

# TRAJECTORIES

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## From the Chair

### On the Virtues of Analyzing the Particular

**James Mahoney**

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Question: What do nearly all of the best works in the field of comparative and historical sociology have in common (other than their high quality)? Answer: the analysis of particular times and places. If one goes down the list of prize-winning articles and books in the section, one finds again and again works that seek to understand, interpret, and explain specific cases. To take just three recent and quite different examples: In her remarkable work *Economists and Societies* (2009), Marion Fourcade explores the reasons why the profession of economics turned out so differently in the United States, Britain,

and France. Karen Barkey's stunning book *Empire of Difference* (2008) analyzes center-periphery dynamics to explain the evolution and longevity of the Ottoman Empire. And Michael Mann's disturbingly illuminating study *The Dark Side of Democracy* (2005) looks at ethnic cleansing on a case-by-case basis around the world.

The concern with understanding specific places and times makes the field of comparative and historical sociology a "case-oriented" enterprise (Ragin 1987). Unfortunately, one consequence of this case-oriented approach is that work in this field is subject to the charge of generating "merely historical" contributions. Over the years, there have been many times when I have found myself having to explain the value of research aimed at analyzing "particulars" (i.e., specific times and places). In this short essay, I summarize five different responses concerning the value of interpreting and explaining specific historical cases (the list is hardly exhaustive). These answers call attention to the virtues of analyzing the particular—virtues that may not characterize research that seeks to generalize broadly about trends for large populations of cases.

**Virtue #1:** *Knowledge about particulars is valuable for its own sake.* Here it is important to recognize a basic difference between work that grows out of the experimental template (including most large-N statistical research) versus much comparative and historical sociology. Under the experimental template, the goal of the analysis is to estimate the average effect of a given treatment or variable across a large population of cases. One attempts to say something about causal effects within a population, not within specific cases. By contrast, with comparative and historical research, the goal of the analysis is often precisely to explain the occurrence of specific outcomes in particular cases (or to interpret the meanings of specific events and processes in particular cases). The findings thereby generated are intrinsically valuable. For example, we read Barkey's book in part because we want to know why and how spe-

cifically the Ottoman Empire managed to persist so long (and why it evolved in the ways that it did). The book succeeds in part because of what it teaches us about a historically particular case.

**Virtue #2:** *The analysis of particulars generates new theory.* The close engagement with case material that characterizes comparative and historical research is invaluable for concept formation and generating new theory. While methods can structure an analysis, they cannot provide concepts, orienting ideas, and hypotheses. These must come in part from the scholarly imagination, which in turn is stimulated by specific case knowledge and the effort to make sense of particulars. For example, while Fourcade's work is focused on the history of the economics profession, it tells us something quite important about how American, French, and British society and culture work in general. Likewise, while focused

on the Ottoman case, Barkey's work ends up providing a kind of general recipe for starting an empire and sustaining it.\*

It is important, also, to be clear about the value of theory generation—which is sometimes assigned a secondary status next to theory testing. From the standpoint of comparative and historical research, the "problem" of analysis is often precisely one of

theory generation. That is, at the onset, we lack fully adequate concepts and hypotheses for interpreting and explaining our cases. We therefore cannot work under the illusion that good theories already exist and can be mechanically applied or tested. We must, instead, reformulate existing theories and create new concepts in order to make sense of the cases under study.

**Virtue #3:** *The study of particulars in the past provides important lessons for the present and future.* Real-life concerns and choices often

*Just as detectives and juries can arrive at reliable inferences about particular events, so too can comparative and historical researchers.*

\* How to start and sustain an empire: (1) begin at the boundary of multiple subsystems, (2) use your network position to establish new cultural forms at this center position, (3) redistribute resources in exchange for allegiance, (4) create institutions that allow adaptability, (5) permit multiple cultures and kinds of rule in periphery zones, (6) eliminate peripheral elites that resist change and slowly incorporate sympathetic elites into the center, and (7) don't allow periphery elites to work together.

involve assessments about processes whose outcomes are driven by the complex intersection of multiple events and actors. Empirical studies that focus on the net effects of single variables may not be useful for choosing wisely when confronted with such processes. Comparative and historical studies that examine past trajectories involving this complex intermeshing of separately determined processes may provide more insight. For example, although Mann's book is focused on explaining specific cases, he provides much useful advice for thinking about how past processes of genocide will (and will not) continue into the future. He ends up concluding that nearly all future genocides will occur in the Global South. Consequently, policy needs to be directed at poorer parts of the world, not at the societies in the Global North that have historically been the location of genocides. Mann's account also generates some specific policy recommendations to avoid the murderous history of the North. At the top of his list is the creation of a World Criminal Court with full U.S. backing, which, while perhaps unlikely, would go along way toward preventing the most atrocious violence.

**Virtue #4:** *The study of particulars in the past provides normative lessons for the present.* As legal scholars are well aware, general normative principles about what is right and wrong are often derived from the analysis of specific instances—i.e., new cases generate new principles. Comparative and historical researchers routinely develop arguments from past cases that are relevant to political and normative evaluation in the present. For example, Fourcade's analysis of economists leads to a more profound normative conclusion than the familiar critique that economic models do not fit reality very well. Her work suggests that universal economic models are actually constructed in quite different ways, depending on national context. In turn, these differences strongly shape the capacity of the economic models to be used in specific ways, for good or ill. In the case of the United States, the professionalized and rational-choice orientation of economics helped the discipline align with the private sector and the state. From this position, economists in the U.S. can influence basic normative standards, including the definition of concepts such as "discrimination," "pollution," and "welfare."

**Virtue #5:** *We may be able to do a better job of explaining outcomes in particular cases than generalizing about average causal effects for large populations of cases.* Although well-designed experiments can do an excellent job of estimating the average effect of a treatment within a large population of cases, studies that must rely on observational data are fraught with problems related to correctly specifying the causal model. Comparative and historical studies avoid certain obstacles by focusing the research goal on explaining or interpreting particular cases (rather than generalizing about average causal effects). Just as detectives and juries can arrive at reliable inferences about particular events, so too can comparative and historical researchers. In fact, many of the methods used by comparative and historical researchers have more in common with detective work than laboratory experiments. Here is not the place to spell out these methods. But the point remains that inferences about patterns in particulars are often more reliable than inferences about average effects in populations using observation data.

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Finally, a few notes about the section: Our financial status is quite good, due to the excellent management of our secretary-treasurer, Victoria Johnson, and to the judicious use of funds by our past-chair, Elisabeth Clemens. We hope to use some of the existing money for a special reception at ASA 2011 in Chicago. Our section membership is slightly down (668 members by last count), but so is membership for nearly all sections. Nina Bandelj and Isaac Martin are heading up a committee to boost our numbers (my thanks to both of them). Please help out by signing up your students for section membership!

Please also nominate outstanding comparative and historical work for section prizes (see in this issue for further details). I especially call your attention to the new Theda Skocpol Dissertation Award. Dissertation advisors and chairs are strongly encouraged to nominate dissertations defended and filed between January 1, 2009 and December 31, 2010. Further information can be found on our webpage<sup>†</sup> (thanks to Robert Jansen for managing the webpage). I am grateful to the many people who agreed to serve on the prize

<sup>†</sup> <http://www2.asanet.org/sectionchs/>

committees: Nina Bandelj, Bart Bonikowski, Nitsan Chorev, Chad Alan Goldberg, Jeff Haydu, Meyer Kestnbaum, Greta Krippner, Mara Loveman, Isaac Martin, Dan Slater, Lynette Spillman, Matthias vom Hau, Andreas Wimmer, and Jonathan Wystzen. Finally, many thanks to incoming-chair Neil Fligstein for organizing our ASA 2011 sessions and to Emily Erikson and Isaac Reed for editing this newsletter.

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## Methodological Pluralism in Comparative-Historical Research

### Thoughts on Sequences in Comparative Historical Sociology<sup>‡</sup>

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If there are no beginnings and endings, there are no stories.

Virginia Wolf, *The Waves*

Even the most cursory reading of empirical research in comparative historical sociology reveals a profusion of references to the sequential nature of social and historical process.<sup>§</sup> In addition to these empirical studies, ongoing theoretical and methodological debates within historical sociology have centered around the related roles of contingency, interdependence, narrative, and sequences in historical explanation (see, for example, Abbott 1997, Skocpol and Somers 1980, Kiser and Hechter 1998, Mahoney 2000). In spite of all these references (and despite Abbott's best efforts to organize the field (eg., 1992 1995, Abbott and Tsay 2000)), there is still little consensus among scholars about what a *sequence* refers to, let alone how we might best handle sequences theoretically or methodologically. One consequence of this cacophony is that it elides important differences between types of historically observed sequence, and obscures the distinction between sequences that reflect well- institutional-

<sup>‡</sup>My thinking on this topic has benefited greatly from ongoing conversations with Andrew Abbott, Peter Bearman, and Steven Pfaff. Please do not cite without permission. Email: stovel@u.washington.edu.

<sup>§</sup> For example, we see sequential thinking invoked in analysis of the fall of the Bastille (Sewell 1996, Bearman, Faris and Moody 2000); in comparative studies of the French communes (Aminzade 1993); in a variety of diverse analyses of protest activity (Tarrow 1991, Minkoff 1997, Pfaff 1996); in models of careers systems (Abbott and Hrycak 1990; Stovel, Savage, and Bearman 1996, Spilerman 1977); in micro-level models of the interactions associated with lynchings (Griffin 1993), strikes (Biggs 2000), and hierarchy formation (Chase 1980); in a narrative of state building in early modern Europe (Clark 1995); and in a study of Southern lynching (Stovel 2001). Sequences also play a crucial role in the heralded methods known as 'process-tracing' (Goldstone 1997, Mahoney 2008

alized processes and those sequences that are evidence of social restructuring.

