But organizational efforts to overcome quantitative parochialism can play only an auxiliary role. To derive greater benefits from the national varieties of quantitative history, individual scholars must become more willing to run the risks of international dialogue. Impressive beginnings have been made. We only have to go on.

Formalization and Quantification in Historical Analysis *)

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A Wave or Formalization

In historical analysis, the first great wave of formalization started in the 1950s and began to lose its force in the 1970s. Now it has spent itself. When and how will the second wave arrive, if it ever does? Let us address the question in characteristic historical fashion: by examining the first wave carefully, to see if it displays regularities that help specify the conditions under which something similar might occur again.

Formalization? I mean a variety of procedures that match descriptions of events, structures, and process with explicit models of those events, structures, and processes. Formal methods do not necessarily involve quantification or computing; analyses of linguistic, spatial, or temporal structure, for example, often proceed quite formally without computers and without any direct intervention of mathematics. In history, however, the formalization that concerned history's technical innovators in the 1960s and 1970s typically included quantification and/or computing.

Among historians as a group formalization gained a number of energetic advocates during 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; they saw the possibility of letting inarticulate people speak for themselves through 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, points in time, or something else--into portraits of the entire sets, and into means for studying variation among the individual units.

Full-fledged formalization in history involves four activities: conceptualization, measurement, modeling, and estimation. Conceptualization concerns the statement of an historical question as a problem susceptible of formal treatment--for example, conceiving of a plantation as a kind of firm (and thus suitable for analysis in terms of the economics of the firm) or of a community as a closed population (and thus available to the demographic analysis of fertility change in closed populations). Measurement refers to organizing the evidence in standard, comparable form, for example by assembling similar records

*) This paper is a substantially revised version of "Neat Analyses of Untidy Processes." Working Paper No. 5, Center for Studies of Social Changes, New School for Social Research, which appeared in International Labor and Working Class History 27 (1985): 4 - 34. The earlier version has a larger bibliography and a more extended discussion of labor history, but says less about other historical fields.

of income and expenditure for all households in a village. Modeling involves the formal statement of an argument concerning the expected pattern of a phenomenon, for example the explicit retrodiction that in a given German town more of the *Mittelstand* than of other classes will turn out to have supported Hitler. Estimation, finally, means matching model to evidence in order to see how well the model fits, for example by means of a statistical procedure, the correlation coefficient, that determines how close to linear is the relationship between wage levels and class voting.

All formalization requires some version of conceptualization, measurement, modeling, and estimation, but analysts do not necessarily give them equal attention. Formalizing historians have, in fact, devoted little of their ingenuity to conceptualization, modeling, and estimation. Often they have unwittingly accepted the concepts, models, and estimation procedures that are implicit in a particular quantitative routine, for example by running a straightforward ordinary least squares multiple regression of electoral results on social characteristics of the populations of electoral districts--an act assuming implicitly that the electoral districts are coherent, independent units, that the social characteristics of those units somehow cause the votes of their electorates, that strong causality would show up as a linear increase or decrease of one sort of vote as a function of increase or decrease of a particular social characteristic, and so on. Often historians have truncated their formalizations: taken considerable care with measurement, only to interpret the measurements informally, for instance by constructing a time series of strike activity and then inserting it into a non-quantitative discussion of rising or falling class consciousness. Historians have, on the other hand, made great contributions to measurement; they have, for example, devised ways of reworking religious records into solid indicators of fertility, mortality, and nuptiality; research done on the resulting historical evidence has altered our ideas of the conditions for large-scale population change.

Formalization had important successes in historical research. Without formal analysis based on 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 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 irruption 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–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 leds 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 1).

Lorwin and Price's statement, although restrained and sensible in its context, rings quaintly today. "Existing frontier of research endeavor" ? In economic, demographic, and electoral history, quantification has ceased being an adventure in itself; historians in those specialties quantify as a matter of course. Almost everywhere else, however, 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 true frontier. As Erik 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 born the brunt of this critique, though it includes many non-quantitative forms of history as well 2).

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

1) Val R. Lorwin and Jacob M. Price, eds., The Dimensions of the Past: Materials, Problems, Opportunities for Quantitative Work in History (New Haven, 1972) 10.

2) Eric Monkkonen, "The Challenge of Quantitative History," Historical Methods 17 (1984): 86 - 94.

