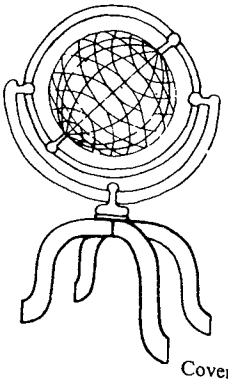

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The Old New Social History and the New Old Social History*

Charles Tilly

Social History Renewed?

In the spring of 1968, the learned journal *Daedalus* convened a covey of historians. The group included some established sages, such as Felix Gilbert. It also brought in people—for example, Frank Manuel, Eugene Genovese, Lee Benson, and David Rothman—who had been exploring new techniques and materials, or attempting to employ in historical analysis ideas and procedures that had grown up in the social sciences. A number of them were coming to be known as practitioners of something called the "New Social History".

Several of the participants prepared memoranda in advance, and some of the memoranda dealt with such esoteric topics as "cliometrics" and "prosopography". The words tripped the tongue, but stirred the imagination. For, in the 1960's, many historians felt that historical theory and practice alike were undergoing great changes. Some felt the changes threatened

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the proper performance of the historian's function: Jacques Barzun (1974), for one, fulminated against "psycho-history" and "quanto-history" as pseudo-history. Others felt that history finally stood on the threshold of Science: Lee Benson, for example, spoke of the likelihood that "[T]he conditions will exist in the not distant future for American political historians to achieve the scientific estate predicted by Buckle, or, more precisely, . . . that such conditions will exist for those individuals able and willing to pay the psychological costs required to break free from old routines" (1966: 16). Most alert historians, whether with fear or hope, sensed that the profession faced imminent choices whose consequences could profoundly transform the history, and the historiography, that they had learned.

Participants in that 1968 meeting disagreed about the future of the past. Yet they agreed on the desirability of discussion. So *Daedalus* flew to another conclave, this one in Rome. Then came a pair of journal issues, and finally a whole book. The book, published in 1972, appeared under the title *Historical Studies Today*. Its topics covered a wide range: quantitative history, the New Urban History, oral history, an epitaph for the old political history, a mixed assessment of recent applications of psychology to historical analysis, and—as promised—a thoughtful treatment of prosopography.

Convened today, how would a similar set of historians pronounce on the future of history? What has come of the 1960's promises? What changes in historical practice have occurred since then? What lessons have we learned? Concentrating on social history, broadly defined, let us wander among these questions, without making too strenuous an effort to lock their answers in place. Let us pay particular attention to the historical endeavors which in the 1960's began to display the stigmata of social science: self-conscious explication of concepts and models; deliberate comparison of individuals, groups, places, or events (often many of them) placed within a common framework; and fixation on reliable forms of measurement, frequently involving numerical treatment of evidence. Eco-

nomic history, archeology, demographic history, urban history, plus some kinds of political, labor, agricultural, and family history qualify. Diplomatic history, intellectual history, the history of science, art history, and other branches of agricultural, labor, and political history, in contrast, generally remained aloof from the New Social History and its entanglements in the social sciences. Important changes were and are occurring in those fields as well, but I shall neglect them here, in favor of the fields I know best: the various enterprises known loosely as social history.

Lawrence Stone, Prosopographer and Prophet

In those fields, prosopography became more than a catchword. It became a crucial practice. Through all the Daedalian discussions, the chief prosopographer present was Lawrence Stone, the distinguished historian of England. Stone had published a massive collective biography of the English aristocracy, and was then engaged in a vast analysis of the changing character of England's landed classes. The centerpiece of that analysis was, in fact, an ambitious venture in prosopography: a large catalogue of four "samples" of country houses and their owners down through the centuries. Lawrence and Jeanne C. Fawtier Stone once described that study as

> designed to apply statistical methods of analysis to data of varying quality, in order to test some subjective impressions and traditional assumptions about English social structure and social mobility in the Early Modern and Modern periods. [A footnote at this point credits grants from the Mathematical Social Science Board and the National Science Foundation.] It is generally agreed that England was historically the first of the modernizing societies of the world, and in particular that she was the first to industrialize and the first to evolve a stable and broad based constitutional structure. For over a century it has been part of conventional wisdom that these phenomena can be partly explained in terms firstly of the slow growth of the middle class of business and professional men, and secondly of the ease with which this middle class could move upward through the social and political

> systems. So far, however, these is no reliable body of statistical information with which to check and evaluate the truth of this bold and far-reaching hypothesis. This particular study is narrowly focused on a single aspect, namely the degree of interpenetration of the landed and merchant/professional classes as tested by the changing composition of the local rural elites (Stone & Stone, 1972: 56).

In this study, then, prosopography would begin to verify previously hypothetical arguments concerning social mobility in England from 1540 to 1879. A "reliable body of statistical evidence" would supplant the "subjective impressions and traditional assumptions" that had so far prevailed.

Writing his more general statement for the 1972 *Historical Studies Today,* Lawrence Stone displayed cautious optimism. If historians kept their heads and hearts, he suggested, prosopography could sharpen their eyes. "Prosopography", "collective biography", or "multiple-career-line analysis", he pointed out, all referred to a rather old procedure that had simply acquired a new range of applications. It was "[T]he investigation of the common background characteristics of a group of actors in history by means of a collective study of their lives" (Stone, 1972b: 107). That old procedure, properly followed, had healing powers. It could, he declared,

> combine the humane skill in historical reconstruction through meticulous concentration on the significant detail and the particular example, with the statistical and theoretical preoccupations of the social scientists; it could form the missing connection between political history and social history which at present are all too often treated in largely watertight compartments, either in different monographs or in different chapters of a single volume. It could help reconcile history to sociology and psychology. And it could form one string among many to tie the exciting developments in intellectual and cultural history down to the social, economic, and political bedrock (Stone, 1972b: 134).

Thus the new ways in history could lead historians to basic social processes without losing them their contact with day-to-day experience.

Lawrence Stone certainly had his finger on the right button. In one form or another, collective biography surely constituted

the single most influential innovation in the historical practice of the postwar period. As Stone said, it was not entirely new: Sir Lewis Namier (1957) had long since biographized eighteenth-century Parliaments, Roman historians had been perpetuating prosopography for decades, and "collective biography" is one more name for the method of Crane Brinton's old book (1957) on the Jacobins. Nevertheless, at least four features distinguished the collective biography begun in the 1940's from its predecessors: (1) its extension from clearly-visible elite populations to run-of-the-mill militants, ordinary workers, and even entire communities; (2) the corresponding increase in the sheer numbers of persons described; (3) the wide use of statistical description, sometimes including statistical models adopted from the social sciences; and finally, (4) the sheer range and frequency of its application. Urban history, population history, labor history, and some branches of political, economic, and intellectual history all created their own standard forms of collective biography. Later, the histories of the family, of migration, and of racial and ethnic minorities incorporated collective biography as a central procedure. Historians acted as if they believed Stone's 1972 credo: that collective biography revealed the pattern of events and social relations while maintaining contact with individual experience.

