
SOCIOLOGY, MEET HISTORY

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AS SOCIOLOGY MEETS HISTORY: PLAN OF THE BOOK

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Mercurial Views of the Seventeenth Century

Durant l'Este de ceste annee le Roy estant a Paris fut adverty par un nomme le capitaine Belin, qu'en Limosin, Perigord, Quercy, & en quelques provinces des environs, plusieurs Gentils-hommes faisoient des assemblees pour relever les fondemens de rebellion, que le feu Mareschal de Biron & ceux qui estoient de sa conspiration y avoient jettez; & ce fut le pretexte ordinaire des rebelles, scavoir, pour descharger le peuple, & pour faire que la Justice fust mieux administree a l'advenir par ceux qui l'exercent: & toutes fois leur dessein n'estoit q pour pescher en eau trouble, & sous l'apparence du bien public s'engraïsser des ruines du pauvre peuple.

During the summer of this year the King, who was in Paris, was warned by a certain captain Belin that in Limousin, Perigord, Quercy and other nearby provinces a number of Gentlemen were meeting to restore the bases of the rebellion that the late Marshal Biron and his co-conspirators had laid down. They had the rebels' usual pretext: to lighten the people's burden, and to make sure that those who were charged with the administration of justice would do better in the future. Nonetheless their real hope was to fish in troubled water and, in the guise of the public good, to fatten themselves up at the expense of the poor people.

The year is 1605; the King, Henry IV of France; the source, Le Mercure françois, an early ancestor of the daily newspaper. For a twentieth-century reader, it is a curious, exhilarating experience to savor Le Mercure: to have the noble rebellions, the assassination of Henry IV,

the Thirty Years War coming in as current news.

If the twentieth-century reader is a sociologist, this curious experience offers a challenge to reflection on the character of his discipline, and on sociology's relationship to history. The Mercure's reporter, after all, is proposing an age-old interpretation of rebellion.

The interpretation runs like this:

1. Self-serving, manipulative troublemakers drawn from discontented segments of the dominant classes enlist gullible rebels from the common people.
2. The common people pay all the cost, and get none of the benefits -- if any -- of rebellion.

Elites and authorities often hold that theory today.

In its context, the Mercure's interpretation is not absurd. A major form of rebellion in sixteenth- and seventeenth-century France was, indeed, an alliance between a small group of discontented, self-seeking nobles and a large group of aggrieved commoners. The weight of taxes and the maladministration of justice were, indeed, widespread grievances and frequent justifications for rebellion. The organizers of rebellion did, indeed, often decamp with the gains and escape before royal vengeance struck them down. So far as it goes, in fact, the Mercure's analysis only contains one substantial error: it underestimates the extent to which the "common people" acted knowingly on their interests; it treats ordinary rebels as a shapeless, manipulable mass. That error, many twentieth-century analysts of twentieth-century rebellion have made as well.

The Mercure offers many more occasions for sociological reflection. In 1608, for example, we have the story of the Guilleris, three noble brothers from Brittany. During the recent Wars of Religion, the brothers "had followed the League party under the Duke of Mercoeur, and had performed under his leadership as valiant, brave soldiers." On demobilization, they had formed a robber band. "The promenade of all these robbers crossed many parts of France," reported the Mercure, "all the way to Normandy, the

Lyon region, and Guyenne. On the highways leading to the fairs and markets of Poitou they posted notices on trees, reading PEACE TO GENTLEMEN, DEATH TO PROVOSTS AND ARCHERS, AND THE PURSE FROM MERCHANTS." ("Provosts and archers" were essentially the royal and municipal police of the time.) The governor of the Niort military district called together a force of provosts, besieged the Guilleris' castle, and finally took eighty prisoners. The youngest Guilleri brother was executed (Mercure françois 1608: 289-290).

Shades of Robin Hood! Although we have no evidence that the Guilleri bandits gave to the poor, they certainly felt they had the right to take from the rich. In the context of the time, their quick change from valiant soldiers to dangerous criminals was rather more a shift in attitude, name and coalition than an alteration in their day-to-day behavior. As the people of the ravaged French countryside testified repeatedly, it was often hard to tell the difference between troops and bandits. The transformation from cavalryman to highwayman, the formation of a roving band, the posting of declarations, the siege and the execution all portray for us a world in which a model of armed conquest was readily available. By no means did the national state have the monopoly on armed conquest.

Not that the state was powerless. The king, his retainers, his clients and his bureaucracy formed a greater, stronger cluster than any other in France. He who touched the royal person or prerogative paid the price. When Ravallac assassinated Henry IV in 1610, the king's counselors rolled out the terrible, clanking apparatus of royal justice. In a public execution before the Paris city hall, the hangman assaulted Ravallac's body with molten lead and red-hot iron. Then it was time for drawing and quartering. "After the horses had pulled for a good hour," reported

the Mercure,

Ravaillac finally gave up the ghost without having been dismembered. The executioner having broken him and cut him into quarters, people of all sorts went at the four parts with swords, knives and staves; they snatched the parts from the executioner so eagerly that after having beaten, cut and torn them, the people dragged the pieces here and there through the streets on all sides, in such a frenzy that nothing could stop them" (Mercure françois 1610: 457).

As was customary on such occasions, the day ended with the burning of the bloody remains in bonfires throughout Paris.

The lurid killing of Ravaillac, and the many other public executions recounted in the Mercure, add two more elements to our understanding of seventeenth-century France. First, we appreciate the importance of exemplary justice and punishment, as opposed to an effort to apprehend all violators of the law. Seventeenth-century authorities did not seek to punish all offenders, by any means; they sought to deter potential delinquents by the quick and visible chastisement of a few. The mounting of bloody examples dramatized the power of the authorities without overtaxing their limited judicial capacities. Second, we recognize the participation of ordinary people -- as spectators and, to some degree, as critics and participants -- in the process of retribution. On other occasions, that popular participation in justice provided a warrant, or at least a model, for the people's taking the law into its own hands. Tax rebellions and attacks on profiteering officials took the forms of assemblies, deliberations, declarations, condemnations and, sometimes, executions. Exemplary justice and popular participation faded away in later years, as the government's repressive power grew and the separation between accusers and accused increased.

Later in that same year of 1610, the Mercure reported yet another execution at Paris' Place de Greve, in front of the city hall. This time the victims were three gentlemen of Poitou: du Jarrage, Chef-bobbin and Champ-martin. The courts had convicted them of:

preparing a Manifesto which tried to stir the people of Poitou into rebellion, and to induce the people to join [the three gentlemen] in taking up arms, in order (they said) to change the state into an Oligarchy -- France, they imagined, not being well governed. Unworthy to die by the sword like nobles, they received the wages of their disgrace: the hangman's rope (Mercure françois 1610: 512).

Thus we learn that the law decapitated nobles and hanged commoners. We glimpse the standard routine in which rebels, like highwaymen, posted declarations of intent before striking at their enemies. And we begin to sense the prevalence of rebellion in the early seventeenth century.

The news flashes from 1605, 1608 and 1610 present more than one challenge to the sociologist. The first challenge is to say how the nearly four hundred years of experience and thought which have intervened since then have improved our understanding of rebellion and of other sorts of conflict. (The answer, I regret to say, is: precious little, and that little mainly through a) conceptual refinements and b) clarification of the connections between major conflicts and the routine pursuit of everyday interests.)

The second challenge is to lay out categories within which the general changes occurring in the France of 1605 will make sense: modernization, class struggle, agrarian bureaucracy, something else. (Although any reply we make to that challenge today is bound to be controversial and incomplete, I favor stressing the development of capitalism and the growth of national states as the contexts of

seventeenth-century struggles.)

The third challenge is to examine what difference, if any, it makes whether we approach the events of seventeenth-century France as sociologists or as historians. (My answer is that in practice sociologists and historians approach the analysis of such events rather differently, but in principle there are good reasons for seeking, not one grand synthesis, but several different syntheses of sociological and historical practice.) For reasons that will become clearer as we proceed, we might call the three challenges the problems of collective action, of structural change, and of historical analysis. These three problems have brought the varied essays in this book into being.

What Sort of Book is This?

The book you have before you is both broader and narrower than the area defined by the three challenges. It is broader in that it takes up many other topics besides rebellion and seventeenth-century France: the use of computers, the origins of the proletariat, the thought of Emile Durkheim, and several more. It is narrower, much narrower, in that it offers only fragmentary treatments of collective action, of structural change and of historical analysis. The book reports a number of different forays into the terrains of collective action, structural change, historical analysis and, especially, into their common ground.

Most of the book's chapters first emerged from my typewriter as occasional papers, and most of them have remained unpublished until now. An "occasional paper" is a contribution prepared expressly for a particular occasion -- typically a meeting or a series of lectures organized around a common theme. The original version of "Computers in Historical Research," for example, was one of many papers presented to a meeting commemorating the contribution of John Von Neumann to the

development of the electronic computer. "The Long Revolt Against Industrial Capitalism", on the other hand, is the text of a television lecture within a series on the history of work.

In one way or another, all the papers are by-products of two long, linked inquiries: 1) into large-scale structural change in western countries since about 1500; 2) into changing forms of conflict and collective action in the same countries over the same time span. The large-scale changes which receive the most attention in the book's essays are statemaking and the development of capitalism. The countries in question are most frequently France and England, less frequently other countries of western Europe, only rarely the United States and other countries elsewhere. Under the headings of conflict and collective action, the papers deal most regularly with revolutions, rebellions, collective violence, strikes, demonstrations, food riots and related ways of gathering to act on shared interests and grievances.

The disadvantage of the occasional paper as a contribution to knowledge is that the theme of the occasion is not necessarily the main theme of the author's work; the result is sometimes a certain stretching of the argument or the evidence to make it reach the common theme. (Despite later plastic surgery, "Social Movements and National Politics" still shows stretch marks resulting from the effort to make the connection between a meeting on social movements and a research project in which social movements, as such, are not the object of analysis.)

The advantage of the occasional paper is the converse of its disadvantage: it requires researchers to break out of their routines and specify the connections between their work and problems that interest other people. ("Does Modernization Breed Revolution?", for instance, deliberately addresses

the weaknesses of widely-held ideas concerning political development, while most of my work on conflict and collective action simply ignores political-development ideas as fruitless.) The net effect of the advantage and the disadvantage is to produce a set of essays that is somewhat wider-ranging and rather more polemical than the main body of the work from which it comes. All this certainly applies to the essays which follow: they contain a good deal more exhortation, appreciation, application and exploration -- and a good deal less documenting, measuring, specifying and refining -- than my own normal daily round of work.

History's Place

All the exhortation, appreciation, application and exploration has somehow to do with history. The word "history" refers to a phenomenon, to a body of material, and to a set of activities. As a phenomenon, history is the cumulative effect of past events on events of the present -- any present you care to name. To the extent that when something happens matters, history is important. Analysts of industrialization, for example, divide roughly into people who think that essentially the same process of capital accumulation, technological innovation, labor force recruitment and market growth repeats itself in country after country, and those who think that the process changes fundamentally as a function of which other countries have industrialized and established their shares of the world market before a new section of the world starts industrializing. The second group attaches greater importance to the phenomenon of history than the first group does. "Historical Analysis of Political Processes," later in the book, takes up the ways in which analyses of the past vary from ahistorical to very historical. Other essays illustrate that variation in practice. But in general they argue the importance of the influence of past events on the present: the importance of history.

As a body of material, history consists of the durable residues of past behavior. The vignettes from Le Mercure françois with which we began are misleading in this regard. They perpetuate an easy misunderstanding, one which often wanders into manuals of historiography: that "historical records" consist mainly of narratives of various kinds. Chronicles, confessions, autobiographies, eye-witness reports and other sorts of narratives are actually a tiny portion of historical material. Most historical material consists of fragmentary by-products of social routines: the remains of stone walls, trash heaps, tools, beaten paths, graffiti, and so on. As it happens, historians have concentrated on the written materials remaining from the past. But the written materials, too, are mainly fragmentary by-products of social routines: birth records, judicial proceedings, financial accounts, administrative correspondence, military rosters and bills of lading are far more numerous than are narratives of any sort.

All documents are not equally valuable in reconstructing the past. If we are trying to understand the pattern of rebellion in seventeenth-century France, one memorandum from Richelieu will be worth a thousand biblical glosses (or, for that matter, pornographic poems) from the monks of St. Germain-des-Prés. Still, coming to terms with the historical record means, among other things, appreciating how much of the seventeenth-century writing went into pious essays and pornography.

What of history as a set of activities? The central activity is reconstructing the past. That activity, too, easily lends itself to misunderstanding, to the supposition that the main historical problem is to establish the facts of what happened in the past. Establishing what happened is a hopeless program. It is hopeless for two reasons which become obvious after a little reflection. First, the supply of information about the past is almost inexhaustible. It far exceeds the capacity of any historian to collect, absorb, synthesize and relate it.

The historian has no choice but to select a small portion of the available documentation.

Second, what matters, among the innumerable things that happened in the past, is a function of the questions and assumptions the historian brings to the analysis. To the historian who concentrates on the histories of regimes, and who believes that in any particular regime the attitudes and decisions of a few statesmen make all the difference, records of births, deaths and marriages are trivial. Records of births, deaths and marriages are crucial, on the other hand, to the historian who is trying to explain why industrialization occurred when and where it did, and who believes that fluctuations in the labor supply strongly affect the feasibility of industrialization. Historians therefore select radically among available sources and facts.

Other specialists -- geologists, archeologists, classicists, paleobotanists, for example -- also draw selectively on the past. Yet they are not, in general, historians. The distinguishing features of the historical profession are these:

1. Its members specialize in reconstructing past human behavior.
2. They use written residues of the past: texts.
3. They emphasize the grouping and glossing of texts as the means of reconstructing past events.
4. They consider where and, especially, when an event occurred to be an integral part of its meaning, explanation and impact.

Historians are people who do these four things. Professional historians are simply the people who certify each other as competent to do the four things.

As in other fields, the Ph.D. serves as the chief certificate of competence in history. The history Ph.D. is a peculiar experience in one regard: although the reconstruction of past behavior, the location

and transcription of relevant texts and the analysis of those texts are the historian's distinguishing skills, the average historian-in-the-making has almost no serious practice in these skills until the last phase of his/her training. Very few historians, for example, ever enter an archive before they begin work on their doctoral dissertations. Before that time, they are busy learning other people's syntheses: basic sequences, critical events, rival interpretations, major books. Within a limited number of time-space blocks (Classical Greece, Latin America since 1816, etc.) they are learning what they might later have to teach to undergraduates. They are also, it is true, learning to write expository prose and to criticize other people's arguments. But their teachers only give them serious exposure to the basic historical skills after they, the students, have mastered their share of the discipline's ideas and beliefs. Within any particular specialty, that is, the professionals recognize each other by means of their orientation to a common literature.

