TRAJECTORIES

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CONTENTS

From the Chair: History in the Gear of Social Change

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At times, Karl Polanyi warned, history shifts into "the gear of social change." This admonition appears near the end of *The Great Transformation* (chapter 20), his magisterial account of the development of market society. Institutional arrangements, he argued, develop through a "double movement" by which society contains the disruptions produced by self-regulating markets. Polanyi insisted that these efforts to contain the "cascades of social dislocation" that follow market expansion were reflexive and spontaneous. But they are not guaranteed to succeed. Reflecting on the possibility of institutional paralysis, he turned to an analysis of his own present. And it was not a pleasant present. Writing as World War II was grinding on but at a moment when one could begin to contemplate a post-war world, Polanyi diagnosed the roots of the conflict in a deadlock in the interplay of expansive markets and spontaneous movements of social protection. This deadlock was seized by different forces in different countries, with many eventually turning to the "fascist solution" which eradicated the institutions of democracy through which the reflex of social protection is expressed.

Pointing to the history of mobilization for factory legislation, historical research may dispute Polanyi's contention that self-protection is spontaneous. Starting from different theoretical priors, scholars may contest his assumption that organized classes will be the bearers of social conflict. But his core contrast between reflexive preservation of the system and conscious contestation over the basic framework of economic and social life illuminates our present. As a kind of found experiment, this is a moment that lays bare both the requisites for social reproduction and the possibilities for change.

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Arguably, we are in one such moment which may prove to be a heroic effort at societal repair or a turning point which opens the door to new forms of social organization. In either case, change of some sort will follow. Consequently, this is an opportunity for historical sociologists to think about large-scale processes of social change in real time and to ask questions about the things that rarely if ever are preserved in archives: popular beliefs, partially coherent rationalities, efforts to marshal resources to support strategies, and narratives-in-formation. In our everyday life, we can be the ethnographers we don't have for the rise of capitalism, the genesis of revolution, or the construction of empire. The character of the moment also requires us to pay attention to the balance between processes of reproduction and potential transformation. Among the many surprising developments of the past year has been the rapidity with which the financial industry has roared back into life. After the rupture, as Tocqueville said of what followed the French Revolution, the new regime would be reconstituted out of the pieces of the old. Yet this apparent reconstitution has proceeded alongside significant readjustments in consumer behavior, trade, and employment as well as new tides of political discontent. In our tightly-coupled economic system, any one of these shifts might have the potential to generate new and still more destabilizing feedback loops. Or not. In the end, it all depends. So the key question is: on what?

By virtue of either coincidence or providence, comparative historical sociology is well-prepared to engage these puzzles. As I argued in the *Annual Review of Sociology* two years ago, historical sociology has increasingly turned to questions of emergence and eventfulness (Clemens 2007). Informed by increasingly sophisticated archival research and new approaches to formal analysis, the corpus of historical sociology has highlighted distinctive categories of interesting events and processes: social caging, group emergence, the genesis of cadres or networks of actors, the interplay of multiple institutional orders, and the consequences of exogenous shocks. This, in turn, has led to new strategies for defining cases and structuring comparisons. With respect to the processes of social change and institutional transformation, we have a much better field guide by virtue of the collective scholarship of recent decades. What the subfield lacked at that moment, just two years ago, was a shared orientation to packages of theory and a substantive puzzle comparable to the debates over the emergence of capitalism, revolution, and the formation of the nation-state which had energized earlier cohorts. But events have remedied this absence of an organizing puzzle with quite a vengeance.

Our response need not be presentist, anchoring every discussion and project to the current crisis. But regardless of our particular area of expertise, our theoretical engagement with processes of social change and reproduction can inform the construction of illuminating analogies across time and place. To date, public conversation has been dominated by efforts to draw parallels between the economic causes of the Great Depression and the current moment as well as often facile comparisons between the Roosevelt and Obama presidencies. Yet historical sociologists can also offer powerful comparisons that illuminate the complex political and social responses to crisis, the many ways in which they may contribute to repair, rupture, or transformation. Last summer's ASA session on "turning points" as well as the post-ASA mini-conference on "Comparing Past and Present" provided impressive examples of how to bring historical sociology to bear on our understanding of the processes of social disruption and change.

For that gathering in Berkeley, Rebecca Emigh deserves another round of thanks. With the help of many others, she organized a day of intense conversation that featured exciting new work by graduate students, full professors, and everything in between or along some other dimension. This sets a high standard for the section which will no doubt be met by the sessions that our incoming chair, Jim Mahoney, is organizing for the 2010 meetings in Atlanta. By that time, we will have another round of outstanding scholarship to honor including, for the first time, the Theda Skocpol Dissertation Award. The call for nominations will be forthcoming very soon, but mark February 15 on your calendars as the due date for all the awards.

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Giovanni Arrighi, 1937-2009

Giovanni Arrighi: An Appreciation

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How can one summarize the intellectual contributions of a thinker who ranged in his writing over 700 years of capitalist development, who surveyed the full span of continents, who analyzed social processes ranging from the peasant's dusty field to the "commanding heights" of the world capitalist system, who drew eclectically from classical and contemporary social theory and from an astonishing array of historical sources, and who had a fascinating back story of his own to imprint this work with a sense of vitality, humanity, and perspective? Given this scope and range, it is tempting to summarize Giovanni Arrighi's career simply by observing that *he was right*. He was right about the "signal" crisis of U.S. hegemony occurring in the 1970s, with "terminal" crisis to follow 40 to 50 years later; he was right about the rise of finance as marking the decline of U.S. hegemony (as with all world hegemonies); he was right about the ultimate shift of the center of gravity of global capitalism to the East. There still may be lurking skeptics, but with respect to these and many other of his most central claims, Arrighi's various propositions have never looked better than they do at this moment.

Nevertheless, I want to avoid the temptation to measure Arrighi's intellectual contribution by tallying up his various predictions, for if some predictions were right, others were plainly wrong. More importantly, while Arrighi was bold—and playful—enough to freely offer his views of what he thought the future would hold (he usually hedged his bets by offering a number of possible scenarios)—his enterprise was not primarily predictive but rather explanatory. In this sense, we shouldn't gauge Arrighi's accomplishment by scanning the horizon for emerging events that may or may not validate his perspective. If there is one thing that Arrighi taught us, it is that capitalism as a historical social system is continually shifting its shape. While Arrighi was as imaginative as anyone in anticipating the form these various shifts would take, he could not possibly anticipate every twist and turn. Indeed, the capacity for surprise is for me one of the great joys of Arrighi's work.

So in this sense, we should not judge Arrighi's contribution by looking forward. Neither, I would argue, should we judge his contribution by looking back. Historians and area specialists will quibble with many of the details of Arrighi's account of ancient Chinese political history, the dynamics of Britain's imperial economy, or the trajectory of Cold War politics, among many other events depicted on Arrighi's broad canvas. This is to be expected. Without giving Arrighi too much license with history, I do want to remind readers that his objective was not to provide a definitive account of any of these events, but rather to use the past selectively in order to tease out recurrent patterns that could provide clues to the nature of the current conjuncture.

For these reasons, I propose to examine Arrighi's contribution not by how well his arguments delineate the past, nor how these arguments project into the future, but rather by how Arrighi's analysis illuminates the *present* moment. In this regard, my judgment is that Giovanni Arrighi has provided one of the most penetrating and original accounts of the dynamics of contemporary capitalism that we have available—an account fully worthy of some of the best writings of Marx, Weber, and Polanyi—all thinkers on whose insights Arrighi builds. It is not a perfect account by any means—there are gaps in it, and some inconsistencies—but without this account, I think we would be utterly lost in our attempts to come to grips with the current crisis. In the remainder of this essay, I want to elaborate on this assertion by pulling out a few key insights that Arrighi gives us for orienting ourselves to recent events in global financial markets. This is not intended to provide a comprehensive overview of the body of work that Arrighi produced, but rather a highly selective tour focused especially around arguments developed in *The Long Twentieth Century* (1994) and the middle sections of *Adam Smith in Beijing* (2007).

First, to start with the most fundamental of these insights, is the very notion of *financialization*. It is easy to forget—now that talk of "financialization" fills the nightly news broadcasts—that not so very long ago, the term evoked raised eyebrows and puzzled stares. Arrighi was one of the first, if not *the* first, to use the term financialization. He used the concept in a very specific way to refer to a phase of capitalist development in which profit making occurs increasingly through financial channels rather than through trade and commodity production (Arrighi 1994). Not only has Arrighi been vindicated by events, he has also been vindicated by data, which very strongly support the claim that the U.S. economy has undergone a process of financialization in recent decades. According to data I've analyzed, the financial sector generated between 25 and 40 percent of total profits in the U.S. economy in recent years. And this figure, while striking, is likely a conservative estimate of the extent of financialization in the U.S. economy because *nonfinancial* firms are themselves heavily reliant on financial sources of revenue. In this sense, an adequate measure of financialization would account for both the profits of the financial sector and the financial income of nonfinancial firms. Once we've made these adjustments, financialization looms very large in the U.S. economy indeed (see Krippner (2010) for elaboration on these points).

Why is this significant? Consider the most prevalent account of the current crisis. According to this account, the implosion in global financial markets that began in September of last year was the inevitable result of a speculative mania in the U.S. economy centered on the real estate market (e.g., Shiller 2008). This view rests on a now venerable academic literature that describes properties *intrinsic* to financial markets that generate speculative bubbles. In particular, the tendency of economic actors to become overconfident in extrapolating from current high returns becomes a self-fulfilling prophecy as investors acting on the belief that asset prices will continue their upward trajectory propel markets ever higher (Kindleberger 1978).

For Arrighi, the current financialization is only the latest episode in a recurrent historical pattern characterized by the regular alteration of two phases: a phase of "material expansion," in which profits accrue through the normal channels of trade and commodity production, followed by a phase of "financial expansion," in which profit making shifts from trade and production into financial channels

There is little doubt that a powerful asset price bubble fueled the rise—and now fall—of finance in the U.S. economy in recent years. In this regard, the conventional view offers a compelling account of the current crisis, although it is also limited in important ways. Most critically, the emphasis on processes *internal* to financial markets means that this approach cannot explain why certain *historical* periods seem more prone to episodes of financial exuberance than others. In order to understand why the decades since the 1970s have been characterized by serial asset price bubbles, for example, we cannot simply point to the tendency of investors to become overconfident following a period of sustained prosperity. Rather, it is necessary to put speculative manias on a wider analytical canvas by investigating the social and political conditions that provided fertile ground for the turn to finance in the U.S. economy over the last several decades. Such an analysis suggests that financialization represents a broader transformation of the economy, with deeper historical roots, than a focus on speculative manias allows.

In short, there is a broader political economy of the turn to finance, and this is precisely what Arrighi's analysis of financialization provides. This brings us to the *second* key insight offered by Arrighi—namely, that the current turn to finance, as with all such periods of financialization, is a response to crisis. In particular, Arrighi suggests that the current turn to finance has structural roots in the stagnation of the 1970s, a period in which intensified global competition eroded the profitability of U.S. firms, encouraging capitalists to withdraw from productive investment and instead channel capital toward financial markets. Of course, the notion that the origins of our current predicament can be dredged out of the sediment of the 1970s is not unique to Arrighi, but shared by a range of schools of political economy, particularly of the Marxian variety (e.g., Bellamy Foster and Magdoff 2009; Magdoff and Sweezy 1987). What sets Arrighi apart from this broader body of the scholarship is the way that he situates his analysis on a larger world historical scaffolding.

For Arrighi, the current financialization is only the latest episode in a recurrent historical pattern characterized by the regular alteration of two phases: a phase of "material expansion," in which profits accrue through the normal channels of trade and commodity production, followed by a phase of "financial expansion," in which profit making shifts from trade and production into financial channels (Arrighi 1994). The phase of material expansion coincides with the emergence of a new hegemonic power whose ascension to the leadership of the world capitalist system rests in part on some organizational innovation that creates expanded opportunities for enterprise – the invention of the joint stock company in the 17th century, or the vertically integrated multinational corporation in the 20th century, for example. Initially, this innovation is associated with "positive sum" competition among capitalists in different national economies, as the expansion in the productive capacity of the economy far outstrips the claims of competing capitalists for a share of the profits. As the material expansion proceeds, however, competition intensifies and eventually becomes "zero-sum" in nature, driving down returns from productive investment and precipitating a shift into financial activities as firms search for refuge in an increasingly adverse economic environment. For Arrighi, this shift into financial activities signals the beginning of the end of the current hegemony.

For some, this theoretical apparatus – in which shifting world hegemonies are tied to the regular alternation between material and financial expansions – evinces a kind of hard structuralism. I disagree with this assessment: I find Arrighi's basic framework to be very supple and adaptable. In this regard, if there is a flaw here, as Arrighi himself acknowledged, it is not that this formulation is too rigid but rather that it is somewhat indeterminate.

This brings us to a *third* key insight that Arrighi offers about the process of financialization which concerns the central role of the state in the turn to finance. For it is not only the tendency of capitalists to switch into financial activities when faced with declining profits that drives financialization, but also the state's tendency to compete aggressively for capital in international financial markets as tax revenues decline, ballooning state debts and creating profits for the financial sector intermediaries that finance these obligations. The indeterminacy in Arrighi's analysis here reflects the fact that it is not always entirely clear whether the state or capital leads this process, with each of his major works *The Long Twentieth Century* and *Adam Smith in Beijing* suggesting a somewhat different emphasis in this regard. Indeed, from the dizzying heights of world historical analysis, it is sometimes difficult to make out state and capital as distinct actors, and Arrighi further blurs the line between these agencies by invoking a "bloc" of governmental and capitalist organizations that are presumed to work in tandem. Nevertheless, Arrighi's insistence on the centrality of the state is of fundamental importance, and in this regard, his account again diverges from the conventional account of the current crisis where the state is notable mainly for its *absence*—and in particular for its failure to regulate shady mortgage dealings. Arrighi's account, in contrast, suggests a far more active role for the state, not merely in failing to *contain* financialization, but in structuring and organizing the turn to finance in the first instance.

A *fourth*—and final—key insight given to us by Arrighi is the notion of the "financial fix." Actually, Arrighi did not (to my knowledge) use the term "financial fix," which was introduced by Beverly Silver (2003) drawing on Arrighi's analysis of financial expansions and on David Harvey's (1999) closely related notion of the "spatial fix" in order to refer to the manner in which financialization allows capital to temporarily overcome barriers to its continued accumulation. The financial fix is certainly one of the most intriguing ideas developed out of Arrighi's work, and Silver has made a major contribution in elaborating on the concept and explaining how financialization resolves contradictions of accumulation *at the point of production*. Just as the spatial mobility of capital alleviates competition in overcrowded production locales, so too the diversion of capital from production into finance and speculation allows capitalists a reprieve from competition, and also gives capital critical leverage over labor. But, of course, the claim in Arrighi's work is broader than this: financialization is presumed to have resolved not only the crisis of profitability by alleviating competition among firms and disempowering labor, but also the wider social and political crisis in which this economic crisis was embedded (see Arrighi 2007; Arrighi and Moore 2001). In this regard, Arrighi has provided us many tantalizing hints but not, I think, a full accounting of how the financial fix offered a resolution to the crisis conditions of the 1970s—and how that resolution is now unwinding before our very eyes. This is not to find fault with Arrighi's magisterial vision of world capitalist development, but only to suggest that he left some work still to be done.

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Conference Report

The Past, Present, and Future of Comparative Historical Sociology: Reflections on the Comparative-Historical Mini-Conference

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The theme of this year's comparative-historical mini-conference was "Comparing Past and Present." It reflected the best of our sub-field, with lively yet cordial debate, a plurality of approaches to understanding historical events and their contemporary relevance, and what seems to be the hallmark of comparative-historical sociology: grappling with big sociological questions. As well, the conference demonstrated the potential for comparative-historical sociology's relevance to understanding contemporary sociological and political problems. In this way the mini-conference reflected the past and present of comparative-historical sociology, weaving together historically central themes and debates in our sub-field to relevant contemporary concerns.

In attending the conference I think we were able to see the past in our present, the present in our past, and through seeing both of these we can see a potential future for comparative-historical sociology. In this essay I will discuss what I observed from the vantage point of a young sociologist, concluding with what I think this means for the future of comparative-historical sociology. In doing this I hope to suggest a direction for becoming more than a "luxury good" in sociology.[1] Comparative-historical sociology offers a distinctive set of concepts for understanding social processes (e.g., path dependence, contingency, conjuncture) that would benefit sociology broadly as a discipline. Thus, we are more like water for the discipline than we are a fine wine. Two caveats are in order before I begin. First, in making sense of the conference and suggesting a path for our future I will draw liberally from the papers to make what I think are the relevant connections across them. Emily and Isaac have offered me complete freedom to write this as I see fit, and I hope the authors of these papers find something to appreciate in my characterization of their presentations. Secondly, because the mini-conference was organized as overlapping sessions I could not attend all the sessions to see all the papers, so I will only comment on those I was able to attend.