Second Thoughts

A decade after the Princeton meeting, however, Lawrence Stone had lost his old zest for the new ways. In 1979, he hailed the "revival of narrative." He concluded that events had come back into style, as the techniques and determinism that had captured the historians of the 1960's began to lose their appeal. "Many historians," wrote Stone,

> now believe that the culture of the group, and even the will of the individual, are potentially at least as important causal agents of change as the impersonal forces of material output and demographic growth. There is no theoretical reason why the latter should always dictate the

former, rather than vice versa, and indeed evidence is piling up of examples to the contrary (Stone, 1979: 9).

The bedrock had crumbled to sand. The new ways had become old ways, suspect in their turn.

Three different sorts of *soi-disant* "scientific" history were therefore, according to Stone, losing their followings: the Marxist economic model, the French ecological-demographic model, and the American cliometric method. The supporters of all three had once, said Stone, claimed to be on their way to cast-iron solutions

> for such hitherto baffling questions as the causes of "great revolutions", of the shifts from feudalism to capitalism, and from traditional to modern societies. This heady optimism, which was so apparent from the 1930s to the 1960s, was buttressed among the first two groups of "scientific historians" by the belief that material conditions such as changes in the relationship between population and food supply, changes in the means of production and class conflict, were the driving forces in history. Many, but not all, regarded intellectual, cultural, religious, psychological, legal, even political, developments as mere epiphenomena. Since economic and/or demographic determinism largely dictated the content of the new genre of historical research, the analytic rather than the narrative mode was best suited to organize and present the data, and the data themselves had as far as possible to be quantitative in nature (Stone, 1979: 7).

The revival of narrative, it follows, registers the decline of that "economic and/or demographic determinism". As the author of studies of class structure, social mobility, educational enrollments, and the pattern of revolution—all significantly informed by the models and methods of contemporary social science—Stone should know whereof he speaks.

What caused the revival of narrative? What chased the decline of analytic history? Stone catalogues these causes:

a. Widespread disillusion with economic determinism in history, most likely promoted by a decline in the ideological commitment of western intellectuals—especially when it came to Marxism;
b. revived awareness of the importance of political and military power;

c. the mixed record of quantitative work, especially when carried out by large research teams, based on the use of computers, and embodied in sophisticated mathematical procedures.

These new conditions, as Lawrence Stone sees the situation, freed historians to try once more "to discover what was going on inside people's heads in the past, and what it was like to live in the past, questions which inevitably lead back to the use of narrative" (Stone, 1979: 13).

Stone saves his strongest disapproval for the work of "cliometricians". (He names only the inevitable Robert Fogel and Stanley Engerman, leaving his readers to recall the bad examples "we all know.") The cliometricians "specialize in the assembling of vast quantities of data by teams of assistants, the use of the electronic computer to process it all, and the application of highly sophisticated mathematical procedures to the results obtained" (Stone, 1979: 11).

Against these procedures, Stone lodges the objections that historical data are too unreliable, that research assistants cannot be trusted with the application of ostensibly uniform rules, that coding loses crucial details, that mathematical results are incomprehensible to the historians they are meant to persuade, that the storage of evidence on computer tapes blocks the verification of conclusions by other historians, that the investigations tend to lose their wit, grace, and sense of proportion in the pursuit of statistical results, that none of the big questions has actually yielded to the bludgeoning of the big-data people, and that "in general the sophistication of the methodology has tended to exceed the reliability of the data, while the usefulness of the results seems—up to a point—to be in inverse correlation to the mathematical complexity of the methodology and the grandiose scale of data-collection" (Stone, 1979: 13). For this eminent European social historian, the large enterprises that took shape in the 1960's have obviously lost their attractions.

E.J. Hobsbawm, likewise an eminent European social historian, has recently published a commentary on Lawrence Stone's later essay. Hobsbawm (1980) doubts that the revival

of narrative is as extensive as Stone suggests, and questions in any case whether it constitutes a rejection of the earlier hopes for social history. The visible changes in historical writing, according to Hobsbawm, more likely represent:

1. experiments in presenting the *results* of complex historical analysis;
2. attempts at synthesis of those varied results;
3. the extension of the ideas and procedures of social history to areas of inquiry—notably political history—that had previously been left outside;
4. the desire to have a well-defined and sharply-portrayed social situation serve as the historiographical junction between large social processes and individual historical experience.

These factors, replied Hobsbawm to Stone,

> demonstrate that it is possible to explain much of what he surveys as the continuation of past historical enterprises by other means, instead of as proofs of their bankruptcy. One would not wish to deny that some historians regard them as bankrupt or undesirable and wish to change their discourse in consequence, for various reasons, some of them intellectually dubious, some to be taken seriously. Clearly some historians have shifted from "circumstances" to "men" (including women), or have discovered that a simple base/superstructure model and economic history are not enough, or—since the pay-off has been very substantial—are no longer enough. Some may well have convinced themselves that there is an incompatibility between their "scientific" and "literary" functions. But it is not necessary to analyse the present fashions in history entirely as a rejection of the past, and in so far as they cannot be entirely analysed in such terms, it will not do (Hobsbawm, 1980: 8).

The issue is squarely joined. On the one side, Stone interprets recent trends in the writing of history as signs of disillusion with what we must now, alas, call the old new social history, as augurs of the rise of the new old social history. On the other side, Hobsbawm sees the same trends, somewhat minified by comparison with Stone's estimates of them, as likely evidence,

that historians are now building on the accomplishments of the sort of social history that began to flourish in the 1960's.

Both our observers agree that historical practice has recently shifted, even if they disagree on the extent of the shift. They differ in their views of the attitude that shift reflects, and of the relation between the new practices and the old. On the whole, my reading of recent trends is closer to Hobsbawm than to Stone. I think, however, that Hobsbawm misses the extent to which historians of a decade ago oversold themselves on the explanatory powers of the social sciences, not realizing that those disciplines were much more effective in specifying what had to be explained and in ruling out superficial explanations than in producing explanations that could satisfy the average historian. The overselling made disappointment, and a new search for deep causes, inevitable. Hobsbawm also fails to bring out the paradoxical link between the demographic and economic determinisms that many of us began to favor in the 1960's and a sort of voluntaristic populism—a belief, in its simplest form, that ordinary people make their own history.

To a large extent, the dialectic of historical research, rather than alterations in historians' consciousness, accounts for the shifts in practice from the 1960's to the 1970's. We ought to take pleasure from the fact that the competing explanations of the shift themselves fall into a determinist and a mentalist mode. The debate between Hobsbawm and Stone recalls one of the old, fundamental disagreements about the natures of history, historiography, and social reality.