In the United States, professional history is a large field, and predominantly a teaching field. At its peak in 1970, the American Historical Association had about 20 thousand members. The demographic and economic contraction of the following years brought the number a little below 16,000 by 1977. That is still a great many professionals. History was smaller than the giants among research fields: chemistry, engineering, biology and psychology. Yet it approached the size of physics, and stood in the same range as such fields as mathematics and Anglo-American literature. In 1977, some 17 thousand people who had received Ph.D.s in history from 1934 through 1976 were known to be living in the United States. During the early 1970s, the profession was grinding out about a thousand new Ph.D.s each year. In 1976/77, the figure was still 961: 36 percent in American history, 27 percent in European history, and the remaining 37 percent in a great

variety of other fields.

The great bulk of historians who make their livings as historians do so as teachers. In 1976/77, of all history Ph.D.s known to be employed, 96 percent worked for educational institutions. Of all working Ph.D.s, 79 percent were in teaching, 6 percent in research, 6 percent in management and administration, another 6 percent in writing and editing, and the final 3 percent in other sorts of jobs (all figures from AHA Newsletter, December 1978, or National Research Council 1978). Of these thousands of practitioners, most spent most of their time teaching American or European history to young people who had no intention of specializing in history. Many devoted some of their non-teaching time to research and writing. A few hundred of them actually published books and articles reporting their historical work. Those writers were the profession's nucleus. They provided the chief connections among previous work, current research, what students were learning, and what the general public was reading about history. They set the tone of historical practice.

The Historical Zoo

I hope my description does not make the historical profession seem smoothly organized, neatly hierarchical or deeply coherent. In reality, the practice of history resembles a zoo more than a herbarium, and a herbarium more than a cyclotron. In a cyclotron a huge, costly, unified apparatus whirs into motion to produce a single focused result; history does not behave like that. In a herbarium, a classificatory order prevails; each dried plant has its own niche. Historians do divide up their subject matter and their styles of thought into diplomatic, economic,

intellectual and other sorts of history, but the divisions are shifting, inexact . . . and often ignored in practice.

A zoo? Yes, watching historians at work does have something in common with strolling from the polar bears to the emus to the armadillos. Each species of historian is confined to an artificially reduced habitat, fenced off from its natural predators and prey. In the historical zoo, however, the inmates often leap the barriers to run through the spectators, to invade other cages, and even occasionally to change themselves from one sort of beast into another. Intellectual history becomes cultural history, social history edges over into economic. Nevertheless, at any given point in time the boundaries are real and significant: practitioners on one side of a line or the other have their own journals, their own associations, their own jargons, their own professional agendas. That they should be further subdivided by time and place (Modern American Intellectual History being one recognized specialty, Medieval European Economic History another) only accentuates the fragmentation of historical practice.

What happens in the zoo? Do not trust studies of historiography to tell you. Historiographers rely almost exclusively on the skills of biographers, intellectual historians and philosophers: they do not analyze history as a concrete social activity, but as the development and application of general ideas. For every discussion of how Lewis Namier actually did his work, we have a dozen discussions of the possibility, in principle, of contributing to historical knowledge by means of the sort of collective biography that Namier created.

Jerzy Topolski's massive Methodology of History, for example, begins with the complaint that

Earlier statements by historians on their own research techniques reveal the nature and degree of their methodological awareness. A few decades ago when Marc Bloch was writing his The Historian's Craft and the science of scientific method was not so far advanced as now, historians took little interest in explicit problems of methods. Since then, much has been said about the science of history without the participation of historians. Today the practitioners of historiography have to be more aware of methodological considerations (Topolski 1976: 3).

To remedy earlier oversights, Topolski devotes 600 pages or so to Patterns of Historical Research, the Objective Methodology of History, the Pragmatic Methodology of History and the Apragmatic Methodology of History. He energetically reduces the problem of historical knowledge to a special case of the problem of knowledge in general. But he writes nary a page on an actual historian's workaday approach to his research.

If we are to believe the historiographers who do portray flesh-and-blood historians, on the other hand, historians spend most of their time forming, joining or combatting Schools of Thought, focus their analytic efforts on puzzles posed by history, and do most of their own analyses by thinking themselves into the circumstances of historical actors in order to reconstruct the states of mind which led them to act as they did. We might reflect on this characterization of E.P. Thompson's work:

Attempts to partition society for purposes of analysis often build upon Marx's insight that a group's economic function generates a distinctive class culture and social system as well as particular economic interests. In The Making of the English Working Class (1963), E.P. Thompson brilliantly used the Marxist notion of class to analyze the class consciousness or culture of British workers in the eighteenth and nineteenth centuries. Thompson contended that class is not an abstract concept that can be lifted out of context and treated as a static category. If class consciousness is "largely determined by the productive relations into which we are born," he wrote, it still develops over time and is conditioned by particular experiences. Class consciousness cannot be deduced from general principles, but must be studied historically. Thompson insisted that although the rise of class consciousness follows similar patterns in different times and places, it never occurs "in just the same way" (Lichtman and French 1978: 110-111).

Thompson did, indeed, use the Marxist notion of class brilliantly. He did, in fact, emphasize the conditioning of class consciousness by particular experiences. Yet the summary suggests that Thompson chose (for unstated reasons) to study British working-class culture, then chose to set up his study as an analysis of class consciousness, then developed a theory of class consciousness in order to deal with the available evidence.

The intellectual context is missing. Especially lacking are two sorts of controversy: about whether England somehow escaped from a revolutionary situation in the first half of the nineteenth century, about the conditions

under which workers develop militant class consciousness. A reader of Thompson who ignores this context is likely to be puzzled by his repeated, vigorous, indignant, sometimes dazzling critiques of nineteenth-century observers (such as Francis Place and Andrew Ure) as well as of twentieth-century historians (for instance, John Clapham, R.F.W. Wearmouth, George Rudé and Neil Smelser). Thompson must knock down a lot of bystanders in order to make his own way to the reviewing stand.

Now, E.P. Thompson is not only a talented historian but also an adroit polemicist. With a flick of his pen he can summon an image of an entire worker's movement, or dispatch an opponent to oblivion. Most historians fall short of his accomplishments in either regard. Yet they try. Historiographers tend to ignore, or conceal, how much historical writing consists of documented commentary on previous historical writing. Instead, they give us an historian who dreams up questions on his own, and then goes to the sources to find the answers to those questions.

Historical Practice as Social Structure

Real historians behave rather differently. In order to be clear and concrete, let me concentrate for a while on American historical practice. In the United States, by and large, a practicing historian embeds himself in a segment of the profession: modern Latin American economic history, Tokugawa urban history, or something of the sort. The basic differentiation is three-dimensional:

1. place (Africa, Asia, Brazil . . .);
2. time (Medieval, Renaissance, Early Modern, Modern, Contemporary, to take a common way of dividing up European history);
3. subject matter (political, intellectual, diplomatic, social, etc.)

Courses and graduate programs in American universities divide up in roughly the same ways. As a result, most historians work mainly in one time-place-subject subdivision of the profession, but are comfortably

familiar with one or two more. Someone who works competently in four or five of the hundreds of pigeonholes defined by these dimensions is considered broad indeed.

As a social structure, each historical subdivision has two main elements: an interpersonal network and a shared agenda. The network's nodes consist of major teachers and their former students. The shared agenda has several components: a set of pressing questions, an array of recognized means for answering those questions, and a body of evidence agreed upon as relevant to the questions. Some, but not all, networks formalize their existence by giving themselves a name, an association, a journal or other professional impedimenta.

American specialists in the history of the family, to take one recent case, long plied their trade as no more than a particularly well-connected clump in the network of social historians. At the end of the 1960s family historians -- encouraged by the success of their European counterparts -- began to differentiate themselves more decisively from other social historians. This historical network (like others tainted with social science) connected people who were interested in the same phenomena across a wide variety of times and places; historians of modern Africa talked to historians of ancient Rome. During the early 1970s, American historians of the family created conferences, an association and a journal of their own. By that time, a well-demarcated subdivision of the profession had come into existence; a college department could say it wanted to hire a historian of the family, and a well-oiled mechanism of communication and validation would whirl into action.

Historians with an entrepreneurial flair ordinarily play important parts in this sort of institution-building. By these means (as well

as by editing, reviewing, refereeing and other time-honored means of scholarly promotion and control) they help set the intellectual agenda. In history, specialists who are well-connected outside their own country -- particularly those who are connected with scholars in parts of the world whose history they are studying -- carry significant extra weight; even if they have few ideas of their own, they commonly serve as conduits and interpreters of work being done elsewhere. Because of this structure, historians who are already well-placed find it fairly easy to reproduce themselves by connecting their own graduate students (and, sometimes, a few other carefully-selected clients) to the structure.

The intellectual agenda itself consists of questions, means for answering questions, and a body of evidence. As in many other disciplines, the historians in a given specialty implicitly orient the bulk of their work to a handful of crucial questions. In American political history, for example, whether the War of Independence constituted a full-fledged popular revolution, whether the Civil War was the inevitable denouement of a long struggle between two antithetical ways of life and why no durable socialist movement arose in the United States stand high on the agenda; they compel much more attention than such questions as whether nineteenth-century changes in suffrage altered the national structure of power. A young historian who wants to make an impact on other historians will pose a fresh answer to part of one of the crucial old questions, will help refute one of the established answers, or will assemble a new body of evidence supporting an answer that is already in competition.

The orientation to a compelling set of questions, however, creates an interesting ambivalence. On one side, the historical profession lies in wait, posing compelling questions, demanding new answers, and insisting on a demonstration of familiarity with previous work in the field as well as with the available evidence. On the other side, a larger public calls for interpretations which are lively, lucid and self-contained. What is more, the professionals reserve a particular admiration for the historian who reaches that larger public without compromising technical standards. In that, they resemble many of their colleagues in the humanities, but differ from most of their colleagues in the natural and social sciences. The natural and social scientists tend to doubt the seriousness of anything that reads too easily or sells too well. The humanists tend to think of the supreme accomplishment as a work which is at once accessible and profound. The humanists and historians are bookish; although they prize the well-turned essay, they cherish the well-read book.

Historians are more concerned about contact with the general public than are most academic intellectuals; even the narrowest specialists cheer the colleague who writes graceful, accessible prose. They envy or admire the author who can write historically acceptable best-sellers. Consider the books which won the Pulitzer Prize from 1968 through 1978:

- | | |
|------|---|
| 1968 | Bernard Bailyn, <i>Ideological Origins of the American Revolution</i> |
| 1969 | Leonard W. Levy, <i>Origin of the Fifth Amendment</i> |
| 1970 | Dean Acheson, <i>Present at the Creation: My Years in the State Department</i> |
| 1971 | James MacGregor Burns, <i>Roosevelt: The Soldier of Freedom</i> |
| 1972 | Carl N. Degler, <i>Neither Black Nor White: Slavery and Race Relations in Brazil and the U.S.</i> |

- 1973 Michael Kammen, *People of Paradox: An Inquiry Concerning the Origin of American Civilization*
- 1974 Daniel J. Boorstin, *The Americans: The Democratic Experience*
- 1975 Dumas Malone, *Jefferson and His Time*
- 1976 Paul Horgan, *Lamy of Santa Fe*
- 1977 David M. Potter, *The Impending Crisis*
- 1978 Alfred D. Chandler, *The Visible Hand: The Managerial Revolution in American Business*

Biographies and broad new interpretations of American experience dominate the list. Those are the contributions for which the intellectual world as a whole rewards historians. When rewarding each other, historians are somewhat more likely to give attention to new techniques and new varieties of evidence. The American Historical Association's Bancroft Prize, for example, has gone to books with these titles:

- 1973 *Fire in the Lake: The Vietnamese and Americans in Vietnam*
The U.S. and the Origins of the Cold War
Booker T. Washington
- 1974 Frederick Jackson Turner
The Other Bostonians
The Devil and John Foster Dulles
- 1975 *Time on the Cross*
Roll, Jordan, Roll
Deterrence in American Foreign Policy: Theory and Practice
- 1976 *The Problem of Slavery in the Age of Revolution*
Edith Wharton: A Biography
- 1977 *Class and Community: The Industrial Revolution in Lynn*
Slave Population and Economics in Jamaica
- 1978 *The Visible Hand: The Managerial Revolution in American Business*
The Transformation of American Law, 1790-1860

Biographies still stand out among the prize-winners, but general reinterpretations of American life appear to attract the Bancroft judges less than they do the Pulitzer Prize committees. Fresh answers to old questions on the historical agenda win praise from the insiders. As the inclusion of Fogel and Engerman's *Time on the Cross* (with its econometric analyses of the profitability of American slavery) indicates,

the fresh answers may even be controversial, and may even build on the social sciences. Yet on the whole technical *tour de force* take second place to graceful expositions of subjects which interest the literate public. Thus the historical scholar who craves his peers' esteem must find a way to surmount the dilemma: solidity versus accessibility.

The newly-trained historian faces the dilemma in its extreme form. The doctoral dissertation in which he has just invested four or five years ordinarily addresses a precise sub-question of one of the Big Questions, reviews previous answers to that sub-question meticulously, catalogs and arrays the available sources, and cautiously lays out the evidence for a new reply to the sub-question -- in short, situates itself exactly with respect to an existing literature. But now, the dissertation completed, the young historian's career depends on publishing a book. A few fresh Ph.D.s have the good fortune of access to monograph series which publish books greatly resembling dissertations. Or they have a topic and a dissertation committee which permit them to make light work of the connections with the field. Most of them, however, must think about turning a manuscript heavy with scholarly apparatus into something quite different: a book whose buyers generally care little about the state of the literature, but are looking for a rounded, convincing, comprehensible treatment of the subject at hand. As editors and thesis advisors learn to their pain, the transformation commonly requires the dismantling not only of the dissertation, but also of the former graduate student's training in documentation and cross-reference. To become working historians, the newcomers must unlearn their graduate educations.

