WHAT GOOD IS URBAN HISTORY?[1]

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What good is urban history? Five sentences sum up my message:

1. In principle, urban historians have the opportunity to be our most important interpreters of the ways that global social processes articulate with small-scale social life.
2. In practice, they have turned unseeing eyes to the challenge.
3. They need not remain forever blind.
4. Although urban historians exhort a great deal, their work does not respond to exhortation; only concrete examples—preferably including dissertation-size chunks—will move them to new forms of investigation.
5. No use talking about it; someone will have to *do* it!

My business here is to restate, elaborate, illustrate, qualify, and defend these five sentences. I hope to do so vividly, anabiotically (that is, in a way that will help resuscitate the dormant body of urban history), and without vituperation. I will argue that urban historians can move toward a central position in history as a whole by taking two large steps: *first*, by boldly addressing history's central questions rather than huddling, cramped, in the shelter of urban history's conventional problems; *second*, by adopting a reflective historicism. To minimize distracting errors of act and judgment, I will illustrate my sermon chiefly by reference to fields of urban history in which I have been working myself.[2] No one should therefore draw the conclusion that my personal agenda should take priority over others that will have similar effects on the discipline.

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First, the move toward centrality. What opportunity do urban historians face? "The role of social history," Olivier Zunz declares, "is to connect everyday experience to the large structures of historical analysis and major changes of the past. We believe that history should illuminate the complex interplay between large structural changes and alterations in the character of the dynamics of populations, social hierarchies, and routine social life."[3] Since Zunz and I thrashed out that conception of social history's mission together, you will not be surprised to learn that I agree with it. I only want to add that *urban* history plays a starring role in the drama.

To treat urban history as quintessential social history gives us the means of addressing central historical questions such as:

- How, if at all, do the ways that ordinary people cope with daily life impinge on power and policy at a national or international scale?
- What actual difference does it make to the quality of life what sort of political system people live under?
- Do rising technological and organizational complexity rob life of its spontaneity and wonder?
- How and why did capitalism come to be the dominant form of economic organization in western countries?
- How and why did relatively large, centralized, and unified national states displace the city-states, city-empires, dynastic empires, and federations that once predominated among the world's states?

Those who do not care for such suprahistorical questions can use urban history to address grand problems at a national scale. In American history, for example, urban historians can reasonably ask:

- To what extent did widespread demands for popular sovereignty inform American struggles of the 1760s and 1770s?
- Did the Civil War pit fundamentally incompatible ways of life against each other?
- Did a new alliance of big capital and the state crush working-class radicalism in the 1870s and 1880s, thus producing what analysts now describe as American exceptionalism?
- To what degree, and why, did the range of trades open to urban blacks narrow after 1890?

- Did militant feminism and other political mobilizations of women reinforce each other, contradict each other, or proceed in relative independence of each other?

In all these cases, and many more, cities offer privileged sites for study of the interaction between large social processes and routines of local life. Urban historians not only have superior access to the sites, but also know more—or should know more—than other historians about the bases of variation in these regards from one time and place to another. Let me return to the study of variation later. For now the crucial things to understand are: a) Such pressing questions fall clearly within the purview of urban history; b) In the past, when urban historians such as Arthur Schlesinger, Sr., Oscar Handlin, Stephan Thernstrom, or Gary Nash *have* asked such questions effectively, nonspecialists have taken notice; c) Most of today's urban historians are not articulating such grand questions forcefully; they are playing a cautious, constricted game. Urban historians should move to the boldest edge of social history.

Not all of urban history, as actually practiced, falls into social history. Histories of architecture, urban form, urban government, or writings about cities contribute significantly to urban history but stand adjacent to social history rather than inside it. Let me salute those forms of urban history, which I frequently enjoy and use, then turn to the resuscitation of their neighbors that overlap with social history. Much of urban history deals directly with connections between global social processes—not only urbanization, but also trade, circulation of ideas, epidemics, commercialization, population growth or decline, proletarianization and capital accumulation, state formation and transformation, growth or decline of world religions, and so on—and the lives of individuals, households, shops, and neighborhoods. Urban history inevitably deals with such processes because cities constitute major sites and junctions for them even when and where most people live in the countryside. Behold a salient example: The study of protoindustrialization (expansion of manufacturing through the multiplication of small producing units) deals extensively with villages and farmsteads, yet no one will understand it without seeing its tight connections with urban markets and merchants.[4]