In light of this persistent confusion, my aim in this short note is to advance the case for treating sequences as a temporal trace of social structures. The advantage of this approach is that it locates social sequences in more familiar concepts of institutions, interaction, and historical process, and therefore may help focus attention on identifying mechanisms that generate specific sequences.

### What are sequences?

I rely on a theoretically catholic definition of sequences, derived from my reading of historical sociology: *social sequences are empirically observed, temporally ordered regularities*. This definition is intentionally broad, and the first three elements—that sequences are empirical, temporal, and ordered—are all relatively straightforward. Serious consideration of the issue of social regularity, however, provides an important point of departure for understanding the relationship between generating mechanisms and observed sequences—and provides an analytic basis for excluding a whole host of historical moments from the category "sequences."

### *Sequences are empirically observed*

While it is possible, and in some instances perhaps useful, to consider exclusively hypothetical or theoretically derived sequences (see Harding 2003, Morgan and Winship 2007, and Mahoney and Goertz 2006, on counterfactual reasoning), it make sense to begin with empirically observed sequences. Traces of empirically observable sequences are composed of either (1) a set of diverse or heterogeneous states (e.g., jobs held) or events (e.g., protests, elections, coups);\*\* or (2)

\*\* We might also consider a variation on the first type of empirical trace: a set of diverse narrative *elements* catenating into a discrete event or phenomena. Here—as in process-tracing—the emphasis is on disaggregating a single event in order to better understand or model the process by which it came about. Micro-level examples include Larry Griffin's careful examination of the sequence of events associated with a particular lynching (Griffin 1993), and Michael Bigg's study of the interaction sequences associated with strike activity in 1886 (Biggs 2000); similar approaches dominate in macro-level accounts of political change and economic development. While such analysis of the unfold-

the temporal distribution of a single type of event (e.g., strikes, riots, lynchings).

In and of themselves, neither sets of diverse events nor series of values of a single event are necessarily evidence of an underlying reactive or structured feedback process. However, absent such an empirical trace we are hard pressed to evaluate whether a generating process might be meaningfully sequential.

#### *Sequences are temporal*

While we as analysts may retrospectively observe sequences as complete entities, from the actors' perspective sequences unfold prospectively through time (though of course the scale and measure of time will vary dramatically from context to context). The key is recognizing that sequences span (and link) temporally distant moments, and in this respect are a form of what Danto terms "narrative sentences" (1985). The question we must ask is, does temporality play a role in causality (see Stinchcombe's excellent writing on this point (1968), also Haydu 1998). For, as we all know, sometimes a series of events can be organized into a temporal sequence without any causal chain linking a prior state to a subsequent state.

#### *Sequences have order effects*

Arguably one of the most theoretically powerful and analytically complex features of a sequence is the idea of an order effect. Put another way, the specific ordering of events or elements in a sequence distinguishes the content of one social trajectory from another. Thus order effects move us beyond the simple occurrence of events (the aim of much normal science), and focus our gaze directly on situations where the significance

*...order effects move us beyond the simple occurrence of events (the aim of much normal science), and focus our gaze directly on situations where the significance of an event is conditioned by the presence or absence of other events.*

of an event is conditioned by the presence or absence of other events (Abbott 1992, Stovel 2001). Above and beyond claims about contingent effects, in sequential situations significance comes from *when* an event occurs, and whether it precedes or follows other known occurrences. For example, we may discover that for much of the 20th century, a life course trajectory in which marriage followed children had very different consequences than one in which children follow marriage; models that examine only the occurrence of marriage, or children or even their joint occurrence, will overlook the consequences of the order in which these events occur (see Hogan 1978 and Rindfuss, Swicegood, and Rosenfeld 1987 for examples of analyses of this type).

#### **Sequences and regularity**

In addition to being empirically observed traces of social phenomena in which temporal order carries meaning, social sequences are characterized by some form of regularity or repetition. In his empirical work on sequences, Abbott has been a great advocate of the importance of searching for frequent empirical regularity in whole sequence structures (Abbott and Hrycak 1990, Abbott 1995, Abbott and Tsay 2000). Typical pat-

terns, he argues, are crucial both for actors themselves and for analysts trying to make sense of social phenomena.

Yet from where I sit the common use of the term *sequence* has ignored important distinctions in the type, and source, of regularity in social trajectories. Consider two types of sequential regularity: on the one hand, the concept of a business cycle refers to regular periods of expansion and contraction in an economic system; in such instances, we observe a regular pattern *within* a sequence. Such patterning, of one event (or state) following another in predictable and connected ways, may shed light on deep and general logics of social, political, or economic activities. On the other hand, regularity may mean a prevalent pat-

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ing of a series of heterogeneous events is the 'bread-and-butter' of much historical sociology, it may –or may not– meet the definition of a sequence offered here.

tern that repeats *across* observations, as in the case of a well-defined career path.<sup>††</sup> In this latter case, discovery of a prevalent sequential pattern may reveal the presences of a highly institutionalized process that aligns the production of actors with relevant positions in society. Hence different forms of empirical *regularity* connote different underlying processes that might generate the observed social sequence. In the remainder of this note I illustrate the difference between within- and across- sequence regularity, drawing examples from two major areas of study—social movements and careers.

### **Within- and Between- Sequence Regularity**

#### *Sequences in social protest*

A classic example of within-sequence regularity is the idea of a protest cycle (see especially Traugott 1995 and Tarrow 1991 1995). Such protest cycles are composed of a characteristically ordered set of events that reflect the interactions between activists and state-controlling actors. Tarrow compares protest activity with business cycles, arguing that each is triggered by structural or situational events, but unfolds (in regular ways) as a result of actions and reactions to the initiating event. “In a protest cycle, as in a business cycle, the original factors that give rise to protest are structural, but they cannot explain directly all the actions that take place within it. Once the cycle begins, the actions of some groups trigger responses by late-comers, which may be independent of the structural factors that incited the early risers.” (1985, p. 51)

Hence, while there is variation in the regularity of expression, increases in violent or confrontational protest events are linked to rises in more conventional expressions of grievance. In addition, during periods of heightened activity, protest activity spreads from the center to the periphery, thereby offering a theoretical linkage between a cyclical process and its capacity to spread to other contexts (where it is likely to be repeated). Finally, peaks of protest cycles may trigger innovation in the repertoire of collective action activities

available to subsequent social actors—even as the basic cycle of protest is repeated.

The recognition that the dynamics of social and political protest often generate a regular sequence of events (both at the micro-level, as individual events unfold, and at the macro-level, as protests trigger responses), has refocused scholars' attention on the temporal interdependence of actors in the social arena. Further, some of this work suggests that protest carries with it an endogenous logic that transcends local conditions on the ground. It may strike some as ironic that even during moments when actors seek to reshuffle social arrangements through extra-legal events, such predictable reactive logics hold, yet this is exactly why such sequences are powerful; social sequences reflect the operation of powerful social structures through time.

#### *Sequences in occupational work histories*

At the individual level, the classic example of a sequential pattern is the career. Like a protest cycle, a career is a temporally ordered sequence of states (here, jobs), yet with careers the social significance derives not from internal regularity, but from prevalence in a particular society. Spilerman emphasized the importance of empirical prevalence when he defines a career line as “a work history that is common to a portion of the labor force” (Spilerman 1977, p. 551).

By studying the careers of a collection of individuals we may observe a set of regular career lines. The analytic concept of career lines (with its imagery of career ladders marked by regular upward promotion) is strongly associated with modern, bureaucratic internal labor markets, which help regulate employment relations. In such cases, complementary labor market institutions structure the temporal experience of workers by offering relatively predictable opportunities for upward mobility—reflected in empirically observed sequences of jobs. Sometimes, by examining data drawn from successive cohorts of workers, we may observe the emergence of fundamentally *new* career lines—which reflects a change in the institutional environment. In our historical analysis of career systems at Lloyds Bank, we used optimal alignment techniques to trace the extent to which career structures at Lloyds Bank were transformed during the first quarter of this century, finding that changes in

<sup>††</sup> To further complicate the issue of regularity, common patterns may be observed in independent socio-historical contexts (different countries at different moments in time) or may appear in settings where cases are —arguably— subject to a common environment.



both the internal environment (technological advance, increased use of women for clerical tasks) and the external environment (industry-wide mergers, the changing meaning of localism, demand for white collar labor) created the context for radical restructuring of long-term employment patterns (Stovel, Savage, and Bearman 1996).

Perhaps one of the most theoretically interesting features of career lines is that although they are built from the past, employee knowledge of the shape of existing career lines affects expectations (and, potentially, employees' strategic behaviors) prospectively—as when workers attempt to align their own experience with standard expectations of upward mobility during a work history. In this respect, career lines are social sequences that represent the enactment of a social script: actors themselves recognize or believe in a cultural or normative sequential order

and orient their behavior toward their understanding of a particular sequential process (Wu 2002). Key to this insight is the dual nature of careers: they are produced jointly through the structured actions of employers and employees. If an observed sequence is wholly determined by an external structure, its existence or significance may be lost on particular actors. Yet because careers are at least in part produced by organizations and labor market structures, individual actors rarely can completely control their own destiny. Thus because they force us to consider the interaction between structural and subjective factors, the study of social sequences can guide our attention to the duality of forward- and backward-looking social process.