But not completely. The skillful manipulation of acceptable sources remains an essential part of the craft. The problem for the professional is how to convey the insider's signs of authenticity without

impeding the outsider's access. His book must contain enough "primary" sources -- texts produced as a direct effect or observation of the historical circumstances under analysis -- to demonstrate his familiarity with the era and its materials. Yet he must weave the sources into a coherent argument. The argument, in turn, must differ in some significant way from those proposed by earlier authors. The entire procedure requires a lawyerly handling of the evidence.

Handling the Evidence

What is that evidence? At the borderlands of anthropology and history, potsherds, wall-paintings and paving stones serve as the historical record of distant civilizations. Some historians of art and culture work with buildings, sculptures and pictures. Students of the recent past have tape recordings and films at their disposal. Philippe Ariès and Lawrence Stone have made funerary sculpture speak to us about the family life of earlier centuries. Yet the great bulk of the evidence that historians learn to use -- and do use, in fact -- consists of texts. Historians are the specialists par excellence in reconstructing social life from its written residues.

Within any particular historical specialty, however, practitioners tend to recognize only a limited range of texts as useful to their enterprise. In most subdivisions of history, ostensibly direct testimonies by major actors -- autobiographies, depositions, private letters, and so on -- have long held pride of place. In the history of the family, such testimonies complement marriage contracts, birth registers, household property inventories and other records of routine transactions. A military historian, on the other hand, is unlikely to pay much attention to routine domestic transactions. At least a military historian is unlikely to pay much attention until someone else shows that birth

registers and the like yield fresh answers to the questions the discipline is already posing.

A significant part of historical innovation consists, indeed, of showing that new sources will answer old questions better, or differently. During the 1960s, Stephan Thernstrom almost single-handedly reoriented American urban history by demonstrating that with appropriate statistical processing readily-available city directories and similar enumerations of the local population would yield estimates of the rates and directions of occupational mobility among different segments of the population. He created individual biographies by following the same person from one record to the next, collective biographies by summing up the experiences of all members of a given cohort, class or ethnic category. Thernstrom modeled many of his procedures on those of sociologists who had been studying twentieth-century mobility patterns, and found ways to make them work in a nineteenth-century context with nineteenth-century evidence. He cannily chose to study the very Newburyport, Massachusetts -- "Yankee City" in pseudonym -- whose twentieth-century class structure Lloyd Warner and associates had examined in such detail, and whose nineteenth-century class structure Warner had sketched from the local people's memory and myth.

Thernstrom's findings countered the notion of a slowing of mobility from a fluid nineteenth century to a rigid twentieth century. They also suggested different patterns of mobility for different ethnic groups. His analysis therefore bore on two of the classic questions of American urban history: whether the nineteenth-century city was a sort of opportunity machine which gradually slowed down, whether the ethnic and racial diversity of the American working class hampered the development of common living conditions, class consciousness and collective action. Other

historians immediately took up Thernstrom's challenge and his model of analysis; not only city directories, but also manuscript censuses and a variety of other records suddenly became relevant to pressing questions of the field.

Today's historiography grows from yesterday's history: just as previous historians have set the current questions, they have identified the proper means for answering them. The means vary from one historical subdivision to another. Because so many major questions in American political history turn on the mentalities and calculations of the chief actors -- the Founding Fathers, Abraham Lincoln or, more rarely, The People -- the favored means consist either of documenting those mentalities or of rearranging the existing evidence in a new interpretation of mentalities and calculations which appears to be more consistent, economical and/or plausible than the available interpretations. The conventional means of documenting mentalities proceed through the exposition of correspondence, of public writings, of utterances, or perhaps of the materials of folk culture: songs, slogans, tales, pictures and the like. Some historians have lavished attention on voting records, and have built up large quantitative analyses of the correlates of one voting preference or another. Three of America's most energetic organizers of quantitative electoral studies speak of

the electoral statement as a means of penetrating the outer structure of political life and charting the subterranean arena of conflicting values, interests, and desires that exist in most societies (Silbey, Bogue and Flanigan 1978: 4).

The persistent secret hope of voting analysts is, I think, not so much to absorb political history into political science as to establish a new, reliable means of documenting popular mentalities.

Reinterpretation, however, scores more points with fellow historians than does documentation. Historians share with artists and literati a deep admiration for the ability to state and defend an "original thesis". An able young scholar must, in consequence, take the greatest care to differentiate his arguments from those of his mentor; there is nothing worse in history than to be thought imitative -- better dull than dependent! That drive to identify a topic and a approach, then to make them your own, accounts for a feature of historians' behavior which frequently puzzles outsiders: if two people discover that they are working on the same topic, instead of competing to solve the problem faster and/or differently (as people in many other fields would do), they tend to divide up the territory: one drops the topic, both redefine, or they work out a division of labor. A "responsible" thesis director will not let his student continue working on a topic if he discovers that someone else is further along with the same topic.

Historians commonly rationalize this behavior by saying that it takes a long time to become familiar with a topic and that competition for the same unique body of evidence is likely to hamper the work of both investigators; it is therefore doubly inefficient to have two people working on the same problem. But such arguments apply a fortiori in fields where research is more expensive, and in which no such rule applies. In fact, the rule resembles the rule of serial monogamy: adultery is unacceptable, but divorce and remarriage are desirable solutions to marital discord. Once Historian A has written her book, it is fair play -- even high adventure -- for Historian B to go back to the sources and tear up A's argument. The stress on originality and the emphasis on reinterpretation dovetail.

Reinterpretations and Theories

This complex social structure helps explain how historians can so easily shrug off work by non-historians which, from the outside, looks highly relevant to their concerns. It helps account for the mystique of primary sources and archives. It clarifies why the recurrent call for something like a "general history of civilizations" (e.g. Marrou 1967: 1475) attracts polite applause, but no action. Even the "total history" advocated by a Fernand Braudel turns out in practice to be time-place history which broadens the range of sources and processes under examination. Historians recognize fellow specialists by their familiarity with a set of conventional categories and facts concerning a particular ensemble of places and periods, their competence in locating and using a set of sources (usually writings of various sorts) agreed upon as relevant to the events which took place in those periods and places, and their orientation to the current body of doctrine and controversy about those periods and places. The worker who deals familiarly with those categories, facts, sources, doctrines and controversies, who builds an argument and a body of evidence which reinterpret some or all of the categories, facts, sources, doctrines and controversies, gains recognition as a genuine historian. The reinterpretation starts from the knowledge that previous practitioners have left behind.

And why not? Any coherent field proceeds by elaboration and criticism of previous work. Even poems and symphonies often define themselves in relation to previous poems and symphonies. I stress the connection between current and previous work in history only because historians have worked out their own distinctive version of that connection: cutting the past into time-place blocks, posing a limited set of questions about each block, paying exceptional attention to the questions the literate public is asking about that period and place, giving priority to politics, being concerned about the didactic, moral and political implications of the historical experience under analysis, insisting on the virtues of familiarity with

a basic set of texts concerning that experience, and valuing the individual mastery, understanding and interpretation of the available texts. Given this organization of inquiry, we should not be surprised to find historians proceeding in something like the fashion of literary critics: moving, textes à l'appui, from reinterpretation to reinterpretation. Not for most historians the economist's derivation and estimation of a model from neo-classical economic theory, or the sociologist's effort to bring data to bear on two conflicting hypotheses. No: an historical reinterpretation should produce a new understanding of the place, time, phenomenon and underlying question under study.

Nevertheless, the means of reinterpretation vary from field to field within history. Demographic history, for example, has a technical edge: one shows that the methods by which earlier historians arrived at crucial conclusions were faulty, and that other methods produce substantially different conclusions. Thus Thomas McKeown begins his challenging reinterpretation of the causes of modern western population growth with a modest demurrer:

Demographers and historians interested in the pre-registration period have attempted to provide a substitute for national records by exploiting the information available on parish registers and bills of mortality. Can we, from such sources, expect to get a reliable national estimate of fertility, mortality and cause of death? I do not think so (McKeown 1976: 7).

This hesitant seed explodes into a giant shade tree, cutting the sun from all its competitors. McKeown systematically sets up the accounting problem, steadily counters alternative accounts of population growth (he is especially deft at cutting down arguments which stress the early contributions of medical improvements to life expectancy) and gradually builds up a case in which better nutrition plays a central part.

Reorientations in political history, on the other hand, rarely spring from methodological innovations. An impressive case in point is quantitative political history: although dozens of historians have undertaken the measurement and modeling of elections, of legislative behavior and of political elites, and although the advocates of quantitative analysis have been among the most vociferous critics of narrative and biographical approaches to political history, the field continues serenely to reward studies of Thomas Jefferson and of the American political temper.

The variation in question-posing from one subfield in history to another gives the lie to two easy interpretations of the role of theory in historical analysis. (I am not speaking of the role that theory could or should play, but of theory's actual place in the routine activities of working historians.) The first easy interpretation is that history is essentially atheoretical: a miscellany of facts and opinions. The second is its contrary: that theory plays about the same part in history as in any other analysis of human affairs, except that historians' general theories are usually commonsense, or poorly explicated, or both.

Neither is correct. The practitioners in each subfield of history create their own agenda and establish a limited number of theories as relevant to the answering of questions on the agenda. Both the agenda and the available theories change in spurts, as new reinterpretations come along. The reinterpretations, in their turn, respond to the internal agenda, to new ideas in adjacent fields, and to events in the world at large. Ultimately, the most consistent points of reference for all these agendas and theories are the political histories of large time-space blocks: Why did European states and their extensions come to dominate Asia and the rest of the world after the eighteenth century? Why did "traditional" China give rise to a far-reaching socialist revolution? Such master questions give rise to the subquestions on which most

historical work actually focuses: why, for example, Britain became the dominant colonial power in the eighteenth and nineteenth century, or whether the Chinese revolution of 1911 somehow anticipated, or even caused, the struggles which eventually produced a Communist regime. Theories of capitalism, of liberalism, of industrialism, of class struggle ultimately guide historians' inquiries into the multiple subquestions. Elsewhere in history, the master questions and relevant theories are different, but just as well defined.

Many of the relevant theories are themselves historically rooted. "Historically rooted" means embedded in time: focused on some historically specific setting or process such as the growth of a capitalist world-economy after 1500, or at least postulating some important alteration in a process depending on where it occurs in a time-sequence. (Alexander Gerschenkron's discussions of the "advantages of backwardness" in industrialization -- the chief advantage being that a latecomer can profit by the successes and failures of early industrializers -- provide an example of the second sort of historically-rooted theory.) The historical rooting of historians' theories is neither self-evident nor universal; general psychological theories, timeless models of organizational structure and ahistorical conceptions of political processes show up regularly in historical analysis. Nevertheless, the historical grounding of the historians' master questions also predisposes historians toward historically grounded theories proposing answers to the questions.

Stinchcombe's Challenge

Arthur Stinchcombe has recently offered an account of the place of theory in historical analysis which differs somewhat from mine. In his Theoretical Methods in Social History, Stinchcombe pursues the theme that "One does not apply theory to history; rather one uses history to develop theory" (Stinchcombe 1978: 1). General ideas are illusory:

The argument here is that such ideas are flaccid, that they are sufficient neither to guide historical research nor to give the resulting monograph the ring of having told us about the human condition. These ideas are good for introductions and conclusions, for 1-hour distinguished lectureships, for explaining briefly what our profession is all about, and for other functions in which easily comprehensible and inexact ideas are useful. They are not what good theory applied to historical information looks like, and consequently their being psychologically anterior has no epistemological significance. It is the fact that "theories of social change" consist of such flaccid general notions that makes them so much less interesting than studies of social changes (Stinchcombe 1978: 116-117).

Effective studies of social change, according to Stinchcombe, identify deep causal analogies among sets of facts, then build the sets of facts thus established into cumulative causal analyses of the particular processes of change under study. Facts are deeply analogous if they have similar causes and similar effects; we might build a deep analogy among different forms of time- and work-discipline imposed on workers by pointing out that they all result from the effort of owners to increase their discretionary control over the factors of production, and all tend to sharpen the division between work and non-work.

Stinchcombe goes farther; he argues that proper causal analogies identify "similarity in what people want and what they think they need to do to get it" (Stinchcombe 1978: 120). Thus in our analysis of time- and work-discipline, we might claim that in case after case owners and workers are locked in the same strategic conflict: each side seeking to extend its control over the factors of production, but adopting a distinctly different

strategy for doing so. By such a deep analogy we anchor a fact in a particular historical situation: these owners and workers in this place and time are locked in a characteristic struggle over time- and work-discipline. The core of an effective historical analysis, however, is not the establishment of single facts. It is, in Stinchcombe's view, the construction of a sequence of facts (each established as a fact by means of proper causal analogy) into a cumulative causal process in which each fact creates the conditions for the next. Thus we might find a new market opening up, entrepreneurs increasing the work they farm out to local weavers in order to meet the expanded demand, entrepreneurs making profits and accumulating capital, some entrepreneurs trying to increase their volume and their profits by standardizing the product and the conditions of production, those same entrepreneurs inventing or adopting means of time- and work-discipline such as grouping previously dispersed workers into the same shop, workers resisting by means of sabotage, mutual pressure and strike activity . . . and so on indefinitely. The mark of a good Stinchcombian analysis is not that the whole sequence repeats itself in many different situations. It is that the causal status of each step in the sequence is established by a deep analogy with other similar situations elsewhere, and that the effects of one step are the causes of the next.

Most narrative history, thinks Stinchcombe, is seductively misleading. It gives the appearance, but not the substance, of such causal sequences. Most narrative history is superficial because the deep analogies are missing; the author substitutes an easy, unverified reading of the intentions of the chief actors or (worse still) a presentation of the sequence of events as the working out of a dominant Force or Plan. Sociologists who stumble into history, Stinchcombe suggests, commonly go wrong because the conventions of narrative history mislead them into thinking they can substitute their own

(presumably superior) Forces, Plans or readings of intentions for the historians' pitiful versions. The sociologists' pretensions convert a harmless, if ineffectual, literary device into a pernicious mishandling of the historical record.

Stinchcombe attaches his provocative arguments to detailed, ingenious exegeses of the work of four historical analysts: Leon Trotsky, Alexis de Tocqueville, Neil Smelser and Reinhard Bendix. (None of the four conforms to the image of the archive-mongering professional historian I constructed earlier, Smelser and Bendix even less so than Trotsky or de Tocqueville. For Stinchcombe's main argument, however, it matters little whether the analyst's raw materials are texts or other historians' glosses on texts.) When they are good, Stinchcombe concludes, they all do pretty much the same thing: they work effectively with deep analogies. When they try to apply very general models to large historical sequences, conversely, their results are as vacuous and misleading as anyone else's. Theoretical Methods in Social History ends with these words:

The moral of this book is that great theorists descend to the level of such detailed analogies in the course of their work. Further, they become greater theorists down there among the details, for it is the details that theories in history have to grasp if they are to be any good (Stinchcombe 1978: 124).

