Neat Analyses of Untidy Processes

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A Wave of the Past

How times have changed! Toward 1970, historians of many stripes were becoming enthusiastic about formal methods of analysis. "Formal methods" include a wide range of procedures that match descriptions of structures and processes with explicit models of those structures and processes. Formal methods do not necessarily involve quantification or computing; analyses of linguistic, geographic, or temporal structure, for example, can proceed quite formally without computers and without any direct invocation of mathematics. But the formal methods that absorbed history's technical innovators in the 1960s and 1970s typically included quantification or computing, or both. Why and how did the enthusiasm of the 1960s dwindle?

Taking advantage of the freedom provided by a rubric called "controversies," this brief essay will present thinly documented speculations concerning the past and present of formal methods in labor history and will end with a conjecture about their future. The main body of my own historical research lies outside the heartland of labor history as usually conceived. Yet it often deals with work and workers, and it frequently involves formal methods. The following speculations and conjecture therefore come from an interested outsider who has a particular concern with the effective use of formal methods in history as a whole. The scattered examples will come mainly from historical research on Europe since 1700.

Formalization gained a number of forceful advocates among historians in the 1960s. To some, the increasing availability of formal procedures for the investigation of large numbers of cases opened the way to science and certainty. A kind of populism attracted others to the possibility of letting inarticulate people speak for themselves via the real behavior reflected in parish registers, arrest lists, and similar sources. In either case, the path toward formalization typically led through collective biography: the assembly of standardized descriptions of individual units—persons, households, firms, places, events, or something else—into portraits of the entire sets and into means for studying variation among the individual units. When numbers became substantial and equipment became available, historians often called on computers to collate the descriptions and carry out the analyses of variation.

The approach had important successes. Without formal analysis based on

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collective biography, we would lack almost all of historical demography, most city-by-city studies of social mobility, major treatments of political activism, and much, much more. Demographic, social, urban, and economic history all underwent significant renewals through the introduction of formal analysis and collective biography. That many wheels spun idly and that the ratio of results achieved to effort expended was often painfully low goes almost without saying; such things usually happen when unprepared people start experimenting with complex new techniques and equipment. On balance, nevertheless, the introduction of formal procedures enriched the possibilities of historical analysis.

Despite indignant complaints about the eruption of positivism into history, many historians then felt that formalization and quantification were the wave of the future. Jacob Price and Val Lorwin—no wild-eyed enthusiasts, they—introduced their volume on quantitative history with the declaration that

> from France to Scandinavia to Japan, quantitative ways of thinking, quantitative approaches, and quantitative methods have entered the mainstream of historical investigation. In all areas, major quantitative work is now being done, and even more is likely to be done in the immediate future. The neglect of the possibilities of quantitative research by so many American historians working on topics outside of United States history leads to an unnecessary restriction of their analytical techniques and an unfortunate enfeeblement of their results. Not all problems are equally suitable for quantification; nor will quantification ever become the exclusive or even preponderant form or mood of historical investigation. Yet if historians in the United States and other English-speaking lands working on the history of other countries wish to move to exciting frontiers of research endeavor in their respective areas of interest, a greater proportion of them than at present will have to think and work in part quantitatively. (Lorwin and Price 1972, 10)

Price and Lorwin's statement, although restrained and sensible in its context, rings quaintly today: "Exciting frontier of research endeavor"? In economic, demographic, and electoral history, quantification has ceased being an adventure in itself, while almost everywhere else quantitative analysis has lost much of its following. It is now fashionable to decry formal methods as sterile and reductionist, to insist on the centrality of consciousness, mentalities, and culture in historical experience, and therefore to regard textual explication, retrospective ethnography, and the construction of intelligible narratives concerning daily experience as history's frontier. As Eric Monkkonen, an experienced quantifier, reports: "From scholarly journals to the *New York Times*, historians have been castigating themselves for excessive narrowness and a decline in

the public voice of their profession. This critique has been articulated through a call for a return to 'the narrative,' which seems to mean well told, dramatic stories of the past, which attract large readerships, public attention, and respect. Indirectly, quantitative history has borne the brunt of this critique, though it includes many nonquantitative forms of history as well" (Monkkonen, 1984, 89). The new critique has an ironic side. It arrives more or less in step with the long-awaited appearance of major works of quantitative social history such as Wrigley and Schofield's *Population History of England* (1981) and Stone and Stone's *An Open Elite?* (1984). But because Lawrence Stone himself has lent an influential voice to the critique, it represents more than a discordant noise in the profession.

At least in the Anglo-Saxon world. Continental Europe looks different. There, formal analyses are still proliferating: studies of Nazi membership, enumerations of Swiss *Aktivierungsereignisse,* content analyses of medieval texts, and much more (see the bibliography for examples). There, furthermore, even studies concentrating on qualitative variations and states of mind commonly turn to some sort of formalization as an auxiliary to their analyses. Daniel Roche's treatment of eighteenth-century French provincial academies (1978), for instance, deals mainly with the organization and culture of those quintessential Enlightenment institutions but does not hesitate to map, graph, or quantify the activity of the provincial savants: not only such obvious features as social origin and age at death, but also more esoteric matters such as themes of poetry read and contents of appointment letters. Continental institutional, cultural, and intellectual historians often turn to formal methods of analysis.

To some extent, the difference between Anglo-Saxon and Continental European reliance on quantification reflects differences in the questions being asked. Generally speaking, quantification provides little help in attempts to account for single instances of anything, especially if the explanations being considered rest on general traits of the individual, group, or place involved. Quantification becomes more useful as a function of complexity of the explanatory model, intrinsic quantifiability of the phenomenon to be explained, importance of variation to the argument, and number of units observed. Any form of "exceptionalism" tends to make quantification uninteresting, even distasteful. Thus the greater readiness of continental scholars to place their cherished subjects in a comparative frame, and yet to employ complex arguments, inclines them toward quantification.

What about Labor History?

Where we place labor history in changing historical practice depends on how we bound the field. Analyses of strike activity, for example, have not undergone the rise and fall of quantification that I have described. On the whole, the frequency, complexity, and formality of strike analyses have risen throughout the last

few decades. Do all those time-series treatments of strikes by economists belong to labor history? Outside of strike data, on the other hand, international comparisons of labor movements have not moved significantly toward quantification during the period since World War II. Do those comparisons constitute the heart of labor history? What of Olivier Zunz's 1982 study of Detroit's changing social geography from 1880 to 1920? David Levine's work (1984) on English proletarianization? John Bohstedt's study (1983) of riots in England and Wales from 1790 to 1810? All concern workers and, to some extent, work. In or out? Probably neither and both.

How shall we bound this peculiar field? As labor historians actually organize their enterprise, it resembles a twin star surrounded by a great deal of cosmic debris. The stars revolve around each other, each simultaneously repelling and attracting its partner. The first star is the development and change of national labor movements. The second star is the connection among the organization of production, class formation, and workers' collective action. The two have clear affinities: Workers' collective action provides a significant share of the information under the heading of "national labor movements," while the organization of production and class formation combine to provide many of the explanations historians propose for the development and change of national labor movements.