The Past in the Present

The opening and closing plenaries each revisited central theoretical and methodological debates around how to conceptualize time, how to observe temporal processes, and how to build theoretical models that reflect our preferred conceptual and methodological apparatuses. In the opening plenary John Hall, Edgar Kiser, and Jeffrey Paige developed statements on how to build theory within a comparative-historical framework. Hall proposed moving away from an objective notion of chronological time toward a "history of times" in which we look at how time is lived differently in different periods and how social action is produced differently in this lived time. Time can be experienced differently across time, thereby producing distinctive modes of social action in relation to time. Thus, our understanding of social action in any historical moment must be situated in subjectively understood time. Paige tackled a similar problem empirically by questioning the concept of "revolution," focusing on a version of a "negative" case in Bolivia. Unlike the typical negative case where an event theoretically should have happened but did not, in Bolivia the event (revolution) happened, just not the way theory suggests it should have happened. The standard temporal sequence of events culminating in the seizure of the state (which has not happened in Bolivia) does not apply to the Bolivarian revolution. Instead, electoral change, namely the election of Evo Morales to the Presidency in 2005, produced a massive shift to the left in Bolivian politics. Subsequently, a democratically elected Morales dramatically increased the minimum wage and nationalized the gas industry. With such a distinctive sequence of events has the contemporary concept of revolution reached the limit of its his-

[1] I am stealing this phraseology of comparative-historical sociology as a "luxury good" from Edgar Kiser, who credited it to Monica Prasad.

torical usefulness, as Hall argues regarding our conception of time? Does the Bolivarian "revolution" suggest we need temporally-dependent theoretical concepts for bridging the past and the present? In contradistinction to Hall's and Paige's historically-dependent theory-building projects, Kiser gave us a reason to answer no to the above questions by arguing for a realist program of theory-building in comparative-historical sociology. In this model of research, theory actually gives us key concepts and causal mechanisms to deploy in understanding the past, suggesting we need temporally-independent concepts that will bridge the past and the present.

Versions of this debate on using theory in comparative-historical sociology have been played out in the pages of our top sociological journals. In this way the conference reflected the past of our sub-field lurking in the present. It is not clear how to resolve these debates, and more than likely that we will continue them for years to come, analyzing a history that has yet to occur. Perhaps the unfolding of history will point us toward nuances in our big debates we did not see before, even if not a settling of the debate itself (which of course seems unlikely). Hall and Paige each suggest we need to continually revise our theoretical tools to meet the unfolding of history, while Kiser's model suggests revising entire theoretical programs if history challenges them. Either way theory is central, and must continually interact with empirical research. And if nothing else as a PhD candidate having perhaps more recently than others read these debates, there is a level of voyeuristic excitement in seeing the debate unfold in front of you. The past is always lurking in the present.

> *[John] Hall and [Jeffrey] Paige each suggest we need to continually revise our theoretical tools to meet the unfolding of history, while [Edward] Kiser's model suggests revising entire theoretical programs if history challenges them.*

The Present in the Past

If the past is always lurking in the present, we can also see the present shaping what we see in the past. Many of the papers at the sessions sought to use our theoretical and methodological tools to address currently relevant empirical and political questions. Of the sessions I was able attend, papers addressed the current economic crisis and the potential decline of the U.S. empire—hot topics in the current political climate. Glancing through the remaining sessions one could glean papers addressing immigration law, political asylum, forms of political action, religion and politics, and debates over the use of torture—more contemporarily relevant questions.

The session I attended on "Economic Systems" generated significant debate over why the economic crisis happened and how to politically address it. The session started with Rebecca Emigh's paper on capitalist transitions, and in some ways was reminiscent of the past in the present by revisiting many of the old themes discussed at the opening plenary. Given so many trajectories to capitalism can we suggest any unifying explanation of the capitalist transition? Is theorizing about these transitions fruitless? Turning to a classic mode of theorizing for comparative-historical sociology, she suggests thinking in terms of necessary and sufficient conditions for a capitalist transition. Comparing England, Tuscany, Russia, and China she finds that sectoral relations facilitating industrial growth were present prior to the transition in all cases suggesting they may be a necessary condition for the emergence of capitalism.

Turning to more contemporary economic change Monica Prasad offered a novel explanation of why the current economic crisis began. She focused on the controversy over the repeal of Glass-Steagall to overturn the legal distinction between commercial and investment banks in the U.S. The novelty of this is that the European countries never had such a legal distinction, leading us to wonder if the repeal of this law contributed to the onset of a financial crisis in the U.S. (as has been argued) why did Europe not experience the crisis decades ago? Prasad argues that the reason the repeal of Glass-Steagall had an effect in the U.S. is because there is a credit-welfare trade-off in which citizens can obtain necessary goods (education, health care, etc.) through either the welfare state or through taking on debt. The absence of strong welfare state provisions in the U.S. led Americans to take on debt, ultimately leading to a credit bubble once the repeal of Glass-Steagall facilitated speculative investing. Europe had no need for such credit given their welfare state provisions, limiting the size of their financial system and the potential for speculation in the absence of a separation of commercial and investment banking.

Fred Block took the crisis as the starting point in his paper. In an appeal to begin a "new New Deal," he outlined four elements to reinvigorate economic growth, both domestically and globally. He argued for 1) income redistribution to facilitate demand, 2) creating new sources of demand (e.g. green projects), 3) the regulation of speculation to prevent the recurrence of a financial collapse, and 4) the channeling of previously speculative capital into productive uses. Block optimistically saw the current crisis as an opportunity for populists to seize on the conflicts within elites, potentially generating elite allies to mobilize for at least some of this new New Deal.

These papers were provocative enough to generate significant discussion. Given the current economic crisis much debate revolved around the practicality of Block's new New Deal. Could these changes be achieved at a global level, where Block suggests they must be (as well as domestically)? Could all of them be implemented? One is left wondering if divisions among elites, somewhat like Emigh's sectoral relations, are potent enough to generate significant political economic change. While Prasad generated a coherent and provocative theory of the current crisis, is it empirically correct? The discussant Marion Fourcade showed that the credit expansion is not linked to new spending for things the welfare state fails to pay for. But, home mortgages, the primary use of new credit, were also likely used to finance education and potentially other consumption tied to the absence of a welfare state. Clearly each of these papers opens up new lines of inquiry into how our past shapes our present.

Another session, "Empire," took a comparative-historical lens to the question of imperialism, which is often underplayed in contemporary politics but is as relevant as economic crisis. Julian Go compared America's contemporary imperialism to Britain's imperialism leading into the early 20th century. There are clear similarities between these empires, such as their use of military force when their economic relationships begin to break down. However, Go found important distinctions as well. The key distinction between these is that while Britain engaged in colonialism the U.S. has established client relationships in which other states are dependent but not directly controlled by the U.S. This is reminiscent of Paige's time-dependent notion of revolution. Here we see that what imperialism is and how it works is contingent on the historical moment of the empire. U.S. imperialism post-dates colonial practices, meaning imperialism has to operate differently. The question for contemporary scholars is, when will the U.S. empire break down, and what imperial power will replace it?

Krishan Kumar also examined the British empire, but asked how empire shapes national identity. He argues that empire creates a system of meanings in the imperial country, and national groups position themselves relative to this system of meanings. Thus, we can see that identities are shaped within imperial countries, with this system of meanings around the notion empire shaping individual identities. In this way Kumar uses historical sociology to bridge the micro and macro.

Finally, Paul Frymer returns to the U.S. empire, but takes us to its early imperial adventures. He demonstrates that contrary to assumptions of a weak state in early American history, the state was quite active in expanding its territorial reach across the continent. Focusing on the creation of treaties with France and Spain to capture pieces of the continent and the use of force against natives on the continent, he demonstrates that the U.S. has perhaps been an imperial power since its founding even if through more subtle means at times (e.g. treaties). Again, we see that what constitutes imperialism may be dependent on our notion of what imperialism actually is.

As with the debate around the current economic crisis, this session generated debate over what empire is and where it is going. In particular, when and how will the U.S. empire decline? How should we think about empire generally when what it actually is and how it operates can shift and mutate over time, harkening back to Paige's earlier question of what constitutes a revolution? As with any good debate, these questions were raised and debated, but the conversation will have to continue. Nevertheless, what we see here and in other sessions at the mini-conference is an attempt to use the past to inform the present, from the constitution of economies to imperial projects.

The Future in the Past and Present

In the closing plenary James Mahoney put forward a programmatic statement for directly using the past to understand the present and to even make predictions about the future. At least one goal in his developing this statement is to illuminate present political and policy-oriented discussions using insights from comparative-historical sociology. His argument was no doubt controversial in a room full of diverse historically-minded sociologists, yet there is undoubted appeal in his ultimate goal. Sociology is only relevant if others think it is relevant, and it is our job to use the past to inform the present and future. To use Mahoney's example, most of us want to know, whether or not universal health care is good or bad for us. As historically-minded sociologist we have a unique vantage point from which to actually answer that question. Even if we cannot answer the moral question (good or bad) we should be able to answer the empirical question, what kind of consequences will [insert policy] have? Mahoney suggested several layered conditions under which we may be able to produce an answer. He starts with the standard social scientific unconditional predictions or covering laws that apply universally. In the absence of being able to generate universal predictions we can develop scope conditions to enable us to make more limited conditional predictions. In the absence of being able to generate these scope conditions he argues that perhaps we can use the past to generate broader theoretical "lessons" (rather than predictions) that may help us use the past to understand the present and suggest what the future may hold. It is this kind of thinking, searching for the conditions under which we can make predictions or draw lessons, that will make comparative-historical sociology relevant to sociologists studying contemporary issues, and perhaps to policy-makers and general political discourse.

Following Mahoney were two exemplar pieces of comparative-historical sociology that highlight the range of methodological approaches we can take to linking the present to the past. Andreas Wimmer and Yuval Feinstein employed event history analysis to analyze nation-state formation from 1816 to 2001, searching for a globally recurring pattern of state formation. They find that nation-state formation is most probable when power within a territory shifts in favor of a national movement. Doing this they demonstrate the usefulness of statistical modeling in comparative-historical sociology when one is searching for probabilistic causes of a recurring outcome. At the other end of the methodological range, Isaac Reed and Julia Adams present seven theoretical models for understanding cultural processes in the rise of modernity (epistemic rift, racial recognition, the fall of patriarchy, social subjectification, compensatory reenchantment, ideological totalization, and memetic replication). These seven models represent the existing options for comparative-historical sociologists grappling with how culture shaped the emergence of modernity and its trajectory, and their goal is to offer these for us to combine and draw on in understanding cultural processes of modernity. While not explicitly methodological in their goal, the consequence is to suggest observing cultural signification in the origins and development of modernity which requires analyzing historical documents of what people say and do. These two papers then illustrate the range of options available to comparative-historical sociologists, that coupled with the range of theoretical models suggested in the opening plenary provide a plethora of ways of linking the past to understand the present and think through the future.

Conclusion

In conclusion I think that this mini-conference ultimately reflected the ways in which our subfield can shape the future of sociology as a discipline as well as the broader project of public sociology. We see in Rebecca Emigh's work the importance of thinking in terms of necessary and sufficient causes. Monica Prasad's paper suggested how complex contingencies can produce distinctive trajectories. Julian Go's research points toward the importance of timing in the constitution of social processes. But in a more general way these conceptual tools and theoretical models that comparative-historical sociology offers are not merely relevant for big macrohistorical processes of the past like the emergence of modernity, capitalist transitions, revolutions, and imperialism. They have relevance for all of sociology, down to the micro-level interactions of everyday life. Understanding this moves us from being a luxury good within sociology to defining the core of sociology. We understand that events develop in unpredictable sequences, full of contingencies, conjunctures, and path dependencies. The timing and duration of events matter in producing their effects. These are the tools we offer not because we study the emergence of big transitions, but because this is how social life is organized and develops.

Thus, this mini-conference illustrates how the future of comparative-historical sociology has the potential to shape the future of sociology (this from a wide-eyed PhD candidate). Comparative historical sociology has always grappled with the big questions of macrolevel change, but it has deeper implications than the historical topics we tend to focus on. The diffusion of what we claim to be central to understanding modernity, economic development, and imperialism, namely timing, sequencing of events, contingencies, conjunctures, and path dependence to name a few, are likely also useful for sociologists studying contemporary employment relations, organizational change, economic transactions, and family formation to name a few. This mini-conference reminds us that sociology is about social *process*, and the concepts of the comparative-historical tradition point the way toward understanding a variety of those processes. Let's hope that the future of sociology lies in the future of the comparative-historical sociology.

Books Symposium

Author Meets Author Meets Critics

The Underdevelopment of Capitalism: Sectors and Markets in Fifteenth-Century Tuscany, by Rebecca Jean Emigh (Temple University Press, 2009).

The Art of the Network: Strategic Interactions and Patronage in Renaissance Florence, by Paul D. McLean (Duke University Press, 2007)

These two wonderful books were the subject of an Author Meets Critics panel at the 2009 ASA annual meeting in San Francisco. What follows are the comments of the three critics (Emily Erikson, Randall Collins, and Rosemary Hopcroft) followed by responses from Paul McLean and Rebecca Emigh.

The editors would like to thank Richard Lachmann for arranging for, and gathering together, the texts that make up this portion of the newsletter.

Comments by Emily Erikson

University of Massachusetts, Amherst

First off, these are two remarkable books. They are theoretically sophisticated, empirically rich, and make substantive contributions to well defined problems in the fields of networks, economic development, and comparative-historical sociology. Additionally, both use a multi-method approach that samples evidence at different layers of social process. This strategy is not only methodologically sound, it is also, in my opinion, an enviable approach to theory-building, based in the mechanics of everyday interactions. I also can't help but argue that both are network books. This is more obvious in McLean's *Art of the Network* but Emigh's argument also turns upon relationships, in her case, inter-sectoral relations.

There is a lot going on in each book and both make more than one type of contribution, but they also make more immediate arguments, which I want to address first. For McLean the big substantive payoffs of the book come with an explanation for the delayed emergence of a modern Italian state and the construction of the modern conception of self. McLean argues that patronage ties so enmeshed the Florentine government, it couldn't achieve any real measure of autonomy. In both stories (Emigh's and McLean's) a lack of decoupling doomed Florence and Tuscany to economic and political stagnation. In McLean's case this is particularly interesting as social networks were the glue that bound everything in place, whereas, today social networks can be considered mechanisms for decoupling from formal institutions exactly because they are more fluid than institutional ties. For example, there are numerous stories of networks being used to bypass red tape within organizations. I wonder if this is a historical transition in the effect of network ties or a contextual effect or something else?

McLean also argues that these network interactions were the key component blocking state formation—and not class or identity-based interests. There are two ways to interpret this. Networks have the capacity to explain social outcomes at a finer level of granularity than most theoretical concepts in sociology; it is just a different way of describing a slightly clunkier concept for identifying something that is—at bottom—also simply a set of relationships between individuals. For example, you can use network tools to analyze and describe class structures. On the other hand, networks can be conceptualized as entirely distinct from things like class or elite groups. I am wondering what is more accurate in the case of Florence? Is the network a better description of the social process that has been roughly identified by other researchers or does it fundamentally alter the way we understand the process to have unfolded?

McLean doesn't just address historical problems with network analysis, he also advances network analysis by adding substantive content, embedding it within a context and looking at tie formation. But, this process in the book takes a lot of research that is very specific to the period under study. My question here is whether it is always necessary for network analysts to engage in this kind of deep contextualizing research and if it is always feasible.

Finally, McLean intimates that hypocrisy is responsible for the emergence of the modern sense of self. To me this is a very attractive idea, and I love the contradiction between having to know your true self in order to present a false front. I think it sits well with Gore Vidal's portrayal of the

United States as the land of hypocrisy when we also all know it as the land of individuality. I was wondering if the author could reflect on whether this production of self is a one-time event or something that might be operationalized and studied cross-nationally, for example.

Emigh focuses on capitalism rather than statemaking. She convincingly argues that the spread of urban markets into rural Tuscany eroded rural market institutions. If this process also undermined the possibility of a transition to capitalism, this leaves us with the paradoxical conclusion that the spread of capitalism impeded the growth of capitalism. One the one hand, this is good because it reminds us that within capitalism it is very easy to settle into suboptimal situations. On the other hand, it is confusing.

