



TIMELINES

HISTORY OF SOCIOLOGY
AMERICAN SOCIOLOGICAL ASSOCIATION

April 2007

E-Newsletter No.8

Message from the Chair

Eleanor Townsley, Mount Holyoke College

Doing History of Sociology

The methods, problems and focus of the history of sociology were taken up in a Didactic Seminar organized by Ed Tiryakian at the Montreal meetings last August. Here, we reproduce contributions from Uta Gerhardt, Ida Harper Simpson, Jennifer Platt, and Charles Tilly, and encourage responses. Together, these fascinating papers not only raise large questions about the history of sociology but also provide insight and practical suggestions for doing history of sociology.

First is Ida Harper Simpson's reflective analysis of creating a history of the Southern Sociological Society. It is an elegant account of the relationship between the sociological logic of composing a history and the materials available from formal and informal archival records of this important sociological institution.

Second, Uta Gerhardt's ruminations about archival work on Parsons' biography describes what we might understand of Parsons' life and work by analyzing the historical record about the process of his academic writing over time, and by looking at unpublished material, memoranda and correspondence.

Third, Jennifer Platt offers a set of considerations for comparative macro-sociological work in the history of sociology, emphasizing the importance of description and comparison, reviewing types of data sources and their use, and providing an invaluable appendix of archival sources in the United States and Europe.

Finally, in a paper which serves as an analytical introduction to the collection, Charles Tilly draws parallels between different approaches to doing history of sociology and different approaches to doing historical sociology more generally. He distinguishes what he calls epochal syntheses, retrospective ethnography and critical comparison, narrating a history of how sociologists have used different perspectives over time.

Enjoy.

All the very best, Eleanor

Please join me too in thanking the exceptional candidates who have agreed to stand for office in the section this year.

For Chair-elect:

- Charles Camic, Northwestern University
- Terry Clark, University of Chicago

For Council, 2 seats of 3-year terms:

- Ira Cohen, Rutgers University
- Marcel Fournier, University of Montreal
- Richard Swedberg, Cornell University
- Joyce Williams, Texas Women's University

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Fifty Years of the Southern Sociological Society: Change and Continuity in a Professional Society. Methodological Perspectives on Studying the History of the Southern Sociological Society*

Ida Harper Simpson
Duke University

* *Invited paper given at the 105th annual meeting of the American Sociological Association, Montreal, Canada, August 12, 2006, as a didactic seminar for the History of Sociology Section*

The Southern Sociological Society was founded in 1935. In anticipation of its fiftieth year, the Executive Committee appointed a Golden Anniversary Committee to make plans for the occasion. Lee M. Brooks and Alvin L. Bertrand's *History of the Southern Sociological Society* (1962) had been commissioned for the Society's twenty-fifth anniversary, and the Committee recommended a fiftieth-year history. I accepted an invitation to write this second history. I decided against simply extending Brooks and Bertrand's earlier work by adding happenings from 1960 to 1985 (the next twenty-five years) in favor of writing a new fifty-year history of the Society. Preparatory to deciding what kind of history I would write, I visited the Society's Archives at the University of Kentucky to find out what kinds of information it housed. There were personal accounts, mainly by the presidents; the secretary-treasurers' reports; committee reports; copies of annual programs; and newsletters. The documents spanned the Society's life from its founding onward, though the information was far from complete for all years. The annual programs were the most continuous documents.

I had long been impressed by Everett C. Hughes' (1952) concern that the American Sociological Society (later renamed the American Sociological Association) should be wary of shifting from a disciplinary to an occupational form of organization. (An occupational organization is oriented toward its market position and the careers of its members.)

After I perused archival information and reflected on my observations of the Southern Sociological Society, I felt that the available information would enable me to write a history guided by Hughes' observation. Accordingly, the thesis for my history was that the Southern Sociological Society evolved from being a community of scholars interested in studying the problems of the South

to being a professional society. I wanted to describe how the change occurred
Methodological Issue

The first and perhaps the most important methodological perspective for my research was defining the research problem. I saw my history as "a study of the processes that transformed a scholarly society into a professional association" (Simpson 1988, vii). How might I discern changes and their significance by drawing chiefly on archives? I realized quickly that I needed to view change and conditions fostering it within the context of the social environment of SSS. Relevant dimensions included the South, whose problems many sociologists of the South sought to address through research; the development of sociology as a subject of study and research in the nation and in Southern colleges and universities; and the resources on which the Society drew, including the population of potential members.

Therefore, my second methodological concern was to find sources of information about the development of sociology in the nation and in the South. Among the most useful sources were studies of the growth of sociology in colleges and universities in the United States, especially L.L. Bernard's works (1909, 1918); *American Journal of Sociology's* annual lists of PhD's by institution, title and author; "news and notes;" and committee reports of the American Sociological Society; Howard W. Odum's, *American Sociology* (1951); and in the South (Bernard 1918, 1937; Odum 1938) and a few interviews I conducted with old, middle-aged, and young SSS members. Odum's *American Sociology* is a fifty-year descriptive history (as seen by Odum) of all of the first half-century's ASS presidents and their presidencies. The primary sources, mentioned above, give primary information, consistently collected and reported over long periods of time.

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Organizations change through long-term evolutionary processes and through deliberate revolutionary actions. These are markedly different kinds of changes. Unlike revolutionary change, which visibly alters an organization, evolutionary change occurs largely unnoticed to become visible only over relatively long periods of time. SSS had experienced revolutionary as well as evolutionary (i.e., processual) change. Thus, my third major concern was to assure myself that appropriate data to study the two kinds of change could be had.

