
TWO REPORTS ON SOCIOLOGY AND HISTORY

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TWO REPORTS ON SOCIOLOGY AND HISTORY

reports on session 2 (Historical Analyses of Social Stratification, Mobility and Conflict) and session 3 (Alternative Approaches and Methodological Problems in Historical Analysis) of Symposium II (Sociology and History), Ninth World Congress of Sociology, Uppsala, Sweden, 17 August 1978

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Charles Tilly, University of Michigan, rapporteur

CONTENTS

Session 2: Historical Analyses of Social Stratification, Mobility and Conflict	1
Program of the Session	1
General Discussion	1
Rudolf Andorka, "Age, Cohort and Historical Factors Affecting Social Mobility in Hungary, 1930-1973"	5
Lars Olsson, "The Impact of Industrialization on the Social Environment of Children and Youth"	6
Robert W. Slenes and Pedro Carvalho de Mello, "Paternalism and Social Control in a Slave Society: The Coffee Regions of Brazil, 1850-1888"	8
Walter Goldfrank, "Fascism and World-Economy"	9
Alf Lüdtke, "The Role of State Violence in the Period of Transition to Industrial Capitalism"	10
Yoshio Sugimoto, "Peasant Rebellion and Ruling Class Adaptation at the Time of the Meiji Restoration in Japan"	11
Conclusions	13
Session 3: Alternative Approaches and Methodological Problems in Historical Analysis	14
Program of the Session	14
General Discussion	14
Igor Kon, "Sociology and History"	17
V. Dobrianov, "Polyvariantness of History, Recreative Historical Prognostication, and Sociology"	18
Robert J. Antonio and Parviz Piran, "The Loss of History and Man in Baconian Sociology: The Relevance of the <u>Annales</u> Critique"	19
Harriet Friedmann and Jack Wayne, "The Landed Classes, and the Agricultural Crisis in the Late 19th Century: A Comparative Analysis of Great Britain and Germany"	19

Session 3 (continued)

Nicole Bousquet, "La carrière hégémonique de l'Angleterre au sein de l'économie-monde et le démantèlement des empires espagnol et portugais en Amérique au début du XIXe siècle	21
Ian Winchester, "The Application of Western Literacy Growth Models to the Third World"	21
Dieter Groh and associates, "Sociological Approaches to German Social History"	22
Conclusion	24

SESSION 2: HISTORICAL ANALYSES OF SOCIAL STRATIFICATION, MOBILITY AND CONFLICT

Program of the Session

Rudolf Andorka (Hungary): "Age, Cohort and Historical Factors Affecting Social Mobility in Hungary, 1930-1973"

Lars Olsson (Sweden): "The Impact of Industrialization on the Social Environment of Children and Youth"

Robert W. Slenes and Pedro Carvalho de Mello (Brazil): "Slavery in the Coffee Regions of Brazil, 1850-1888: Labor Systems and Social Institutions"

Walter Goldfrank (U.S.A.): "Fascism and World-Economy"

Alf Lüdtke (German Federal Republic): "The Role of State Violence in the Period of Transition to Industrial Capitalism"

Yoshio Sugimoto (Australia/Japan): "Peasant Rebellion and Ruling Class Adaptation at the Time of the Meiji Restoration in Japan"

Charles Tilly (U.S.A.): rapporteur

Kálmán Kulcsár (Hungary): chair

The six papers in this session fall into two broad categories, which we might label 1) economy and social organization and 2) political processes. The reports by Andorka, Olsson and Slenes/Carvalho de Mello examine different instances of the interaction between the reorganization of production and major social processes. Those of Goldfrank, Lüdtke and Sugimoto take up the relationships between large social changes and particular political forms. If we had to find a common ground for all six papers, it would have to be something like this: how the development of the economic and political structures which dominated the twentieth-century world reshaped other features of social life.

If that is our theme, it is a very old theme indeed in sociology; one could reasonably argue that it is the theme which brought the discipline of sociology into being in the first place. Our nineteenth-century predecessors observed the expansion of industrial production, the spread of capitalism and the growth

1. As I prepared this report at the end of June, I had drafts of the papers by Olsson, Slenes/Carvalho de Mello, Goldfrank, Lüdtke and Sugimoto, but no more than a preliminary sketch plus some earlier papers by Andorka. This report therefore may be inaccurate when it comes to Andorka's contribution.

of powerful national states. As they observed, they began to think and write about the possibility that these great changes had uniform effects and followed general laws of development. Uniform effects and general laws? The exploration of that possibility preoccupied the early sociologists. Despite the fragmentation and empiricism of twentieth-century sociology, the idea that industrialization, the growth of capitalism and the expansion of national states might have uniform effects and general laws of development continued to frame a great deal of subsequent work. The competing inquiries set into motion by Karl Marx, Emile Durkheim and Max Weber are still being pursued today.