Yet some tensions also drive them apart: uncertainty about whether the national arena is generally the coherent and relevant one for workers' action; instances in which national workers' politics (or what passes for workers' politics) correspond only weakly to grass-roots action; similarities among the experiences of workers in similar industries but in very different national contexts; desire to explain variations in orientation and action from one group to another within the same country; and so on. The two stars continue to revolve around each other without merging into a single giant sun.

The rest of the solar system consists of everything else: local and regional labor movements; the three elements (production, consciousness, and action) taken singly; the urban geography of work and class; segregation of work and workers by sex, race, age, or national origin; labor migration; working-class families; daily life; and many of the other topics on which contributors to, say, *International Labor and Working Class History* actually work. These topics remain peripheral to the field of labor history in three senses: first, in being less certainly part of it, and more possibly part of other specialties such as urban or family history, than are the core topics; second, in being marginal to such unifying models, statements, and research programs as come along; and third, in seeming less important to professional practitioners of labor history.

Labor History as a Discipline

The last point deserves elaboration. In any discipline, members organize themselves in two fundamental ways: by creating a bounded interpersonal network,

often one that is formalized via organizations, meetings, journals, and similar devices, and by establishing a shared agenda that includes pressing questions, certified means of answering those questions, and a recognized body of relevant evidence.

Let us concentrate on the pressing questions. All historical fields having any practical coherence organize around a very limited number of "payoff questions"—questions that define the field, whose pursuit requires little or no justification among practitioners, with respect to which specialists are instantly alert to new answers, confirmations of disputed answers, or challenges to widely accepted answers. At any given moment, only a limited number of alternative answers to the big questions are typically in play; otherwise, members of the craft worry about its disarray.

Given labor history's twin structure, it actually organizes around two partly independent sets of questions. One set sums up to the very broad query, What relationships exist among the organization of production, the formation of social classes, and workers' collective action? Under that broad rubric fall narrower and somewhat more manageable questions such as, Which kinds of workers, in what circumstances, most regularly engage in class-conscious militancy, and why? That and a few other questions inform a significant share of research and writing in labor history.

The other cluster of questions cumulates to this one, What historical circumstances determine the rise and fall of militant or effective national labor movements or both? This question, unanswerable as stated, breaks into a small series of less general queries. Within labor-history-defined-as-national-movements, one of the few venerable payoff questions concerns why there is so much more socialism in some countries and periods than in others. Broadly speaking, the main alternative answers to that old problem now under serious consideration are variants of the following:

1. The organization of capitalist production varies significantly over time and space, and only some (few) versions of it promote sharp confrontations of labor and capital; those confrontations produce support for socialist programs.

2. The political strategy of states and national elites—for example, cooptation and corporatism—strongly affects the availability and viability of a socialist reply to capitalist power.

3. Other features of social life, such as the presence of ethnic divisions, the diffusion of bourgeois styles of life, or the structure of workers' residential communities, govern the extent of working-class consciousness, and therefore the support for socialism.

4. Specific historical leaders and experiences, such as responses to the Depression of the 1930s shape the political choices and possibilities available within any particular state.

Stated so generally, to be sure, these answers could all be true simultaneously. Only when a historian specifies one of the statements further (for example, by claiming that American geographic and class mobility diminished working-class consciousness) or assigns preeminence to one of them (for example, by insisting that working-class socialism appears only in early phases of rapid industrialization) do sharp contradictions develop. But historians, including labor historians, proceed by alternation between the deliberate sharpening of such contradictions and the judicious synthesis of competing arguments. The choices, and the balance among the choices, remain fundamental to their work. At a given time, only a handful of such questions define the overall agenda of the entire field.

The Peripherality of Formal Analysis

Labor history as I have described it has an indefinite boundary, a chaotic periphery, and a relatively well-defined core. Labor historians regard historical research and writing as important to the extent that it renews understanding of the conditions underlying national fluctuations in the militancy and/or effectiveness of worker action; helps connect the organization of production, the formation of social classes, and worker collective action; or both. That brings us back to the place of formal analysis in labor history. By and large, the successes of formal analysis have occurred in labor history's periphery. They include: time-series analyses of the determinants of fluctuations in national levels of strike activity; treatments of the organizational bases of workers' collective action; studies of the demographic correlates of different sorts of industrial organization; reconstructions of labor migration and its consequences; quantitative portrayals of occupational mobility; and research on the urban geography of migration, work, and workers. The bibliography includes a number of examples. These sorts of studies have great merits. But they do not address the organizing questions of labor history directly.

Consider the problem of explaining national fluctuations in the militancy or effectiveness of worker action or both: Formal analyses of strike activity and quantitative treatments of the organizational bases of workers' collective action begin to address that problem. Yet labor historians tend to question their validity and relevance on the grounds that the formal analyses in question consider too narrow a range of action, fail to provide convincing evidence on the orientations of the workers involved, and ignore the political context.

Basically, labor historians concerned with national fluctuations seem to want one or both of two things: persuasive reconstitutions of the shared states of mind of the principal actors at different points in time, and tactical replays of the interactions among various groups of workers, labor leaders, capitalists, political powerholders, state officials, and other significant actors in the national arena. Formal studies of strike activity and of the organizational bases

of worker collective action set some limits on the possible reconstitutions of shared states of mind, but they provide no effective means for getting at them directly. In principle, it is possible to capture tactical interplay in formal models; in practice, the difficulties of measurement and modeling entailed by the analysis of fluctuations in the national politics of labor will exceed anyone's technical capacity for some time to come. Instead, labor historians are likely to continue with analytically informed narratives and broad, complex comparisons of a few national experiences at a time. Neither of those enterprises will yield readily to formalization.

Or take the other core problem: the connections among the organization of production, class formation, and worker collective action. Several of the formalized analyses mentioned previously obviously touch on the problem: studies of organizational bases of worker collective action, labor migration, and social mobility. Yet labor historians tend to insist on the consciousness and experience contained in class formation and the political interaction affecting worker collective action. They also tend to broaden both "class formation" and "worker collective action" to embrace a wide range of behavior. In those circumstances, the existing formalizations become peripheral to the real enterprise, and the formalizations that are possible in principle become enormously demanding.

Common understandings of labor history's core focus on matters that yield to formal analysis only with great difficulty. Class consciousness is the obvious, and no doubt the most important, example. But recently different varieties of culture have preempted the territory previously occupied by class consciousness.