The data to discern evolutionary change needed to be uniformly and consistently recorded over time in order that they might be standardized and quantified as appropriate. Standardized data enable a researcher to make repeated observations over long time periods, with confidence that the same underlying object is being described at successive times. Luckily, the SSS Archives had fairly complete yearly series of annual programs; secretary-treasurers' reports on membership, attendance of meetings, business meetings, and finances; and committee reports, including those on elections, nominations, publications, and some historical events whose data might be quantified. I used these sources to infer evolutionary changes, including trends in membership, its employment, and its representation in annual meetings and in governance; differentiation of fields of sociology; and the rationalization and professionalization of SSS. My study deals chiefly with these trends because they have constituted most of SSS's change.

Even though revolutionary change was infrequent, I found it more challenging to study. A few of the incidents include founding of the society, decisions about meeting places and arrangements that would properly accommodate African American members, and the formation of a women's caucus. Once a change had been enacted, it was plainly evident.

SSS's second meeting had such an occurrence stemming from a racial incident. The Society's elite founding group included Charles S. Johnson of Fisk University. SSS's first meeting, in 1936, was in Atlanta at the Biltmore hotel. The Local Arrangement Committee's selection of the Biltmore was guided by "... the attitude of the management toward Negro delegates. Aside from room privileges, Negro delegates are welcome to all private conference rooms and to all private arrangements for the annual dinner and luncheon." (Letter from President Krueger to the Executive Committee, March 9, 1936). The hotel in defining the meeting and dining rooms as private sidestepped the segregation laws of Georgia.

The second annual meeting, held in Birmingham, was at the Tutwiler hotel. The Local Arrangement's Committee, which included Monroe N. Work of Tuskegee Institute, was able to get conference rooms for racially integrated meetings, but the management was unyielding with respect to a racially integrated banquet. The Society took swift action and struck the banquet from its annual program, effective with the 1937 meetings. No banquet reappeared until twenty-four years later. SSS was a leader in the South in including African Americans and women in its governance. Charles S. Johnson of Fisk University was an early president (1945) as was Katherine Jocher of the University of North Carolina in 1943-44.) Other revolutionary actions lacked the strong consensus that led to the swift decision not to have a banquet unless all members were able to participate. These were more testy to study.

Understanding the events leading up to the changes and locating data to identify their coalescing into new organizational directions required serious study. I was helped by relating events in the SSS to those in ASS, in regional sociological associations, and in sister social science associations in the South. Where might I locate information on such incidents and views about them? During the first quarter century of SSS, before the custom of telephoning and later, emailing, letters and to a lesser extent unpublished notes and news articles reported major SSS events. In other words, since revolutionary change is initiated and confronted at the individual level, my main sources for information on critical events that challenged the status quo were personal correspondence, interviews, and minutes of actions taken during business and committee meetings. In the case of contentious situations, I found personal documents worrisome, in part because the perspectives from which they were written were often not clearly specified and therefore the underlying views of the writer were not always apparent. In addition, I rarely had information on disparate views for consideration as a counterpoint to those of the writer.

Discerning and interpreting the two kinds of data – personal vs. standardized – differ. I never thought twice about my interpretation of standardized data because the definitions and procedures used by the Society to record the information were relatively uniform across years, and when the definitions changed, the indicators of changes were noted, such as the criteria for membership. Moreover, the main data sources (member, annual meetings, business meetings, financial reports, etc.) were specified by the Society's Constitution and/or other governing regulations. In my mind, I felt confident that the

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objective data were valid and reliable and would yield a trustworthy picture of the long-term processual changes.

The processes that moved the Society in the direction of a professional organization included the differentiation of fields of sociology, the rationalization of its procedures of operation, and the professionalization of the Society through actions that oriented the Society toward the marketplace of sociology. Sources that I used to identify differentiation of fields were the annual programs, while those from which I inferred increased rationalization included secretary-treasurers' annual reports on activities of that office, minutes of executive and other committees and business meetings, and other archival materials such as news and notes. For example, rules and regulations were formalized to govern participation in the annual program, registering and name-tagging members attending the annual meetings, standardizing the place for holding the annual meeting, setting time limits for meeting sessions, adopting accounting rules for the annual budget and its allocation for different activities, appointing a parliamentarian, specifying ways to distribute committee reports to the membership, setting dates for publishing and distributing the annual program, legislating rules for preparing ballots and conducting elections, and other similar procedures that regularized the Society's yearly activities.

Committee and business meeting reports were my main sources of information on the professionalization of SSS. Over the years, the annual business meeting became an arena for promoting and protecting professional interests. In 1955, a decision was made to discontinue program sessions on social work. Beginning in the 1950s, business meeting motions were increasingly enacted urging federal governmental support of social science research, and in a few cases authorizing president to write open letters to federal legislators in order to defend sociology against belittling attacks such as Senator William Proxmire's Golden Fleece awards. The number of meeting days were increased to enable more disciplinary sessions and committee meetings. The Society instituted awards, recommended by its Honors Committee, for distinguished careers as sociologists, for distinguished scholarship on women and on race relations, and for graduate and undergraduate student papers given at annual meetings. From the 1960s onward, the championing of sociologists' interests as sociologists became an expected feature of business meetings.

