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Reviewed work(s):
Published by: The Johns Hopkins University Press
Stable URL: http://www.jstor.org/stable/2700953
Accessed: 14/08/2012 22:08

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THE "NEW" SOCIAL HISTORY
IN THE CONTEXT OF AMERICAN HISTORICAL WRITING

Laurence Veysey

Historians are subdivided in more complex ways than members of most other academic disciplines—quadruply, by nation or region of the globe, by time period, by thematic category (social, political, intellectual, and so on), and by cognitive predilection, sometimes but not always tied to a political outlook (e.g., Marxist, Freudian, or—perhaps still commonest of all—a naively antitheoretical empiricism). It is a mildly shocking thought that of these four kinds of division, time period—our supposed reason for being—may actually strike many of us as the least important.

For about a decade, from the mid-1960s to the mid-1970s, the cleavage that seemed to matter most was between leftists and nonleftists. And this shock wave has not yet died out. The presence of colleagues who are vigorous Marxists may still set afire the deepest passions in us, greater, for instance, than those triggered by the ritualized snobberies of Europeanists toward Americanists. This is so because more orthodox historians suspect that Marxists, as also indeed some Freudians, are not cognitively "open" in the same way as they are. Of course in making this assessment non-Marxists often exaggerate their own "openness," confusing it with the collective heterogeneity they have made in terms of various kinds of intense specialization. And, on the political plane, non-Marxists too easily forget that liberalism, defined as faith in the beneficent power of the federal government to bring about social justice, is itself now only the predisposition of a shrinking minority of Americans, though they frequently seek to present it as "the trend of history" to their students, just as Marxists do in regard to their own agenda.

There are signs, however, that the Marxist shock wave has been increasingly assimilated. It has lost some force by internal splintering. In the last few years varying conceptions of it among its practitioners have made the label seem almost meaningless. Eugene D. Genovese threw out economic determinism; Immanuel Wallerstein soft-pedals the once fundamental distinction between industrial and preindustrial capitalism, so as to invoke a continuous history of exploitation by "core" nations of the Third World.1 What is left, beyond a strong emotional identification with the oppressed? In part, this decline of
dogma records the surreptitious spread of greater historical "openness" within Marxist circles themselves. Leftists do not like always to admit that they also are often pulled into an excitement over the complexities of the past for their own sake. A frequent pattern, not much talked about, is for leftists to merge increasingly into the older scholarly ethos as time passes. This might have meant a victory for liberalism, if liberalism were not itself so shaky.

Politically defined factions among American historians will grow less important the longer the current age of relative political calm continues. Instead historical scholarship in the United States will again most importantly be sorted according to thematic categories—at the grossest level, the three-fold split between political, social, and intellectual.2

From this standpoint the single most important line of division among American historians separates those who see all historical particulars in terms of the evolution of social structures from those who do not. Among the latter are political historians of an older outlook and semipopular orientation who treat political conflict in much the same spirit as team sport,3 not inquiring deeply into the social origins of the actors or exploring the ways in which their routinized conflicts might be illusory.4 But one also finds intellectual historians who reject the centrality of the concept of social structure from their own very different insistence upon the independence (from everything else) of widely shared patterns of thought within men's minds. Perhaps the most exciting contest now occurring among historians in the United States involves the adherents of these three persuasions. In this quite self-conscious struggle during the past dozen years social historians have been the aggressors, while both political and intellectual historians have been placed increasingly on the defensive.

Political history has always dominated the entire discipline, in precisely those quantitative terms which social historians admire as evidence of anything (courses taught, books published). This has changed only somewhat. Even today, the review sections of journals reveal that the great majority of books published by historians still treat conventional political or diplomatic topics.5 Clearly, then, some other, nonnumerical, standard is being invoked in the common judgment that political history is "threatened" or "on the defensive" in comparison with social history. Indeed, this standard lies in the nebulous, unquantified realm of estimates about intellectual excitement.6