Wrigley and Schofield's *Population History of England* and the Stones' *An Open Elite?* 3). But since 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 4). In contrast, Continental Europe looks different. There, formal analysis are still proliferating: studies of Nazi membership, enumerations of Swiss *Aktivierungsereignisse*, content analyses of Medieval texts, and much more. 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 provincial academies, for instance, deals mainly with the organization and culture of those quintessential Enlightenment institutions; yet Roche does not hesitate to map, graph, or quantify the provincial savants' activity: 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 5). 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 in question. Quantification becomes more useful as a function of a) the compexity of the explanatory model, b) the intrinsic quantifiability of the phenomenon to be explained, c) the importance of variation to the argument, and d) the 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 subjects in a comparative frame, and yet to employ complex arguments, inclines them toward quantification.

Clearly, the post-1950 wave of formalization did not strike all parts of the historical shore with equal force. At one extreme, such specialties as economic and demographic history made formal methods their standard procedures. At the other, fields such as intellectual history, diplomatic history, and the history of science remained almost untouched by formalization. In between, political history, urban history, social history, labor history, and related subdisciplines

3) E. A. Wrigley and R. S. Schofield, The Population History of England, 1541 - 1871: A Reconstruction (London, 1981); Lawrence Stone and Jeanne Fawtier Stone, An open Elite? England 1540 - 1800 (Oxford, 1984).

4) Lawrence Stone, "The Revival of Narrative: Reflections on a New Old History," Past and Present 85 (1979): 3 - 24.

5) Daniel Roche, Le siècle des Lumières en province: Académies et académiciens provinciaux, 1680 - 1789 (Paris, 1978), 2 vols.

divided by specific subject; the study of social mobility, industrial conflict, urban segregation patterns, elections, and household structure became quite formal, for instance, while students of power structure, war, revolution, gender, urban planning, and social movements rarely ventured into formal analysis of their evidence. Within these intermediate fields, methodological struggles, line-drawing, mutual suspicion, and name-calling multiplied.

Disciplinary Agendas

Although these struggles entailed plenty of misunderstanding, they did not result from simple ignorance. Disciplinary agendas were at stake. In any discipline, members organize themselves in two fundamental ways: a) by creating a bounded interpersonal network, often one that is formalized via organizations, meetings, journals, and similar devices; b) by establishing a shared agenda which 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 which 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.

Labor history provides a case in point. Labor history is a bipolar field. 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 perhaps a dozen other questions inform the bulk 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 and/or effective national labor movements? This question, unanswerable as stated, breaks into a small series of less general inquiries. Within labor-history-defined-as-national-movements, one of the few venerable payoff questions is "Why so much more socialism in some countries and periods than others?" Broadly speaking, the main alternative answers to that old query 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 strategies of states and national elites--for example, cooptation and corporatism--strongly affect 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 experiences and leaders, such as responses to the Depression of the 1930s, shape the political choices and possibilities available within any particular state.

Put so generally, to be sure, these answers could all be correct 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 arise. 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 point in time, only a handful of such questions define the overall agenda of the entire field.

Labor history 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 a) renews understanding of the conditions underlying national fluctuations in the militancy and/or effectiveness of worker action, b) helps connect the organization of production, the formation of social classes, and worker collective action, or c) both. By and large, the successes of formal analysis have occurred in labor history's periphery. They include:

a) time-series analyses of the determinants of fluctuations in national levels of strike activity,

b) treatments of the organizational bases of workers' collective action,

c) studies of the demographic correlates of different sorts of industrial organization

d) reconstructions of labor migration and its consequences,

e) quantitative portrayals of occupational mobility and of social ties among different occupations, and

f) research on the urban geography of migration, work, and workers.

These sorts of studies have great merits. (At least I hope so, since my own efforts in labor history lie almost entirely in these areas). But they do not address the organizing questions of labor history directly.

The organizing questions, on the other hand, resist formalization. Remember the ideal conditions for useful quantification: 1) an explicit, complex model of the process or structure under analysis, 2) intrinsic quantifiability of the phenomen to be explained, 3) importance of variation to the central arguments, 4) large number of units. Although the major models of labor history are often complex, they are rarely explicit. Many of the major phenomena figuring in those models, such as class consciousness and revolutionary will, are not obviously quantifiable. Variation is a sometime visitor to the central arguments of labor history; although the differences between two countries are often at issue, even that minimum comparison serves mainly to identify the unique properties of each individual country. And the central arguments of labor history rarely deal explicitly with large numbers of units, except in the sense that they sum up the experience of all workers, all labor unions, and so on.

Where Formalization Works

Many other historical fields resemble labor history in these regards. Intellectual history, the history of science, diplomatic history, political history, the history of warfare, and most synthetic national histories (e. g. the histories of India or China) rarely employ explicit models, deal with intrinsically quantifiable phenomena, analyze variation systematically, or treat large numbers of units--at least not all at the same time. And these characteristics stem directly from a concentration on payoff questions that resist formalization.

Within labor history, consider the problem of national labor movements. Formal analyses of strike activity and quantitative treatments of the organizational bases of workers' collective action begin to address that issue. 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.