### **Beyond Regularity**

This note has highlighted various ways in which the covering term 'sequences' may refer to a wide variety of empirically observed streams of events

that reflect the temporal trace of social structures. Of particular interest is the distinction between deep social logics that endogenously produce temporal regularity (as in business or protest cycles), and institutionalized processes that produce actors who reproduce regular orders of events (as in careers). My focus on differentiating within- and across- sequence regularity, however, has ignored another object of study that frequently invokes sequences, namely, those streams of historical events that are neither repeated nor regular, but where future events are clearly contingent on early events. It is without a doubt true that this broad class of topics, which includes some revolutions and other moments of widespread political and or economic change, has always animated much of the field of comparative historical sociology. Yet in our enthusiasm to identify critical junctures (eg., Capoccia and Kelemen 2007), or specify the conditions for path dependency (eg., Pierson 2000, Mahoney 2000), we should not ignore the extent to which

examination of regular social sequences provide a realistic view of how social structures shape experience through time.

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*...career lines are social sequences that represent the enactment of a social script: actors themselves recognize or believe in a cultural or normative sequential order and orient their behavior toward their understanding of a particular sequential process.*



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## Using Network Analysis in Comparative-Historical Research

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"The enterprise of employing structural analysis in historical research is a particularly fruitful way to generate new insights and questions and to re-frame or resolve old ones" (Gould 2003: 241-2).

A cleverer person than I wrote this not so long ago, and probably in a more widely accessible location than this ephemeral thought-piece in an ASA newsletter. I could come in considerably under my allotted quota of words if I simply said: go read Roger's book chapter on this topic. However, a couple of things have changed in the last five years or so that incline me to offer a slightly different gloss than the one Roger offered.

First, since the initiation of the 'new science of networks' in the late 1990s (Barabási 2002; Watts 2004), there has been an explosion in the application of network concepts across disciplines, including sociology. This explosion has reverberated within historical sociology to a noteworthy extent. It no longer will do to examine only Bearman (1993), Padgett and Ansell (1993), and Gould (1995) to cover the state of the art in historical network analysis. Second, in his essay, Gould expressed caution about the "over-enthusiastic" (2003: 242) application of network concepts to data that is not explicitly about social relationships. I think I know what he means. If anything, we are in a more dangerous time now than ever before with respect to the overhasty application of network tools in our research. For God knows what reason I am reminded of a very old tv commercial. Announcer says: "Dogs *love* cheese!" Well, people *love* networks. Can't get enough of them, really. Figure out a networks angle. There's this thing called Facebook. And so on. I say all of this in my grumpiest tone of voice while also casting an admiring eye on many an ingenious use of network tools by young researchers. I don't want to discourage anyone from using network tools creatively. However, to believe network tools will deliver us the pot at the end of the scientific rainbow is illusory—although skill with those tools might land you a job in a premier business school! One problem continues to be—perhaps less so in historical sociology than in other disciplines—a fetishizing of the method for its own sake, without anchor in the substantive puzzles we are trying to understand. The challenge lies in using network concepts, especially relatively new (or fashionable) network concepts, to tell us something persuasively real, but also fresh and interesting, about historical conjunctures and historical development.

What can network analysis do for us? Here are some quick and dirty answers to that question. First, using network methods is one way *to think relationally* in a systematic and rigorous way about social life (Emirbayer 1997). It's certainly not the only way to do so, but it encourages that mindset and offers tools for operationalizing that theoretical meta-perspective. Second, as Gould (2003: 258) pointed out, network methods have helped us think about the way *group identities*

emerge out of, get reinforced, or get suppressed, through patterns of interaction. Third, network analysis can help us to identify the salience or influence of particular structural positions or network configurations and thereby explain the advantages (or disadvantages) accruing to occupants of those positions. Work from the 1990s was excellent on these fronts, but newer work also makes stimulating contributions. For example, Uzzi and Spiro (2005) examine the small world network structure of Golden Age Broadway to assess its impact on participants' creativity and success. Here the positions of particular actors, but also the structure of the network *as a whole*, are explored as factors contributing to innovation. Paolo Parigi (2010) examines networks of acolytes, and concomitantly networks of miracles, supporting the canonization of particular saints in the seventeenth century Catholic Church.

Successful pleas for canonization were typically supported by a support network of *hybrid* social composition, as well as by convincing narration of the miracles as a *linked* set of events. I (2011) have tried to show how Polish elite social networks became cleaved in the late 18<sup>th</sup> century in such a way that clearer lines of political difference emerged, ushering in a transition from factional politics to party-based politics. And Sourabh Singh (2010) has ambitiously mapped the evolving social composition of the post-Independence Indian political elite, showing in particular how Indira Gandhi's centrality with respect to her family dynasty, combined with her *marginality* with respect to the Congress Party establishment, contributed to the formation of her particular style of politics and provided impetus for the imposition of the state of Emergency in 1975.

More recently, it seems to me a new and slightly different set of themes are also being explored through the use of network tools. One is to move beyond arguments about the consolidation of group identities to study the *emergence* of

new groups, organizational forms, or industries, out of network interactions. This is the driving force behind Woody Powell's work with several collaborators (Powell et al. 2005) over the last many years on the emergence and development of the biotech industry out of the evolving interactions among different types of organizational actors, spurred by the input of crucial resources from government agencies and universities. Powell's forthcoming book with John Padgett

(Padgett and Powell 2011) with contributions by several other authors promises to be a state-of-the-art statement of this vein of research.

Another theme of developing importance has been to establish how actors' positions in certain social networks enable their position or mobility in other networks—a so-called '*multiple net-*

*works*' perspective. Some recent work I have been doing with Neha Gondal on personal lending in Florence (Gondal and McLean 2010) points in this direction, as well as the research John Padgett and I have carried out on Florentine commercial credit (Padgett and McLean 2011). I tried something along the same lines, although with less methodological sophistication, in work on office-holding in early modern Poland (McLean 2004), where I documented the effect of marriage network connections over time on different subgroups of the Senatorial office-holding elite. Henning Hillman (2008a, 2008b) has explored the classic networks theme of brokerage in different historical cases, but in so doing has shown the importance of *economic* elites in the construction of *political* alliances. And I can't resist expressing my admiration for Ann Mische's (2007) theoretically fecund treatment of the way skilled student activists managed the overlap among multiple networks of constituents in Brazil's 1992 democratization campaign. In fact the '*multiple networks*' theme is one where questions of culture and of networks seem to impinge most

*The challenge lies in using network concepts, especially relatively new (or fashionable) network concepts, to tell us something persuasively real, but also fresh and interesting, about historical conjunctures and historical development.*

vitality on each other (Pachucki and Breiger 2010).

Yet another important theme currently flowering, in part inspired by the exponential random graph models developed by network methodologists (for example, Robins et al. 2005), is to investigate *how historical patterns of microinteraction aggregate into large-scale structures*. For example, Gondal and I (2010) use an ERGM model explicitly in our research on Florentine lending to link the distribution of particular dyadic and triadic configurations to the organization of the network as a whole, and in turn to the *meaning* lending took on in the aggregate. But the work of Powell et al. (2005) also exemplifies the theme of linking micro to macro. They use both the attributes of actors and the characteristics and history of dyadic ties between biotech firms and their collaborators to assess statistically which micromechanisms are most responsible for generating the macrostructure of the evolving field of American commercial biotechnology. One could include the work of Erikson and Bearman (2006) here, too, as they demonstrate by means of anchoring a social network to its exact geographical coordinates how individually opportunistic voyages undertaken by English traders in the 17<sup>th</sup>-19<sup>th</sup> centuries accreted unintentionally into, as they put it, a “dense, fully integrated, global trade network.”

The *longitudinal* treatment of social networks is a crucial and growing part of networks research in historical sociology. A recent book by Quentin Van Doosselaere (2009) is a noteworthy example, a study of the changing social composition of economic activity (interactional activity like contract-making, insurance underwriting, and so on) in Genoa over the course of three centuries. The work also speaks to the micro-macro theme I alluded to above: the 20,000-plus economic ‘partnerships’ he studies agglomerate into economy-level structures manifesting various degrees of cohesiveness and centralization, which in turn tell us about the organization of economic activity in the aggregate. In turn the bigger point surely is that social embeddedness was crucial for the Commercial Revolution and thence the takeoff of ‘capitalism’ from the very beginning. A comparably incredible dataset and analytical effort is offered by Stark and Vedres (2006) in their analysis of the entire history of the post-socialist Hun-

garian economy as a gradual, patterned growth of networks of mutually dependent firms, in part generated through the process of property recombination.

Going beyond a series of snapshots in the representation of evolving networks is a desirable next step. Software developments are gradually allowing us to *depict network evolution dynamically*. One subtle and visually alluring (though not statistical) procedure for grasping network dynamics is the SoNIA program Skye Bender-deMoll and Dan McFarland have developed (<http://www.stanford.edu/group/sonia/>; also see Moody, McFarland and Bender-deMoll 2005). Here the growth and decline of networks, the flow of participants in and out, the changing identities of the actors, the emergence of large components and the like, can all be grasped visually—something one would expect historical sociologists to appreciate greatly, given our stress on the temporality of social life—although SoNIA is not that great at handling and representing the *multiple temporal rhythms* that comparative-historical sociologists know affect social outcomes (Pierson 2003). I (2010) am currently exploring the value of SoNIA for assessing how the Polish elite marriage market evolved over many decades, and I’d like to see others try their hand at the program! Perhaps we will get to the point of actually visualizing conjunctures, or seeing alliances grow past critical threshold points, or assessing the centrality of actors with more sensitivity to how positions of power can be temporally evanescent.