Political history, it seems fair to predict, will always endure. It is the kind of history with the broadest lay audience. Its deeply entrenched following, whose appetite a large segment of the historical profession continues to serve, surrenders to the fascination of observing the nuances of more or less ritualized conflicts among contending parties or factions, both within and between nations. To make these spectator enjoyments seem not only fully respectable
but even quite essential to understand, we need only remind ourselves that the wielders of governmental power have more potential than ever for helping or harming ordinary folk. Traditional narratives, biographies, and psycho-biographies of such leaders may be expected to go on rolling forth regardless of the contempt of social or intellectual historians. A very large share of those currently teaching American history in our universities and colleges, for instance, continue to believe that a "proper" understanding of such topics as Jeffersonianism, Jacksonianism, progressivism, and the New Deal deeply matters. What to outsiders may seem at times exquisite squabblings over minutiae (has this or that "strain" within progressivism been duly appreciated?) remain central items in the vocabulary and imagination of what is still the largest single faction of historians of the United States. When a comparably picayunish concern is shown for the mutual relations of Roosevelt, Churchill, and Stalin, only the most inhumanly consistent devotee of the history of anonymous millions in preference to the story of elites might be found to insist that Soviet-American cooperation was already so unlikely in historical terms, outside of a threat to mutual survival, as to make the words and deeds of individual statesmen and their advisers relatively inconsequential. Similarly, at least this close to the events, it would seem very difficult to argue that Richard M. Nixon was simply a man in the general mold of American presidents, only a shade distinguishable from John F. Kennedy or Franklin D. Roosevelt. To the degree that such points are acknowledged, social historians remain unable to convince their peers that all history should be reduced to the study of large groups of people.

Some political historians, adopting what is often called the "new" political history, seek in effect to retain their special concern with electoral politics while making it a branch of social history, particularly (in America) the history of ethnic conflict. Voting, as a customarily recorded act comparable in this respect to being born or dying, is of course highly amenable to quantitative treatment (though not to the sampling of individuals). So too are the backgrounds of groups of officeholders. The relatively few yet highly conspicuous practitioners of the "new" political history gained the prestige of social historians while dealing with the most conventional of historical subject matter, thus having it both ways.

The situation of intellectual history is very different. An upstart persuasion, widely launched in American universities only in the 1940s and 1950s, it furnished for a time much the same spirit of fresh excitement that has more recently centered in social history. As an anti-Marxist approach to history, arguing for the power of ideas as the causes of events, it fit appropriately into the climate of the Cold War and contributed to the ethos of holistic nationalism.

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Intellectual history was more demanding than the earlier political history; its subject matter was more rarefied; it required the historian to be more like an intellectual himself. For this last reason it was often strongly disliked by political historians. Intellectual history, in sum, could be attacked as a rather precious cult. Yet it was politically very safe, and—what usually mattered most of all for its adherents—it was deliciously complex, taking seriously as historical sources some of the most obscure and difficult writings of literate minorities.

During the drastically altered period of the 1960s, intellectual history came to be attacked as "minority" history of the wrong kind, reflecting the values of elites rather than of downtrodden groups. Leftists commonly so accused it (unless it dealt with the history of admired radical thinkers), while social and political historians in general were ready to join in the effort to put down what had long been regarded as a pretentious interloper within the guild. Some critics saw intellectual history as highly interesting but untenable in its claims for the causative power of ideas, at least those ideas associated with "high" culture. This was a relatively kind position. For others, formal ideas of any sort were as repugnant as Christian dogma as explanatory tools, in an age which had long since learned to view human motivation more realistically and therefore more skeptically.

Intellectual historians thus emerged with few friends. It is still too soon to tell whether intellectual history will shrink away as a result of its numerous kinds of vulnerability. A much discussed tactic of accommodation to newer strictures has been a shift toward studying popular culture and values rather than those of cultivated elites. Yet to do this from literary sources is extremely difficult, and, like the "new" political history (except that it is not usually quantified), the result is something close to annexation into an all-encompassing social history. Intellectual history might do as well by remaining true to itself, though with a new humility as to the kind of extreme specialization it represents. There should always be room in the discipline for austere explorations of such subjects as the history of philosophy, even more than there is room for topics like the history of electric streetcars.

What, then, finally of social history—the aggressor—itself? The "new" social history emerged in the 1960s quite separately from Marxist history, though riding the climate of engaged interest in the nonelite population. Its canons might be summarized as follows: that history should be viewed in terms of the processes affecting the great majority of people alive at any given time, with special attention to the anonymously downtrodden, those whose standard of living and prestige are the lowest (this corollary helped build a specious bridge toward Marxism), and that the historian should be intensely skeptical of literary sources of evidence, always the product of a small elite,
instead making use of whatever bare quantitative data exist to assure that one's conclusions are truly representative of the social aggregate being discussed. To be sure, most social historians continued to milk conventional evidence as well, to help dramatize realities, but only with the sternest reminders that one could not accept it apart from the backdrop of careful attention to the problem of typicality.

