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THE "NEW" SOCIAL HISTORY AND THE SOUTHWEST:
THE DALLAS SOCIAL HISTORY PROJECT

by *Harvey D. Graff*

It does not take long for a newcomer to Southwestern history to discover that this region, with all its glorious legends and dramatic events, truly lacks a systematically recorded past. The Southwest abounds with the fruits of a long tradition of solid historiography and lodges of literateurs' lore. However, when the researcher looks below the level of colorful portrayals of personalities and battles and the saga of frontier settlement, he or she finds the basic ingredients of history as yet untouched. This is principally the case in social and economic history—and especially that of the modern style. The bare bones of social development, population profiles, and social differentiation and their interaction with a developing economy have simply not received the dry but grounded attention of the historian or social scientist. This failing is especially glaring when we turn to questions which now dominate modern historical inquiry: the structures and configurations of past society, be they social, demographic, familial, or economic. Certainly urban history remains largely in the same relatively unstudied position.

The student of local history may be thinking that this assessment is unfair. However, aside from long out-of-date town and county histories and some worthy amateur efforts, the only published professional work in the urban history of Texas is Wheeler's *To Wear a City's Crown: The Beginnings of Urban Growth in Texas, 1836-1865*.¹ Regardless of the interpretation advanced, the book suffers from important limitations. It does not describe or analyze urban development in much of the state, and the developments it does focus upon are treated informally and anecdotally. The analysis owes little to conceptualizations of urbanization, and it lacks both comparisons and indicators of change and development. Specifically, it owes little to the important and growing literature in the "new" urban history; the book hardly reveals the dominant research tendencies of the last decade.²

Other recent studies in Texas history seem more centrally related to the "new" social history.³ They attempt to draw upon more modern research strategies, adopting the systemic, quantified approach and drawing upon routinely-generated records such as the Federal Census Population Schedules which are available from 1850-1880. Nevertheless, each study lacks much attention to explicit conceptualization, comprehension of the social processes which underlie the patterns isolated, or much social theoretical awareness. They are preliminary and beginning studies, not final products of a new historical method or approach.

It is therefore difficult to avoid the conclusion that, in published studies at least, the social history of Texas lies fallow. It seems barely touched by a dozen years of work and example in the "new" social and urban histories, untouched by the many and diverse sources for stimulation, challenge, or encouragement—American, English, or French. Exceptions are rare. The handful of studies referred to here surely do not form an inclusive listing, but they are sufficient to establish the point: the social past of the region awaits modern, systematic, and social-scientific examination.

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In recent years, much attention and a burgeoning literature, too large to cite here, has been devoted to "new trends in history," the relationships between history and the social sciences, and the validity of quantification.⁴ For well over two decades debates have raged between the "humanistic" or "traditionalist" school of historians and the "social scientific" wing of the profession, although it now seems that much of the squabbling has turned into a more fruitful awareness of mutual limitations and strengths and a growing movement toward a more productive methodological pluralism.

Especially important in the newer forms of historical research are methods, sources, and techniques. However, the primary advancement relates not only to these matters but perhaps more centrally to explicit conceptualization and an approach to analysis which is systematically and comparatively oriented. As a strategy the "new" history often involves a basis in quantification, to achieve a greater breadth of generalization and problem solution and a social scientific perspective. More rigorous and explicit formulations of problems and research design, the search for different sources of information, and greater precision in both formulation of questions and of answers are demanded. Quantification underlies much of this research, with its reliance upon the value of standard and comparable data over a valid chronological span and with its close relationship to social theoretical concerns. The use of historical data which is routinely-generated, or reproduced at regular intervals, is equally critical. Of course researchers must be equally aware of the limitations of this approach and must recognize which problems and questions are amenable to numerical solutions and which are not. In terms of urban and social history, the new directions encourage the formal identification of relevant factors, their measurement, and the examination of impact and interaction. In these directions, great progress has been made in recent historical scholarship.⁵

The "new" urban and social histories are now maturing, but as suggested earlier, Texas and Southwestern history have barely been influenced. Very few studies extend west of the Mississippi or east of California. Recent and current work, however, have provided useful guidelines to problems and approaches as well as to difficulties and obstacles.⁶ The Dallas Social History Project, as introduced here, seeks to apply the approach, indeed advance upon it, to the past of Dallas. The Project will attempt to combine the most fruitful of techniques from traditional and more recent historical methods, through a large-scale collaborative orientation to historical inquiry.⁷ A wide variety of topics and questions, as suggested below, establish the boundaries for the investigation: importantly, they are among the most prominent in the "new" social and urban history, ranging from family and childhood to social structure and elite analysis.