Now, there is a conclusion calculated to offend almost everyone: historians, historiographers, theorists, history-seeking sociologists. Even if it is wrong, any statement which strikes at so many cherished interests with the same blow deserves serious attention.

It is not wrong. There is much truth in Stinchcombe's cantankerous

argument. Much supposed application of general theories to history does consist of assigning resounding names -- rationalization, modernization, secularization, hegemony, imperialism -- to known facts. The search for deep analogies is, indeed, a key to effective historical explanation. Narrative history does commonly give an illusory appearance of causal solidity, an appearance which shatters as we reach out to grasp the connections. Stinchcombe's main points are correct.

Yet they are only correct within stringent limits. Let us distinguish between two processes: the one by which historians arrive at conclusions, and the one by which they make those conclusions intelligible and convincing to other people. The two processes intertwine, but they are never identical, and sometimes quite different from each other. Stinchcombe's analysis of historical practice deals almost exclusively with the second process: how historians make their conclusions intelligible and convincing to others. The central issues, furthermore, are epistemological; the point is not to say how most run-of-the-mill historians do their work from day to day, but to identify the conditions under which we could reasonably accept historical accounts -- and instructions for producing historical accounts -- as valid.

When it comes to arriving at conclusions, as opposed to validating them, historians can and do rely on broad theories. They do so in two ways: 1) the agenda for any particular subfield of history has a theoretical edge; the student of demographic history, for instance, can hardly escape the influence of the ever-present theory of demographic transition; 2) haphazardly or rigorously, the search for evidence relevant to the subfield's questions entails a theoretical choice. The American historian who examines the treatment of slaves by undertaking a detailed study of slaveholders' diaries, while neglecting the records of slave auctions, makes an implicit choice favoring a theory in which slaveholders' attitudes

are significant determinants of slave experience. Historians may arrive at deep analogies, but they begin with theories, crude or refined.

Even in the area of validation, real historians rarely conform to Stinchcombe's prescriptions. Their practice is narrower in some regards, and broader in others. It is narrower in that historians ordinarily require validation which goes beyond logical conviction. The two most pressing requirements are that the analysis be relevant to the existing historical agenda, and that it be based on irrefutable texts. It is broader in that historians do commonly grant validity to forms of argument which Stinchcombe forcefully rejects: psychologically compelling narrative, and effective naming of an era, a group or an intellectual current. That such practices are widespread does not, of course, make them sound. Still, their prevalence makes it clear that (for all the delightful exegesis of Trotsky, Tocqueville, Smelser and Bendix) Stinchcombe's main business is not a description but a prescription.

Within Stinchcombe's chosen limits, however, I have only one substantial objection to his argument. The general theories which Stinchcombe dismisses as irrelevant to historical explanation commonly contain instructions for the identification of deep causal analogies. Theories are tool kits, varying in their range and effectiveness, but proposing solutions to recurrent explanatory problems. Some of those instructions are worthless, some are misleading, and some are good. But it is normally better to have a bad tool than none at all.

Why? Because explanatory problems recur in history as they do elsewhere. When a problem recurs, why make the same mistakes over again? Even a bad theory generates standard ways of solving recurrent problems, reminders of difficulties on the way to the solutions, and a record of past results. Toward the end of the nineteenth century, Emile Durkheim

elaborated a theory of social differentiation and its consequences:

The theory includes, among other things, a sort of race between differentiation and shared beliefs: if a society's shared beliefs accumulate faster than it differentiates, change is orderly; if differentiation proceeds faster than shared belief, disorder (suicide, industrial strife, protest, sometimes even revolution) results. Durkheim's theory is bad. As "The Uselessness of Durkheim in Historical Analysis," later in this volume, indicates, it not only generates invalid historical analogies (for example, between individual crime and collective protest) but also misstates the causal similarities among situations (for example, different streams of rural-to-urban migration) which are, in fact, analogous. Yet even this bad theory has advantages as a tool of historical analysis. First, it crystallizes a line of argument which is pervasive in western folk sociology, and therefore quite likely to turn up when historians confront suicide, industrial strife, protest and other presumed varieties of disorder. It saves time, effort and confusion to identify the main lines of the argument at the outset, rather than to have it enter the account piecemeal. Second, it contains instructions for analogizing and marshalling evidence in support of the analogy: the user must at a minimum make a showing that the people detached from existing systems of shared belief have a particular propensity to disorder. Finally, its repeated explicit use produces a record of successes and failures (in the case of Durkheim's theory, I believe, mostly failures) in arriving at satisfactory causal analogies. The record should eventually teach the users of that particular toolkit something about the scope and value of the solutions it contains.

And there are good theories. Leon Trotsky (to take one of Stinchcombe's favorite theorists) proposed a theory of dual power: loosely stated, that an essential precondition of revolution is the emergence of an alternative concentration of power, a counter-government, to which the bulk of the

population can switch its allegiance if the existing government demonstrates its incapacity or intolerability. That is, I think, quite a good theory. It contains a set of instructions for analyzing a pre-revolutionary situation: look for the dual power, check the conditions for acquiescence of the population to the existing government, watch for defections, and so on. In short, press this particular analogy.

Trotsky's theory of dual power is an especially appropriate example because it is not just a good theory, but also an historically-grounded theory. Trotsky grounds his analysis on an explicit comparison of the English Revolution of the seventeenth century, the French Revolution of the eighteenth, and the Russian Revolution of 1917. That sets limits to the theory's domain; as Trotsky formulates it, the theory is not likely to operate well outside the world of fairly strong, centralized and autonomous national states. The restriction is the price we pay for a theory which works effectively within those limits.

According to this account of the place of theory, and according to Stinchcombe's treatment of deep analogies, the potential place of the social sciences in historical work is very large. Whatever else they do, the social sciences serve as a giant warehouse of causal theories and of concepts involving causal analogies; the problem is to pick one's way through the junk to the solid merchandise. Only a few fragments of the historical profession, however, have regular contact with the day-to-day work of the social sciences. Even fewer have anything to do with sociology -- at least with sociology as a research discipline. To be more precise: a relatively small number of historians in a few specialties carry on a continuous dialogue with the social sciences, including sociology. A substantial minority of historians find themselves interested by arguments and, especially, by concepts emerging from one social science or another, although they are not prepared to introduce into their own work the research procedures and modes of analysis which accompany

the arguments and concepts on their home grounds. A majority of historians station themselves in a range running from indifference to hostility: from no greater interest in the social sciences than in many other parts of the western intellectual heritage over to outright detestation of the idea and the practice of social science.

A Survey of Historians

A mail survey I organized in the late 1960s cast some light on the walls between history and the social sciences. (For details, see the appendix to Landes and Tilly 1971.) Just under 600 historians holding regular appointments in 29 important departments of history somewhere in the United States sent in usable replies to a questionnaire concerning their involvement in the social sciences. The questions split between a) checklists and short-answer questions inventorying professional training and various forms of contact with the social sciences and b) open-ended requests for opinions and proposals concerning graduate training, support for research, disciplinary identification and related questions. Assembled and tabulated, the responses amount to a statistical map of the zoo.

Some of the free comments were pungent:

I CONSIDER THE SOCIAL SCIENCES TO BE PSEUDO-SCIENCES LIKE ALCHEMY OR ASTROLOGY. THEY DO NOT ASK THE RIGHT QUESTIONS, NOR DO THEY LEAD TO REPUTABLE SOLUTIONS.

THE SOCIAL SCIENTISTS ARE TRYING TO IMITATE THE MATHEMATICIANS AND THE PHYSICISTS - THIS IS A DEAD-END.

WE NEED MALTHUSIAN RESTRAINT IN RESEARCH, NOT EXPANSION, SUPPORT OR ENCOURAGEMENT. DEMAND QUALITY & ACCEPT NO SUBSTITUTES.

(in answer to a question about how to encourage and facilitate further training for practicing historians) ALLOCATE SOME OF THE FUNDS USED FOR QUESTIONNAIRES TO TRAINING.

Others were, to be sure, more hospitable. A few hardy souls confided that they actually were social scientists who happened to be lodged in departments of history. But the bulk of the respondents clearly saw the social sciences, including sociology, as the other side of the wall.

Their formal training pointed them in that direction. The percentages getting their basic degrees in history, the social sciences, area studies, language and literature and other fields ran like this:

<u>Percent getting degree in:</u>						
<u>degree</u>	<u>history</u>	<u>social sciences</u>	<u>area studies</u>	<u>language/literature</u>	<u>other</u>	<u>total</u>
B.A.	73.2	7.9	3.6	10.0	5.3	100.0
M.A.	80.1	5.9	6.6	3.5	3.9	100.0
Ph.D.	88.5	3.1	3.9	3.1	1.4	100.0

The handful of social-science Ph.D.s in these outstanding history departments came mainly from economics and international relations; not a single sociology Ph.D. appeared. ("And a good thing, too!" I can hear most of the respondents exclaiming.) In fact, the chief news is that historians breed historians. The geographic specialties broke down as follows:

United States	31.6%
Latin America	4.9
Europe	47.7
Asia	9.1
Africa	2.0
Other	<u>4.7</u>
Total	100.0

The great bulk of these people were specialists in the nineteenth and twentieth centuries.

The percentage of U.S. specialists is a bit lower, and the percentage of European specialists substantially higher than among the new Ph.D.s of 1976/77: as I mentioned earlier, 36 percent of the new Ph.D.s were in American history, 27 percent in European history. Although other interpretations of the discrepancies are possible, I believe they represent a) the tendency of lower-prestige departments to emphasize American history, and of high-prestige departments to give priority to European history, and b) a temporal shift toward American history. As the student body, and therefore the demand for history teachers, contracts, the profession as a whole shrinks toward its irreducible core: American political history.

When asked about their specialization within their time-space blocks, the historians distributed themselves this way:

political	22.4%
diplomatic	9.7
intellectual	15.0
science	3.4
economic	7.0
social	13.8
other	<u>28.7</u>
total	100.0

A near-majority, then, were dealing with the political, diplomatic or intellectual histories of their areas. The top ranks of the historical profession, in short, then consisted largely of men (I use the masculine term advisedly) trained in history from undergraduate days onward, and focusing their attention on the recent history of western countries.

About half the historians in the sample had received what they

regarded as "substantial" training in one or another of the social sciences. (In general, "substantial" meant at least a graduate minor in the subject.)

The percentages claiming substantial social science training ran as follows:

political	51.3%
diplomatic	56.4
intellectual	27.8
science	11.1
economic	75.0
social	34.3
other	62.6
all fields	48.4

The only surprising thing about the high proportion of economic historians reporting substantial social science training is that it isn't higher: a full quarter of the specialists had not received training in economics. Nor is the low percentage for historians of science surprising; their outside training is in natural science, and they are commonly hostile to the claims of the social sciences. Intellectual historians resemble them in their antipathy for the social sciences, but differ in getting most of their outside training in literature. The social historians are badly off: professing a serious interest in the social sciences, but having a fragile hold upon them.

Although historians in general value individual work and eschew collaboration, the principle varies significantly by specialty. When asked whether they had ever done collaborative work, our historians said yes in these proportions:

political	15.1%
diplomatic	16.4
intellectual	6.3
science	5.6
economic	29.5
social	11.1
other	17.9
all fields	15.3

The pattern is essentially the same as for training in the social sciences: economic historians at one extreme, intellectual and scientific historians at the other.

The pattern alters considerably, however, when we ask who gets support for his research. Table 1 presents some simple information on that question. The table summarizes financial support from the historian's own institution, outside grants, and released time for research during three years from the summer of 1964 to the summer of 1967. The results break apart the intellectual historians and the historians of science, despite their great similarity in other regards; the intellectual historians come across as poor cousins. The general rank order of privilege is roughly:

science
economic
social = political = diplomatic
intellectual

I do not think the rank order has changed substantially since then.

A few themes recurred throughout the open-ended sections of the questionnaire responses: the need for a greater variety of requirements in graduate training (different languages, substituting statistics for

Table 1. Historians' Survey: Support for Research by Field of Specialization

Indicator of Support	Specialization Named								
	No Answer	Politic	Diplo- matic	Intel- lectual	Science	Eco- nomic	Social	Other	Total
Percent receiving no financial support from university in last three years	22.0	31.9	27.3	29.1	27.8	25.0	21.2	35.8	28.6
Percent receiving over 4,5000.00 from university	20.0	12.6	29.1	19.0	22.2	38.6	26.3	17.9	21.3
Percent receiving no outside grants in last three years	46.0	58.8	58.2	63.3	33.3	45.5	57.6	62.6	57.1
Percent receiving over 5,000.00 in outside grants	22.0	22.7	18.2	13.9	44.4	38.6	26.3	21.1	23.2
Percent receiving no released time for research in last three years	70.0	67.2	52.7	63.3	44.4	36.4	62.6	58.5	59.9
Percent receiving over six months released time	14.0	21.8	16.4	15.2	27.8	20.5	11.1	22.0	18.1
Number in category	50	119	55	79	18	44	99	123	587

a language, permitting a genuine concentration on economics, etc.) depending on the particular specialization the graduate student was pursuing; the value of properly-organized "think tanks" and other devices for bringing scholars together for considerable periods; the need for funds to support research by graduate students. Others were frequently mentioned, but with a sharp division of opinion: retooling through formal training after the doctorate (a minority of the commentators rejected such an enterprise as a pretentious waste of time, and recommended the substitution of time for individual reading and reflection); historians as social scientists (the small number who had degrees in economics or anthropology generally assumed that social science was the point of their work, another small group drawn especially from the literary branches of history insisted on a place among the humanists, and many more either expressed ambivalence or rejected the choice humanities/social science in favor of history -- just plain history); larger grants for the expensive varieties of research (some few said this would simply enrich the boondogglers and take more historians away from their true functions of teaching and informed reflection).

All in all, the 1969 survey of historians in elite institutions divides the profession's leading members into four categories:

1. a very small number who work essentially as social scientists, in regular contact with demographers, economists, or other fellow-specialists;
2. a small minority -- probably no more than 20 percent -- who maintain an active interest in social-science work adjacent to their own;
3. another small minority -- perhaps another 20 percent -- who vigorously reject any association of history with the social sciences;
4. a majority who maintain a polite but skeptical attentiveness to the portions of the social sciences which bear on their own work.