A curious aspect of the "new" social history is that it is almost never pursued as such. Instead, what is pursued is demographic history, urban history, the history of the family, of women, blacks, Chicanos, or native Americans, the history of radical social movements, the history of social mobility. The society, in its overall dimensions as an evolving structure, is hardly ever studied—so that many of the more myopic specialists among social historians must themselves be counted on the side of the earlier mentioned dividing line that pays almost no heed to social structure!

A reason for this is clear. Social historians emphatically reject the holism of the older intellectual historians and also the dualistic lines of conflict (such as progressives versus standpatters) of the older political historians. Instead the inhabitants of a given nation-state are seen to form an extraordinarily complicated mosaic. City-dwellers are very much unlike country-dwellers, men unlike women, rich unlike poor, "permanent" families unlike those of transients or immigrants, immigrants of one background unlike those of another. Each element in the mosaic must therefore have an utterly separate history. And there is little incentive to try to piece these histories together into a whole, aside perhaps from using the slippery rubric of "modernization," because the parts are seen as the realities, the whole as an artificial construction sustained by politicians and financiers.

The "new" social history, greatly influenced by the French Annales school, has turned out to be very diverse. Quantification has affected it most unevenly—least of all in the history of women and many ethnic groups. Sometimes its novelty and distinctiveness have been questioned by its critics, who point out that much the same version of history had been put forth by such American figures as James Harvey Robinson, under the very label of the "new history," in the early years of this century. Yet that affinity was more rhetorical than real, for the social historians of the 1960s, like the social activists, had for the first time glimpsed the true "bottom" layer of the society in a sustained way, and their standards of evidence and argument genuinely broke deeper ground. That this is so, but also that social historians have introduced limitations and biases of their own, may be illustrated by turning to two possibly familiar examples, the first involving descriptions of eighteenth-century Philadelphia by older and younger historians, the second a similar contrast in the treatment of immigration and social mobility in Boston.
Carl and Jessica Bridenbaugh's chapter-length discussion of Philadelphia in the age of Benjamin Franklin, published in 1942, has no explicit overall theme; it moves quite randomly from one subtopic to another, with little sense of any connected argument. A few elementary statistics are brought in. The Bridenbaughs' tone is friendly toward the cosmopolitan values of the merchant elite and toward high culture (apart from religion). Their account gravitates toward the "amenities" of life on that level of society; it is concerned, for instance, with the aesthetics of contemporary buildings. The Bridenbaughs were clearly happy to see their summary contribute to a comfortably familiar image of civic pride. Casually at the end, with no real evidence, they remark that Philadelphia at that time was an environment marked both by materialism and idealism, by individualism and social "communion and interchange" (pp. 26-27).

Providing a summary of roughly the same length on the same city in the same period, Sam B. Warner, Jr., was concerned in 1968 with Philadelphia only as an instance of "privatism" (his concept for the kind of mentality that fails to lead to socialism). Most of his evidence is tied to this single dominating argument. Indeed, one senses that Warner cares little about any possible uniqueness of Philadelphia, that he might just as readily have chosen to write about New York or Baltimore to make the same points. Warner is interested only in *types* of cities; Philadelphia is merely a good illustration of the preindustrial kind. Warner also sees cities primarily in terms of spatial patterns, not aesthetics or mental states (beyond the mentality of "privatism," which dictates the use of space). Warner's view is often openly retrospective or presentist in standpoint; his aims throughout his account are to identify types and stages in urban history, looking backward from the present, and to indict capitalistic individualism for what it has given us in the twentieth century. Yet, with one map, an extensive use of statistics, and some graphic descriptions of building and land use patterns, Warner gives us a far more vivid feeling of what it must have been like for most people to be alive in eighteenth-century Philadelphia than we can ever receive from the Bridenbaughs. Finally, where the Bridenbaughs had lauded the public spirit and charitable works of mid-eighteenth-century Philadelphians, Warner accuses the very same people of having had next to no government and, by implication, extremely little public spirit. Warner's view of Philadelphia is far more structurally precise, culturally unspecific, and, above all, bleaker and less flattering.