As I understand it, Emigh subscribes to a Weberian definition of capitalism (and a Marxian definition of process). Capitalism therefore depends upon the orientation of social actors to rational decision-making and the accumulation of profit: when the majority of actors are rationally pursuing economic profit, we have capitalism. But it is also universally recognized as our current economic system, which began operating somewhere around the nineteenth century. So, the word has an analytic meaning and a material-historical meaning, both of which are operative in the text. I ended up translating the idea 'capitalism delayed capitalism' into 'a rational approach to profit making, in Tuscany, impeded the progress toward a transition into capitalism.' But since 'industrial capitalism' also appears in the text, I really think that the phrase means that locally profitable decisions in Tuscany slowed down the progress toward the industrial revolution in Tuscany and/or the potential levels of economic development. None of this points to a fault in Emigh's text since capitalism is widely used to mean all of these things, but I can't help but wonder if a little more clarity would help. In particular, if economic development is treated as synonymous with capitalism, it is very hard to come to the realization Emigh leads her readers toward—that capitalism is not always the most efficient economic system and does not inevitably lead to progress.

I also want to ask, not only about the definitions, but also the actual events. In the book I saw more than one possible path linking the particular form of sharecropping to the failure to transition to capitalism. It is possible that, one, although it did increase yield, the urban-rural sharecropping did not increase agricultural productivity *enough* to support the increase in urbanization needed for the growth of industry. A second possibility, emphasized in the book, is that the form of sharecropping undercut the emerging domestic market. I hesitate over the second reason for a couple of reasons. Based on the evidence in chapters 6 & 7 it seems that sharecropping did produce wage laborers. This means that it must have contributed to creating a labor market. Also, although the market for land was clearly overtaken by Florentines, this does not mean all markets were decimated. In fact, if rural sharecroppers had increased their profits, but could no longer buy land, it seems plausible that they would have looked for other valuables in which to invest – and pass along as part of the dowry. The increased disposable income could have increased the market for luxury goods (i.e. pepper, salt, imported cotton). So, although I believe it's a very elegant argument, and that there is ample evidence that land markets were destroyed, was that enough to wipe out all market institutions?

I want to also ask would the presence of a rural gentry or a domestic market have been enough to produce the transition to industrial capitalism in Italy? There are problems with the state in what was to become Italy, but I'm particularly interested in addressing the lack of expansion in overseas trade. Emigh seems to implicitly take the side of Malthus/Dobbs/Brenner in the very old, yet unresolved debate. She argues that its really domestic production that matters, not overseas trade. Although she does inject markets into the equation; it is just that they are local or sectoral markets. Britain, the implicit case comparison, had well-developed local markets—but also a hugely expanded market in the American colonies and a tremendous increase available raw materials. Could economic development in Britain have been sustained (as Wallerstein puts it) without the international setting? And by comparison, wasn't this lack also partly responsible for the lack of development in Tuscany.

Finally I want to pose a question to both authors. Both books have an interesting tension between the analytic and historical. In Emigh, Florentines are capitalist before capitalism. In McLean, Florentines are networkers before the 'age of networks' (i.e. now). In both cases we have types of individual behavior (networking, profit maximization) that are considered typical of certain periods. In both cases it is clear that people engaged in these behaviors in many times and places – modern, pre-modern, medieval, classical, as far back as you want to go. But, there are also periods defined by the predominance of these behaviors. The results are conundrums such as, how can there have been one transition to capitalism when many societies and individuals have been capitalist throughout world history? Or, how can there be a network society when all societies are organized through social relations?

It seems to me that this is a central problem with doing comparative historical work. We want to explain historical transitions (big, unique events) with repeatable social processes, but these kinds of explanations make the transitions seem less big and unique.

I think both authors have done a wonderful job of explaining historically situated transitions (or the lack thereof), but when they weigh in on "the great transition" (and by this I mean modernity more so than Polanyian industrial revolution), they do so by generalizing features of *this one great transition* to the extent that it is more like a particular realization of a type of transition that has occurred many times. To be clear, in Emigh's case, I do not mean that a hundred or so countries have transitioned to capitalism; I mean that, for example, inequality and economic prosperity have increased and diminished in hundreds of thousands of societies over hundreds of thousands of years.

I wonder how the authors felt about this tension in these works. Should we think more about the general features or the specific features, and which is the real explanandum? And I can't help but ask further to what extent do the authors believe that comparative-historical sociology should continue to engage with "the big question"—the transition to modernity.

Comments by Randall Collins

University of Pennsylvania

Covering two books about Tuscany in the fifteenth century is something like pairing two books about the United States in the twentieth century, if the books were Goffman's *Frame Analysis* and Domhoff's *Who Rules America?* The micro-macro connection is crucial here, and Paul McLean's *The Art of the Network* is a major effort to examine the micro side of macro phenomena: network dynamics, career-making, and patronage politics. He does this by examining hundreds of letters to powerful patrons asking for jobs, posts in military expeditions, tax and debt relief and other favors. This is a major step forward in documenting the micro-moves which are made and cultural tools which are put into use.

Some aspects of using networks are no doubt applicable across historical periods and into the present. Nevertheless, what jumps out of McLean's materials are some major historical changes, for instance in what "friendship" means. In contemporary American usage, friends are persons you have fun with in your private leisure; at their most serious, friendships are backstage relationships where personal feelings are revealed and discussed sympathetically. But in Renaissance Florence, *amici* are much more strategic; the term implies being a loyal member of a political faction of fellow clients of a particular patron. We are in the presence of a structure built around patronage network politics, where a distinct private sphere of leisure and personal matters apart from the public sphere of power and work does not exist. Although Tuscan society is not a traditional one in many respects, as both McLean and Emigh argue, it is not a modern one either. What lies in between these two concepts is something that both books help to reveal.

McLean's analysis of patronage maneuvers takes on deeper significance if we fill in the larger social context not explicitly discussed in the book. During the 1400s, there were five large Italian states, including the territorial possessions of the papacy, all more or less constantly at war in one alliance configuration or another; plus a large number of smaller city-states, also under attack; plus two big external powers, Spain and France, which repeatedly intervened in Italy. Foreign relations were largely dynastic politics based on marriage alliances, and these easily gave excuses for war since one could usually find some ancestral claim to inherit territories from previous diplomatic settlements. There was a great deal of side-switching, breaking and remaking of alliances, and a mafia-like atmosphere of conspiracy; above all since the church was an instrument of factional politics, cardinals and bishops were appointed through deal-making and family connections—

despite the ideology of clerical celibacy—and papal elections were notoriously influenced by bribery. Moreover these states had no reliable monopoly on violence; military forces were largely mercenaries from foreign regions, who might easily quit during a campaign, go on strike for better pay or to protest non-payment; treachery was common and clever political leaders might follow up negotiations with assassination. Renaissance Italy was among the most cynical periods in human history, with an extremely sharp division between the rhetorical protestations of frontstage discourse and the backstage realities. McLean's data hint at this in his finding that patronage-seeking letters use more inflated and idealized rhetoric the more distant the link geographically or in an upward status direction.

Domestically, Florence was also full of struggle. Street gangs of the poor practiced extortion from passersby, engaged in stoning each other, and gave a riotous tinge to public festivals; aristocratic youth also formed carousing groups which brawled in the streets. During times of political conflict, rival youth gangs played a major part in factional attacks. Thus we should think of personal connections as operating in a situation where state monopoly of violence was weak, and volatile group alliances were potentially dangerous.

McLean lacks data on how successful were the patronage efforts of his letter-writers. From the point of view of the patron, there must have been many rival requests, and their strategy must have been to spread favors around and balance shifting factions. Networks were flexible and voluntaristic, not fixed—a big change from the strongly corporate guilds of the 1300s, as McLean notes. But Florentine networks were not modern either. Today these kinds of patronage relationships would be regarded as illegal, corrupt, and scandalous. That is to say, with the rise of the legalistic modern state and the spread of impersonal bureaucracy, personal networks are under pressure to severely limit what favors they can convey.

Even in the 15th century, those Florentines who not were members of the Medici patronage machine resented it. The revolt which threw out the Medicis in the 1490s, under the leadership of the monk Savonarola and his religious purification movement, sharply attacked favoritism and privilege, especially in matters of taxation—it was an attack on rule by *amici*. Savonarola's movement also directly attacked another favorite tactic of the Medicis—rule by magnificent public spectacles, festivals, gaudy display including the great upsurge of Renaissance art. In terms of micro-sociology, the Medicis ruled not only by networks but by mass collective interaction rituals, generating crowd solidarity and impressiveness. But solidarity in this public form was volatile. Savonarola's movement turned the public festivals against the privileged classes, holding public ceremonies of purification and destruction, such as the "bonfire of the vanities" where luxuries were burned. The reformers mobilized street gangs to ransack homes of the rich searching for luxuries—not unlike the *Basij* militia recruited from among lower-class youth which recently in Tehran attacked educated sophisticates. Servants were encouraged to inform on their masters for their crimes against morality. This puritanical regime failed in only four years, because it had no organizational form to put in the place of networks of favoritism; it could not build an impersonal bureaucracy, and relied both for support and for executive action upon mass public assemblies, whether for preaching sermons or for directly assailing their enemies. The downfall of this republic of religious hysteria came in a bizarrely fitting way: Savonarola was challenged by a rival monastic order to a contest of miracles in the public piazza, monks from both sides attempting to walk through huge bonfires; while they squabbled over the rules of the contest, the fires were drenched by a rainstorm, and the crowd's mood turned against the Savonarola faction; street gangs stoned them, broke up their preaching in the cathedral, and jeered their torture and execution in yet another public spectacle. Eventually the Medicis came back, and the patronage networks solidified into a more rigid structure of dynastic inheritance.

McLean accurately sums up the Florentine experience as a stalled transition to the modern state. Not that the patronage networks were widely accepted as legitimate; it is clear they were resented by many, and that the flowery rhetoric existed to try to cover up this situation, as well as intensify appeals for loyalty where loyalty was most problematic. But the main alternatives in Florentine politics were even more unstable; and no full-scale transition came about, either in state structure or in the capitalist economy. The analytical lesson must be that a personalistic network of favors is not the primary basis of modern social organization, even though networks have been an important adjunct to bureaucratic formality wherever the latter was established. Personal networks mitigate bureaucracy, but they cannot substitute for it.

Rebecca Emigh's The *Underdevelopment of Capitalism* concentrates on the economic side of the failed transition. She raises a neat theoretical problem: Tuscany had all the preconditions but the capitalist transition did not come about; and this is so whether one follows the Weberian theoretical lineage of institutional conditions, or the Marxian lineage emphasizing primitive accumulation. Tuscany had all the social and legal institutions and the material conditions for thriving markets in all the factors of production, including a rural market in land; this should have been decisive, since elsewhere the transition first came about by a takeoff in agricultural capitalism. Instead, Emigh shows, the wealthy urban sector took over the Tuscan rural markets, destroying their dynamics, and thereby cutting off further expansion of urban capitalism as well.

The analytical lesson must be that a personalistic network of favors is not the primary basis of modern social organization, even though networks have been an important adjunct to bureaucratic formality wherever the latter was established.

There is a terminological problem throughout Emigh's analysis, which sheds light on the underlying theoretical problem. Tuscany had capitalism but failed to undergo the transition to capitalism--this self-contradictory formula is repeated many times in slightly different words. Sometimes she substitutes "industrial capitalism" for the second repetition of "capitalism," but that does not solve the problem since what we are seeking is a capitalist takeoff whose first clear exemplar precedes the industrial revolution by 150 years, the speculative Dutch tulip market of the 1630s, which is to say, agricultural capitalism. Weber was clear that there are several kinds of capitalism, some of which have an inner dynamic of transformation while others do not. Weber called the modern form "rational capitalism" but today this seems too contentious and value-laden a term; and to call it "modern" capitalism is a purely relative term that gives no clue to its analytical distinctiveness. A better term would be "Schumpeterian capitalism"—that is, capitalism of constant innovations alike in the productive process, technology, organization, consumption, and marketing. Schumpeter once defined this dynamically self-transforming capitalism as "enterprise carried out with borrowed money"—an analytically useful definition because it includes both the entrepreneurial organization of capital and a financial system which transfers credit from one circuit to another. Schumpeter regarded the existence of crises and business cycles as an historical indicator of the existence of dynamic capitalism, a conception which brings him closer to Marx, who captures one essential component of capitalism that other theorists omit: capitalism generates not only growth but inequality and periodic crisis.

Thus Emigh's problem can be stated more clearly: why didn't the existence of the Weberian (and Northian, Marxian, etc.) preconditions in Tuscany give rise to Schumpeterian entrepreneurial capitalist dynamism? I would stress that it is not helpful to become side-tracked into a meta-theoretical discussion of whether any given conception of capitalist dynamics renders capitalism "inevitable." The question is whether Schumpeter's model of the capitalist process (or some other, superior model) adds the key elements which are lacking in Weberian, Marxian etc. theories of preconditions. We should not regard Schumpeterian capitalism as eternally self-reproducing, once established; Schumpeter himself thought that the entrepreneurial mechanism at its core could be destroyed, for instance by organizational changes and government action. Thus any really good theory of capitalism would explain the conditions not only for its takeoff and expansion, but its crises and self-destruction as well. And in fact Emigh's study is centrally important because it comes so close to this problem.

So what is the answer to the question of the stalled transition in Tuscany? Two answers are prominent. McLean's answer, in effect, is that patronage network politics inhibited the transition to the modern bureaucratic state, and that this undercut the institutional conditions for Schumpeterian dynamics. Part of the question that still needs to be explored is the nexus between financial markets, capitalist entrepreneurs, and the state's own finances. Something must have clicked into place for the Schumpeterian model in Holland at the time of the great tulip speculation; and England at the time of the Glorious Revolution of the 1690s created a crucial financial mechanism in the other early Schumpeterian case. As Geoffrey Ingham showed, modern banking came about when the Bank of England monetized the state debt. This gave the banking system the security to make loans at a velocity of circulation, and an expectation of long-term profits, that set if off from previous private banking operations. The tax-extracting apparatus of the modern state, and the Schumpeterian financial underpinnings for modern capitalism, were raised to a firm institutional level as part of the same process.

Thus the Medicis as bankers must also have been implicated, but in a negative way, in the failed Schumpeterian transition in Tuscany. One wonders if their activity as political brokers did not play into their economic activities in a structurally fateful way. I anticipate that we will learn a lot from studying the three-way nexus of finance, government and markets—for instance, in Simone Polillo's current work on a later historical period on the struggles between conservative banking and wildcat banking.

The other answer draws on a Marxian theme. Capitalism is destructive. It does not just steadily expand; it goes through cycles during which it destroys markets, producers and capital, and impoverishes workers. And down-cycles will not be followed forever by up-cycles; the theory predicts a future downfall of the entire system, as the extent of inequality and concentration of capital grows too great in relation to constriction of labor and lack of consumer demand. The key problem is the balance between capitalist concentration and capitalist diffusion, a balance which keeps capitalism going and expanding (and in Schumpeterian terms, innovating) or on the contrary destroys its own dynamic. Emigh's work reminds us to see this self-destructive quality throughout the history of capitalism in all its forms.

Comments by Rosemary Hopcroft

University of North Carolina Charlotte

Rebecca Jean Emigh's *The Undevelopment of Capitalism* and Paul D. McLean's *The Art of the Network* cover roughly the same period of Tuscan history and complement each other nicely. Emigh's book covers rural development and economic change in fifteenth century Tuscany, McLean's book concerns the culture of the residents of Florence, the city dwellers in about the same time. Both books make good use of original archival research. But it is not just the rural/urban complementarity and the shared historical methodology that makes the two books fit together nicely, it is also their sociological stance of being critical of economic or rational choice models of the actor. In what follows I would like to critique the two books separately, and then critique their critique of rational choice theories. I argue that in both cases they are fighting straw men, and suggest that their historical sociology can stand on its own better without such activities.

Emigh's book utilizes a negative case methodology by looking at the case of Tuscany—a case where it seems industrial capitalism might have emerged but did not. It is a highly valuable contribution since much previous historical sociology has focused on England, where a successful transition did occur, and little of the work examining stalled transitions has focused specifically on the Tuscan case. The book is a nice blend of original archival work with a synthesis of the existing literature. It represents a huge effort which has been many years in the making. The book represents historical sociology at its best, showing how the social context of fifteenth century Tuscany both promoted the rural economy yet in the long run led to its demise.