If so, is there anything new about the work in sociology and history on today's program? The answer is yes. Several features of these papers reflect recent trends in the area of overlap between the two fields. Taken as a group, the papers illustrate the increasing importance of specifically historical work in contemporary sociology. That increasingly common style of work differs from past efforts to sweep very large blocks of historical experience into grand developmental schemes. It also differs from the use of a schematic view of historical development as a context for sustained investigation of the present. The difference is that it takes history seriously. Taking history seriously involves regarding what happened (and not just why it happened) as problematic; considering the specific times and places in which social processes operate to shape the forms and consequences of those processes; recognizing that historical evidence presents its own demanding problems of collection, measurement and analysis. Thus Slenes and Carvalho de Mello do not simply interpret historical accounts of nineteenth-century Brazil, but dig into the documents surviving from that time and place to reconstruct the historical record.

Yet the work reported here is far from conventional narrative history. How so? Especially in three regards. First, on the whole our authors are explicit, even self-conscious, about the concepts, models and arguments they are attempting to apply to particular historical circumstances. Second, in general they undertake deliberate comparisons among relevant cases in order to verify and refine their arguments. Third, in most cases careful measurement figures importantly in their analyses. Sugimoto, for example, painstakingly lays out competing general models which might account for conflict in Meiji Japan, precisely delineates the regions and events he is comparing, and punctiliously measures the characteristics of the regions and events as he proceeds to test the competing general models. None of these is a standard feature of narrative history. They are the trademarks of the analytic, sociological history that has been flourishing for a decade or so.

The historical work which has emerged is not simply sociology with historical data. Look again at the papers on our program. Time figures in most of the analyses in a distinctive way: it is not the featureless, universal time of abstract sociological models, or the repeated cycles and sequences of standard developmental schemes, or the rich, specific but fragmented moments of narrative history. Instead, our contributors move toward a fourth alternative comprising assumptions about time which are nearly heretical in sociology as a whole: a) that when something happens in a large social process significantly affects how it happens -- for example, that the nature of state coercion changes radically from early capitalism to late; b) that the explanation of large social changes which occur in a particular country at a particular point in time is not likely to lie in the internal logic of that country's structure and development, but in its relationship to a major historical process such as the elaboration of a world capitalist system. Both these assumptions challenge the timeless logic,

the broad evolutionary schemes and the repetitive developmental models which have long been the sociologist's main choices in dealing with time. By adopting this sort of historicism, our contributors faithfully represent a subtle but powerful innovation which has been common in recent work on the frontiers of history and sociology.

Some sides of recent sociological history, however, are not readily visible from these papers. A reader of the papers could, for example, easily underestimate the great impact of collective biography: the piecing together of standard life histories for large sets of individuals, households or other social units in order to derive evidence of aggregate changes and variations in the characteristics of the populations under study. In demographic history, in the historical study of class structure and social mobility, in work on past conflict and collective action, and in several other areas of historical research, collective biography has transformed the prevailing ways of handling the available evidence. Although other parts of the research of Andorka, Slenes/Carvalho de Mello, Sugimoto and probably Olsson as well do involve rigorous collective biography, the papers in this session do not show us that procedure at work.

Again, none of the papers on our program explicitly formulates its argument as a mathematical model. The analyses of planter rationality by Carvalho de Mello and Slenes rely, at least implicitly, on mathematical models of market behavior. Sugimoto's statistical analyses commit him to standard linear models of correlation, and perhaps of causality. But we do not find directly represented in these contributions the explicit mathematical modeling which has become an increasingly common way of formulating the arguments examined in historical work. Not that our authors should have worked mathematically. My point is the obvious one: we cannot take the wide-ranging papers of this session as an exhaustive survey of the realities and possibilities of current research in sociological history.

Let us review the papers one by one before returning to reflections on their collective significance.

Rudolf Andorka, "Age, Cohort and Historical Factors Affecting Social Mobility in Hungary, 1930-1973". Since Andorka's paper for this session has not reached me, I am relying on his abstract and on a related paper he presented to the ISA Research Conference on Social Stratification in Dublin, April 1977. Andorka and his colleagues at the Central Statistical Office in Budapest have been studying Hungarian censuses and national sample surveys. They are trying to identify changes in the patterns of intra- and intergenerational mobility which occurred with the socialization and industrialization of the Hungarian economy, and to sort out the impact on mobility patterns of such major events as the Second World War. They analyze the occupational positions of large samples of adult males, either comparing son's occupation with father's or comparing the occupations held by the same man at different points in time. "Occupational position" consists of location in a nine-category classification (executive, manager, professional/other non-manual/skilled, and so on) or in a condensed version of that classification. The statistical analyses are large but simple: they comprise the preparation of different sorts of origin-destination tables for successive birth cohorts and selected points in time. In the reports I have seen, the investigators do not attempt a statistical decomposition of the overall mobility patterns into the effects of cohort, time, and structural change in the economy. Instead, they seek to reason about the three sorts of effects through inspection of their tables.

