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The Dimensions of Quantitative Research in History

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THE DIMENSIONS OF QUANTITATIVE RESEARCH IN HISTORY

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The Dimensions of Quantitative Research in History

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TO

Lionel W. McKenzie

AND

Frederick Mosteller

who were instrumental in

launching the MSSB program in history

Preface

THIS IS the introductory volume in a series sponsored by the History Advisory Committee of the Mathematical Social Science Board in order to encourage the application of mathematical methods to historical analysis. Princeton University Press will publish the series under the general title "Quantitative Studies in History." This volume lays stress on the scope of the quantitative methods that are today being applied in history and on the variety of issues to which these methods are germane. Each of the subsequent contributions will focus on a major historical problem. Other volumes in the series are listed on p. ii.

The Mathematical Social Science Board (MSSB) was established in 1964 under the aegis of the Center for Advanced Study in the Behavioral Sciences "to foster advanced research and training in the application of mathematical methods in the social sciences." The following fields are each represented on MSSB by one member: anthropology, economics, history, geography, linguistics, political science, psychology, and sociology. The three methodological disciplines of mathematics, statistics, and computer science are also represented. Members of MSSB are appointed, subject to the approval of the Board of Trustees of the Center, for a term of four years. At the present time the members of MSSB are:

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MSSB has established advisory committees to plan its activities in the various substantive fields with which it is concerned. The current members of the History Advisory Committee are listed on page ii above.

Supported by grants from the National Science Foundation, MSSB has organized five major classes of activities.

(1) *Training Programs*, which last from two to eight weeks during the summer, are designed to provide young pre- and post-Ph.D.s with intensive training in some of the mathematics pertinent to their substantive field and with examples of applications to specific problems.

(2) *Research and Training Seminars*, which last from four to six weeks, are composed of both senior scientists and younger people who have already received some training in mathematical applications. The focus is on recent research, on the intensive exploration of new ideas, and on the generation of new research. The training is less formal than in (1); it has the apprentice nature of advanced graduate work.

(3) *Advanced Research Workshops*, last from four to six weeks, but they are almost exclusively restricted to senior scientists and are devoted to fostering advanced research. They afford the possibility of extensive and penetrating contact over a prolonged period, which would otherwise probably not be possible, of men deeply steeped in research.

(4) *Preparation of Teaching Materials*. In some areas, the absence of effective teaching materials—even of suitable research papers—is a very limiting factor in the development of research and teaching activities within the university framework. The Board has, therefore, felt that it could accelerate the development of such materials, in part, by financial support and, in part, by help in organizing their preparation.

(5) *Special Conferences*. Short conferences, lasting a few days, are organized to explore the possibilities of the successful development of mathematical theory and training in some particular area that has not previously been represented in the programs, or to review the progress of research in particular areas when such a review seems warranted.

Robert William Fogel, CHAIRMAN
History Advisory Committee, MSSB

Chicago, Illinois
November 1971

Acknowledgments

THE AUTHORS have benefited from the criticism and suggestions of many scholars. Some are indicated in the footnotes to the individual essays. Others are listed in the Appendix.

Many of the administrative burdens connected with the production of this volume were borne by Mrs. Marilyn Gore. She was in charge of the arrangements for the June 1969 conference described in the Appendix; she was the conscience of both the authors and the editors in the struggle to meet the various deadlines; and she typed parts of the manuscript.

Mrs. Babu Jones served as an efficient and diligent editorial assistant. Susan Leibundguth aided in the herculean task of checking and correcting the footnotes.

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THE DIMENSIONS OF QUANTITATIVE RESEARCH IN HISTORY

Introduction

WILLIAM O. AYDELOTTE, ALLAN G. BOGUE,

AND ROBERT WILLIAM FOGEL

THIS COLLECTION of essays, dealing with the applicability of mathematical methods to history, is designed as a teaching vehicle. Its purpose is to show by some examples the way in which quantitative methods can be used and have recently been used in historical research. The object is to demonstrate the advantages and limitations of these methods for historical purposes, not by an abstract discussion of methodology, but by a series of essays that attempt to apply such methods to a wide range of concrete historical problems.

The decision to emphasize substantive research, and to refrain from including any papers devoted primarily to questions of method, was considered and deliberate. It does not imply any disparagement of methodological or theoretical presentations, which are at times necessary and can be most useful. It seemed, however, that, at this point in the discussion of quantitative techniques, an emphasis on the actual problems of research would be more helpful. This is partly because books that offer guides on various kinds of technical problems are already available, and others are in the process of being published. More important, however, is the point that a judgment of the advantages of an innovation must ultimately rest upon what is done with it: whether research along the lines indicated has been undertaken and carried through, and whether it has produced results of interest. There has been a good deal of controversy over the value of quantitative methods, and misunderstandings on the subject have arisen. A few examples of different kinds of quantitative research now under way may help to clear the air and to indicate, more precisely than would be feasible by other means, both the possibilities and the limitations of this approach. The essays that follow, though they make use of methodological innovations, are all addressed to specific historical problems. This does not mean, of course, that technical matters are ignored since, in the formal study of substantive questions, it is often necessary to say a good deal about method and about research strategy. In these papers, however, these matters are treated as the means to particular ends rather than being discussed in general terms.

The object of this collection of essays can also be described in a broader context. The book is a response to, and an attempt to aid and stimulate, one of the most interesting developments of recent historical scholarship. In the last generation there has been a growing concern among historians with large analytical questions, and with studying these questions by formal methods including, when the evidence permits, quantitative techniques. Historians have increasingly come to appreciate the effectiveness of statistical tools in coping with the problem of finding uniformities in the data at their disposal, and in providing the means of making inductive inferences by logically defensible procedures. These methods have proved valuable, not only because they serve as powerful instruments of analysis, but also because they give access to reservoirs of important historical information that could hardly be exploited effectively without them. A number of members of the profession have come to hope that a variety of different kinds of historical problems, heretofore discussed only in rather general terms, can by such means be treated more effectively and brought closer to a solution on the basis of ordered knowledge. The considerable amount of experimentation by historians along this line has produced results that have already attracted a good deal of attention.

The methods that have demonstrated their value for historical purposes vary greatly in level of complexity. A mere summary of the data or a reclassification of them in a relatively simple manner can sometimes bring to light important uniformities, or demonstrate the absence of expected uniformities, in a way that makes possible significant revisions of earlier views. In the course of time, however, historians using such methods have become bolder and more far-ranging in applying them. They have passed from a mere summary of the characteristics of a group to the development of indexes by which different degrees of the same characteristic in different individuals could be measured. They have attempted not only descriptive statistics, where all members of a population are examined with regard to the points in which the student is interested, but also statistics of inference in which the attempt is made, with proper safeguards, to deduce from the study of a sample something about the attributes of the larger population from which the sample was drawn. Some scholars, notably the econometricians, have attempted to recombine primary data into constructs that conform to rigorously defined concepts. While some statistical presentations can be followed by almost anyone, others are so technical and abstract that they leave most members of the history profession, except for the handful with mathematical expertise, far behind.

There are differences, also, not only in the level of complexity but in the kinds of techniques used. The choice between these will depend on the interest of the student and on the kind of problem he wants to study. If, for example, he is interested in establishing the existence of blocs of voters in a legislature, he may want to make use of cumulative scales; if he is primarily concerned with the explanation for such group behavior, regression analysis may be more to his purpose. Statistical devices can also bring to light different features of the evidence in which different scholars may be interested. For a student concerned with prediction, with ascertaining what is the likeliest outcome of a set of circumstances, the value of the statistical method is its ability to normalize. A student may, however, for good reasons, be more interested in deviations, in the extent and range of departures from the usual pattern. For him, statistical techniques can not merely provide the context, the norm from which these individual cases departed, but can also be used to measure the extent of the deviations and to summarize, in whatever categories prove most convenient, the number of cases at each level of deviation.

