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October 1980

CRSO Working Paper No. 218

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Revised version of a keynote address to the Conference on New Directions in History, State University of New York At Buffalo, 3-4 October 1980

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 which 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 1960s, many historians felt that historical theory and practice alike were undergoing great changes. Some felt the changes to threaten the proper performance of the historian's function: Jacques Barzun, 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 "the 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" (Benson 1970 [1966]: 29). 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, they had learned.

Participants in that 1968 meeting disagreed about the future of the past. Yet they agreed about the desirability of discussion. So Daedalus flew on 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 1960s' 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 1960s 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; fixation on reliable forms of measurement, frequently involving numerical treatment of evidence. Economic 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 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 catalog 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, there 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 and 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" which 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 which had simply acquired a new range of applications. It was "the investigation of the common background characteristics of a group of actors in history by means of a collective study of their lives" (Stone 1972: 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 1972: 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. It was not, of course, entirely new: Sir Lewis Namier had long since biographized eighteenth-century Parliaments, Roman historians had been perpetrating prosopography for decades, and collective biography is one name for Crane Brinton's old book on the Jacobins. Nevertheless, at least four features distinguished the collective biography begun in the 1940s 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 which had captured the historians of the 1960s 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 onee, said Stone, claimed to be on their way to cast-iron solutions

> for such hitherto baffling questions as the causes of "great revolutions" or 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 caused the decline of analytic history? Stone catalogs these causes:

1. 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;

2. revived awareness of the importance of political and military power;

3. the mixed record of quantitative work, especially when carried out by large research teams, based on 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 investigators 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, 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 which took shape in the 1960s 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 doubts that the revival of narrative is so 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 analyses;

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 -- which had previously been left aside; and

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 consequences, 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 1960s.

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 resembles Hobsbawm's more than Stone's. 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 which many of us began to favor in the 1960s, 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 1960s to the 1970s. 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 historiographical 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-70s peak of more than a thousand 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 topc. It concerns the so-called New Urban History. (During the meeting for which I prepared the earlier version of this essay, Michael Frisch handed me an already-published article of which I was unaware; it makes all the major points which follow, and more, with far fuller documentation: Frisch 1979. I don't know whether to be pleased with the independent confirmation of my analysis, or dismayed at the duplication of effort.) Early in the 1960s, 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. (Therstrom 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 <u>Poverty and Progress</u> 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 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 century 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, yes, Buffalo arrived under the microscope 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, or 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.

Second, 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 judgments regarding social mobility in a society (Decker 1978: 250).

We have in Decker's statement a summary of the thoughts which 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; 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 which 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 which gave ample attention to the organization of work, the quality of life, the everyday struggles, the forms of militancy. Alan Dawley's treatment of Lynn's leatherworkers combined the now-standard analyses of occupational mobility with searching examinations of belief and action. David Montgomery recreated the rhythms of work in Pittsburgh, as he and Bruce Laurie both revealed the patterns of organization and competition underlying the brawls and protests of nineteenth-century Philadelphia. A German, Dirk Hoerder, discerned the doctrines of popular sovereignty embedded in the workers' riots of revolutionary Massachusetts. Herbert Gutman 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; Olivier Zunz' exquisitely fine analyses of land use and population distribution in Detroit; Allan Pred's treatments 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, 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 1960s brought a quickening of enthusiasm 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 which continued; as the 1970s 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 which developed in closest concert with specific social sciences: archeology, economic history, 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 1960s, let us say, toward the more elegant, subdued wood and brass of the late 1970s. Economic historians who were thoroughly conversant with economic models and econometrics (such as Jan de Vries, the author of a superb text on The Economy of Europe in an Age of Crisis) 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 on an English village, Terling (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 Terling 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 and 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 Terling: 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, manorial 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, 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 which 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 have undertaken a similarly comprehensive -- and computer-coordinated -- analysis of a single parish, Earls Colne, from 1400 to 1750. Jean-Claude Perrot 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 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 which 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 of social-scientific models in history have sometimes portrayed them as quintessential expressions of American vulgarity and imperialism, in fact the initial impulse to both has often come from Europe, 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 led Ansley Coale created in the 1960s, and the huge collections of machine-readable evidence created by the Michigan-based Inter-University Consortium for Political and Social Research since the 1950s. 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?

That 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 which goes by that name, with its controversies over evolution and materialism, its debates over the origins of ideologies of honor, its explorations of the intricacies of kinship and language, 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 have 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' dramatic reconstructions of sixteenth-century festivals and follies, not to mention Geertz' own portrayal of a Balinese cockfight. William Sewell has recently written a book 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 of skilled workers in Hamilton, Ontario, from 1860 to 1914 includes an exceptionally clear statement of the 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 early twentieth-century world.
>
> Beyond the data, however, looms the theoretical framework within which this study evolves. While sections of thebook 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 rigour 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 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 fifty 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, and 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 wtih 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 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 publics 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 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 their 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, and Nancy Cott

-- 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 arenas of their lives; second, 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 Ariès, 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 changes in attitudes, _mentalités_, _Weltanschauungen_. Thus for Ariès 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 implications for class formation. On the small scale, the work of Wrightson and Levine exemplifies the sort of synthesis which has its counterparts in other work on England, France, Germany, and Sweden. On the large, 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, seventeenth-century volume on the 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, communications, 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. By the start of the third volume, indeed, Braudel is trying to use Immanuel Wallerstein's model of the world-economy as the means of organizing the whole vast historical experience. Braudel abandons that effort without quite saying so, but he never abandons the linkage of the full range of social experience to the structures of production, distribution, and consumption. He believes and demonstrates that the material conditions of everyday life vary in consonance with shifts in the international economy. He shows that those material conditions shape the full range of human organization, from sex to belief to power structure. In order to do so, he draws repeatedly on demographic analyses, on local economic studies, on the whole array of topics which have made his journal Annales a byword for historical innovation.

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 which 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."

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