The group's analyses document some major patterns of mobility in Hungary from 1930 to recent years: substantial flows out of agriculture and artisanal occupations; large flows into non-manual jobs; a long-range decline in the proportion of workers moving from category to category within their own careers; a compensating long-range increase in the proportion of new workers entering the labor force with

occupations different from their fathers. The experiences of successive cohorts differed; for example, the men born from 1923 to 1927, who were beginning their occupational careers around the end of World War II, acquired managerial and other non-manual positions earlier and with greater frequency than other cohorts. There also appear to be historical effects which cut across cohorts and which do not reduce, in any simple way, to structural shifts within the labor force. From 1951 to 1957, for example, both intragenerational and intergenerational mobility were higher than in the previous period, despite a decline in the rate of change in the composition of the labor force. Many explanations are, of course, possible: emigration of non-manual workers who then had to be replaced, delayed effects of wartime deaths on occupational opportunities, changes in educational policy, alterations in job recruitment practices, still others. If we may complain that Andorka's analyses neglect women and fail to specify the place of formal education in the changing mobility patterns, we may be pleased that they lead easily and directly to the investigation of the impact of Hungary's particular historical experience on the country's changing patterns of movement from occupation to occupation. In the findings we discover a coalescence of the sociology and the history of social mobility.

Lars Olsson, "The Impact of Industrialization on the Social Environment of Children and Youth". Olsson's group at Lund is studying, in effect, the creation of adolescence. They begin with the premise that before the advent of large-scale industrial production, most children worked -- but in various forms of household production. In the early stages of large-scale industrialization, many children went to work in factories and other big workplaces. Later, reformers decried the evils of child labor, other reformers advocated increased schooling, and in fact child labor diminished as schooling increased. But, argues Olsson,

the technological requirements of industrialists (rather than, for example, any rise and fall of heartlessness) determined the rhythm of child labor. The example of Malmo's tobacco factory shows that in the early stages the city's poor and broken families supplied their children's labor cheaply with the encouragement of the authorities, and that in the later stages the decline in the employment of children depended mainly on the changing technology of production.

Yet eventually child labor did decline. Olsson argues plausibly that only with industrialization did large numbers of children begin to pass through a stage in which they were physically capable of productive work but did not work. Since youngsters had previously received a good deal of their socialization to adult roles through work, rings the argument, the decline in children's work produced youths who were not only idle but unsocialized. Frightened adults tried to control and to socialize the newly-created adolescents by extending compulsory schooling and by forming adult-controlled youth groups. The research itself follows the experiences of several Swedish towns from 1800 to 1930. They are towns in which many children once worked in industry, and in which it is possible to follow changing labor practices, educational policy and recreational activity. The argument and the research design immediately raise intriguing questions: Is the shift in children's labor as completely determined by autonomous technological changes as Olsson's account makes it appear? Don't the employer's demands for labor discipline, the strategies of the families which put their children to work, the sheer availability of different categories of potential workers affect both the frequency of children's wage-work and the rate of adoption of new technologies? To what extent did changing links between schooling and occupational opportunity alter children's and parent's own evaluations of the value of staying in school?

What is the evidence, aside from alarmed statements by older people, of a genuine break in socialization as a result of declining children's work? We can be grateful to Olsson and his collaborators for putting these still-pressing questions on our agenda.

Robert W. Slenes and Pedro Carvalho de Mello, "Paternalism and Social Control in a Slave Society: The Coffee Regions of Brazil, 1850-1888". Slenes and Carvalho begin by reviewing the ideas of slaveholder paternalism in the recent work of Genovese and of Fogel and Engerman; the first emphasizes the role of a paternalistic ideology in mediating class and race conflicts; the second, paternalism as a form of rational management under exceptional conditions. Slenes and Carvalho then present their own view of slaveholder paternalism as a combination of patron-client relationships and ruling-class ideology likely to arise under these conditions:

- 1) insecurity and limited life chances for the bulk of the population;
- 2) a dominant class which is vulnerable to the escape and the resistance of its workforce;
- 3) stability of local communities;
- 4) an integrated dominant class which is actively involved in managing its economic interests.

