BIG CHANGES IN SOCIAL HISTORY

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Olivier Zunz, ed. *Reliving the Past: The Worlds of Social History.* Chapel Hill: University of North Carolina Press, 1985. ix + 296 pp. Maps, bibliography, and index. $29.00 (cloth); $9.95 (paper).

Social history's hundred flowers campaign, Olivier Zunz in effect announces, has gone on long enough. Suffering from "fragmentation and a diminished focus," lacking "a clear program," increasingly "uncritical" in the use of social scientific theories, the field "now needs reordering." In place of geographically and topically narrow studies, overspecialization, and subordination to frameworks borrowed from the social sciences, we are to have "large syntheses" about "major transformations" common to many countries, and theories and methods are apparently to be generated largely within history itself (pp. 3–10). To connect the overview from the top to the perspective from the bottom up, Charles Tilly provides an arresting slogan in the first of the book's five single-authored historiographical essays, each on the modern history of a major region or country: "How did Europeans live the big changes?" (p. 11). Exemplary monographs having failed to discipline the field, the writers turn to the genre of the hortatory literature review. What is the new program and how likely is it to remake social history?

European social history, Tilly asserts, "grew up in opposition to political history," and although it presently includes attempts to "place politics in a social context," to recapture the ethos of a particular place, time, or group, and to bring evidence from the past to bear on "present-oriented theories," its "central activity . . . concerns reconstructing ordinary people's experience of large structural changes." Two of these changes affected ordinary people's everyday lives most deeply and underlay other transmogrifications – state formation and "the prevalence of work for wages under conditions of expropriation." Therefore, the "unifying, motivating task of European social history since about 1500 is [ought to be?] this: connecting the changing experiences of ordinary people to the development of capitalism and the formation of national states" (pp. 13–17).

To achieve the goal of documenting these changes, reconstructing ordinary lives, and relating the transformations to each other as well as to lived ex-

perience, Tilly argues, we need quantitative studies of large numbers of people and events, using such techniques as collective biography or time series of strikes. But these must be supplemented by intensive, detailed analyses of the "meaning" of representative happenings. Either alone will not do, for the one risks crude and misleading abstraction, the other, populist antiquarianism. The methods and falsifiable arguments of social science history must therefore be blended with humanistic hermeneutics. Even "good" ideas and concepts (Tilly usually avoids the word "theory") from the social sciences pose risks for historians because they may not fit or entirely cover past circumstances. Certain "bad" ideas, such as functionalism and the view that rapid social change produces more individual and collective disorder than slower alteration, should be discarded as either vacuous or empirically false. Of the theories that will relate his two transitions to quotidian existence, Tilly has little to say.

It may be that the tension in Tilly's paper – the comment applies to the others as well – between literature review and agenda-setting accounts as much as his distaste for large theories for his failure to map out his program more comprehensively or to argue for it and against other problematics more explicitly. The published studies that he praises or condemns (with his usual insight) are rarely more than peripherally concerned with his two great transformations. Instead, they treat literacy, mobility, family structure, strikes, minor wars, fertility. The implications about state formation and capitalism that he does draw from previous works are largely negative: the notion of a major break in the nineteenth century between an immobilized, rural, subsistence farming, premodern era and one uniformly and inexorably altered by a technologically driven industrial revolution into a differentiated market society has been overturned, he thinks, in nearly every particular. The generalizations in his "interest-oriented" sketch of state and capitalist development are few and simple. People oppose attempts to control their lives, but once institutions develop, they fight within them. The centralization of work in large capitalist-controlled factories was more important than technological change in shifting power from workers to owners. The extension and regularization of national governmental processes strengthened civilian bureaucrats and representatives of the public and weakened local potentates.

