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Monkkonen, Eric H.

Veröffentlichungsversion / Published Version

Zeitschriftenartikel / journal article

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### Empfohlene Zitierung / Suggested Citation:

Monkkonen, E. H. (1994). Lessons of social science history. *Historical Social Research*, 19(3), 140-146. <https://doi.org/10.12759/hsr.19.1994.3.140-146>

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## Lessons of Social Science History

*Eric H. Monkkonen\**

**Abstract:** This paper presents the presidential address to the eighteenth annual meeting of the Social Science History Association (SSHA). Three things we can learn from the characterization of the group's organizational and intellectual history. The first lesson we can learn that social science historians are completely unable to predict their own future. For example, it is now clear that the early optimism and aggressiveness as well as fears of mainstream exclusion of the 1970s were excessive if not unwarranted. Social science history quickly did become a highly respected mode of historical and social science research. The second lesson we can take from the group's history has to do with the erratic session sizes. These tiny sessions often turn out to have been intense, memorable, and significant learning experiences. The third lesson we can learn is that our narrow carefully structured interests and research have more weight and lasting ability than their immediate impact may index.

I am pleased to be able to address this, the eighteenth annual meeting of the Social Science History Association: I have many valued memories of presidential addresses, but my favorite was Jerry Clubb's 1984 talk in the Chinese restaurant in Toronto, where speakers, waiters, and many other patrons all competed in a cacophonous, noisy free-for-all. Jerry did not even try to finish the talk, so we had to wait until it appeared in the journal (Clubb 1986). This memory perhaps is followed by the first official meeting in 1976, when the University of Pennsylvania hosted a reception in what I remember as an Egyptian tomb, where we all felt crude and out of place. And this memory is jostled by other SSHA conferences, where meeting engaged scholars from other disciplines at roundtables at lunch forced me, at least, to learn, to stretch, and, finally, to appreciate other research paradigms. And of course there are other memories, especially those of the many sessions at which the panelists out-

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\* First published in: *Social Science History* 18:2 (Summer 1994), p. 161-168.

Address all communications to Eric H. Monkkonen, University of California L.A.,  
Dep. of History, 405 Hilgard Avenue, USA-Los Angeles, Cal. 90024-1473.

numbered the audience. These personal memories do not really mark the history and trajectories of this organization, which I will briefly rehearse.

Incorporated in Michigan on 1 October 1974, the SSHA reflected the efforts of scholars brought together by the Inter-university Consortium for Political and Social Research (ICPSR) and the American Historical Association's (AHA) ad hoc Committee on the Use of Quantitative Data in History. From early mailings of these organizers, it appears that the people involved had two or three different motives, ranging from Lee Benson's wish to use social science to change the world, to other, somewhat more modest desires to enlighten historians (and historical journal editors) about the value of statistics and theory, to a desire to shake up crudely historical social science. These latter wishes were based on the growing achievements in political research on long-run voting series, the significance of historical research in economics, and in history the attention-grabbing successes of mobility studies. In its earliest years a good proportion of the group represented the feeling that quantification equaled social science and that social science history was going to conquer the world. This was a period marked by small meetings - about one-third the size of this one - few members, and a sense of mutual exclusion from the professional mainstream coupled with a completely unjustified optimism.

The early 1980s saw a period of clustering (or, some might say, fracturing). The subspecialities grew more technical, and distance between them increased. I can't prove it but will propose that the conferees attended fewer and fewer sessions outside of their own expertise, that as interdisciplinary exchange increased, substantive and thematic boundary crossing diminished. Much of this was the simple consequence of collective growth in technical standards: the noninitiated or untrained could not participate as was possible when there was less technical sophistication.

It was then that the world-beating optimism declined, to be replaced by a tone of pessimism about our place in the intellectual world. (I note that this pessimism was as irrational as the earlier optimism: the larger world didn't even know of the SSHA's existence in either era.) But precisely at this moment, in 1982, Bob Fogel, that great counterfactualist, in his carefully researched presidential address showed that members of this small, interstitial if not downright marginal organization were actually prospering, getting jobs and even tenure. Some here must remember that talk: as I recall, Fogel's assertions were met with nods of approval, expressions of astonishment, and a strong leaven of disbelief.