Nevertheless, the periphery constrains the core. Collective biography, as the central evidence-producing procedure of formal analysis, necessarily sets limits on a wide variety of arguments in labor history. Findings of studies dealing with labor migration, industrial conflict, daily life, and other "peripheral" subjects set limits on plausible reconstructions of the connections among production, class formation, and collective action, or on the explanation of fluctuations in national labor militancy and effectiveness. Studies by Victoria Bonnell (1983), Diane Koenker (1981), William Rosenberg (1978), and others concerning the organization and action of workers in Moscow and Petrograd, for example, now make it virtually impossible to portray working-class involvement in twentieth-century Russian revolutionary movements as a consequence of the thrusting of uprooted peasants into big-city industrial life. Again, research on the dynamics of rural industry by Franklin Mendels (1980, 1983), David Levine (1977, 1984), Yves Lequin (1977), and others has established the wide extent of rural proletarianization—and therefore of a kind of class formation—in Europe before the period of capital-concentrated industrialization, the complex interdependence between proletarianization and population growth, and the importance of regional systems linking the labor and capital of city and country. Over

the last two decades, important findings on such matters have emerged from formal analysis and would have been less likely to emerge without formal analysis.

Formalization, then, does have a bearing on the core questions of labor history. Under what circumstances might we expect formal analyses to become everyday activities of labor historians, as they have for economic, demographic, and urban historians? Three possibilities come to mind: (1) that some group of scholars who are directly addressing labor history's core questions will develop a kind of formalization that will transform the field; (2) that the core will shift to questions that now remain in the periphery and for which effective formal procedures exist; (3) that an intellectual revolution will establish a new core lending itself directly to formal analyses. None of the three is likely.

It is possible, but improbable, that some great success will establish formal analysis in the core of labor history. American urban history once concentrated on urban biographies and general portrayals of urbanization. It shifted rapidly toward some kinds of quantitative work when Stephan Thernstrom (1964, 1973, 1977) and a few other pioneers demonstrated that through a variety of collective biography urban history could produce results bearing on one of American history's grandest questions: To what extent is the United States a land of opportunity, and how much has that opportunity changed over time? In retrospect, one can see readily that the question has a quantitative, structural component that lends itself to formal treatment. In prospect, it is not so easy to see that either of the dominant agendas of labor history—the one linking production, class formation, and working-class action or the one dealing with national labor movements—will yield to formal treatments that most labor historians will recognize as contributions to their field.

It is possible, but even less probable, that the periphery will transform the core—that because of the transformation of our understanding of labor history via work on such matters as labor migration, gender, or industrial conflict, the established triad of production, consciousness, and collective action and the standard inquiries concerning national labor movements will come to seem less central to the entire enterprise. To some extent, such shifts *have* occurred in economic and social history; peripheral questions (such as how, if at all, industrialization transformed social relations within families) became core questions.

The creation of an entirely new core is unlikely and unpredictable. If it occurs at all, changes in the political environments of scholars concerned with labor—the success of a certain kind of revolution, the failure of another, a fundamental shift in the positions of workers and organized labor—will surely play a part in the redefinition of labor history's subject matter. In that unpredictable event, the discipline's organizing questions could move toward problems that lend themselves to formal analysis. They could also, however, emphasize problems that are even less amenable to formalization. This possibility therefore leads to no forecast at all.

A final disclaimer. I do not claim that a shift to formalization, or to the sorts of peripheral questions that lend themselves to formalization, would "improve" or even "clarify" labor history. I do claim that in the present organization of the field a great expansion of formal analysis at its core is very, very unlikely. Not unless the organizing questions of labor history change significantly will computing, quantification, and other formalizations become central to the discipline. That is my conjecture.

Promise or threat? Labor historians have that to decide for themselves.

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A Fable of the Bees: In Reply to Tilly

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Vast numbers throng'd the fruitful Hive;
Yet those vast numbers made 'em thrive . . .
Some with vast Stocks, and little Pains,
Jump'd into Business of great Gains;
And some were damn'd to Sythes and Spades,
And all those hard laborious Trades.

Bernard Mandeville,
"The Fable of the Bees"

Charles Tilly adds an authoritative voice to the growing chorus of doubt about cliometrics. One after another, once-determined quantifiers seem to be losing confidence. "Historians & Computers: Has the Love Affair Gone Sour?" asks Robert Swierenga. Lawrence Stone notes approvingly "The Revival of Narrative," and even cocksure prophets of "scientific history" are billing and cooing at the dowdy practitioners of "traditional history." Of course, not all militant number-crunchers have become so conciliatory. "The social-scientific merchants have developed not only an extensive trade, but a large demand within the historical community for their valuable products and a comprador class to look after their interests in the new territory," writes Morgan Kousser. "Isolationism would be ill advised even if it were possible."[1] But Tilly never was one to bluster this way, and when such a reasonable, self-composed quantifier turns Hamlet, it suggests there might be something rotten in the state of cliometrics.

What is it? And what does it have to do with labor history? Should labor historians worry that their field has not been transformed by cliometrics the way economic, urban, and demographic history have been, or should they be relieved to know they can now get by without having to learn matrix algebra?

On one point there should be universal agreement: To count, or not to count; that is *not* the question. No one should doubt the need for statistical measures of the changing size and social composition of the working class relative to other classes, the level and distribution of wealth and income within and between classes, or the allocation of labor votes among mainstream and radical parties. And no one need believe that the more esoteric, the more significant a statistic is; the most significant statistic is not the one altogether impossible to understand. Even simple numbers can carry important weight. Whether industrial workers are 20 percent or 40 percent of the work force in any given period is undeniably significant; where state power is seized in the name of the working class, it is worth knowing whether workers represented one-half or a tenth of the population. Nor can anyone rightly object to the more sophisticated statistical techniques per se. Keeping track of the sex, national origin, property ownership, and occupational biographies of hundreds of thousands of people normally requires sampling, significance testing, correlation coefficients, and all the rest. So long as labor historians persist in studying the behavior of large groups of people and the structures of their everyday lives, careful counting will be necessary.

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Of course, all of this assumes that a significant historical question is being addressed. It is probably no coincidence that many of the more worthy efforts in quantification started out by addressing the venerable (or antiquated) question of American exceptionalism. The resulting answers varied widely. The small library of mobility studies inspired by Stephan Thernstrom has tended to emphasize mobility as a solvent of class consciousness, socialist ideology, and worker discontent. However, Peter Shergold believes the most important fact of American working-class material life is neither individual mobility out of the working class nor Sombart's "shoals of roast beef and apple pie," but the vast gap between the lowest and the highest paid workers. Meanwhile, P. K. Edwards finds the American industrial battleground exceptional in being rife with struggle, as measured by the incidence of strikes and lockouts.[2] It is critical to recognize that these studies are important not so much because of their empirical findings alone, but because they bring to bear well-documented arguments upon a significant historical question.