In the light of that argument, they present the methods and materials of their comparison between two coffee-growing municipios in southeastern Brazil. The project is very ambitious. Their preliminary results leave Carvalho de Mello and Slenes doubtful about any significant differences in planter ideology between the two regions, but optimistic about finding the correlated differences in master-slave relations and in the acceptance of the ruling-class ideology by the slaves that their argument leads them to expect. Their early findings also lead them to attribute a greater impact of slave resistance on slaveholder behavior than either their own argument or previous writing on the subject suggests. In central-west Sao Paulo province, they speculate, slave resistance was so widespread and effective that it inclined slaveholders to consider abandoning slave production for free labor. If so, the initial model will require some repairs. It appears that the

vulnerability of the dominant class to worker resistance only promotes paternalism within narrow limits: enough resistance to threaten its control, not enough to destroy it. It is refreshing to encounter an argument which is sufficiently specific, and a research program which is sufficiently rigorous, that new evidence can lead to an immediate reformulation of the chief hypotheses.

Walter Goldfrank, "Fascism and World-Economy". Goldfrank's paper is of a very different type: a thoughtful sortie into a vast terrain. He urges on us three different connections between the structure of the capitalist world-system in the twentieth century and the emergence of different varieties of fascism. The first connection he proposes is this: the combination of economic contraction throughout the system with a shift of hegemony from Britain toward the United States made extreme forms of nationalism attractive to competitors in the world-system. However -- and this is the second proposed connection -- different versions of fascism appealed to nationalists in the core, the semi-periphery and the periphery of the system, precisely because their economic and political interests varied as a function of position in the world-economy. Third, the very processes which drove the world-system -- capital accumulation and interstate competition -- also produced the impulse to fascism. In addition to these three arguments, Goldfrank offers a catalog of theories about fascism, a tribute to the thought of Karl Polanyi on the subject, a schematic summary of the development of fascism from sect to movement to regime, and some reflections on the unlikelihood of a fascist renaissance in our own time.

That many complex themes clutter the paper, and keep Goldfrank from developing any one of them to the point that we can see the full argument or assess its plausibility. As a result, the paper's basic assertions start out looking very strong, and end up looking rather weak. At first Goldfrank

suggests that the operation of the capitalist world-economy explains the rise of fascism in its many varieties. But in the detail we find indications that the relation of a particular country to the world-system (especially its location in core, semi-periphery or periphery) affected the content of its own form of extreme nationalism. Nor does the argument unfolded provide any clear and general explanation of the difference in strength of fascist movements from one country to another. At best, we come away with a set of provocative observations about particular nationalist movements and a set of categories for the next round of analysis.

Alf Lüdtke, "The Role of State Violence in the Period of Transition to Industrial Capitalism". Lüdtke, in contrast, pursues a general argument through the intensive study of a single historical experience. The general argument comes from Marx: that with the capitalization of production came a transition from extra-economic to economic forms of coercion, from external control to internal control. The case in point is Prussia from 1815 to 1848; Lüdtke also offers brief comparisons with France and Britain in the eighteenth and nineteenth centuries. The theme of his analysis is policing, in a broad sense of the word: the whole set of coercive means the authorities used to control the behavior of workers and the poor. Did Prussian policing operate to fend off threats to property, to protect the interests of employers, and to prevent the collective action of workers? Did it display the expected transition to more routinized and indirect forms of control? Lüdtke's answer to both questions is, I think, "Yes, but . . ." In the first case, his extensive work in Prussian archives and in nineteenth-century administrative writing brings out the class basis of repression, indeed of the very definitions of order and disorder. Yet it also leads to a pair of important qualifications: first, the great and continuing influence of the landed gentry -- not the

quintessential capitalist class -- in the whole apparatus of coercion; second, the considerable attention given by the authorities to control of the mobile propertyless classes rather than of settled industrial workers. In the case of the transition from external coercion to routinized internal control, Lüdtke gives us a detailed account of the growth of civil police and makes some passing remarks concerning the rise of anticipatory surveillance of workers' activities, but ends up stressing the continuing involvement of the military in crowd control, the military character of much ostensibly civilian policing, and the great discretion the Prussian military retained in defining threats to public order. The discretionary powers of the Prussian army, in Lüdtke's view, constituted one of the most important differences between the German experience and that of France or England. We might therefore ask how well the evidence supports the original argument: during the period of Prussian history under observation, the hypothetical transition from external to internal means of coercion is not at all obvious.