In his review, Gould also noted that “the abstraction of network methods can help to reveal commonalities across diverse contexts—an enormously valuable feature for comparativists interested in uncovering regularities” (2003: 251). That sounds great. And there is some hope that with precise operationalization of structural concepts like centrality, influence, structural equivalence, and diffusion, we can offer persuasive evidence of general patterns of social interaction. But as historians, or at least quasi-historians, we also know history is very messy and selection of comparable cases is fraught with difficulty. If we have a fixed institutional object in many cases—let’s say, “the state,” to take an example anything but at random—we can handle comparison. I don’t think practitioners of network methods have

yet developed a clear set of cases to compare. What is Florence like, or unlike? Part of this is a problem of data availability, part of it is the massive difference between cases on all kinds of contextual dimensions. It would be great to generate comparability in the application of key terms. But I fear the comparability of cases remains fragile and consequently our ability to develop general findings remains rather feeble. If I were to put it cynically, what we find is a potpourri of highly individual cases used to illustrate specific theoretical arguments which we hope accumulate into a set of abstract and general principles of social organization; but those principles then require considerable translation to be applied to specific other cases. That, to me, remains a shortcoming.

By way of wrapping this up, let me say I believe networks and history 'go together,' in that history provides us with relational data on populations, and alliances, and markets, and social movement mobilizations, and so on, that, perhaps simply because they have been preserved, strike us as *worthy of study*. Also contributing to a perceived affinity between networks and history is the relative *fixity* of the past, rendering it especially conducive to structural analysis. Perhaps most important, we have some intuitive sense that momentous events are produced out of *interactions* that gather in interesting ways and *processes* of social change that well up from below. Network analysis can help us tap into those phenomena—although confirming our intuition formally, and comprehensively, and cogently, using structural evidence, remains an imposing challenge.

Network approaches can offer, in my opinion, intricate, ingenious, and aesthetically pleasing explanations. Reading exemplary studies in this area one feels a certain exhilaration that we are seeing the world in a highly refreshing way. Network methods can give us access to broad patterns, macrodynamics, and increasingly, a precisely specified but also interpretively nuanced way to connect microbehavior to structural outcomes. The static quality of network depiction in the past will give way to more processual analyses and representations. And so I am optimistic about the future of a networks approach in historical sociology, even though I still adamantly support thorough immersion in real-world historical

data to complement the structural stories we tell, and to ensure that they merit credence.

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### **Mathematical and Simulation Models (with research & model evaluation suggestions)**

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*Introductory Note:* Sections A.3 and A.4 below are an excerpt from a longer invited paper, entitled "A Larger Model-Building Context for Visual Models," that I presented in Florence in March 2010 at an European University Institute Workshop on "The Past, History and Visualisation." Understanding the concept of "model" broadly (and slightly privileging the "visual models" category in view of the workshop theme), that paper begins with a 5-part typology of models, including types mathematical and simulation & gaming.

*Excursus on a part of the Chinese Revolution.* I believe the goal of encouraging further development of comparative-historical sociology's for-

mal model-building flank is best served by illustrating *one concrete context* where investment in ambitious model-building might shed light. Although on first impression the model-building direction proposed here may appear to be a version of simulation modeling, crucial insight comes from a type of mathematical conservation law (conservation of Poincaré index), so that major analytical power also comes out of mathematical modeling.

The year was 1937. Following the Marco Polo Bridge incident, the Japanese Army rolled into North China. The Chinese Nationalist government was soon forced far into China's vast interior. Yet the Japanese Army in North China had the capability to control little more than urban areas and connecting rail lines. Into this power vacuum moved the Chinese Communists, to that point (as of the Long March's end in 1936) largely confined to a remote area of China's northwest.

In a dramatically successful instance of political warfare on a massive canvas, Mao's forces spent much of World War II consolidating power across the North China countryside. While significant fighting with the Japanese (and the Nationalists) did occur, much of the relevant dynamic consisted of disciplined, cumulative political mobilization & organization, gauging progress county-by-county – or village-by-village. This was a significantly decentralized socio-political process. Although the statistics tell a story of success – the Chinese Communist Party (CCP) grew from about 20,000 members in 1936 to around 1,250,000 by 1945 – there were formidable difficulties to be surmounted along the way (obstacles that are becoming clearer as the historiography advances). Among those obstacles (in many mixes depending on locale, creating a situation where the CCP could take little for granted) were parochial peasants; entrenched local elites; Nationalist supporters; all interwoven in a such a tangled skein that the CCP itself at times turned to ethnographic techniques to seek clarification of local roots of opposition (!); and, of course, the Japanese army plus military forces loyal to the Nationalists – or to warlords like Yan Xishan.

Capturing the distilled essence of the resulting amalgam of opportunity, contingency, and (ultimately) major Communist momentum would be a first-rate analytic target for formal model-building. Here I offer a concrete suggestion.

Thinking analogically (as all good model-builders should!) there is an undeveloped (so far as I am aware), but potentially far-reaching and analytically valuable, analogy between (1) the historical process just profiled and (2) a specific type of nonlinear dynamic system behavior. At the outset it needs to be recognized that the proposed direction of model-building pivots on a particular scientific style (see A.3.3 in my EUI paper), one that would accept an ideal-type description of the stunningly complex, locally variable, socio-political dynamics of Communist mobilization whose details Chinese and Western historians are gradually clarifying, in exchange for a type of basic analytical insight on a large canvas (what mathematicians call “a global mathematical level”) that no available historiography can rival.

To get the model started – consciously putting aside the many refinements and qualifications that reading of relevant historiography invites – consider a contest between individuals of two “types”  $M$  and  $\sim M$  (think “mobilized to support the Communist cause” vs. “not mobilized”) who are distributed across a network of weakly coupled “islands.” Dynamics at each island are governed by frequency-dependent selection with a threshold.<sup>‡‡</sup> Taking a very simple case, if the frequency of the  $M$ s is  $<$  threshold at a given island, the contest there tends (in a stochastic process sense) to decrease that frequency toward 0 (think: “local Communist support is eliminated”); if the frequency of  $M$ s  $>$  threshold, the opposite result obtains with  $M$  frequency then tending to increase toward 1 (think: complete local Communist control). Take as the initial condition one island having 100%  $M$ s and all others having 100%  $\sim M$ s. Starting from this initial condition, the mathematical behavior of interest then occurs when the single initially 100%  $M$  island is able, through a *joint* process of migration (or other communication) between islands and dynamics within islands, to “tip” first its neighboring islands, then the neighbors of those islands in turn, and so on – until the  $\sim M$ s have been displaced by  $M$ s in the full island network. When one island is

<sup>‡‡</sup> For clarity, it should be noted that this threshold relates to frequency of the  $M$ s in a particular population. It is hence a population-level construct, by contrast to the thresholds in Mark Granovetter's “Threshold models of collective behavior,” *American Journal of Sociology*, 1978, 83, 1420-43, which are individual-level constructs.



able successfully to tip, say, 1,000 islands in this way, the resultant takeover effect – which in our 1980 book Paul R. Levitt and I termed the “cascade effect” – is indeed dramatic.

From the vantage of an external observer, what that observer would see as a successful cascade unfolds is an apparently inexorable march of the  $M$ s to replace all the  $\sim M$ s – a storyline that resonates with much received historiography on the expansion of the Eighth Route Army across North China. *Yet on deeper analysis it turns out that a successful cascade, when it occurs, is often far from inevitable.* For example, a seemingly slight variation in migration and dynamics may lead to a very different outcome, one where the  $\sim M$ s end up out-competing and eliminating the  $M$ s.

In the *longue dur e* of the biological evolution case that is focal in the 1980 book, there is no strategic actor waiting in the wings to facilitate & guide the dynamics of a sought-for Boorman-Levitt cascade – e.g., tinkering strategically with the inter-island couplings (which, in a human population context, would naturally involve flows of communications in addition to people). In adapting the Boorman-Levitt formalism to the 1937-45 Chinese case, there would need to be an enrichment of the formalism to capture the powerful – if often incomplete – strategic control exercised by Chinese Communist leadership in Yanan over flows of personnel, information, and other re-sources among relevant “islands” (e.g., villages or other geographic localities in North China).

Clarifying an operational target of such control, recall the American Civil War and Abraham Lincoln’s comment about the Mississippi after the Union capture of Vicksburg (a key turning point in that struggle): “The Father of Waters

again goes unvexed to the sea.” Transposing this imagery from *physical space* to *phase space*, one basic facet of the North China strategic challenge facing the Chinese Communist leadership may be modeled as the tuning of interactions between islands (think flows of key cadres, for example) to enable fixed points A and B existing in a phase plane that is basic to the Boorman-Levitt cascade model’s analysis (see Fig. 1) to coalesce and annihilate one another – thereby freeing a trajectory starting far in the northwest corner of the phase plane to “go unvexed” to fixed point C located in the northeast and corresponding to a successful “takeover” by the  $M$  type. (The relevant mathematical theory here related to conservation of the Poincar e index [see Boorman & Levitt, 1980 book, p. 103].)

