analyzed, but the inclusion or exclusion of a particular case should not be made to depend on a judgment of intentions.
THE PROBLEM OF BIASED ENUMERATION

The definitions fundamentally affect the units included, as do the sources available for the enumeration of units. The lower the threshold (in terms of what it takes to include an event, person, place, or period of time in the sample), the more acute the problem. One can therefore mitigate the problem by raising the threshold, at the obvious cost of loss of cases and of generality.

There are other remedies. Instead of employing a high threshold, one can divide a more inclusive sample into several levels of inclusiveness and then conduct the basic analyses separately for each level. If the separate analyses produce substantially the same results, one can probably combine the levels with some confidence. If they produce quite different results, unfortunately, it will be hard to know whether that is because of biased enumeration or because the smaller scale disturbances actually work differently from the large ones.

A third safeguard is to conduct the enumeration independently from more than one source and compare the samples produced. In the study of France, for example, the basic enumeration of disturbances came from the scanning of two Parisian newspapers for each day during the periods under consideration. Lists of disturbances were also prepared from certain long and comprehensive series of police reports in the French national archives and from historical yearbooks that were available for quite a few of the years following the starting point of 1830. Such comparisons discouraged any hope of arriving at the same list of disturbances from these diverse sources, but they did indicate that the newspapers provided the fullest enumeration and that their chief bias was toward the overreporting of events in big cities.

The fourth safeguard—normally a very expensive one—is to organize one's own enumeration system, to catch the events as they occur. Students of racial disorders in American cities are already experimenting with the organization of teams of skilled observers in ghettos. Their independent enumeration of events, participants, and so forth could widely applicable means of enumerating units involved in collective violence. It might also be possible to induce organizations already engaged in the detection and control of collective violence (police departments, gang workers, civil rights organizations) to collect and provide information according to standards acceptable to researchers.

THE PROBLEM OF FRAGMENTARY DOCUMENTATION

There is a dilemma here in that some units provide rich and idiosyncratic documentation, some provide enough for systematic comparison with other units, and some provide almost nothing at all. If the analysis is pushed toward the richly documented units, either many other cases are jetisoned or comparisons are undertaken that in most cases provide no information; if the lowest common denominator is relied upon, a great deal of information is sacrificed. Some form of multilevel analysis will help meet the difficulty. Sometimes the investigator can draw a large sample for which sketchy information can be assembled at low cost. (If his units are American cities, for instance, he may be able to assign every city a rough "civil disturbance" score for a given year and then combine that score with information already assembled in the card version of the County and City Data Book.)¹ Then he may be able to afford the drawing of a small unbiased subsample on which he assembles detailed information. Three sorts of analysis ensue: crude analyses of the large sample, fine analyses of the subsample, and comparisons of the fine and crude analyses for the subsample alone. This procedure gives the researcher some grounds for generalizing the results of the fine analysis of the subsample to the entire sample.

Such tactics, however, work only if the investigator can acquire detailed information on any unit he happens to select. When his sources hold him prisoner, as is often the case in ¹U.S. Census Bureau, County and City Data Book (Washington, D.C.: Government Printing Office, published annually).
historical work, a multistaged approach will surely help, but it will not provide a sure means of generalizing from small sample to large. The best he can do in this case is probably to define that stratum of his large sample for which detailed data are likely to be available, draw an unbiased subsample of that stratum, push for full documentation of that subsample, and then conduct the three kinds of analyses already mentioned.

THE PROBLEM OF MULTIPLE UNITS OF ANALYSIS

The difficulty here is that most students of collective violence are dealing with more than one sort of unit (events, movements, localities, individuals, or populations) at the same time. They must reconcile their treatments of the various units with each other and with the data at hand. Some ways of linking different kinds of sources to each other have already been mentioned. The conclusion is rather obvious: One ought not to seek to reduce the analysis to a single type of unit or to devise independent analytic procedures for each type of unit; rather, one should seek to integrate the analyses of different types of units with each other. The researcher who is simultaneously collecting accounts of incidents in a certain city and interviewing a sample of residents of that city should seek to determine which of the incidents in his collection most directly affected different members of his sample and which of his respondents were most directly involved in different incidents. Then he will be able to use each body of data to amplify the other. In dealing with contemporary collective violence, a combination of survey research, field observation, and documentary analysis will no doubt pay off more than a heavy effort directed to only one of these.

