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Some of these will fail; as they do, we will not hesitate to abandon them. The point is to bring energy, clarity, and pleasure into sociological reviewing.

## **The Tyranny of Here and Now**

Charles Tilly

*New School for Social Research*

About fifteen years ago, a prominent economic historian told me it was time for sociologists to perform the same coup in social history that economists had brought off in economic history: sociologists should reorganize the historical discipline's theory and practice as their own special province of applied social science. By then economic history—which had previously included a great deal of description, much institutional analysis, but little formal modeling—had become an intensely technical field, with a large number of its arguments coming directly from neoclassical economics. My interlocutor thought sociologists should try a similar revamping of social history.

It didn't happen. Since then sociologists have strongly influenced the ideas and practices of social historians; talk of social mobility, of life tables, of log-linear modeling has crept into social historians' journals, meetings, and yarn-swapping. Yet social history has not dissolved into sociology. Social history has survived—even thrived. These days, in fact, social historians often consort with anthropologists, and sometimes dare to turn their backs on sociologists as uncouth, crass, and insensitive to meaning. How could a discipline with so little visible intellectual fiber resist the analytic power of sociology?

Answer: the fiber was invisible, but tough. And the analytic power of sociology, when applied to the serious problems of social history, often turned out to be illusory. Social history, as usually practiced, examines the connections between the lives of ordinary people and very large structures and processes. Social historians commonly ask these sorts of questions: how do relations between parents and children change with the expansion of markets for wage-labor? In what ways do the work experiences of landholding peasants, serfs, and free agricultural laborers differ? What is it like to live through a revolution? These questions normally apply to a particular era and locality, although the era and locality are sometimes as broad as, say, the western world since the year 1000. In that perspective, a large part of what we call sociology resembles a

social history that is oriented to a single place and time: here and now. To the extent that time and space are defining elements of a social phenomenon—as they are, for example, in any meaningful analysis of capitalism—the effort to absorb its social history into sociology, as presently practiced, will impoverish the social history and distort the sociology.

To be sure, sociologists often try to escape the tyranny of here and now by generalizing about many, or even all, places and times. (Dangers: that abstraction will offer the illusion but not the reality of escape from time, that generalizations having plenty of meaning and value for circumscribed times and places will become ludicrous and empty when extended to all human experience, that the concepts themselves will be embedded in particular time-place settings.) Of course, many sociologists study change. (Dangers: that short samples of long processes give misleading evidence of their character, that the regularities in social processes vary with their time-place settings.) True, people have by now practiced sociology in enough different settings over a long enough period that we have many heres and nows to choose from. (Danger: that investigators who collate instances of a phenomenon treat them as independent “cases” rather than historically-connected events.) Nevertheless, the bulk of sociological work involves the interpretation of social experience within stringently limited places and times, as a function of the major structures and processes impinging on those places and times. In these regards, sociology looks less like an encompassing science than a cramped social history.

Both disciplines, indeed, rely heavily on the same basic procedure: collective biography. Collective biography consists of the assembly of uniform observations on multiple social units—individuals, households, organizations, communities, and so on—within a defined population of such units, followed by the collation of the observations into statements about uniformities and variations within the population. Survey research obviously belongs to collective biography; so does most demography, historical or otherwise. Less obviously, historical and contemporary criminology, major parts of past and present urban analysis, many treatments of family organization, and a large share of all work on political participation by sociologists and social historians proceed by means of collective biography.

Nevertheless, most of the time we can tell the two groups of practitioners apart. Sociologists, on the whole, insist on more explicit conceptualization and hypothesis-testing than do social historians, adopt much more elaborate formalizations of their arguments, and worry more about technical problems of modeling, measurement, and estimation than do their colleagues in social history. The two groups, furthermore, tend to be concerned about somewhat different questions: questions based on particular kinds of structures and processes for the sociologists, ques-

tions based on particular times and places for the historians. Confronted with the same set of observations about family life during a revolution, for example, a sociologist is likely to ask "How do families deal with stress? What stresses do revolutions produce?" while a social historian is likely to ask "How did the modern family come into being, and did this revolution play a significant part in its emergence? Did family life in this revolution follow the pattern of family life in the French Revolution?"

The sociologist's questions are not necessarily more general than the historian's; since sociologists actually construct most of their generalizations from the recent experience of western countries, their questions may even be less general. But the sociologist's questions differ in emphasizing the characteristics of structures and processes, in minimizing the constraints of time and space. Differences therefore remain between retrospective sociology and social history, between contemporary sociology and social history of the present.