The contrast between two studies of Boston, Oscar Handlin's *Boston's Immigrants* (1941) and Stephan Thernstrom's *The Other Bostonians* (1973) is at least equally striking. Handlin's book is primarily a narrative. Its use of statistics is more than merely decorative, but figures are brought in only on an occasional basis to establish facts. By a later standard, there is a very
offhand attitude about evidence. Handlin tries to strike a note of comprehensiveness by providing tidy rhetorical summaries of motives and social patterns: for example, “Boston [in 1845] was a comfortable and well-to-do city in which the people managed to lead contented and healthy lives.”¹⁶ Today one cringes at such language. Handlin unabashedly quotes an elite literary figure, Ralph Waldo Emerson, to show “the fundamental ideas and basic assumptions permeating the social and economic structure of the society” (pp. 20-21). But Handlin was less theoretically naive than Bridenbaugh, embracing, for conceptual underpinnings, the cultural anthropology of Ruth Benedict.¹⁷

Many people would regard Stephan Thernstrom’s book as a purer instance of the “new” social history than Warner’s description of Philadelphia, because its text consists entirely of a discussion of statistical tables—how they were calculated and what they appear to mean. One might interpret the shift to such a format as anticipating an entirely new degree of reader skepticism about the adequacy of the evidence and the diversion of just about one’s entire energies as an author to the attempt to overcome it. Thus history has become far less obviously literary.

With Handlin, a reader might well be mainly concerned over revealing word choices; for instance, is it ethnocentric to use such terms as “pest hole,” “slothfulness,” “a brood of evils,” and, in a most unclear context, “malignant growth” when describing Irish slums? With Thernstrom, one is primarily trying to critique his reasoning about the evidence.¹⁸ The book is, from start to finish, an argument. It begins with a self-conscious listing of the particular questions he intends to ask of the past. His aim is to answer these questions, not to be comprehensive. On the other hand, he wants his study to be systematically comparable with those of other cities. Like Warner, Thernstrom is not much interested in what is special about the city he is studying; he is concerned with the city and is, so to speak, “rooting” for the typicality of Boston. Thernstrom’s Boston is meant to be a building block in a mosaic.¹⁹ Unlike Handlin, Thernstrom will bring in cultural explanations for phenomena only reluctantly and apologetically, as a last resort (p. 168). His view of human motivation in the Boston context appears to assume that the desire to rise in social position was central to the “inner” outlook of most Bostonians during the past century.²⁰ Perhaps for these reasons, Thernstrom’s version of the “new” social history is far less tuned to conflict than narrative social history such as Oscar Handlin’s.²¹ Though claiming to arise out of a concern for the history of nonelite masses, quantitative history like Thernstrom’s has little in common with leftist-inspired history, which is conflict-oriented or at the very least tied to questions of power and domination and is usually antiquantitative in its approach to evidence.

Quantitative social history such as Thernstrom’s has won enormous atten-
tive evidence, fallacies in historical reasoning have come to be more widely and forcefully perceived.23 Oddly this has meant that quantitative history, despite its high prestige, has become surprisingly vulnerable to counter-suggestion in specific instances. Logical objections can very often be raised, not only as to how the evidence was gathered or sampled, but to central aspects of the ultimate interpretation. For instance, Thernstrom's book discerns a surprisingly high rate of upward social mobility in late-nineteenth-century Boston. Yet, as he once briefly admits, the figures largely derive from the more settled spectrum of the population where fathers' as well as sons' occupations appear in the records (p. 80). The other major point of Thernstrom's book was to establish an enormously high rate of sheer geographical movement, that is, the existence of a large, predominantly poor floating population. If transients are left out of the social mobility statistics Thernstrom has put together with such great pains, then a skeptical reader might well argue that those latter statistics are so biased upward as to be meaningless.

Surprisingly often quantitative history is brittle as well as rigorous; it sits like an enormous skyscraper of enterprise which can all too easily be toppled, or at least partly undermined, with a crowbar supplied from the arsenal of its own kind of logic. If this is so, it may well stem from a genuinely important rise in the critical standards applied to works of history in the United States over the past fifteen years. Intermittently these standards reveal themselves in published reviews. Never has so much attention been paid to such issues as the adequacy of evidence, the need for precision over the exact nature of the social aggregate being discussed (such as "Americans," "black Americans," "middle-class women"), and the strength or weakness, in logical terms, of particular explanations. Our capacity to criticize works of history as they come forth has outrun our capacity to write them in terms that will withstand such criticism. Historical criticism, in the more demanding circles, may be in much better shape in the contemporary United States than substantive historical writing.24

Yet with all this greater sophistication about historical arguments, the very
highest amount of prestige may still be awarded to an historian who uncovers some incontestable yet previously unknown fact of major importance. Discovery may still count the very most; consider, for example, Stephan Thernstrom's and Peter Knight's disclosure of the previously unsuspected huge numbers of transients in nineteenth-century American cities, Tamara K. Hareven's revelation of kinship patterns affecting work assignments among New Hampshire textile workers, or Herbert G. Gutman's discovery of naming practices among some slaves which reveal their continuing autonomy from white culture in certain crucial respects. On this most fundamental level, standards of historical scholarship may not have changed all that much in the last hundred years, and the major recent discoveries have come in the field of social history.