How the Models Matter: New (and Newer) Urban History

Should we care about these historiographic currents? I think so. They affect the definitions and justifications all of us offer for the historical enterprise. They influence the system of priorities and rewards we impose on each other. And, most important, they affect historical practice at its most vulnerable

point: the doctoral dissertation. The number of Ph.D.'s in history awarded each year in the United States is down from its early 1970's peak of more than 1,000 to the vicinity of 900. Yet it is probably still true that the majority of all person-hours devoted to professional historical research goes into the preparation of doctoral dissertations. It could well be true that the majority of all pages of professional history published report research undertaken for doctoral dissertations. (We can measure the perverse individualism and/or inefficiency of professional history by the fact that most dissertation-writers only acquire the essential skills of their trade—locating documents in archives, criticizing and synthesizing those documents, linking their findings to the existing literature, and so on—in the course of doing their dissertation research, and largely on their own.) The subjects and styles of those dissertations, so far as I can tell, respond much more decisively to shifting assessments of the viability of one sort of research or another than do the works-in-progress of the discipline's veterans. Students look to the future, and their teachers encourage them to take the risks. When history's authorities credit one model or discredit another, their colleagues often challenge them vigorously and sometimes modify their own practices in small ways. But it is their students who really change direction. Those students, even today, hold future practice in their hands.

My favorite example is quite germane to our general topic. It concerns the so-called New Urban History. Early in the 1960's, Stephan Thernstrom demonstrated that information from widely-available sources such as city directories and manuscript censuses could be reshaped into origin-destination tables similar to those sociologists used to analyze occupational mobility from father to son or within a worker's own career. (Thernstrom himself has graciously reminded us that Harriet Owsley, Frank Owsley, Merle Curti, and Sidney Goldstein had done some of the pathbreaking technical work; nevertheless, it was Thernstrom's *Poverty and Progress [1964]* that made young historians take notice.)

In the case of Newburyport, Massachusetts, Thernstrom produced evidence indicating that ethnic groups had not

simply differed in their rates of "success", but had adopted somewhat different strategies for securing their families' futures; that in the aggregate little occupational mobility occurred, but the net movement was slightly upward; that occupational mobility had not declined substantially over time; and that unskilled workers were very likely to move on—to leave the city—when they didn't move up. The demonstration attracted attention because of its technical virtuosity. It attracted attention because Thernstrom managed to expose the false historical assumptions sociologist Lloyd Warner (1963) had made concerning Newburyport (his famous Yankee City). Most of all, it attracted attention because it bore, at least indirectly, on great questions of American history: Was nineteenth-century America the land of opportunity? Did that promise fade for America's later immigrants? Did mobility and ethnic fragmentation reduce the chances for working-class militancy in the United States?

Graduate students were especially quick to see the promise of this new form of collective biography. Soon dozens of doctoral dissertations were in progress, pursuing the historical analysis of social mobility community by community, group by group, and source by source. At a famous meeting on the nineteenth-century city held at Yale University in 1968, Thernstrom and a crowd of collaborators—mainly youngsters, by the standards of the historical profession—identified themselves as a new school of historical practice. When Thernstrom and Richard Sennett edited the conference papers, they published their book with the subtitle *"Essays in the New Urban History"*.

Then, with a lag for the agonies of writing and rewriting, came the flood of theses, articles, and monographs: Philadelphia, Omaha, Chicago, Milwaukee, Boston, Birmingham, Los Angeles, San Francisco, Hamilton, Poughkeepsie, Troy, Kingston, and Buffalo crossed the viewing screen in the company of many other North American cities. Although the analysis of social mobility never generated the excitement elsewhere that it did in North America, collective biographers likewise began sorting out manuscript census records and similar sources as the means of reconstructing city populations in Europe and

other parts of the world. For the most part, that was what historians meant when they spoke of the New Urban History.

Looking back at this torrent of activity in 1975, Stephan Thernstrom commented wryly that "I am now inclined to believe that, just as the Holy Roman Empire was neither holy, Roman, nor an empire, the new urban history is not so new, it should not be identified as urban, and there is some danger that it will cease to be history" (Thernstrom, 1977: 44). He pointed out the dangers of thoughtless imitation, uncritical compilation of defective sources, bureaucratized team research, and slovenly sliding into the uncouth prose of social science. He did not, however, point out that the wave he had started was spending itself.

Why and how? First, the inherent limits of the one-city occupational mobility study were becoming increasingly visible. There was the difficulty of tracing the lives of people who arrived or left outside the city itself—and, for that matter, of distinguishing arrival or departure from erroneous recording and, more important, non-recording. There was the uncertainty that averages or variations over many cities, based on the occupational titles reported for adult males, actually represented the structure of American opportunity. There were the debatable assumptions about class and mobility built into the very method: that occupations formed a well-defined, unitary rank order; that the central issues concerned the rates and paths of achievement by individuals, families, and groups; that one could reasonably postpone the analysis of labor markets and employers' hiring strategies until the differentials were there to be explained. There were other technical and conceptual problems which critics and practitioners had uncovered.

Secondly, urban historians were finding the statistical answers yielded by their historical sociology unnecessarily thin. As Peter Decker put it, at the close of his own recent collective biography of San Francisco's white-collar workers in the nineteenth-century,

> The internal differences, if recognized at all by historians, are too often described only through statistical measures and techniques. Rarely are they explained within the social context in which they occur. This context includes the hopes, aspirations, and anxieties of those whose lives are being measured. To exclude these considerations, through the exclusive use of quantitative techniques, is to disregard how individuals perceive their own reality and to preclude any normative judgements regarding social mobility in a society (Decker, 1978: 250).

We have in Decker's statement a summary of thoughts that have been occurring to a great many historians—not just American urban historians—working in the shadow of social science. They have discovered facts we all should have recognized long ago: that statistical analyses in themselves almost never yield unambiguous conclusions; that the effective use of social-scientific procedures generally requires more, not less, explicit statement of arguments than the average historical account; and that on the whole, existing social-scientific approaches work far better at discrediting superficial explanations and at specifying what has to be explained than at generating explanations that historians are likely to find adequate. To the extent that an adequate explanation entails reconstructing historical actors' experiences of the situations in which they found themselves, collective biography and similar techniques (for all their power in other regards) hold little promise of yielding such explanations.

Alternatives and Syntheses

Not that all students of American cities had bitten as far into the statistical apple as the predecessors whom Decker castigates—urban labor historians, in particular, had managed to construct a kind of populist history that gave ample attention to the organization of work, the quality of life, the everyday struggles, and the forms of militancy. Alan Dawley's treatment of Lynn's leatherworkers (1976) combined the now-standard analyses of occupational mobility with searching examinations of belief and action. David Montgomery (1972) recreated the rhythms of work in Pittsburgh, as he and Bruce Laurie (1973, 1980) both revealed the patterns of organizations and competition underlying the brawls and protests of nineteenth-century

Philadelphia. A German, Dirk Hoerder (1977), discerned the doctrines of popular sovereignty embedded in the workers' riots of revolutionary Massachusetts. Herbert Gutman (1976) mounted a great quest for working-class culture and the making or unmaking of that American working class. Gutman and others, in fact, drew a good deal of their inspiration from European social and labor historians such as E. J. Hobsbawm, George Rudé, Michelle Perrot, and E. P. Thompson.