Although I could easily infer trends, pinpointing the conditions that sustained or redirected them was more trying. For example, African-American membership stood at around nine percent during the founding years

from 1936 until 1949, when it began a decline. Beginning in the 1950's, whites' membership grew rapidly, while African-Americans' slowly declined, with a few exceptional years; such as 1955, when the annual meeting was in Nashville and featured some program sessions at Fisk University. In 1982, the last year of my count, African-American membership had fallen to 3.7 percent.

Conditions related to this decline appeared to have had less to do with SSS per se than with the politics of desegregation in the South, the absolute and relative decline of African-American sociologists in the South, the dimming visibility of African-Americans' institutions of higher learning, and the competition between SSS and the Association of Social and Behavioral Sciences for African-American members. During the period when the civil rights movement was in full swing, the Society voted to hold meetings only in cities that afforded fully integrated services. Unfortunately, these recently-desegregated places -- Knoxville; Miami; Gatlinburg, Tennessee; Ashville; Washington -- were peripheral to the location of the employment of most members and thus increased the cost of attending, previously an incentive for joining SSS. Atlanta, which was within easy driving distance of a sizable number of historically African-American institutions, had long drawn the highest African-American attendance. After the passage of civil rights laws, membership rebounded a bit, but by 1979, it had dropped to an all time low of 2.4 percent and continued to decrease into the 1980's when my study ended.

Clearly, other conditions had helped to depress African-American membership. Among those that I discerned from other sources were the predominance of employment of eligible African Americans in small colleges, the absolute and relative decline in the number of African American sociologists, and lessening visibility of traditional African-Americans institutions in the years after official desegregation of higher education in South.

To investigate the Society's concern about its low membership from small colleges, President Charles U. Smith of Florida A & M University appointed an ad hoc committee to survey the faculty of departments of sociology of small colleges, "predominantly black and predominantly white, from Maryland to Texas about the faculties' membership and participation in SSS." The committee reported that the African-American faculty did not feel "any discrimination with reference to SSS." Rather, many, like their white counterparts, said that although they taught sociology courses, their professional degrees were in education, guidance and counseling, or social

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work, whose professional meetings dealt centrally with their interests. Their institutions' paltry support for professional participation further limited their options to those that matched their professional training. For example, the African-American membership in the Association of Social and Behavioral Sciences increased, while that in SSS decreased.

Apart from the choices of African Americans, racial integration of colleges and universities in the South lessened the visibility of African-American institutions. In the segregated South, the Society from its founding days actively promoted an ethos favoring the recruitment of African-American members and their representation in governance and in participation in the programs of its annual meetings. Its decline appeared to have been fed in part by procedures that accelerated the rationalization and professionalization of the Society.

A reason largely ignored by the Society was the growth of teaching and research opportunities stemming from the expansion of state university systems that established "new universities" in the South as elsewhere (e.g., new universities of North Carolina at Charlotte, at Wilmington, and at Ashville). The sociology faculty of these new universities were overwhelmingly white and swelled the numbers of white sociologists far above the numbers who had populated the old state universities, colleges, and other employing organizations during the first fifteen years of SSS's history. During this period of expansion of higher education, the old state institutions, white and African American, grew in size. Perhaps because of the small size of the labor market of African-American sociologists, African-American institutions increasingly hired whites, while at the same time, white institutions competed to hire from the small African-American pool. These trends helped to erode the long-held view that the Society's officials and membership should represent the region's different types of colleges, universities, and other employers (Simpson, pp. 210-211).

Conclusion

The SSS changed over the fifty years of my study, and the changes reflected Hughes' concern with what he saw as the move of the American Sociological Society from being a society to being a professional organization. The procedures adopted by SSS, such as submission of papers for consideration for the annual program, stated membership criteria, creation of a committee on the profession, and a change in the Society's goals from studying the problems of the South to disciplinary concerns, shifted SSS's goals. It is now a much larger organization that is organized more along the lines of an occupa-

tional group than a disciplinary society. Yet, alongside formalization of the Society, a sense of community and informality had been continued through reunions of old friendships, institutionalization of bluegrass music by SSS members for the open-night get-together, and the regularization of meeting places that implants the meeting place into the collective memories of the Society. Such have contributed to a culture of the Southern Sociological Society that is known for being the "best" regional society. The pleasure from attending its meetings remains a strong complement to the quality of its program.

Through the years the changes that constituted the themes of my history arose out of SSS's adaptation to changes in its environment from which processes evolved that shaped SSS's new directions. How much credence do other SSS members put in my story of its history? To give confidence in it, I asked a panel of elders of the Society to comment on my manuscript. The panel, representing varying types of institutions and social groupings of membership, obliged me with diverse frames of reference to judge the validity of my interpretations. My judgment was also reinforced by accounts of similar situations faced by other scholarly and scientific disciplinary organizations. Procedures to validate my interpretations of individual-level data were among the most worrisome methodological issues I faced in writing the fifty-year history of the Southern Sociological Society.

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DOING THE INTELLECTUAL BIOGRAPHY OF TALCOTT PARSONS

Uta Gerhardt, University of Heidelberg

Presented at the American Sociological Association, 101st Annual Meeting, Montréal CA History of Sociology Section Workshop/Seminar: Methodologies of the History of Sociology

Writing the intellectual biography of Talcott Parsons, arguably the greatest American sociologist, is microlevel inasmuch as the subject is a single individual. But in the life and work of this giant of social thought, the history of twentieth century American society comes to the fore in a way that makes his stature paradigmatic, as it was for the state of the art of our discipline. This judgment on the value of Parsonian sociology, as a baseline, has spurred my knowledge interest in doing the intellectual biography of Parsons.