I see no evidence of a significant shift in the proportions since the late 1960s. Outside the elite institutions, furthermore, I believe that indifference and hostility to the social sciences is more prevalent than in the dominant departments of history. Historians, on the whole, situate themselves as uneasy neighbors of sociologists, anthropologists and other social scientists.

History and Retrospective Ethnography

The relationship to the social sciences which shows up in the survey results follows from the organization of inquiry within history. On the whole, we should expect a discipline which stresses individual mastery of a set of texts concerning a particular time-place block to have gingerly dealings with disciplines which claim to follow models, processes and relationships across time and space by means of abstract concepts, large comparisons and quantitative analyses. The old tension between generalization and particularization is only part of the story. It is not a completely accurate part of the story, either: in fact, historians generalize, too, but under a somewhat different set of constraints -- especially time-place constraints -- from most social scientists. Nor (despite some historians' conception of social scientists as mindless mimics of natural scientists) does the venerable distinction between Geisteswissenschaften and Naturwissenschaften, between disciplines in which meaning, consciousness and will have their place and disciplines in which those factors can be disregarded, capture what is at issue; plenty of social scientists make their livings by analyzing meaning, consciousness and will.

The critical incompatibility, I think, results from the historian's insistence in rooting the analysis in a body of material believed to describe a particular place and, especially, a particular time. It may be a very large body of material (for example, everything known to be in

the relevant archives), or a very large place (for example, Christendom as a whole) or a very long time (for example, the medieval era). But that rooting of analysis in a place and a time via a defined body of residues of the place and time sets off most historical work from most work in the social sciences.

One might think that anthropology would be the exception. After all, both anthropologists and historians tend to be fastidious about the particular, even when they are hoping to generalize. Anthropologists and historians frequently hold up as an ideal the form of analysis which Clifford Geertz, following Gilbert Ryle, calls "thick description": the grasping and rendering of ". . . a multiplicity of complex conceptual structures, many of them superimposed upon or knotted into one another, which are at once strange, irregular, and inexplicit . . ." (Geertz 1973: 10). In short, the interpretation of cultures.

That concern sets anthropologists and historians off from most economists, sociologists, and other social scientists. Ethnographic field work resembles the historian's archival research more than it does the sociologist's survey design or the economist's national income accounting. The Pago-Pago Principle (as Arnold Feldman once called it) unites them: Whenever some social scientist hazards a world-wide generalization about economic change or shifts in fertility patterns, reported Feldman, someone in the back row of the audience stands up and says, "But not in Pago-Pago!" That someone is likely to be an historian or an anthropologist.

On closer inspection, we can discover possible grounds for dissension between the two. Historians tend to be especially concerned about fixing human actions in time, while being less concerned -- or ambivalent -- about fixing them precisely in space. In a generalization about eighteenth-century America, an historian must be very careful to place the statement (and its documentation) before, during or after 1776; if information from

Boston is not available, however, information from Providence or Hartford may well do the job. Anthropologists, on the other hand, tend to be much attached to place, and somewhat more relaxed about fixing human actions in time. The "anthropological present" for a given village may well span a generation. Historians tend to be hesitant or hostile when it comes to the use of categories which were not part of the period's own conceptual apparatus -- for example, the application of the vocabulary of class to an era before the emergence of that vocabulary. Anthropologists quite regularly apply analytic frameworks which would be unfamiliar, incomprehensible, or even offensive, to the objects of their study: formal models of kinship, tracings of interpersonal influence, and so on. The historian's greater anxiety about situating human affairs in time could very well be the basis of serious misunderstanding and disagreement with anthropologists.

As the specialists in time, historians have more than one way of rooting their analyses in time. Let us consider only two alternatives: first, the simple attachment of each action to a particular time; second, the deliberate analysis of change over time. In the first case, we carefully situate American reactions to Britain in 1765 before or after Britain's efforts to impose the Stamp Act, and rule out evidence from after the Stamp Act repeal of 1766 as a tainted guide to American orientations in the previous year. In the second case, we purposefully reconstruct the process by which American opposition to Britain crystallized, and then developed into a revolutionary challenge. The second is more complex than the first, because it includes the first, and adds the problem of establishing causal sequences.

Historians doing both the simple and the complex rooting of analyses in time have recently turned to anthropology for ideas and approaches. The

turn has been especially visible among historians who have wanted to build a rigorous, autonomous social history, a social history which was not a simple appendage to political or intellectual history. Historians of family structure, of popular movements, of peasant life and of similar topics have reached toward anthropology for insights, methods and explanations.

The path from social history to anthropology has generally been indirect. No doubt the most important single innovation in the social history of the last few decades was the widespread adoption of one form or another of collective biography: the systematic accumulation of multiple life histories, or fragments of life histories, in order to aggregate them into a portrayal of the experience of the population as a whole. Historians of class structure have looked at the occupational lives of hundreds of people in one city or another, then compounded them into rates of occupational mobility by class of origin, by religion, by race, by national background, by locality or by some other criterion. Demographic historians have brought together multiple observations of individual persons and events from censuses or vital records, linked the records together, and then used the linked records to examine variations in fertility, mortality and nuptiality. Historians of popular movements have collected information about individual participants, connected the various scraps of evidence concerning the same individuals with each other, then drawn from the connected scraps an analysis of the movement's social composition.

In these and many other applications of collective biography, the point is to move beyond the general impression or the well-chosen example without losing the ability to talk about what happened to the population as a whole. Although the approach of collective biography is not necessarily incompatible with the usual procedures of anthropologists, its logic has

much more in common with the routines of demographers and sociologists. In itself, then, we might have expected the adoption of collective biography to draw historians away from anthropology rather than toward it.

It is the limits of collective biography as a source of satisfying explanations of social action which have often driven historians toward anthropology. Take demographic history as an example. The collective biography of vital events and population characteristics is a powerful way to rule out bad explanations. If it turns out, for example, that the chief difference between periods of rapid growth and of stagnation in the development of a particular city is the rate at which migrants come and go, then any explanation of the city's growth and stagnation in terms of the resident population's vigor is at least seriously incomplete. Yet the strength of collective biography is not in supplying alternative explanations, but in specifying what is to be explained. Historians who have specified what is to be explained via collective biography often find themselves turning to explanations stressing the immediate setting and organization of everyday life, or relying on something vaguely called "culture". That moves them back toward anthropology.

The evolution shows up clearly in the study of popular protest and collective action. Let us stick to France, because the French and francophiles have pioneered in such studies. Until early in the twentieth century, the standard French approach to popular protest and collective action was to infer the attitudes of ordinary people -- "the mob" to authors on the right, "the people" to authors on the left -- from general principles or from the pronouncements of spokespeople, self-appointed or otherwise, of ordinary people. The attitudes then provided the explanations of collective action. Michelet, despite his greater enthusiasm for The People, was no more sophisticated than Taine in this regard.

The socialist historians who began to thrive toward World War I (Jean Jaurès and Albert Mathiez are examples) added substance to the analysis of popular movements, but still worked mainly from the top down. History from below became a general and influential model for the study of popular protest and collective action with the work of Georges Lefebvre from the 1920s onward; Lefebvre's Paysans du Nord made it clear that the materials existed for a rich portrayal of routine social life and of ordinary people in something like their own terms, and for the linking of that portrayal with general accounts of the French Revolution and other major political changes. In the 1950s, collective biography stricto sensu entered the scene with Albert Soboul's reconstruction of the life and composition of Parisian working-class neighborhoods during the early Revolution; Richard Cobb's treatment of the revolutionary militias, George Rudé's analyses of the participants in major revolutionary journées, and many other studies along the same line cemented the joint between collective biography and French revolutionary history.

Yet these authors and their successors soon discovered the limits of collective biography: collective biography told them who was there and something about how those who were there behaved, but collective biography did not in itself provide compelling explanations of the behavior. In the 1960s and 1970s the successors turned increasingly to anthropology as a source of explanations, insights and methods. Two broadly anthropological styles of work became prominent in the study of popular protest and collective action. The first was the close analysis of the cultural materials used or produced by historical actors: songs, sayings, iconography, forms of retribution, and so on. The second we might call "retrospective ethnography", the effort to reconstitute a round of life from the best historical equivalents of the ethnographer's observations, then to use the reconstituted round of life as a

context for the explanation of collective action. In America, Natalie Zemon Davis' sensitive portrayals of sixteenth-century French conflicts illustrate that effort to give an anthropological tone to historical analysis. In France itself, Maurice Agulhon's treatments of nineteenth-century sociality and symbolism illustrate the richest outcomes of the anthropological approach.

In almost none of this work was the influence of academic anthropology very formal or very intrusive. The work nevertheless deserves to be called anthropological because, as compared with previous historical work, it stresses the reconstruction of a round of life and a body of meanings from the perspective of a participant observer on the ground. It also relies on the borrowing of insights from other ethnographies, both historical and contemporary.

Instead of employing retrospective ethnography and the sustained analysis of symbolic structures as a means to the explanation of collective action, a number of French historians have taken them up as worthy enterprises in their own right. The lives of peasants and artisans, in particular, have come in for anthropological scrutiny. Some of the inspiration flowed directly from Fernand Braudel's program of Total History. One of the most impressive and influential examples is Emmanuel Le Roy Ladurie's vast portrait of the peasants of Languedoc from the fourteenth through the eighteenth centuries. It follows the program of Total History in synthesizing observations on climate, land forms, demographic changes, prices, agricultural technology, religious beliefs, popular movements and power structures. It follows the lead of collective biography in building much of the analysis on a massive parcel-by-parcel reconstruction of the uses and ownership of the land over the centuries. The resulting organization of the book is powerfully two-dimensional. The collective biography of the land provides the first dimension, the fluctuations of prices, production and population the second.

In the squares of the two-dimensional grid Le Roy Ladurie inserts his retrospective ethnography. One stunning example is his reconstruction of the 1580 Mardi Gras festivities in Romans, a small city near the Rhône south of Lyon. There, in a time of famine, artisans and peasants "danced their revolt in the streets of the city" before putting it into operation. Jean Serve, a popular local leader, donned a bearskin, placed himself on the consular throne, declared price controls, and led a series of bizarre ceremonial denunciations of the rich of Romans. The events have come to be known as the Carnival of Romans. The rich struck back, murdering Serve and many of his companions. "Thus ended the Carnival of Romans," writes Le Roy Ladurie, "a failed attempt to invert the social order: everything was put back in its proper place, and the dominant classes, at bay for a while, landed back on their feet. To confirm that return to good order, the judges had the effigy of Jean Serve, the rebel chief, hanged upside down, feet in the air and head down" (Le Roy Ladurie 1966: I, 397). Small wonder that Le Roy Ladurie's reconstruction of the Carnival gave rise to a much-watched television dramatization. His analysis exemplifies the application of Geertz' thick description to the distant past.

A number of French historians have followed Le Roy Ladurie's lead, and others have arrived more or less independently at the same project of integrating ethnography into history. Eugen Weber's widely-praised Peasants into Frenchmen uses the local chroniclers, commentators and folklorists of the nineteenth and twentieth centuries as proxy ethnographers. Michel Vovelle and Yves Castan have undertaken the close inspection of routine written materials and iconography for their symbolic content, and for the light they shed on the systems of meanings within which people lived out their lives. Many other varieties of a broadly anthropological approach to historical subject matter have appeared in the last decade. Much of

that work has been initiated, inspired, publicized or actually done by historians closely associated with the journal Annales.

Let us consider just two samples of first-retrospective ethnography which have come from the milieu of the Annales. The first is André Burguière's Bretons de Plouzévet, the second Emmanuel Le Roy Ladurie's Montaillou, village occitan. In different ways, both books illustrate the strengths and the limits of the recent alliance between history and anthropology.

André Burguière received one of the most flattering and challenging assignments a historian has received in some time. In 1962, a team of geneticists, anthropologists, demographers, sociologists and other observers had descended on a Breton village. The village was Plouzévet: the famous Plodémet of Edgar Morin's Commune en France. It had about 3,800 inhabitants. The group had fixed on Plouzévet, among other reasons, because the recurrence of a genetically-based deformity (a displaced hip) suggested an endogamous genetic isolate. Originally, the team had excluded history and historians from the inquiry. As the project wore on, they recruited the historian Burguière to write the general report of their findings. Bretons de Plouzévet is the result.

Burguière's assignment had three parts: first, to write the history of the research project; second, to sum up and (where possible) to integrate the project's diverse findings; third, to write the history of Plouzévet as a context for interpretation of the findings. He found it easier to do the third than the second, easier to do the second than the first. The book he produced is full of valuable juxtapositions and insights. For example, we learn something important about the constant creation and re-creation of "tradition" in discovering that the great decorative coiffes worn on the heads of Breton women were essentially a product of the later nineteenth

century. Burguière raises important doubts as to whether the village as such played, or plays, a fundamental role in local endogamy or, by extension, in a variety of other social relations.

But the point here is not to review the inquiry's varied results. The important thing for present purposes is the difficulty Burguière had in devising an analytic framework which would be at once adequate to the subject matter, consistent with the objectives of the non-historians on the project, and faithful to his historical calling. Burguière devotes some thoughtful pages to that confrontation. He points out the problem of integrating an inquiry which began oriented to the idea that the ultimate and constraining reality was individual and biological, which soon brought in researchers who were convinced that social structures had their own histories and consequences, and which fixed its attention on those aspects of social reality which could be observed and measured directly. Burguière searched for an all-encompassing temporal framework, but finally settled for an old, effective historical device: he organized his account around the vicissitudes of the political elite, and especially around the fate of a single, influential family, the Le Bails. Thus in order to integrate his retrospective ethnography he had to reach outside the ethnographic framework.

Emmanuel Le Roy Ladurie's Montaillou remains more completely within the confines of retrospective ethnography, at the cost of ending up without a general analytic framework. Lest those words sound deprecating, let me say at once that the book is a joy and a revelation. Montaillou, a small village in the Pyrenees, was a hotbed of heresy in the late thirteenth century, and the object of a searching inquiry by the Inquisition in the 1320s. The inquisitor, the clever and persistent bishop Jacques Fournier, left behind a transcript of his inquest which is full of direct quotations from his interviews with the villagers.