American urban history involves much more activity I haven't mentioned: the enormous concatenation of studies of nineteenth-century Philadelphia coordinated by Theodore Hershberg (1978, 1979); Oliver Zunz's exquisitely fine analyses (1977, 1982) of land use and population distribution in Detroit; Allan Pred's treatments (1973) of the time-geography of American urbanization; examinations of American urban migration, of the development of racial segregation, of urban women's work experiences, of job-finding and the creation of occupational communities, of power and class in our cities, and more. "Urban history" overflows its banks, and spreads into the whole plain of American social life. Any one generalization about urban history therefore invites at least two exceptions.

Yet in very general terms my description holds: In most precincts of American urban history, the 1960's brought a quickening of enthusiam for self-conscious conceptualization and modeling, for deliberate (and often quantitative) comparison of multiple units, and for rigorous measurement; this sort of enterprise, with its social-science overtones, tended to separate from the fine qualitative studies of individual and group experience that continued; as the 1970's moved on, more and more doubts about the adequacy of the social scientific model arose among its followers and its critics, and urban historians tried increasingly either to enrich their pallid collective biographies with colorings of individual experience or to discover thicker alternatives to thin conclusions about social mobility and stratification.

The model applies least well to the fields of historical work that developed in closest concert with specific social sciences:

archeology, economic history, and demographic history. Researchers in these fields tended—and still tend—to get the bulk of their training outside of history proper, and to follow the intellectual agendas of the social sciences rather than of history. For better and for worse, that orientation to social science insulated them from the priorities and pressures of other historians. Even in these fields, nevertheless, some shifts occurred—away from the dazzling chrome-and-glass constructions of the 1960's, let us say, toward the more elegant, subdued wood and brass of the late 1970's. Economic historians who were thoroughly conversant with economic models and econometrics, such as Jan de Vries, the author of a superb text, *The Economy of Europe in an Age of Crisis* (1976), showed that they cared to root their analyses in the time, place, and historiography of the changes they were studying.

On the demographic side, Keith Wrightson and David Levine have given us the splendid example of their book (1979) on an English village, Ierling (Essex), from 1525 to 1700. A collective biography of the entire recorded population over the two centuries forms the book's backbone. The fine demographic reconstruction provided a sensitive index of changing fortunes among different classes of the population, as well as some signs as to the character of local social structure. It showed, for example, "that in Ierling the age at marriage and fertility, and not mortality, were the prime agents of demographic control. While the short-run implications of epidemic mortality were of real consequence, they were of little importance in the long run" (Wrightson & Levine, 1979: 72). Thus the strict Malthusian picture of old-regime populations periodically decimated by plague and famine because they outgrew their resources fails to fit the facts.

Yet all was not bucolic harmony in Ierling: the demographic evidence likewise reveals the growth of a large class of poor rural laborers, the increasing division of the parish between landed haves and landless have-nots. That is where Wrightson and Levine provide us with a model for the local social history of the future. For instead of resting with their impressive demographic evidence, they delved into court records, church

records, tax records, and every other scrap they could get their hands on in order to trace the material conditions of existence, the routines of everyday life, the structures of power and punishment, and the affirmations of faith and disbelief. Never have we seen more clearly the emergence of a confident, comfortable class of local notables in pious, sober, responsible, but (above all) firm control of the many hirelings who worked their land. Never have we had better displayed the mechanisms and consequences of the processes of rural proletarianization that took place so widely in Europe.

Wrightson and Levine did an extraordinary piece of work, but their general style of analysis was not unique. Alan Macfarlane and his collaborators (1978) have undertaken a similarly comprehensive—and computer-coordinated—analysis of a single parish, Earls Colne, from 1400 to 1750. Jean-Claude Perrot (1975) has made the demographic history of eighteenth-century Caen the thread for the stitching together of the city's whole round of life. As Hobsbawm (1980) suggests, these new, thick, demographically informed community studies do not represent an abandonment of analytic history. They show us skilled analysts broadening the range of their analyses, and seeking effective ways to communicate their results.

Is Crassness American?

Wrightson and Levine are not Americans or American-trained; the Briton and the Canadian learned their demographic history in a Cambridge that has for years been a fount of the art. E. A. Wrigley of Cambridge and Louis Henry of Paris, very likely the two most influential figures in the creation of the demographic history we know today, have wider followings in Britain, France, and the rest of Europe than in North America. In this field, as in the labor history over which such figures as E. J. Hobsbawm, E. P. Thompson, and their allies have exercised so great an influence, Europeans have commonly led the way. Although critics, European and American, of quantification and social-scientific models in history have sometimes portrayed them as quintessential expressions of American vulgarity

and imperialism, the initial impulse to both has often come from Europe in fact, and their fullest versions have commonly appeared outside of the United States.

The situation resembles the paradoxical processes by which almost every city of the Roman Empire, except Rome, received a "Roman" ground plan, with its *ordo* and *decumanus*, or by which the purest specimen of French feudalism, with grants actually extending through a chain of subordinates from sovereign to peasant, appeared not in France but in Quebec. For the really massive building of centralized, team-operated, computer-based files of "process-produced" historical data, we go to Germany. For the creation of national demographic series extending over centuries of experience, we go to France and Britain. For the coordination, standardization, and computer linking of large numbers of demographically-based community studies, we go to Sweden. Perhaps the most surprising case is this one: if current signs are reliable, almost unimaginably large files of evidence on historical population changes will soon start to become available in, of all places, mainland China. By comparison with these efforts, American forays into historical compilation and computation look modest indeed.

Let me not exaggerate. When one of these large enterprises has taken shape, Americans have usually appeared somewhere on the scene. For example, American Ronald Lee has figured importantly in the Cambridge Group's work on reconstructing English population trends, and American James Lee (no relation) is playing an important part in Chinese surveys of their sources for demographic history. Furthermore, some rather large American undertakings have strongly influenced research through the rest of the world. Two examples are the country-by-country analyses of fertility decline conducted by the Princeton group Ansley Coale created in the 1960's (1979), and the huge collections of machine-readable evidence created by the Michigan-based Inter-University Consortium for Political and Social Research since the 1950's. Still, the American reputation for Big Data and bigger research teams has been greatly exaggerated. In international perspective, the American historical profession includes more than its share of individual

investigators, carrying their handwritten notes about with them, and using no machines more exotic than a typewriter or photocopier.

Will Anthropology Save Us?