An attempt has been made, in the choice of the papers commissioned for the conference and reproduced here, to illustrate the wide range of intellectual concerns and technical approaches that has appeared in recent quantitative research. Although it is scarcely possible, in a volume containing only nine essays, to reflect the full extent of this variety, the editors feel that, within this limited scope, this effort has been reasonably successful. The nine rather dissimilar papers in this book deal with a number of different problems, in different areas of study, and employ different techniques and classes of evidence. Though it cannot be pretended that they cover the whole field, they do go some distance toward indicating the diversity of historical problems to which mathematics can be applied and the diversity of the methods that can be used to study them.

To assist in the preparation of these essays, two conferences were organized.[1] The first, which was held at Harvard University in June 1966, brought together the authors of the papers and a small group of historians, other social scientists, and statisticians. This conference was convened after the authors had clearly formulated the objectives of their papers, but early enough to permit them to take advantage of the suggestions of the social scientists and statisticians with respect to the formulation of behavioral models and the development of appropriate estimating procedures. At the second conference, convened in Chicago in June 1969, the penultimate versions of the papers were reviewed. Approximately sixty historians, social scientists, and statisticians par-

[1] The participants in these conferences are listed in the Appendix.

ticipated in this meeting. The discussion of each paper was initiated by three predesignated commentators. The papers were criticized and challenged both by statisticians, on formal grounds, and by specialists in the field, on general grounds. An attempt was made, in selecting the three commentators for each paper, to include at least one of each: an individual qualified to make technical criticisms and to appraise the soundness of the formal procedures; and also a commentator who, though not necessarily a statistician, and preferably not a statistician, had a recognized preeminence in the field the paper dealt with and could appraise the value of its findings in terms of the general state of scholarship in the area. These interchanges were often stimulating. Many of the criticisms were acute and well founded, and raised important and difficult questions. Statisticians were sometimes able to suggest more effective or simpler means of accomplishing certain ends, or to raise points that necessitated some revision or recasting of the formal claims made in a paper. Historians, by raising wider issues of interpretation, indicated some of the contexts in which the new findings would have to be examined if their value were to be properly appraised, and also called attention to facets of the problems studied that were not covered by the detailed research but that would have to be taken into account in any general description or interpretation.

The editors considered at one point including the comments in the book, but this proved not feasible. A principal obstacle was the fact that many of the speakers took these criticisms to heart and incorporated them, or at least took account of them, in making their final revisions. The comments, though highly relevant to the preliminary versions of the papers presented at the meeting, would not have been appropriate as critiques of the later drafts reproduced here, and we could hardly take further advantage of our guests by asking them to write a new and entirely different set of criticisms expressly for this book. It should be added that the comments have also been most useful to us in preparing our introduction in which we have tried to reflect, even if only indirectly, a few of the questions raised and the arguments pursued in the discussions.

This isn't quite the whole story. The experiment of including non-statisticians among the commentators on a set of statistical papers, though it still seems to the editors to have been a courageous effort and one well worth making, also created some problems. This confrontation brought sharply to the surface, to an extent that may have been surprising to both parties, the difficulties that the quantifiers and the non-quantifiers had in communicating their ideas and concerns to each other. Neither group, apparently, found it easy, in all cases, to get the other to understand what it was driving at. Some of the commentators were troubled by a certain "deafness" on the part of the speakers, a failure to catch the point of what they were saying. They felt that their objections, though courteously listened to, were not fully understood and did not receive satisfactory answers. On the other hand, some of the speakers were disappointed in the nature of the criticisms they received. It was not that the commentators were overly sharp. On the contrary, everybody was terribly polite; and, in any case, the whole purpose of inviting the commentators was to elicit criticisms from them. It was more the other way round. A commentator would sometimes fail to notice the real weaknesses of a paper, the points on which it was most vulnerable, and on which he could have given the speaker a rough time if he had seized the opportunity. In some cases a commentator who was expected to prove a formidable critic would apparently not even grasp the central argument of the paper he had been asked to discuss and would, instead, make observations that were friendly but uncomprehending, insightful in general terms but not directly relevant to the matter at issue.

This is still not the end of the story. There appear to be, to judge from the proceedings of the conference, two problems of communication and not one. Those using statistical methods in history sometimes find it hard to establish a common basis for discussion, not only with their fellow historians, but also with statisticians. Statisticians are powerful and helpful critics of the technical procedures. They sometimes, however, seem to have difficulty in following or display a lack of interest in the theoretical, often non-mathematical inferences that historians may try to extract from the findings. From the historian's point of view, the value of technical research consists in and is determined by the light it can cast on general problems of historical interpretation. Statisticians, as is perhaps to be expected, tend sometimes to be less interested in such problems and less knowledgeable about them. Nor were the practical suggestions of the statisticians invariably useful with respect to the pursuit of the matter in hand. Proposals were occasionally made for the employment of new techniques and devices of analysis which, though they might provide some interesting exercises, did not seem likely to advance the intellectual purposes of the research in any way that was immediately apparent. Quantitative historians sometimes find themselves occupying a delicate middle ground, a kind of no man's land, between statisticians and traditional historians, in which they are trying to apply the technical devices developed in one field to the substantive problems that have been raised in the other. A historian who tries to bridge this gap is sometimes left dangling in between and has trouble in making effective contact with the specialists in either direction, with exactly the two professional groups who should, properly, be able to help him most.

The failure of some historians to catch the issues of the papers might in part be attributed to their lack of technical knowledge. The problem, however, seems to go deeper, and to relate to misunderstandings about the basic objectives of this kind of research: what purposes it can serve, and what may or may not be reasonably expected from it. Though quantitative methods in history have now been practiced for a while, there still appears to be, even at this stage, a real problem, not only in making their findings acceptable, but also in making their purpose understood. It was disturbing to some of the speakers to find that their objectives, their attempts to achieve more precise and reliable knowledge on limited but carefully defined questions, were not recognized as valid and sometimes, apparently, not even perceived by some of their most distinguished colleagues.

One barrier to communication between quantitative and non-quantitative historians may be that some of those who have not attempted quantitative research cherish unrealistic expectations of what it can do: what can be claimed for it or, perhaps, what they think is wrongly claimed for it. The uninitiated tend to expect a breadth of scope and a degree of certainty from statistical investigations which it is not in the nature of the method to yield. They demand, for a statistical presentation, that its arithmetical findings should be indisputable, that it should present a complete explanation of the events it covers, and that it should contribute to the establishment of a general or universal law predicting that certain consequences will invariably ensue from certain conditions. Such expectations are the stuff that dreams are made of and would not be seriously considered for a moment by those who have any considerable experience with this kind of research. The folklore that has grown up around quantitative methods has not only impeded their use but has also worked to prevent a clear appraisal of the results they have been able to produce: the limited but significant tasks that can be performed by using them and that could hardly be performed in any other way. It seems desirable, then, to stress several points here, not to show that the statistical method is too limited to be of use, but rather to indicate the kinds of uses to which it may be put and the kinds of results it can produce.

The belief that the use of quantitative methods will result in universal laws or complete explanations of the circumstances with which they deal rests on a misapprehension of the nature of the approach. Historians have in any case not been notably successful in establishing universal laws or complete explanations. The achievements of the system-builders have, on the whole, not been accepted by the majority of the profession. It is worth pointing out, also, that most of these constructions have not been based on quantitative analysis: the system-builders, for all their claims to be at the forefront, have tended to be methodologically conservative. In any case, a resort to quantitative methods is more likely to restrict than to broaden the focus of a particular inquiry. Attempts at precision limit what can be covered. It is sometimes surprising how small an area it is possible to examine when one is making a conscientious attempt to be exact. In quantitative research it may be hoped that, if the work is successful, some uniformities, some larger simplifying generalizations may make an appearance, as they occasionally have, but these are likely to be within a rather narrowly defined range.