No one to my knowledge has tried her hand at thus combining the unplanned dynamic systems level of the Boorman-Levitt cascade model with this kind of overlay of centralized if partial control, so there is ample intellectual virgin territory here. Along the way there are also further important unresolved challenges for

the model-builder – notably, how to develop a simple but compelling microdynamics to the capture dynamics at a single island (i.e., which would play an analytical role in the North China model akin to that of the Boorman & Levitt “minimal model” in Chapter 2 of the 1980 book). Doing this right calls for model-building imagination plus elbow grease – importantly building on careful analysis of the historiography of Chinese Communism in the Sino-Japanese war years.

Although a good mathematical model can shed light on many levels at once, a key piece of historiographical paydirt from the proposed followup to the Boorman-Levitt cascade model

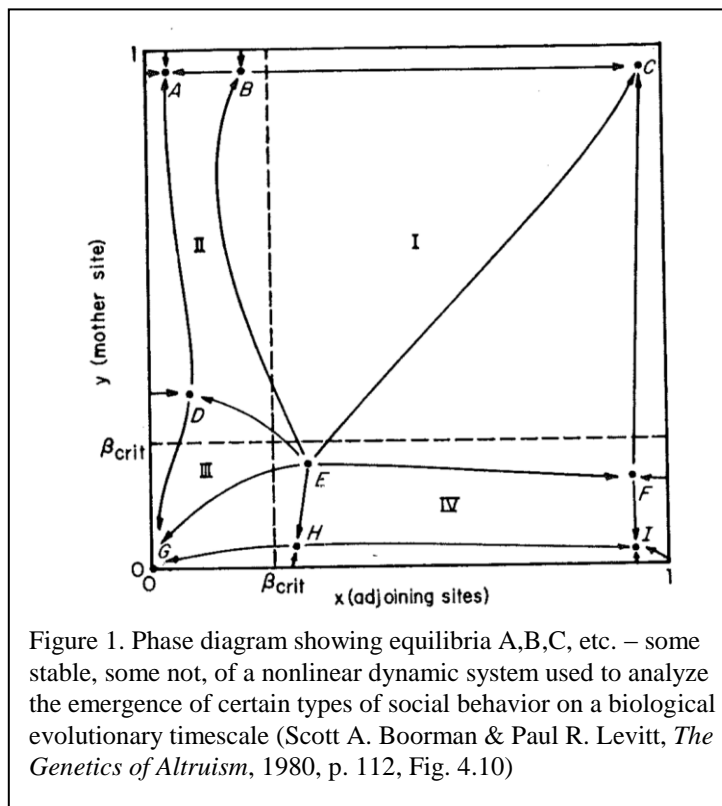


Figure 1. Phase diagram showing equilibria A,B,C, etc. – some stable, some not, of a nonlinear dynamic system used to analyze the emergence of certain types of social behavior on a biological evolutionary timescale (Scott A. Boorman & Paul R. Levitt, *The Genetics of Altruism*, 1980, p. 112, Fig. 4.10)

would be significantly deeper understanding – in a case of world-historical importance – of the nuanced interplay between agency, structure, and contingency in a setting of major sociopolitical mobilization. As a coauthor and I suggested (in a paper published prior to my work on cascades):

nimity, apparently because people expect the internal machinery of such models to be opaque to *everybody*, *not* just to non-mathematicians!) While there are few shortcuts to mathematical expertise, the following are some lines of inquiry that can often be productively explored – in give-

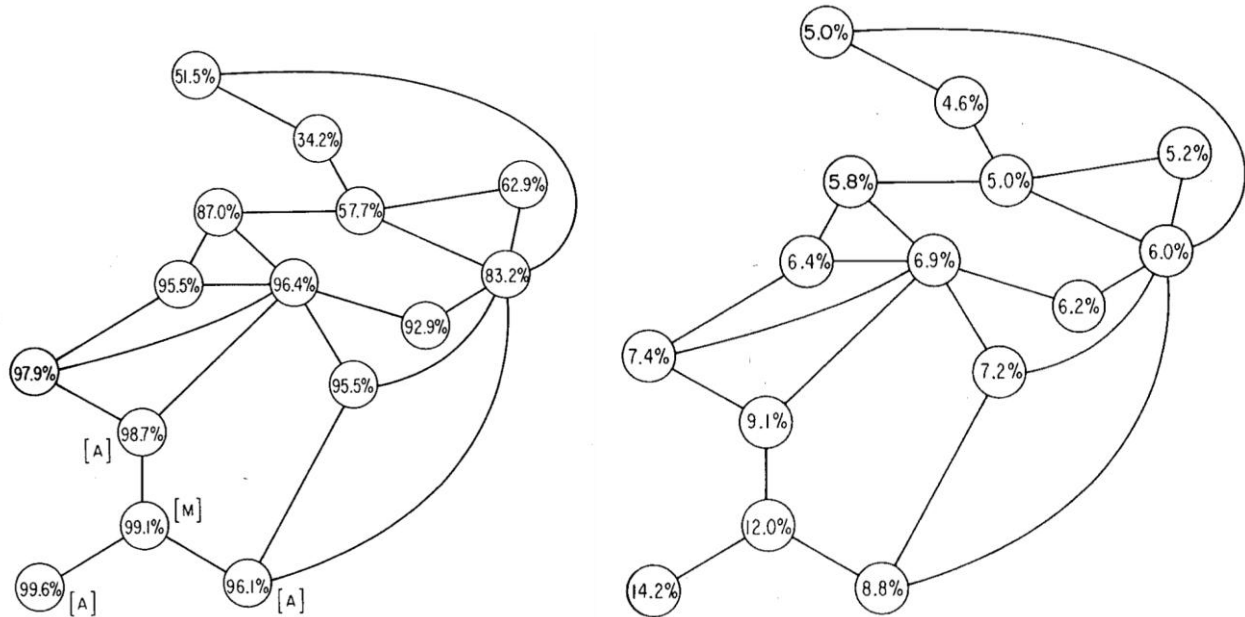


Figure 2a and 2b. Successful vs. unsuccessful cascade attempts (different choices of migration parameter  $m$  (Boorman & Levitt, 1980, p. 84)

“Communist victory, Mao Tse-tung to the contrary, was not inevitable in China.”

### A.3. Mathematical models

Building directly on the power of modern mathematics, this is an enormously rich and internally diverse vein of model-building – yet one whose serious and sustained development in the human sciences is of actually relatively recent origin, only really hitting stride (despite some early creative flashes) in the decades following World War II.

For scholars whose training falls outside of mathematics & allied areas, mathematical models are widely perceived as mysterious – probably more so than any other List A model type (this refers to a list of five basic types of models – verbal, visual, mathematical, simulation & gaming, cultural-analogical). (Perhaps paradoxically, the “black box” quality of many computer simulation models seems to be widely accepted with equa-

and-take with a mathematical modeler – by someone without a lot of technical background:

1. *How deep is the connection between the phenomenon a given model aims to analyze and the mathematics used?* Illustrating one extreme of a depth continuum is mathematical work like that described by the title theme of physicist Eugene Wigner’s famous essay “The unreasonable effectiveness of mathematics in the natural sciences,” which uses words like “eerie” to describe harmonies between, e.g., (a) quantum physics and (b) certain mathematical structures ultimately anchored in properties of the square root of -1.

At the opposite extreme are models – perhaps most commonly encountered in fields that have yet to see much mathematical model-building investment – having little more than the trappings of mathematics, and where the principal analytical insights could be attained by more widely accessible means (as via a verbal model – or a visualization).

The lion's share of mathematical models fall somewhere in-between on this continuum – and an excellent line of questioning for a constructive critic to probe is where a particular model-building effort is located on this continuum. Although the mathematical depth of which Wigner writes is probably not rivaled in any of the human sciences, there is, for example, no question that parts of economic theory have a quite genuine – at times deep – mathematical structure (much of it closely related to the theory of convex sets).

2. *Where is the specific power of a mathematical model coming from?* For example, is that model anchored in traditions of mathematical analysis ultimately flowing from the calculus (as is true of a host of models) – or does the model draw on traditions of abstract algebra (as, e.g., do some important types of social network analysis)? Or does the model build on ideas out of combinatorics & discrete mathematics, which has its own identity as a stream of 20<sup>th</sup> century mathematics? (All these major streams of mathematics contain substreams, so that this broad direction of questioning can be substantially refined.)

3. *What are a given mathematical model's key "stylistic" features?* As a way of coping with the level of abstraction involved, it can often be helpful initially to engage with mathematical modeling on a *stylistic* level. A classic comparative analysis of mathematical/computational modeling styles (not, by the way, confined to population biology) is Richard Levins, "The strategy of model-building in population biology," *American Scientist*, 1966, 54, 421-31, distinguishing models on dimensions of (1) generality, (2) realism, & (3) precision, whose tradeoffs Levins analyzes. (See also exchange between Levins & critics: *Quarterly Review of Biology*, 1993, 68, 533-555.) D.R. Cox (*Statistical Science*, 2001, 16, p. 217) also invokes en passant a concept of style in modeling context.

A separate stylistic difference is between models centered on "positive" vs. "negative" insights or results. The latter are classically exemplified by Kenneth Arrow's famous impossibility theorem (Arrow, 1951). A very important subgenre of the former is mathematical models that "build" something – e.g., a structure – as, for example, do some (though not all) social network models. Taking this "building" idea a step fur-

ther, certain (though again not all) mathematical models contain seeds of an operational capability.