American individualism may help explain one of the major reactions to the alleged excesses of social-scientific history: the self-conscious turn to anthropology as a guide to historical reconstruction. The "anthropology" in question has an odd connection to the discipline that goes by that name, with its controversies over evolution and materialism, its debates over the origins of ideologies of honor, its exploration of the intricacies of kinship and language, and its chronicles of the rise and fall of peasantries and rural proletariats. We might better call the anthropology to which a number of historians have been turning their hopes "retrospective ethnography". The idea is to recreate crucial situations of the past as a thoughtful participant-observer would hve experienced them. Some advocates of retrospective ethnography adopt the Gilbert Ryle-Clifford Geertz program of "thick description"; they tend to hold up as exemplars Natalie Davis's dramatic reconstructions (1975) of sixteenth-century festivals and follies, not to mention Geertz's own portrayal (1973) of a Balinese cockfight. William Sewell has recently written a book (1980) about French workers in the era of the Revolution, which pivots an analysis of changing conceptions of property and group identity on the Geertzian idea that alterations in fundamental concepts are the bases of deep social change, and that those alterations show up in the language of claims and contention. More such efforts are to come.

Although the phrase "retrospective ethnography" has not gained any currency in the historiographical literature, historians following this path often make a deliberate point of their turn to anthropology, and of their dissatisfaction with the old new social history. Sewell himself explicitly invokes cultural anthropology and Clifford Geertz, and self-consciously describes his move away from the structures and determinisms of

standard social history. The preface to Bryan Palmer's study (1979) of skilled workers in Hamilton, Ontario, from 1860 to 1914 includes an exceptionally clear statement of alternatives. It deserves quotation at length:

> Hamilton, as many social historians are well aware, has become one of the most intensely studied communities in North America. Michael B. Katz and his ongoing Canadian Social History Project have utilized quantitative data to launch one of the more sophisticated community studies in the history of social scientific inquiry. While Katz's work demands respect, particularly his structural analyses of inequality, transiency, and social mobility, it remains an open question as to how much numerical data can tell us about culture or conflict. It thus seemed fitting to probe traditional sources (newspapers, manuscript and archival holdings, and local records) to see what they could offer. While such material is truly impressionistic, it has yielded an impressive collection of data that tell us much about obscure corners of the nineteenth- and twentieth-century world.
>
> Beyond the data, however, looms the theoretical framework within which this study evolves. While sections of the book have been somewhat influenced by my wrestling with a kind of structuralist theory . . . the attachment is to a structuralism rooted in historical analysis, informed but not dominated by the approach of the anthropologist. It is, in short, the structuralism of Levi-Strauss, rather than the structuralism of Althusser. Where one has, at least, a partial respect for history and empirical findings, the other is unashamedly antihistorical, masking abstraction with the reification of theory.
>
> This study, then, is no marriage of the social sciences and history. If it does not totally accept the judgement of Richard Cobb that it is unlikely that historians will ever get much profit from the company of social scientists, it cannot argue with Elizabeth Fox-Genovese's and Eugene D. Genovese's recent remarks on the dangers inherent in promiscuous "borrowing" from other disciplines. Far too often, the historian's own lack of rigor has moved him toward the sociologist, the psychologist, the economist, or the anthropologist; and the theoretical gains have been minimal. These advocates of the interdisciplinary approach have often succumbed to the worst kind of defeatism, for in looking for answers to history's interpretive problems they have subordinated Clio to the jargonistic antihumanism of the social sciences, replete with their clinical sterility and elaborate control mechanisms. The past, however, was never so tidy (Palmer, 1979: xii-xiii).

Palmer calls, instead, on the tradition of "empirical Marxism" exemplified by E. P. Thompson. Culture and conflict are

to be his themes, sympathetic reconstruction his method. Although Palmer does not summon Clifford Geertz to testify against the impoverished rigidity of social-scientific history, he does advocate a program of thick description.

The best-known recent example of retrospective ethnography, however, has less to do with Clifford Geertz, and more to do with the old-fashioned village study. Emmanuel Le Roy Ladurie's spectacular *Montaillou* (1975) gives an account of life and love in a fourteenth-century Pyrenean community. It follows an outline that could easily have guided an ethnography done 50 years ago: sex, courtship, marriage, life-cycles, gatherings, forms of solidarity, and on down the checklist. (It would convey the texture of the book a bit more faithfully—and explain some of its best-selling appeal—to enumerate the subjects as sex, courtship, sex, marriage, sex, life-cycles, sex... and so on.) But Le Roy Ladurie does the standard ethnography with exceptional panache, and with an extraordinary source: the transcript of the inquisition's searching interrogations of local people. Montaillou nurtured heresy; the inquisitive bishop sent to track down the heretics followed the trail of mistaken belief into the routines, crises, and peccadillos of day-to-day life. Le Roy Ladurie had the cleverness to handle the transcript like an oral-history tape, splicing its testimonies together with his own commentaries, comparisons, and speculations. Result: a revelation. The reader finds himself in the very midst of a weird, earthy, and yet somehow familiar round of life.

Le Roy Ladurie did not, to be sure, invent the method entirely on his own. Ethnographers such as Oscar Lewis (e.g., 1961) had long since substituted tape recorders for notebooks, and inserted long strips of their taped interviews into their books on rural and urban life. A whole guild of "oral historians", with its public running from general readers to antiquarians through the students of recent history, has sprung into being. Within French history, Le Roy Ladurie had the splendid example of Alain Lottin, who built a broad reconstruction (1968) of Lille's seventeenth-century social life on the base of a journal kept by a modest textile artisan. Instead of settling for an edition of the text with a long introduction and

learned footnotes, Lottin chose to interweave the phrases of the journal with his own portrait of the man, his milieu, and the city as a whole. The portrait relies on the standard sources of demographic, economic, and institutional history. Lottin's *Chavatte, ouvrier lillois* therefore lies somewhere between the structural history of Levine and Wrightson and the retrospective ethnography of Le Roy Ladurie.

Yet another variant of anthropological work has appeared in the history of women and feminism. Ethnographers often put a great deal of thier effort into noting the concrete connections within some important segment of the population. Similarly, some of the most-read American research on women—for example, the writings of Carroll Smith-Rosenberg (1975) and Nancy Cott (1977)—attempts to reconstruct the networks and solidarities linking women to each other. The tracing of interpersonal networks ranges from informal to precise, just as it does in anthropology. In both its historical and its anthropological version, the network analysis serves two purposes: first, to clarify how members of the group cope with difficulties they face in other areas of their lives; secondly, to help explain the solidarities and conflicts that show up in public affairs. This approach becomes controversial, obviously, to the extent that it reduces women's public claims to expressions of their private preoccupations. Competing historiographical traditions, after all, base women's involvement in the struggles for abolition, suffrage, and women's rights on the articulation of real interests, on the development of a solidary, self-conscious social movement, or both.