This restriction of focus, however, is not a disadvantage of the method but, properly considered with all that it implies, its principal merit. What is attempted in this approach is to take more effective advantage of selected parts of the evidence: to seize on those parts of the data that can be handled more strictly, by mathematical means, and to subject them to a more refined analysis. The procedure consists in applying close and exact attention to the limited elements of the general problem under consideration that are capable of being handled in this fashion. Restriction of focus is the price that must be paid for being more sure of one's ground. If this resulted in trivialities, the price might be regarded as too high. This objection is, indeed, occasionally made against formal methods: that they can be applied to only a narrow range of problems, which are often not the most important ones. Certainly it is true enough that not all topics of historical investigation lend themselves to mathematical analysis. Nevertheless, the use of formal methods does not necessarily imply and has not in fact implied the neglect of major problems of historical interpretation. For one thing, quantitative methods, though they limit the historian's reach in the manner just described, greatly extend it in another respect. They make it possible for him to examine the characteristics of and variations in great amounts of data, and to test quickly various alternative strictly formulated hypotheses about them. The method permits easy control over large, in some cases extremely large, masses of information that would be difficult to handle by other means, so difficult indeed that in many cases it would scarcely be practicable to attempt the task.

Furthermore, a concrete finding on a limited point, attained by a systematic marshaling of the evidence, may prove to have important implications for a much larger question. The findings, when their full implications are considered, may provide valuable new insights and permit reformulations and advances in the discussion of considerably more general problems. Fogel has pointed out elsewhere that the questions now being attacked by econometricians are, despite the novelty of the method, the classical issues of American economic history. In political history it has also proved possible to develop detailed projects of systematic research that bear directly on major issues, much studied and disputed, and that make contributions that clearly advance the argument. The papers included in this volume, though each describes a specialized and limited inquiry, are all directed, as will appear from the discussion of them below, to general issues the importance of which can scarcely be doubted.

It is also quite mistaken to expect that the results of quantitative research, even on the limited points they cover, will be or may be expected to be conclusive or final. This is partly because quantitative research in history—though quite a bit of it has now been done—is still a relatively new venture. The paths are not yet well trodden and future directions are not wholly clear. It is still not certain what kinds of problems will prove most rewarding to study, or what technical devices will be most effective in coping with them. A good deal of further experience may be needed before guidelines on these matters are well established. Much more is involved here, however, than the novelty of the approach. It is not in the nature of the statistical method, or of any other research method for that matter, to produce definitive answers to major questions. Though the lack of finality of research results is an old story, unrealistic demands in regard to quantitative research are sometimes made by the uninformed, and it is not always appreciated that quantifiers also suffer from disabilities.

It is wrong to suppose that a resort to numbers affords the student a kind of security unattainable by other kinds of evidence or that a set of papers that use figures will, for this reason, be definitive. The accuracy of the computations by no means betokens a similar precision of knowledge regarding the substantive matters described. There may be errors in measurement and tabulation. In some fields, in research involving the study of social classes for example, it may prove extremely difficult to set up categories so clearly defined that there can be no question which individuals should be assigned to which. Furthermore, statistical results are seldom conclusive since it almost never happens, in an enterprise of any scope, that all the evidence points in a single direction. Most important, statistical manipulations merely rearrange the evidence; they do not, except on an elementary level, answer general questions, and the bearing of the findings upon the larger problems of interpretation in which historians are interested is a matter, not of arithmetic, but of logic and persuasion. It is always arguable whether the results of an investigation have been correctly interpreted. The interpretations presented may arouse controversy, and there may be legitimate disagreement as to how far certain inferences are warranted in the light of all the evidence at our disposal. Anyone who believes, with regard to a large statistical project, that the categories will be wholly unambiguous, that the results will point conclusively to a single position, and that there can be no doubt about what the findings mean, cannot be well acquainted from experience with the practical problems of this type of research.

In recent controversies some of those who attack the new procedures have tried to show in a series of books and articles, a number of which are quite able, that final explanations cannot be achieved in history even with the aid of formal methods. The point may be at once conceded, but it is irrelevant to the discussion of the merits of formal methods, for this is not what formal methods do or what they are supposed to do. Those who use such methods do not, if they know their business, pretend to have achieved finality. Far from this, such a claim would be rejected as absurd, not only by anyone with experience in historical research, but also by anyone who had made a careful attempt to use quantitative techniques to describe and study social, economic, or political phenomena.

What is attempted in quantitative research, as in other research, is not full knowledge of reality but an increasingly closer approximation to it: what has been described, in a mathematical metaphor that is entirely appropriate, as the asymptotic approach to truth. The value of statistical techniques, in the cases where they can be applied, is that they make possible a highly effective deployment of our limited information. They provide means of coping systematically and efficiently with the obstacles in the way of making general statements and afford powerful assistance in the search for uniformities, in the face of the varied and confusing data with which historians are ordinarily confronted. They provide also an accurate means of seeing where we stand, how far the emerging generalizations require to be qualified and how significant are the exceptions to them. These techniques, even if they cannot produce the ultimate, can at least bring us increasingly closer to a position that we can urge with a certain amount of assurance.

This is the kind of task attempted in the research projects described in the following papers. These enterprises are not directed toward the illusory goals of universality and finality. They may more properly be regarded as conscientious and responsible efforts to tackle problems of some moment and to advance our understanding of them through the examination of new classes of information and through the study of these materials by techniques of analysis as refined as the evidence permits. The essays published here do not, of course, constitute the last words their authors will ever say on these various subjects. They are, on the contrary, a set of reports on extensive projects that are still under way, a number of which, perhaps all, are likely to result later in presentations of book length. For most of these investigations there are still gaps in the information and unresolved questions, the examination of which may well raise further problems. The hypotheses adumbrated in these papers are tentative, and additional research may produce neater and more satisfactory conclusions. In view of the difficulty of the tasks undertaken it would be surprising if, in the course of time, some revisions of the conclusions of these papers, and of the theoretical frameworks in which they are cast, were not in order.

These statements are also, inevitably because of their brevity, incomplete. It is impossible in a short paper to deal adequately with the whole range of problems entailed in an extensive investigation, and for the most part the contributors have wisely limited themselves to a single question or set of related questions. The extent of the substantive findings that could be included has also been restricted by the need, in a set of technical papers, to spend a certain amount of time on necessary preliminaries: identifying and clarifying the historical problems to which the research is addressed, and indicating the main lines of the scholarly apparatus that has been set up to deal with these matters.

The editors wish, finally, to offer a few remarks about each paper. No extensive discussion is needed. The statements of the arguments may be left to the authors. Nor would it be appropriate in this place to offer an appraisal of each piece and to enter into argument with the author about it. It may be useful, however, to attempt to put each paper into perspective in terms of the kinds of questions it has tried to come to grips with, the approaches it has used, and the technical and analytical problems that the investigation raises.

These essays, despite their variation in method and in subject matter, are addressed to a limited number of broad historical problems that can be roughly identified, and this general classification has been used as a basis for their arrangement in this book. The first two are concerned with the structure of society and the nature of social mobility, though one treats a restricted elite in England over a period of several centuries and the other a large urban population in a modern American city. The next two deal with the relation between social conditions or social change and the development of radical or violent protest. Both use materials drawn from French history, though one is concerned with the late eighteenth century and the other, primarily, with the mid-nineteenth. The fifth essay discusses patterns of voting in the American electorate. The sixth and seventh are both concerned with legislative behavior in the mid-nineteenth century, though they deal with different countries and different problems: one takes up alternative research strategies for identifying the location of political power; the other treats the patterns of voting and of political choice incident to the breakup of a major party. The last two papers deal with the factors shaping certain aspects of government intervention in the economy, one on the local level and the other on the national level. Certainly none of these general topics can be dismissed as unimportant. All of them have, on the contrary, been matters of central concern to scholars for some time.