Much of urban history's agenda, furthermore, deals implicitly or explicitly with the impact of global processes on small-scale social life or (more rarely) the impact of small-scale social life on global processes. Every study of an urban real estate market touches the changing ways in which capitalists and political authorities interact to manage space. Each examination of work and family in cities enters a site buffeted by the great winds of economic and demographic transformation. All analyses of migration, race, religion, class, and ethnicity in cities treat, at least implicitly, influences that operate on a regional, national, or even international scale. We read Olivier Zunz's superb history of Detroit between 1880 and 1920 not only to learn about that beleaguered city but also to understand how the growth of factory-based industry reshaped American social life as a whole.[5] While disagreeing sharply with many of Mumford's ideas, Zunz takes up the challenge set by Lewis Mumford: to trace relations among large shifts in economic organization, alterations of urban geography, and changes in the quality of social life.

Dare I praise Mumford? Eric Monkkonen scores Mumford as a snobbish sentimentalist who neglected the details and realities of urban history.[6] One need not share Mumford's sometimes arrogant and inconsistent tastes, however, to recognize the power of two features that set him off as a great urban historian: first, his insistence on the close connection between internal lives of cities and particular configurations of power and production within which they lay; second, his fashioning of a theory in which the relative concentrations of state power and of commercial-productive activity stamped the character of urban life, including the degree to which it was tolerable at all.[7] Mumford's Baroque City differed from his Coketown chiefly in their relative emphasis on royal power and industrial production. Whatever we think of Mumford's analysis, it demonstrates the feasibility of fashioning theories that cross city boundaries to provide coherent accounts of life on the small scale and the large.

Monkkonen also argues that the short-lived "new urban history" collapsed because its practitioners failed to develop a more effective way of summarizing its results, avoided reflection on the more general significance of its main findings, and turned away from the enterprise rather than countering the widespread criticism it generated.[8] I remem-

ber the checkered history well, as Stephan Thernstrom's coeditor of a series in which appeared major studies concerning urban social mobility and as the teacher of many students who undertook urban history as collective biography. I recognize that what looked like a wave of the future in the late 1960s began to resemble spent foam on a littered beach only a decade later. Although they continue to thrive in European historical demography, massive urban collective biographies have almost disappeared from American urban history. Monkkonen's factors played their part. But Monkkonen misses the decline's more general causes.

The turn from urban history as collective biography corresponded to a wider disillusionment with formalization broadcast by such former social-scientific stalwarts as Lawrence Stone.[9] The various populisms of the 1960s, which had borne with them great hopes for systematic history from below, gave way to more cynical, discouraged, and elitist interpretations of popular experience. In its own terms, the Thernstromian program suffered dramatically from diminishing returns, both because no one figured out how to get a firm grip on city-to-city and year-to-year variation and because its greatest impact arrived first, in the very idea of confronting American mobility myths with presumably hard evidence. When it began to appear that few sons had left their fathers far behind, urban historians searched elsewhere for keys to social change, and publishers lost interest in printing yet another study of Irish and Italian workers' social mobility—or lack of it—in yet another middle-sized city.

Urban collective biography has not entirely disappeared; it remains important, for example, in studies of immigration, ethnicity, occupational history, and political mobilization.[10] A number of investigators are applying it effectively to the study of conflicts.[11] It is a pity that no similar method, nicely fitted to a dissertation-size chunk of research that will be comparable to other dissertation-size chunks, has appeared to replace the local mobility study. But we should not regret the mobility study's passing. I now believe (but, alas, did not see twenty-five years ago) that the conventional study of social mobility rests on a series of misconceptions: that occupations form neat hierarchies, that they determine their holders' life chances, that explanations of lifetime and intergenerational movement from occupation to occupation rest

chiefly on individual characteristics, and so on.[12] It neglects three fundamental features of social inequality:

1. Few inequalities actually compound into uniform hierarchies; viewed over more than a single pair, most varieties of inequality are fragmentary and inconsistent. A web provides an apter metaphor than a ladder.
2. Inequalities between any two social units vary significantly from one setting to another as a function of resources available to each party in that setting and relations with third parties activated by presence in that setting.
3. Any particular actor's power, influence, and control over resources generally decline with time and distance. One party is often very powerful, rich, or prestigious *vis à vis* another on his home territory, but weak elsewhere; a theorist of inequality might try to contend with this difficulty by calling the party that exercised superiority in a larger territory or long time period higher-ranking, but only at the risk of neglecting variability in the parties' relative power, wealth, or prestige.