In comparison with older forms of urban and social history, the approach adopted here centers upon analysis rather than description or narration, theories of change and development (in this case, urbanization), and whole societies as far as the extant sources allow. More importantly, the city is conceptualized as a social system in which the interaction of dynamic processes and the diversity of peoples took place. Linear metaphors of growth, concentration on the articulate and visible citizens and notions about uniqueness of one place are eschewed. Most helpful to the student in these regards are the books and papers of Eric E. Lampard, Stephan Thernstrom, and Samuel P. Hays, who illustrate new paradigmatic approaches to urban social studies in the past.⁸ Together, they stress the importance of the demographic, ecological, and spatial dimensions in understanding social development. They discuss the often separately studied groups and components of the city in their changing interrelations and in terms of systematic and systemic conceptions. The city is seen in terms of a variety of

processes and structures in flux, whose overall relationships constitute the social entity we identify. Of course, the forms of interaction are not universal, but they are dependent upon the composition of the population, the historical circumstances, migration, locational factors, economic variables, distribution of power, external relations, and often chance factors. Each of these elements constitutes a system in its own right, while the city may be either more or less than the sum of the units. Comparatively, the systems and their subsequent development of any one place may be usefully compared to those of other places, urban or rural.

Into this highly abbreviated and schematicized overview comes the Dallas Social History Project; its goals and interests enumerated below. It joins the ranks of an increasing number of urban social history projects, derivative of the "new" histories, focusing on such disparate places as Hamilton, Ontario, Poughkeepsie, New York, Amiens, France, and Philadelphia.⁹ It develops directly from recent historical tendencies: quantitatively, conceptually, theoretically, comparatively; however, it hopes, like most adolescents, to transcend its origins. The new social and urban histories have remained in many ways incomplete, although they have had a dramatic impact on the historical profession. They have not succeeded, however, in escaping the limitations of the case study of an individual area or of conceptual failure to systematically and cohesively link the many elements which comprise the systems of cities. Furthering the development of urban history, this Project plans to surmount these limitations through a proper recognition of past weaknesses at its inception. The Dallas Social History Project represents a new generation of historical urban social research with its awareness of comparative and conceptual challenges: toward that end, we seek to establish a new framework for historical analysis and the following introduction is offered as our first experimental attempt.

THE DALLAS SOCIAL HISTORY PROJECT: AN INTRODUCTION

The criticisms of the social and urban history of the Southwest and Texas advanced above apply squarely to Dallas' past. Texas' second largest city and the commercial-financial center of the region, Dallas remains historically obscure. Although it has been served well by the amateur historians, there is no apparent justifiable reason or special circumstance which accounts for the lack of professional and scholarly attention. Certainly, there is no lack of primary records or of a dynamic past or the important social processes. Consequently, the history of the city and the county (along with much of the region) lies unstudied and unreconstructed. The story of the city on the Trinity with its blend of races, social classes, mixed economic activities, urban problems and longterm historical significance has been left to anecdotal accounts as substitutes for serious research, for cultural transmission and understanding. Not surprisingly, the result is a blurred sequence of events, and a lack of fundamental knowledge about population change and composition, distribution of wealth and power, geographic transformations, and vital events.

Nevertheless, Dallas' modern form, the culmination of the historical processes of immigration, stratification, differentiation, urbanization, and modernization stretch exposed to view and the critical records remain readily retrievable in local, state, and national repositories. The forces of change, therefore, are accessible for isolation, analysis, and interpretation. The Dallas Social History Project proposes to begin the reconstruction of the city's past, beginning with the nineteenth century, through the systematic exploitation of the routinely-generated records. Simply, we seek to blend historical imagination and sensitivity with the socio-demographic methods of the "new" social

history, thus explicating the structure of Dallas society from its founding in the 1840s, continuing the study through the first several decades of the twentieth century. We will trace the changes in the structural processes of population, society, geography, and economy, and illustrate the path of social change with its local impact.