What a source! Le Roy Ladurie treats it as a voluminous set of ethnographic field notes. He adopts a simple and relatively conventional outline for the report of findings: "ecology" (that is, social geography), then "archeology" (that is, social relations). Within the two major sections, we find chapters on standard ethnographic topics: sexuality, courtship, marriage, life-cycles, gathering places, forms of solidarity, and so on. Le Roy Ladurie brings the material into brilliant light by embedding chunks of the transcript in his text, by ingenious portrayals of the village's principal characters (including the sexual adventures of the local priest, Pierre Clergue), by punctuating the description with unexpected but often revelatory references to distant times and cultures, by an agile play of hypothesis, inference and speculation. The result may well be our most comprehensive account of the daily life of a medieval village. Le Roy Ladurie gives the lie to the historians' frequent complaint that their sources do not permit them to reconstruct the vulgar details of everyday existence.

The works of Le Roy Ladurie and of Burguiere give us enviable models for the integration of historical and anthropological concerns. Yet they do not really illustrate the convergence of history and anthropology. The discipline of anthropology is far broader than ethnography. Indeed, important segments of the profession consider the standard forms of participant observation to be relics of the past. Much of the current action in anthropology concerns the formal analysis of symbolic structures, the humanization of biology and ecology, the development of evolutionary models, the rigorous treatment of kinship, demography and household structures. But they are for the most part alternatives to ethnography, not additions to it. The portion of anthropology with which French and francophile historians have worked most effectively is only a small part of the field, and in some regards a backwater.

Furthermore, the influence of historical work on anthropological practice has been slight. Few anthropologists know much history, fewer know much about historical research, and fewer still employ the historian's models, materials or insights in their own work. The flow of influence between anthropology and history, as practicing disciplines, has been largely one-way. Under these circumstances, to speak of convergence between the fields is an exaggeration.

"Social Science History"

Yet something called "social science history" has arisen. There is even a journal by the name of Social Science History, in addition to journals of economic history, demographic history, social history and the like. How is that possible? The topics of articles in the first volume of Social Science History give an idea:

"The Institutional Context of Crossfiling"

"Urbanization, Industrialization and Crime in Imperial Germany"

"The Evolution of Public Perceptions of Adenauer as a Historic Leader"

"The Congressional Game: A Prospectus"

"Sampling for a Study of the Population and Land Use of Detroit in 1880-1885"

"The Social Functions of Voluntary Associations in a Nineteenth-Century American Town"

"Town and Country in Nineteenth-Century Germany: A Review of Urban-Rural Differentials in Demographic Behavior"

"Black Yellow Fever Immunities, Innate and Acquired, as Revealed in the American South"

"The Growth of English Agricultural Productivity in the Seventeenth Century"

"The Changing Context of British Politics in the 1880s: The Reform Acts and the Formation of the Liberal Unionist Party"

This incomplete list shows the variety of topics which crowd in under the name of social science history: elections, public opinion, legislators,

urban structure, fertility, disease, and so on. The list does not show the unusual features of the style and contents: full of tables and graphs, frequently summarizing results or hypotheses as equations, self-conscious about techniques of analysis, speaking frequently of models, hypotheses and problems of measurement, obsessed by comparisons over time and over space. These are the stigmata of social science history. And social science history is flourishing.

Social science history is flourishing for two main reasons: 1) a number of social scientists have become interested in working seriously with historical materials; some of the leaders in American social science history are actually based in departments of political science, sociology and economics; 2) a few special fields of history have invested heavily in social-science approaches to their problems and their evidence. A small proportion of a large discipline, augmented by outsiders, is enough people to create and sustain the institutional apparatus of a sub-discipline. Of the 15 to 20 thousand professional historians in the United States, perhaps a thousand consider themselves to be practitioners of social science history.

The subdiscipline of social science history is unusual. It is one of the few specialties in history not to be defined by a time, a place and an aspect of social life. Although they come disproportionately from the fields which are otherwise known as social, economic and political history, the topics which comprise social science history do not form a logically coherent block. Historians have not previously considered most of them to belong together. Nor are they/^{simply}the topics which come, in principle, closest to the preoccupations of the adjacent social sciences. The spread of social-science practice has not even followed a principle of adjacency within history; separate geysers of social science history have erupted through plains of conventional historical practice.

The subdiscipline has other peculiar features. The common literature to which its members are oriented is rather thin, and mainly methodological. Since no single, coherent social science exists, the historians involved attend to different literatures within the social sciences, depending on the special historical topics which concern them. Almost all the historians in the discipline have dual or triple allegiances, for in addition to being devoted to social science history as such, they work in specific time-place fields, and often seek to make contributions to the social science disciplines -- economics, anthropology, demography, and so on -- with which they are most closely associated.

People trained outside of history commonly play large roles in social science history. Technical innovations frequently come from outside the subdiscipline; new ways of storing evidence, new statistical techniques, new models often migrate in from nonhistorical work in the adjacent social sciences. The common ground of social science history, in the last analysis, is not substantive; instead of being committed to common problems, however defined, its members share an attitude, a relationship to the historical profession as a whole, and a small amount of technical lore.

If this shaky common ground were the whole of social science history, one could readily understand the suspicion which greets it elsewhere in history, and easily predict its rapid disappearance. What gives social science history its strength, however, is that it is composed of a number of smaller clusters, each of which does share problems, materials and procedures. As a practical approximation of these clusters, we might take the topics officially represented on the program committee of the 1979 meeting of the Social Science History Association: Theory; Methods and Teaching of Social Science History; Labor History; Social Structure and Mobility; Family History; Ethnicity; Urban History; History of Education;

Economic History; Demography; Electoral, Party and Legislative History; Bureaucracy; Elites; International Relations; Diplomatic History; Violence; Public Disorder; Criminal Justice; Legal History. (Among the/dozen members of the committee, incidentally, six were based in departments of history, four in departments of political science, one in a department of economics and one in a department of sociology.) The clusters are of two overlapping kinds: historical specialties which have long existed, but which in recent years have developed close working relationships with one or another of the social sciences; specialties which essentially came into being as a result of the interaction of history and one of the social sciences.

In the first category the most prominent case is economic history. During the 1960s, economic historians began adopting economic models and econometric methods as standard elements of their intellectual armamentarium; it is now hard to enter the field at all without having considerable training in economics. In the category of new specialties, the most dramatic case is demographic history. (Many of its practitioners call the field historical demography; the changed emphasis itself tells us something about the field's character; see Gaunt 1973.) Although the specialty's intellectual origins go back to the political arithmeticians of the eighteenth century, demographic history has only existed as a substantial, distinctive body of knowledge since the 1960s.

Somewhere between the cases of economic history and demographic history fall the other major enterprises of social science history: quantitative urban history, the study of social mobility, and so on. Each of these specialties has its own relationship to some portion of the social sciences, and each shares some pool of problems, materials and procedures. Each has the makings of a distinct subdiscipline.

How Do History and Social Science Coalesce?

Why these areas and not others? From a logical point of view, they are no more obvious candidates for social-scientific work than other subjects which have remained inhospitable to social science: military history, the history of science, the history of popular culture, agricultural history and biography are cases in point. In all of these fields, there exists a body of related systematic work somewhere in the social sciences, and a scholar or two have made the effort to apply the approaches of social science to the historical problem. Yet, unlike economic history or the history of the family, these fields have not moved noticeably toward the social sciences.

It is possible, in principle, that the explanation lies in the relative power of the ideas and procedures available inside and outside of history: fields whose guiding ideas are relatively weak, one might think, tend to succumb to social-scientific enchantment. I think, however, that it has more to do with the compatibility between the existing structure of the historical field and the styles of analysis which prevail in the adjacent areas of social science. The crucial question is this: will existing social scientific approaches to a given problem yield fresh and/or superior answers to the questions which historians are already asking? If the answer is yes, and if someone with sufficient credentials as an historian to attract other historians' attention demonstrates the way to fresh and/or superior conclusions, others follow quickly. Graduate students begin proposing investigations to confirm, duplicate, elaborate or refute the new conclusions. Since revised doctoral dissertations make up the bulk of the monographs published in history, the new approach has a considerable impact on the books historians are reading five or ten years later. The easier and the more general the procedures involved, the more quickly graduate students and junior scholars follow.

The study of American slavery illustrates the point very well. The efficiency and profitability of slavery in America's cotton regions before the Civil War are crucial problems because they bear directly on several fundamental questions: whether southern planters had a strong economic interest in slavery; whether the Peculiar Institution was likely to collapse of its own weight; whether the greater efficiency of northern agriculture and of free labor were further threats to the economic viability of the South; whether the Civil War was a logical outcome of the confrontation between incompatible sectional interests. These questions stirred American politicians and historians from the time of the Civil War onward. In the late 1950s, however, Alfred Conrad and John Meyer began to redefine the profitability of slavery as a question of formal economics, and began to derive estimates of that profitability from evidence on costs, prices and production in the South. Their estimates portrayed slave-powered agriculture as a relatively efficient and profitable system. That work shifted the terms of the debate, and started the stream of econometric research on slavery which eventually included the efforts of Robert Fogel, Stanley Engerman, Gavin Wright, Richard Sutch and a number of other expert economists. Although non-economists such as Eugene Genovese and Herbert Gutman continued to play important parts in the assessment of the character and consequences of American slavery, the proposal of an economic answer to an old historical question opened the way to an invasion of that part of history by economists.

The invasion resembled the great migrations of the Mongols or the Normans: although their arrival deeply transformed the social structure at their destination, eventually the newcomers and the older settlers assimilated to each other. The economists began by acting as if they were simply

going to incorporate American economic history into neo-classical economics, and leave nothing worthwhile for the historians to do. Eventually, however, the economists began to respond to the peculiarities of the American nineteenth century, even to interest themselves in the historical problems posed by that time and place. At the same time, historians began to learn the strengths and weaknesses of econometric analysis, even on occasion to learn how to do it. As Eugene Genovese once put it:

. . . the finest products of the new school have transformed themselves from economists who work on data from the past into economic historians in the full sense -- into historians who are primarily concerned with economic processes within larger social processes and who therefore struggle to define the extent to which economic processes are autonomous and the ways in which they are contingent. The better traditional historians, analogously, did not deny a degree of autonomy to the economic sector and did not reject the new methods; they tried to take full account of the new work while reevaluating the relationship between economic behavior and social behavior as a whole (Genovese 1975: 533).

By 1978 -- twenty years after Conrad and Meyer -- Gavin Wright was prefacing an important econometric study of the Cotton South with the declaration that the fruits of econometric economic history "have frequently been valuable and stimulating, but I now believe that it is a mistake for economic history to define itself merely as economics applied to old data. Instead, economic history offers a distinctive intellectual approach to the study of economics, a view of the economic world in which historical time plays a fundamental role" (Wright 1978: xiii).

American economic history is in no sense reverting to the status quo ante. Any historian who now wants to be heard on the viability of slavery or any number of other topics in nineteenth-century history has to be familiar with the econometric work on the subject, and may well have to undertake some econometric analysis of his own. The basic training in the field now includes a substantial amount of economics; indeed many of the new people in the field are getting their training in departments of economics. But four further changes have taken the field past the point at which it seemed that economic history might simply vanish into economics:

1. The economists began to act as if the time and place -- the historical setting -- significantly constrained the operation of economic processes which had previously appeared to be timeless and universal.
2. The economists began to respond to the questions historians in general were asking about the time and place.
3. The historians became sufficiently familiar with the procedures, products and pitfalls of econometric work that they could assimilate and criticize its results.
4. Historians and economists alike began to identify problems that were crucial, but not easily handled by the available economics.

In the process, as Genovese says, a distinct specialty of economic history -- neither strictly economics nor strictly history -- began to form.

The changing historiography of slavery provides a paradigm for the diffusion of social scientific approaches into historical inquiry. Similar, less complete, transformations have occurred in the historical study of family structure, cities, social class and a number of other topics.

That highly selective coalescence of portions of history with segments of the social sciences accounts for the curious structure of social science history as a whole: instead of being the edge of the social sciences as a whole with history as a whole, it is a collection of many different edges.

Still, the social science historians have the common ground of prisoners of war: the common ground which results from originating in one broad tradition, and being confronted with another. On the one side, there is the historical tradition, with its rooting of analysis in a time and a place by means of a defined set of products, mostly texts, of that time and place. On the other side, the social scientific tradition, with its distinguishing features: explicit conceptualization and modeling of the phenomena under study; a strong emphasis on measurement; the deliberate use of comparison, often quantitative comparison, to establish the strength and direction of important relationships. The attempt to reconcile these two traditions gives social science history a certain methodological unity.

The subdiscipline also bears a paradoxical strain of populism. Paradoxical, because other historians often resist the numbers and abstractions of the social sciences on the ground that they are inhumane. Yet in field after field the appeal of social-scientific approaches has been that they facilitate the bringing of ordinary people back into the historical record, permit the historian to rescue them from abstraction and to gain a sense of the day-to-day conditions of their lives. Ordinary people leave few diaries, letters and novels, but their experiences leave documentary evidence nonetheless. The documentary evidence shows up in birth certificates, marriage contracts, notarized transactions, conscription registers, tax rolls, rent books, censuses, catechetical records and other routine sources. One

of the greatest contributions of the social sciences to historical practice has been to suggest means of combining the fugitive mentions of individuals in such sources into biographies -- individual biographies, and collective ones as well.

The most obvious example of that populist use of collective biography is one we have already discussed: the systematic study of political militants and revolutionary crowds. In the 1950s, Albert Soboul, Kåre Tønnesson, Richard Cobb, George Rudé and other students of revolutionary France followed the lead of Georges Lefebvre in attempting exhaustive enumerations and descriptions of different important groups of activists. Their quantitative work was very simple and not very extensive, but it demonstrated the existence of abundant evidence concerning ordinary participants in the Revolution. Although entirely non-quantitative, the rich essays of E.P. Thompson and E.J. Hobsbawm on the lives of workers likewise displayed the promise of history "from the bottom up". It did not take social scientists long to see that the resulting redefinition of the historical agenda gave them an opportunity to apply their own skills to the available evidence. A segment of social science history devoted to the study of crowds, militants and ordinary workers grew up.

The growth of demographic history was in some ways contrary to that of crowd studies, yet it produced a similar result. While the urge to study crowds originated within history, the historical study of vital processes grew very largely from the concerns of demographers. French demographer Louis Henry, in particular, sought to pinpoint the conditions under which deliberate fertility limitation became part of a way of life. The search for the origins of unreversed declines in fertility has long been one of

demography's dominant preoccupations. Henry's pivotal insight was to realize that the same sorts of materials that antiquarians used for the construction of genealogies would, with great care, yield fine measures of fertility, mortality and nuptiality. He and his collaborators developed a form of collective biography -- "family reconstitution" -- using the registers of births, deaths and marriages the Catholic Church had established for its parishioners. The method yielded important results, including indications of much greater variability in pre-industrial fertility than had previously been thought to be the case. Other research groups elsewhere (notably the group working with economic historian E.A. Wrigley and intellectual historian Peter Laslett at Cambridge University) took up similar inquiries. The early agenda was largely demographic; it was, in essence, an effort to modify and refine the theory of demographic transition.