More than anything else, prevailing conceptions of social mobility entirely neglected the exercise of power—power of ethnic groups and power of political authorities. That recognition leads to identification of the fundamental problem: Treatment of each city as a sample case from a national frame blinds analysts to relations between processes generating or sustaining inequality in any particular city and regional, national, or international flows of capital, labor, and political power. Urban history connects with general history through just such interactions.

The study of state formation provides an unexpected but compelling case in point. Everywhere that cities have grown, their ruling classes have gained privileged access to the trade and capital of large regions. States have always expanded through the amassing of armed force. At times the distinction between cities and states has virtually dissolved; historically, city-states and city-empires in which state authorities and urban ruling classes overlapped have constituted one of the most common forms of government at the larger scale. Where urban elites and state authorities formed partly distinct categories, they sometimes warred and always bargained; they could find common ground to the extent that states acted to protect trade and capital and to the extent

that merchants and capitalists helped finance armed force. The relative strength of coercion-wielders and capital-wielders, however, has varied tremendously from region to region and time to time. In Europe over the last millennium, for example, merchants have generally been freer to act in the commercial belt from northern Italy to Flanders and southern England than elsewhere.

That variation wrote itself out in both directions: in the form of states whose bourgeois held significant power and whose armies benefited from relatively effective state financing and in the form of cities within which commerce left its imprint everywhere. State and municipal politics varied accordingly. In the quintessentially capitalist Dutch Republic of the eighteenth century, for instance, both warmaking and revolution were indistinguishable from the politics of Amsterdam and other cities.[13] In Poland, on the other hand, great nobles not only repeatedly strangled royal power, but kept merchants from gaining any significant autonomy, much less political power; urban form reflected the dominance of a landed nobility.[14]

American urban history offers plenty of opportunities for similar two-sided analyses, if only historians will remove their blinders. It also provides a splendid arena for renewal of historicism.

In the social sciences and history, the most prominent current of the last decade has no doubt been the drive toward the treatment of social life as discourse, and away from belief in reliable knowledge based on observation or external reconstruction.[15] Among the dwindling number who still believe that social life follows knowable, orderly principles, however, sociologists and other social scientists have recently given increased credence to thoughtful historicism. That sociological historicism examines how the residues of action at a given time constrain subsequent action. It goes at least a step beyond Robert Merton's old, important analysis of purposive social action's unanticipated consequences by showing how the embedding of social action in time and place affects the possibility of social action in subsequent times and places; whole chains of causation, unique yet profoundly regular, result.[16]

Merton accomplished only half the job: He enumerated good reasons why purposive action so regularly produces unexpected outcomes, thus raising doubts about all rational-action accounts of social behavior. But he left untouched the other half: how purposive social

action nonetheless produces systematic, durable social structure. The answer, it seems to me, lies in four principles: a) all social interaction consists of incessant errors, constantly corrected, b) people draw their correction mechanisms from historically accumulated shared understandings, from culture, c) interaction occurs within constraining webs of previously established social relations that it alters incrementally, and d) both culture and social relations change systematically and lay down durable social structure. That is why historicism matters.

In his discussion of the way that craft organizations persisted within some industries well into the era of mass production, Arthur Stinchcombe has long since provided an important example of that sort of historicizing analysis.[17] Allan Pred has similarly shown how existing connections among cities in eighteenth-century North America constrained subsequent growth of the North American urban system.[18] In a phrase faintly echoing Karl Marx, Pred has preached that "[p]eople do not produce history and places under conditions of their own choosing, but in the context of already existing, directly encountered social and spatial structures."[19]