4. *What is a given mathematical model's relationship to data?* That relationship may vary greatly – indeed far more than is commonly appreciated. For example, many non-modelers automatically assume that any mathematical model is somehow "fitted to" (i.e., tested against) quantitative data. In fact, many mathematical models – including some highly influential ones – are too abstract, and also often too data hungry, *ever* to be tested in detail against empirical data in the manner envisioned by textbooks of scientific method. For example, for close to a century – from work of Walras in the 1870s to the 1960s advent of the Scarf algorithm & computable equilibria – the general equilibrium theory (GET) modeling tradition in economics had quite limited tangency to the world of economic data. *Yet GET has played a fundamental & highly positive role, spanning multiple human generations, in helping to clarify & refine basic economic concepts – and, on a separate level, its existence has also provided a level of conceptual unity & intellectual discipline for economics far outstripping that of many other major social science areas.*

Although the details vary, within both game-theoretic and, separately, social-network-analytical traditions there is again great diversity in how the formal models relate to data.

5. *Is a given mathematical model best approached as a stand-alone product, geared to variations on a single substantive theme or question – or should it be approached as a representative of a larger family of distinct but interlocking models shedding light on a major substantive area containing diverse questions?* An example of the latter is models extending the mathematical population genetics tradition of Sewall Wright, J.B.S. Haldane, & R.A. Fisher to shed evolutionary light on patterns of intraspecific altruism & related social phenomena. A scientific hunch that this research direction might in fact turn up a major family of interlocking models with substantial intellectual unity & coherence was the genesis of my work with Paul R. Levitt leading to *The Genetics of Altruism* (1980).

As a challenge question, one may speculate that other, quite different but comparably rich, families of interlocking mathematical models have yet to be discovered by anybody.

#### A.4. Simulation models & gaming

A simulation model is a representation of some process, commonly one unfolding in real time. The model helps the analyst to explore that process – e.g., what happens if a slight change in assumptions is made. While mathematical analysis may also be able to shed light on questions of this type, what gives simulation a potential edge is its promise – provided the simulation is correctly implemented in software – of clarifying a process too complex to be clarified by purely mathematical modeling (or indeed other!) means.

Computing advances have *partially* turned simulation into a truly potent analytic tool, at its best capable of shedding light on previously inaccessible research questions. However, unlike the universalistic theorems or other general insights that analysis of a mathematical model often provides, simulation results are typically enormously particularistic – tied to specific assumptions reaching down to a level of particular numerical values assigned to particular parameters. In the background lies a version of the “curse of dimensionality.” At least in the human sciences, simulations often require specifying values for *many* parameters (which themselves may be of complex types, e.g., “network structure”). Outside of a few set-piece contexts (e.g., some short-term prediction problems), empirical evidence constraining those values is commonly limited. The upshot is that the analyst is called on to make numerous choices that are little more than educated guesswork. In this kind of situation it is, of course, prudent to run a simulation for a wide range of possible parameter combinations, with an eye to comparing outcomes & hope-fully achieving relatively robust insights. But if there are, say, 25 different parameters in play (as is not unusual in simulating complex social dynamics), and three values of each one are picked for exploratory purposes, the analyst needs to explore & compare results of running the simulation  $3^{25}$  or  $243 \times 243 \times 243 \times 243$  times under different combinations of assumptions. That is a number with quite a few zeros. Then too, one must also recognize that this analysis is still fairly coarse-grained (e.g., a more plausible account of the underlying process might call for, say, 100 parameters and 5 values of each). Directly exploring all resulting

possibilities then outstrips any physically possible computing capability.

It is a good rule of thumb that moving a simulation from concept to practice always calls for more assumptions – i.e., more parameters – than is first apparent. One situation in which parameters particularly proliferate is when (often in the name of greater realism) one starts allowing for actors’ individual differences within a simulation model.

Although in specific contexts the tale just told may err on the side of pessimism, these cautions about simulation are broadly justified – and perhaps especially so when making attempts to simulate the *complex, little-explored, scantily documented* processes of the sort in which historians or historically-minded social scientists are often most interested. Need for realistic thinking about what simulation can – and cannot – be expected to accomplish is particularly important as an offset to uncritical views in some simulation-oriented quarters. Enthusiasts of simulation are at times inclined to try to convert the difficulties just depicted into a rhetorical *selling point* (!) of simulation models, playing up – under banners like versatility – such models’ capabilities for exploring *any given one* of a vast set of possible parameter combinations (probably true enough, but at the same time often somewhat irrelevant for the reasons just outlined).

One way of trying to avoid these analytical pitfalls is by narrowing focus, away from “comprehensive” analysis of a process (which for many reasons may be infeasible) and towards identifying & analyzing counterintuitive or otherwise interesting *qualitative* outcomes that a particular process *may* produce under defensible assumptions. Some examples of work of this type, each pivoting on a specific idea having intellectual edge, are:

(1) Thomas C. Schelling, “Dynamic models of segregation,” Journal of Mathematical Sociology, 1971, 1, 143-86 (computations showing, in a context of stylized models, how a high level of residential segregation can emerge out of decentralized interactions in a spatially structured population whose members fall into two categories A & B; dynamics stem from the idea that, as Schelling puts it [p. 148], A & B individuals “may not mind each other’s presence, may even

prefer integration, but may nevertheless wish to avoid minority status” in their neighborhood).

(2) Michael D. Cohen, James G. March, & Johan P. Olsen, “A garbage can model of organizational choice,” *Administrative Science Quarterly*, 1972, 17, 1-25 (enormously original & intellectually provocative pioneering simulation model of dynamics of a “garbage can” organization [an offshoot of the notion of an “organized anarchy” where organizational goals are unclear; organizational technology ambiguous; & members’ participation fluid]; although the specifics have been challenged by some later analysts, the spirit of the analysis retains vitality and this paper is a must-read for any scholar interested in dynamics of historical bureaucratic empires in the sense of S.N. Eisenstadt’s 1963 book).

(3) Scott A. Boorman and Paul R. Levitt, 1980 book, Chapters 3-5 (mathematical plus simulation analysis of how subdivision of a population into incompletely isolated “islands” may permit possibilities for successful innovation that would be foreclosed in a system closer to “perfect” connectivity. (This idea may have potential to spin off a mathematical “movie,” with an innovation’s win/lose/draw fate unfolding in simulation – perhaps as a human audience roots for its success!) (*This is the model that underlies the Chinese Revolution application proposed above.*)

These models can all be captured in software. A different sort of simulation is when human beings take part in what may be an all-human or part-human, part-machine simulation. An overview of the resulting “gaming” area – valuable for its broad comparative perspective – is Martin Shubik, “On the scope of gaming,” *Management Science*, 1972, 18, pp. P20-P36.

## Theda Skocpol Dissertation Award, Inaugural Winner

Dan Lainer-Vos, 2009. "Nationalism in Action: The Construction of Irish and Zionist Transatlantic National Networks." Ph.D. dissertation, Sociology, Columbia University. (Dissertation Chair: Gil Eyal.)

**Statement from Award Committee:** "Nationalism in Action" uses insights gained from Bruno Latour's actor-network theory as well as from Leigh Star's concept of "boundary objects" to move the study of nationalism in new directions. Lainer-Vos' work takes its place in a new generation of nationalism studies that shift the focus of analysis from the territorially bounded and geographically specific nation state to the diaspora communities of displaced nationals.

Lainer-Vos views nation building as a practical organizational accomplishment. Using primary archival materials, he creates two case studies—the first focuses on Jewish Americans, and the second on Irish Americans. He treats encounters between these two dispersed national groups and their respective homeland movements as a site of strategic research. Lainer-Vos argues that national identity and sentiment may be created beyond, as well as within, the borders of the nation state depending upon the technologies of connection that develop. His core theoretical insight is that these technologies are always ambivalent because the ties they develop do not ignore or repress the differences between the homeland and the diaspora. On the contrary, these technologies exploit difference in order to create lasting associations—based upon "cooperation without consensus."

The dissertation has a twofold comparison. First, it examines financial technologies that attempted to tie the diaspora to the homeland. It focuses upon attempts to float national bonds in the Irish and Jewish communities in the US between 1920 and 1951 respectively. Whereas the Irish bond drive tore the community apart; the Israeli bond drive was the core of the long lasting Jewish American commitment to the state of Israel. Second, Lainer-Vos examines cultural technologies—specifically Jewish American summer camps in Israel and Irish American Gaelic Athletic Association—again with startling different results.

"Nationalism in Action" is a *tour de force* of archival research and theoretical acumen. It is historical and comparative in every dimension. It will surely make a wonderful first book and it is eminently worthy of being the recipient of the first Theda Skocpol Dissertation Prize.

**(Note: Please see the call for nominations for next year's Skocpol Award below)**

## Section Awards Winners 2010

### **Barrington Moore Book Award**

*Winner*

Andrew G. Walder, 2009. Fractured Rebellion: The Beijing Red Guard Movement. Cambridge, MA: Harvard University Press.

*Honorable Mentions*

Marion Fourcade, 2009. Economists and Societies: Discipline and Profession in the United States, Britain, and France, 1890s to 1990s. Princeton: Princeton University Press.

Chad Alan Goldberg, 2008. Citizens and Paupers: Relief, Rights, and Race, from the Freedmen's Bureau to Workfare. Chicago: University of Chicago Press.