THE PROBLEM OF ATTACHING VIOLENT ACTION TO ITS CONTEXT

The problem of linking violent events to the individuals, localities, or populations they involve opens up the largest

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methodological problem of all: how to represent and measure the connections which, by our theories, account for the production of a certain variety of collective violence by a certain kind of social situation.

The problem appears in a small way in the scheme of stages of racial disorder the Lemberg Center has been using. The scheme might be diagramed:

![Diagram showing stages of racial disorder: Event → Fracas → Confrontation → Siege → Roman Holiday]

Any such sequential scheme raises a tough question: What are the probabilities that one stage will actually lead to the next and that one possible outcome rather than another will occur? Each of these "stages" has other possible consequences than the diagram represents. To take only the precipitating event, many events comparable to those which start major racial disorders occur without leading to fracases, confrontations, or Roman holidays. In order to test theories that attach considerable importance to the precipitating event, information must be available concerning the proportion of all such events that precede racial disorders as well as the proportion of disorders that begin with such events. This would, of course, include data on routine arrests, minor fist fights, and everyday life on the ghetto streets, as well as the number of potential precipitating events that lead nowhere. On-the-street observation by local teams would help a great deal; so would the analysis of police blotters and the review of diaries or time budgets of ghetto dwellers, if they could be persuaded to keep them.

The same problem assumes much more serious proportions in temporal and ecological analyses of collective violence. Having shown that protest and economic hardship vary together over time (as argued by R. B. Rose and W. W. Rostow for example), how can the connection between them be identified? Statistics show that cities in which Negroes enjoy little representation in politics and police forces also have high frequencies of violent protest; but how can the links between the two variables be specified definitely enough so that one course of action can be recommended over another?
Evidently by hypothesizing concerning the links and testing for their existence by other forms of research; by giving particularly close attention to the correlates of changes in the variables to which importance is being attributed; by watching for the results of deliberate interventions in those social arrangements when they happen to occur; and perhaps even by manipulating them experimentally and learning whether the effects correspond to our theoretical expectations.

At best, the sort of methodological survey undertaken here sets the agenda and starts the shop talk. This chapter contains little good, practical advice, doubtlessly because the topic of collective violence sprawls, big and awkward, over too large a space. The space has been paced off here in only a preliminary way.

SUMMARY OF DISCUSSION

Five major methodological issues were identified by Dr. Tilly: the problem of basic definition, the problem of biased enumeration, the problem of fragmentary documentation, the problem of multiple units of analysis, and the problem of attaching violent action to its context.

The Problem of Basic Definition

This problem is one of defining what constitutes an instance of violence, civil disorder, or whatever descriptive term is used. The very proliferation of vocabulary in this area indicates that people have trouble with definitions and with deciding what will constitute involvement.

So little is known about the effects of defining either collective violence or involvement that it is necessary to test the effects of the definitions themselves, probably by accumulating methodological experience on what happens to the actual enumeration of participants or events when the definition is changed.

Dr. Tilly amplified his warnings against the use of motive as a criterion in definition:

The conclusion I've drawn from my own work is that the definitions of collective violence had better avoid motives. If you start your work saying, "I'm only going to include those events in which the participants themselves are angry, or are protesting, or are directing their activity against the state or some other group," then it turns out that the most difficult, controversial, inaccessible piece of information is the one that everything depends on, and the alternatives are obviously to use some more readily observable feature of the event or the person to define inclusion or noninclusion in the study. Now this is probably obvious, but it's certainly causing enormous amounts of trouble in
historical and political work on collective violence. So the great temptation is to begin by defining the phenomenon itself in terms of the intentions of the participants, and time and time again this has undermined the analytic effort because everything depends on the investigator's initial judgment of intentions.

Difficulties arise when a population for collective violence research is defined without including information concerning motive in the definition. What are practical measures of definition? One possible criterion for defining incidents to be included could be whether or not the authorities intervened. That could be the test of the seriousness of an event.

Another is a criterion of scale--using incidents above a certain size that involved Negroes. Within that group of events, the research would sort for other variables in which he was interested, perhaps sorting for incidents that produced some clear racial content, those of ambiguous racial motivation, and those clearly not racial in intent.

Using formal rather than motivational criteria in the first sweep of events and then using motivational or other criteria to further limit inclusion of specific incidents in the research raises further questions. At what point does one feed in the additional set of criteria to make the further limitation, and what kind of criteria can be used to distinguish "collective" violence with some ideological content from a whole group of collective forms of criminal behavior, such as the classic gang fights in American cities? How can one determine, in gang fights involving Negroes and whites, fine differences between some racial content, routine criminal activity, or mixtures of the two? It is beneficial to "cast a wide enough net" to pick up such things as gang fights, if only to help the researcher decide whether or not what he occasionally labels racial disorder looks very much like other routine events he would not normally look at.