When push comes to shove, labor historians who are concerned with national labor movements seem to want one or both of two things: a) persuasive reconstitutions of the shared states of mind of the principal actors at different points in time, b) 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 provide no effective means for getting at them directly. Dealing with strikes in nineteenth-century Massachusetts, for instance, Carol Conell is able to build mathematical models whose empirical application strongly suggests an important conclusion: skilled workers timed and located both their organization and their strike activity to maximize the impact of withholding their labor, and the advantage of organization and timing to them was significantly greater than it was for less skilled workers. But Conell's results cannot tell us whether skilled workers made self-conscious calculations to that effect [6].

On the side of strategy and tactics, 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 these 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 in my earlier list 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 "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 only with great difficulty to formal analysis. 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. If the current drift toward retrospective ethnography, individual experience, and discourse continues, formalization will spread slowly, remain at its present low level, or even decline in significance.

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 explanations of fluctuations in national labor militancy and effectiveness. Studies by Victoria Bonnell, Diane Koenker, William Rosenberg, 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 movements as a consequence of the thrusting of uprooted peasants into big-city industrial life [7].

6) Carol C o n e l l, "The Impact of Union Sponsorship on Strikes in Nineteenth Century Massachusetts," unpublished doctoral dissertation, University of Michigan, 1980.

Again, research on the dynamics of rural industry by Franklin Mendels, David Levine, Yves Lequin, 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. These findings limit our possible accounts of the qualitative experience of industrialization. They thereby make more dubious the oncepopular explanations of working-class action that stressed the shock of abrupt-exposure to industrial conditions [8]. Over the last two decades, important findings on such matters have emerged from formal analysis, and would have been less likely to appear without formal analysis.

Conditions for Change

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 that lends itself directly to formal analyses. None of the three is likely.

It is possible, but improbable, that some great success will establish formal analysis at 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 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 [9]? In retrospect, one can see

7) Victoria B o n n e l l, Roots of Rebellion: Workers' Politics and Organization in St. Petersburg and Moscow, 1900 - 1914 (Berkeley, 1983); Diane K o e n k e r, Moscow Workers and the 1917 Revolution (Princeton, 1981); and William G. R o s e n b e r g, "Workers and Workers' Control in the Russian Revolution", History Workshop Journal, 5 (1978): 89 - 97.

8) Franklin M e n d e l s, "Seasons and Regions in Agriculture and Industry during the Process of Industrialization," in: Sidney P o l l a r d, ed., Region und Industrialisierung: Studien zur Rolle der Region in der Wirtschaftsgeschichte der letzten zwei Jahrhunderte (Göttingen, 1980); David L e v i n e, ed., Proletarization and Family Life (Orlando, Fl, 1984); and Yves L e q u i n, Les ouvriers de la région lyonnaise, 1848 - 1914 (Lyon, 1977), 2 vols.

9) Stephan T h e r n s t r o m, Poverty and Progress: Social Mobility in a Nineteenth

readily that the question has a quantitative, structural component that lends itself to formal treatment. In prospect,however, 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 through work on such matters as labor migration, gender, or industrial conflict the standard questions concerning national labor movements or the established triad of production, consciousness, and collective action 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.

Let me add a 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. To the extent that members of the discipline move toward questions involving explicit models, systematic variation, comparison of many cases, and intrinsically quantitative phenomena, conversely, they will become receptive to formalization.

The same reasoning applies, I believe, to the rest of history. In political history, diplomatic history, intellectual history, and a number of other fields, no large expansion of formalization will occur unless the dominant questions change. In any of the fields someone could devise a formal method that would recast a major question, currently peripheral questions that lend themselves to formalization could become more pressing, or an intellectual revolution that replaced

Century City (Cambridge, 1964); and "The New Urban History," in: Charles F. D e l z e l ed., The Future as History (Nashville, 1977).

the core questions could occur. As the use of computers for such routine tasks as the preparation and storage of texts increases, historians might find themselves drifting into the pursuit of questions that only computers make practicable. As the findings of those fields that have invested heavily in formalization, such as economic history, impinge on the questions people are asking in other fields (for example, by stretching out the "industrial revolution" over such a long period that it stops being a plausible explanation of abrupt changes in popular politics), historians in unformalized fields may find themselves compelled to formalize, if only to drive away the formalizers.

No doubt we can invent other scenarios that would produce a rapid, large increase in historical formalization. Nevertheless, the main points remain: in today's practice of history, with few exceptions, the dominant questions around which practitioners organize resist formal analysis; those questions guide a great deal of research and change rather slowly. Without a substantial alteration of those questions we have no reason to expect a rapid expansion of formalization.