The fact that some of these essays are, in this general way, related in theme does not, however, mean that exact comparisons can be made of their findings. Comparative studies can be useful and fascinating, but they need to be planned in advance: similar questions must be asked of similar data. The general problems on which these investigations bear are many faceted, and they have been approached in these essays in rather different ways. A more appropriate way of looking at these contributions is to regard them as illustrating the complexity of major issues of historical interpretation and the variety of different kinds of investigation that can aid in the study of them.

To give only one illustration of this point, out of several that could be offered, the first three papers are all concerned, in part at least, with social mobility. They treat three different countries: Lawrence and Jeanne Stone deal with England, Stephan Thernstrom with the United States, and Gilbert Shapiro and Philip Dawson with France. A comparative study of this problem in these three settings would be most interesting, but it cannot be attempted on the basis of these materials. The Stones and Thernstrom are traveling quite different routes. It is not only that the Stones deal with a far longer span of time, or that they consider the period before 1879 and Thernstrom, as it happens, the period after 1880. More important, the Stones are concerned with the penetration of men of new wealth into a restricted governing elite while Thernstrom deals with the social structure of a large American city. The authors also use quite dissimilar classes of evidence and techniques of analysis. The focus of the Stones' paper might seem closer to that of the paper by Shapiro and Dawson, who are also interested in the upward mobility of a middle class into an aristocracy. The central concerns of the two projects, however, are different. It is not only that Shapiro and Dawson deal with a single year, and the Stones with three centuries, or that their principal body of evidence is the *cahiers*, whereas the Stones' is primarily architectural, though they use more conventional sources as well. There is an even greater disparity in objectives. The Stones are collecting evidence regarding the existence and extent of mobility. Shapiro and Dawson, on the other hand, are interested in the effect of opportunities, or the lack of opportunities, for upward mobility upon the development of radical protest against the *ancien régime*.

1. Lawrence Stone and Jeanne C. Fawtier Stone, "Country Houses and Their Owners in Hertfordshire, 1540-1879"

The structure of British society and the composition of the British social and political elite have been, from various angles, central concerns of scholarly investigation for some time. The subject has aroused brisk controversy, and on a number of points vigorous disagreement still persists. The fierce debate over alternative social interpretations of the political events of the seventeenth century, though some of its fury is now spent, has by no means wholly subsided. Accounts of modern British history in class terms have attracted a good deal of criticism, and a number of those who have written on the subject have doubted whether rigorous class definitions are feasible at all and have suggested that it may be unprofitable to attempt to make use of such categories.

The concept of social class has been, for the last century and a half, one of the principal devices used to make political and social history intelligible. It is clear now, however, that historians have not always used this concept to good advantage. Early formulations were crude and gave an inadequate idea of a complex reality. They have not proved such useful summarizing devices and have not provided such convincing explanations of events as once was hoped. It is to be regretted that much of the theorizing about social classes was done before there existed any considerable accumulation of the results of systematic research on social stratification. Vague and naïve concepts on this subject, though we now know that they have little relation to the facts, still survive in the clichés and assumptions of a good deal of historical writing to confuse and to plague us. The assumption that some writers appear to have made, that classes could be easily defined and identified, has broken down as a result of more careful work. We have now, thanks to research over the last generation both in history and in other fields, a clearer picture of the complex nature of social stratification and of the kinds of research questions that it may be profitable to ask about it. A number of recent historians have tried to take advantage of these insights and have sought to develop social categories that would be less vulnerable, trying to derive them not from abstract theory but from what has been observed about the trends of the evidence. The first four papers in this volume, though they deal with quite different subjects, are all concerned to some extent with this problem and all make efforts in this direction.

One figment of the historical imagination that has particularly irritated some modern students is the supposed "rise of the middle classes." This phenomenon, located by A. F. Pollard (writing in 1907) in Tudor England, identified by Piers Plowman as occurring in the fourteenth century, and assigned by other writers to other periods, has been used to account for a variety of things and has indeed, as J. H. Hexter says, tended to serve "as the ultimate solution of all the problems of explanation in European history from the eleventh century on." Such interpretations of British history have, however, become increasingly unacceptable to historians, particularly as a result of research and discussion over the last several decades. It is hard to show, on closer scrutiny, that the "middle classes," whoever they were, rose, or at least that they rose all that much, at the times when their rise was supposed to have made all the difference. Arguments couched in these terms can be supported, as Hexter has shown in his witty and acute contribution to the discussion, only by employing class definitions so loose that they will not bear scrutiny.

In the light of recent evidence one is apt to be more impressed by the extraordinary retention by the British landed class of a large part of its social prestige and political influence until quite recent times. There is ample testimony to its continued predominance even in the nineteenth century and, to some extent at least, in the twentieth century as well. This eminence of the landowners lasted even into a period when the political and economic bases of their power had to a large extent been undermined or eroded. The most striking social event in modern British history, Hexter has argued, and the one that most needs to be accounted for, may be not the rise of the bourgeoisie but its conspicuous failure to rise, and the survival of the landed class which, by a series of adaptations to new circumstances, successfully maintained its power through all the vicissitudes of three-quarters of a millennium. We do not yet, however, know the whole story, and debates over the rise and fall of major social groups in England still have a certain unreality, and will continue to until enough research has been done to give us a more reliable account of the principal lines of the evidence.

Lawrence and Jeanne Stone, in the project described in the first essay, are attempting to make a concrete contribution to the discussion of this slippery and elusive topic by supplying some hard data. In order to measure the degree to which members of the business and professional classes could and did move upward, they propose to use the ingenious device of a study of the ownership and transmission of large country estates. By examining the extent to which these big country houses changed hands, and by assembling and analyzing biographical information about the owners and purchasers, they hope to provide some definite information on the penetration of members of the so-called middle class into the ruling elite. Though the analysis may not tell the whole story of this penetration, it will at least make use of materials that have not been systematically exploited for such a purpose before and that are of unquestionable importance.

Research on the history of the landed class in Britain has of late been much aided and stimulated by the opening to scholars, particularly since World War II, of increasing numbers of private archives of landed families. It is a particular interest of the Stones' research, however, that they use, in addition to these more conventional sources, architectural evidence as well. The value of such evidence for social history has occasionally been noted by others. H. J. Dyos, for example, in his book (1961) on the suburb of Camberwell in the Victorian period, took account of the relation between architectural structure and social structure and made a number of interesting suggestions. The pursuit of this intriguing line of inquiry may, however, be even more rewarding for a rural community than for a suburban one. This is because of the central role that the country house has played in British life, a subject to which a number of scholars, such as H. J. Habakkuk and F.M.L. Thompson, have already given some attention. The landed estate in Great Britain has, over the last several centuries, had a social and political significance far transcending its economic value. The great country house served as the basis of its owner's weight in the community, provided a physical expression of the standing of a family, and also, once the development of the strict settlement had made it possible to use the estate as a vehicle of family purpose, afforded a sense of the identity and continuity of the family from generation to generation.

In this investigation the Stones hope to cover an extended time span, 1540 to 1879. Lawrence Stone has already made a massive and widely acclaimed contribution to the study of the British elite in the first century of this period, in his book *The Crisis of the Aristocracy, 1558-1641* (Oxford, 1965), and his intimate acquaintance with the social history of the late Tudor and early Stuart periods gives him an impressive initial advantage. The Stones now propose to extend greatly the coverage in time, though dealing with a much more restricted topic. They intend to make comparative studies of three counties: one, Hertfordshire (with which the present essay in large part deals), immediately adjacent to London and presumably, for that reason, atypical; one further removed from the capital; and one at a considerable distance.