Moreover, a researcher must have a clear definition of the population for that set amount of time. After the population is defined, these events have separate attributes: composition, size, important motivation, number of slogans or observable manifestations. These events may not fall in a single dimension. Some events might fall primarily on a racial dimension and others might interact (racial and territorial dimensions, for example).

Problems are involved in using scale as a definitional criterion. When things are defined as routine, they are not likely to be reported in newspapers and other obvious sources. There is thus a close relationship between the data-recovering system and the defining system--the more frequent an occurrence, the less likely it is to be recorded. This would demand a very high threshold when scale is used as a criterion of definition. On the other hand, there is no necessary connection between routineness of an action and lack of some trace of it. In fact, when disorders or disturbances become routine, a body of specialists usually develops to record and deal with them, and the information concerning some routine disorder or disturbance (such as a labor strike) will most likely be more reliably recorded and managed than before.

A further consideration is that although routine disturbances may be recorded, the records probably do not concentrate on small-scale events. Therefore research requiring these records would necessitate extensive retrieval work. The problem could be handled by changing the definition of a population to exclude small-scale events whose data would be difficult to recover.

This type of limitation, "threshold manipulation," leads to the problem of deciding on an appropriate scale for the particular environment or context of the research. One way of handling this problem is to index the maximum normal level of congregation in a community and to use this as a standard against which to compare any particular incident within the community. Another possibility, albeit a crude one, would be to relate the size of the incident to the size of the community itself if the community were used as a unit of analysis.

Problems of definition would be easier to solve if they were considered a function of the researcher's goals. If the researcher first asked the question of why he was studying riots, he would find it simpler to define what units he needed to answer his question.
Scale has something to do with the researcher's foreknowledge of his sample. A researcher who is not on sure ground must have a larger universe; if he reduces the size of his sample, he must be sure that it is typical. The word "riot" has something to do with scale and magnitude of effects, but hardly anyone is yet sure where to make such cutoffs. One of the problems of definition is that a researcher often must deal with events when he has very few conceptual tools for establishing the delimiting factors.

Sometimes motivation is excluded from the definition; then, with the formal definition, the researcher generates a particular population of events and then proceeds to accept motivation as one of his important variables and to redefine and re-sort his population. There is little difference between doing this and starting right off with motivation in a definition.

One way to get around the tricky question of motivation is to ask how the event is perceived. The consequences of events are really the results of how they are perceived rather than what they actually are. Instead of delving into motivation (whether or not an event is motivated by racial considerations) the question could be asked after the fact whether or not the event was perceived as a racially connected one.

The Problem of Biased Enumeration

Even with a workable definition, most researchers do not have access to a body of data that permits them to identify all events or individuals of that kind easily. They are thus prevented from making a reasonable sampling of all such events or individuals or other units.

If a research design is built around an enumeration procedure that gets at different populations via different methods, there will be a risk that the differences between the populations that are measured are not representative of the larger populations from which the individuals were drawn. That is to say, different kinds of people are involved in the networks that are used to gain access to particular populations, and the network itself strongly influences the information obtained through it. What if two populations are used for comparative purposes in a study? Access to one is gained through public health clinics; access to the other is gained through friendship or through churches. Is there a systematic difference between these populations that will interfere with whatever variable is being tested on both so that, in effect, one winds up testing characteristics of the populations rather than the variable? Is it possible that response to an investigator varies systematically with the access network used? The problem relates also to the earlier discussion of the effect on research results of the ideology in a research center. For example, has the use of black militant interviewers and access to black influencers through militant black staff members at the Lemberg Center resulted in biased enumeration?

One way to check on enumeration is to manipulate the threshold. This device is used not only to cut down the amount of information available and the number of cases to study, but also to make a compromise between the number of cases that are definitely relatively unbiased and those not so certainly in this category.

A second alternative is to use more than one means to enumerate the population and to ascertain on which part of the population there is agreement between the sources. In some cases it is possible, by using three or so independent means of enumerating events, to find out what difference the source itself makes.

The importance of discovering whether or not a reality exists outside the perceptions of any event is debatable. A researcher may be able to proceed with perceptions as his raw data. However, there must be some consensus as to what constitutes the true population or events to be studied, some agreement on approach to enumeration, even though it is approached through different networks.