Such reasoning contrasts sharply with unhistoricist or antihistoricist explanations of social life, including the life of cities, as the immediate effect of market forces, or prevailing national attitudes, or other causes that act instantaneously, generally, and heedless of prior events. Antihistoricist thought often appears in historical analysis, for example in great swaths of economic history. A fine example of antihistoricist thinking in urban history comes, indeed, from Lewis Mumford. In Mumford's *City in History*, the prevailing conjunction of political and economic power largely determines the activity and form of cities, regardless of the paths by which they have arrived at their present condition. A historicist alternative could, in principle, fly as bold and sweeping as Mumford's analysis; so far, however, in Mumford's domain the chief large-scale historicist enterprises consist of archeological, town-planning, and architectural compendia. Such studies sometimes cover large spans of time and space, but in those cases always lack synthetic power. A truly historicist synthesis to compete with Mumford's stands high on the agenda of an anabiotic urban history.

Allan Pred grounds his sophisticated analysis in two elementary postulates: a) a person can only be in one place at a time, and b) each

combination of location and time, and its residues, affects the possibilities for action at subsequent locations and times. In a provocative book, Pred calls for "merging of historical study, human-geographical inquiry, social analysis, and the (re)formulation of social theory."[20] He proposes taking seriously the process that Anthony Giddens calls *structuration*: historical formation of situations delimited by time and space whose enduring social arrangements people take for granted and allow to shape their actions.

Cities constitute prime examples of such constraining situations, with their recognizable street plans, distributions of stores, transport lines, labeled and segregated neighborhoods, configurations of political power, patterns of policing, and much more to take for granted. Examining the creation and influence of such situations, including cities, entails a difficult set of tasks. It entails accounting for the creation of the setting's constraining features through interaction of local social relations and those that cut boldly across time and space. It entails tracing ways the constraints shape those local activities people pursue more or less deliberately. It entails following processes—job-finding, courting, spending money, and more—in which where and when they happen strongly affects *how* they happen.

Common sense, you say? Yes, profoundly. But much of social science, and some historical analysis, counters this brand of common sense. It proceeds as if concrete time and space hardly affected the operation of social mobility, job performance, child care, homicide, or saving for a rainy day; societywide mentalities, the state, prevailing discourse, social control, class, gender, and the market (all, in common analyses, timeless, spaceless entities) caused social mobility, job performance, child care, homicide, and saving for a rainy day in essentially the same way from one time-space setting to the next. Even urban historians, who should know better, ordinarily oscillate between the time-space particularism of local history and grand timeless, spaceless processes, causes, and effects. Either they take cities as undifferentiated points within interurban processes, such as urbanization and migration, or they take city limits as boundaries for the analysis of ostensibly self-contained urban processes.

The historicist line of thought gains power from its easy junction with the theory and research that Harrison White, Ronald Burt, Barry

Wellman, and others call "structural sociology"; it begins with the idea that the fundamental units of social organization are neither individuals nor societies, but social relations.[21] Social relations between pairs of individuals compound into networks whose form varies significantly, for example from the long chains of relationship created by migration between a Jamaican village and Toronto to hierarchical connections among mobile engineers or surgeons. The analysis of network structure makes it easier to see constraints placed by a given configuration of social relations on succeeding sets of social relations among the same actors.

The linking idea is simple and powerful: Past social relations and their residues—material, ideological, and otherwise—constrain present social relations, and consequently constrain their products as well. Once an employer has established ties with a particular source of labor, those ties affect her subsequent recruitment of labor, and may well reproduce themselves. Once developers have laid down a certain urban structure, that structure defines opportunities for further development. Once people adopt a certain national language, that language circumscribes the other people with whom they can easily communicate. Such processes produce connectedness within time and space that goes beyond simple temporal and spatial autocorrelation; every existing structure takes the place of many theoretically possible alternative structures, and its very existence affects the probabilities that the alternatives will ever come into being. In short, social processes are path-dependent. Their explanation, furthermore, always requires specification of counterfactuals—possible outcomes that did not occur in these times and places. That is, to repeat, why history matters.