In the book Emigh argues against a variety of theories of economic change and presents her own "dialectical Weberian" theory that links sectors with markets. She acknowledges that her approach shares some commonalities with theories that are grouped under the rubric "new institutional economics," but she differentiates her approach from economic approaches by stating that (p. 32): ".. markets are not merely economic institutions that are constrained or enabled by culture and politics. They are inherently economic, cultural, and political." In another instance she states (p. 56):"Politics and culture, that may be ignored in economic models, also affect linkages and, more generally, sectoral relations." Yet this is a false distinction. The point of the new institutional economics is that institutions such as markets and laws *are* cultural and political creations, and that the institutional context shapes all economic change. Culture and politics are certainly not ignored.

Given this critique of the new institutional economics, it is ironic that one of the most illuminating chapters in the book is the chapter on sharecropping. This chapter shows how sharecropping increased with distance from Florence because of the increase in monitoring costs (p. 116), and it is based strictly on the transaction cost theorizing that is at the heart of the new institutional economics.

Emigh links her theory of markets with a theory of sectors to explain the Tuscan case, in which the Florentines had more resources and power and were able to dominate rural markets for land. This led to the unmaking of rural markets. Yet the role of sectoral inequalities in inhibiting economic growth is also entirely consistent with economic approaches such as those of Epstein (p. 21) or other economists' work on urban bias (p. 52).

Emigh further states that economic theories suggest that "capitalist development perpetuates itself" when conditions are right, yet suggests that the Tuscan situation proves otherwise. She suggests that you had all the conditions for capitalism in Tuscany along with emergent rural capitalism (p. 21), but over time it "underdeveloped." I would like to take issue with both premises of this argument. First, were there really all the conditions for capitalist development in late medieval and early modern Tuscany? Many analysts, including new institutional economists concerned with economic change in history, have pinpointed several conditions for economic growth. They include the rule of law (plus enforceable property rights and contracts) and low levels of rent seeking or taxation by elites (p. 59). Emigh suggests that these conditions were in place in Tuscany.

Yet is this true? As McLean notes in his book, taxation was high in fifteenth century Tuscany, and was difficult even for very rich Florentines to deal with. A lot of the patronage letters he examines were from people who were trying to get their taxes reduced. Certainly, given that England did not have a system of *direct* taxation such as was already standard in Tuscany in the 1400s until the Napoleonic Wars in the 1800s– well after the industrial revolution, this suggests that taxes may have been more effective in preventing growth in fifteenth century Tuscany than Emigh suggests. Urban control of prices for agricultural products was another indirect method of taxation and added to the burdens of rural producers.

Second, was there really emergent rural capitalism, complete with rural capitalists? Emigh argues that rich Florentines who invested in the land and endeavored to increase production were capitalists. Yet wealthy urban Italians had owned country estates since Roman times, to provide them with fresh produce and an escape from the problems of the city—including plague, epidemics and social unrest (Adrian Goldsworthy, *How Rome Fell*, Yale University Press, 2009). So there was a tradition of such behavior in this region for over a thousand years. Emigh shows that the Florentines did invest in agriculture and improve its productivity, which seems capitalist-like behavior. Yet overall the Tuscan agriculture was not able to produce enough food for its own people. Food had to be imported. England, in contrast, was self sufficient in grain until the nineteenth century.

Nor did the Florentine's rural estates make them very much money. As Emigh notes—most of the landowner's income came from elsewhere, notably trade in cloth. Most of the landowners lived in Florence and were absentee landlords. They were not trying to make as much money as possible from their estates. These rural landowners were nothing like the landowners of sixteenth and seventeenth century England, whose entire livelihood often came from the land. Last, as the economic historian E.L. Jones notes in his book *Growth Recurring: Economic Change in World History* (Clarendon Press, 1993) such periods of intensive economic growth have occurred all over the world at various times in various places, but then similarly petered out just as it did in fifteenth century Tuscany.

In sum, all the conditions for capitalism were not in place in Tuscany. As Emigh argues, the transition to capitalism did not take place in Tuscany in part because of the context of inequality between the urban and rural sectors, but this is entirely consistent with economic theories such as the new institutionalist economics. As an argument against economic theory Emigh's book fails. As historical sociology that demonstrates the importance of social context for economic change it succeeds. This is the value added in Emigh's historical sociology, and straw man attacks on economic theory merely distract from it.

McLean's book is an analysis of patronage letters written by Florentines in Renaissance Italy. All of the letters are flowery and stylized, but often I found them fascinating, especially the recommendation letters between real friends. They gave me the feeling of peering into a world long gone, a world full of friends, enemies, love and hate, things that are entirely understandable because they characterize every human society.

McLean's first purpose is to critique various theories of networks. He critiques James Coleman's theory of social capital because he suggests it does not show how people's identities are created through the process of networking. He criticizes network theorists for putting too much emphasis on a person's location in the network and overlooking the role of individual strategic action. Instead, he uses the identity formation theorizing of Bourdieu, the culture as toolkit approach of Swidler, and Goffman's observations on the presentation of self in everyday life. That is, people's identity and habitus reflects their structural position, while they simultaneously use culture strategically to obtain valued goods and present desired representations of themselves to others. Through his analysis of the patronage letters McLean shows how they do this.

This sort of structural-cum–culture-in-action approach has much to recommend it. Every society has socially agreed upon formulas for attaining valued goals, whether those goods are a high status position, a spouse, or privileged treatment for oneself or one's family members. However, I think McLean's critique of Coleman's treatment of social capital is flawed. Coleman suggests people use their social ties to get ahead, he doesn't say they *do not* influence identity. Coleman was not trying to explain individual identities, and I do not think he would disagree with McLean on this point. Social ties continue to have an important influence on personal identity in our contemporary world. Who you are is in part a product of your position in the social status hierarchy—a position you occupy because of your relationships with others. Think of what Henry Louis Gates Jr. repeatedly said to the policeman who arrested him on his front porch (much in the news this summer): "You don't know who you are messing with!" By that, he meant he is high status and therefore well-connected. Indeed he is well-connected, the president of the United States is his friend!

His second purpose is to contribute to the understanding of the emergence of the modern sense of self. McLean also suggests that these patronage letters throw light on a transitional period between traditional and modern selves, and thus illuminate the emergence of the modern sense of self. Yet the patronage letter has a long history in this part of Italy. It was used extensively by the Romans, for instance. The historian Adrian Goldsworthy in his book *How Rome Fell* (2009) says they were the primary tool for career building and upward mobility among the Roman elite. So if the patronage letter illuminates a transitional self, it was the same self as in Roman times.

I suggest that the people in the letters are not so different to ourselves. What has changed is the circumstances. We no longer have a personalistic state, but a modern state where what counts is the rule of law that is supposed to trump personal connections and status position. The fact that it does not do so in all instances indicates to me the durability of human nature. McLean argues that the efficacy of the personal ties revealed in the patronage letters prevented the emergence of the modern (impersonal state) in Florence. Yet personal ties were the basis of what was a patron-client system, with the Medici family at the very top. The Medici had no interest in changing this situation, and as long as they (or another family) maintained power it was highly unlikely it would have changed. Thus, the efficacy of personal ties was itself based on the concentration of power at the top of the system. The form of the state created the efficacy of personal ties, it was not the personal ties that influenced the form of the state.

As circumstances change, what people do changes. McLean shows this himself when he shows that the content of patronage letters changed over the course of the fifteenth century as circumstances changed. People were responding to a change in the situation—the consolidation of power by the Medici. This is the sociology in this book – that it is the context that matters, not the people. This is not the same as saying that people's actions are overdetermined by their structural position. Individuals' personal situations and motivations differ, so actions differ, even though we can recognize the fundamental humanity in all. This is why I think that McLean's discussion of the emergence of the modern self is flawed. The selves are the same, the circumstances (including appropriate cultural forms) aren't.

In both books, economic theory is often presented as a foil. Yet why? Rather than resorting to straw man arguments against economic or rational choice theories, I suggest that sociologists should do what they do better than the economists. Sociologists are skilled in analyzing context, particularly social context, which is always important because people are social beings. This social context includes local cultures, social groups, networks, institutions, and demographic patterns. Both Emigh and McLean do a good job of analyzing the social context of late medieval/ early modern Tuscany, and how it was not congenial to sustained economic development or the birth of the modern state. I suggest it is this richer understanding of context, especially social context that is the valued added of any sociological analysis. Straw man critiques of economic theory are a waste of time. But who am I to say so?

Reply by Paul McLean

Rutgers University

What are the primary goals of *The Art of the Network* from a comparative-historical sociological perspective? It may surprise readers that this has been, and continues to be, a very challenging question for me to answer! I feel pretty good about the argument I give about how culture operates at the micro level of practical action, and how cultural practices are constitutive for network structures. I'm hoping those arguments will prove enlightening for people. I'm also fairly content with the idea I adumbrate that multiple meanings of a concept (such as honor) can and do accrete over time in ways vital for social interaction. But what about the fundamental questions of social, economic, and political *change*, institutional lock-in, and the rise of *modernity*, that have been the bread and butter of comparative-historical sociology? And what about the comparative-historical method of conducting sociological research? What does the story of Florence contribute on these fronts? And what does *my* research in particular contribute on these fronts? I'm happy to have this occasion, and the comments of these insightful critics, to work out my answers to these questions.

I'll tackle the method question first—not that it's easy, nor will my answer be complete, but it is somewhat more manageable than the 'modernity' question, raised in different ways by all three critics. An important part of what we do is not just tell a story about our own case (although Florence is a powerfully seductive case!). Instead, we provide tools, concepts, ways of thinking, styles of inquiry, to inspire others as they tackle their cases. Hopefully, eventually, rather than 'explaining' modernity, we build up, collectively, a general toolkit of mechanisms, concepts, narratives, what have you, to make more nuanced sense of historical change. I'd be hard-pressed to say, for example, *what* I think Roger Gould's work is—what master analytical framework(s) it provides—yet I think I (and *you*) should ask Roger Gould-like questions.

I do *not* believe that enriching sociological understanding of social dynamics always requires time-intensive "contextualizing research." As Herbert Simon (among others) has noted, we don't always have to break open the black box; we can learn a lot from simplified models, even if we don't exactly know *how* they work. I am not about to complain that other methods 'aren't detailed enough' or 'aren't cultural enough.' In particular, in response to one of Erikson's queries, I would say that societies clearly differ in the extent to which network notions can be usefully applied to explain 'everything' that is going on. The scope with which network activity applies is variable: sometimes limited to very small bands of elites, sometimes absorbing broad swaths of the population.[2] [And yet, I still would advise historical so-

[2] I would never deny that formal organizations, for example can lay down rules that govern behavior. Such lines of authority can be formally represented as a network, but much of the usual substantive sense of what a network is and what

ciologists: take the time and look, at least a little bit, for the salience of network-like phenomena in your case!]

We should aim to be richly empirical, but methodologically ecumenical. My hope is not that we converge on some particular theoretical perspective, but we do converge on something more vague—yet also more intriguing—than that. Here I have in mind *aesthetic* issues: I would argue, perhaps somewhat provocatively, that we should try to produce arguments that are *not* simple, but *are* elegant, or intricate, or nuanced, or that in some other way convey our sense that the social world is complexly ordered, and that change happens in some complex but beautiful way. [And I'd love to see sociologists develop a new aesthetics of what makes for compelling sociological explanation.]

Now let's move beyond the work-as-exemplar notion to more substantive considerations. I mean here, the ambitious goal of theorizing historical change, and the challenging task of cogently describing *what happened when* to produce concrete outcomes different from those we saw in the past.

We should aim to be richly empirical, but methodologically ecumenical.

A starting point here is to assert my conviction about the sheer complexity of historical change. One thing is clear: the closer we get to ground-level, the more complex and fractal everything seems to be. 'Modern' spaces are pockmarked with archaisms; 'modern' institutions are adumbrated in 'pre-modern' societies. *Complexity* should be our guiding concept in all of this. Different scholars may take exception to the following definition, but let's just posit that complexity entails the emergence of new phenomena and new entities—frequently unexpected phenomena and entities emerging at vastly differential rates in different locales—out of the complicated, often intermittent, typically localized, interactions among preexisting component parts. At least, that's Part One of my definition.

So, one of my key theoretical goals in the book, perhaps the guiding idea as Collins mentions, is to explore how local-level (micro-level) interaction dynamics are linked to, and even produce, big (macro-level) outcomes. This goal generally requires both careful attention to the generative processes of local interaction, and some effort at figuring out how those carefully specified local interactions concatenate into these outcomes. These outcomes typically arise unbeknownst to participants, because they are busy attending to their own local and/or short-term goals.

For example, a local activity like writing a letter to get a job is reproduced, over and over, by many actors. The interaction dynamics among these actions, along with some exogenous factors, produce first mimetic imitation, followed by a little innovation which yields inflationary tendencies. The result is a kind of migration of practices—and along with them, the actors they constitute—over time into a newly demarcated zone of activity. But that new activity has feedback effects on actors' sense of what is going on, and it leaves traces in the form of texts that give practical advice or otherwise trace the trajectory that has been traversed and give it a name. One of the most interesting and important developments occurs when local changes acquire enough sticking power to become relevant in larger domains of human activity. Here I feel I must take issue with Hopcroft. She writes, concerning Romans, Florentines, and us, "The selves are the same, the circumstances (including appropriate cultural forms) aren't." Not true. This is because the "self" is not some universal vessel into which variable contents are dumped. It does not exist as some fundament. The development of a new self is an emergent phenomenon, producing a new actor upon the stage—similar to the way that a new perspective on nature emerged in the early seventeenth century, bringing with it a profound transformation in the relationship between humans and their environment.

On the basis of this viewpoint, I agree with Erikson's assertion that one of the big challenges of doing comparative-historical sociology is "to explain historical transitions (big, unique events) with repeatable social processes." But the conundrum—that we thereby "make the transitions seem less big and unique"—is somewhat alleviated

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'networking' is (a word I don't much like) is absent in such a setting.

when we adopt the view that outcomes are the products of complex *interactions*, sometimes involving otherwise innocuous elements, rather than the result of simple iterated processes or, worse still, the effect of the presence or absence of major 'entities' like 'a middle class' or 'marketization' or 'favorable fiscal policy.'

Finally, then, let's turn to the modernity question. I fear I am indeed guilty of holding on to the importance of Florence as my 'cherished hypothesis.' But let's try to be level-headed about the prejudices we no doubt acquire when we choose cases. The emergence of the self, the emergence of markets and tools of credit: are these once-and-for-all developments? Undoubtedly not. The Roman 'self' lay dormant for centuries until extensively imitated and recaptured in the Renaissance. Certainly there is some hiatus between the development of the self I observe in the Renaissance and a more widespread, grounded, unshakeable notion of the modern self that comes many decades later. And even if *elements* of the Italian city-state form and certain Renaissance perspectives on statecraft remained into future centuries, much of that form, and certainly that form in its instituted entirety, disappeared, as Hendrik Spruyt noted some time ago.[3]

Did, then, Florence constitute a decisive turn in the sporadic historical appearance of multiple capitalisms and multiple networked societies? I believe it did, though of course more always needs to be done to trace the genesis of future concepts, institutions, and practices back into any soil, including the Florentine. Institutions of banking, elements of artistic representation, notions of *realpolitik*, styles of interaction in urban settings: these are the kinds of elements that can be durable practical protocols for action—even as stock markets, state sovereignty, and agrarian capitalism and other elements we take to be integral to modernity did not yet appear. The very richness of the Florentine archival and textual record both typifies its historical importance, and has permitted some of the practices that grew there to migrate over time and space through future imitations and adoptions.

Returning to a more general theoretical level, then, an important question we should ask is: To what extent do the materials of an earlier time evaporate, and to what extent do they perdure as available scrap for innovative recombination? Context, as Hopcroft notes in a shorthand way, matters a ton for emergence. But sometimes when certain materials arise—practices, styles, institutions, texts—they congeal. They become transposable practices or ideas in the toolkit, as it were—available equipment for further transformations and lock-in. They acquire transposability *beyond* their context. History is not deterministic, but it only flows in one direction, and some events—which ones must be determined through historical and sociological analysis—develop a staying power to influence future development, rather like how the arrival of certain organisms in an ecology forever transforms that ecology, because those new forms become durable agents and react back upon the existing ecology. This is Part Two of the definition of complexity: emergent phenomena (can) stick around to influence future behavior.

It is curious how some of these materials are viewed as hopelessly outmoded from our contemporary perspective. Collectivizing agriculture is not like to happen again soon. Parsonsian functionalism looks pretty dead as a set of orienting principles. Sycophantic rhetoric of the exact sort found in Florentine patronage letters may not make a big-time comeback. As citation scholars tell us, some works—many works—will simply *never* get cited ever again. So the process of historical change, complex as it is, involves both the preservation of some past elements, and the merciless burying of others. And of course the significance of even lasting elements for inducing or inhibiting 'chemical' reactions varies greatly over time. But these are processes we should try to clarify.