A similar division appears in the history of the family. On the "anthropological" side (to stretch the term a bit), we have writers such as Philippe Aries, Randolph Trumbach, Edward Shorter, and our mentor, Lawrence Stone. Although they disagree among themselves in many regards, they converge on the interpretation of changes in family life as expressions of change in attitudes, *mentalitiés, Weltanschauungen*. Thus for Aries the rise of overriding individualism in our own era has broken the old solidarity of the family. On the "materialist" and "political" sides (to use a pair of equally tendentious terms), we have such interpretations as that of Louise Tilly and

Joan Scott, who portray alterations in family structure under industrial capitalism as collective shifts in strategy conditioned especially by changes in the organization of production. Since contemporary anthropology actually contains energetic spokesmen for "materialist" and "political" accounts of social life, the distinction between anthropological and other approaches to social history begins at this point to lose all clarity. Nevertheless, the distinction remains. It represents an old division within anthropology itself: between those who, on the whole, give explanatory priority to culture, belief, or will, and those who give priority to material conditions and power.

Materialism Lives

Despite all I have said, materialism has by no means disappeared from social history. As Hobsbawm's reply to Stone indicates, social and economic historians have been trying to sort out and synthesize the mass of new evidence that has been accumulating on the world's large economic, political, and social transformations. For European history since 1400 or so, the grand themes have been the concentration of capital, the growth of a proletarian labor force, the creation of powerful national states and systems of states, the emergence of mass politics at a national scale, the rise and fall of European hegemony, the decline of fertility and mortality. Hobsbawm himself has made important contributions to the synthesis. Far from withering away, the discussion of these themes is gaining in coherence and energy.

By and large, this work (like Hobsbawm's) is broadly Marxist in conception: at a minimum, it begins with analysis of the organization of production and its implication for class formation. On the small scale, the work of Wrightson and Levine exemplifies the sort of synthesis that has its counterparts in other work on England, France, Germany, and Sweden. On the large scale, promising recent syntheses take the form of Kriedte, Medick, and Schlumbohm's essays on protoindustrialization (for all their loose ends), of Lis and Soly's survey of

poverty and capitalism in Europe (for all its lack of attention to variation from region to region), of Immanuel Wallerstein's second volume on the seventeenth-century European world-system (for all its controversial treatment of the "strength" of different seventeenth-century states).

The award for the most sumptuous (if not for the most conclusive) recent synthesis goes hands down to Fernand Braudel's giant three-volume exploration of capitalism and material life from the fourteenth century onward. Braudel's scope extends beyond Europe to the world as a whole. He takes in almost all the social history I have been reviewing, and more. Demography, technology, communication, geography, politics, and cultural production flow together, and through each other, in his account. Braudel finds parallels, common threads, and interdependencies where the rest of us barely dare to venture factual summaries. Hard to classify as a Marxist—or as anything else—Braudel nonetheless comes through as a thoroughgoing materialist. That materialism appears at each of the three levels treated by the book's successive volumes: the routines and constraints of everyday life; commercial structures and capitalism; world economies and interdependence.

Two decades ago, Braudel's rambling survey of the sixteenth-century Mediterranean displayed an extraordinary sense of the interdependence among structures and changes that seemed remote from one another, or even antithetical—for instance, the rise and fall of upland banditry as a function of fluctuations in lowland state power. Now he conveys that same sense at a scale that dwarfs the Mediterranean and the sixteenth century: his subject has become the experience of the entire world from the fifteenth through the eighteenth centuries. Even those four centuries do not contain him, as he moves backward to the Roman Empire and forward to the 1970's. In three bulging volumes, Braudel attempts no less than a general account of the processes by which the capitalist world of the nineteenth and twentieth centuries took shape.

Braudel's account lacks the schematism of the work of an H. G. Wells or a V. Gordon Childe. Complexities, nuances, con-

tradictions, and doubts fill every chapter. The marvelous, abundant illustrations—plates, graphs, maps, diagrams, and tables by the hundreds occupy about a fifth of the text—nearly always lend new insights, yet rarely fall neatly into a developing argument. Indeed, Braudel often makes an explicit distinction between his procedure and the assembling of evidence for a connected set of propositions. As he begins a survey of a number of instances in which agricultural capitalism became dominant, for example, he declares that "our aim is not to study these different cases for their own sakes or to seek the means of preparing an exhaustive list for the whole of Europe; we only want to sketch a line of reasoning" (Braudel, 1979: II, 245).

As crystallized in titles and subtitles, the topic's three divisions run as follows: (1) material culture and the structure of everyday life; (2) economy and the workings of exchange; (3) capitalism and world-time. The breakdown does distinguish the emphases of the three volumes. It does not, however, reflect a causal hierarchy or a tight analytical model that we shall see clearly as we work our way through one volume after another.

In the first part, Braudel seeks to describe how the techniques of production, distribution, and consumption varied throughout the world—especially the Western world—over the four centuries after 1400, and to show how those techniques shaped everyday experience. The first volume reveals the richness of Braudel's reading and reflection. Backed by his engaging and well-produced illustrations, he gives us disquisitions on epidemics, on agricultural techniques, on the varieties of herring, on the vagaries of clothing style. Yet a careful reader encounters surprises and disappointments. For one thing, it eventually becomes clear that—despite the ample demographic documentation on which he draws—Braudel has little concern with vital processes as such. The opening section on population avoids most of the questions on which European historical demography has focused: the responsiveness of vital rates to economic fluctuation, the relationship between household structure and

fertility, the onset of long-term declines in fertility, and so on. Braudel concerns himself with population size, growth, and decline mainly as indices of power, welfare, and vulnerability to the environment.

Again, as the volume proceeds Braudel builds up a case for inefficient transportation as a major brake on European economic growth. Yet he never quite manages to reconcile that conclusion with his earlier portrayal of the Mediterranean shipping routes as speedy "liquid roads", or with the sort of evidence Jan de Vries has assembled concerning the great importance of low-cost water transport in the economic development and communication structure of the Low Countries. At a minimum, one might have expected a comparative analysis of the advantages enjoyed by regions that had access to navigable rivers, canals, and seas.

Most of all, Braudel tantalizes his readers by raising fundamental questions, then leaving the questions to levitate themselves. One example is his discussion of Lewis Mumford's claim that nascent capitalism broke up the narrow frame of the medieval city by substituting the power of a new merchant aristocracy for that of landlords and guildmasters: "No doubt, but only to link itself to a state which conquered the cities, but only to inherit the old institutions and attitudes, and entirely incapable of doing without those institutions and attitudes" (1979: I, 453). Another is the conclusion of a long, informative treatment of the variants and interactions of money and credit: "But if one can maintain that all is money, one can also claim, on the contrary, that all is credit: promises, reality at a distance. ... In short, the case can be made first one way, then the other, without trickery" (1979: I, 419). Indeed, the so-called "conclusions" of the entire volume have the same ambivalent tone, with an additional note of complaint about the inadequacy of the available evidence:

> I would have liked more explanations, justifications, and examples. But a book is not indefinitely expansible. And in order to pin down the

> multiple aspects of material life, it would require close, systematic studies, not to mention whole sets of syntheses. All that is still lacking (1979: I, 493).