My approach to the work of Parsons can be characterized through three background facts that have influenced my use of the Parsons papers accessible in the Harvard University Archives – a thesaurus indispensable for anyone writing on Parsons's oeuvre.

For one, not only did Parsons's doctoral dissertation written in German and submitted to the *Philosophische Fakultät* of Heidelberg University deal with capitalism as it had been analyzed by Max Weber. But Parsons remained a Weberian throughout his career, returning to the study and restudy of Weber whenever he ventured upon a new subject area until the 1960s and 1970s. Such lifelong reliance of Parsons on Weber's approach and findings in his own classic works, evidently, had to be substantiated through archival material.

Second, rescuing sociology from the prongs of Spencerian social Darwinism was not only what drove German sociology around the turn of the century into accentuating methodology. But in Parsons's lifetime, National Socialism, whose racism derived from Darwinism, reigned supreme during more than a decade. The political initiatives and intentions of Parsons as he defended democracy against its enemies, even those who were sociologists themselves, needed proof that could only be found in memoranda, correspondence, and similar unpublished materials. The latter, stored as they are in the

Harvard Archives, were the main source for my contention that Parsons's sociology was political in the sense that Max Weber had clarified. Both thinkers maintained that value neutrality is fully compatible with the sociologist advocating humanitarian ideals, provided that he keeps separate his science and his taking sides in the political debates of the day.

The third presupposition of my work is that I assume that Parsons was a conscientious thinker and a keen academic who would never be satisfied with inconsistent analysis. Contrary to the many critics who claim that Parsons's work is inconsistent or even naïve, I aimed to show from the material how he prepared his manuscripts meticulously and revised them over and over again. This, too, could be demonstrated only if I found and made known the material that documented how thoroughly he would rework his writings.

These three knowledge interests required that I had to consult the Parsons papers as they have been preserved in the Harvard University Archives. In order to use archival materials, however, thoroughness and patience are indispensable for the researcher. These usually are virtues of historians. Fortunately, I am a historian supplementary to being a sociologist. In the German academic culture of yesteryear when I received my education first from the Frankfurt Institute of Social Research and later the Free University of Berlin, students were required to study more than one discipline. The norm was to study one major field plus at least two minor fields – when all three, to be sure, were part and parcel of a rather rigorous examination at the end of the usual five-to-six-year university education. My major field was sociology. Since I began my academic career in Frankfurt where Theodor W. Adorno taught and students were required to take philosophy, I had to study Georg Wilhelm Friedrich Hegel in order to have the

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right conceptual approach for understanding the thought of Max Weber and Émile Durkheim, as well as for that matter at the time unavoidably, Karl Marx. My other minor subject was history, and I admit that I chafed under the onus to first translate medieval Latin into classic Latin before using the respective sources in essays and seminar papers that also referred to literature in French and English. Sociology, therefore, being mediated through philosophy and history, was for me a discipline that had to do with the relationship between the conceptual and the historical layers of social thought in various empirical societies. This approach may have influenced my impression of Parsons's *The Social System*, when I became fascinated with it: of all his books this one became an eye-opener. To me, this work was both methodologically consistent in a Weberian sense and historically courageous, as it touched upon the post-World War II role of the U.S. in world politics.

I hasten to add that between 1960 and 1964, I was a member of the German S. D. S., or rather that faction of the Socialist Student Association that made sociology their intellectual program for opposing the Vietnam War and deeming possible a better world deriving from the Germans having learned the lessons from the Holocaust. Needless to say, I had to defend such an idiosyncratic view of Parsons against some of my comrades, who tended to see him as an arch conservative. Eventually, I abandoned the S. D. S., but not Parsons. My archival work started seriously in the late 1980s, when I felt that the Weberian roots of Parsons had to be unearthed, and the political message in his sociology needed better understanding.

Doing the intellectual biography of Parsons, I can say, has been a keen interest of mine for nearly two decades. To be sure, my original idea was to find details about a Conference on Germany After the War in the spring of 1944, for which Parsons wrote the seminal paper outlining the transformation from Nazism to post-war democracy in Germany, *The Problem of Controlled Institutional Change*. An eventual outcome was my collection

Talcott Parsons on National Socialism published in 1993. Subsequent to that book, I wanted to show that the knowledge interest according to which Parsons as a sociologist defended American democracy was valid. Abandoning a standpoint that failed to see how he focused on anomie as well as integration, a two-pronged structure of social action, seemed vital. So I embarked on researching Parsons's papers – decade by decade – over more than twelve years, to find evidence that went into, citing the original subtitle of the book, *Four Chapters of an Intellectual Biography*. To be sure, the book shows how defending democracy against its enemies was Parson's aim throughout his work. In the 1930's, as he produced *The Structure of Social Action*, he opposed Spencerian social Darwinism. In the early 1940s as he made National Socialism the epitome of anomie, he focused on origins of prejudice and paths of reconciliation. In the late 1940s, he addressed science in connection with U.S. politics in the wake of the deployment of the atom bomb. Last but not least, in the 1960s, as Chapter IV argues, he conceptualized civil society introducing the triad of pluralization, upgrading, and intellectualization. The two chapters that I shall add one day to the book as it stands, should deal with the 1950s, a decade when he embraced psychoanalysis, and the 1970s when he revised his entire approach in order to deal with the new biology connected with the work of, among others, Ernest Mayr. To be sure, doing the intellectual biography of Parsons, for me, is not a finished task but, rather, an ongoing endeavour. The more than 10,000 copies of material from the Archives that I had duplicated by the Harvard Imaging Services are an indispensable source for further inspiration.