The crossover into history occurred when Wrigley, Pierre Goubert and other economic historians began to interpret fluctuating vital rates as indicators of welfare, and to examine the covariation of demographic fluctuations with swings in the economy. Goubert, for example, traced the devastating effect of periodic food shortages on the death rate in parishes of the Beauvais region, as well as the remarkable recuperation of fertility once the crisis was past. That line of analysis articulated neatly with the already-established interpretation of French economic history as a series of well-defined cycles. In France and elsewhere, the inquiry broadened from there: some investigators refined the study of demographic processes, others worked at bringing other routinely-produced documents into the analysis of everyday experience, still others concentrated on the connections between demographic processes and their economic context. By this time, formal demography, economic modeling and statistical analysis

were becoming commonplaces in this particular branch of historical research. A new variety of social science history was emerging.

Is Quantification the Essence?

In field after field, the leading edge of the change was some form of quantification. Because of that uniformity, many non-quantitative historians mistook the prow for the whole ship: they thought that quantification was the essence of the new movement, that its proper name was "quantitative history", that its practitioners claimed everything could and should be counted. The advocates themselves compounded the misunderstanding. They delighted in showing how much historical reasoning which appears in non-numerical prose is nonetheless crudely quantitative: more or less, growing or contracting, crisis or continuity recur throughout historical writing. Each of them has an implicitly quantitative content (cf. Fogel 1975). Such arguments invite deliberate quantification.

The point is important, for it provides the demonstration that the quantifiers are not simply amusing (or abusing) themselves, but pursuing significant questions which are already on the historical agenda. Yet the argument is misleading, for two reasons:

1. available quantitative models and statistical techniques are inadequate to deal with many of the more-or-less statements which do, indeed, abound in historical argument;
2. quantification is only the most visible piece of a much larger analytical apparatus -- an apparatus of deliberate conceptualization, explicit modeling, painstaking measurement and self-conscious comparison.

The defense of quantification therefore both oversells and understates the likely impact of social-scientific approaches on historical practice.

Partly because of the inevitable discrepancy between early claims and later realities, leaders of the movement toward social science have recently taken to writing disclaimers. The disclaimers commonly say, in effect, "I never promised you a rose garden." In 1975, we find Lawrence Stone, one of the pioneers of quantification in English history, portraying most of the social sciences as treacherous allies on their way to internal collapse. He deplores the heedless adoption of quantification, especially as the core of large-scale research projects and specialized graduate programs. He castigates the excesses of psychohistory. And he criticizes the tendency to apply simple, one-way, causal explanations to the complexities of history.

"The basic objection to these threats to the historical profession," declares Stone, "is that they all tend to reduce the study of man, and the explanation of change, to a simplistic, mechanistic determinism based on some preconceived theoretical notion of universal applicability, regardless of time and space, and allegedly verified by scientific laws and scientific methods" (Stone 1977: 38). "It may be," he continues,

that the time has come for the historian to reassert the importance of the concrete, the particular and the circumstantial, as well as the general theoretical model and the procedural insight; to be more wary of quantification for the sake of quantification; to be suspicious of vast cooperative projects of staggering cost; to stress the critical importance of a strict scrutiny of the reliability of sources; to be passionately determined to combine both quantitative and qualitative data and methods as the only reliable way even to approach truth about so odd and unpredictable and irrational a creature as man; and to display a becoming modesty about the validity of our discoveries in this most difficult of disciplines" (Stone 1977: 39).

Veterans of revival meetings will immediately recognize this passage as a deployment of the "Sinner, Beware!" technique: the preacher fixes his gaze over the congregation's head, points a prophetic finger, and forecasts doom for unrepentant sinners. He names no names, and the sins in question appear as ominous labels -- lust, greed, gluttony -- rather than concrete actions. Most of the congregation receive the double thrill of self-satisfaction and righteous indignation, a few thin-skinned souls feel guilty, and the emptiness of the condemnation passes unnoticed. No reader, after all, is likely to cheer "quantification for the sake of quantification", much less "projects of staggering cost". The social scientists and historians who are the objects of these complaints are likely to reply, hurt and puzzled, "Who, me?" Few readers will dare deny the importance of the concrete, the value of strict scrutiny of the sources, the attractiveness of modesty, and so on. Yet Stone's sermon is a disservice to historians. It is a disservice because it misrepresents how the interaction between history and the social sciences has usually worked itself out, and misstates the choices now before the profession. The critical choice, indeed, is one I have barely mentioned: whether to help the social scientists make proper use of historical materials and historical analysis.

Sociology Reaches for History

The choice is more critical today because several social science disciplines which had long operated far from history -- notably anthropology, sociology and political science -- have recently reached out to reestablish their historical connections. Let us focus on sociology. The discipline of sociology grew out of history, via large schemes designed to place all historical experience into coherent master sequences. Auguste Comte's Theological, Metaphysical and Positive stages of thought and Herbert Spencer's grand evolution of human societies were simply two of the most

prominent among many such schemes. Since Comte coined the term sociology and Spencer gave it wide currency, however, the two schemes helped define the infant discipline. Quickly the historical content drained out of sociology in favor of an effort to create a timeless natural science of society. Although Max Weber and some of his successors were zealous historical practitioners, on the whole twentieth-century sociologists committed themselves to the study of the present; they showed less and less inclination to consider history important, either as a set of influences on contemporary social processes or as a field of inquiry worthy of sociological attention.

Yet in the 1960s and, especially, in the 1970s, sociologists did begin to reach for history. Historical analyses of industrialization, of rebellion, of family structure began to appear in the journals that sociologists read. Departments of sociology began hiring specialists in something called "historical and comparative analysis". Sociological authors began to write as if when something happened seriously affected how it happened. Some few sociologists actually began to learn the basic historical skills: archival exploration, textual analysis and the like. History began to matter.

What happened? Among many strands, I see two as strongest. First, the social scientific work which had been proceeding in history doubled back on the social sciences. The successes of historical demography provided a model for contemporary students of marriage and the family as well as for other demographers. Historical studies of crime, of voting, of urban structure, of social mobility were sufficiently fruitful and/or provocative with respect to prevailing sociological doctrines that sociologists started to think of them as more than mere tours de force. Second (and more important), disillusion with models of modernization and development turned students of large-scale social change toward history.

The disillusion with developmental theories followed a decade or two of enthusiasm after World War II. During the palmy days of developmentalism, western economists hoped to export the secrets of economic growth to the "underdeveloped" world, and sociologists imagined other forms of development -- political, social, educational, urban, and so on -- to accompany the economic growth. The reaction against developmental theories had several different origins. Development of any sort proved difficult to engineer: capital accumulation, family planning, land reform and other desiderata of development turned out to meet more powerful resistance, and to have more extensive political ramifications, than optimistic western theories promised. The theories themselves fell on hard times: on the whole, they were inadequate to the task of explaining what was actually happening in the Third World. Their political premises -- especially the implication that western-style party politics was an inevitable, desirable concomitant of other forms of development -- excited the anger of Third World intellectuals and powerholders alike. Among other things, the standard conceptions of political development clashed with the explanation of the disadvantages of poor countries as consequences of western imperialism; that was, after all, an attractive alternative in the many former colonies that were acquiring statehood and undertaking planned national development. In the course of the widespread opposition to American warmaking in Southeast Asia during the 1960s, many social scientists in the West (including the United States) became aware of, and sympathetic to, the anti-imperial and neo-Marxist alternatives to development theories. They even began to contribute to the building of those alternative theories themselves. Developmentalism fell into disrepute.

But why and how were the alternatives to developmentalism historical? Largely because, in one way or another, they portrayed the current situation of poor countries as the outcome of a long, slow, historically specific process of conquest, exploitation and control. Thus André Gunder Frank and

other students of Latin America spoke of "underdevelopment" not as the primeval condition from which the still-poor areas of the world had to be rescued, but as a product of the dependency of their economies on those of the world's dominant powers. "[T]he expansion of the capitalist system over the past centuries," wrote Gunder Frank,

effectively and entirely penetrated even the apparently most isolated sectors of the underdeveloped world. Therefore, the economic, political, social, and cultural institutions and relations we now observe there are the products of the historical development of the capitalist system no less than are the seemingly more modern or capitalist features of the national metropolises of these underdeveloped countries. Analogously to the relations between development and underdevelopment on the international level, the contemporary underdeveloped institutions of the so-called backward or feudal domestic areas of an underdeveloped country are no less the product of the single historical process of capitalist development than are the so-called capitalist institutions of the supposedly more progressive areas (Gunder Frank 1972: 4-5).

Such an argument denied the idea of a developmental process which repeated itself over and over in different parts of the world, denied the division of the world into "traditional" and "modern" sectors, with the modern transforming the traditional into itself, denied the validity of any analysis which took a single self-contained society as its unit of analysis. All these denials moved analysts of the contemporary world closer to an explanation of the present as the outcome of an historically specific struggle for power and profit. The fact that Marx and Lenin provided the theoretical linchpins of the whole alternative system of thought further promoted the concern with history.

A prestigious example of the move toward history appears in the work of Immanuel Wallerstein. Wallerstein, an Africanist, published sympathetic studies of decolonization: Africa: The Politics of Independence, The Road to Independence: Ghana and the Ivory Coast, and others. As of 1966, he was arguing that

the imposition of colonial administration created new social structures which took on with time increasing importance in the lives of all those living in them. The rulers of the colonial system, as those of all social systems, engaged in various practices for their own survival and fulfillment which simultaneously resulted in creating movements which in the long run undermined the system. In the case of the colonial situation, what emerged as a consequence of the social change wrought by the administration was a nationalist movement which eventually led a revolution and obtained independence (Wallerstein 1966: 7).

In his arguments of the time, history's rôle was limited: in any particular colony, the past practices of the colonizers accounted for the current political situation. Later, Wallerstein came to see the entire sequence of colonization, exploitation and decolonization as part of a single historical process: the incorporation of peripheral areas into the expanding capitalist world-system.

Wallerstein tells us that he first explored western history in a search for parallels with the African experience, in hopes of identifying a standard process of modernization. But the difficulties of drawing boundaries around the societies in question, of identifying the stages in their development and of making meaningful comparisons of seventeenth-century with twentieth-century states eventually came to seem more than technical problems to overcome; they grew into fundamental objections to the enterprise.

"It was at this point," writes Wallerstein, "that I abandoned the idea altogether of taking either the sovereign state or that vaguer concept, the national society, as the unit of analysis. I decided that neither one was a social system and that one could only speak of social change in social systems. The only social system in this scheme was the world-system" (Wallerstein 1974: 7). By this path he arrived at a deeply historical conception of the problem, in which what happened before made all the difference to what happened next. That new conception drew the onetime Africanist back to a general study of the origins of the capitalist world-system in the European sixteenth century.

Since the time of Rosa Luxemburg and Nikolai Bukharin, the idea of a capitalist world-economy has been a standard tool of Marxist analysis (see Palloix 1971). Gunder Frank's idea of the "development of underdevelopment" falls squarely into the tradition. Easter European historians such as Marion Malowist have long used a similar set of ideas to explain the connections between the commercial capitalism of northwestern Europe and the agrarian economies of the East during the fifteenth and sixteenth centuries. By virtually dissolving the national economy into the world-system, Wallerstein simply takes an extreme Luxemburgian position. Nor does he undertake original archival research to establish his position; The Modern World-System summarizes the writings of many other historians. Wallerstein's special contribution is to propose a synthesis -- a synthesis between a well-known line of thought about the capitalist world-economy and Fernand Braudel's bold treatment of the entire Mediterranean during the formative years of European capitalism as a single, interdependent system. (This conjunction makes it less surprising that the enthusiastic comments on the book's dust-jacket came from Fernand Braudel, Eric Wolf and André Gunder Frank.) He then sets out to write the long-lacking narrative of the world-economy's historical development. In his swing from single-country studies of

political modernization to world-wide studies of capitalism's development, Wallerstein epitomizes the substitution of historical analysis for the developmentalism of the 1950s and 1960s.

Wallerstein's world-system analysis keeps to the enormous scale of the developmental schemes it is meant to replace. He aspires to stuff the whole of human history since 1500 into a single sack. Except when writing textbooks or end-of-career reflections, professional historians almost never work at that scale. Most other sociologists who have taken up historical analyses in recent years have also chosen a smaller scope than Wallerstein. Comparative history has been an important choice; S.N. Eisenstadt's The Political Systems of Empires has served as one sort of model, Barrington Moore's Social Origins of Dictatorship and Democracy as another. Those are formidable models for emulation, but talented newcomers have met the challenge; Theda Skocpol's searching comparison of the French, Russian and Chinese Revolutions, in States and Social Revolutions, is a case in point. Other sociologists have turned down the scale yet another notch or two: Michael Hechter on internal colonialism in Britain, Daniel Chirot on the politics of Romania, Michael Schwartz on a single important farmer's movement in the American South, and so on down to the level of a single community. Some of America's best sociological talent is going into historical studies.

The movement has caught on, and is likely to be around for some time. Elsewhere in sociology, historical approaches to crime, collective action, power structures, occupational differentiation and a host of other topics are becoming commonplace. The sociologists in question are not turning into historians: as a rule, they are not learning to do archival research; nor are they taking their questions from the prevailing historical agenda, or suppressing their inclinations to explicit modeling, careful measurement and deliberate comparison. They are, on the other hand, edging toward the

adoption of genuinely historical arguments: arguments in which where and, especially, when something happens seriously affects its character and outcome. The result, I predict, will not be a general rapprochement of sociology and history, but a counterpart to the earlier development of separate social-scientific specialties within history: a highly selective shift of particular topics to historical analyses and historical materials.

Selective or not, the shift is important. It is enlarging the place of historically-grounded theories, and challenging the place of theories which disregard time, in sociology: the development of capitalism instead of modernization, the growth of an international state-system instead of political development. It is expanding the opportunities to formulate and test models of long-term change on reliable evidence concerning substantial blocks of time instead of the sham comparison of presumably "backward" and "advanced" areas at the same point in time. And it is increasing the number of sociologists who, instead of treating the works of historians as if they were raw but solid evidence simply awaiting a sociological gloss, detect what is problematic in existing historical interpretations, and know how to go about correcting them. Even if social science history, within history, is reaching a plateau, historical work within sociology is continuing to grow.