### **Best Article Award**

Dan Slater, 2009. "Revolutions, Crackdowns, and Quiescence: Communal Elites and Democratic Mobilization in Southeast Asia." *American Journal of Sociology* 115 (1): 203-254

### **Reinhard Bendix Student Paper Award**

*Winner*

Anoulak Kittikhoun (CUNY Graduate Center, Political Science), 2009. "Small State, Big Revolution: Geography and the Revolution in Laos." *Theory and Society* 38 (1).

*Honorable Mention*

Bart Bonikowski, "Shared Representations of the Nation-State in Thirty Countries: An Inductive Approach to Cross-National Attitudinal Research."



## Call for Submissions, Section Awards 2011

### BARRINGTON MOORE BOOK AWARD

The section presents the Barrington Moore Award every year to the best book in the area of comparative and historical sociology. Nominated publications should have been published during the two years prior to the year of the award (i.e., for the 2011 award only books published in 2009 or 2010 will be considered). Books may be nominated only once for this prize. Thus, books nominated last year cannot be considered again for the 2011 award.

Books may be nominated by authors or by other section members. Non-authors may nominate a book by sending a letter or email to the prize committee members. Non-authors should ask authors to arrange to have the book sent to each member of the committee. Authors may nominate their book by sending a letter or email to the prize committee members and making arrangements for each member to receive a copy. Nominations must be received by **February 15, 2011** to be considered.

The committee members and their email and mailing addresses are:

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### BEST ARTICLE AWARD

The section awards this prize every year to the best article in the area of comparative and historical sociology. Nominated publications should have appeared during two years prior to the year of the award (i.e. for the 2011 award only articles published in 2009 or 2010 will be considered).

Authors or other members of the section may nominate an article by sending a letter or email to each member of this prize committee along with a paper copy of the article. The letter and copy of the article must be received by each member of the committee by **February 15, 2011** to be considered.

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## THEDA SKOCPOL DISSERTATION AWARD

The section presents the Theda Skocpol Award every year to the best doctoral dissertation in the area of comparative and historical sociology. Eligible dissertations must have been defended and filed between January 1, 2009 and December 31, 2010.

Dissertations may be nominated by dissertation chairs, advisors or current department chairs. We ask that each nomination letter include a brief discussion of the specific strengths and contributions of the dissertation. Self-nominations are not allowed for this award. Dissertations may be nominated by sending a letter or email to each member of this prize committee. Authors are then responsible for providing each member of the committee with a printed copy of the dissertation. Both the nominating letter and the dissertation must be received by each member of the committee by **February 15, 2011** to be considered.

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## REINHARD BENDIX STUDENT PAPER AWARD

The section presents the Reinhard Bendix Award every year to the best graduate student paper in the area of comparative and historical sociology. Submissions are solicited for papers written by students enrolled in graduate programs at the time the paper was written. Both published and unpublished papers will be considered.

Students may self-nominate their finest work or it may be nominated by their mentors. Authors and mentors may nominate a paper by sending a letter or email to each member of this prize committee along with a paper copy of the article. The letter and copy of the article must be received by each member of the committee by **February 15, 2011** to be considered.

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## Call for Submissions, Section Sessions, ASA 2011 in Chicago

**Session Type:** Open Submission

**Session Title:** Islam and the Modern World

**Session Description:** Papers on any and all comparative-historical aspects of Islam the religion, the civilization, and the religio/political engagement with modernity will be seriously considered. Also, papers on the emergence, expansion, and geopolitical engagement of Islam, Islamic Empires, and social movement groups/parties (Islamist, jihadist, and other) since the 7th century CE will be considered. Finally, papers on comparative theory of, say, Ibn Khaldun vs. Marx on history's inner dynamics, or the post-Weberian ascetic theory of Sayyid Qutb, or any other comparative-theoretical studies, will be seriously considered.

**Session Organizer:** Albert Bergesen, University of Arizona, [albert@email.arizona.edu](mailto:albert@email.arizona.edu)

**Session Type:** Open Submission

**Session Title:** Pluralism and Methods in Comparative-Historical Research

**Session Description:** Comparative-historical research is no longer confined to purely comparative methods. In many ways, the field has become a testing ground for innovative approaches to social research. This panel seeks to showcase the different methods used in comparative historical research and explore their relationship to the substantive and theoretical problems of the field – for example, do different and innovative methods produce new information about old problems in the field or open up entirely new research trajectories.

**Session Organizer:** Emily Erikson; Yale University; [emily.erikson@yale.edu](mailto:emily.erikson@yale.edu)

**Session Type:** Open Submission

**Session Title:** Empires and Civilizations

**Session Description:** Comparative-historical studies have recently turned their attention to empires but at least since Weber and Shils among others they have long been interested in “civilizations.” Empires and civilizations can each be thought of different forms of human association. This panel showcases comparative-historical research that illuminates them comparatively. What

are the dynamics, forms, and contours of empires across time and space, and how do they differ from or articulate with civilizations? What exactly are their differences or similarities, connections and tensions? How do they fare as social and political forms? Theoretical and theoretically-grounded empirical papers are welcome.

**Session Organizer:** Julian Go, Boston University, [juliango@bu.edu](mailto:juliango@bu.edu)

**Session Type:** Open Submission Roundtables

**Session Title:** Comparative and Historical Sociology Roundtables

**Session Description:** None

**Session Organizer:** Bart Bonikowski, Princeton University, [bartb@Princeton.EDU](mailto:bartb@Princeton.EDU)

## Letter to CHS on the Real Utopias Theme of ASA 2012 From Erik Olin Wright

The theme for the 2012 Annual meeting of the ASA is “Real Utopias: Emancipatory projects, institutional designs, possible futures.” Here is how I described the core idea of this theme in the ASA newsletter, *Footnotes*:

“Real Utopias” seems like an oxymoron: Utopia means “nowhere” – a fantasy world of perfect harmony and social justice. To describe a proposal for social transformation as “utopian” is to dismiss it as an impractical dream outside the limits of possibility. Realists reject such fantasies as a distraction from the serious business of making practical improvements in existing institutions. The idea of real utopias embraces this tension between dreams and practice: “utopia” implies developing clear-headed visions of alternatives to existing institutions that embody our deepest aspirations for a world in which all people have access to the conditions to live flourishing lives; “real” means taking seriously the problem of the viability of the institutions that could move us in the direction of that world. The goal is to elaborate utopian ideals that are grounded in the real potentials of humanity, utopian destinations that have accessible way stations, utopian designs of viable institutions that can inform our practical tasks of navigating a world of imperfect conditions for social change.

Exploring real utopias implies developing a sociology of the *possible*, not just of the *actual*. This is a tricky research problem, for while we can directly observe variation in what exists in the world, discussions of possibilities and limits of possibility always involve more speculative and contentious claims about what could be, not just what is. The task of a sociology of real utopias, then, is to develop strategies that enable us to make empirically and theoretically sound arguments about emancipatory possibilities.

I am hoping that many of the sections of the American Sociological Association will be enthusiastic about engaging this theme in some of the sessions which they directly organize, but I also hope that members of different ASA sections will submit proposals to the program committee for thematic panels which explore the problem of real utopias within their subfield.

There are a number of interesting ways in which the theme of “real utopias” is of relevance to the Comparative and Historical Sociology section of the ASA. Perhaps the most obvious is the role of utopian ideas in various historical processes and movements, but I also think it would be interesting to have discussions on the various historical experiments in intentional communities, both in the United States and Elsewhere. The analysis of the 20<sup>th</sup> century efforts on revolutionary transformations from above could also be framed in a way relevant to the real utopias theme. And I am sure there are many other topics that members of the Comparative and Historical sociology section could think of. My hope is that there are people in the section who will find this theme relevant to their interests and will creatively elaborate proposals for panels at the 2012 meeting. Information about submitting proposals for the meeting can be found at: [http://www.asanet.org/footnotes/septoct10/2012\\_0910.html](http://www.asanet.org/footnotes/septoct10/2012_0910.html).

To facilitate such proposals I thought it might be helpful if I shared some of my general ideas on the structure of the thematic and plenary panels for the 2012 meetings. This is all quite tentative – the first real meeting of the program committee where these and other ideas will be discussed will be in early December – but it may give people some idea of the kinds of things I hope to see happen. What follows, then, is a brief sketch of the different kinds of panels around the theme of Real Utopias I would like see at the meeting.

### **I. Real Utopia Proposals Sessions**

Each of these sessions will revolve around a proposal for a real utopian design to resolve some domain of problems. Examples would include: unconditional basic income, market socialism, equality-sustaining parental leaves, participatory budgets, random-selection democratic assemblies, worker cooperatives, stakeholder corporations, solidarity finance, democratic media, etc. The ideal here is to recruit an anchor person for the session who we know has already worked extensively on formulating such real utopia designs

rather than simply a person who has thought critically about the theme (although there will certainly be flexible on this). This format will not be appropriate for all of the themes around real utopias; it will be especially effective for those problems around which there exists an on-going discussion of alternative institutions.

My idea is for the sessions to be organized as follows:

- We will create a dedicated website for these sessions.
- The person who anchors these sessions will prepare an elaborated proposal for institutional designs around some theme which will be posted online by early 2012. While of course these essays will include some discussion of what's wrong with existing structures and institutions, the goal is for them to sketch the central contours of alternatives. By this I do not mean a detailed "institutional blueprint", but rather a careful elaboration of the core principles of an institutional proposal. My expectation is that these will be in the 10,000 word range, although some could be longer.
- In some sessions there could be two competing or contrasting proposals. Having two different proposals could make for a very lively session for some topics.
- The website will allow for comments and dialogue so that these proposals can be part of a discussion prior to the meeting. I am not sure yet precisely what the best design for the website would be, but I am hopeful that it will be an interactive site rather than simply a passive site.
- At the session there will be a very brief – 15-20 minute – presentation of the proposal and at most one commentary, or perhaps a contrasting proposal. I want to avoid panels with lots of presentations and little time for debate and discussion.
- In *Footnotes*, section newsletters, and other modes of information dissemination we will encourage people to look at the proposals before the meeting and to come to sessions with issues they want to raise. While of course we want to avoid long-winded speeches from the floor, I think

somewhat longer than usual interventions could be constructive.