In conclusion, I very much wonder along with Erikson if we should hold onto the master narrative of the emergence of modernity. We should *not* do so if it means succumbing to the ease of talking about that emergence teleologically—as if earlier cases were aberrant *because* they did not eventuate in what sooner or later would come to pass. Avoiding this pitfall is one of the benefits of thinking in terms of negative cases, as Emigh does. We also should not succumb to simple linear notions of the transition to modernity, nor even to one-time, big-bang-like theories of discontinuous but completely irreversible development.

[3] Of course history is not without its revivalist Machiavellian moments either.

That viewpoint is not consonant with the reality. I think I was given a job at Rutgers years ago largely because in response to a question about the relationship between the traditional and the modern, I extemporaneously drew trajectories for the two concepts on the blackboard as long, nearly parallel, faintly overlapping lines, and located Florence somewhere in the middle ten feet of the diagram! Change is sporadic, locally rooted, faltering, turbulent. Nevertheless, I am sufficiently a Weberian to believe that something—many things—about our world today distinguish it from time gone by: an accretion of practices, institutions, beliefs, and entities that have haltingly acquired a durable foothold and changed our world as a result. Accordingly, not all futures are possible. Some have been ruled out by events and locally spawned machinations of the past.

Response by Rebecca Jean Emigh

University of California, Los Angeles

I am very grateful to Richard Lachmann and Elisabeth Clemens for organizing this panel and to Rosemary Hopcroft, Emily Erikson, and Randall Collins for providing such thoughtful and insightful comments. I hope I can clarify some of my points to sharpen my arguments.

In comparative historical work, scholars are accustomed to using some variant of the Millian method of similarity and difference, even if only implicitly. So, they often look for cases where factors and outcomes are unambiguously absent or present to facilitate comparison. For example, they might look for cases where the preconditions for the transition to capitalism were unambiguously present and such a transition occurred, or the preconditions were absent and no such transition occurred. Or, they might look for a case where there was a single, highly important, missing precondition that explained the missing outcome. Such cases seem straightforward because the factors and outcomes are unambiguous. Nevertheless, these cases actually present a different—and sometimes unrecognized—problem. In the cases where the outcome occurs, it is easy to assume that all the factors contributed to the transition; in cases where the outcome did not occur, it is easy to assume that the factors contributed to the lack of such a transition. Of course, in theory, a perfect set of cases that allows the Millian joint methods of similarity and difference to be combined makes it possible to distinguish between necessary and sufficient factors, but in practice, such a set of cases is rarely, if ever, present for macrohistorical phenomena. Thus, much comparative historical work uses a "factors produce an outcome" strategy in a loose way.

Comparative and historical sociologists need additional methodological tools, such as negative case methodology (Emigh 1997c), to complement this usual strategy. In such cases, a theory gives the wrong prediction: the preconditions (or factors or causes) for some event were present, but the outcome did not occur. A careful empirical analysis of the negative case shows how the theory must be expanded to explain the case. Tuscany is such a negative case: the preconditions for capitalism (given by Marxist, Weberian, neoinstitutionalist, and sectoral theories) existed early in history, but there was no early transition to full-scale industrial capitalism. Or, stated somewhat differently: there were many capitalist institutions, but no overall capitalist system. Likewise, there was a precocious and strong city state of Florence and a regional or territorial state of Tuscany, but a modern nation state did not emerge there (it was, of course, Italy instead). But the case does not follow the usual Millian logic because the factors are all present and the outcome is not.

Such cases are extremely interesting because of the puzzles they pose, and the negative case method is particularly useful for analyzing these "near misses." But, these cases also create analytic difficulties, as Collins, Erikson, and Hopcroft's comments point out. In Tuscany, a mixed economy with precapitalist and capitalist elements prevailed for many centuries before a full-scale transition to industrial capitalism. Thus, the Tuscan glass can always be half full or half empty: social institutions may be viewed as precapitalist or capitalist depending on what is emphasized. Previous research tried to simplify this problem by using the usual analytic strategy: Tuscany had feudal elements so it was feudal; it had capitalist elements so it was capitalist. Or, Tuscany was missing some crucial capitalist precondition, so it remained feudal (see review in Emigh 2009:18, 203–205). In fact, both Hopcroft and Collins revert to this sort of analysis. Hopcroft points to precapitalist—or at least noncapitalist—elements to explain why there was no transition to capitalism. Yet, as I show, these elements either did not have the predicted effect or were also present in England where agrarian capitalism spread (e.g., Emigh 2009:97, 202). She claims that I argue that "all the conditions for capitalism in Tuscany along with emergent rural capitalism" "complete with rural capitalists" existed (i.e., the standard "capitalist preconditions lead to capitalism" argument.) Yet, Hopcroft misses my main point. Contrary to her claims, she and I agree that there was no emergent rural capitalism in Tuscany as there was in England, with rural land owners who were singularly forced to innovate (Emigh 2009:205–209). In an attempt to use the usual strategy, where a set of factors leads to a given outcome (here, in particular, that noncapitalist elements prevent the development of capitalism), she conflates the existence of preconditions or capitalists with full scale capitalism, (Also, Emigh [2009:21] discusses the particular neoinstitutionalist precondition of states' enforcement of property rights in relation to market restrictions that in fact did not have the predicted effect in Tuscany.)

Collins gets caught in the same sort of trap by suggesting that "there is a terminological problem throughout Emigh's analysis" because of my supposed argument that "Tuscany had capitalism but failed to undergo the transition to capitalism" and that this "self-contradictory formula is repeated many times in slightly different words." Like Hopcroft, however, Collins conflates the preconditions for capitalism with a capitalist system in a similar attempt to force the case into the standard "factors produce an outcome" framework. And, tellingly, despite this supposed confusion, Collins easily summarizes my main argument correctly: "Tuscany had all the preconditions but the capitalist transition did not come about." Thus, it is not so much the unclarity of my argument, but an attempt to fit the Tuscan case back in a straightforward "capitalist preconditions lead to capitalism" box, that drives Collins's argument.

Such an analysis leaves us with a theory that capitalism is self perpetuating, despite Collins's argument to the contrary. Collins proposes a Schumpeterian definition of capitalism whereby it can be identified by "constant innovation" that is "dynamically self-transforming." Such a definition equates capitalism with progress, making it impossible to understand how capitalism can be self-destructive. Or, in reference to Erikson's comment, we should not treat economic development as synonymous with capitalism. Collins claims that Schumpeter takes account of capitalism's self-destructive tendencies by arguing that "organizational changes and government action" can eventually substitute for "the entrepreneurial mechanism." This may be true, but then of course capitalism cannot be defined by constant innovation once it is not entrepreneurial. Thus, because a Schumpeterian definition assumes the efficiency of capitalism, Collins's solution would prevent me from considering how capitalism undevelops, which he considers one of the strong points of my work.

Finally, he deploys the usual analytic logic: he proposes looking for missing Schumpeterian preconditions that explain the missing Schumpeterian outcome. I think that Collins's idea to develop a Schumpeterian theory of transitions to capitalism is quite fruitful, but I suspect that Tuscany will be a negative case for such a theory as well, as contrary to his suggestions, most empirical evidence shows that fifteenth-century Tuscany was more developed fiscally than England at that time (e.g., in Tuscany, there was more direct taxation, more advanced corporate structures, earlier establishment of a tradable public debt, etc., than in England).

I am not arguing that the Millian method, in either its strict or loose form, should be abandoned, I am simply suggesting that we need to find creative analytic space for negative cases and analyze them on their own terms, without trying to force them back into a "factors produce outcomes" analytic strategy. If we make use of negative cases, we come to very powerful conclusions: in Tuscany, for example, that capitalist elements can undevelop capitalism (which cannot be that surprising given the state of the economy in 2009).

I would also like to comment on some of the specific criticisms. What about the specific components of Hopcroft's more general "precapitalist elements lead to precapitalism" argument? First, did high taxes in Tuscany delay the transition to capitalism? Of course, Hopcroft is correct that some neoinstitutionalists argue that if states confiscate most of producers' surplus through taxation, they will have few incentives to invest or innovate. However, despite Hopcroft's assertion that I am making "straw men" out of economic theories, I have done just the opposite. I have carefully assessed what the concrete empirical effects of the proposed neoinstitutionalist mechanisms would have been. First, taxation would have had to have had differential effects across commerce and agriculture, if Florentines were successful merchants, but ineffective agronomists. But there were no overall differential taxation rates for commercial and agricultural assets. Both types of assets were taxed at the same rate (though there are some small, but probably offsetting differences in what was taxed).[4] Second, what was the effect of differential urban and rural taxation—did high rural taxation prevent rural inhabitants from realizing profit? Again, this seems not to have been true. Rural inhabitants were given large deductions when they worked their own land, and they paid no taxes on income from sharecropping. In contrast, Florentine landlords did not receive this deduction, and they paid taxes on agricultural income. Third, individuals responded to tax incentives, but only when they did not conflict with their other economic incentives, such as profit or establishing property rights (Emigh 2002; 2008). When they did conflict, these other incentives always trumped tax incentives. There is no doubt that taxes were high and rich and poor alike whined about them. Assessment had become more standardized during this period (though individuals still tried to lower their assessments); payments, however, were highly variable. So, it made sense to complain bitterly at the point of payment. Finally, this time period was one of tax rationalization—direct taxes are always more efficient than indirect taxation; that Tuscany had direct taxation earlier than England, yet capitalism developed in England and not in Tuscany, is simply more empirical evidence that contradicts neoinstitutionalist economic theory, instead of supporting it as Hopcroft suggests.

Second, what about the argument about market restrictions? It is a standard neoinstitutionalist argument that market restrictions reduce individuals' incentives to invest and innovate because such restrictions make it difficult or impossible to realize a profitable return. And, of course, there were such restrictions in Tuscany, such limitations on grain movements, rationing, price fixing, etc., that could have reduced individuals' incentives to invest in agriculture. And, many scholars pointed to such restrictions as feudal and thus tried to explain the Tuscan outcome. Instead of doing this, though, I looked in more detail to see what the actual effect of such restrictions was. In fact, such restrictions did not have the predicted effect. First, during the fifteenth century, restrictions were much lighter than in the past (Emigh 2009:24–25), yet investment was not dramatically altered. Second, I show that such restrictions did not prevent investment in agriculture, which in turn increased productivity (Emigh 2009:175, 209). Third, what would have happened without such restrictions? Even if the neoinstitutionalist economic argument is true and fewer restrictions would have produced more investment in agriculture, this would have produced exactly the same outcome, only more rapidly: the spread of sharecropping that undermined the growth of rural markets thereby delaying the transition to capitalism. Thus, these restrictions did not have the specific effects predicted by neoinstitutionalists.

Hopcroft's third point about noncapitalist elements is, of course, also correct. There were traditional uses of rural estates to escape from the city, to demonstrate social status, or to provide fresh produce. However, such uses—which also existed in England—were secondary to the main function of rural estates. They formed specific roles in overall capitalist business portfolios: to provide a secure return for merchants and to provide a way to transmit commercial profits across generations (e.g., Emigh 2009:94–97, 121, 202). It was these unambiguous capitalist economic interests that drove decisions to invest, not comparatively minor issues about relative taxation, restrictions, or fresh eggs.

Erikson points to two other economic arguments that I could have missed. First, she notes that sharecroppers had more income than smallholders, so they could have invested in other valuables, such as dowries, and purchased luxury goods, thus creating a domestic market. From an economic point of view, income is income, so rationally, rural inhabitants should have consumed it

[4] There may have been slight differences in how the assets were taxed: Florentines tended to be taxed on capitalized income from land, while they tended to be taxed on value of commercial assets. Thus, it is possible that it was easier to hide profit in commercial assets. On the other hand, Florentines were given deductions for agricultural assets and not taxed on loans to share tenants, which may have provided compensatory tax relief.

or invested it.[5] However, my argument is that the social constitution of markets is crucial to what rural inhabitants can do with income. As my analysis of smallholding shows, participation in land markets was key to participation in labor and commodity markets. Most transactions were made on account (cash circulated, but it was in relatively short supply) so permanent residence and landownership were important preconditions for many transactions. Perhaps the most suggestive evidence comes from the difference in the composition of dowries in smallholding and sharecropping regions. In the smallholding regions, dowries included commodities (such animals, clothes, and household implements), land, and cash. In the sharecropping region, however, dowries were always composed of cash (Emigh 2009:74–142–145). Smallholders' property devolution practices entailed commodities because their land ownership allowed them to participate in markets.

Should we engage in debates about transitions to modernity? Yes, because the terms, modern and post-modern, all imply a pre-modern referent. Yet the pre-modern period is the one about which sociologists have the least empirical knowledge.

Second, Erikson asks about theories of trade. Am I implying that trade is unimportant and that only the domestic situation is important for transitions to capitalism? Not really; instead I am offering a corrective to a literature that has often focused on the role of trade in Tuscany: Florentine merchants were of course primarily engaged in trade in luxury goods. So, Erikson is right; it was extremely important that these economic activities began to wane just before the Europeans discovered the so-called new world. Thus, in this book, I am providing detailed information on the domestic situation to show how the trade economy and rural economy were linked. In particular, the lack of a robust domestic market was one reason that the Tuscan economy was so dependent on foreign trade, which in turn made it highly susceptible to external developments like European expansion (Emigh 2009:19).

More broadly, Erikson and Hopcroft are right: many of these economic theories that I reject seem to be generally consistent with the Tuscan case. However, careful inspection shows that when pressed on specifics, such theories cannot explain the outcome of delayed transition to capitalism. Erikson states my alternative theory perfectly: taking a Weberian understanding of capitalism, my work shows how rational pursuit of profit-making delays a transition to full-scale industrial capitalism. Or, we could interpret my findings in a Marxist way: capitalist social relations of wage labor and private property do not create full-scale industrial capitalism. Thus, by using this paradoxical case, I interpret Tuscan history in a novel way—capitalist preconditions relations do not always produce full-scale industrial capitalism. And, by doing so, I argue that I did not make a straw person out of economic theory; in fact, on the contrary, I took these theories very seriously to understand my case. I agree with Hopcroft; this book is not an argument against all of economic theory as I use a lot of it.

Nevertheless, I deploy economic theory in three sociological ways to make some specific criticisms of it. First, most economists—and sociologists, for that matter—treat the relationship between culture and economy in terms of two ordered independent and dependent variables. Indeed, I agree with Hopcroft that when culture and politics are considered by economists, they are part of the institutional context that shapes economic

[5] Erikson wisely points to one of my biggest remaining empirical puzzles—did sharecroppers have disposable income, and if yes, what did they do with it? Of course it is always difficult to know whether they had any disposable income; many were indebted to their landlords, so it is possible that they had very little extra money. As best as I can tell, agricultural surplus was not channeled to population growth: age of marriage was not dramatically lower and fertility not substantially higher in sharecropping regions than in smallholding regions (Emigh 1997a, 1997b).

change—or in my language, they use "culture from the outside" arguments that culture affects the economy. Sociologists also consider "culture from the inside": the way that economic systems affect cultural ones (Emigh 2009:34–36). Yet, neither of these standard views explains the Tuscan case (Emigh 2009:214–216). Instead, I am theorizing an interactive view of culture and economy that shows how they are constituted dialectically as social domains. Thus, I am making a much more complicated criticism of economic theory than the usual complaint that economists ignore culture, which Hopcroft claims I am making.

Second, when I analyze transaction costs (Emigh 2009:110–117), I make extensive use of economic theory, as Hopcroft notes, but I am using it quite differently than economists would. I am not assuming utility maximizing rationality on the part of landlords and tenants; instead I am using the theory to understand Weberian substantive economic interests, the form of rationality, and the nature of sharecropping (whether it was capitalist or not). An economist would never deploy the theory to make these points. Neoinstitutionalist accounts of sharecropping assume that is it a rational strategy (in the utility maximization sense). And, more generally, in comparing the differences in the form and meaning of "rational" action between sharecroppers and smallholders, I am going beyond formal rational choice models that assume a similar utility maximization rationality for all rural inhabitants.

Third, even more generally, by looking at this mixed precapitalist and capitalist case, I am addressing Erikson's conundrum exactly. That is, we know that there are many times and places where there are some capitalist elements, but no capitalism. And, there are times when there is a capitalist system per se. So, how do we analyze them? As Hopcroft notes, Jones and other economic historians tend to dismiss these "close calls" or period of extensive economic growth without a transition to capitalism as having happened throughout history. But these "close calls" are interesting because they force us as researchers to analyze the role of each factor very carefully. We cannot simply assume that they have the predicted effects as we can with straightforward cases.