Yoshio Sugimoto, "Peasant Rebellion and Ruling Class Adaptation at the Time of the Meiji Restoration in Japan". Yoshio Sugimoto's study of Japan from 1830 to 1871 introduces a style of analysis we have not yet encountered in this session, although it is often employed in historical work on political processes. Sugimoto takes seventy administratively-defined regions of nineteenth-century Japan, characterizes their economic structure, describes some aspects of political activity in those regions, then formulates and tests arguments concerning the links between economic structure and political activity via a straightforward correlational analysis. (Let me not make it sound too easy: the volume of evidence Sugimoto has digested and the ingenuity of some of the measurement are both remarkable.) He is particularly concerned to examine the possible connection between the frequent peasant rebellions of Meiji Japan --

1,029 in forty years, by the count he adopts -- and the successful opposition of the lesser samurai to the feudal regime. Behind peasant rebellion, he wants to discern the effects of "exogenous" capitalization: the process by which outside merchants, rather than primary producers, promoted commercialization and concentration; in Japan, exogenous capitalization was especially visible in silk production, as compared with the important role of peasant producers in the growth of cotton production. Sugimoto's abstract states the main findings as succinctly as I could:

- a) peasant uprisings were prevalent in areas where agricultural commercialization was facilitated by exogenous merchant capital outside the peasant community; b) peasant uprisings were scarce in areas where institutions propagating ideology functionally similar to the Western Protestant Ethic were strong; c) the movement organized by the innovative portions of the elite sector against the Tokugawa Shogunate regime was powerful in areas where feudal local governments succeeded in establishing a monopoly in the sale or manufacture of commercially profitable commodities; and d) these two types of confrontation against the feudal system were negatively associated with each other. The samurai movement organized in the absence of popular revolt became the principal force to grab power in the Meiji government which took the initiative of modernization from above.

Sugimoto moves from his findings to a series of reflections on two additional problems: how the outcomes of the conflicts he has analyzed affected the subsequent development of Japan, and whether the Japanese experience provides a model of transition from feudalism to capitalism which is neither producer-led nor merchant-initiated, but dominated by a fragment of the old feudal classes. We hear echoes of Barrington Moore. Following Moore and following the general

logic of Sugimoto's analysis, the clear next step is to move away from the flat, cross-sectional comparison of regions, to examine whether the structure, content, timing and interplay of the many conflicts from 1830 to 1871 actually correspond to the rhythms of commercialization and political opportunity.

Conclusions.

The six papers in this session are agreeably free of exhortations and declarations about sociology and history in general. They do not tell us that it is feasible, or desirable, or complicated, to bring sociology and history together. They simply do it. As compared with the bulk of sociological writing on historical themes, these papers are exceptionally sensitive to the nuances and demands of the available historical evidence. Our authors are unwilling to treat history either as a flat space lined only by giant abstract Trends or as a storehouse packed with Data one can easily cart off for analysis somewhere else. Yet they are not content to write the history as it has always been written: Not one of our authors regards an accurate reconstruction of what happened, a sensitive portrayal of the way it felt, or even a new insight into what the people of the time were trying to do as the final aim of his analysis. All our contributors are trying to combine history and sociology in order to improve our understanding of large social processes, processes which are still shaping the world today. For that, we must thank them.

Program of the Session

Igor Kon (U.S.S.R.): "Sociology and History"

V. Dobrianov (Bulgaria): "Polyvariantness of History, Recreative Historical Prognostication, and Sociology"

Robert J. Antonio and Parviz Piran (U.S.A.): "The Loss of History and Man in Baconian Sociology: The Relevance of the Annales Critique"

Harriet Friedmann and Jack Wayne (Canada): "The Landed Classes and the Agricultural Crisis in the Late 19th Century: A Comparative Analysis of Great Britain and Germany"

Nicole Bousquet (U.S.A.): "La carrière hégémonique de l'Angleterre au sein de l'économie-monde et le démantèlement des empires espagnol et portugais en Amérique au début du XIXe siècle"

Ian Winchester (Canada): "The Application of Western Literacy Growth Models to the Third World"

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This session's papers array themselves in roughly descending order of generality:

1. general statements on sociology and history: Kon, Dobrianov; Antonio & Piran;
2. broad comparisons of major historical eras and processes: Friedmann & Wayne, Bousquet;
3. surveys of particular research problems and approaches: Winchester, Groh.

To my regret, we have no papers presenting monographic work which analyzes a single historical event or situation; the individual studies which comprise the program of Groh's research group are the closest we come. That is regrettable because most of the important contributions of sociology to historical work

1. As I prepared this report at the end of June, I had drafts of the papers by Kon, Dobrianov, Antonio & Piran, Friedmann & Wayne, but only a related paper by Groh, a projet by Bousquet, and the title of Winchester's paper. This draft therefore neglects the contributions of Groh, Bousquet and Winchester.

occur at that level, because the thorniest problems in the application of sociological methods to historical problems and materials appear there, and because such monographic studies provide the clearest confrontation between the everyday assumptions and practices of sociologists and historians. When they begin working together on the same community, the same rebellion or the same change in family structure, people from the two disciplines often swallow their slogans about the unity of knowledge, and recognize for the first time the genuine differences in approach which separate them.