The Problem of Fragmentary Documentation

Fragmentary documentation and biased enumeration are often intertwined problems. What happens when one relies on the agents of social control themselves (police, social workers, youth leaders) to provide him with the enumeration
that he needs? What is the experience with putting his own observers in place?

Police department tape recordings of police actions during the nights or times of disturbances have often been found useful; however, this information usually needs to be checked. One form of validation, as was used in a study of the Roxbury riot, was to use interviews with participants and observations about their participation as one body of data and subsequent talks with city officials about their responses to the events as another body of data.

An informal grid system with observers stationed at intervals has also been used to record a chronological record of events. The events were used as a takeoff point for interviews with both control agents and demonstrators. Into this data was fed additional tape-recorded information, such as police calls, to facilitate comprehensive knowledge of these activities. Hopefully, this will eventually become a community-monitoring operation on a continual basis.

Auxiliary sources of manpower for observations and documentation have been local members of the Council of Churches (who were monitoring the nerve center in Roxbury) and newspaper people who were being trained and equipped for faster communication in crises. As their skill and accuracy increased, they became a more important source for establishing chronology in events.

Newspaper reports of mass meetings have not always been a very accurate source of documentation but have been perhaps consistently inaccurate enough to be usable if correction was made for the inaccuracy. In Rochester, the newspapers consistently magnified the number of participants in mass meetings by some 25 to 50 per cent over the last three years, 1965-68. In Memphis, however, newspapers underestimated participation.

Constant observers often have been useful, but the need to employ enough personnel over a long period of time in non-crisis situations so that they could function effectively in crisis situations called for an investment few research organizations were prepared to make.

The Study of Collective Violence

Another way of getting around fragmentary documentation is to use a subsample of a larger population for which documentation is only fragmentary. The subsample is then analyzed more carefully and in detail, and one can estimate from the results of subsampling what might happen in the larger population if the documentation were more complete.

How useful is survey research? Survey research is considered to be advantageous in obtaining uniform information about whatever unit is being researched. However, survey research is only a useful order of information. Survey research should never be used as an exclusive basis of information about anything. Much survey research is tapping into a body of ritualized sentiment that has very irregular relationships to what people may really think and even more irregular relationships to what they in fact do.

If one recognizes clearly the order of information that is being obtained from the survey research, such information can be extremely useful. But it is dangerous to impute the true range of people's actual sentiments and the true range of relationship between sentiments and behavior to results obtained from survey research alone, despite the fact that survey information more than occasionally conforms with the behavioral observations that one makes.

How accurate is the prediction that is made on the basis of survey research? Probably .6 is a good correlation between those who say they will do something (for example, participate in a riot) and those who actually do it. There is a tendency to overstate the degree to which a person intends to participate in any action.

Nonetheless, it is doubtful that as much could have been learned as quickly in the studies described herein if some method other than survey interviewing had been used.

The Problem of Multiple Units of Analysis

This problem is a particularly tricky one in studies of collective violence because there is interest in the community as a unit, in individuals and how they behave, and in events
and how they occur, each of these forming at some time a unit of analysis. Dealing with multiple units of analysis of different levels of complexity often presents no particular problem methodologically. The only problem is the challenge that is presented to one's ingenuity.

However, when there are two pools of data, one a description of events, independently collected, and the other a description of individuals, produced by survey research, the difficulty is to link these two. Whereas there is no problem in principle, in practice the problem is that these are two very ill-bounded groups.

In addition to the problem of how the questions are asked, there is the problem of how to bring together the data collected for the different units--on the one hand, data on the natural history of riots and, on the other hand, data about individual participation or involvement from surveys. These are questions that research groups framing survey applications will have to cope with individually.

How relevant is survey research and what are the relative merits of survey and observation techniques in learning about riots? The geography of the riot automatically imposes limitations on the sample. That is, if one is sampling randomly a set of points, where there are peaks of riot activity, he should be able to determine that riot activity was reported from those points. Moreover, riot observation is a relatively random kind of event. The people who observe riotous kinds of events (looting, arson, sniping) may not be distributed the same way in which samples are to be taken, resulting in built-in bias. If one is going to make aggregate observations, they must be aggregated on the basis of the same kind of random distribution that the observations of riot events are going to obtain. One solution to this problem is to weigh the observations by the quality of the observation. The more opportunities or the more access the observer has for viewing, the more weight his opinions should have.

Attaching Violent Action to Its Context

What kind of incidents precipitate violence? If these