Consider some examples. Proletarianization of one generation of workers strongly affects opportunities of the next generation of workers to become capitalists, artisans, or peasants. Efforts of great powers to build up the military capacities of friendly Third World states shape the likelihoods that national armed forces will take over those states. Creation of collective-action repertoires through struggles between powerholders and their challengers limits possibilities of action for all parties in the next round of struggle. Intergroup conflicts over jobs, land, or political power create new social actors: active social classes, occupational guilds, political communities, ethnic groups, parties, and

so on. The presence of those organized actors then alters the character and outcome of conflict. The social organization of migration affects the subsequent welfare of migrants and their descendants, among other reasons because some forms of migration build means of capital accumulation within families and ethnic groups, while others individualize whatever accumulation occurs. In all these processes, time and place matter fundamentally; when and where they occur affects *how* they occur. They therefore fall into history's domain.

Social historians and urban historians have found this ostensibly straightforward lesson difficult to learn. The difficulty shows up clearly in studies of immigration to American cities, where abstract models of adaptation and assimilation uneasily encompass the concrete, swirling contingency of social life. For many years, American analysts of immigration saw it chiefly as an abrasive confrontation between preindustrial cultures and the hard, rational, prosperous ways of American life in which immigrants painfully shed old ways and adapted, with varying success depending on their origins, to demands of urban industrial organization. How many days urban historians have wasted trying to deduce or verify or falsify "assimilation" by constructing tables comparing people who have lived in a city varying lengths of time! Ideas of human capital have compounded the difficulty by portraying immigration as a sort of footrace in which the previous training, current fitness, familiarity with the course, will to win, and inbuilt cunning of different runners determine the order of their arrival at a common finish line.

Fortunately, however, the situation is changing. In recent years' rethinking, as John Bodnar sums it up,

> what emerges is a clearer portrait of the process of social change stimulated almost incessantly by the changing imperatives of the marketplace and the diverse responses of human beings themselves. Their response, conditioned by their social situation, familial status, and ideological orientation, becomes a variable itself helping to structure not only their own life path but even somewhat the all embracing economic system. Ordinary individuals are rescued from the status of victims; they are not simply manipulated by leaders, their class standing, or their culture, but active participants in an historical drama whose outcome is anything but predictable.[22]

Restoring agency to ordinary immigrants, the new sociology and social history of immigration has also restored history to immigration; instead of timeless recurrence of social mobility, we have contingent actions of individuals and groups set firmly in time and space, with durable but likewise historically contingent consequences.

Do not get me wrong. I am not for a moment advocating a return to descriptive particularism on the ground that human life is complicated, varied, and unpredictable. On the contrary, I am claiming that human life conforms to a deep order. That order does not lie, however, where urban historians ordinarily look for it: in uniform behavior of large categories of people or standard sequences of multiple lives. It lies instead in the combinatorics of multiple causes.

Relations among circular migration, chain migration, and ethnic solidarity illustrate what I have in mind. Contrary to the logics of minimizing distances and multiplying opportunities, over and over again people have established regular migrations between two widely separated locations, then concentrated their migration within that bipolar system rather than continuing their search for opportunities outside of it. Instead of maximizing, they have satisficed. Chain migration is, of course, the arrangement in which social ties persist between people at a particular origin of migration and a particular destination of migration, with people at the destination sending back information about new opportunities, recruiting new immigrants, and helping them make the move; every student of immigration has fantastic but perfectly familiar tales to tell about sustained flows between small villages and urban neighborhoods that lie thousands of miles apart.

Migration chains often—I think almost always—originate in circular migration, in circuits whose members stay tied to the same base, but periodically move away to earn money by peddling, digging, harvesting, building, working in factories, or other easily transportable activities. In such circuits, earners typically restrict their expenses and repatriate a significant share of their income, investing it in land, consumer durables, or social solidarities back home. Under some conditions, a few members of such circuits prolong their stays at one of the destinations. Those conditions are regular and comprehensible, but we do not yet have a satisfactory general model of their operation; here is another worthy challenge for urban historians.

Frequently, but not always, such exiles originate migration chains. Chains often continue to work in conjunction with circular migration; many workers spend some time at the destination only to return home with their earnings and learnings. Which ones stay, and which ones return, depend only distantly on their original intentions and qualifications. Instead, it depends closely on broad comparisons of opportunities at origin and destination. Many chains, nevertheless, produce large net movements between remarkably distant origins and destinations. Ethnic identities and institutions take on greater salience where a) emigrants and their descendants find themselves competing for jobs, housing, political power, and neighborhood space with members of other groups defined by origin, and b) success in those competitions depends on mutual aid within each population. The character of an ethnic community in a big city therefore results from the history and current organization of its migration system, the relative attractiveness of origin and destination, and its competitive position within local markets for jobs, housing, power, and space.