Five hundred pages into a dense compilation-cum-synthesis, one wonders.

In the second volume, Braudel proceeds from a survey of the techniques by which people in different parts of the world exchanged goods to a discussion of various types and scales of markets. He then tries to identify the peculiarities of capitalism as activity and organization, before examining its articulation with social hierarchies, state structures, and broad forms of civilization. Despite a thick and thoughtful survey of definitions, Braudel never quite lays out a working definition of the capitalism he has in mind. It takes a while to see that he has chosen to emphasize the conditions of exchange rather than the relations of production; he has thus aligned himself, among recent combatants on that bloody field, with Immanuel Wallerstein and André Gunder Frank, and separated himself from analysts such as Robert Brenner and Witold Kula. In response to Kula's claim that the landlords who "refeudalized" eastern Europe did not, and could not, calculate as capitalists, Braudel declares:

> To be sure, that is not the argument I wish to challenge. It seems to me, however, that the second serfdom was the counterpoint of a merchant capitalism which took advantage of the situation in the East, and even, to some extent, based its operation there. The great landlord was not a capitalist, but he was a tool and a collaborator at the service of the capitalism of Amsterdam and other places. *He was part of the system* (1979: II, 235).

What, then, is that capitalist system? Gradually, Braudel reveals a vision of capitalism as an arrangement in which two or more large, coherent, market-connected "economic worlds" become linked and interdependent through the agency of big manipulators of capital. Thus, in European history, the role of *grand commerce* in the development of capitalism becomes paramount. Thus, in Braudel's view, a single capital-concentrating metropolis tends to emerge as the dominant center of any capitalist world-economy.

Braudel's tack moves us in a very different direction from the identification of capitalism as the system in which the holders of capital control the basic means of production, and reduce labor to a factor of production, a commodity one buys and sells; in that sort of definition, the confrontation of a capitalist with a proletarian—a person who depends for survival on the sale of labor power—occupies the very center. With Braudel, we do not recognize capitalism by its characteristic social relations, but by its general configuration. With the alternative, we recognize a capitalist system by the prevalence of a social relationship that we can observe at the smallest scale. It is the difference between a blancmange and a Saint-Honoré: the smallest spoonful of the almond jelly is still blancmange; but unless crust, cream, and iced puffballs come together in the right pattern, you have no Saint-Honoré. Paradoxically, with Braudel's Saint-Honoré capitalism, once we have identified the dish as a whole, every part of it qualifies as Saint-Honoré. That is how Braudel can say of the noncapitalist landlord: "He was part of the system."

The exchange-oriented definition has some analytical advantages. For one thing, it trains attention on the enormous importance of bankers, merchants, and other capitalists who knew nothing of production but plenty of prices and profits; their activities greatly facilitated changes in the relations of production. For another thing, the exchange-oriented definition brings out the continuity between small-scale and large-scale production under capitalism, and thus reduces our fixation on factories, large firms, and labor under conditions of intensive time- and work-discipline; the concentration of capital and of work-spaces certainly made a difference to the autonomy of workers and the quality of work, but cottage industry and related forms of production often proceeded in a thoroughly capitalist manner. The exchange-oriented definition of capitalism steers far clear of a misleading emphasis on the technology of production.

Still, the disadvantages of Braudel's definition outweigh the advantages. The definition, in turning away from technology, abandons the relations of production entirely. Enco-

mienda, hacienda, slavery, and, as we have seen, serfdom all become capitalist forms of labor control. Large chunks of world experience become capitalist. The historically-specific analysis of the development of capitalism as a system gives way, paradoxically, to the very inquiry it was supposed to replace: the search for explanations of the British and western European "takeoff".

In fact, Braudel gives some signs of compromising the excessive broadness of his definition; in this regard, as in many others, he neglects to stick to his announced principles throughout the inquiry. Having committed himself to a conception of capitalism involving the linkage of two or more large, distinct markets by capital-wielding merchants, he has already committed himself to seeing the whole of those markets as integral elements of a capitalist system. Yet he persists in searching within those markets for signs of the emergence of capitalism. Thus he declares for the end of the old regime that "The majority of the peasant world remained far from capitalism, its demands, its order, and its progress" (1979: II, 255). Thus he concludes that "Capitalism did not invade production as such until the moment of the Industrial Revolution, when mechanization had transformed the conditions of production in such a fashion that industry become an arena for the expansion of profits" (1979: II, 327). If consistency be a hobgoblin of little minds, Braudel has no trouble escaping that particular demon.

When Braudel is not bedeviling us with our demands for consistency, he again parades his indecision. Throughout the second volume, he repeatedly begins to treat the relationship between capitalists and statemakers, then veers away. Savor this summary of his efforts:

> Finally and especially, must we leave unanswered the question which has come up time after time: Did the state promote capitalism, or didn't it? Did it push capitalism forward? Even if one raises doubts about the maturity of the modern state, if - - moved by recent events—one keeps one's distance from the state, one has to concede that from the fifteenth to the eighteenth century, the state was involved with everyone and everything, that it was one of Europe's new forces. But does it explain everything, subject everything to its control? No, a thousand times no. Furthermore, doesn't the reverse perspective work as well? The state favored capitalism and came to its aid—no doubt. But let's reverse the equation: the state checks the rise of capitalism, which in its turn can harm the state. Both things are true, successively or simultaneously, reality always being predictable and unpredictable complexity. Favorable, unfavorable, the modern state has been one of the realities amid which capitalism has made its way, sometimes hindered, sometimes promoted, and often enough moving ahead on neutral ground (1979: II, 494).

Yes, it appears, we *must* leave unanswered the question that has come up time after time. When we arrive at the same point again and again, we begin to suspect we are walking in circles. That is the price, I suppose, of walking with a great muser.

The third part of Braudel's *magnum opus* begins with a delineation of world-economies as the fundamental units of analysis, and continues with a roughly chronological portrayal of the successive world-economies that prevailed in Europe and elsewhere in the world. The survey is complicated by the simultaneous efforts to specify the changing places of smaller areas and individual cities within those world-economies, to trace the interactions among world-economies and—as if that were not already enough—to explain how and why Europe finally became the world's master and its prime locus of large-scale industrialization. Here especially Braudel lets shine a scintilla of sentimental chauvinism: Why did France never quite become Number One? At one moment, Braudel permits himself the speculation that the demands of Paris were to blame. In the mid-sixteenth century:

> Did Paris miss the chance to acquire a measure of modernity, and France with her? That is possible. It is permissible to blame Paris' possessing classes, overly attracted to offices and land, operations which were "socially enriching, individually lucrative, and economically parasitic" (1979: III, 280; the quotation is from Denis Richet).