Some investigators, however, work with one foot on each side of the line. That is especially true of historical community studies. During the last two decades, various forms of collective biography have revolutionized the social history of communities. Not so long ago, urban historians typically attributed a general character to the inhabitants and the site, described the built environment in some detail, and concentrated their analyses on the actions of economic, political, and cultural elites. Although good architectural histories and studies of dominant classes have continued to appear, the balance has changed profoundly.

The introduction of descriptive demography has had the largest impact on historical studies of communities. Family reconstitution, the aggregative analysis of vital events from population registers, and the study of mobility and population composition by means of manuscript censuses, tax records, and similar sources have not only made possible more inclusive portrayals of local populations, but also specified more clearly when and where significant changes in social experience occurred. Studies of migration to Johnstown (Morawska, 1985) and to Pittsburgh (Bodnar, Simon, and Weber, 1982) show considerable improvement over their predecessors through reconstructing the interpersonal networks that linked migrants to their origins and destinations and relating the finding of jobs at the destination directly to the operation of those networks. (One of the pleasant ironies in these sorts of studies is that they make early sociological inquiries such as the Pittsburgh Survey important sources for historical analysis.) Although all of these studies drew on far more than standard demographic sources, in each of them the painstaking extraction of information from manuscript censuses provided the central specification of the population to be examined and the variations to be explained.

However, collective biography goes far beyond strictly demographic sources and analysis. Thus Hanagan (1982), Aminzade (1981), and a number of other students of nineteenth-century France have assembled collective biographies of political activists from records of political repression and surveillance. Many specialists in twentieth-century social history, furthermore, have combined compilations of documentary sources with retrospective interviews of survivors. As recent collections of essays on social history edited by Levine (1984) and by Zunz (1985) illustrate abundantly, collective biographers are studying everything from marriage customs to revolution.

The dominance of collective biography in social history, it is true, has fomented its own rebellions. In the name of mentalities, discourse, *Alltagsleben*, narrative, and retrospective anthropology, some social historians have shifted emphasis to the analysis of evocative texts, rituals, and events. Sabean (1984) reconstructs the interplay of person, community, and power relations in sixteenth-century Wurttemberger villages as a form of discourse; although the collective biography of village families aids him mightily, at the center of his analysis stands the explication of a limited number of rich, problematic texts: some inquiries into individuals' refusal to take communion, an account of a prophet who preached during the Thirty Years War, and so on. Just as Ginzburg (1980) draws out the world of a sixteenth-century miller from a single journal, Sabean uses his texts to reconstruct a round of life—a round he regards as made coherent by shared meanings.

Some social historians have entirely forsaken collective biography for their own versions of historical ethnography. In fact, anthropologists and social historians have to some extent swapped customs: In a book whose title translates as *Class and Culture. Social-Anthropological Perspectives in the Writing of History*, Robert Berdahl and his fellow social historians, including Sabean (1982), essentially define "anthropology" as the closeup study of culture, meanings, and mentalities. For most of them, the usual forms of collective biography have become suspect for their reification of structure and their failure to grasp meaning. Yet when such a skilled ethnographer as Ségalen undertakes to study "fifteen generations of Bretons," she draws her *fil conducteur* not from a series of rituals, symbols, or tales, but from overlapping genealogies, vital records, and censuses that stretch from 1610 to 1981. Demographic facts, she says,

in addition to their intrinsic interest, stand at the heart of the subject. How do population changes connect with the region's economic and social balance? What relations exist among the standard of living, the variables that affect it, and the system of inheritance? The facts of fecundity, fertility, mortality, and nuptiality have primary importance (Ségalen, 1985:43).

Not that collective biography and the study of meaning contradict each other. Having set down a firm demographic frame, Ségalen herself turns to marriage contracts, leases, household inventories, photographs of weddings, and personal interviews, among other sources, for information about the changing texture of social life.

William H. Sewell, Jr., also works both sides of the line. His *Work and Revolution in France*, published in 1980, followed anthropological insight in tracing the character and origins of corporatist ideas among French workers in and around the Revolution of 1848. But his newest book relies much more heavily on collective biography. *Structure and Mobility: The Men and Women of Marseille, 1820–1870* displays the benefits and the costs of speaking simultaneously to collective biographers in sociology and history.