My working from the archival material in doing the intellectual biography of Parsons, over a period of nearly two decades, has made it exceedingly clear to me that social theory that refuses to take into account the historical circumstances of the society it analyzes, is an entirely unsatisfactory undertaking.

Section on History of Sociology Mission Statement

The purpose of the Section on the History of Sociology is to provide a forum for sociologists and other scholars interested in the study of the historically specific processes shaping the development of sociology as a profession, an academic discipline, an organization, a community, and an intellectual endeavor. The Section serves its members as a structure 1) to disseminate information of professional interest, 2) to assist in the exchange of ideas and the search for research collaborators, 3) to obtain information about the location of archival materials, 4) to support efforts to expand such research resources and to preserve documents important to the history of sociology, and 5) to ensure that the scholarship of this group can be shared with the profession both through programming at regional and national meetings.

Introduction to the Didactic Seminar on Methodologies of the History of Sociology

Charles Tilly

Columbia University

Presented at the American Sociological Association Annual Meeting, Montréal,

August 12, 2006

During the heyday of Social Relations at Harvard, our convener Ed Tiryakian and I met as graduate students there. Although the program's ambitious scope exposed us to personality psychology, social psychology, and social anthropology as well as sociology, its vigorous vision of the social scientific future did not include history as a discipline. Nevertheless, the faculty offered at least three contrasting models of working relations between sociology and history. My initial mentor Pitirim Sorokin constructed grand historical schemes in which historical research served mainly as raw material for the detection of successive socio-cultural stages. George Homans, in contrast, alternated between observations of contemporary social interaction and skilled historical analyses of such topics as Medieval village life and migration of cultural traits from Frisia to East Anglia. Barrington Moore Jr. took a different tack from Sorokin and Homans, plunging into comparative history to answer questions concerning political processes including, of course, the social origins of dictatorship and democracy. Fatefully for my own subsequent work, Homans and Moore ended up co-directing my doctoral dissertation on the French Revolution.

We might call the Sorokin, Homans, and Moore versions of history-sociology relations Epochal Synthesis, Retrospective Ethnography, and Critical Comparison. In epochal syntheses from Gianbattista Vico onward, great phases and types of human experience serve both as the object of analysis and the settings within which to place smaller-scale phenomena that require explanation. In retrospective ethnography from Montesquieu onward, empathetic reconstruction of alien times and peoples uses current understandings of social processes to show how those instances fit into a known range of variation. In critical comparison from Alexis de Tocqueville onward, a salient question concerning origins and causes guides the logical confrontation of dif-

fering historical experiences. Epochal syntheses have lost most of the favor they once enjoyed in sociology, but retrospective ethnography and critical comparison remain as two competing visions.

Epochal syntheses do not require empathy with historical actors or even closely reasoned comparisons, but they do depend on strong theories of what drives human change and variation. Although they fall far from my own historical practice and even frighten me because of their easy misuse, I predict a revival of epochal syntheses in sociology as biology's evolutionary models and findings become increasingly dominant in public discourse; why should sociologists let the world's Jared Diamonds monopolize the discussion? In any case, retrospective ethnography and critical comparison continue to struggle for the souls of historically oriented sociologists.

To an important degree, the struggle pits dispositions against processes. If you believe that individual and collective dispositions constitute the fundamental causes of social behavior, you will be trying to explain historical events by reconstructing their participants' motives, emotions, and states of consciousness. Retrospective ethnography lends itself to that reconstruction. If, in contrast, you believe that cognitive mechanisms only take their places as social causes among a wider range of environmental and relational mechanisms, you will more likely move to process accounts of historical events. Critical comparison lends itself to the identification of mechanisms and processes that make a difference.

In the seminar we are conducting here, history means mainly intellectual history. But the same choices apply. Analysts of intellectual situations, change, and variation can adopt epochal syntheses, retrospective ethnography, or critical comparisons. Not having yet

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heard the papers, I can only guess at their distribution among the three endeavors. Let me guess that we will hear little of epochal synthesis, but witness an interesting confrontation between retrospective ethnography and critical comparison. An intellectual biography of Pitirim Sorokin or Talcott Parsons easily draws on retrospective ethnography, but histories of American sociology, women sociologists, and the Southern Sociological Society – not to mention general discussions of research issues in comparative histories – makes primary concentration on the reconstruction of motives, emotions, and states of consciousness an awkward base for explanation. To be sure, I also guess that some of our contributors will attempt some of each: placing empathetic reconstructions of thinkers' situations within critical comparisons. But that hybrid effort will turn out to produce its own perplexities when it comes to tracing causal chains. Let's see if it works out that way.

No intellectual historian myself but an enthusiast for comparative-historical analysis in sociology and elsewhere, let me shift the ground to an area where retrospective ethnography and critical comparison clearly do compete with each other. Looking at the field they define broadly as historical sociology, Julia Adams, Elisabeth Clemens, Ann Shola Orloff, and a host of contributors have recently produced an impressive volume of critical and synthetic essays. They call their book *Remaking Modernity*. The hefty book undertakes two related tasks: to interpret changes in the practice of historical sociology, broadly defined, since World War II; and to make the case for culturally situated interpretation as a superior alternative to the deterministic, externalist accounts of social processes most of the book's authors see as having prevailed during the later twentieth century. As method, interpretation clings to retrospective ethnography, just as much of the previous work Adams et al. criticize clung to critical comparison.