Still, the last decade has been a time of convergence between sociological and historical practice. The tilting of history toward sociology began earlier. Not long after World War II, in fact, historical demography began to mature as an enterprise with its own problems and logic -- first in France and Britain, then elsewhere. Although some of the initial impulse toward rigorous historical demography had come from the desire to identify well-documented past experiences with which to compare the demographic situations of poor countries in the contemporary world, it was a long time before demographers and other sociologists began to draw regularly on the findings of historical demography; historians who wanted to understand particular blocks of French, British, Japanese or other history were long the chief beneficiaries.

Again, historical studies of stratification and mobility were for some time nearly a one-way street: much application of sociological methods and models to situations in which historians were already interested, little impact of those studies on sociological practice. Even now the pioneering investigations of a Soboul or a Thernstrom, for all their familiarity to historians concerned with social class, are little known to sociologists. The same is true for a number of other areas of contact between sociology and history: the study of family organization, of community structure, and more. While historians have

anxiously inspected, and sometimes adopted, the sociologists' models and methods, the sociologists in nearby areas have at most expressed a polite curiosity about the doings of their historical neighbors.

Elsewhere, however, sociologists have bent toward history. While in history the initial interest in sociology was chiefly methodological, in sociology the turn toward history represented a genuine change in the definition of valid problems and theories. I believe the most influential single factor was the debacle of non-Marxist theories of development. Theories implying a standard path of modernization for all countries made it easy to paste together the relevant history from textbook cliches. As Third World critics challenged the applicability of western developmental models to their own experience, the challenge made westerners wonder how well the theories applied to their home grounds. As historians sought to use contemporary development theories as frameworks for analysis of the past, the difficulties they encountered likewise raised doubts about the theories themselves; those doubts generated new historical work. Finally, the opposition -- internal and external -- to American and western European governments increasingly took the intellectual form of criticisms of the development theories their dominant classes used to guide or justify their dealings with the poor countries of the world. Some of that criticism consisted of historical research on the actual processes by which parliamentary democracy, capitalist property relations, American hegemony, industrial social organization and other arrangements which had once appeared to be natural outcomes of "development" came into being. The turn toward Marxist theories of development as an alternative brought sociologists into a rich and active body of historical work on political and economic processes. As a consequence of these and other changes, the

last few years have brought an impressive increase in the amount of sociological energy going into concrete historical studies on the small scale and the large. We have had studies of the changing world-economy, of revolutions, of transformations of family structure under capitalism, and of a host of other topics which most sociologists would once have treated as problems of modernization. It has been an exciting time to work at the frontiers of sociology and history.

The papers in this session reflect some, but not all, of the recent historical revival. Before considering what the papers may tell us in general about the promise and problems of that revival, let us review them individually.

Igor Kon, "Sociology and History". Kon offers us a very general discussion of the convergence of the two disciplines, stressing the importance of Soviet contributions and Marxist theory. He suggests that as western sociologists turned from the narrow empiricism of the 1930s and 1940s toward the analysis of social change and historical development, they had little choice but to take history seriously. I have already given some of my reasons for thinking it was not so simple. Kon presents family history as a characteristic area of recent interdisciplinary collaboration. That is true, but it is also an outstanding example of asymmetry: the impact of demography and other branches of sociology on the historical study of families has been enormous, while the discoveries of family historians have so far had little impact on the ways that sociologists think about family structure. Why that should be so, and what sociology loses as a consequence, are well worth discussing.

Kon closes his essay with general reflections on the necessity of a systems approach to historical development, on the value of historical analysis in revealing the relativity and transience of apparently universal categories and institutions, and on the futility of the recurrent debate between the advocates of "scientific" and "humanistic" conceptions of history. At this level of

generality, the reflections can be little more than platitudes. The inquiry only becomes concrete and engaging when it comes down to asking how to deal with particular historical cases and processes: how much of the change in family strategies that accompanied the expansion of capitalism, for example, we can infer from the definite historical evidence of changing fertility, nuptiality and household composition, and how much we must supply from other evidence, from general models, or from our historical imaginations? Now, that debate is proceeding in one of the most productive possible ways: via competing studies of concrete historical experiences.