GOALS OF THE PROJECT

In this as yet formative state, the Dallas Social History Project is guided principally by three goals:

First, *the history of the city of Dallas* obviously represents the most direct, tangible, and popular goal. In the modern form of the traditional "urban biography," this aim is distinct and discrete; the comprehension and interpretation of the early and adolescent years of development and their wide publicization, in scholarly as well as popular formats, are central. It is important, however, that this (especially in light of the following) not be viewed as a limiting, narrow, or demeaning goal; it is still the stuff of history. Moreover, only from a local basis and grounding can a superstructural interpretation of urban society be constructed.

Toward this end, and that of the other goals, too, the Project will collect, systematically record and analyze virtually all available records for the period of study, approximately 1846-1930. The methodological approach will fall first upon records which are regularly available, comparable over time, and amenable to systematic and quantitative analysis as well as machine-processable. These of course are the routinely-generated records: principally, they include:

U.S. Federal Census manuscript schedules, 1850, 1860, 1870, 1880

State of Texas Censuses, 1847, 1848*

Tax rolls

Vital statistics registers: birth, death, marriage (availability is somewhat spotty)

Wills

Deeds and other land records

City directories

Business records

Banking records

Surveys, land plats, and maps

Voluntary association membership rolls

Educational records

Newspapers

Jail and court records

Oral histories

The collection, preparation, and interpretation of these sources form the main thrust of the study and will occupy the Project for at least several years. Supplementary records, in addition, such as newspapers (used in ways different than above), diaries, memoirs, letters, private papers, and local lore, as well as oral reminiscences, will be critically examined.

*I am currently searching for the original schedules of these censuses; if any reader is aware of them, I would appreciate that information.

To list the materials of the "new" urban history, alas, barely hints at the difficulties and complexities involved in their exploitation. Naturally the historian must not only be versed in historical context and historical method, but he or she must also be trained in social scientific research approaches, statistics, and electronic data processing. Yet these are the more simple obstacles to hurdle. These types of data, as all other data, are not simple to interpret; the student must consciously and consistently, with explicit assumptions, interpret the materials which he or she exploits. The vagaries of nineteenth-century handwriting, occupational terminology, age-heaping, changing names of women, wealth and income data all severely complicate this requirement. This result is a challenge, yes—but often a most frustrating one.

Special problems await researchers of these records: they have received a good deal of attention. The first is that of nominal record-linkage, or the joining together of two or more distinct records or bits of data which pertain to the same individual. For example, if one has two lists, one from a census manuscript and, say, a tax list, both identified by name, the task is to scientifically match them so that the records of the same individual may be combined.¹⁰ To this we might add the data of a city directory, for instance. Several computer systems specialists are at work designing automatic linkage packages and several methods of combining machine-sorting with manual checking are available. Nevertheless, the problem is a severe one, exacerbated by the high rates of population mobility of the nineteenth-century city. This is one obstacle which the Dallas Project must scale.

A second problem is occupational classification. Any one who has even glanced at the page-after-page lists of individual occupations in published census aggregates, let alone the schedules themselves, has been confronted with literally hundreds of occupational names. Obviously, the researcher cannot deal with such a diversity, but must classify the occupations into groupings based on a conceptualization of social classes, status hierarchies, and horizontal or functional structures. This is no mean feat, as contemporary stratification studies, with all their supplementary data, attest. The historian sometimes has no more than intuition and knowledge of the local economic structure on which to base the classifications. In other cases, records which include wealth, property, income, or servants may be combined in the establishment of hierarchical scales. This is an equally important challenge to the Project.¹¹

Attention to these "technical" problems, however, must not detract from the more fundamental goals. This indeed has been detrimental to some researchers in the field. Historical data, we must remember, is always approximate, and the search for precision and exactitude can become debilitating. Technical problems should not intrude unnecessarily upon more interpretive and substantive ones, although some may do so properly. This is a further risk which must be averted, and which a stress on the larger conceptual framework should avoid.

A third problem awaits the attempt to conduct a systematic oral history of cohorts of older Dallas residents. Their contribution to such a study should be obvious, but the task of research design and conduct of the exercise is virtually unprecedented in North American studies. As well, the relationship of this evidence to other materials is equally severe.