Thus, I am arguing that simply by pointing to the predominance of capitalist elements or feudal elements, we indeed do not know whether there will be a capitalist or feudal system. We need to look more carefully at how these elements actually work in social context. In Tuscany, there were capitalist elements that delayed a transition to a capitalist system. I am not ruling out that elsewhere capitalist elements might lead to capitalism or even that feudal elements might lead to capitalism. I am just arguing that the preconditions alone are not determinative.

This leads to Erikson's big question: should we engage in debates about transitions to modernity? Yes, because the terms, modern and postmodern, all imply a pre-modern referent (Emigh 2005). Yet, the pre-modern period is the one about which sociologists have the least empirical knowledge. After all, only really crazy sociologists bother to learn virtually extinct languages and old forms of handwriting, while putting up with endless comments by non-historical sociologists about the "usefulness" of studying an "old" topic that "no one" is interested in (not that I am bitter). But unless we do actual empirical research about the premodern period, we run a huge risk of stereotyping it by simply assuming that it is the opposite of modernity, which in the end, of course, makes it impossible to understand how modernity might be different from premodernity, in turn making it impossible to understand ourselves at all.

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Book Symposium

Author Meets Critics

Apocalypse: From Antiquity to the Empire of Modernity. By John R. Hall (Polity Press, 2009).

This excellent and ambitious new book was the subject of an Author Meets Critics panel at the 2009 SSHA annual meeting in Long Beach, CA. What follows are the comments of the three critics (Ari Adut, Ivan Ermakoff, and Robin Wagner-Pacifici), and by a response by John R. Hall.

Comments by Ari Adut

University of Texas at Austin

In *Apocalypse: From Antiquity to the Empire of Modernity*, John R. Hall presents us with a masterful genealogy of the apocalyptic imagination from its sources in Mesopotamia to our day. His provocative analysis reveals that the apocalyptic trope, despite its religious provenance, has proven resilient in modern times. Moreover, this is a book which stresses the performativity of the apocalyptic. Hall shows that the visions of the end of historical time have been very consequential in religious and political life. Finally, the book deserves much credit for laying bare the complex articulations and tensions between the apocalyptic and other kinds of temporalities—especially in modern times. In my comments, I will first go over the main arguments of this incredibly rich book and then offer some reflections, comments, and criticisms.

According to Hall, the apocalyptic first emerges with monotheism – more specifically with Zoroastrianism. He argues that primordial and archaic societies lacked any strong sense of either history or apocalypse. And it is one of the most fascinating insights of this book that in their origins historical consciousness and apocalyptic imaginary go together. There is a certain elective affinity between the two. Archaic societies are characterized by what Hall calls "synchronic time"—the temporality of here-and-now. Following Eliade, Hall argues that all meaningful action in early societies is ritualistic and that collective archetypes organize rituals especially through mythic narratives of eternal return. So synchronic time is allied with mythic circularity. In this logic, the linearity that we commonly attribute to time disappears through the endless imitation of archetypes and through the repetition of paradigmatic gestures. According to Eliade and Hall, for the ancients, the concept of history as an extended chronological sequence of events does not exist. There are of course events, but they are mythified and removed from their temporal sequences.

Hall claims that the apocalyptic emerges with the emergence of historical consciousness. The apocalypse involves the end of time, so it necessarily entails some understanding of time as a linear process. So there is a certain historicity. But what we have here is not exactly the modern, objective, linear time. This is because unlike modern time, apocalyptic time is teleological. It posits an end that is preordained. Moreover, this end provides meaning to the process that led to it.

So there is a close connection between the apocalyptic and monotheism. The former is in many ways the product of the latter in Hall's account. Hall argues that with monotheism "the temporal structures of God's purposes become historical and in some cases apocalyptic." I would say that the ideas of the world having an end and the world being created by one creator actually reinforce and justify each other. It is by ending the world that the monotheistic God reveals and proves his powers both to the doubtful and to rival deities, and the world can come to an end only if there is one God. As Hall's analysis reveals, historical temporality first makes its appearance in human consciousness in apocalyptic form. And here the apocalypse is the endpoint of the extended unfolding of the shared telos of a people, either defined ethnically as in Jews or ideologically as in the case of Christians. Apocalypse is an intense moment of crisis, when hopes for redemption generate all kinds of action. It is followed by a timeless eternity. Different theologies posit as to what is to be done by the faithful when the time comes, if one can hasten the process, as to what happens specifically after time ends, etc.

As Hall shows, apocalyptic narratives very much molded the lived experience of Jews, Christians, and Muslims. These narratives were also strategically shaped. Elites mobilized divine narratives of final judgment, second coming, messianic intervention, etc. for strategic purposes. The narratives regarding the end of the time by early Christians were for instance strategically modified in view of specific political and recruitment objectives. Similarly, Islamic expansion was very much legitimated by the apocalyptic visions of the eventual triumph of Allah on earth and the redemption of the faithful in paradise. Islamic expansionism was to hasten God's triumph and Muslims' redemption in the eventual apocalypse.

The apocalyptic reigned in medieval times and in early Modernity in Europe. A good deal of entrepreneurial action by political and religious authorities as well as by their contesters was legitimated this way. Both the religious establishment and dissent defined their missions and enemies in apocalyptic terms. For instance, apocalyptic notions energized the crusades and the Inquisition. In turn, heretics such as the Cathars saw the pope as the Anti-Christ. Compared with other heresies, the apocalyptic ones proved more effective. In effect, the figure of the apocalypse was an important ingredient of the Reformation. It was Luther who said, "The spirit or soul of Anti-Christ is the pope, his flesh or body the Turk." Dissenting Protestants justified violent action on each other with apocalyptic terms. Apocalypse makes a central appearance in the English revolution as well. Puritan insurgents saw themselves as fulfilling a messianic mandate. This made their violence a justifiable part of a holy war. In effect, Hall argues that all modern revolutions have apocalyptic elements in terms of their temporal attitudes, their utopianism, their religious fervor, the absolute confrontation that they stipulate between good and evil, and their violent means.

Hall shows that the apocalyptic imaginary remained a powerful component of the dominant and counter-culture of modern capi-

The Reformation severed divine authority from its temporal counterpart, pacified sectarian warfare, and nationalized religion. But the apocalyptic did not go away. Hall argues that it was simply channeled into the secular realm. The apocalyptic had, comparatively speaking, not been a central element of the conflict between the monarchy and its Protestant competitors in France. The revolutionary apocalyptic would take a secular form in the Hexagon. Hall argues that the ideological fundamentalism that we see during the Reign of Terror and the use of violence to establish a republic of virtue cannot be understood unless we pay attention to the apocalyptic dimension of the French Revolution.

In Hall's account, political modernity was ushered in significant part by the apocalyptic. But once political modernity was established with the great revolutions and with the modern state, the apocalyptic had to be restrained by the forces it had itself brought into being. Moreover, modernity meant the rise of divergent types of temporalities, particularly the rise of objective, linear time—time as measured duration, that is, time as something that can be rationalized, counted, commodified. This new temporal mode, the temporality of capitalism and of the modern rational administration, is of course at odds with the apocalyptic.

Yet the containment of the apocalyptic was only partially successful. Hall points out that the apocalyptic is a "hydra…regenerating with each effort to contain it." The apocalyptic marked all the modern revolutions. It bestowed meaning and energy to imperialism. It seeped into various modern philosophies of history. The teleologies that we find in the Hegelian and Kantian visions of history have traces of the apocalyptic. The Marxian theory of history, which stipulated the end of all class society as well as the withering of the state in the wake of a violent rupture, has more than traces of the apocalyptic imagination. In effect, most Enlightenment models of progress tend to envision a final state where there is no more progress and where there is some kind of a final reconciliation between conflictive elements.

Hall shows that the apocalyptic imaginary remained a powerful component of the dominant and counter-culture of modern capitalism. He finds strong apocalyptic tropes in America's religious civil society through history. He finds similar elements in the ideology of the political left as well. He points to the apocalyptic forms that revolts against modernity took by considering revolutionary communism, anti-colonial messianic movements, and guerilla warfare. He skillfully fleshes out the apocalyptic aspects in contemporary Islamic terrorism. Apocalyptic warfare sacralizes terror, gives meaning to its violence, and increases the movement's recruiting potential. At the same time, Hall argues that the attitude of the American government with its Manichean and alarmist discourses in the war on Terror has also taken on apocalyptic trappings after 9/11.

This is a very ambitious book. Mixing dazzling erudition and conceptual subtlety, it covers a lot of ground--empirically, historically and theoretically. It can hardly be faulted for what it leaves out, especially given the ubiquity of the apocalyptic in history. But a surprising omission are the various modern secular apocalyptic visions that involve some inevitable technological catastrophe or decline, those which end the world as we know it with a cataclysmic event. Consider for instance global warming narratives. It seems that in a perverse way science feeds the apocalyptic. The more we know about the world, the more its end (or rather the risk or the possibility of its end) becomes conceivable (both factually and normatively) in our rationalist imaginary. Hall tends to look for the apocalyptic in religious or political ideologies, but in the contemporary era many apocalyptic scenarios are produced, in an evitable fashion, by the rationalism of risk society. And ironically the technocrats of risk society and their critics almost collude with each other in generating these narratives.

Another issue has to do with the dual nature of the apocalyptic as narrative and as temporality. Hall rightfully points out that the apocalyptic is not merely a narrative, not merely a discourse. It is also a certain temporality. I agree with him, and I was wondering if he could say a bit more about the relationship between these two aspects—between the narrativity and the temporality of the apocalyptic. (The work of Paul Ricoeur may be relevant here.) It seems that there is a certain dialectic. The apocalyptic narrative restructures historical time through the generation of political disruptions and crises. At the same time, crises often call for apocalyptic narratives that would give some kind of meaning to the otherwise contingent, absurd, and chaotic threats that crises invariably impose on us. Somewhat related to the duality of time and narrative, we see that the apocalyptic (the religious and political apocalyptic, that is) is riven by tensions—even antimonies. On the one hand, it involves a certain inevitability, stemming either from the logic of history or from divine will. At the same time, the apocalyptic is a call for action—often violent action—by those to whom the narrative is directed at. It seems to me it is the tensions between inevitability and action, between linearity and finitude, between meaning and anarchy that make the apocalyptic so resilient, powerful, and deployable for political purposes. I was wondering if John Hall could say a little more about these tensions or dualities.

Yet another matter that I would like John to address has to do with the strategic use of the apocalyptic—especially by secular actors. Now, for the revolutionaries, the apocalyptic takes its power in part from its melding of destiny and action. It places the faithful on the side of God or of history. This can motivate or justify all kinds of risky, radical, brutal action. But motivation and justification are not the same things. In the case of revolutionary discourses like communism and anti-colonialism, the apocalyptic can motivate violence by presenting it as necessary, inevitable, emancipatory. But this is not always the case. The apocalyptic does not always have a motivating function in all secular revolutionary ideologies. It may be merely or mostly justifying. For example, in the French Revolution, the discourse of the revolutionaries grew apocalyptic only during the Terror, and mostly during its second half. We don't see many apocalyptic narratives until then. The revolution did not get violent because of the apocalyptic discourse. Rather, the apocalyptic discourse emerged after the revolution got violent in a situation of generalized fear and distrust, where things got out of hand and revolutionaries got increasingly desperate and started going after each other. So discourse justified more than it motivated. And the rhetorical excesses of the apocalyptic imaginary did not convince everybody. Not everybody was taken in, for instances, by Robespierre's millenarian rhetoric. Many other revolutionaries—his rivals—thought that his discourse was mainly a cover for his personal political ambitions. Robespierre thought along the same lines for others. I realize that differentiating motivation from justification is not easy, theoretically or empirically, but I was wondering if Hall could give it a try in the case of apocalyptic discourse.

A final issue concerns violence. The apocalyptic predicts, urges, and justifies violence. Toward the end of the book, Hall addresses the moral problem that the sacred violence of revolutionary apocalyptic projects – both secular and religious – poses. And here, Hall extensively quotes from Maurice Merleau-Ponty's *Humanism and Terror*. In this book, Merleau-Ponty differentiated between two kinds of historical periods. The first type refers to routine politics. Here, politics is largely violence-free; established laws are enforced. In the second type of historical period, we have revolutionary situations where the old order crumbles and needs to be remade. Here, as one's liberty is often perilous to the liberty of the others and to the common good, violence is inevitable. But not all violence is morally justified. Violence comes in two forms: progressive or self-serving. And the only way we can differentiate between the two and justify progressive violence is by considering the outcome of violent action. Using Merleau-Ponty's conceptual framework, Hall says, "History, its agents and witnesses, and their failures and successes will answer questions about the meaning of 9/11, Islamic Jihad, and the Empire of Modernity's policing of terror… " Then he asks, "Will violence turn out to be have been progressive in its consequences for the advancement of…. Reason in history or merely 'self-serving'?" He adds, "Only by limiting violence to the pursuit of a transcendent Reason, Merleau-Ponty argued, can we avoid the grim dystopian world of adventurism and a 'senseless tumult,' or what might amount to 'the new dark ages.'"

Now, this is certainly not a central matter, but reading the book I was not completely sure about Hall's position toward Merleau-Ponty's moral philosophy. And I would be very interested in hearing more on this and on the moral issue of political violence in general. Now *Humanism and Terror* was written when Merleau-Ponty was a Marxist. And even though Merleau-Ponty was not ignorant of the horrors of Soviet totalitarianism, he was in this book attempting to justify Stalinist terror by pointing out its pursuit of a transcendent purpose--classless society. Revolutionary violence thus becomes apocalyptic, and hence legitimate and inevitable. I am a great fan of the Merleau-Ponty of the *Phenomenology of Perception*, but I find this Merleau-Ponty chilling. Hall himself points out to the dangers of this sort of consequentialism. And it is not impossible that a world where we are in the pursuit of a transcendent Reason will be as violent, or even more violent, than a world where we are merely pursuing our narrow interests. But in any case, the French philosopher dropped his apocalyptic bent a couple of years later. The Korean War revealed, for Merleau-Ponty, that the USSR was an aggressive imperialist power. In *The Adventures of the Dialectic*, written in 1952, Merleau-Ponty defended parliamentary democracy, a nonapocalyptic temporality and a liberal morality—these three things somehow having elective affinities with each other. So again, I was wondering if Hall could talk a bit more about the moral status of the apocalyptic violence and elaborate on what he thinks of Merleau-Ponty's moral and political philosophy as expressed in *Humanism and Terror*.

But these are minor matters. John Hall's *Apocalypse* is a *tour de force*. This wonderful book shows that the history of the apocalyptic is closely intertwined with that of the historical and political consciousness. And this history, which does not seem to be over, reveals much about modernity, especially its temporal complexities. Hall bridges sociology, history, and religious studies with admirable ease. This excellent work is a major contribution to the study of political and religious action as well as to the sociology of temporality.

Comments by Ivan Ermakoff

University of Wisconsin – Madison

1. A key contribution of *Apocalypse* is to place the phenomenology of time at the center of the inquiry. The apocalyptic imaginary implies a peculiar orientation to time—one that could be described in the terms of a negative dialectics. Sanctioning the birth of historical consciousness, the apocalyptic breaks with "the cycling back of history into myth" (87) and with the here-and-now of primordial and archetypal time. Simultaneously apocalyptic expectations seek to free themselves from the time of history. There will be a huge and radical clean-up at some point. After the clean-up, people will simply live their life and this will be the end of history as we have known it full of chaos, misery, and injustice. Hence, apocalyptic eschatologies are both within history (since they mark the emergence of historical consciousness) and outside of it (since they imply its negation). This book sets the terms of this peculiar equation in a lucid and very enlightening way.

A second— equally significant — contribution is to trace the different variants of this equation across history. *Apocalypse* describes the initial formulations of what could be called the apocalyptic complex in the context of the rise of monotheism; it charts out the rearrangement of the relationship between religion and state as they staged their crusades against various Others and multiple heretics; it highlights the secular rechanneling of this imaginary in the wake of the reformation struggles; and the profusion of forms taken by the apocalypse in the modern era. John Hall elegantly weaves together a rich description of these historical developments with nuanced theoretical insights.

My comments directly relate to these two contributions and hinge on two sets of questions. First, can we identify the genetic formula of the apocalypse as a system of representations? In particular what do moments of foundation tell us about the factors leading to the emergence of such systems? Second, is there a unique template underlying the multiple forms observed in history? Do these variants reflect the same generic blueprint?

2. Hall identifies four moments in the genesis of apocalyptic representations: Zoroastrian religious themes, old Judaism, Christianity and Islam. These four moments are foundational. They set the map of subsequent forms. From these four moments can we infer a typical pattern of emergence?