So long as quantifiers ride herd on their techniques and do not allow their "neat analyses" to take over the interpretation of "untidy processes," they will have much to contribute to the dialogue about the meaning of the past. We can all think of worthy examples from the "new" political history, family history, or economic history. In labor history, Thomas Dublin's census trackings of rural migrants to Lowell mills and the changing urban patterns of family life, sex ratios, and housing enabled him to clarify the process of class formation and collective action. In the case of *The Rebellious Century* by Charles, Louise, and Richard Tilly, statistical techniques have been properly subordinated to probing historical questions.[3] The longitudinal indexes of collective violence are not made to stand alone; indeed, taken alone, they do violence to the fabric of history by conflating early nineteenth-century food riots with early twentieth-century fascist putsches. Not content to leave things in such a pureed state, the Tillys emphasize the changing historical context in which social groups struggled for power. Such books seem to fit well E. H. Carr's description of the historical method as a continuous process of molding facts to interpretation and interpretation to facts.[4]

Trouble begins when the whole fluid historical process is squeezed to fit simple sociological models (or worse yet, mathematical ones) easily susceptible to quantification. That the best behavioral models and the most sophisticated statistical techniques do not necessarily produce the best history has been demonstrated repeatedly by numerous dubious achievements in quanto-history. We can all think of examples, from the stupefying antiquarianism of some local studies of voting behavior, the journey to work, or mobility, to the monumental reductionism of *Time on the Cross,* in which slavery is reduced to a mere model of productive efficiency, a kind of interpretive "black hole" from which no insight can possibly escape. For different reasons, the risk in putting simple sociology and sophisticated technique ahead of theoretically informed analysis is well illustrated by Olivier Zunz's *Changing Face of Inequality.* Readers will learn much about the human geography of Detroit from this carefully crafted book, but little about the relation between changing forms of production, the evolving structure of society, and the resulting transformation in the modes of popular action. Using proxy data—ethnic origin for ethnicity and occupational prestige for class—Zunz finds that residential segregation by occupation increased faster than segregation by ethnic origin between 1880 and 1920. I see no reason to doubt that this is the case. But does this mean that the rise of the auto industry caused a "shift from ethnicity to class?" Was not Detroit already an industrial city in the late nineteenth century, with the organization of production between employers and wage laborers characteristic of an industrial class system? And will any labor historian take seriously Zunz's conclusion about the emergence of a labor movement in America: "The labor movement, then, postdated the creation of industrial America rather than growing with it and affecting its destiny."?[5] When will historians stop buying the cheap wares of simple sociological dyads like *Gemeinschaft/Gesellschaft?*

But is there something deeper, a fatal flaw at the core of formal analysis, that limits what it can do for the reconstruction of the past? If so, it lies in its evasion of the question of free will and determinism, or, if you like, the question of the interplay of social consciousness and social being. Not all quantifiers claim to practice "formal analysis," but those who do seem to mean they are searching for patterns and regularities in the data that can be portrayed in explicit models of behavior.[6] The intellectual genealogy of formal analysis reaches back to nineteenth-century empiricism and positivism. In so far as formal analysis accepts that the data have an independent existence in the past until discovered and processed by the historian, it affects the same posture as Acton, Ranke, and other nineteenth-century empiricists toward the pristine facts of history. This naive epistemology has somehow survived all the assaults upon it: American pragmatists like Becker and Beard in "revolt against formalism" insisted that the facts of history owe as much to the world view of the historian as to the documentary residues of the past; the British Marxist School insisted with Carr that history is "a dialogue between past and

present"; the *Annales* School and latter-day structuralists insisted on the importance of the *problematique* in reconstituting the past in the present.[7] All to no avail as far as formalists are concerned.

Equally perplexing is the persistence of positivist conceits handed down from the Comtean world where definable "laws" determined human behavior. To be sure, no modern day formalist speaks in a determinist way of "laws" and, instead, adopts the more modest language of "models" and "patterns." That may be a response to the scolding that economic determinism took at the hands of liberal philosophers in the 1950s who stressed "indeterminacy" (Popper), value judgments (Morton White), and "foxes" of little facts in preference to "hedgehogs" of big ideas (Berlin). But there are quite different grounds for challenging positivism that are not so freighted with defense of the "Open Society" (namely capitalism). In both his narratives and theoretical pieces, Edward Thompson has charted one route out of determinism by shifting from the "laws of historical progress" to "the logic of historical process." Raymond Williams speaks of determination not in terms of ironclad causes, but in terms of limits and pressures.[8]

One might add that there is no incompatibility between free will and determinism; to the contrary, unless the consequences of an action are in some fashion determined, choice is utterly meaningless. And to say that the flow of events is the result of continuous interplay between choice and circumstance, or freedom and necessity, producing willed action whose sum adds up to something no one willed, is to state the fundamental premise of historical materialism: "Men make their own history, but they do not make it just as they please; they do not make it under circumstances chosen by themselves, but under circumstances directly encountered, given, and transmitted from the past."[9]

Now, for what it is worth, I learned this principle from the Mississippi Civil Rights movement years before coming upon the *Eighteenth Brumaire of Louis Bonaparte,* and I mention this to emphasize that the dividing line here is not between formal analysis and Marxism, but between formal analysis and any view of history where people freely will their own determined fate. Indeed, it is not too much to say that the presence of will (conscious and unconscious motive, moral choice, reason) in the context of given environments is what sets human history off from natural history, and the society of men and women apart from the society of bees. But instead of confronting this problem head on, formal analysis slips into a behavioral "model" and pretends to explain the interaction of will and circumstance in terms of "variables." But how can a set of variables labelled "class" and "ethnicity" grasp the predicament facing a turn-of-the-century U.S. labor organizer to whom ethnic loyalties were both the sinew and the solvent of class solidarity? How can any multiple regression assign historically valid mathematical weights to the mixture of motives behind the American Federation of Labor's grudging support for women's pro-

tective legislation? What percentage was solidarity across sex lines, and what percent was the opposite—a clever stratagem to expel women from the labor market? And how can models of "regularities" and "patterns" cope with the inevitable ironies and contradictions of real history, with the fact, for example, that the great gains made by the labor movement in the New Deal soon became great chains of state-regulated labor bureaucracy?

The only one of Charles Tilly's pessimistic forecasts about the future of formal analysis in labor history with which I disagree is his hunch that a transformation in the "core" problems would lay the field open to far more formal analysis. To the contrary, in the aspects of labor history now being transformed, the driving forces have rarely been formalist. For example, theories of patriarchy have been far more influential than quantitative techniques in opening the discussion of how the restructuring of family life and the reorganization of production affected gender and class relations in the twentieth century.[10] Conversely, the influence of formal models of the labor market (proletarianization—homogenization—segmentation) have fallen entirely within the orbit of Tilly's "core" of production/class/collective action.[11]

If there is to be "some great success," perhaps it will come in studying not the society of human beings, but the society of bees. Not only does bee society exhibit many instructive analogies to human society, but the fascinating regularities of life in and about the hive also lend themselves perfectly to mathematical modeling. There is a simple organization of production and simple reproduction based on a functional division of labor between the queen (the mother of them all), the workers (industrious but undeveloped females), and the drones (fertile but otherwise superfluous males). Taking this division of labor as the dependent variable, one might ask which of the independent variables—sex or class—exhibits the greater explanatory power. The workers, as an exploited class, produce the honey necessary to the existence of the hive, while the leisured queen and idle drones subsist off their labor. But let them have a little royal jelly in the larva stage, and they all become queens: sex over class. And what then? Merciless civil war, as the queens sting each other to death until but one survives to reign over a lifeless realm, laid waste by starvation when all the workers became queens and ceased their productive functions. Here is the nightmare of conservatives from Burke to sociobiology: Workers' revolution means the destruction of society. Indeed, so it goes in functionalist sociology.