This paper is an early report on an extended project of research, the data for which have not yet been fully assembled or analyzed. The findings presented cover only a part of the story that the Stones hope eventually to tell. They have described the broad outlines of the changes in house construction that their investigations have so far revealed. They had given much less space, however, to the identity of the owners and purchasers of these houses. This topic, though it is of course central to the main question raised in the research, is a large subject, and the Stones have preferred to reserve a full discussion of it for a later presentation. Their paper deals also with certain central problems of method to which careful attention must be given at the outset: the nature of the evidence, how far it lends itself to their purposes, and the kinds of categories, both of houses and of men, that can be set up on the basis of the available information.

A research enterprise of this kind faces substantial difficulties, both technical and conceptual. Although the Stones have found the sources of information extensive, especially for Hertfordshire, they have not always been adequate to their purposes. The data vary in reliability, and certain gaps in them have presented difficulties. On the whole they have found evidence about the houses less comprehensive and less easy to get than evidence about their owners. The complexity of the data and the variety of the purposes for which they wish to use it have also created coding problems, though work on these is by this time well advanced and a satisfactory codebook has been prepared.

In this research, the Stones must deal with formidable problems of taxonomy. The task of drawing lines of social demarcation, though in a project like this it is inescapable, is one of the most difficult and disheartening in the study of modern English history. It is not easy to devise class definitions that are precise enough to permit tabulation and analysis but that also reflect traditional class concepts to an extent that is sufficient to make them relevant to current historical controversy. The Stones have, actually, two sets of problems, since they need to classify both houses and individuals. The solutions, they argue, must inevitably be to some extent arbitrary. Their evidence so far seems to indicate that, both in the size of houses and in the social significance of their owners, there exists a smooth continuum in which there are no obvious breaks. Hence any cut-off point must be artificial. This, in turn, raises the difficult question of how far a segment of society, necessarily defined by an arbitrary cut-off point on a smooth continuum, can justly be regarded as an identifiable social group.

The Stones' candid and informed discussion brings out how many policy decisions must inevitably go into the rules adopted for tabulating the figures, and to how great a degree subjective judgments are unavoidable in working with social definitions and lines of demarcation. It is perhaps an advantage to them that their sample is relatively small, 151 houses and about 1,500 houseowners, which permits an intimacy of knowledge of the details that may provide some guidance in the final determination of the groups to study. Readers will examine with interest the ways in which the Stones have endeavored to cope with these matters and the kinds of categories they have been able to set up. The value of their contribution will consist not only in the results they are able to produce but also in the kind of headway they can make against some of the intractable problems that have been a recurring source of trouble to students working in social history.

2. Stephan Thernstrom, "Religion and Occupational Mobility in Boston, 1880-1963"

The paper by Stephan Thernstrom contributes significantly to the body of findings on population mobility at the local level which have been accumulating since the 1930s. So early as 1933 James C. Malin published the first of his series of reports on settlement patterns and the turnover of farm operators in Kansas, thus becoming the first American historian to make major use of the population and agricultural census manuscripts of the federal and state enumerations. Malin was interested in the persistence of farm operators and their descendants, in the age structure of the farmers and their wives, in their nativity and subsequent residence and in many aspects of the farm business. He reported that the usual settler was older than the Turnerians believed, that the age patterns of frontier adults probably approximated those in older areas, and that a particular frontier region did not in turn serve as a major source of population for the frontier which developed immediately beyond it.

Malin discovered that the flow of population out of frontier communities was not greatly different in good years and bad years but that inflow diminished sharply during periods of depression. In his population research, Malin concentrated on sample townships in the various rainfall belts of Kansas, analyzing the persistence of the members of each cohort of new settlers found in each census through subsequent enumerations down to 1935. He reported that the dramatic changes in physical environment experienced in moving westward across Kansas apparently did not much influence the turnover of settlers. Rather, the members of each new group appearing in the manuscript census behaved very similarly to the new cohorts of earlier and later censuses. All groups experienced a considerable loss of members in the period immediately following arrival and then showed a definite tendency to stabilize, losing cohort members more slowly thereafter. Although primarily concerned with native-born settlers, Malin noted that the foreign-born residents identified in his research were more persistent than the natives.

Although the contemporary implications of Malin's work on population persistence inspired an economist of the U.S.D.A. to attempt a replication study, its immediate impact upon historical scholarship seems to have been less than we now know that it deserved. More attention was attracted during the 1940s by the work of Frank L. Owsley and a number of his students at Vanderbilt University, who used the data in the 1850 and 1860 manuscript censuses in an attempt to reconstruct the economic and social structure of areas in the antebellum South. They documented to their satisfaction the existence of a "yeoman" population large enough to destroy the stereotype of a monolithic slaveholding South—a yeoman population moreover that might have served as the seed bed of the humanitarian democracy necessary to end the slave system. In this research Owsley and his students were primarily interested in the aggregate picture of the social and economic structure which they could develop rather than in the mobility and economic fortunes of the individual Southerner. In retrospect it is unfortunate that the Vanderbilt scholars did not systematically identify the members of the census cohort of 1850 in the 1860 census data and analyze the changes that had taken place in their social and economic status. Such work might have provided some extremely interesting evidence as to the "openness" of Southern society.

Although a number of scholars were using Malin's methods during the 1950s and early 1960s, it was particularly Merle Curti's study of the settlement process and the development of democratic institutions in Trempeleau County, Wisconsin, published in 1959, which emphasized the utility of research based upon the manuscript census rolls of the nineteenth century and related materials to a new generation of historians. *The Making of an American Community* reflected not only Curti's interest in areal mobility as studied by Malin, but also Owsley's concern with social structure. Curti correctly noted the important relationship between the two. His study provides an analysis of settler turnover, and also attempts to show the general distribution of property and the degree to which individuals improved their economic position from one census to the next. In a particularly interesting section of the research, Curti ascribed social status to both rural and urban occupations and then tabulated the totals of those who had risen or declined in status between 1870 and 1880.

Curti published his study of Trempeleau County in 1959, after a decade of work upon it. In 1964 there appeared the first book length study in which an author using similar methods concentrated upon eastern urban dwellers, Stephan Thernstrom's *Poverty and Progress: Social Mobility in a Nineteenth-Century City.* This book is a painstaking study of the members of the laboring class in Newburyport, Massachusetts, based primarily on the manuscript census rolls of the years between 1850 and 1880. Although using some of the same analytical techniques as had Curti, Thernstrom developed various ingenious methods of his own. He was able to show that, while considerable numbers of Newburyport's workers improved their status by acquiring property, the number who improved their status by moving into more highly regarded occupations was much smaller. Thernstrom's findings allowed him to refute various of the preconceptions of twentieth-century social scientists who had studied aspects of social mobility. Well written and persuasive, *Poverty and Progress* has been of major importance in attracting a considerable number of young scholars to the study of social mobility or related topics in various American towns and cities.

Thernstrom's contribution to this volume of essays is drawn from a larger study of occupational mobility in Boston during the late nineteenth and twentieth centuries. Carrying his concerns beyond the problems treated by Curti and those considered in his earlier research, Thernstrom examines the degree to which members of various ethnic groups in Boston were able to enhance their occupational and social status and to maintain improvements in these respects into the succeeding generation. His data suggests that young blue-collar Catholics in Boston were as proportionately successful as the scions of Protestant working-class families in finding their way into white-collar occupations. But a larger percentage of the Catholics had blue-collar fathers than did the Protestants. And once they had improved their positions, the Italian or Irish workers were more apt to lose them or "skid" than was the Protestant or Jew. Catholics from working-class families experienced less upward intergenerational mobility than did Protestants and young Catholic men from middle-class families were less successful than young Protestants of comparable origins. After considering the possibility that discrimination, peasant background, confinement in the ghetto, differential fertility rates, or institutional completeness accounted for his findings, Thernstrom suggests that cultural values provide the most likely independent variable. His materials, however—he explains—leave unclear the question of whether religion or national origin contributed most strongly to those cultural values. But in conclusion he notes that his findings are congruent with Weber's great thesis linking the "Protestant ethic" to the rise of capitalism in Europe and more specifically the corollary to this thesis, holding that "exposure to 'the Protestant ethic' would continue to predispose Protestants to success in the market place in later historical periods, and that Catholicism would continue to inhibit the worldly aspirations of its adherents."