Urban historians should find almost everything I have said obvious; I have simply called attention to a standard feature of urban experience. Here, then, is my claim: Chain migration is contingent and particular on the one hand, yet exceptionally orderly on the other. The order consists not in the repetition of the same sequence over and over (although my simplified account may have fostered that illusion) but in the recurrence of critical choices: stops on a migration circuit, settling of exiles, maintenance of communication, remittance of income, availability of opportunities for work, housing, protection, and sociability at destinations, and so on. Those choices themselves follow historicist logic, depending heavily on what has happened previously in particular times and places. The combinatorics of those choices produce the diverse histories of particular migration chains and, for that matter, of circuits that never produce chains. I do not claim we know the combinatorics well. I claim that we are dealing with highly coherent urban processes that historicist thinking illuminates.

A historicist view does not merely clarify how people move from one distant place to another; it helps make sense of connections between migration and durable forms of inequality, including those forms people organize as ethnicity—as structured differences according to imputed national or racial origin. The social organization of mass migration surely contributed powerfully to the creation of durable inequalities in American cities and the United States as a whole. If urban historians want subjects to bring them fame outside their own bailiwick, the explanation of durable ethnic and racial inequality, as well as of differential cohesion within ethnic and racial categories, certainly qualifies. The analysis of migration, however, simply illustrates the great opportunity urban history faces, and has not so far taken full advantage of. Cities constitute our best laboratories for investigation of historical contingency—the way that social action in a given time and place constrains what will happen next there and in adjacent places, what will happen after that, and so on through long strings of path-dependent processes.

With all these references to "path-dependence," "historicism," "contingency," and similarly abstract notions, readers may fear that I am recruiting players for a strange game on an alien field—economics, sociology, or philosophy, perhaps, but not urban history. Certainly I am claiming that urban historians could participate more centrally in epistemological and ontological debates that now range across history and the social sciences. But I also claim that urban historians who follow my advice will command more attention from fellow *terre-à-terre* historians as well as from general readers. For to unravel the causes and effects of durable inequality, of violent xenophobia, of stable democracy—all clearly subjects within the purview of urban history—is to enlighten and even to improve the world at large.

Yet my jeremiad is almost by definition a waste of time. I expect no one to jettison current urban research and rush out to follow my demanding agenda. Urban historians, for the most part, love particulars and fear grand schemes. If one thing is clear about them, furthermore, it is that for all their love of hectoring each other, they respond almost exclusively to concrete, imitable examples of good work that help answer questions they regard as worth addressing. They look for practical demonstrations of feasibility and profitability of new approaches rather than the cleverness of appeals that people make for those approaches. If practical examples come in modules that conform nicely to the requirements of doctoral dissertations, so much the better; urban history demands so much devotion that most practitioners have

only one sustained monograph in them, their doctoral dissertation and its revisions.

The rapidity and enthusiasm with which urban historians adopted Stephan Thernstrom's technology thirty years ago proves that they are not simply stodgy, but properly skeptical of unsubstantiated promises. Clearly for the program I have sketched to have any impact on the practice of urban history, some of us will have to go out and roll up our sleeves, take risks, show results, and demonstrate the superiority of an urban history that attacks big questions, links large processes with local life, and takes the effect of time and place seriously.

The most I can hope for now is a ricochet: I wish that some of the shells I have propelled against the rocks of this vast canyon would rebound to precisely the spots where teachers and students are searching for tinder, in hope of lighting fires that people will see outside the canyon. If sparks from my clumsily loaded, awkwardly fired, and excessively loud blunderbuss reach some dried and yearning wood, perhaps we can again see great bonfires in urban history, from whence sleepy smoke so long has risen.

NOTES

1. Revised version of an address to the Research Conference on Modes of Inquiry for American City History, Chicago Historical Society, October 1990, whose text circulated under the same title as Working Paper 99, Center for Studies of Social Change, New School for Social Research, 1990. A Czech translation of the working paper appeared as "K cemu je dobra historie mesta?" *Sociologicky casopis* 28 (1992), 437-50.