My point is not that all formal analysis is either politically conservative or an appendage to functionalism. Nor is it that quantification and model building are irrelevant to the study of human society, or to labor history. Rather I think it is important to recognize the limits inherent in these techniques. Cliometrics turns out to be not some radical new epistemology, but the old empiricism. Formal analysis turns out to be not a new way of solving the problem of causation, but the old positivism. It was always false hope to expect more. Hamlet has made a welcome arrival on the scene.

NOTES

1. Robert Swierenga, "Historians & Computers: Has the Love Affair Gone Sour?" *OAH Newsletter* (November 1984); Lawrence Stone, "The Revival of Narrative: Reflections on a New Old History," *Past and Present* 85 (November 1979): 3-24; R. W. Fogel and G. R. Elton, *Which Road to the Past? Two Views of History* (New Haven, 1983); J. Morgan Kousser, "Quantitative Social Scientific History," in *The Past Before Us*, ed. Michael Kammen, (Ithaca, 1980), 456.

2. Peter Shergold, *Working-Class Life: The "American Standard" in Comparative Perspective, 1899-1913* (Pittsburgh, 1982); P. K. Edwards, "The Exceptionalism of the American Labour Movement: The Neglected Role of Workplace Struggle." (1983).

3. Thomas Dublin, *Women at Work* (New York, 1979); Charles Tilly, Louise Tilly, and Richard Tilly, *The Rebellious Century 1830-1930* (Cambridge, Mass., 1975).

4. E. H. Carr, *What Is History?* (New York, 1961), 34-35.

5. Olivier Zunz, *The Changing Face of Inequality* (Chicago, 1982), 5.

6. Besides Tilly's brief definition in the paper under discussion, see David Herlihy, "Numerical and Formal Analysis in European History," and Allan Bogue, "Numerical and Formal Analysis in United States History," *Journal of Interdisciplinary History* 12 (Summer 1981). For a balanced assessment of the strengths and limits of formal analysis, see Peter D. McClelland, *Causal Explanation and Model Building in History, Economics, and the New Economic History* (Ithaca, 1975). McClelland gives formal analysis the inflection of behaviorist psychology, suggesting that causal explanation comes down to a stimulus/response pattern: "similar stimuli, experienced by people with similar dispositions, will result in similar actions" (86), or at least they *probably* will. For a quasi-Hegelian discussion of causation, see Jon Elster, *Logic and Society: Contradictions and Possible Worlds* (Chichester, 1978); for a pragmatist view, see Morton White, *Foundations of Historical Knowledge* (New York, 1965).

7. See Cushing Strout, *The Pragmatic Revolt in American History* (Ithaca, 1958); Morton White, *Social Thought in America: The Revolt Against Formalism* (Boston, 1947); Gareth Stedman Jones, "From Historical Sociology to Theoretical History," *British Journal of Sociology* 27 (September 1976): 295-305.

8. E. P. Thompson, "The Poverty of Theory," and "An Open Letter to Leszek Kolakowski," in *The Poverty of Theory and Other Essays* (New York, 1978), 84-86, 330-31; Raymond Williams, *Marxism and Literature* (Oxford, 1977).

9. Karl Marx, *The 18th Brumaire of Louis Bonaparte* (New York, 1963), 15.

10. See the papers prepared for the Northern Illinois Conference on Labor History by Mari Jo Buhle, "Gender and Labor History," and Alan Dawley, "Labor, Capital, and the State in the Twentieth Century" (Dekalb: Northern Illinois University Press, in press).

11. David Gordon et al., *Segmented Work, Divided Workers* (Cambridge, England: 1982).

Response to Charles Tilly's "Neat Analyses of Untidy Processes"

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In October, 1984, the National Indowment for the Humanities sponsored a conference at Northern Illinois University intended to further the search for a "synthesis" in American labor history. Confronting the explosive growth in the field over the past decade, conference papers addressed diverse topics such as race, gender, and class, and attempted to reach some understanding of how these factors were related. Many participants clearly had raised expectations on the first day that a better understanding would result between the core of American labor history and its recently developed periphery.

After two full days of presentations, questions, and debate, in the opinion of many in attendance, a generalization on the current state of American labor history could be made. The course of the discussion and comment was clearly dominated by a majority group of scholars who focused analysis on the relationship between the organization of production, the formation of social classes, and workers' collective action. Workers were generally presumed to be inherently militant in a political sense; an absence of militancy was generally the result of a lack of effective political power. Indeed, the struggle for political and economic power, for most in attendance, generally explained the nature of workers' movements and objectives. To be sure, female historians argued that gender often explained workers' orientation; some even linked consciousness with one's standing in a racial or ethnic community. But defenders of gender, race, and ethnicity were fewer in number and much less frequently heard from during the conference proceedings.

Ultimately the conference reaffirmed a recent trend among American labor historians. The traditional preoccupation with the relationship between the organization of production, the formation of social class, and collective action and the struggle for political power has survived the "formal methods of analysis" and social history orientation of the 1970s. During the present decade it has reasserted itself with new vigor and preempted other perspectives, which threatened to mute considerations of class formation and conflict. Possibly this resurgence has become even more compelling since the election of Ronald Reagan in 1980 and the decline in influence of organized labor in national political affairs.

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To suggest that the core of American labor history is intact and reinvigorated is not to suggest that it has remained completely indifferent to the perspectives generated by the "new" social history of the past decade. Workers are considered more broadly than ever before, as the structure of the Northern Illinois conference suggested. The point made at the conference and in most recent scholarship, however, is that after any discussion of the broader dimensions of working-class life (such as gender, family, and culture) scholars must always return to a consideration of the workplace and the class struggle. The core can be slightly expanded, but it can never be changed. In other words, culture or gender are usually acceptable to the "core group" if they are considered simply as a larger part of the class struggle; history on the periphery remains unacceptable to the vast majority of American labor historians if it does not ultimately emanate from core concerns of conflict and power. The core extended is evident in such excellent recent works as Roy Rosenzweig's study of leisure among workers, Thomas Dublin's account of women workers, and Nick Salvatore's treatment of the public and private side of Eugene Debs.[1] The core remains nicely intact, however, in the recent studies by Alan Dawley, Nelson Lichtenstein, David Montgomery, and David Brody, which focus on the struggle for power and class conflict to the exclusion of other considerations.[2]