Genuinely new, however, if not well illustrated by Thernstrom or Warner, is the shift in American social history toward the introduction of outside points of reference, that is, to analogous phenomena lying beyond the isolated case being discussed, and across international boundaries. This perhaps began with the interest that developed among Americanists over the nature of slavery in Brazil. It continues on many fronts, as, for instance, when historians of the New England town show familiarity with equivalent communities in England, France, and Sweden.

The extremely important trend is toward placing any single nation, such as the United States, in context, both structurally and intellectually. Our minds recede further and further from it, as if we start looking at it from a station in space, while on a different level we retain our intimate familiarity with it. This extreme duality of perception is what gives the best contemporary American historical writing its strength.

A consequence must be the questioning of previously unexamined assumptions about American uniqueness. The internationalism of basic historical processes in the modern world, in political as well as economic and social realms of life, grows more obvious. Despite the possible inadequacy of the concept of "modernization," I hope that this insight will become the primary historical theme of the near future, drawing United States historians still further out of their isolation and their sometimes nearsighted preoccupation with local trends and events that were not all that different from those going on in some other parts of the globe, if at slightly later dates.

The "new" social history helped to produce this broader perspective, but, like Marxist history, it now shows signs of losing its initial thrust. Though it has spawned vast projects, it has often left its practitioners strangely weary after a few years of immersion in it. Leading figures in the movement sometimes confess their desire to go back toward conventional sources. They can admit to being overwhelmed by the Pandora's box of interpretations they
have opened up. One hopes that their central message—the need for every historian to be consciously concerned with problems of representativeness in evidence—will not become blurred, for in it lies the greatest single hope of a generally improved standard in historical writing since the emergence of Germanic scholarship in the nineteenth century. It would be too bad to see this insight shrunk away in the doldrums of a morning-after.

As our universities enter a period of stasis or decline, there is a real danger that scholarly energy will dissipate through lowered morale and that historical writing will now become more aimlessly eclectic, less exciting, than it has been at its best in America during the past quarter-century. Excitement comes when there is a definite intellectual cutting edge within the discipline, arousing controversy and also promising discovery. (We need not be believers in dialectic to admit that.) A series of waves—intellectual history in the 1950s, then Marxism, and alongside it the more diverse "new" social history—have furnished these cutting edges. Could the outright transformation of American history as we have customarily thought of it, its merger into the history of the modern industrial world at large, furnish a new visionary matrix, giving meaning to our various extreme specializations? Then, the "new" social history would have led toward something lying beyond its own initial impulse. But, alas, in the training and energy required, this would be the most demanding kind of history of all.

Professor Veysey, Board of Studies in History, University of California, Santa Cruz, has adapted this essay from one on historical writing in the United States which will appear in Contemporary Developments in Historical Studies, ed. Georg G. Iggers and Harold T. Parker (1979).


2. Of course various other kinds of history (diplomatic, economic, legal, psychohistorical) might fairly claim autonomy on a part with these three. Yet I believe these three labels do reflect the most conspicuous elements or factions within American history departments. Psychohistory strikes me more as a curiously delayed spin-off from the climate of the 1950s, when Freud was so much in vogue, than as a major new direction in historical writing.

3. Arthur M. Schlesinger, Jr., is no doubt the most distinguished example.

4. Years of discussion over the celebrated issue of "consensus" or conflict as the key to American politics and culture have abated, it now being generally recognized that conflicts have been too central and numerous to allow for the kind of argument against their importance put forward by Daniel J. Boorstin, The Genius of American Politics (Chicago: University of Chicago Press, 1953), but that, on the other hand, the most important conflicts have been ethnic, regional, or even (in the realm of labor history) class-oriented, rather than involving the lection struggles of Republicans and Democrats.