V. Dobrianov, "Polyvariantness of History, Recreative Historical Prognostication and Sociology". The formidable title of Dobrianov's paper introduces a plea for a synthesis of social planning, historical reconstruction and sociological analysis. In the ideal case, social planning specifies a set of alternative outcomes which are possible during some span of time, sociology investigates the connections between those outcomes and other social conditions, and history traces how the outcome which actually emerged came to prevail over the other possibilities. Although he would apparently tolerate some historical concern with the past, Dobrianov is mainly concerned with the history of the present and the future: the continuous production of "prognostications" concerning possible future states of social life, coupled with the continuous comparison of unfolding realities with unrealized possibilities. Historians would, in his program, become "historiologists," "historical scientists." Historians will not, I think, rush to Dobrianov's banner. Since, in any case, Dobrianov provides no concrete illustrations of the triumphs of his new science, we may reasonably dawdle a bit before committing ourselves.

Robert J. Antonio and Parviz Piran, "The Loss of History and Man in Baconian Sociology: The Relevance of the Annales Critique". Antonio and Piran trace the prevalent empiricism of American sociology back to the errors of Francis Bacon -- to Bacon's conceptualization of the world "as a finite manifold of facts that could be laid bare through careful observation and classification." They claim (to my surprise) that the chief mode of theorizing in sociology consists of summarizing currently acceptable empirical generalizations, and that modern sociological theorists assume that empirical generalizations which hold for the present hold for all time. Piran and Antonio call for a radical critique of Baconism, proposing as a guide the critique of the "trivial, narrow and ideological nature of event history" by historians associated with the French journal Annales. They regard the Annales emphasis on long trends and on the connections of apparently disparate social phenomena as an effective antidote to the ahistorical fragmentation of Baconian sociology.

As someone who has long consorted with the scholars of the Annales, and has much admired their zeal, vision and individual creativity, let me express a personal doubt that they will help us rebuild sociological theory. The Annales have no body of theory of their own; their theories come from everywhere and nowhere. They have, instead, an openness to innovation, a readiness to collaborate, a desire to make unexpected connections, a distrust of narrative political history, a willingness to risk comparisons and analogies, a marvelous set of exemplars in Marc Bloch, Lucien Febvre, Fernand Braudel, Emmanuel Le Roy Ladurie and others. Formidable assets, these, but not the stuff of a new sociological theory.

Harriet Friedmann and Jack Wayne, "The Landed Classes and the Agricultural Crisis in the Late 19th Century: A Comparative Analysis of Great Britain and Germany". Friedmann and Wayne begin with a conundrum: if industrial capitalism triumphed in many western countries during the nineteenth century, how did the landed classes

hold on to their power so long? Their answer, in two nutshells, is that the landed classes 1) staffed nineteenth-century states in disproportionate numbers, and thus had the state apparatus to protect their interests and 2) also organized effectively outside the state. As they point out, such an explanation attributes more autonomy to political processes than simple economic determinism will tolerate. They illustrate and defend the argument via a comparison of Germany and Britain. The two cases suggest themselves because the landed interest -- as a class collectively and effectively pursuing its own advantage -- survived the agricultural crisis of 1873-1896 in Germany, but succumbed toward the end of the nineteenth century in Britain. Why the difference? In Britain, by the Friedmann/Wayne account, the greater advance of large-scale manufacturing meant a more powerful industrial bourgeoisie and a more extensive involvement of organized workers in national politics. Germany then becomes the more troubling case. Friedmann and Wayne propose a composite explanation: greater need of the German industrial bourgeoisie (faced, among other things, with powerful international competition and an already-enfranchised working class) for the support of the banker-bureaucrat-soldier coalition which governed Prussia, demand for military protection of industries located near frontiers which were ringed with hostile powers, plus the deliberate organizing activity of the landed classes. The first two conform to our conventional explanations of the Junkers' survival, while the independent importance of the organizing activity requires more argument and evidence than the paper provides. The two-nutshell argument which survives is, I think, different from the one Wayne and Friedmann state at the outset. It seems to run: 1) the rise of the industrial bourgeoisie did produce a transfer of state power, but with a lag attributable to the previous staffing of the state and the political organizing

of the landed classes; 2) the tactical alliances struck by the rising industrial bourgeoisie also affected the distribution of power over the state. In that form, the Friedmann/Wayne argument has much in common with the treatment of the same comparison by Barrington Moore.

Nicole Bousquet, "La carrière hégémonique de l'Angleterre au sein de l'économie-monde et le démantèlement des empires espagnol et portugais en Amérique au début du XIXe siècle". (At this writing, I have to take Bousquet's argument from a proposal written in September 1977.) Bousquet proposes to connect the Latin American independence movements of the early nineteenth century to the reorganization of the capitalist world-economy of the time. The inquiry has many steps, but the central argument is this: so long as France and Britain battled for control of the world-economy, Spain and Portugal were relatively free to exploit their colonial empires; but as Britain acquired hegemony, her need for markets for manufactured goods eventually made the opening up of Latin America desirable. In the course of presenting and defending that argument, Bousquet proposes to contrast this sort of world-system analysis with the conventional country-by-country natural history of decolonization. Since both the historical and the methodological argument have been made before, the delicate point will be to establish the direct links between the presumed British interest in penetrating Latin American markets and the actual facts of national liberation in that part of the world.