At first sight, it seems that apocalyptic expectations are the child of monotheism (40). The argument could be cast as follows: monotheism asserts a unique principle of transcendence. Because it is unique, it is also exclusive and absolute. One does not want to toy with it. Multiple gods, by contrast, have the unfortunate tendency to blur the picture. They are likely to vie with one another or to engage in turf wars about their respective domains of competence. All this is not very serious as the ancient Greeks knew all too well. A unique and exclusive God is in much greater position to concentrate all the wrath and power imputed to the divine.

If we follow this line of argument the clue to the genesis of the apocalyptic lies in the symbolic logic of a belief system organized around the core idea of an uncompromising and all-powerful transcendence. Yet, as we read *Apocalypse*, it soon appears that this cannot be the whole story and that the conditions presiding over the emergence of the apocalyptic are quite diverse and complex.

At one end stand the zealots who embody resentment in the context of political and religious subjugation (34). The evocation of the apocalypse is a way to reassert the prospect of a future and definitive victory. This prospect motivates the launching of a full-fledge offensive. It also breaks with the experience of subjugation. In this pattern, apocalyptic narratives develop in contexts of setbacks (20). It is in the face of crucial and humiliating defeats that historical actors develop their sense of history (24, 34, 41). The apocalypse is a political project driven by resentment (34) ─ a resentment all the more powerful that those suffering subjugation and persecution are God’s chosen people.

At the other end stands Islam. Here resentment does not appear to be the driving force. Rather, the prospect of a final battle takes shape in the context of political expansion. The apocalyptic innovation of Islam proper is to erect warfare to the status of divine principle. “Jihad, as apocalyptic war, became a triumphant manifestation of God’s destiny” (40). The apocalypse serves a political project marked by expansion.

In either case, violent political conflicts figure prominently in the genesis of apocalyptic eschatology. These political conflicts yielded the "most intense experiences of historicized time" (41). From this perspective, the clue to the emergence of apocalyptic representations lies in the dynamics of violent group struggles. We cannot understand this emergence process without considering how groups relate to power and assert their power claims. The political dimension is key. “The apocalyptic can empower those who charismatically invoke it” (42).

Thus, *Apocalypse* offers two possible types of genetic arguments. One traces the logic of emergence to the symbolic logic of a belief system. The other portrays this emergence as grounded in violent political conflicts. Furthermore, the political explanation draws attention to a broad variety of contextual scenarios characterized by very different logics of conflict: the resentment of subjugation (e.g. the zealots), the perception of external threats (e.g. the launching of the Crusades) or political expansion (e.g. Islam). Obviously, the call for apocalyptic war is not the exclusive province of those who are deprived of power and autonomy.

Given this multiplicity of explanatory leads, shall we conclude that there can be no theory of the factors leading to the emergence of apocalyptic eschatologies?

3. A powerful theme of *Apocalypse* is the claim that the apocalyptic has “no single mold” (144). The broad variety of historical forms surveyed in this book testifies to this point. After the Reformation, the apocalypse became secular. In the modern era, it is also part of the counterculture (147-152). These forms are now so diverse and varied that Hall relies on the image of the “hydra” (118). Hence, there is not one brand of apocalyptic representations, but several. They differ with regard to their level of abstraction, the role imputed to violence and war, and the representation of the future. "The apocalyptic is open to diverse resolutions" (40). It can take the form of quietist anticipation or holy war (24). *Apocalypse* carefully documents this polymorphic character.

If indeed the resolutions are multiple, can we still discern a common thread between these different variants? The most referential exemplars whether the focus is on statements (e.g., the Book of Revelation, Joachim the Fiore, Thomas Müntzer) or movements (e.g., the Zealots, Islam, the Crusades, the Anabaptists, the Puritans) sketch a set of constitutive elements: the invocation of an all-powerful, unique and exclusive transcendence, the prospect of a radical break, an ideology of violence as purifying and the representation of an end of history. As we shift attention to more modern variants, however, some of these elements no longer appear to be constitutive or present.

For instance: environmental predictions of an ecological disaster do point to the end of human history (225). In this respect, they generate their own brand of apocalyptic expectations. Yet, these expectations may be far away from any reference to a transcendence. Along very different lines, radical groups often theorize and justify their violence in the name of a purifying project. Yet, this project needs not be grounded in the eschatology of the end of history (e.g. the RAF in Germany 157, 159). In these conditions, what do we gain in describing the practice and the visions of these groups as “apocalyptic”? What justifies the use of the concept as an all-encompassing descriptive category?

Comments by Robin Wagner-Pacifici

Swarthmore College

In synchrony with the original Greek meaning of “Apocalypse”—that that being to uncover or disclose—John Hall’s book, *Apocalypse: From Antiquity to the Empire of Modernity*, performs its act of historical disclosure through the lens of apocalypse. Indeed, the book reconfigures the history of the West as driven, in large measure, by apocalyptic demiurges. It thus accomplishes a major feat of historical re-vision. While acknowledging that apocalyptically inflected religion was not the only vehicle through which people developed an historical consciousness (other vehicles including trade, technology, and science), John wants to push the apocalyptic trope into the foreground. The advance of this approach is to present history in a dramatically new key – and it’s a compelling one. Part of what makes it so compelling is that thinking with apocalypse opens up associated historical themes to deeper understandings. These include themes of the relationship of modernity to apocalyptic thinking, the theme of time and temporality, and the theme of the conceptualization and uptake of historical events.

Partly because these themes are ones that deeply interest me and partly because of my inability to expertly comment on the more specific historical analyses of apocalyptic motifs in Judaism, Christianity and Islam in the book, I want to build my comments around the way these three themes appear in book.

John writes that he aims to “provide a configurational history that identifies key historical shifts of modernity’s development, aligned along the axis of the apocalyptic” (203) The road to this development of modernity is one in which the apocalyptic has escaped the exclusively religious domain, having migrated into realms of apparently secular statecraft, revolution, and imperialism. Thus both al Qaida and the West’s “War on Terror” are the modern heirs of an apocalyptic orientation toward history and war. John terms the “War on Terror” complex as one deriving from what he calls the Empire of Modernity—“that historically emergent generalized global complex of governing projects and strategic power initiatives centered in the West, and militarily, in the U.S.” (4) But simply *granting* that an apocalyptic attitude can escape its original religious home and domain, is not tantamount to explaining *how* it does so and in what way its extra-mural manifestations connect with its original ethos. The *how* is a good deal more ambiguous, and John highlights that ambiguity. For example, at times John argues that Western imperialist conquests beyond Europe drew *ideological appeal* from early religious apocalyptic crusades. At other points, the apocalyptic is said to be *realigned* in relation to modernizing states. At other points it is suggested that the early religious apocalyptic movements might be *genealogically related* to later revolutions (notably the French Revolution), or that they might merely provide templates for *functionally equivalent reinventions*. There's a genuine dilemma here—did later, apparently secular, apocalyptic movements draw on earlier apocalyptic ideas and energies as formal models and templates or did they absorb some of the religious animus of its origins? Realignment, relocation, reworking, reinvention and rearticulation (all terms employed in the book) are not exactly synonymous. John is forthright in recognizing these variations on a theme and in raising the questions associated with alternating claims of genealogy and equivalence. While discoveries of apocalyptic themes outside of religion proper, and outside of what is commonly understood as an apocalyptic epoch, does force us to view *modernity* very differently, such questions remain. Part of John's strategy for answering these questions is, I think, to take an indirect path. Rather than attempting to answer these questions directly, John productively approaches them through an examination of the phenomenology of modern temporalities.

Early in the book, and drawing from his own prior incisive theoretical work on multiple temporalities ("The Time of History and the History of Times," *History and Theory*, 1980), John identifies distinct modalities of human orientation to time: the here and now; collective synchronic time; diachronic time; strategic time; post-apocalyptic time; and transcendence ("bracketed time available as infinite"). In different historical epochs, people navigate through life by moving into and out of various available temporal modalities. The mere invention of a particular temporal orientation does not guarantee its social uptake—for example John notes that, "the capacity to measure diachronic time does not assure its use in the organization and coordination of social life." (82) Nevertheless, one of his findings regards the role of apocalyptic thinking in the introduction of a linear historical temporality into a cyclical, mythical temporal order. John writes: "Th[e] cycling back of history into myth began to break apart when Zoroastrian dualists and Jewish, Christian and Muslim monotheists separated the beginning of history from its end, placing the unfolding earthly struggles of God's people in the middle, in historical times." (87) John writes that *apocalyptic crises* are essentially crises in time and of temporality. For a diachronic historical temporality, apocalyptic moments are, "its most intense moments of crisis, when hopes for redemption spawn a variety of courses of action." (41) These crises fatally yoke together the strategic time of military action and the diachronic time of history to open up a door to eternity, or (in modernity) to some post-apocalyptic functional analog. The great accomplishment of this complex and theoretically sophisticated analysis is that it actually provides the reader with a feeling for these intermittent and differently capable temporal orders. It also allows the reader to feel the push and pull of these diverse temporalities, none of which, by itself, satisfies all ontological and political dilemmas.

The introduction of a *strategic temporality* focuses on the pivotal role of *events* in John Hall's study. A study of Western history as one dominated by apocalyptical thinking and apocalyptical movements illuminates the way in which world-historical actors conceived of events and crises. Dramatic events "end the world as we know it" (3) and "reshape the relations of many individuals at once to history." (20) They redeem peoples and reveal ways out of history. Thus, events are significant in and of themselves in a history read apocalyptically.

And here, with the conjoint introduction of hybrid temporalities and critical events, John Hall's book takes a decisively significant theoretical turn. Apocalyptic movements and thinking has become a mechanism for thinking about humans in time more generally. The latter chapters of *Apocalypse* work out what I would call a combinatory theoretical program that draws from the interrelations between the diachronic time frame and that of the strategic. John writes that: "The interrelations between actions framed in diachronic and strategic temporality are facilitated because both these temporal orientations are centered on unfolding sequences of events. Disjunctures and aporias, when they occur, tend to arise because of the difference between the diachronic emphasis on repetition and calculability, versus the emphasis in strategic temporality on one-off actions meant to shape the future course of events." (213) Modernity, notes John, plies its way with an overt commitment to diachrony, while imperialism is endlessly invested in strategic time (one military event after another). And much of the later chapters of the book are devoted to exploring how the "Empire of Modernity" has bound these two apparently antithetical temporalities together. But in doing so, the last chapters of the book also step outside the substantive preoccupation with apocalyptic politics to comment on the way that, "Empirical social research replicates this divide in the dichotomy between (diachronic temporality) sociologies of organizations and institutions, on the one hand, and, on the other (strategic temporality) sociologies of social movements, rebellions, revolutions, and war." (129) I was struck by this apparent theoretical aside—both because the statement captured something I had sensed but could not articulate about conventional and codified categories of social research and because it allowed me to think about what a more combinatory approach to e.g. organizations and revolutions might allow us to see (without turning us into social scientific imperialists!). In other words, I think that John Hall's exploration of apocalypse has ended up giving us nothing less than an analytical strategy toward synthesizing *structuralism and pragmatism.* It provides an angle of vision on repetition and ongoingness, an angle of vision on disruption and disjuncture, and a way of seeing them both subsumed under a singular ethos.

> *John Hall's exploration of apocalypse has ended up giving us nothing less than an analytical strategy towards synthesizing structuralism and pragmatism.*

Of course, the apocalyptic cannot be the only rubric by which we can experience the time of repetition and the time of strategic intervention, but it is a dominant one. What might be an alternative? Here John provides a tantalizing, but under-analyzed clue. He allows that there have been exceptional events that are not apocalyptic in nature. One significant non-apocalyptic event was the American Revolution: "[M]illennial theology did not drive the Revolutionary War for independence from the United Kingdom, nor did theocratic visions find their ways into the ensuing constitutional construction of the US." (148) How are we to account for the non-apocalysm of the American Revolution, particularly given the way that apocalyptic thinking did have a significant hold on the early colonists' thinking?

Part of the answer to this question might lie in the nature of the authority forged in the codification of the Declaration of Independence and in the Constitution. Several scholars, including Hannah Arendt, Jacques Derrida, and Bonnie Honig have focused on the second sentence of the Declaration, and particularly on the "We hold" clause that begins that sentence. With differences in approach and analytical terminology, these three scholars are in agreement that this clause is revolutionary in itself—performing the act of creating a collective authority in and with the very declaration of it. There is no appeal here to a transcendent god or traditional king—no external source of authority to anchor this bold self-constituting collectivity. Certainly, war is the strategic backdrop for this statement (and thus the alternatives are not non-violence versus violence). Further, as all three commentators ruefully acknowledge, the rest of that second sentence of the Declaration reaches out to such external authority as natural law and the creator. Nevertheless, the desire and the ability to *hold* through self-constitution, however briefly, seems to point a way out of the diachrony/strategy apocalyptic vortex.

This alternative to apocalyptic imaginings, while obviously not the focus of the book, allows us, perhaps, to comprehend better the apocalyptic animus detailed in the book. The counter-point reference to the American Revolution in the book allowed me to go back to John's discussion of the way an apocalyptic sense of history originates with collectivities claiming they are beleaguered by an "Other." Whether they are holders of power or are countercultural movements aiming to overthrow the established order, apocalyptically oriented actors appear to require a threatening, illegitimate Other. John identifies such an element early on, as when he writes that during the Medieval period: "apocalyptic meanings typically became fixed in relation to the definition of an Other that constituted a threat to the sacred." (77) And he highlights the connection between monotheism and an apocalyptic sense of history, as one God and one community of believers become linked in destiny (with the inevitable triumph over designated others). Establishing and responding to a threatening other seems thus to be a necessary feature of an apocalyptic sense of history. While such juxtapositions are certainly also possible in alternative historical modalities, they do not seem to play such a dominant role. And, in the end, the painstakingly careful historical survey of the apocalypse in history in John's book might allow us to see that such differences in emphasis can be extremely consequential. Perhaps, though this may sound overly optimistic, we may be experiencing a slight diminution of an apocalyptic sense of history in our own Empire of Modernity through just such a muting of the role of the Other.

Finally, I want to thank John for writing this provocative and intellectually bracing work. It has made me view modernity differently—no small task. I am honored to be part of its discussion.

Reply by John R. Hall

University of California, Davis

To begin, I wish to thank my colleagues for their thoughtful and thought-provoking readings of *Apocalypse*. And I am pleased that the book's basic themes seem to come through, in particular, my use of the Apocalyptic as something like a chemical reagent to retheorize modernity. At the outset, let me reaffirm that *Apocalypse* is an exploratory venture in interpretive macro-history, that it raises more questions than it can answer in any solid way, and it depends upon, but fails to fully plumb, the insights from a very wide range of scholarship. Rather than addressing the comments of Ari Adut, Ivan Ermakoff, and Robin Wagner-Pacifici each in turn, I will discuss common and overlapping themes and issues.

Apocalypse, as Robin Wagner-Pacifici observes, re-visions modern history by way of a phenomenology of temporalities. Tracing the history of the apocalyptic in the long run finally cashes in the promissory note that I wrote in *History and Theory* now almost 30 years ago, proposing "a history of times" by way of an excavation of "the times of history." But *Apocalypse* does its cashing-in partly by theorizing "the Empire of Modernity"—the articulation of diachronic temporalities of panoptic administrative and bureaucratic modernity with strategic temporalities of economic and military empire. As Robin remarked, the book takes a decisively theoretical turn once I consider the Empire of Modernity, by which point the apocalyptic has "escaped" purely religious significations to appear clothed in secular garb. The theoretical turn, in so far as it "works," does so because the temporal phenomenology that I laid out maps the various social arenas where different social theories have their relevance and explanatory or interpretive power. In this light, many of the best social theories that seemingly contradict one another may not do so at all. Rather, their relevances are tied to alternative arenas—for example, to the world of everyday life, or the symbolic and ritual work of and by and for communities, or the pursuit of strategic action under conditions of competition or conflict within arenas ranging from stock markets to war. Thus, mapping the phenomenology of modernity can help facilitate a new rapprochement and synthesis among diverse social theories.

Why might a "structural phenomenology of temporalities" have such potential? We can address that question by proceeding from Robin's very interesting suggestion that the book achieves a synthesis of structuralism and pragmatism via conjoint consideration of ongoingness and repetition, disruption and disjuncture. A path for considering the sorts of analyses by which this synthesis operates can be cleared by addressing two parallel questions—Ari Adut's, about two frames of the apocalyptic—time and action; and Ivan Ermakoff's, concerning whether there is some genetic "system of representations" that constitutes the apocalyptic.