More recently, this organization has entered an era of continual surprise: some maturity (Dan Smith's remarkable attempt when he was president to try to make the organization grow up and restructure by surveying many committees, ex-presidents, and networks and then writing up his analysis and recommendations, as « Needs of a Mature Organization<sup>^</sup>»; an increased organizational complexity and scale that have made us turn to professionals to organize

our local arrangements; a need to forgo institutional memory to make things happen for actual written records (as in Barbara Hanawalt's canonical and essential text, »Lore and Practice for the Social Science History Associations which tells us how we do things); the unexpected emeritus status of many of the organization's founders; the continued and not completely understood numerical growth; the flourishing of the networks; and the turn of some of our members to narrative methods which explicitly or implicitly reject the quantitative orientation present at this organization's birth.

What three things can we learn from this characterization of the group's organizational and intellectual history? First, we can learn that social science historians are completely unable to predict their own future. For example, it is now clear that the early optimism and aggressiveness as well as fears of mainstream exclusion of the 1970s were excessive if not unwarranted. That is, while SSHA members became neither the force for social change nor the singular way to do world-beating research, social science history quickly did become a highly respected mode of historical and social science research. The recent series of articles in our own journal on history and the social sciences makes the point abundantly clear. Andrew Abbot, Susan Kellogg, Hugh Rockoff, Richard Dennis, and David Robertson's surveys of sociology, anthropology, economics, geography, and political science establish the now very large scope and impact of our collective activity (Monkkonen, 1994). Only one of these authors is in a history department, I note. If not mainstream, social science history is now an accepted component of all of the social sciences. It has overflowed whatever boundaries that existed when it began. As another example of our own unpredictability, it is also clear that the more recent pessimism of the late 1980s, the fear of being eclipsed by discourse analysis, was followed by growth and organizational maturity. The organization's cash assets in 1975 were \$6,467; today they are, well, a little more than that. Its most pressing internal problems fall on the shoulders of the program chair and cochair, who must manage the requests of nearly 700 scholars.

The second lesson we can take from our own history has to do with the erratic session sizes. I hope that I am not the only one here who has participated in sessions so tiny that the handful present all wonder if they are not the victims of some nasty cosmic joke. Especially painful are those occasions when it turns out that one-fourth of the audience of four has inadvertently come to the wrong room. Yet, it is these sessions that often turn out to have been intense, memorable, and significant learning experiences. Of the many such sessions I have participated in, I regret sticking with only one, in Rochester, where I was one of the few to miss watching the building next to the hotel being blown up (by the Xplo Corporation).

The third lesson we can learn is that our narrow, carefully structured interests and research have more weight and lasting ability than their immediate impact may index. For instance, the early work of young demographic historian Allan

Sharlin, in whose name we make an annual award still animates research in urban history. Jan DeVries, Lynn Lees, Paul Hohenberg, and many others working in European urban history now build on Sharlin's innovative argument that early modern cities were not demographic deathtraps but could and did sustain modest growth without immigration.

So far I haven't mentioned the most puzzling aspect of this organization and its members' activities, one which troubles some of its members and journal readers on occasion. What is social science history, anyway? This usually unasked question is bound to embarrass and throw many of us into confusion in a manner similar to that of urban historians when asked to define a city. It is fun to say that in its beginning, many members of the organization (including me) thought that social science history meant quantitative history which used statistics to estimate and evaluate, or that at the least it included inherently quantifiable arguments and evidence. Of course, they knew very well that much of social science and of history was not quantitative, and what they may have meant was history done so as to not be anecdotal, determined by the whims of the day or past journalists. On the one hand, the words like »systematic,« »rigorous,« »consciously theoretical,« »comparative,« and »longitudinal« (all from the opening statement in the organization's journal), characterize their original intentions. On the other hand, the opening statement which appeared in the first journals and still appears also averred that quantitative analysis should be used »when appropriate. This qualifier indicates that the group was never quite as maniacally single-minded as it may have seemed.

Now, in recent and very different articles in *Social Science History*, both Bill Sewell and Randy Roth have critiqued these rather fuzzy and implied definitions of social science history, Sewell starting with the »literary barbarism« of the phrase »social science history« and Roth with bad science (Sewell 1992: 480; Roth 1992). Sewell argues that in their »early years« social science historians rejected the humanities as atheoretical, but that the humanities at that very time were becoming more theoretical. And then, in the late 1970s and 1980s »many social science[tist?] historians became dissatisfied with the intrinsic limitations of quantification« and »turned« toward cultural anthropology or literary theory (Sewell 1992: 481). The essence of Sewell's critique, then, is not so much that social science historians or their practice was worn out, or impossibly ambitious, but that it was limited. He would argue, I guess, that quantitative methods and whatever else marks social science history off from other kinds of history are less often appropriate than this organization's founders and many of its members might think.