2. E.g. Charles Tilly, "Transplanted Networks," in Virginia Yans-McLaughlin, ed., *Immigration Reconsidered. History, Sociology, and Politics* (New York, 1990); "Police, Etat, contestation," *Cahiers de la sécurité intérieure* 7 (1991), 13-8; "Cities, Bourgeois, and Revolution in France," in M'hammed Sabour, ed., *Liberté, Égalité, Fraternité. Bicentenaire de la Grande Révolution Française* (Joensuu, Finland, 1992), 14; *Coercion, Capital, and European States* (Oxford, revised ed., 1993); "Entanglements of European Cities and States," in Charles Tilly and Wim Blockmans, eds., *Cities and the Rise of States in Europe, AD 1000-1800* (Boulder, 1994); with Eiko Ikegami, "State Formation and Contention in Japan and France," in James L. McClain, John M. Merriman, and Ugawa Kaoru, eds., *Edo and Paris. Urban Life and the State in the Early Modern Era* (Ithaca, 1994).

3. Olivier Zunz, "Introduction" to Olivier Zunz, ed., *Reliving the Past. The Worlds of Social History* (Chapel Hill, 1982), 5-6. For reviews of social history, see Joseba Agirreazkuenaga and Mikel Urquijo, eds., *Storia Locale e Microstoria: Due Visione in Confronto* (Bilbao, 1993); Lenard R. Berlanstein, ed., *Rethinking Labor History* (Urbana, 1993); Peter Burke, *History and Social Theory* (Ithaca, 1992) and *The Art of Conversation* (Ithaca, 1993); Julián Casanova, *La Historia Social y los Historiadores* (Barcelona, 1991); Arlette Farge, "L'histoire sociale," in François Bédarida, ed., *L'histoire et le métier d'historien en France 1945-1995* (Paris, 1995); Carlo Ginzburg, *Clues, Myths, and the Historical Method* (Baltimore, 1986); Carlo Ginzburg and Carlo Poni, "The Name and the Game: Unequal Exchange and the Historiographic Marketplace," in Edward Muir and Guido Ruggiero, eds., *Microhistory and the Lost Peoples of Europe* (Baltimore, 1991); Michael P. Hanagan, "New Perspectives on Class Formation: Culture, Reproduction, and Agency," *Social Science History* 18 (1994), 77-94; Don Kalb, "Frameworks of Culture and Class in Historical Research," *Theory and Society* 22 (1993), 513-37; Carola Lipp, "Histoire sociale et *Alltagsgeschichte*," *Actes de la Recherche en Sciences Sociales* 106-7 (1995), 53-66; Christopher Lloyd, *The Structures of History* (Oxford, 1993); Erik Monkkonen, "Lessons of Social Science History," *Social Science History* 18 (1994), 161-8; Germán Rueda Hernanz, ed., *Doce Estudios de Historiografía Contemporánea* (Santander, 1991); Dennis Smith, *The Rise of Historical Sociology* (Philadelphia, 1991).

4. Alain Dewerpe, *L'Industrie aux champs. Essai sur la proto-industrialisation en Italie du Nord (1800-1880)* (Rome, 1985); Myron P. Gutmann, *Toward the Modern Economy. Early Industry in Europe, 1500-1800* (Philadelphia, 1988); Peter Kriedte, Hans Medick, and Jürgen Schlumbohm, "Sozialgeschichte in der Erweiterung - Proto-industrialisierung in der Verengung? Demographie, Sozialstrukture, moderne Hausindustrie: ein Zwischenbilanz der Proto-Industrialisierungs-Forschung," *Geschichte und Gesellschaft* 18 (1992), 70-87, 231-55.

5. Olivier Zunz, *The Changing Face of Inequality. Urbanization, Industrial Development, and Immigrants in Detroit, 1880-1920* (Chicago, 1982).

6. Eric H. Monkkonen, *American Becomes Urban. The Development of U.S. Cities and Towns, 1780-1980* (Berkeley, 1988).

7. Lewis Mumford, *The City in History. Its Origins, Its Transformations, and Its Prospects* (New York, 1961).

8. Monkkonen, *America Becomes Urban*, 28.

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