#### *Partial list of potential Topics for Proposal Sessions*

Below is an initial list of possible thematic panels built around real utopia proposals. I have identified these sessions by the central principle of the proposal (for example, Unconditional Basic Income) rather than by the general topic or target of a proposal (eg. Healthcare), except where I do not have a specific real utopian proposal in mind. Because of my own expertise, most of the topics I have thought of revolve around political and economic issues. Nevertheless, it would be good if some of these thematic proposal sessions revolved around cultural issues of various sorts and around egalitarian and social justice issues that are not exclusively socio-economic in character (gender, race, sexuality, etc.). Some of these topics may be more suitable for general thematic sessions rather than for the proposal sessions.

1. Unconditional Basic Income
2. A democratic media system
3. "High road" capitalism
4. Democratizing finance
5. Participatory budgeting
6. A democratic, egalitarian system of campaign finance
7. Deliberative referenda
8. Gender: Parental leaves for gender equality
9. Parecon (participatory economics)
10. A framework for a digital network economy
11. Building the Scientific Commons (publications, data dissemination, etc.)
12. Community policing
13. Worker-owned Cooperatives
14. Pensions, labor's capital, solidarity finance, wage earner funds
15. Randomocracy, citizens assemblies
16. LETS (local exchange trading systems)
17. Globally just Fair trade
18. Market socialism
19. Intellectual property – the creative commons
20. Public education

21. Universities
22. Healthcare

## II. Film/documentary sessions

I think it would be interesting to have a number of sessions which present documentary films on exemplary and iconic cases of social innovations to solve problems. The intention here is not to have cheerleading films, but documentaries that analyze specific kinds of leading cases. The films could either be presented by the filmmaker or by an expert who researches the case and could lead a discussion following the film. Most documentaries which are thematically relevant on these issues tend to be mainly about social movements and struggles – sometimes of the “heroic struggle” variety – and not so much about outcomes, institutional innovations, actual transformations of social structures. So, I am not sure exactly what is available.

Examples could include things like:

- The *kibbutz* – there are a number of films that are retrospectives on the kibbutz experience
- *Holding Ground* – a film about the Dudley Street neighborhood association
- Public transportation – I understand that there is an interesting film about innovative public transportation in a Brazilian city, but I have not seen it
- Local food, alternative agriculture

## III. Thematic panels around broad topics and disciplinary subfields

Some of the topics listed under Real Utopia Proposals sessions could be shifted to these regular thematic sessions if we don't find a suitable anchor person with a well-worked out institutional/transformational proposal. And some of the topics listed below, of course, could also be moved to the institutional proposal category.

In terms of format, I have a strong preference for sessions which do not have so many presentations that there is little time for discussion, and generally I prefer sessions without discussants – my experience is that it is usually more interesting to have discussion from the floor unless the discussant is really engaged in a debate with a specific argument (as in the proposal sessions). For these

sessions, then, I would generally like three presenters and no discussant.

*Some possible topics for general thematic sessions*

1. Consumerism
2. The corporation: alternative models for more democratic/participatory governance
3. Carework
4. Future studies as a framework for envisioning real utopias
5. The Cleveland cooperatives initiatives
6. Mondragon, Emilia-Romagna and other exemplary worker cooperative districts
7. Utopian thinking within sociological theory
8. Utopian and dystopian visions
9. Marxism and real utopias or Marxism vs real utopias
10. Energy
11. Global Warming
12. The family
13. Sexuality
14. Childhood/children
15. Cities
16. Multiculturalism
17. Linguistic justice
18. Race, racial justice
19. International migration
20. Methodological issues: nonevents and possible futures
21. Criminal justice: crime & punishment
22. The military
23. Intentional communities
24. 19<sup>th</sup> century utopian communities
25. Transforming culture
26. Local food
27. Alternative Agro-food Systems
28. The Internet
29. Wikipedia
30. Creative commons
31. Voluntary simplicity
32. The Chicago participatory budget experiment
33. Transhumanism
34. Science policy

## IV. Plenary Panels

The program contains up to three plenary sessions – one on Friday evening and the in the noon slot on Saturday and Sunday. Tentatively, I am thinking of the following possibilities:

1. *Big Ideas for Real Utopias*: This could be one or two of the plenary panels, depending on other plenary suggestions. The idea would be to have a panel(s) featuring very prominent, articulate advocates of specific real utopian proposals. I envision three presentations for this panel, each around some Big Idea. One idea is also for these panelists to lead a proposal-thematic session (category I above) on the day after they are on the plenary panel. This would make it possible for there to be intensive discussion of the high profile ideas presented in the plenaries.

If we have only one plenary session of this character, the topics could include, for example, some of the following:

- Basic Income
- A democratic media system
- Participatory Budgets and direct democracy
- Gender Equality and the family
- Cooperatives

If we have two panels of this sort, one could be built around democracy issues and one around equality issues:

Democracy:

- Making Elections truly democratic
- Participatory budget and direct democracy
- Democratic media

Equality

- Basic income
- Gender equality and family
- Cooperatives

2. *Energy, the environment, and global warming*:

This plenary would focus on institutional designs for countering global warming and other aspects of ecological crisis rather than just the nature of the problem itself. Mostly when I have seen panels and discussions of these issues the discussion of institutional design is pretty thin. There is a sharp indictment of existing consumption and production patterns and a call for dramatic transformation in how we do things, but little discussion of the mechanisms for accomplishing this

and how sustainability and low growth can be institutionalized and reproduced.

3. *Sociology as Real Utopia*: I am less sure about this, but it might be possible to have a session which reflected on the nature of the discipline and academic life, and asked what the real utopia vision for sociology might be.



## Member Publications

- Almeida, Paul D. "Globalization and Collective Action," pp. 305-326 in Kevin Leicht and J. Craig Jenkins, eds. *Handbook of Politics: State and Society in Global Perspective*. New York: Springer, 2010.
- Almeida, Paul D. "Social Movement Partyism: Collective Action and Political Parties." pp. 170-196 in N. Van Dyke and H. McCammon, eds, *Strategic Alliances: New Studies of Social Movement Coalitions*. Minneapolis: University of Minnesota Press, 2010.
- Almeida, Paul D. 2010. "El Salvador: Elecciones y Movimientos Sociales." *Revista de Ciencia Política* 30 (2): 319-334, 2010.
- Bandelj, Nina and Elizabeth Sowers. 2010. *Economy and State: A Sociological Perspective*. Cambridge, UK: Polity.
- Bandelj, Nina and Matthew C. Mahutga. "How Socio-Economic Changes Shape Income Inequality in Central and Eastern Europe," *Social Forces* 88(5): 2133-2161.
- Berman, Elizabeth Popp and Nicholas Pagnucco. "Economic Ideas and the Political Process: Debating Tax Cuts in the U.S. House of Representatives, 1962-1981." *Politics & Society* 38: 347-372, 2010.
- Calvo, Esteban, Fabio M. Bertranou, and Evelina Bertranou. "Are Old-age Pension System Reforms Moving Away from Individual Retirement Accounts in Latin America?" *Journal of Social Policy* 39(2):223-234, 2010.
- Ceron-Anaya, Hugo, "An Approach to the History of Golf: Business Practices, Technologies of the Self, and Symbolic Capital." *Journal of Sports and Social Issues*. 34 (3), 2010.
- Chase-Dunn, Christopher, Thomas D. Hall, Richard Niemeyer, Alexis Alvarez, Hiroko Inoue, Kirk Lawrence, and Anders Carlson. 2010. "Middlemen and Marcher States in Central Asia and East/West Empire Synchrony." *Social Evolution and History* 9:1:52-79, 2010.
- Cook-Martín, David and David FitzGerald. "Liberalism and the Limits of Inclusion: Race and Immigration Law in the Americas, 1850-2000." *Journal of Interdisciplinary History* 41:7-25, 2010.
- Ermakoff, Ivan. "Theory of Practice, Rational Choice and Historical Change," *Theory and Society* 39: 527-553. 2010.
- Feuillet-Liger, Brigitte, Kristina Orfali and Therese Callus, eds. *Who is my Genetic Parent? Donor Anonymity and Assisted Reproduction: a Cross-Cultural Perspective*. Bruxelles: Brylant, 2010.
- Gellert, Paul K. "Extractive Regimes: Toward a Better Understanding of Indonesian Development" *Rural Sociology* 75(1): 28-57, 2010.
- Gellert, Paul K. "Rival Transnational Networks, Domestic Politics and Indonesian Timber," *Journal of Contemporary Asia*, 40: 4, 539-567, 2010.
- Gunnoe, Andrew and Paul K. Gellert. "Financiarization, Shareholder Value, and the Transformation of Timberland Ownership in the USA," *Critical Sociology* 37(2) 1-24, 2011
- Haglund, LaDawn, *Limiting Resources: Market-Led Reform and the Transformation of Public Goods*. University Park, PA: Pennsylvania State University Press, 2010.
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