According to the editors, the volume's contributors belong mainly to a third wave of postwar historical sociology. The small first wave, including such scholars as Barrington Moore Jr. and Reinhard Bendix, rejected the presentism and modernism of sociological contemporaries, notably including Talcott Parsons. A substantially larger second wave surged during the 1970s, organizing around questions (although not necessarily answers) posed by historical materialism. While displaying considerable respect for first wave pioneers, Adams, Clemens, and Orloff treat the second wave as hegemony to be toppled. The second wavers, they claim, still cling to the illusion of settled modernity. What is more, they defend their obsolete conceptions by means of intellectual power plays:

Historical sociologists, like other academics

and intellectuals, have unconsciously depended on this sense of settlement, of achieved modernity, and are disoriented by its loss. So it is natural when they react with nostalgia for old totalities, a past of imagined theoretical stability, or with a sense of perceived threat – by policing the boundaries of intellectual inquiry to try to forcibly settle things anew or by simply refusing to debate or consider new ways of thinking.

As a named member of the first and second waves, I winced to read about our alleged misconceptions and misdeeds. My mission here, however, is not to defend myself and my second wave companions, but to portray the vision of relations between history and sociology implied by the Adams-Clemens-Orloff analysis.

The third wave of the 1990s and thereafter, according to this chronology, rejected Marxist problematics in favor of an emphasis on culture, consciousness, and interpretation. Accordingly, "both actors and the relationships among them are understood as profoundly constituted by culture and historical conjuncture, rather than as reflections of some underlying system of economic relations." In Richard Biernacki's version, for example, the shift from second wave to third involved moving from means-end reasoning to the reconstruction of situations within which social actors act. Action becomes not the pursuit of well-defined ends by instrumental means, but a "problem-solving contrivance."

Since, as Biernacki points out, Max Weber organized much of his analysis around means-end schemata, a surprise awaits the reader of *Remaking Modernity*. For the book's most widely discussed and cited author is none other than . . . Max Weber! In this book, Weber thrives, Marx dies, while Foucault and (more surprisingly) Emile Durkheim survive as sources of inspiration.

Weber attracts these theorists for two separate reasons: because he stands as the quintessential historically informed sociologist, and because his version of means-end analysis places the conscious actor at center stage. Foucault occupies such a large place, according to the editors, because he "captures the historical emergence of normalizing discourses and 'technologies of the self' and traces the processes by which they are embedded in and help create a range of disciplinary complexes." Thus Foucault, for third wavers, provides a connection between ambient culture and situated social action.

Durkheim likewise provides retroactive ratification for a third wave position. Durkheim, "abominated" by the second wave according to the

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editors, returns as the patron saint of social determination for cognitive categories. The book's retrospective ethnography centers on the image of conscious human actors who actively organize their worlds using materials supplied to them by the ambient culture. To that extent, they remain prisoners of available language and (to use a term the book draws repeatedly from Pierre Bourdieu) doxa. We begin to see why the authors devote so much energy to bashing the interest-based analyses of second-wave Marxism. Interests derived from locations within social structures contradict culturally embedded phenomenology as the fundamental explanation of social action.

By a circuitous route, comparative-historical sociology thus returns us to intellectual history, and more specifically the intellectual history of sociology. In both fields, the debate between retrospective ethnography and critical comparison continues.

TIMELINES



Historical Methodology: Comparative and Macro

By Jennifer Platt, University of Sussex

My remit was macro and comparative work. I have chosen to treat this as including foreign and international topics, rather than focusing only on strictly comparative work, as there are interrelated issues which cut across those boundaries. Readers may draw on such material only as background to their central topics, but most of the basic points to be made remain essentially the same for such uses. The experience I draw on is work on the history of sociology in countries foreign to me (especially the USA!), which has sometimes made comparative points as part of the discussion, and on international social-scientific associations. I have also edited and contributed to collections, with authors from a number of countries, which have aimed to suggest comparative conclusions.

First, an issue not raised as often as it should be in methodological discussion: what questions are appropriate to ask? A key general issue is that different national sociologies have been – though in varying ways and to varying extents - parts of world sociology. They have been institutionalised at different periods, under different local historical circumstances. How different have they been in consequence? Diverse models of the relation of national sociologies to international sociology have been, or could be, proposed – for instance:

- Local historical specificity: each case is distinctive, for reasons arising from its unique historical conjunctures.
- US hegemony: US intellectual, financial and numerical dominance overrides minor local specificities. (This version can be modified by a Bowling Alone effect, in which that hegemony is seen as a feature of a limited historical period around 1945-65.)
- More complex centre/periphery models: it is recognised that, for instance, former colonial powers may

act as metropole for their ex-colonies, or migrants may still look to their country of origin, so that there is more than one ‘centre’.

- Cohort effects: national sociologies continue to bear the mark of the historical periods at which they first became institutionalised and, even if they follow similar developmental trajectories, reach equivalent points at different periods
- Globalisation: national boundaries are losing their social meaning, and the same patterns and influences are present world-wide.
- ‘New Institutionalism’: for reasons stemming from the characteristics of the international institutional setting national sociologies all are, or will be, essentially the same.

Such models may be the starting points of research, or may be finishing points. Whichever they are, it is important to note that, despite their obvious elements of overlap, or the possibility that more than one might sometimes apply in part, they are not the same; explanations need to distinguish between them, which has implications for the data required.