Both Tilly's descriptions and his prescriptions raise many questions. How, precisely, did he determine what topics have attracted more attention than others? If European social historians, without really knowing it, have been working primarily on his project for years, why aren't there more positive, generally agreed upon results? What are those scholars who have concentrated instead on, say, demography or religion or science and technology, to do now? Must they give up their subjects, or redefine them so as to fit into the "capitalism and the state" format, as physicists are rewriting their grant proposals to fit under the rubric of "Star Wars?" Why are there two fundamental trends, rather than one or six? And why these two? On exactly what grounds may one say that one transformation caused or was more fundamental than another?

On a more theoretical level, how does Tilly conceive the relation between the three subjects – capitalism, the state, and everyday life? Is the latter, in a sense the dependent variable, to be studied in order to chart the effects of perturbations of the two "independent" variables, or is it of interest in itself? How can we determine the effects on everyday life of these two variables without considering contemporaneous alterations in other facets of life, such as religious behavior or technology or diet? Should we view the development of capitalism or the state as unilinear, the state everywhere becoming gradually more centralized, the economy, ever more market-oriented, at different rates in different places, to be sure, but with the same directional trend? Is so, how does the scheme differ from the deservedly maligned concept of "modernization," and what are the implications of the experiences of countries that centralized their governmental structures very early (China) or very late (the U.S.) and have, from time to time, devolved authority back to the provinces? What is the aim of the whole exercise – to discover generalizations about the relationship between capitalism and the state, to reveal the effects of each on routine existence, or just to provide a convenient label for an otherwise disparate set of studies? If generalizations of some sort are desirable, what is the role of theories from political science and economics, which would seem to have some relevance to the two trends, and why aren't they discussed here? Although there is a place for induction in the development of theories, Tilly is surely too well aware of the philosophical frailty of commonsense empiricism to believe that facts, unvarnished and unassisted, can speak for themselves. Where, then, are theoretical guidelines for the enterprise to come from?

Tilly cannot be expected to answer all questions – any close reader could think of many more – in a single essay. Nonetheless, before his program goes much further, these and other matters will have to be addressed.

In an essay on synthesis in North American social history, Olivier Zunz attempts, by focusing on the relationship between industrialization and the assimilation of ethnic groups into mainstream society, to demonstrate that social historians can originate theory themselves without relying on sociology and can relate their studies to larger interpretative issues. Extending the analysis of his 1982 book, *The Changing Face of Inequality*, Zunz contends that in the nineteenth century, members of immigrant groups in many large

cities enjoyed the freedom to choose to seek employment within ethnic communities or in the larger economy. This "dual opportunity structure" fostered economic mobility at the same time that it allowed the maintenance, at least temporarily, of "semiautonomous" ethnic enclaves with their own stores, churches, political organizations, and middle classes. Sometime between 1910 and 1925, however, the drawing power of employment in large factories collapsed this double into a single economic structure. Thereafter, economic opportunities for late-arriving groups in the cities (most eastern and southern European immigrants, and presumably, blacks, hispanics, and post-1965 Asians) were constrained, and ethnic neighborhoods and institutions lost their bourgeoisies. Twentieth-century economic assimilation coexisted with a weakened cultural and religious pluralism. Ironically, as the economic order homogenized and simplified, the spatial correlation between class and ethnicity increased, because the more successful members of ethnic groups left the old neighborhoods, jobs, clubs, and churches. Potentially drawing together studies of social ideology, mobility, labor and urban history, and the development of ethnic communities and institutions, and emphasizing the necessity for scholars to pay more attention to the emerging bureaucratic middle class, the Zunz thesis is in part a response to the call by Bernard Bailyn and others for overarching syntheses.[1]