*Structure and Mobility* attempts “a closely argued analytic description—a kind of closeup sociological portrait—of an entire urban society during the epochal changes of the nineteenth century” (Sewell, 1985:xiv). “This study,” declares Sewell, “is informed by causal hypotheses drawn from sociology, but it uses them more to order the description of social life and social change in Marseille than to subject them to systematic statistical tests” (xv–xvi). Giving time and place priority over structure and process, Sewell puts down his weight on the historical side of the line. Or so he says at the outset. Later, as we shall see, he leans more heavily on conventional sociology than is good for his analysis.

Nineteenth-century Marseille certainly deserves close description. The old port grew from about 100,000 to 300,000 inhabitants between 1820 and 1870, and thus remained (after Paris and Lyon) France’s third-largest city. During those fifty years, Marseille saw first a concentration of capital, labor, and production, then a transformation of trade: steamships, new docks, reorganized financial structure.

Instead of describing or explaining those changes, however, Sewell concentrates on the characteristics and experiences of ordinary people. He deals successively with occupational structure, occupational status, demographic characteristics of neighborhoods, components of urban growth, migration, crime, and occupational mobility, both male and female. His systematic evidence comes especially from manuscript censuses, civil marriage registers, and records of criminal prosecutions—not combined into a single biographical file, but used for the most part separately. (In other work, Sewell has also used records of surveillance and prosecution to create files of political militants, but those analyses play almost no part in *Structure and Mobility*.) The 115 tables, plus multiple maps and graphs, report a large effort over more than fifteen years of research.

What has come of the effort? Detailed, defensible descriptions of

the labor force of Marseille by sex, neighborhood, place of birth, and literacy around 1820, 1850, and 1870; ranking of occupational categories on (a) proportion of grooms in a category who signed the marriage register, (b) proportion of grooms in a category who had businessmen, professionals, or rentiers as their witnesses, and (c) proportion of brides who gave no occupational title (and therefore were presumably not working for wages); cross-tabulations of characteristics (e.g., age, occupational status, place of birth) of brides, of grooms and, less often, of both jointly; tabulations, usually bivariate, of criminal convictions by offense, occupation, birthplace, sex, and other characteristics of the offender; comparisons of occupations of brides and grooms with those of their fathers and fathers-in-law; a few other combinations of the items just mentioned. Through most of the book, Sewell strides from tabulation to tabulation like a tour guide, pointing out the salient features of each one, and gradually building up an interpretation of the evidence.

Sewell guides effectively. His lucid writing makes it easy to follow his successes and failures alike. The successes are substantial. Sewell's portrayal of Marseille lacks the demographic, institutional and cultural richness of such eighteenth-century studies as Garden's on Lyon (1970), Bardet's on Rouen (1983), and Perrot's on Caen (1975), and lacks the political depth of such nineteenth-century treatments as Agulhon's on Toulon (1982), Lequin's on Lyon (1977), and Merriman's on Limoges (1985). But in compensation *Structure and Mobility* provides our fullest description of the working population of any major French city in its period, our only extensive analysis of occupational mobility in such a French city, and the best opportunity by far for systematic comparison with other cities whose labor force and mobility patterns sociologists have studied elsewhere. What is more, Sewell presents one of the few descriptions of *female* occupational mobility that is available for any city anywhere, past or present.

Sewell's immediate interpretations of his evidence, furthermore, are generally clear, cautious, sensible, and amply documented. True, he has no defensible ground for his explanation of the higher rates of criminal conviction for temporary migrants as a consequence of "disorientation" (232) or of the considerable downward mobility of Italian immigrants (who almost certainly came mainly from relatively prosperous Piedmont, Savoy, and Lombardy) by claiming "their values and work habits had been formed in an economically backward country" (257). But such dubious and undocumented interpretations invade the pages only rarely. And his sustained critique, theoretical and empirical, of Chevalier's arguments (1958) concerning the disorganizing effects of migration gives great point to Sewell's contrary findings on the subject.

The book's principal weaknesses result from three related, delib-

erate choices. First, Sewell chose to exclude power, conflict, and politics (which he has treated very effectively elsewhere) from this analysis of nineteenth-century Marseille. Second, he chose to draw arguments, models, and comparisons mainly from contemporary American sociology—more precisely from American analyses of stratification and mobility. Third, he chose to organize the book as a series of glosses on quantitative descriptions of Marseille's population. In short, despite his initial declarations, Sewell placed his study squarely within the tradition established by American studies of social mobility.