Some such models appear in evaluative grand historical narratives: for instance, the triumphant spread of science, the vicious effects of imperialism which need to be overcome by indigenisation, the sad failure of sociology to establish a sufficiently critical approach. Such evaluative approaches tend to distract attention from specifically historical issues, but can still be a useful source of hypotheses. One function of the history of sociology has been to create ancestor myths; another function is to correct them.

Design issues

As other authors in this symposium emphasise,

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the traditional model of research design in which a hypothesis is stated and data collected to test it has important intellectual advantages. However, that tradition is not necessarily helpful for historical work, and certainly is not viable as a model of the whole process; there is a real place for descriptive work, as a prerequisite for the formulation of plausible and appropriate hypotheses. I argue that general saturation in period and topic is an almost essential stage for adequate understanding of material initially unfamiliar in both time and place. This applies to sources too, since in historical work one cannot create the data, but has to work from what is already available. It is no good formulating a great question which the sources cannot provide data to answer. A strategy of starting from the sources and seeing what questions they suggest may in practice be best. Thus a programme of work, in which one topic deepens one's knowledge and leads on to the next, may be a better model than a single project. That disadvantages those starting work in the field, but even limited studies can be useful bricks in the collective wall. Despite the constraints posed by the availability of material, the choice of cases to study is still an important methodological issue. Comparison is crucial for explanation. An explanation may look obvious when a single case is studied, but for a national sociology the counterfactual conditional - what would have happened had things been otherwise - is likely only to be found abroad, and may disconfirm the initially 'obvious'. The outcome may turn out to have been essentially similar when local circumstances were different, or different when at least some of them were similar.

In comparative work, it is particularly important to try to choose cases appropriate to the problem - which will not always be the same ones. For example, if the consequences of being a small country were to be examined New Zealand, Finland and Singapore might be a good set of cases, or for the effect of Catholicism maybe Ireland, Quebec and Italy? Note, however, the obvious point that while if Catholicism as such, for example, has specific effects those must appear in otherwise diverse Catholic cases, but one can only be confident that it is Catholicism which has produced the effects if they are not also shown in mainly Moslem or Protestant countries. When such a hypothesis has been developed on the basis of initial study, the practical strategy may in effect be a grounded-theory one, in which further cases are chosen to test emerging interpretations.

Some data sources and how they can be used

Archives

Material located abroad is likely to be more expensive to

access, so to identify strategic archives can be particularly important in minimising costs. Some general US archives are more useful than one might expect on international issues; for instance, the archives of the Ford and Rockefeller Foundations both contain extensive material not just on research which they funded abroad, but on the general state of the social sciences in countries which their representatives visited. (Remember, too, that an archive of an individual's papers commonly holds substantial materials sent by other people, so it does not provide data only on the focal person.) Sometimes less useful than one might expect for creating time series are the archives of bodies whose key staff, or office base, have repeatedly changed, because practices (and languages) are likely to have changed too. Particularly attractive are those archives, such as the Rockefeller one, which provide research grants to enable potential users to visit them. Similarly, the Canadian government offers grants for research trips to Canada, and some European countries have similar arrangements (though those may be confined to countries with which they have exchange plans, or nationals of other EU members.)

When planning research trips, it is important to bear in mind that people playing central roles internationally are often not the same as the most prominent national intellectuals, or the best known writers. The ideal to aim for is archives which represent key persons who were active networkers, whether by temperament or by formal role; Stein Rokkan can be taken as a fine example, actively connecting a variety of cross-national institutions. Details of a few archives which I have found particularly helpful for work on international topics, especially the history of international associations, are in the appendix.

Secondary data

Some easily accessible sources have been surprisingly neglected. One is the extensive international system of data archives; these have now existed long enough to be mined for historical data to track changes in research practice, or in topics and concepts in substantive areas. Another, published, source is data collected on the state of sociology in the past, or impressionistic general reviews of the situation; these were contemporary at the time, but now they can be used historically, perhaps to construct a time sequence which, though not offering ideally exact comparisons, is much better than nothing.

Some rather less formal sources may be sug-

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gested which can also be of value:

Being an insider already

This obviously has its weak points, and makes it especially important to look out for other perspectives, but it certainly helps to know something about how an institution works in practice (rather than in the formal paperwork on procedures), to be familiar with the local myths and ideologies, and to have an idea where some of the bodies are buried.

Hanging around the office

The value of this was an accidental discovery, made when I was examining files in the small, cramped office of the International Social Science Council in the UNESCO building in Paris. I could hear what everyone was saying, to each other or on the phone, and so observed what languages they used to each other, how they dealt with daily bureaucratic practicalities, how different UNESCO workers form a community cutting across formal boundaries between nations and organisations and may often change jobs within the internal labour market... I felt that I got a much better feel for what the organisation was like in this way.

Hanging around the subject

Saturation in trivia of no evident intrinsic interest often in practice turns out to be useful later, if only in helping to understand what is meant on some small point, or in recognising a standard rather than a personally chosen phrase. Names provide some examples. When a Leopold is referred to in informal letters as Polly, or Jean Marie can be a man's name in French Canada, one may be misled about gender relations if one does not realise this; if one does not know that there have been two Howard Beckers in US sociology, or John Goldthorpes in British sociology, one risks producing some curious historical connotations.

Substantive issues with methodological consequences

Substantive issues often have consequences for methods and potential interpretations of the data. Examples below are drawn from experience in my own areas of work but are, I hope, suggestive of the sorts of problem that one may need to deal with in other areas too.