5. Although when the counting is shifted to titles of dissertations in progress, social history (in all its phases) now emerges as the clear winner. From the list of them in Journal of American History 64 (1977): 285-308, I count 121 in political history, 152 in social history, and 64 in intellectual history. Those, however, that can be called social history are overwhelmingly
on rather trivial or conventional topics not inspired by the methods of the "new" social history. The "new" social history is practiced by a tiny elite within the profession.


7. For a recent example of a highly traditional summary narrative of American political history (inspiring one commentator on it to remark, "those textbooks of the 1940s were not putting us on"), see Robert Kelley, "Ideology and Political Culture from Jefferson to Nixon," American Historical Review 82 (1977): 531-62, and comments on it, pp. 563-82.


10. For a recent account of the growth of intellectual history as a historical subfield, see Felix Gilbert, "Intellectual History: Its Aims and Methods," in Historical Studies Today, ed. Gilbert and Graubard, pp. 141-58.


12. The attempt to apply modernization theory to United States history culminates so far in Richard D. Brown, Modernization: The Transformation of American Life, 1600-1865 (New York: Hill and Wang, 1977). But the concept of modernization has been attacked as vague, or as unduly deterministic, and its use may have reached its peak. Its great virtue has been the way in which it has worked against nationalistic parochialism by attempting to see the history of the modern world as a unit and the nations largely as illustrations of universal processes. It is to be hoped that this international perspective can be retained even if historical trends are looked at with less reductionism than the concept of modernization possibly imposes; see the closing paragraphs of this essay.


14. Sam Bass Warner, Jr., The Private City: Philadelphia in Three Periods of Its Growth (Philadelphia: University of Pennsylvania Press, 1968), pp. 3-21. The Bridenbaughs, for example, had seen religion as merely fading into irreligion, but Warner (p. 3) sees it as directly overcome by greed.

15. Warner's judgment that eighteenth-century Philadelphia was practically ungoverned seems to stem from a planning-oriented perspective in the mid-twentieth century. These conflicting standpoints lead to a highly revealing factual clash. The Bridenbaughs had described how "at intervals along [all] these thoroughfares [of Philadelphia in the 1770s] some five hundred public pumps supplied the citizens with their water, and never failed to make an impression on visitors" (Rebels and Gentlemen, p. 11), while Warner states: "there were no public schools, no public water, and at best thin charity" (Private City, p. 10). For Warner, eighteenth-century Philadelphia was a "community" only in the limited, literal sense of offering a high degree of face-to-face contacts among its inhabitants.

The main similarity between the Bridenbaughs and Warner, as social historians, lies in their common awareness of the importance of economic conditions and such factors as social class, social mobility, and ethnicity. On topics of this kind the two accounts overlap a good deal, although the Bridenbaughs emphasize trade routes linking Philadelphia with the outside world (their effect in creating a cultural cosmopolitanism is an implicit theme of their chapter), while Warner emphasizes artisans' work and living patterns within the city itself, since they formed the bulk of the population.

17. Or at least of that generation of anthropologists more generally. The subtitle of his book (ibid.) reveals this.

18. Questions of language, however, return in Thernstrom’s book at the level of word choices made to describe particular statistical results — e.g., that Bostonians had “good chances” to rise (Stephan Thernstrom, *The Other Bostonians: Poverty and Progress in the American Metropolis, 1880-1970* [Cambridge, Mass.: Harvard University Press, 1973], p. 73) — because each such summary statement conveys a given degree of optimism or pessimism in viewing the workings of American society. Any book containing words as well as figures no doubt will retain this dimension.

19. Thernstrom’s image of a city such as Boston also seems more mechanistic than Handlin’s, as when he calls Boston “a major importer and a major exporter of human raw material” (ibid., p. 29).

20. In fairness, this is only an inference from Thernstrom’s general silence about inner mental states, together with his decision that social mobility is a centrally important theme to explore.

21. Of course this is ironic, since Handlin explicitly dislikes ethnic conflict (e.g., see *Boston’s Immigrants*, p. 229). In his treatment of blacks (*The Other Bostonians*, pp. 176-219) Thernstrom uses his statistical sources to emphasize the bleakness of the life-chances of the group, reenforcing a highly critical view of white domination in American society. This is in striking contrast to Handlin’s astonishingly rosy view (*Boston’s Immigrants*, pp. 179-80, 212-13) of black prospects in a lightly earlier Boston.


24. A suggestion recently made by Henry F. May in conversation with the author.