Thernstrom has faced formidable problems in this piece of research. None of his data, of course, were compiled initially with answers to his specific kinds of question in mind, and he had to "manufacture" his data from several different sorts of records, a situation that always complicates the construction of time series or the analysis of periodic observations. Some of the data were incomplete and the numbers in his various ethnic groups differ considerably, his sample understandably yielding a much smaller number of Jews than of Roman Catholics or Protestants. Like others doing comparable research he has had to satisfy himself as to the size of sample which can be considered adequate for his purposes and the relevance and meaning of significance tests. In all these respects Thernstrom has made an honest effort to face up to the issues involved and, although in retrospect he might have preferred to have drawn larger samples, his defense of his methods carries the ring of conviction and common sense. But the methodological issues raised by the paper run deeper than mere problems of data retrieval and sampling. There has been a tendency in this type of research, as in the newer political history, to engage in "number smashing" unrelated even to middle range, let alone grand social or political theory. Thernstrom's comparative analysis of the conclusions in related research, his emphasis on the fact that modern findings must not be assumed to apply to earlier time periods, and his careful use of Weberian theory all give encouraging evidence of the rewards that historians may find in the judicious and explicit use of theory.

At the Chicago Conference, Donald J. Bogue described this paper as being a "very important" contribution to the literature of historical demography. Quite aside from the substantive findings it is notable for the thoughtful manner in which Thernstrom has considered alternative explanations of the data. This research also emphasizes the success which the new historians of urban social processes are having in utilizing data sources long disregarded or undiscovered. In its contingency tables and probability analysis the paper also reflects the growing statistical expertise of the modern scholar as contrasted with the raw scores, percentages, and averages used by the authors of antecedent research.

3. Gilbert Shapiro and Philip Dawson, "Social Mobility and Political Radicalism: The Case of the French Revolution of 1789"

The third and fourth papers also involve questions of social analysis but in a different context, since they are primarily concerned with the relation of social and economic circumstances to political attitudes and to political action. The discussion of the relation between socio-economic conditions and political events has, of course, a long history, going back well before Marx, and has received attention from many distinguished historians since, both Marxists and non-Marxists. The issue has unquestionably been a major one in the modern study of politics.

On this subject, as on others, the enthusiasm of some scholars would appear to have overreached itself. Social theories have sometimes been used to explain historical developments in ways so careless as to provoke a substantial amount of deserved criticism. A number of historians of the present generation have called attention to the crudeness of prevalent class concepts, to their weakness particularly on the psychological side, and to the obvious presence of other elements besides class interest in political motivation. Some critics have seriously questioned whether class concepts have, or can have, much explanatory power. Hexter identifies, as one of the notions that has played a considerable part in his own thinking and writing, the view that: "The only way you can fit history into what is roughly described as the economic or class interpretation is to leave out half or three-quarters of what happened and not ask any very bright questions about the remnant."

So categorical a dismissal would apply, one might suppose, more to a rigid and obsessive application of such theories in the teeth of the evidence than to careful research in which an attempt was made to weigh and appraise the effect of various determinants. In any case, in view of the results of recent massive investigations of the American electorate, it is difficult still to maintain that social backgrounds and political attitudes are in all cases wholly unrelated. The point must be stated cautiously, for the relationship between the two has proved to be far from consistent, not only because of the complexities of most systems of social stratification which preclude the easy identification of class groups, but also because other things besides social background have been shown to be related to political choice: religious and ethnic differences, regional loyalties, rural-urban conflicts, sex, age, and the political attitudes of one's parents. Even so, the trend of the evidence is strong. Seymour Martin Lipset attempted in his *Political Man* (1960) to bring together the results of many research studies on political behavior. His conclusion, regarding the relation of political to social conflicts, was that, while political parties may renounce the principle of class conflict, "an analysis of their appeals and their support suggests that they do represent the interests of different classes. On a world scale, the principal generalization which can be made is that parties are primarily based on either the lower classes or the middle and upper classes." Whether this means that the concept of class has explanatory power can, of course, be argued, since it is elementary that a correlation doesn't prove a cause-and-effect relationship. Yet the correlations are impressive enough to merit study and promise to be helpful in attaining some greater understanding of the nature of political behavior. Whether such further research would result in a general or sweeping "class" interpretation, or whether it would suggest that this line needs to be played down and that other matters may be more important than previously realized, remains to be seen. It is worth mentioning that a certain amount of recent work points in the latter direction.

This general issue, the relation of social and economic circumstances to political attitudes and events, has, as might be expected, engaged a considerable part of the attention of those interested in the history of the French Revolution. Class interpretations, almost Marxist in character, have been put forward by some of the most distinguished scholars in the field. Lefebvre describes the French Revolution as "the crown of a long economic and social evolution that made the bourgeoisie the mistress of the world." Soboul holds that "the revolution is explained in the last analysis by a contradiction between the relations of production and the character of the productive forces."

Attempts have been made over the last few generations to accumulate systematically organized information that would help to test generalizations of this character or that would at least make it possible to assign them a more concrete meaning. These include, for example, Crane Brinton's pioneering study of the Jacobins (1930), and Donald M. Greer's statistical monographs on the victims of the Terror (1935) and on the emigrants (1951). Such books have had considerable effect on the development of scholarly opinion even though, in the course of time, qualifications have been expressed both about the ways in which they summarized the evidence and also about the inferences that they presented. The theoretical implications of various kinds of social definitions have been extensively explored by other scholars, for example by Alfred Cobban, who held that historians needed sharper tools than the Marxist ones and that some of the most famous Marxist concepts such as "bourgeoisie" and "feudalism" were too imprecise to be useful for the discussion of eighteenth-century society. Cobban dealt, in a series of books and articles, with problems involved in the interpretation of the evidence we have on these matters and reached a thoughtful and carefully argued position which, though not all scholars in the field accepted it, still commands respect. These are only examples for purposes of illustration: there has been much further work. The discussion of the French Revolution in these terms has produced so huge a literature that no brief recapitulation could do anything like justice to it.

In general, the more modern approach, the innovation that seems to be of value, is not to argue whether classes exist, or who were the bourgeoisie or the aristocracy, but rather to look for social indicants of various types and to consider what conclusions may legitimately be drawn from the correlations they produce. Both the third and fourth papers reflect this line. Both of them, instead of rearguing the old Marxist thesis or discussing empty questions of abstract class definition, try to define the social concepts they use in empirical terms and to see what can be made of the evidence.

Gilbert Shapiro and Philip Dawson, in the third paper, express the view that, despite the long argument over the social interpretation of the French Revolution and the various conflicting opinions that have been put forward, there has not been available, up to now, much concrete evidence on which a judgment could be based. They attempt to cast some light on this general question by presenting information on a point which, though it is narrowly defined, they regard as important. Their paper, which is deliberately restricted in scope, takes up the single question of the relation of political attitudes to expectations of opportunities for entrance into the nobility or, on the other hand, to the lack of such expectations, in France in 1789. The matter is of considerable interest since major theorists have disagreed on whether radical sentiment is more likely to be the consequence of opportunities for upward mobility, with the disorientation that arises in such a situation, or whether radicalism more generally ensues from the frustration of hopes for upward mobility. Shapiro and Dawson have in particular considered the hypothesis, classically formulated by de Tocqueville, that the former of these two suppositions may be correct and that revolutionary feeling may be strongest in the regions of a country that are most advanced economically and in other ways and where, presumably, there were the most considerable opportunities for men to move up in the social scale.