They do it differently in other countries

We all know that things are different elsewhere, but may not realise some of the implications. If, for instance, state control of higher education is direct in one country

and indirect in another, to start from the former and treat the latter as showing absence of state control is likely to mislead. There may be no way in which the specific character of different countries can be described on the same variables to ensure precise comparability, though sometimes there can be plausible arguments about the functional equivalence of, say, religion and official communism. The 'comparative' model where several authors each write about their own country is liable to have this problem, as they treat different aspects as relevant. Each author, too, needs to know something of how other units differ to identify what is worth mention, which may not be the same as the features appropriate for a domestic audience.

How to characterise large units

To characterise whole national sociologies, or periods of history, only makes sense if there were indeed some truly shared general features. Macro-level institutions, or major historical events such as wars, qualify easily, but compositional features such as which theories were intellectually dominant, or how women differed from men, pose greater problems. Articles in leading journals, or the careers of great men (sic) and their schools, are easiest to study, but cannot be treated as representative without confirmation from wider data.

International people

The people active in international organisations may formally represent their countries, but cannot be taken as representative of them in any sampling sense. Even if they were typical national sociologists to start with, which many have not been, the experience they gain from their international role is likely to make them less typical.

Misleading documentation

It is tempting to use lists of committee members, and minutes recording policy decisions, as indices of influence and policy. But some committee members do not attend meetings, for reasons such as travel costs, visa problems, preoccupation with their prestigious visiting professorships... In addition, shortage of funds may mean that decisions in principle cannot be put into practice (and minutes may not record later what was *not* done). The converse also applies – a surprising proportion of activities relating to Brazil took place in the ISSC when a Brazilian millionaire was its president, though that represented no formal policy.

When a body reports to super-ordinate funding bodies, or to a public in whose eyes success is important

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to its legitimacy, window-dressing is to be expected, and failures are less likely to be reported. Successes, on the other hand, are likely to be claimed as the responsibility of every body in any way involved: whichever first approved a proposal in principle, all contributors to funding it, associations which found researchers to collect the data, the universities to which the researchers belonged...

Representation of (minority) groups

Formal structural features of organisations may lead to apparent under-representation of some groups, which can be quite misleading if taken to indicate prejudice or discrimination. In the ISA each 'Research Committee' [section] has one representative on a council which votes for executive members and has policy responsibilities. Members do not distribute themselves randomly across RCs, which means that those who cluster in particular areas – for instance, women in the RC Women in Society, or people from the global South in RCs associated with development – will for that reason have fewer representatives at the next level up than their total individual membership of the ISA might suggest was appropriate.

Relations between organisations

There is a system of relationships among national and international social-scientific bodies: the ASA belongs to the ISA which belongs to the International Social Science Council, and it also belongs to COSSA with other US social-science associations. The ISA has competed with the International Institute of Sociology, and the SSSP was founded from dissatisfaction with the ASA. Individuals' memberships represent something about their training, interests and career strategies; their directions may also sketch a pattern of hegemony. Arguably there is a total social system of sociology, and we cannot fully understand particular parts unless we take into account their location within that system.

Some of the points made above amount to urging that there is no clear boundary between the history of sociology and the sociology of sociology. Let us strive to make our historical work also be a salient part of sociology in general!

Appendix

Some good archival sources for international sociology

US sources

Rockefeller Archive Center: <http://archive.rockefeller.edu>

Contains records of the Rockefeller foundations (which

in the interwar period sometimes seem to have funded almost everything), but also of some other bodies such as the Social Science Research Council. Extremely helpful staff, pleasant rural setting. Located in upstate New York, an easy train journey from New York City.

Ford Foundation: archives are very hard to find on the internet, but the address is 320 East 43rd Street, New York, NY 10017

Robert Angell papers: <http://www.hti.umich.edu/cgi/f/findaid/findaid-idx?c=bhlead&idno=umich-bhl-85849>
University of Michigan's Michigan Historical Collection, Bentley Historical Library, Ann Arbor, Michigan.

Reuben Hill papers: <http://special.lib.umn.edu/findaid/xml/uarc00411.xml>
University of Minnesota Archives, Elmer L. Andersen Library, University of Minnesota, Minneapolis.
These two sociologists were both presidents of the ISA, and Angell also directed UNESCO's Social Science Department.

Sources in Europe

IISG (Internationaal Instituut voor Sociale Geschiedenis - International Institute of Social History) at Cruquiusweg 31, 1019 AT Amsterdam, The Netherlands.
ISA archives: <http://www.iisg.nl/archives/nl/files/i/10751882full.php>, but it also has many more materials some of which might be of interest. Everyone speaks English.

Stein Rokkan papers: state archive in Bergen - Statsarkivet i Bergen, Årstadveien 22, 5009 Bergen, Norway. Web site is rather Norwegian, but everyone speaks English, and e mail will reach it at statsarkivet.bergen@arkivverket.no
Has very extensive Rokkan papers, relevant to political science as well as sociology, many international ones in English.

UNESCO <http://www.unesco.org/general/eng/infoserv/archives/guide.shtml>, located in UNESCO HQ at 7, place de Fontenoy, 75352 Paris 07 SP, France.
UNESCO played a key role in establishing the post-war system of social-science associations, as well as having its own social research. Again English is spoken, and much is available in English, though some French helps.

London School of Economics archives: <http://catalogue.lse.ac.uk/cgi-bin/Pwebrecon.cgi?DB=local&PAGE=First>, in central London. I have not used these archives for international material, but they have papers from Morris Ginsberg, T. H. Marshall and Tom Bottomore, all of whom were very active in the early ISA; Marshall also directed UNESCO's Social Science Department.