Roth, in contrast to Sewell, urges us to look more carefully at current scientific practice. He argues that social science historians must exert care not to mimic a 1920s description of science, associated with the hypothetico-deductive method as described by Hempel and other positivists. Instead, we should pay more attention to the past twenty years of the sciences, to developments in

nonlinear models. We would learn that the linear models we tend to use so exclude feedback (much less human consciousness) as to caricature beyond utility the nature of processes in nature, much less in society. That is, while parsimony may be a good thing, it should not preclude appropriate complexity. Roth shows how even in small populations, social change can oscillate in nonlinear, seemingly chaotic ways. These ways can be modeled and understood, he argues, but not in the ways of old social science. If Sewell wants us to spend more time visiting literature departments, Roth would have us stop by the biomathematics or perhaps chemistry seminars on the way.

I see no reason we can't do both, though we would be dabblers, no doubt. Intellectual workers cannot possibly master the fundamentals of all crafts they use, especially as the fundamentals themselves undergo constant revision. We typically heed most carefully those calls to rethink our practice which are most generalizable or those which expose a foundation shakier than we knew. The most generalizable ones get the broadest audience and are often the most interesting, even if we don't exactly see how we should apply what we have learned to our actual projects. Those which expose a shaky foundation often scare us most when we know the least.

I learned while editor of this organization's journal that a broad range of definitions of its scope obtain in the scholarly community. What a group of serious scholars is willing to define as social science history extends much farther than any one person might guess. At the same time, I learned while we may not be able to say what social science history is, we can say what it is not. I also learned that challenging critiques and »should« and »ought« arguments keep us alive, self reflexive, and lively. There is in the kind of work this loose organization encompasses a remarkable flexibility, because once one is an interdisciplinary traveler, one is forever decentered.

My take is that lack of foundational clarity is a virtue, not a vice, and that keeping the flavor of social science history neither clear nor instantly coherent is the key to the health, energy, and meaning of the organization. A look at the programs of very early conferences uncovers this variety. For example, on Saturday, at 1:30 in 1977, one had a choice of sessions on SPSS versus System 2000, civic norms in Buenos Aires, and power and diplomacy in Europe. No narrativity, it is true, but there was a whole session devoted to holism (Susan James, who went on to write *The Content of Social Explanation*!, Australian scholar Dennis Phillips; and philosopher Louis Mink). These annual meetings are a venue for scholars from different disciplines to learn to talk to one another as they address substantive problems. This is a challenging task because successful scholarship is in part successful because of what it shuts out. Therefore, engagement across disciplines will never be easy. But if our sessions can bring together social scientists on specific topics, then social science history is working.

Size of sessions has little relationship to what gets learned in them or to their quality or even impact. In fact, given our aspiration to address substantive and

theoretical problems using the strengths and methods of the different disciplines, small sessions may be an essential element of this organizational project. Few of our discipline-oriented colleagues will ever submit their work and egos to the close criticism of a substantively knowledgeable person from another discipline. Let us say it: this can be tough, and small sessions can make the scrutiny even closer and tougher. I hope that the SSHA continues to provide a setting where the small session can persist.

Which leads me back to the optimistic and clear-eyed beginnings of this organization. What its founders wanted has turned out to be difficult to achieve and sometimes different from what they had in mind (Bogue 1987; Benson 1984). But the SSHA, in its small scale and its decentralized and flexible use of networks, has proved to be an organization where new ideas can be developed, old ones refined, bad ones forgotten. It has also proved to be a place where people from different disciplines can engage on a level playing field. We are all here because of our research. There are no jobs to be found. When people speak, we have to listen to what they say, because we don't know their status - they are from a different discipline. The rewards for participating in such an organization are in doing better work. It is a rare privilege to be a part of such a group.

### Note

Eric H. Monkkonen is professor of history at UCLA. He received his Ph.D. from the University of Minnesota in 1973. His most recent book is *America Becomes Urban: The Development of US. Cities and Towns, 1790-1980* (University of California Press, 1988). He is now engaged in research projects on homicide in New York City, 1790-1990, and the 1705 census of Turin, Italy.

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