Ian Winchester, "The Application of Western Literacy Growth Models to the Third World". (Since Winchester's paper has not arrived as I write, the only basis I have for inferring his likely argument is a 1977 essay, "What Microdemographic Methods Have Added to Our Knowledge of the Historical Past.") Permit me a weak prediction. Winchester's argument, when it appears, will

break into two parts: a) those western literacy growth models which present the rise of literacy as a consequence of increases in communications and in schooling, which are in turn supposed to be consequences of industrialization or "modernization," do not and cannot apply to Third World experience, since in the contemporary world communications changes commonly occur ahead of, or even independently of, industrialization; b) in fact, those models do not even apply very well to western historical experience; among other things, industrialization often depressed literacy (at least in the short run), and many western populations learned to read and/or write quite independently of industrialization, urbanization and other supposed prerequisites of extensive literacy. If so, the argument will be worth making and worth debating.

Dieter Groh and associates, "Sociological Approaches to German Social History". (Although the paper was, by Groh's report, finished some time ago, I have not received a copy. My sources are a pair of informal proposals from Groh, plus his 1978 paper, "Basisprozesse und Organisationsproblem. Skizze eines sozialgeschichtlichen Forschungsprojekts.") The social-historical group at the University of Konstanz has launched an ambitious program of inquiries into Basisprozesse: roughly speaking, protests, strike activity, organization-building and other means by which workers articulate their interests. They mean the program to constitute at once a) a critique of standard historical approaches to the labor movement and b) a systematic reconstruction of Marxist analyses of the connections among the organization of production, routine social life and collective action. Their concrete investigations deal with different groups of German workers from the eighteenth to the twentieth centuries. The particular projects described in this paper include the socialization of workers in Baden before the Revolution of 1848 (Rainer Wirtz), the demands and

complaints of journeymen in eighteenth-century towns (Andreas Griessinger), strikes in the Saar and the Ruhr before 1914 (Detlev Puls), German strikes as a whole from 1890 to 1914 (Hans-Peter Jäck); and the discrepancies between Hitler's Weltanschauung and German decisions, 1938-1945 (Bernd Laufs). In all these cases, and more, the researchers are attempting to use standard social-psychological models: from dissonance, from learning theory, and so on. Now, any such matching of collective phenomena with individual-level models faces an ascending scale of difficulty. In principle, it is relatively easy to match the concepts with the phenomena under study -- for example, to show that one can reasonably think of eighteenth-century journeymen as learning to make certain demands. It is much harder to test the theories involved on the collective phenomena, since a) the theories only yield firm predictions about individual behavior and b) the evidence for verifying those predictions at an individual level is rarely available. It is most difficult of all to show that the social-psychological processes involved explain the collective phenomena in question; after all, the Basisprozesse the Konstanz group are considering consist almost entirely of interactions with authorities, employers and rival groups of workers; at a minimum the explanations of interactions require accounts of the actions of all the parties. That side of the program does not look very promising.

Another side does. The Konstanz group is also seeking to "decode and reconstruct the immanent rationality or social logic of the behavior of various lower-class groups -- agrarian, plebeian and proletarian" and thereby to follow the connections between collective action and everyday life. E.P. Thompson and Pierre Bourdieu are the models for the effort. Of course, all the secret of such an effort is in the doing: it can be done very badly or very well. Nevertheless, the proposal is appealing in itself, for it provides

an opportunity for a rich new synthesis of sociological and historical approaches to the analysis of collective action and of everyday life.

Conclusion

The papers in this session illustrate a simple principle: if you want to advance the collaboration of sociology and history, look for an important, concrete historical change which presents problems for the conventional wisdom in both disciplines. Wayne and Friedmann's problem -- how the land-based Junkers kept their power so far into the industrialization of Germany -- is a good example. History then provides you with the luxuries of time-perspective, opportunity for comparison, rich and varied evidence. Sociology, for its part, offers concepts, models and strategies of analysis. That we should have to think twice (as Antonio and Piran remind us) before applying to historical materials the empirical methods we adopt routinely and thoughtlessly in sociological analyses of the contemporary world is all to the good. That standard historical explanations of mass behavior, with their homogenization of motives and mentalities, should shatter when confronted with sound sociological thinking can only make the encounter of history and sociology more fruitful. Why not, then, abandon exhortation for action?