Zunz neither offers his scheme as the only one that American social historians should work on nor indicates what additional ones might encompass the experiences of other groups at different times or in different segments of the economy or outside big cities. As an organizing concept for a part of the field, it seems promising, and it ought to be elaborated and tested by him and others. What role did politics and labor union organization play in the (asserted) transformation? Did the replacement of political patronage, often distributed in the late nineteenth century on an ethnic basis, by civil service positions simply parallel or partially cause the changes he describes? Did the increase in local, state, and national regulation put small businesses at an increasing disadvantage, compared to large businesses? To what degree was this an intended consequence? Can Gabriel Kolko's 1963 *Triumph of Conservatism* merge with Zunz? How important were the "Americanization" drives of the World War I era, the *de facto* collapse of immigration after 1914 and the *de jure* closure of 1924? To what degree did public high schools and colleges, expanded as a result of political decisions, turn ethnics into "Americans" and prepare them for white collar, mainstream employment? How do country-bred WASPs who moved to the cities fit into the picture? On purely empirical grounds, how generalizable are the findings from Zunz's Detroit case study? Was economic mobility for the disadvantaged really less after than before the Great War everywhere? To what degree have ethnic businessmen persisted, especially in the Cuban, Mexican-American, and new Asian communities? Did all middle-class ethnics desert their original turf, or did many live elsewhere, but return for mass or lodge or business?

Because of their subjects, the book's other three essays—William B. Taylor's on Latin America from 1500 to 1900, David William Cohen's on Africa, and William T. Rowe's on modern China, may attract less attention from readers of this journal. In a comprehensive critique of the "dependency" literature, Taylor condemns it for being vague, static, and immovably committed to economic determinism, for treating "dependent" societies as abstract, conflict-free, and powerless, for ignoring the partially autonomous influence of the state on society, and for its authors' superficial analyses of historical evidence. As a means of charting the development of the state, Taylor highlights studies of the social composition and the secular as well as the religious roles of the priesthood. As an index of political ideologies, he analyzes temporal and spatial variations in both the incidence and the meanings attached to various cults of the Virgin Mary. However far off the framing of comprehensive generalizations in Latin American social history, one of the chief areas of future study, Taylor concludes, should be the relation between states and societies. Social historians must rediscover law and politics.

An exercise in what might be called poetic anthropology, full of long inserted quotations from primary sources that often bear only shadowy relations to his text, Cohen's paper questions Tilly's stress on major changes and Zunz's on synthesis. Concerned that concentration on overarching, European-derived concepts will shift attention away from indigenous societies and fearful that abstract enquiries will sacrifice too many details about the day to day existence of the peoples of a vast and diverse continent, Cohen is a "splitter" in the midst of a crowd of "lumpers." Perhaps this conflict of views reflects the uneven development of different fields as much as it does matters of taste. Generalization is effortless when scholars know little, very difficult though often attempted when they know a great deal, but nearly impossible in between. African history may currently be in midpassage.

Although the book's format no doubt discouraged him from pointing them out directly, Rowe's clear and comprehensive review of recent international scholarship on China is full of implications for Western, especially European historiography. Was China ever "feudal," and if so, when and why did it cease to be? At what point did the Chinese economy, highly commercialized for centuries, become "capitalistic," and what role did the imperial state play in retarding further development into full-fledged industrial capitalism? Are these Western categories applicable to China?

New questions, new theories, new methods, new sources of data—all of

these may transform a field, and all four have played a part in the social history movement of the past generation. It is not so clear that new organizing categories for previous studies, which is largely what Tilly et al. provide, can or should reorient a discipline. If social historians are as cordoned off geographically as the writers allege, few may read more than one or two of these papers, thereby missing possible connections, sparks of insight that might cross-fertilize work on different continents. And readers will have to make the connections themselves, because the writers do not make explicit the potential import of studies of one continent for the historiography of others, but only respond to Tilly's scheme. The book is therefore largely another chapter in the saga of European intellectual imperialism, rather than an inspirational exercise in comparative history. As interesting as these excellent and up to the moment review essays are, it is difficult to see how they will lead to big changes in the research lives of social historians.

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1. Bernard Bailyn, "The Challenge of Modern Historiography," *American Historical Review* 87 (February 1982): 1–24.