Much American social history since 1970 spoke to issues concerning workers and the labor movement and relied on formal methods of analysis. In its stress on private lives, collective biography, mobility, the uses of urban space, and the family, this body of scholarship generally avoided political issues and conflict. Core historians criticized it for neglecting the struggle for power and control in society and in the workplace. After comparing much of the "old" and the "new" labor history, David Brody has written that workers must ultimately be seen in the context of job and industry.[3] Many of the reviews of the "new" labor history assert this point repeatedly. Fears have been expressed that emphasis on individuals and families has virtually crowded collective action off the historical stage.[4] The most common charge has been that works connected to the periphery have essentially resulted in a history with the politics left out. Seldom, however, has the reverse been true. Core labor historians have been remiss in calling attention to the fact that "political" labor history has failed to probe private lives and cultures and assess the manner in which they may have influenced politics and collective action. Generally any discussion of labor history centering on class structure, the workplace, militancy, or political conflict is considered inherently truthful and acceptable. Studies that link workers' action and thought to the "material reality" of the workplace and, by implication, to the means of production are applauded almost instinctively.

Even though the core continues to dominate American labor historiography, signs of a mergence with the periphery are evident. The recent work by

David M. Gordon, Richard Edwards, and Michael Reich advanced the thesis that the evolution of modes of production could not be fully understood without an assessment of the "social structure of accumulation," the total environment of institutions, legal systems and (by implication only) cultural systems that shape the particular direction of capitalist investment in production.[5] Although such a view does not exactly call for a linking of the social history of workers with the traditional core, it does acknowledge the need for a larger perspective than the one that currently dominates the field.

The slowness of the core to expand its view of history is somewhat surprising, because interesting advances in critical theory have taken place which threaten to modify much of the Marxist foundations of the core. Consider the concept of ideology. Both European and American writers have argued that various forms of ideology, usually attributed to the ruling class, have captured the allegiance of workers and dissuaded them from their historical mission to initiate class conflict. Nationalism, consumerism, or even the family have been used to explain how "class consciousness slips into false consciousness." This view has been taken further and greatly altered by the work of Michel Foucault. Arguing that ideas cannot be reduced to modes of production, Foucault has shifted the focus of attention away from the ideas of the intellectuals and elites (who own the means of production) toward the variety of societal institutions that more directly affect the everyday life of working people. Ideology emerges not from those who are most powerful, intellectually or politically, but from the "encounter" of institutions and individuals: criminal and criminologist, child and parent, unemployed worker and welfare agency. As Mark Poster has incisively written, Foucault opposes the central doctrine of historical materialism upon which the concept of ideology rests: the distinction between the base and superstructure.[6] Whereas the views of Foucault certainly do not provide a theoretical explanation of all of the "new" social history of workers (since institutions do to some extent replicate social distinctions created by unequal ownership of the means of production), they do provide an impetus to much of the scholarship that has characterized the periphery and that has only mildly affected the core. Worker thought and behavior must be viewed in a more complex totality. Society itself is not simply stratified from top to bottom but punctuated with countless "encounters" between institutions and individuals. The implication of these "encounters" has never been fully studied by historians of either the core or the periphery; it is by no means always clear just who is influencing and controlling whom.

Finally, a body of social history, folklore, and anthropology, not often read by core historians of American labor, has produced a profile of ordinary people who manage to fashion a degree of control in their lives in spite of economic structures and dominant institutions. Studies of peasants in Europe and immigrants in urban America have frequently uncovered a process by which ordinary people created behavioral and thought patterns from both the macrocosm of social structure and the microcosm of locale, memory, and group. These cultures helped ordinary individuals to understand and control their lives on one level even if they could not alter the larger structure of industrial capitalism. This everyday world was simultaneously tied to the means of production and independent of it, although the extent to which it was tied to production has received considerably more attention than its link to culture. Stubbornly the core continues to dominate the periphery.[7]

Charles Tilly has performed a useful service. His insightful depiction of a core and a periphery in labor history is highly accurate, as is his assertion that by common understanding scholars of labor history's core focus on matters that yield only with great difficulty to formal (or any other form) of analysis. He is probably correct in suggesting that formal analysis will not substantially change the core. This has certainly been the case thus far. Formal analysis, as Tilly defines it, has lost something of its punch and appeal in the United States. But this fall from favor has not resulted merely from a new interest in consciousness, culture, and narrative. Rather, the traditional core of American labor history has "resisted" the scholarly fashions of one decade and reasserted itself in another.

NOTES

1. Roy Rosenzweig, *Eight Hours for What We Will: Workers and Leisure in an Industrial City, 1870–1920* (New York, 1983); Thomas Dublin, *Women at Work: The Transformation of Work and Community in Lowell, Massachusetts, 1826–1860* (New York, 1979); Nick Salvatore, *Eugene V. Debs: Citizen and Socialist* (Urbana, Ill., 1982).

2. Alan Dawley, *Class and Community: The Industrial Revolution in Lynn* (Cambridge, Mass., 1976); Nelson Lichtenstein, *Labor's War at Home: The CIO in World War II* (New York, 1982); David Montgomery, *Workers' Control in America* (New York, 1979); David Brody, *Workers in Industrial America* (New York, 1980). See also Ronald W. Schatz, *The Electrical Workers* (Urbana, Ill., 1983); Daniel J. Walkowitz, *Worker City, Company Town* (Urbana, Ill., 1978).

3. David Brody, "The Old Labor History and the New: In Search of an American Working Class," *Labor History* 20 (Winter 1979): 111–26.

4. See John J. Bukowczyk, "Immigrants and Their Communities: A Review Essay," *ILWCH* 25 (Spring 1984): 47–57.

5. David M. Gordon, Richard Edwards, and Michael Reich, *Segmented Work, Divided Workers: The Historical Transformation of Labor in the United States* (New York, 1982).

6. Mark Poster, *Foucault, Marxism, and History: Mode of Production versus Mode of Information* (New York, 1984).

7. See Pierre Bordieu, "Marriage Strategies as Strategies of Social Reproduction," in *Family and Society: Selections from Annales*, ed. R. Forster and O. Ranum, (Baltimore, 1975); John Bodnar, *The Transplanted: A History of Immigrants in Urban America* (Bloomington, Ind., 1985); Henry Glassie, *Passing Time in Ballymenone: Culture and History of an Ulster Community* (Philadelphia, 1982); Tamara K. Hareven, *Family Time and Industrial Time* (New York, 1982).