Other problems require attention as well. For example, the 1850 and 1860 censuses did not ask the relationship of household residents to the head of household. This crucial data for family and kinship analysis must then either be inferred or derived from estimates weighed from the 1870 and 1880 schedules, as Hershberg's Philadelphia Project is now attempting. Incomplete lists complicate

matters especially when combined with the mobility phenomenon; finally, the paucity of vital statistics records reduces the possibilities for sophisticated and systematic demographic calculations and for kinship analysis.

Nevertheless, in the face of the large amounts of data extant and the many important questions awaiting attention these problems are less than paralytic. Rather, they are frustrating and irritating, but they are susceptible to imaginative solution and hard work.

In terms of this first goal, a final effort will be addressed to the involvement of local groups and individuals and the generation of local interest. This is both self-serving and community-serving. The latter aim is obvious, while the former rests on the need for people to search through attics, desk drawers, records, and photo albums as well as through their memories. Each town in Dallas County has a local historical society and the city has a Historical Society, Historic Preservation League, and a Genealogical and Historic Society. The Dallas Social History Project will form ties of mutual cooperation with as many of these organizations as possible.

Thus the first goal, not a unique or isolated one, lies in the history of Dallas.

Secondly, the Dallas Social History Project aims to become *an urban historical laboratory*. This goal may well be somewhat slower in realization than the former. As it develops, the Project will simultaneously provide an important training function to both students and staff of this and hopefully other area universities—graduates and undergraduates—provide an historical basis or component for contemporary social analyses of the city, and furnish a center for the testing, creation, and operationalization of social historical research methods and techniques. Toward these ends, undergraduate instruction is now related to the Project, graduate student involvement is beginning, and several new graduate programs are in the planning stages which would involve the Project directly. As well, the Project is already cooperating on contemporary social research efforts in order to avoid the perils of a historical social science and to give them a longitudinal and contextual grounding.

Concretely, and without detracting from its purely research and interpretative activities, the Project is seeking to sensitize social scientists to the importance of the historical dimension in their work and should soon be able to provide them with a "unseable past" for this city. Of equal importance is the attempt to introduce other historians and "humanistic" students to the potential contribution of social scientific and quantitative research innovations. In this connection, it is important to note that a numerical or statistical basis need not (and should not in itself) imply a non-humanistic perspective; the emphasis remains on the individual in his or her society and, importantly, the approach includes those historical actors whose presence is lost in traditional research designs.

The laboratory functions will become rather technical at times, involving social scientists, computer programmers, systems analysts, in addition to traditional students of society, past and present. We will of course attempt to find solutions to some of the problems discussed above as well as establish effective coding systems, sampling techniques, and data management procedures. Finally, the systematic approach will be extended to the collection and analysis of photographic sources, newspapers, and other cultural sources. This is only consistent with my emphasis on methodological pluralism and striving toward a perhaps unreachable total history.

The Dallas Social History Project's third large-scale goal is the one perhaps most important to professional scholars; it is the most abstract activity of the effort. This goal relates to *comparative social history and social theory*.

Obviously, the history of no one place rests in a vacuum—conceptual, contextual, or geographic; nor are the complex processes of change, continuity, and development (regardless of their relationship and outcomes) unique to any one place. Thus, the framework of the study is quite explicitly cast in comparative and theoretical terms. With respect to the former, the increasing amount of data and the developing data base being constructed from the work of the several nineteenth century urban social history projects makes comparative research both practical and virtually required. Most of this material, coded and machine-readable, is available to the project, from the Five Cities Project and the Inter-University Consortium for Political Research, Ann Arbor. Detailed comparisons of social structure and social change therefore comprise one central concern. It is now or it soon will be possible to systematically analyze Dallas' patterns in the light of those from Hamilton, Buffalo, Pittsburgh, Montreal, Philadelphia, Kingston, Poughkeepsie, Montreal, London, Cape Breton and Halifax, Boston, Detroit, Western European cities and towns, a variety of small cities and towns, as well as an increasing number of rural areas. Data also will be gathered from less urban areas of Dallas County for local comparisons. The prospects are as exciting as they are challenging. Not only is the task (though perhaps not with all these places) prodigious, important problems of comparability of data and classification must be confronted. Nevertheless, all this is required if real progress is to be made.