Fortunately, Braudel's gloom never lasts long. Soon he sets off on a knowledgeable exploration of the changing regional divisions within the French economy—one of the finest

surveys of the subject anywhere. That conversational mode provides both the charm and the frustration of the volume.

Precisely because the conversation ranges so widely, a look back over the third volume's subject matter brings out an astonishing fact: the grand themes of the first volume—population, food, clothing, technology—have almost entirely disappeared! Despite the sense of material life as a constraint on human choices so well-conveyed by that first volume, now we seen nothing of constraint. Braudel's discussion of the peopling of North American colonies (1979: III, 348ff.), for example, involves no effort whatsoever to judge the contributions of changes in fertility, mortality, nuptiality, migration, or their relations to each other. Indeed, by this point Braudel has become so indifferent to population problems that he settles for graphs of English fertility and mortality changes (1979: III, 489) drawn from G. M. Trevelyan's ancient text on social history. Despite contrary indications in the opening volume (and despite the crucial place of Braudel's collaborators in the development of demographically based social history), Braudel makes no significant effort either to analyze demographic dynamics or to incorporate them into his explanatory system. Somehow that no longer seems to be part of the problem.

What is? Early in Volume II, Brandel calls his readers' attention to a perplexing situation. In the sixteenth century, he concludes,

> the thickly settled regions of the world, subject to the pressures of large populations, seem close to one another, more or less equal. No doubt a small difference can be enough to produce first advantages, then superiority and thus, on the other side, inferiority and then subordination. Is that what happened between Europe and the rest of the world?. . . One thing looks certain to me: the gap between the West and the other continents appeared *late*; to attribute it to the "rationalization" of the market economy alone, as too many of our contemporaries still have a tendency to do, is obviously simplistic. In any case, explaining the gap, which grew more decisive with the years, is the essential problem in the history of the modern world (1979: II, 110-11).

The suggestion, tucked into Volume I, that a difference in energy supplies between Europe and the rest of the world might

have been crucial, has by this time vanished. The action of the state has, as we have seen, dissolved as a likely explanation. China, India, and other parts of the world turn out to have created commercial techniques as sophisticated as those of the Europeans. Paul Bairoch's estimates of gross national products at the end of the eighteenth century (quoted with a mixture of consternation and approval in a stop-press revision inserted at III, 460-461) show no significant advantage of western Europe over North America or China—so "initial advantage" loses its remaining shreds of credibility as an explanation.

By page 481 of Volume III, Braudel offers an indirect admission of theoretical defeat: "[T]he Industrial Revolution which overturned England, and then the whole world, was never, at any point in its path, a precisely delimited subject, a given bundle of problems, in a particular place at a certain time." All the previous history recounted in this vast review, Braudel tells us, somehow converged on that outcome. The only way to analyze industrial growth is to break it into its many elements, to take up those elements one by one, and to trace their multiple connections. That Braudel's earlier analyses forecast just such an intellectual strategy, and that Braudel follows the strategy with subtle brilliance, do not eliminate a certain disappointment that no answer whatsoever emerges from all this powerful questioning.

At the start of the third volume, it looks as though Braudel will try to perform his miracle by relying on Immanuel Wallerstein's model of the European world-system, especially its distinction of core, semiperiphery, and periphery. But Braudel eventually opts for a more relaxed identification of the world's economically independent regions, leans against Wallerstein's claim that the European capitalist world-economy was the first one not to consolidate into a political empire, doubts that empires as such stifle the potential of world-economies, and maps out multiple European world-economies well before the supposedly critical unification of the sixteenth century. He follow Wallerstein especially in building his account around the successive hegemonies of capitalist metropolises: Venice, Genoa, Antwerp, Amsterdam, London, New

York. He accepts, for a while, Wallerstein's unconventional characterization of the seventeenth-century Dutch and English states as "strong" states, on the ground that their modest apparatus demonstrated the efficiency with which their dominant classes could work their will. When self-conscious about the problem, he remains faithful to Wallerstein's focus on conditions of exchange, rather than relations of production, as the essential features of capitalism. But in fact he neither uses the core/semiperiphery/periphery scheme as a tool of analysis nor attempts to test it by means of his store of information. It is a grand story, elegantly told . . . and nothing like a definitive solution to the "essential problem". Whatever else it does or fails to do, however, Braudel's grand survey establishes the enormous importance of material conditions in the evolution of European life. Materialism lives.

Conclusion

The point is more general. Notice, as you read Braudel and other syntheses, how little they exemplify the revival of narrative, how rarely they rely on retrospective ethnography, how much they build their cases on the very quantitative, demographic, and social-scientific works that Lawrence Stone has condemned to bankruptcy. Somehow they refuse to go broke. Works of the old new social history have not, it is true, locked together in the Scientific History Lee Benson once anticipated. They have, on the contrary, made the historical specificity of social structures and processes all the more apparent. But the old new social history has made it possible to connect individual experience with large social processes more clearly, precisely, and fully than ever before. Research and writing in that vein continue to thrive in economic history, in the history of the family, in demographic history, in the history of popular rebellion and collective action, in the history of schooling and literacy, in historical studies of poverty, aging, genetics, migration, crime, strikes, ethnicity . . . even in urban history. The practitioners of the old new social history have

found it perfectly feasible to incorporate into their model-mongering, comparative, quantitative, collective-biographical endeavors the devices and insights of retrospective ethnography. We must end up agreeing with the Lawrence Stone of 1972, if not of 1979, and with the E. J. Hobsbawm of 1980: the mission of social history is still to "tie the exciting development in intellectual and cultural history down to the social, economic, and political bedrock."

References

This bibliography includes every item cited in the text, at least one representative item by each author mentioned, a number of recent writings on historiography, plus a variety of recent works in social history. Social historians have been producing publications at far too fast a pace for this to be anything like a comprehensive inventory, and my reading is too selective for it to be a representative bibliography. I have tried simply to provide two or three examples of each kind of research discussed in the article, plus twenty or thirty titles giving the flavor of the social history being produced in the 1970's.

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Class Formation on a World Scale

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Things will improve. Sure they will.
There's no point in grumbling.
Life isn't meant to be easy;
not that it ever was.
We're not too badly off.
The family has all it needs,
a house, and a car,
they can't complain.
The kids are doing well at school;
the girl's in high school, the boy's at university.
We've got a colour T.V., orchids
and air-conditioning. Pray together sometimes.
Yes, you could say we've made it.
No use making a fuss,
could get you into trouble.
Only fools protest, it doesn't do any good.
A man could even lose his job.

It could be Australia; the third line is our present Prime Minister's most-quoted saying. It could be Reagan's North