Response to Charles Tilly

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There is so much agreement between my views on this subject and those expressed by Charles Tilly that what I will say can hardly be construed as constituting a controversy. Let me agree at the outset: Formal analysis and the quantitative methods it makes possible have become standard, indispensable adjuncts to research in labor history, as in social history as a whole. As Tilly puts it, "the periphery constrains the core." The central, most difficult questions in social history (most of which attempt to link political behavior with social background and identity) cannot be answered by quantitative methods alone, because they require one to make inferences about motives and perceptions that are not amenable to formal analysis. That historical actors themselves cannot observe each others' perceptions or motives is responsible for a great deal of the violence and injustice that history has recorded. How actors manage to overcome this difficulty and to act in unison, confident of each others' mutual accord, cannot be explained or even described by means of formal models. An almost preverbal negotiation constantly going forward in human social life conditions everything else.

Certain kinds of actions and conditions can be counted but only because societies themselves standardize and classify these actions and conditions. That is, formal models themselves often play a role in the solutions that historical actors seize upon to coordinate life in communities. A wedding ceremony, a voting procedure, or a picket line are enactments of highly structured, formalized scenarios which, so to speak, prescribe motives for the participants. Bride and groom by conventional expectation wish to cohabit, voters to choose representatives, pickets to restrict access to the workplace. The formal scenario for the action is often arranged to allow for the policing of the actors' motives to make sure that they indeed conform to the prescribed ones. In other words, uncertainty about motives is allowed for in the system. Bride and groom are separately asked if they wish to marry, and witnesses attest to their responses in writing. Voters are put in booths to ensure that they consult only their own preference in voting. Forces of order nervously observe pickets to ensure they do no more than picket, just as pickets observe the entrance to the workplace to ensure that all their fellow workers really continue to want to strike. Formalized public action is one of the principal means by which human

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beings bridge the gap, as it were, between their diverse, inscrutable, private, complex desires and perceptions.

Formal procedures generate formal documents; thus it is sometimes possible to count the occurrence of such procedures in the past. But hard experience has shown that counting marriages, election returns, strikes, charivaris, riots, attendance at church, funeral processions, and so on, is quite different from counting births, deaths, or even geographic mobility. Where a human body is located at any given time and whether it is alive or dead, fertile or infertile, sick, malnourished, or well—these are all matters that do not involve the interpretation of motives. Counting such things involves at least no more dangers than counting atoms in physics or genes in biology. However, even to count marriages, a common preoccupation of historical demographers, is already trespassing on the realm of consciousness and desire. Evidence of this, if any is necessary, can easily be seen in the continuing controversy over the meaning of illegitimacy rates. What it means for a woman to make the complex choice to have a child out of wedlock—however easy it may be to count—is not readily apparent. Merely to use the word *choice* in the previous sentence raises thorny difficulties, for women in many circumstances are constrained to act in a certain way by a combination of social expectations and male betrayals that are as difficult for the historian to observe or to count as they are for the victim to overcome.[1] And all of this has very much to do with what marriage means in a given society. At the same time, until one has counted the marriages and the illegitimate births one does not even know what questions to ask. It is in this sense that quantifiable evidence sets limits on historical inquiry.

In labor history "the periphery constrains the core" to such an extent that the second of Tilly's twin stars—concerning the connections between work, class formation, and collective action—may be said to have been doubled in mass in recent decades, until it virtually dominates the system, largely as a result of research carried out with quantitative methods. The centrality of the artisan to the nineteenth-century experience, a thesis first proposed by E. P. Thompson, has been reconfirmed in scores of studies by now; Tilly's own work and the example of his method have played a key role in this complete reorientation of research on the nineteenth century. Most recently, as he points out, the Russian Revolution has come in for a parallel reconsideration of profound importance, again, as a result of efforts simply to count who was in the workforce, and who was involved in what organizations and collective actions when and how often. That such work will continue to be essential to the labor historian as a prerequisite to framing the right questions is beyond doubt.

The danger of counting "skilled workers," "strikes," union "members," socialist votes, or bars per capita in working-class neighborhoods lies in the fact that each of these things is, like illegitimacy but unlike birth or death, a

social construct. When one sees the massive tables full of precise numbers, the product of so much painstaking effort, one is tempted to forget that each of the things being counted was what it was only because of the painstaking efforts of historical actors to find formal procedures that could shape their interactions. Labor historians have become increasingly aware in recent years that "skill," for example, is a highly variable, in some cases almost an elusive, quality which has as much to do with external perception, internal organization, and political clout as it does with the actual requirements of certain tasks.[2] Likewise, strikes are an essential feature of life in industrial society simply because workers have to stop work in order to do anything else, but immense variation in the forms and significance of collective action is possible within this one limiting constraint.[3] Union membership rates often tell little about the actual political significance of union organizations, especially in times of crisis. Bars may at first appear to be satisfyingly concrete and uniform. But Maurice Agulhon has shown that what is or is not a retail establishment purveying alcohol to the public may become an issue in radicalizing contention between the state and the laboring poor.[4]

Counting the formalized actions or formalized conditions ("married," "illegitimate," "skilled") of historical actors necessarily begs the question of how such standardized forms of actions or conditions came into wide acceptance in the first place or changed over time. This is not an insurmountable objection to such counting; it is merely a necessary limit to the utility of the result. Counting marriages cannot reveal anything directly about the history of the institution of marriage. It may tell one where to look for changes in the institution (as when average age at marriage suddenly drops or extramarital cohabitation rises). But it does not divulge what those changes are. A shift in voting patterns from right to left, a sudden increase in bequests to religious institutions, a secular rise in strike rates—these mean very little by themselves. A quantifiable trend in and of itself can have any number of explanations. But it can also be an indispensable sign post that forces the historian's attention down a new path. It can show that fundamental change is under way without showing what change is under way. This is especially true when the action or condition being counted is subtly changing in form or in its relation to other actions or conditions.

The strengths and weaknesses of formal analysis can be assessed through brief reflection on a concept like union membership. Can journeymen's *compagnonnages* in France before 1789 be considered to be unions? If so, were such unions sufficiently similar to unions in England in 1830 or unions in Russia in 1905 to make it sensible to count union members in these three societies and compare levels of unionization?[5] Answering such questions is a matter of definition, and how one defines a union will imply a great deal about one's whole view of modern history. Hence one almost has to have such a view worked out before one starts counting. At the same time, it is quite possible to

do the counting and to compare the rates of unionization to see what they show without committing oneself in advance to the significance of the result. If the documents allow it, this is almost bound to be useful and thought-provoking. Some would argue, however, that the differences between a union in France in 1780 and a union in Russia in 1905 could be handled through formal analysis of the context, for example, by weighting membership numbers according to whether unions were legal or illegal, existed in the presence of guilds or in the presence of Soviets, were led by workers themselves or by intellectuals with advanced ideologies, and so on. The resultant "index of militancy," as we might call it, might then correlate significantly with other formalized measures derived from these societies, allowing one to test hypotheses about, for example, the relation between modernization and political mobilization.