That placement shows up clearly in Sewell's assumption (implausible, and in any case never tested) that occupations fall into a single hierarchy of "standing," "status" or "prestige." That assumption shapes the entire analysis of "how the changing shape of the male occupational structure interacted with the changing recruitment of young men into the labor force—above all with rising immigration—to create particular patterns of upward and downward mobility, of opportunity and blockage, of movement across class boundaries and recruitment from within" (234). This fosters the further assumption that occupational position was an outcome of "competition in the urban labor market" (250), in which an individual's literacy, urban experience, and other personal qualities were the prime determinants. Those assumptions overlook the employers who made hiring decisions, the system of subcontracting by which foremen and artisans recruited their own labor forces, the longshoremen who organized to control jobs in the port, the dock company that eventually broke the longshoremen's organization, the vast networks of circular and chain migration that fed workers from distant villages into particular urban trades, the significant differences in structure, seasonal employment and migration pattern among Marseille's large boot-and-shoe, construction, soap, sugar-refining, and metalworking industries. All of these facts and more—most of them documented by Sewell's own evidence—challenge the assumption of open competition in a general urban labor market.

From the assumption of open competition Sewell also derives the interpretation of women's declining employment with age as meaning that "employment generally was not very positively valued for women" (68). From the same assumption spring false surprises such as the high proportion of sons of "peasants" (read: men in any agricultural occupation) who ended up in "bourgeois" occupations (read: sales, clerical, and small business; 250). Sewell repeatedly inserts that note of astonishment into his interpretations:

But it is remarkable that by 1869 French men and women who came from departments beyond Marseille's hinterland were as likely to have spouses born in their home departments as born in Mar-



seille . . . This astonishing fact seems to indicate either the existence of tightly knit immigrant communities within Marseille or the persistence of ties between the immigrants and their home communities—and most probably both (207).

The record of the illiterate non-Italian immigrant women who married non-Italian immigrant men in 1869 is particularly impressive. Of those whose fathers were peasants or workers, no fewer than one in four found bourgeois husbands; and of those whose fathers were bourgeois, nearly two-thirds married bourgeois men. Astonishingly, these rates were essentially equal to the rates achieved in the same year by *literate* women who married fellow natives (296; emphasis in text).

No other group of women in Marseille did so much with so little as the non-Italian immigrant domestic servants. Mostly illiterate, from humble backgrounds, they nevertheless married into the bourgeoisie with astonishing frequency (310).

These passages have two genuinely astonishing features. The first is their bald language of competition, of social climbing, of “making it.” The second is their failure to recognize a system of chain migration in which young women and men from a village in Marseille’s hinterland used existing contacts in the city to find employment: the women commonly starting out in domestic service and saving up for marriage; the men moving in considerable numbers into retail trade and minor administrative jobs. This is essentially the same pattern that Moch found in village migration to nearby Nîmes. There, as in Marseille, “Migrant wives were less likely than native-born Nimoises to be married to skilled laborers, but they were slightly more likely to be married to white collar workers and members of the petty bourgeoisie” (Moch, 1983:155).

Sewell falls into the trap of unwarranted surprise as a result of adopting an implicit comparison of his findings with American findings on social mobility and an explicit procedure of conducting his analysis as a commentary on successive tables. Those choices block the obvious alternative: to begin by analyzing the changing social organization of production, hiring, and migration, then to treat the description of employment patterns as explication, test, and refinement of that analysis. The analysis would have called greater attention to changes in the economy of Marseille’s hinterland.

Attention to the hinterland would in turn have made it easier to see that the increasing proportion of “peasants” among migrants to Marseille (of which Sewell makes much) was an outcome of the departure of agricultural wage-laborers from the countryside. The declining seasonal and part-time employment of hired hands, which occurred very widely in the latter half of the nineteenth century, sent large numbers

of proletarians from rural to urban locations, and made rural areas more thoroughly peasant and agricultural than they had been for centuries. If Sewell had compared his findings with analyses of eighteenth- and nineteenth-century regional migration by Châtelain (1976), Lamorisse (1975), and Poitrineau (1983) (which do not appear in the bibliography) and with Lequin's study (1977) of the interaction between Lyon and its hinterland (which does), he would have caught the connections between changes in the economies of capital and hinterland much more effectively.

The difficulty, however, does not lie in a few misconstrued facts or a handful of missed references. It consists of relying on just enough sociology to detach the analysis from its time-place setting, and not enough sociology to make it a fully effective treatment of the structures and processes it examines. The widespread call to integrate sociology and social history, for all its honest appeal, immediately raises the questions: on whose terms, and how? If the cost is the straightforward absorption of historical analyses into existing sociological models, the price is too high.

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