Of equal importance conceptually and analytically are questions of theoretical import, most particularly those related to social process, structural differentiation, and social change. Many notions and constructs exist in the social scientific study of human behavior, urbanization, commercialization and economic development, ecological succession and deconcentration, demographic behavior, racial differentiation, social stratification, mobility and persistence, family organization, and so on. Substantively, then, the Project will be engaged in the systematic examination and testing of prevailing and competing models of explanation for development and change in urban, social, spatial, and demographic phenomena. Overwhelmingly, our perspective lies in the clarification of the wholy concept of modernization and with its applicability to urban places. And who knows, we might even construct theory ourselves.

Research Tasks and Areas of Investigation

Without attempting to be inclusive or exhaustive, the Dallas Social History Project seeks to make contributions to the study of the following areas. Some of these topics are a standard feature of the "new" social history; others are perhaps more innovative.

1. *Stratification and mobility studies.* The presence of slaves, freeborn blacks as well as white and Mexican-American immigrants adds to the study. We hope to compare the experiences of all these groups, especially the emancipated blacks. Of special interest too is the interaction of groups each of which is new to the area: who succeeded, what were their advantages, how did they do it? Patterns of mobility (social and geographic) will be established: groups and individuals will be compared. Other important questions relate to the connections between persistence and mobility, savings patterns, property ownership and servant possession, conspicuous consumption, lifestyles, inter-generational patterns, social class and ethnicity. The existence of a long series of data on wealth-holding should permit a real contribution to be made.
2. *Settlement and land development.* Through the use of city and county directories, land patents, deeds, and transfers, we will attempt to plot

patterns of settlement, residential mobility, centralization and decentralization, ecological development and change, and land use. We are now building a collection of nineteenth-century maps and surveys.

3. *Family history and kinship analysis.* Family history is truly one of the most rapidly developing areas among the "new" social histories. We will study family structure, marriage patterns, adolescence and "growing up," and also attempt to chart kinship networks through the use of wills and marriage records. Wills also allow some analysis of parental control and family authority patterns as well as the position of women in the society, their variation by race, class, and ethnicity.
4. *Business, commerce, and industry.* Through the use of census records, population and manufacturing schedules, city directories, business records, and the newspapers, we will reconstruct patterns of commerce, their development, and change. We will attempt to link residence and workplace and relate commercial location to other spatial considerations. The remarkable Dun and Bradstreet credit ledgers are an invaluable aid in this reconstructive venture.
5. *Demographic patterns.* The census and vital statistics records allow the calculation of indices of fertility, nuptiality, and mortality. These are essential topics in the history of women and the family, relating as well to family patterns, social and economic development, stratification, ethnicity, race, and class.
6. *Cohorts and Lifecycles.* This topic clearly overlaps that of family and demographic historical questions, but in terms of method and approach it requires special attention. Here we will conceptualize individuals both longitudinally and cross-sectionally, comparing and supplementing the results of both operations.
7. *Transport and communications.* Changing patterns of movement, from horse and wagon to urban systems of horse trolleys and electric railroads will be studied as they relate to commercialization, residential segregation, and ecological transformation. Communications networks will be examined in the larger meaning of the phrase.
8. *Education.* The history of education is in a renaissance as most historians are well aware. Importantly, many of its questions relate to urban and social concerns. Thus we will be particularly interested in the evolution of a school system, literacy, patterns of attendance, and racial-ethnic-sexual-and-class discrimination.
9. *Institutional development.* More broadly than education alone, we will establish the development of institutions in the city, and their consequent patterns of differentiation and specialization. We conceive of "institution" flexibly and will examine formal and informal, public and private facilities, the services they provide and the groups they encompass.
10. *Cultural development.* All cultural events and institutions will be identified and studied to shed light on recreational, intellectual, and differential access patterns and their changes over time.
11. *Urbanization.* The final topic in this basic listing is perhaps the most essential and the widest. With Dallas, we begin the investigation on the eve of the city's very creation and will trace its development to urban prominence and hinterland domination. This is a rare opportunity for the historian.

Previous urban histories, old and new, have focused largely on one or two of these topics or social processes. They of course can be studied with significance and validity in their own right. The principal goal and innovation of the Dallas Social History Project, however, is to study them in dynamic interaction,

toward the creation of a larger comprehension of urban society and the impacts of modernization upon it, its diverse components, and their relationships.

The Agenda

With such a large scope and an operation of major proportions, it is essential that a flexible agenda be established and maintained. In the Project's first full year, 1976-77, we planned a bibliographic survey, collecting and locating all the references and primary materials required. Books, documents, and manuscripts are being purchased, copied and filmed.