The problem is that establishing such quantifiable weightings involves the use of strong presuppositions about the nature of society which cannot themselves be tested individually once the index is arrived at. It does not matter whether the index of militancy of Paris journeymen correlates in an expected manner with an index, say, for industrialization of the Paris economy in 1780 or not. In either case, one has no way of knowing if one was right to weight the index in a certain way to reflect the absence of intellectuals with advanced ideologies or the presence of guilds. Is the absence of intellectual leadership a positive or a negative factor, a major or a minor one? These are not questions to be answered at the outset of a study, at least not unless one wants to ignore, for heuristic purposes, a whole range of fundamental issues.

It is my guess that the unavoidable nature of such imponderable difficulties with formal analysis has discouraged historians from seeking out increasingly sophisticated methods of quantitative analysis. A brief review of Tilly's highly useful list of references suggests that most historians continue to use quantitative methods of a very simple kind. Things listed in documents which can be counted are counted; tables of the resulting numbers are drawn up; obviously related variables are compared; and the more sophisticated level of analysis occurs in prose discussions of the numbers. This is certainly true, for example, of Victoria Bonnell's ambitious and exciting study of Moscow and St. Petersburg workers. David Crew's excellent quantitative survey of Bochum is full of tables with titles such as "Assessed Master Artisans as Percentage of All Artisans" or "Occupation and Regional Origin in 1907." Similarly simple measures predominate in Michael Hanagan's *Logic of Solidarity,* Daniel Roche's *Peuple de Paris,* Ranier Wirtz's *"Widersetzlichkeiten,"* or Jean-Paul Brunet's *Saint Denis, la ville rouge.* Even Yves Lequin's impressive quantitative apparatus in *Les ouvriers de la région lyonnaise* seldom goes beyond simple correlations between two variables; the sophistication lies mostly in the means chosen to display the results graphically.[6]

Here lies the real lesson of recent experience with quantitative methods. They are not likely to be abandoned, the much-heralded "return to narrative"

notwithstanding. But they are likely to remain, just as Tilly says, an indispensable set of preliminary procedures which do not answer the core questions so much as aid in their accurate formulation. This is something that was not at all clear as recently as 10 years ago. That the extraordinary value as well as the precise limits of quantitative methods are beginning to become clear is due in great measure to the pioneering work of a number of scholars. The boldness and the flexibility of Tilly's own work in this area have constituted no small contribution to what appears to be an emerging consensus on this matter.

NOTES

1. Compare the highly divergent treatments of this subject in the following studies: Edward Shorter, *The Making of the Modern Family* (New York, 1975); Louise A. Tilly and Joan W. Scott, *Women, Work, and Family* (New York, 1978); David Sabean, "Unehelichkeit: Ein Aspekt sozialer Reproduktion kleinbäuerlicher Produzenten: zu einer Analyse dörflicher Quellen um 1800," in *Klassen und Kultur: Sozialanthropologische Perspektiven in der Geschichtsschreibung*, ed. Robert M. Berdahl et al. (Frankfurt, 1982), 54–76.

2. See, for example, Charles More, *Skill and the English Working Class* (New York, 1980); C. K. Harley, "Skilled Labor and the Choice of Technique in Edwardian Industry," *Exploration in Economic History* Series 2, vol. 11 (1974), 391–414.

3. I have argued this more fully in *The Rise of Market Culture: The Textile Trade and French Society* (New York, 1984).

4. Maurice Agulhon, *La république au village* (The Republic in the Village), trans. Janet Lloyd. (Paris, 1970; New York, 1982.)

5. These reflections are based on William H. Sewell, Jr., *Work and Revolution: The Language of Labor from the Old Regime to 1848* (New York, 1980); Victoria Bonnell, *Roots of Rebellion: Workers' Politics and Organizations in St. Petersburg and Moscow, 1900–1914* (Berkeley, 1983); and also on Cynthia M. Truant, "Solidarity and Symbolism among Journeymen Artisans: The Case of Compagnonnage," *Comparative Studies in Society and History* 21 (1979): 214–26; Michael Sonenscher, "Work and Wages in Paris in the Eighteenth Century," in *Manufacture in Town and Country before the Factory*, ed. Maxine Berg, Pat Hudson, and Michael Sonenscher (Cambridge, England, 1983), 147–72; Gareth Stedman Jones, "Rethinking Chartism," in *Languages of Class* (Cambridge, England, 1983), 90–178.

6. See Victoria Bonnell, *Roots of Rebellion: Workers' Politics and organizations in St. Petersburg and Moscow, 1900–1914* (Berkeley, 1983); Michael Hanagan, *The Logic of Solidarity: Artisans and Industrial Workers in Three French Towns, 1871–1914* (Urbana, Ill., 1980); Yves Lequin, *Les ouvriers de la region lyonnaise (1848–1914)* (Lyon, 1977), 2 vols.; Rainer Wirtz, "Widersetzlischkeiten, Excese, Crawalle, Tumulte und Skandale," *Sociale Bewegung und sozialer Protest in Baden, 1815–1848* (Frankfurt, 1981). See also David F. Crew, *Town in the Ruhr: A Social History of Bochum, 1860–1914* (New York, 1979); Daniel Roche, *Peuple de Paris: Essai sur la culture populaire au XVIII^e siècle* (Paris, 1981); Jean-Paul Brunet, *Saint-Denis, la ville rouge, 1890–1939* (Paris, 1980).

Response to Sean Wilentz's "Against Exceptionalism: Class Consciousness and the American Labor Movement: 1790–1920"

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I agree with Sean Wilentz that theories of American exceptionalism which rest on essentialist assumptions are ahistorical and objectionable on that account. But historical analysis of contexts and periods may well yield the conclusion that the United States, in a particular period, was so different from Europe as to be an exception to the general pattern of capitalist nation-states in that period. Perhaps the word "exception" should not be used because of its associations with ahistorical theories. But I think it can be used to describe the United States from the 1920s through the post-World War II boom. As we enter a new era of world capitalism in our times, perhaps the differences between the United States and other countries will narrow once again. This is possible, and if it happens, American exceptionalism will be no more. During the golden age of American consumer capitalism and the American dominance of the world, however, I don't see how Wilentz can insist that the United States and its labor movement were merely different. Surely, the cumulative impact of the events of the period from 1890 to 1920 (the era of organized capitalism, the second industrial revolution, mass communications and mass politics, imperialism, world war and revolution) resulted in a profound divergence between the United States and Europe. Wilentz does not acknowledge how crucial this period was for the rest of the twentieth century. This is why he does not see that it was the era of the rise of mass socialist parties almost everywhere in Europe and not just in Germany. Although I do think the United States became an exception by the 1920s, I do not think this was an inevitable development. Not only do I agree with Wilentz about the nineteenth century, but I also agree that the American labor movement of the early twentieth century was more anti-capitalist than the traditional wisdom has it. A British-style laborism was in the making, and why it did not crystallize in more places than it did is an open question. My thesis on Chicago addresses this question and it will show that there was more class consciousness at the grass-roots of the AFL than even Wilentz suggests. Nevertheless, by the time labor turbulence subsided after World War I, the outcome was clear. Just as Wilentz's framework glosses over

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