Historical Analyses of Structural Change and Collective Action

Two areas of sociological analysis which stand to gain significantly from the swing toward history are studies of large-scale structural change and of collective action. The search for timeless general models of industrialization, rationalization or political development will yield to twin efforts to identify the master change processes in particular historical eras and to connect specific transformations occurring in those eras to the master processes of change. The attempt to formulate general laws of revolution, of social movements or of worker organization will give way to

a quest for regularities in the collective action of particular historical eras.

For our own era, the two master processes are no doubt the expansion of capitalism and the growth of national states and systems of states. The expansion of capitalism combined the accumulation of capital with proletarianization of producers; increasingly workers with little or no capital sold their labor power to people who controlled substantial capital, and who decided how the capital and labor would be combined for their profit. From a small European base, the capitalists extended their decision-making power to the entire world. Wallerstein's The Modern World-System sums up one major interpretation of how that process worked, but there are others, notably the idea that capitalism was a sort of invention which worked so well that one country after another adopted it. The historical problem is, then, to determine why and how capital accumulation-cum-proletarianization occurred, why and how the system of productive relations expanded, and what were the consequences of that expansion. Time is of the essence, historical analysis indispensable to the enterprise. Yet there remains room for the classic problems which have concerned students of "modernization": why, how and with what effects production moved into large, capital-intensive organizations; what caused the industrial city to come into being; what happened to the peasantry, and so on. All these follow easily from the historical analysis of capitalism's development.

As counterpoint to that analysis, we have the growth of national states and systems of states. An organization is a state, let us say, in so far as a) it controls the principal organized means of coercion in some territory; b) that territory is large and contiguous; c) the organization is differentiated from other organizations operating in the same territory; d) it is autonomous; e) it is centralized; and f) its divisions are formally coordinated with each other. In that sense of the word, states were rare

phenomena anywhere in the world before a few hundred years ago. Yet by the twentieth century states had become the dominant organizations almost everywhere in the world. What is more, states struggled with each other, borrowed each other's organizational innovations, formed hierarchies and interdependent clusters, worked collectively at creating new states, containing old states, and realigning the weaker states to meet the interests of the stronger. In short, not only states but systems of states came to dominate the world.

Again the historical analysis begins with the Europe of the Renaissance, fragmented into hundreds of nominally autonomous political units, none of them resembling a twentieth-century national state. For convenience, without insisting stubbornly on the distinction, we can distinguish between the internal and the external history of statemaking: how particular organizations grew up which asserted dominance over their "own" populations, how those organizations established their power with respect to competing organizations outside. Warming then becomes crucial on both sides of the divide: internally, as the activity which drove the statemakers to tax, conscript, commandeer and disarm a subject population, and thus build up their coercive power; externally, as the primary means by which statemakers established their exclusive rights within their own areas, expanded those areas, and reshaped the form, personnel and policies of other states. How states acquired control over education, welfare, marriage, natural resources, economic activity poses the next round of questions. We move easily to the examination of the central problems of contemporary political sociology: to what extent and how the economically dominant classes control the political apparatus as well; under what conditions a national population is active, organized and informed with respect to

national politics; how riots, rebellions and revolutions occur, and so forth. But we take up the problems with a difference. We take up the analysis of power, of participation, of rebellion as historical problems, ultimately attaching them to the expansion of capitalism and the growth of systems of national states.

Capitalism and statemaking provide the context for an historically-grounded analysis of collective action: of the ways that people act together in pursuit of shared interests. Grounding the analysis historically again means fleeing universal categories. Instead of the eternal behavior of crowds, the particular forms of action people use to advance claims or register grievances. Instead of laws of social movements, the emergence of the social movement as a political phenomenon. Instead of power in general, the modalities of power within a certain mode of production. Capitalism and statemaking provide another sort of grounding as well. For their rhythms and directions dominated the changes in collective action's three fundamental components: the interests around which people were prepared to organize and act; their capacity to act on those interests; and the opportunity to defend or advance those interests collectively. Concretely, we find ourselves examining how and why strikes became standard vehicles for labor-management struggles, the ways in which the expanding intervention of states in everyday life (by taxing, drafting, regulating or seizing control of crops) excited resistance from peasants and artisans, the conditions under which patron-client networks lost their political effectiveness, and similar problems. These problems are, to my mind, sufficiently broad and important to compensate sociologists for the fall from timeless universalism their pursuit entails. And they have the additional compensation

of bringing the sociologist into the rich historical residues of everyday social life. The sorts of residues, for example, that we encountered at the start of the discussion, in the Mercure françois.

Let us return to the Mercure, to see where a program of historical analysis leads us. Now we can reverse the angle of our approach. Earlier we looked at a text, and asked what it could tell us about the era. Now we are in a position to ask how the evidence in the text bears on the analysis of capitalism, statemaking and collective action. Properly read, the Mercure fairly bursts with relevant evidence.

In 1615, Louis XIII (son and successor of the assassinated Henry IV) was fourteen years old; his mother, Marie de Medici, was regent. Louis and Marie faced three linked challenges from within his turbulent kingdom. The great sovereign courts, especially the Parlement of Paris, were trying to consolidate their own autonomy by such means as guaranteeing the heredity of offices, and to extend their power to review and veto royal actions. The king's close kin and rival princes, including the Prince of Condé, alternated between grudging acquiescence and armed rebellion. Protestant consistories in Guyenne were organizing to resist by force the very Catholic marriages of the king to a Spanish princess and of his sister to the Spanish crown prince. The resistance of the courts deprived the king of their sanction for new taxes with which to pay the troops required to put down the rebellions. The king and the Queen Mother turned to cruel old expedients, such as expelling all practicing Jews and confiscating their property. Meanwhile, the rebellious princes faced a parallel problem: how to squeeze the wherewithal for expensive armies from a reluctant population, without driving the population itself into rebellion against them. On the 22d of October 1615, the army of the princes

went to lodge themselves at the little city of Espoungny, two leagues from Auxerre. The inhabitants wanted to hold them off, but the city was forced and pillaged. People have written that rape and violence, more than barbarous, took place, in the church as well as elsewhere. Complaints and murmurs reached all the way to the Prince and to the Duke of Mayenne. They had two soldiers, accused of rape and violence, hanged (Mercure françois 1615: 260).

When they had to (which was often), the princes let the troops wrest their food, lodging, arms and sexual satisfaction from the local population; when the exactions threatened to turn the locals into rebels, the military commanders checked their troops by means of exemplary punishment. When they could, the princes established a more regular system of taxation, parallel to that of the king. As the Mercure's writer commented,

It is very hard on the poor peasants to be trampled by the military, and to pay a double taille as well; they were obliged to do so by the revenue offices set up by the Princes in the provinces of Picardy, Ile de France, Champagne, Auxerrois, Berry, Touraine and Anjou below the Loire. The offices sent their garrisons to seize the richest peasants, and held them prisoner until they had paid not their own share of the taille, but that of the entire village, which they were then supposed to collect from the others (Mercure 1615: 305-306).

That technique, the princes had learned directly from the crown's own tax officers.

Now, it would take a great many more texts to reconstruct the changes going on in the France of 1615. In context, however, these two are enough to identify an unexpected convergence between the interests of capitalists and the interests of statemakers. Capitalists specialized in setting prices

on goods, land and labor, in exchanging them, and in bringing them into larger and larger markets; that is how they accumulated capital. Capitalists had a powerful interest in destroying the capacity of local people to produce for themselves, to barter goods and services, to keep land off the market. Statemakers needed resources which were embedded in local communities -- especially the food, supplies and manpower required to keep large armies going. To the extent that goods, land and labor were being exchanged via a monetized market, and thus had visible prices, it was easier for the statemakers to seize resources: they taxed the exchanges themselves, they used market-derived values to judge the capacity of people to pay, they grabbed the money people accumulated from selling their goods, and they used the tax revenues to buy food, supplies and manpower on the market instead of commandeering them directly from unwilling households. The process had its converse: the enforcement of taxation in money forced people to sell goods, services or land, and thus to expand the market.

Capitalists played facilitating roles at all levels of the process: as local merchants interested in making a profit on the sale of cattle, as purchasers of tax-collecting offices on which it was possible to make a profit, as creditors who advanced large sums to the crown in return for the rights to shares of future tax revenues, enforceable by means of the royal military power. In other regards the capitalists, too, fought the state's advance; but at these crucial points the interests of capitalists and statemakers coincided, and let to an effective coalition. A coalition which, for the most part, excluded the statemakers' rivals and victimized the subject population.

The coalition worked. "Financiers" (as they were called at the time) and royal officials succeeded in greatly expanding royal revenues, and thus made possible the building of large, stable and reliable armies which were largely independent of the great magnates, the king's rivals. Under that sort of effort, the French national budget nearly doubled, rising from about 27 million to about 50 million livres, between 1614 and 1622. The process of building a regular army occupied a full century, and the financing of the army staggered from expedient to expedient up to the Revolution of 1789. Yet the expedients worked, most of the time, and the state swelled in size and power.

The statemakers and financiers faced formidable opposition. Ordinary people resisted the rising taxes, especially when the taxes cut into the necessities of local life and when they visibly profited the local bourgeoisie. Nobles, great and small, fought the growth of a rival civil power and a threat to their own power to tax and exploit the local population. On the principle that the enemy of my enemy is my friend, the rather different interests aligning both nobles and poor commoners against the crown sometimes produced a powerful alliance. The alliance could mean a regional rebellion far fiercer than the typical noble conspiracy or the commonplace popular resistance to the tax collector. As the Mercure's commentator said back in 1605, the rebels' usual "pretext" was, indeed "to lighten the people's burden, and to make sure that those who were charged with the administration of justice would do better in the future." If he may also have been right that "their real hope was to fish in troubled water and, in the guise of the public good, to fatten themselves up at the expense of the poor people," at least we can see why the "pretext" had wide popular appeal. Popular

rebellions, many of them tied to the conspiracies of great nobles, racked the French seventeenth century. The greatest cluster of them all, the series of popular, noble and judicial struggles with the crown we call the Fronde, almost destroyed the monarchy.

With this background, it is easier to understand several puzzling features of France in the seventeenth century: 1) the extent to which popular collective action consisted of resistance to someone else's attempt to take something away -- the recurrent rebellions against taxation being the most dramatic cases; 2) the coexistence of incessant rebellion with successful statemaking; 3) the persistent, and ultimately successful, efforts of the crown to neutralize a fractious nobility via cooptation, concession and repression; 4) the curious coalitions which sometimes sprang up among Protestant zealots, Catholic nobles and nominally Catholic citizens of the towns. All of these make sense in the light of a vigorously expanding state, seconded by a growing bourgeoisie whose interests coincided temporarily with those of the state.

Consider the province of Quercy in 1623. Bypassing the previous arrangement by which the provincial Estates granted tax revenues to the crown, the king had established an Election to collect taxes directly. The officers of the Election had bought their offices, and gained their incomes from the taxes they brought in for the crown. Word spread, says the Mercure, that the region's powerful people would support a popular rising to abolish the Elections. When the new officers came to take office

A certain Douat, a Quercy native . . . about fifty-five or so (who fooled with horoscopes, was a great physiognomist, and fortune-teller, and had always said he would die in action), having gone from parish to parish secretly agitating the populace, put himself into the field at the head of five thousand men, both peasants and other good-for-nothings

who had been discharged from the armies since the peace. The specious pretext of this great rising was the establishment of the new Elections, by which they said the province would be overburdened with tailles [i.e. the basic property taxes], and with the salaries, benefits, fees for signing the rolls, and other revenues that had been assigned to the Election officers. Furthermore, that the richest people of the province, who had previously paid the heaviest taille, up to three or four hundred livres, having bought the offices for their exemption from the taille, they would push the taille onto the little people, including the pro rata surtaxes which are now due on past and present assessments (Mercure françois 1623: 473-474).

The rebels attacked the houses of the new officers. Their force grew to 16,000 men. But the military governor of Quercy attacked them near Cahors, broke them up and captured their leaders:

The next day, the 8th of June, the Marshal had Douat and Barau [a second chief] taken to Figeac for trial. The Provost sentenced Douat to have his head cut off, his body quartered, and his head impaled on a post at Figeac, and also that his four quarters would be taken to four of the principal cities of Quercy and suspended there. This was done the same day (Mercure françois 1623: 477).

Barau was hanged in his home town ten days later. Thus the Quercy rebellion ended like many others: with a few of its leaders punished spectacularly, and the fiscal power of the crown (not to mention the privileges of the bourgeois who had bought the royal offices) confirmed by military force.

Except through the presence of the profiteering bourgeoisie, the experience of Quercy in the 1620s does not trace the trajectory of expanding capitalism very clearly. It does, on the other hand, show the interplay of statemaking and popular collective action. Statemaking impinged deeply and directly on the interests of ordinary people. When they could, ordinary people resisted the threat to their interests. But time and military might were on the side of the statemakers; the people tried repeatedly, and lost repeatedly. Before long, their favored allies, the provincial nobility, had been checked as well. From that point on, such popular rebellions as occurred posed a diminishing threat to the state. In fact, as France rolled into the eighteenth century popular collective action against the state declined somewhat, and action against profiteering landlords and merchants became more prominent. While in the seventeenth century the tax rebellion and the attack on occupying troops or grasping officials had been the more visible forms of popular resistance, the eighteenth century brought food riots, occupations of disputed land and struggles against the landlord's exactions to the fore. Once we see that the food riots acted against merchants and officials who backed merchants, and that the landlords who stirred up the greatest dissension were those who bought most eagerly into the expanding cash-crop market, the shift away from statemaking to capitalism as the focus of popular collective action becomes manifest. The changes in collective action responded sensitively to the trends of structural change.

Do not take this quick sketch of seventeenth-century France as a model for the historically-grounded sociological analysis I am advocating. It lacks the painstaking confrontation of the sources with alternative interpretations in which historians excel. It lacks the explicit modeling, precise conceptualization,

careful measurement and deliberate comparison which are the emblems of good social-scientific work. It lacks the essential specification of the forms and changes of statemaking, capitalism and collective action from one era to the next. The sketch simply evokes the problem: to situate social processes in time and place. The work requires a permanent encounter of sociology and history.

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