Secondly, we are now also collecting aggregate statistics on the city and county, 1847-1900. This will provide us with a framework in which to develop the microanalysis and help in posing initial questions. We are beginning to train research assistants and hope to begin the encoding of census and tax records. Coding and data preparation will take us into years two and three, with the analysis running concurrently. The completion of the project, barring unforeseen problems, should require from five to seven years. The Dallas Social History Project, finally, sees its endeavors as both a challenge and an invitation to other Texas and Southwestern students of history. We seek your cooperation and want to share our materials and interests with you.¹²

NOTES

¹(Cambridge, Mass.: 1968).

²Stephen Thernstrom, "Reflections on the New Urban History," *Daedalus*, 100 (1971), 359-375; and Leo Schnore, (ed.), *The New Urban History* (Princeton, 1975).

³Alwyn Barr, "Occupational and Geographic Mobility in San Antonio, 1870-1900," *Social Science Quarterly*, 51 (1970), 396-403; Richard Lowe and Randolph Campbell, "Wealth-holding and Political Power in Antebellum Texas," *Southwestern Historical Quarterly*, 74 (1975), 21-30; B.T. Williams, "The Frontier Family: Demographic Fact and Historical Myth," in *Essays on The American West*, ed. H.M. Hollingsworth and S.L. Myres (Austin, 1969), 40-65.

⁴See, for example, my forthcoming review and the volumes discussed there, "Counting on the Past: Quantification in History," *Acadiensis*; Robert Berkhofer, *A Behavioral Approach to Historical Analysis* (New York, 1969); Lee Benson, *Toward the Scientific Study of History* (Philadelphia, 1972); William Aydelotta, *Quantification in History* (Reading, Mass., 1970); David Landes and Tilly, *History as Social Science* (Englewood Cliffs, 1971); and E.J. Hobsbawm, "From Social History to the History of Society," *Daedalus*, 100 (1971), 20-45.

⁵See the discussions in my "Counting on the Past" and "The 'New-Math': Quantification, the 'New' History and the History of Education," *Urban Education* (forthcoming); and R.W. Fogel, "The Limits of Quantitative Methods in History," *American Historical Review*, 80 (1975), 329-350.

⁶See, as examples only, Thernstrom, *Poverty and Progress* (Cambridge, 1964), and *The Other Bostonians* (Cambridge, 1973); Sam B. Warner, Jr., *The Private City: Philadelphia in Three Phases of its Growth* (Philadelphia, 1968); and M.B. Katz, *The People of Hamilton, Canada West* (Cambridge, 1975).

⁷Especially interesting in this respect is Theodore Hershberg, "The Philadelphia Social History Project: A Methodological History," (unpublished Ph.D. Dissertation, Stanford University, 1973).

⁸See, for example, Lampard, "The History of Cities in the Economically Advanced Areas," *Economic Development and Cultural Change*, 3 (1954-55), 81-136; and his

"Agnostic Foreword" in Schmore, *op.cit.* Hays, "The Development of Pittsburgh as a Social Order," *Western Pennsylvania Historical Magazine*, 57 (1974), 431-448; and "The Changing Political Structure of the City in Industrial America," *Journal of Urban History*, 1 (1974), 6-39. Thernstrom, *op.cit.* "Urbanization, Migration and Social Mobility in Late Nineteenth-Century America," in *Towards a New Past*, ed. B.J. Bernstein (New York, 1968), 158-175.

⁹See, for example, Hershberg, et al., "Occupation and Ethnicity in Five Nineteenth-Century Cities: A Collaborative Inquiry," *Historical Methods Newsletter*, 7 (1974), 174-216.

¹⁰See Ian Winchester, "The Linkage of Historical Records by Man and Computer," *Journal of Interdisciplinary History*, 1 (1970) 107-124; and E.A. Wrigley, ed., *Identifying People in the Past* (London, 1973).

¹¹See Katz, "Occupational Classification in History," *Journal of Interdisciplinary History*, 3 (1972), 63-88; and Griffen, "Occupational Mobility in Nineteenth-Century America," *Journal of Social History*, 5 (1972), 310-330. More generally, see Wrigley, ed., *Nineteenth-Century Society* (Cambridge, 1972).

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