In testing such a hypothesis what matters most, one might suppose, is not facts but beliefs: not what happens but what people expect, not the real incidence of ennoblement but the judgment of contemporaries regarding the possibilities of ennoblement. It is the second of these, expectations rather than facts, that might be expected to influence men's attitudes. This distinction must be drawn, and Shapiro and Dawson are careful to draw it. Unfortunately, however, it is difficult to see into men's minds, all the more so when they lived nearly two centuries ago. The authors have therefore been compelled, as the only means of handling the subject rigorously, to take as their independent variable not the second alternative but the first, not the social expectations of their subjects, which cannot now be ascertained with accuracy or in detail, but rather what in the existing circumstances their subjects might reasonably have been entitled to expect: what the authors refer to as the perceptible opportunities for advancement. The paper attempts to examine the relationship, in various communities, between opportunities for entry into the nobility, as measured by the number of saleable ennobling offices, and the degree of middle-class radicalism, as measured by the evidence of the *cahiers.*

The project brings up several classes of problems. There can be some technical argument about how much may legitimately be inferred from the figures and how important are the differences they reflect. Beyond this, the principal variables involved in this study, social mobility and political radicalism, are both difficult to measure. Another point, on which there was some emphasis in the discussion of this paper at the conference, is that the authors have centered their attention on a single type of mobility, ennoblement through the purchase of offices, and it is possible that other avenues to ennoblement should also be taken into account, as well as other paths to advancement that may have been open under the Old Regime. Also, since a man could get ahead not only in his own town but, in certain circumstances, elsewhere, there may be some doubt whether the number of ennobling offices in a single community can properly be regarded as the whole story of the expectations of a resident. It could be argued that claims regarding the importance of the independent variable analyzed in this study cannot be accepted unless a number of other possible variables are properly controlled in making the tests. There is also the question whether it was the men who expected to get ahead, or the men who didn't, who wrote the *cahiers*.

The authors have by no means shirked or ignored these problems. They have grappled with the ambiguities of both their central concepts, mobility and radicalism, and have devoted a considerable part of their exposition to problems of definition. They have sought, for example, to distinguish between the possible reactions of someone who had a chance to be ennobled and someone who didn't but was merely a spectator of the process, perceiving the upward mobility of others and drawing his own conclusions. They have also presented an interesting discussion of the kinds of radicalism reflected in the *cahiers*, of which they have made an extensive systematic analysis, giving attention to the circumstances in which the *cahiers* were produced, and considering various alternative ways in which the degree of "radicalism" of a *cahier* could be intelligibly measured. They have dealt with the problem of Paris which might be expected to be a special case and atypical, by presenting two sets of figures, with Paris and without.

The writers of this paper are modest in their assertions about their results. They claim, not to have resolved a major scholarly controversy, but only to have produced evidence that, so far as it goes, appears to tell in a certain direction. It tells, interestingly enough, for the de Tocqueville position and against the Taine-Dollot position. This finding, if further investigation continues to confirm it, is a valuable one and adds something important to our understanding of the period. The present paper, however, gives only a partial account of the extensive research on which it is based. It is, of necessity, largely devoted to an exploration of some of the general questions involved in this research and to the rationale of the technical apparatus that has been set up to deal with these matters. A fuller exploration of the statistical materials will be needed before it is possible to tell how far the inquiry has contributed to a reinterpretation of the history of the period. For the present, the authors have conscientiously laid out the evidence as they found it, and further discussion of the subject can proceed from there.

4. Charles Tilly, "How Protest Modernized in France, 1845-1855"

In the fourth paper Charles Tilly explores the relation between social and economic change and political action in a much wider context. He addresses himself to the question of how far and in what way structural changes in French society, such as industrialization and urbanization, have produced and shaped violent conflict.

The general issue on which this topic bears, the impact of industrialization upon modern society, has long been a central concern of scholars and has precipitated controversies that are, even now, by no means resolved. The blistering indictment by the Hammonds of the social effects of the industrial revolution has been much qualified by revisionist economic historians such as Sir John Clapham and T. S. Ashton, though it has in recent years been vigorously reasserted by, among others, E. P. Thompson and E. J. Hobsbawm. There are many things to consider in making a judgment on this large question and perhaps, with the limited amount of information we have, no final judgment can be made, though the subject was well worth investigating and the conflicting shifts of scholarly opinion have brought to light much that was interesting.

Another way of getting into this question, however, and the line that Tilly pursues in his study, is to examine the relationship between industrialization and mass violence. That such a connection existed, at least for Great Britain, has been argued by E. P. Thompson (1963) who holds that the industrial revolution witnessed the emergence, roughly in the period from 1780 to 1832, of a distinct working class. This group, he states, came to feel an identity of interests that resulted in an increased amount of organized protest from 1780 on, and especially after 1800. This protest, according to Thompson, was a direct result of the consciousness of the workers that the industrial revolution presented a threat to them and of the realization by different groups of workers that they had common interests. Their resentment was expressed in the Luddite riots, among workers whose skills were being rendered obsolete by machine production, in the increase of trade-unionism, and in the development of political pressure groups culminating in the emergence of Chartism in the later 1830s.

In more general terms, one of the most widely accepted views, which Tilly considers at the outset of his paper, has been the thesis that industrialization is, in its social effects, a process of quick disruption followed by slow stabilization. According to this scheme, the relative social quiescence of the preindustrial period gives way to a phase of intense social unrest as the great changes of industrialization and urbanization make their impact, which is then succeeded by a period of greater calm as the industrial society becomes mature, and more sophisticated and efficient techniques are worked out for dealing with the disagreements that arise. This is the familiar model of the hump-backed curve of the growth of violent protest in an industrial society that shows in graph form the relation between civil violence and the stages of economic development measured by such indicants as economic prosperity or the increase of the gross national product. In this model, the incidence of mass violence is low in a society with a primitive economy, increases with the increase of industrialization up to a certain point, and then slopes downward as the economic system becomes more highly developed. Tests of the model have been made by comparing the present state of affairs in a number of different countries that are in different stages of industrialization, and much of the information gathered from contemporary international comparisons of this kind appears to support the hypothesis.

It is Tilly's contention, however, that this hypothesis cannot be accepted until it has been tested, not only for different countries at one time, but also for one country at different times, so as to get some evidence on change over an extended period. He has tried to do this for France. He has gathered and tabulated an immense amount of data on outbreaks of violence in France over the last century and a half and has summarized a section of his findings in his contribution to this volume. In his paper he surveys changes in the incidence and character of collective violence in France over a considerable span of years and then presents a more detailed account of the mid-nineteenth century and particularly of the decade 1845-1855, which he thinks may be a turning point. He has kept in mind the twentieth-century comparison, and in many tables the figures for the three decades 1830-1860 are matched by figures for the three decades 1930-1960. His principal interest, however, is in the 674 disturbances that he has identified as occurring in France from 1830 to 1860, a disturbance being defined as an interaction between at least two formations, in the course of which some person or property was damaged or seized, and in which at least one formation included 50 or more individuals.

Tilly's first conclusion, out of which much of his later argument develops, is that, for modern French history, the model of the hump-backed curve breaks down at once. The history of violent protest in France, according to the data he has been able to assemble, affords little or no support for the traditional view of the relationship between industrialization and violence. The model is entirely inadequate and has so little relation to the course of events that it must be discarded. Tilly has, in fact, been unable to discover any clear connection between industrial or urban growth and the development of turbulence. On the contrary, apart from minor occasional fluctuations, the frequency of disturbances did not greatly change throughout the period of industrialization. Following the lines indicated by this basic finding, Tilly has raised a number of questions that entail some novel ways of looking at the central problem with which he is concerned.

He suggests, for example, that the main currents of collective violence may flow much more directly from the political process than scholars have been accustomed to admit. The yearly fluctuations he has observed seem more closely correlated with political change than with social and economic change. Tilly has come to be concerned, as a result, less with the direct impact of major structural changes and more with the political processes through which these changes may possibly have operated. Nor is he willing to allow that structural changes of an economic or social kind were necessarily the sole origins of political tensions leading to violence. He deals also, for example, with the resistance to centralization: the protest against the imposition by the central government of its powers of taxing, conscripting, and judging in communities that were already accustomed to the exercise of these powers by the agents of smaller, provincial governments. The resistance to central authority, he points out, recurred regularly whenever it had been weakened by war or revolution. Waves of protest against the collection of taxes by the central government, for example, occurred after the revolutions of 1789, 1830, and 1848. Also, the information Tilly has been able to assemble on the objectives of collective violence, though he is aware of the ambiguity of evidence on motives and the caution with which it must be interpreted, appears to reinforce his position regarding the decidedly political character of these outbreaks.

Tilly suggests, however, that, if no change in the frequency of collective outbreaks with the progress of industrialization can be observed, it may be profitable to look for change of another type, and he deals particularly with the possibility of there having occurred significant alterations in their character. He believes that he has found substantial evidence pointing to this. He holds that in the course of time such outbreaks became bigger and briefer. In the 1840s and earlier, he finds, the predominant forms of collective violence—the invasion of fields, the tax revolt, the food riot, the anti-conscription rebellion—were somewhat disorganized and uncontrolled: a type of violence that he describes, in one of the two general categories he has set up, as "reactionary." By the 1860s these forms of protest had almost disappeared and had been replaced by strikes, demonstrations, and similar complex, organized actions—the type of collective violence that he describes, in his other general category, as "modern." The change is from communal contenders (religious groups, villages, members of local markets) to associational contenders (industrial firms and trade-unions). The former type of outbreak was localized and uncoordinated; the latter was disciplined, scheduled and organized in advance, tended to be on a large scale, and was apparently instigated by more highly organized groups. Tilly's paper deals with how this change took place. He suggests, though he has not yet been able to establish this with certainty, that the change began in the 1840s and was largely completed by the 1860s so that the decisive point, at which transition was occurring most rapidly, appears to be the middle decade of the nineteenth century, the decade to which he has given particular attention. The change, as he describes it, was not immediate or absolute: the old forms, though fading away, still showed themselves powerful in a last outburst around 1848; the new forms, though taking over in the middle of the century, became predominant only after long previous cumulation in the most advanced sectors of French society. Tilly does not suggest there were no associational groups before 1845 and only such groups after that; he is trying, rather, to indicate what patterns were general or usual.

Following out this line of argument Tilly proposes the hypothesis that the impact of large structural changes was not direct but indirect: that these changes, though they did not in themselves generate collective violence, may nevertheless have contributed to change its character. The impact of these changes, he suggests, was upon the number, identity, and organization of the contenders, which in turn helped to determine the predominant forms of collective violence as well as the places in which it erupted. The effect of industrialization and urbanization was to bring about a decline in the communal bases for collective violence and an increase in the associational bases. The transition to an industrial society involved a temporary uprooting and disorganization, a state of affairs in which collective violence was less feasible and less effective, but which led ultimately to a new kind of organization among the discontented. Urban-industrial life massed men together in groups and eventually promoted the formation of special-interest associations of which trade-unions are perhaps the most conspicuous example, though there were other kinds as well. Tilly suggests that this organizational process, which transformed the character of the outbursts, may be the basic link between industrialization and collective violence.

These views, if they prove acceptable, are clearly of great interest. We do not, however, as the author points out, have the whole story yet. Tilly's group has already been working for years on assembling and tabulating the data. He insists, nevertheless, that he has as yet arrived at only a primitive stage of numerical description, verification, and comparison, and that he has only scratched the surface in making tests and drawing inferences from this body of materials. Some of his generalizations are, as he has indicated, not yet conclusively established. Furthermore, in this project as in others, basic problems of interpretation arise about which there may be some argument. The argument depends in large part upon the definitions and classifications adopted, and regarding some of these there may be controversy.

It is not easy, for one thing, to be definite about the occupations and social positions of the participants in the disturbances, in view of the complexity of the French social structure and the inadequacy of the police dossiers on certain points on which we should much like to have more information. Tilly has made some effort to compare the characteristics of participants in the disturbances with the characteristics of the population as a whole, but feels that this attempt has not been wholly successful. He found it difficult to reach secure ground in ascertaining the occupations of participants. Nor is it clear that Tilly's two general categories for describing disturbances, "reactionary" and "modern," suffice for the complexity and diversity of the evidence. There was some controversy on these matters in the lively discussion at the conference that followed his presentation. It may be possible that each category includes a variety of activities sufficiently dissimilar so that to group them under one heading can be misleading. It is also possible that the use of such general categories may obscure features of the evidence that might provide simpler and readier explanations of the phenomena described. There are also, as the author is well aware, exceptions to the general trends he has identified. Associational or "modern" conflicts can be observed before the mid-nineteenth century, and even before the industrial period, while communal or "reactionary" conflicts have occurred in the twentieth century. The extent and importance of such exceptions is a serious matter, for they raise questions not only about whether the mid-nineteenth century was a turning point, in the manner suggested in the paper, but also about the hypothesis that the character of these outbreaks was related to economic change. Tilly has made it clear that he is not wholly satisfied with the evidence supporting the view that the turning point in the change of the character of collective violence was in the middle decade of the nineteenth century: one particular obstacle to reaching firm ground on this point is his finding that disturbances were infrequent in the 1850s, which makes it difficult to get a clear picture. There was, apparently, relatively little collective violence during the reign of Napoleon III. Fascinating though these preliminary results are, there are clearly basic problems of historical interpretation as well as of mathematical analysis that will require further thought and attention in later and more extended presentations.

5. Gerald H. Kramer and Susan J. Lepper, "Congressional Elections"

The effort of Gerald H. Kramer and Susan J. Lepper to obtain more precise information regarding the determinants of voting in Congressional elections, the only paper in this volume on electoral behavior, stems out of and carries forward a reassessment of the methods and objectives of American political history that has been in process for some years. So early as 1949 Thomas C. Cochran in his famous denunciation of the "presidential synthesis" gave expression to a growing concern, shared also by others, that the work of American political historians had far less explanatory power than was desirable. Though a number of historians expressed approval of Cochran's statement we can hardly argue, some twenty years later, that they rushed to provide the sociologically sophisticated and state-oriented political histories that he demanded. Yet there has been a gradual shift of focus in the writing of American political history, and it seems reasonable to suggest that much of the quantitative or behavioral work going on now is actually in the spirit of the Pennsylvania professor's manifesto, even if it is not precisely in the form that he originally suggested.

It was Lee Benson who most significantly pointed the way to new directions of research in this field. We sometimes do injustice to forelopers or to "slow publishers" when we suggest that the work of particular scholars has broken new paths. Yet it seems clear, in retrospect, that Benson's long essay "Research Problems in American Political Historiography" in Mirra Komarovsky's *Common Frontiers of the Social Sciences* (1957) was the first important contribution to a "new" American political history, dealing with popular voting and related institutional developments. In this paper Benson suggested that historians could move to a more sophisticated level of analysis if they used popular voting data systematically, comparing the results of