Ari is quite right to invoke Paul Ricoeur, for there is a homology in *Apocalypse* among narratives, social actions, and social temporalities. Meaningful action is scripted in ways that constitute one or another organization of action in relation to the past, the present, the future, and complex combinatories thereof. In phenomenological terms, what Alfred Schutz called the "frame of relevance" for action may invoke and reenact previously established meanings (Mircea Eliade's "eternal return"), or it may posit some goal to be achieved in the future on the basis of present action, or seek to intervene to shape unfolding events, or so forth.

In conceptualizing the time/narrative complex, my approach borrows from Max Weber's strategy of using case-comparative ideal types (e.g., in *The Protestant Ethic and the Spirit of Capitalism*). At the most general level, I posit the apocalyptic as an extreme social and cultural disjuncture in which dramatic events reshape the relationships of many individuals at once to history. The questions that arise in turn have to do with generic ideal types of apocalyptic action, and then, with the cultural specificities of how people orient in apocalyptic ways, especially,

- *When* they position themselves in relation to the apocalyptic crisis – most importantly, before, during, or after the apocalypse, and,
- What *meaningful response or narrative* they advance as the script of action in relation to the apocalyptic.

As Ari observes, there are intense dualities – between divine will and human agency, between politics and religion, between chaos and meaning. These antinomies derive in part from how apocalyptic actors themselves construct inevitability in relation to agency. Given the diversity of possible trajectories along such lines, Ivan wonders, "shall we conclude that there can be no theory of the factors leading to the emergence of apocalyptic eschatologies?"

Developing such a theory is a worthy and challenging project, though not the one I pursue in *Apocalypse*. There, I identify alternative meaningful constructions of the apocalyptic, and I find that certain constructions emerge again and again, throughout history. However, I'm enough of an historicist to note that the conditions and contexts under which apocalyptic constructions emerge change over time – sometimes, as part of a dialectical process in which agents of an established social order seek to contend with, undermine, or harness apocalyptic dynamics. Thus, rather than seeking to develop a general theory to account for *emergences* of apocalyptic eschatologies, I reverse the causal analysis and look to the *significance* of the apocalyptic, when it arises, for macro-historical dynamics.

The agenda of what might be called an "historical critical theory of the apocalyptic" brings us to Robin's point about the variety of apocalyptic processes I observed, and, as a flip side, to Ivan's question about whether it is possible to identify a genetic formula of the apocalyptic—some sort of system of representations, as he puts it. Robin notes the diverse processes that I have identified as shaping the articulation of the apocalyptic with long-term historical developments. She mentions realignment, relocation, reworking, reinvention, and rearticulation, and points to the alternative possibilities of *genealogical connections* between older and newer articulations, versus *functionally equivalent reinventions*.

...we ought not consider the apocalyptic as a "thing" that has irreducible properties and constrained paths of development, any more than "bureaucracy" should be reified as an inalterable social form. Rather, the apocalyptic is something like a vortex, the forces of which shape how other projects and processes play out.

One era of realignment occurred during the Reformation, indeed, with apocalyptic shifts that helped to drive Reformation outcomes. Very briefly: Luther prevailed in Germany because the princes were not willing to contemplate the prospect of Thomas Müntzer or the Anabaptists becoming dominant, and sought to undermine their prospects. Across the English Channel, the Puritan Revolution later unfolded under the flag of an apocalyptic ideology, and the outcome of English confrontations with the apocalyptic ended up transferring from religion to the state the capacity to regulate sectarian heresy in general, and apocalyptic movements in particular.

The lesson I draw from this telescoped narrative is that we ought not consider the apocalyptic as a "thing" that has irreducible properties and constrained paths of development, any more than "bureaucracy" should be reified as an inalterable social form. Rather, the apocalyptic is something like a vortex, the forces of which shape how other projects and processes play out. To address the nature of this vortex, let us turn to Ivan's observation about the diversity of apocalyptic eschatologies, and to his point that sometimes the very diversity begs the question of whether the apocalyptic is a coherent category at all.

Here, precisely, I avoid the standard semiotic model of institutional structures of meaning as sets of binary codes, instead engaging in a more hermeneutic exploration of meaning *making*, with the emphasis on the making. My source of guidance in this endeavor comes from a critical reading of Karl Mannheim's *Ideology and Utopia*. Mannheim offered a compelling and useful typology of alternative ideologies, notably, by describing them in terms of their alternative temporal orientations. But he did not explore the relationships between ideology and action, even though his focus on temporality—given the connectedness of temporality and action—provides an excellent basis to do so.

Phenomenologically, a generic temporality of the apocalyptic encompasses both the lead-up to dramatic world-changing events and their aftermath. However, *action* in relation to apocalyptic temporality is underspecified. Some ideal typical alternatives are worth noting.

- Anticipation (of the inevitable End, a foreordained act of God) may produce preparation, e.g., getting right with God and helping others to do likewise through activities of evangelism and conversion.
- Advancing the final moment of reckoning—and its "positive" resolution—can be undertaken either as "war against the Beast" by those who resist and oppose the established social order, or alternatively, through the triumphalist apocalyptic war of empire.
- Those who seek to escape the apocalypse and live in a (utopian) postapocalyptic situation can work to establish a "heaven on earth."
- Those who prepare to endure the (dystopian) postapocalyptic situation may establish a survivalist community.

However, it is rarely the case that only one of these alternatives surfaces in the absence of others, or, indeed, in the absence of other, *non*-apocalyptic ideological programs.

These considerations now make it possible to explore several theoretical issues in relation to empirical situations. Let us take Ari Adut's point about the difference between motivation and justification, instancing Robespierre and the French revolution. Ari rightly observes that the apocalyptic Reign of Terror was not the only revolutionary script in play, and it was not the one that prevailed in the short term, much less the long run. He also questions whether Robespierre's motivations in the Reign of Terror may not have been less than pristine revolutionary ones. This point should alert us to the principle that—as with political dynamics more generally, and perhaps even more so—the apocalyptic is messy. As Eric Hobsbawm observed in *Primitive Rebels*, revolutionaries and bandits often make common cause—something we see equally with al Qaida and the Taliban in the present day. Hobsbawm also noted that movements are *not static*: they may shift from emphasizing utopia to banditry, from religious asceticism to war, and so on. As these possibilities suggest, apocalyptic rhetoric sometimes masks other motivations. Thus, we can anticipate that empirical instances will range from true apocalyptic belief to completely cynical use of apocalyptic rhetoric for justification. And, whatever the authenticity of apocalyptic motivations, in concrete movements, they often find common cause with quite heterogeneous interests.

A different way of coming at the ironies and ambiguities of the apocalyptic is to consider the range of relevance of the construct. Ari and Ivan both point to global warming and environmental collapse as "apocalyptic" concerns that emanate from science in a way that seems distant from the Othering by religio-political movements and the quest for ultimate transcendence by groups more religious in their eschatologies. Ivan wonders—citing both environmental collapse and Germany's Red Army Faction—what leverage is to be gained by considering them in the same league with, say, the Puritan New Model Army, or even further into messianic and magical violence, Thomas Müntzer and the Peasants' War in Germany.

The empirical question, I would argue, is whether such groups act in similar ways *in time*, that is, in their organization of activities in relation to an apocalyptic moment. That question can only be addressed in the first place by positioning analysis in relation to an apocalyptic framework. Although certainly there are differences, *Apocalypse* suggests that there are also genealogical connections and affinities between seemingly radically alternative movements, say, between Müntzer's divinely inspired insurgency and the propaganda of the deed practiced by the Russian anarchists in the nineteenth century.

What, then, about ecological collapse as contemporary apocalypse? I give passing mention to the subject at the beginning and end of *Apocalypse*, but not the attention it deserves, namely, a study in itself. How might such a study be framed, given my approach?

In the contemporary emergence of concerns about ecological collapse, to invoke the phenomenology of temporality, scientists acting in diachronic time seek to calculate, as precisely as possible (again in diachronic time) what they anticipate as planetary catastrophe. Strikingly, some of the scientists who warn of global warming and ecological disaster share the precision of the old-time religious and pseudo-scientific date setters – for example, Heinrich Bullinger, who prophesied that the world would end in 1666 (a millennium plus the sign of the Beast); or William Miller, who predicted several exact dates in 1843 and 44 before acknowledging that he had gotten it wrong. Of course, the scientists point to different signs, ones we like to think are empirically observable. (Some diachronic analyses tragically suggest that we already have passed the date after which it would be "too late" to intervene successfully.) In order to consider such developments, sociologists of science would want to assert the principle of the "strong programme" – that "wrong" science and "right" science be accounted according to symmetric principles of explanation.

By these lights, the scientific provenance of evidence about ecological apocalypse should make no difference for how we think about it in apocalyptic terms, even though, as Ari points out, the thinking itself is largely a product of rational, calculative analyses of risk. The question is, how is this crisis *addressed* by (ideological) movements—either in apocalyptic or other terms, e.g., through rational, diachronic action, by warring struggles over resources, or under survivalist flags?

Most likely, *popular* responses to ecological crisis in the next few years will be folded into a variety of narratives, notably, capitalist corporate efforts to claim "sustainability," progressive ecological movements, radical Earth First! actions, and movements in relation to the 2012 end of the Mayan calendar, which is the big apocalyptic development on the horizon, now widely inaugurated with the release of the movie *2012* (already, some 2012-ers are taking classic paths of apocalyptic action, e.g., selling their stakes in Europe and moving to parts of the globe that they anticipate will be less touched by the ecological apocalypse). In short, predictions of environmental collapse are translating into diverse narratives about how they should be met, and, as Mannheim anticipated, both apocalyptic and other ideological framings and movements are emerging.

To conclude, let me take up an issue raised in different ways by all three critics – that not all dramatic or violent social change is orchestrated through apocalyptic scripts. The example that Robin instances is the American Revolution. Her central point is that even radical change need not proceed on the basis of constructing an Other as something approximating the Antichrist. For human societies, it would be a welcome development to channel conflict outside apocalyptic frames of meaning.

Perhaps this would be a resolution to Maurice Merleau-Ponty's problematic that Ari has asked me to reflect upon. Merleau-Ponty posited the difficulty of eliminating historical violence, and he advanced the more modest goal of avoiding descent into meaningless chaos by somehow restricting violence to progressive purposes.

In *Humanism and Terror*, published in 1947, Merleau-Ponty drew up a standard of whether violence might be deemed progressive or meaningless. He just had not *yet* come to recognize that Soviet violence had become self-serving and politically meaningless. Thus, I'm inclined to think that what Ari describes as Merleau-Ponty's shift to embrace of "parliamentary democracy, a non-apocalyptic temporality, and a liberal morality" could be framed within the very rubric that Merleau-Ponty sets forth in *Humanism and Terror*, i.e., whether violence can be justified as "progressive."

Here, to venture well beyond *Apocalypse* the book, we can briefly consider modern and postmodern constructions of the relation between social sciences and the containment of violence. The hope of a (diachronic and ideologically progressive) positivism was to eliminate the social conditions that breed violence, and to shift the exercise of power from violent to other kinds of dispute resolution. Today, postmodern doubts about the efficacy and role of the social sciences mean that we are much less optimistic about containing violence. Yet I cannot follow the retreat to a conservative position that regards violence as inevitable and relegates the effort to control violence solely to the political realm. To be sure, the historical social sciences cannot fulfill the positivistic program to eliminate violence. However, much as the English sought to achieve a new rapprochement between politics and various religious tendencies with the Glorious Revolution of 1688, I hope that we can help to promote a new and fruitful understanding of the conditions of our own established order – the "Empire of Modernity" – one that would reduce the structural bases of conflict and violence.

Still, I must end with a cautionary note – that reconstructing the calculus of violence is far easier said than done. The American Revolution was in major respects a child of the Enlightenment. The world violence of today resurrects both religious legitimations and the apocalyptic tendency toward Othering. Thus, the problem of religion has resurfaced as central to our era, over and against ideas borne of the Enlightenment, that secularization would yield religion less and less relevant. To invoke the scholar of religion Adam Seligman, secularity will not solve the problem of religious conflict: religions themselves must also come to terms with the challenge of creating a post-apocalyptic world.

Identities

Editors' Note: In this section, comparative-historical scholars reflect on why they entered the subfield.

Henning Hillmann
Stanford University and University of Mannheim

How did I become a historical sociologist? I have never given much thought to this question. In fact, I rather like to think of myself as an economic sociologist who happens to work with historical data. Still, for one reason or another, I must have decided some time ago that studying the networks of dead people is a good career choice. I wish I could claim a rich intellectual ancestry within my family that set me on the path of studying the past. But I come from a family of public servants and bank clerks. At best, then, I may lay claim to second-hand anecdotal knowledge of modern bureaucracies. But if there is anything in Max Weber's brand of comparative-historical sociology that captured my interest later on, it is the emphasis on elite competition and not so much questions of work and organization.

But back to the beginning. Born in 1970 in Aix-la-Chapelle, I grew up in Brunswick, a city in the North of what used to be West Germany, located right next to the East German border. Brunswick itself was founded back in the deepest Middle Ages and brimming with local history. That history was populated with colorful characters. The most famous being Henry the Lion, the Duke of Saxony during the time of the Crusades. I still remember repeated visits to the crypt of the Dukes underneath the city's cathedral—and in particular a tiny hand-held bell sitting on top of one sarcophagus whose exact purpose is shrouded in mystery (but that's material for another story). History was always just around the corner with countless churches, cloisters, mansions, and all those artifacts that readily feed a child's vivid imagination of the past. However, I doubt that any of these circumstances contributed to my eventual choice of becoming an economic sociologist with a taste for history.

History also turned out to be my favorite subject when I attended the *Gymnasium* in the 1980s (sociology was not even offered, and to the best of my knowledge, it still isn't part of the German high school curriculum). But it was very much the history of great men. Our history teacher once asked us to write an essay about our favorite historical character. The usual suspects were nominated. Instead, I chose Fredrik II, the medieval emperor who figured so prominently in the *The King's Two Bodies*. I didn't know anything about Kantorowicz. It was rather the ambivalence and tragedy of Fredrik's life that appealed to me as a nerdy adolescent. All this merely goes to show that I apparently shared the penchant for the role of great men, yet with little sense of their overall positioning in the broader course of history.

I graduated from high school in May 1989. Only a few months later the Berlin Wall fell and the Iron Curtain was raised. In retrospect, a chain of historical events of epic proportions unfolded right before our eyes. Of course, it would be tempting to say that these events shaped my ideas about historical change and that they had a lasting impact on my identity as a budding sociologist. At the time they happened, however, these events seemed rather unreal and even surreal to me. In a strange way, what happened in Eastern Europe seemed to have more in common with television broadcasts of regime changes in far away places like central Africa or Latin America than with actual events at home. Their full historical significance became apparent only later on, at least to me.

Before entering university, I was very much set on majoring in history. Unfortunately, I had to learn that my high school coursework in Latin was just two years short of the required level. As I was unwilling to complete the additional Latin classes, I browsed the course catalogue and decided to pick the next best thing to history instead. I had no idea what sociology was all about, but it seemed reasonably close to history. Hence, becoming a sociologist was an accident, a second-best choice. To this day, I have rarely regretted that choice. During my undergraduate years, however, history and historical sociology took a backseat. What appealed much more to me at the time was grand social theory (Bourdieu, Lévi-Strauss and other usual suspects). Eventually, I ended up writing an undergraduate thesis on the sociology of intellectuals, a pretty European and terribly navel-gazing topic.

All that changed after I had graduated from Humboldt University and moved from Berlin to New York to enroll in Columbia University's graduate program in sociology. Back in Berlin, I had already become interested in social network analysis, and Columbia was one of the natural places to be in order to do networks. Harrison White and David Stark were already there. Peter Bearman arrived from Chapel Hill just when I started the program in 1998, and he became my advisor pretty much from the beginning. One of the most important things I learned through his training was to aim for deep theoretical insights out of quirky data from intrinsically interesting settings. One fine day, I don't really recall why, Peter gave me Georges Duby's *The Three Orders* to read. I read it as a fascinating account of the long-winding transformation of the social relational foundations of Feudalism. I was hooked. But I took the idea of using quirky data a bit too close to heart and decided to study the diffusion of relics in medieval Europe. Peter's attempt to save me from that enterprise was to suggest to either model the social structure of the Bible or the diffusion of miracles instead. So much for avoiding risky topics. A few years later, another of Peter's students, Paolo Parigi, was much more successful in actually producing the miracles study.

I took one last stab at medieval subjects, focusing on the diffusion of monastic orders. It was around this time, when I started to become nervous that I would never be able to find a good dissertation topic, that a fellow student, Jenn Lena, said to me: "Henning, I think it's really simple—you are interested in things that move around over time and across space" (or something vaguely similar). I think she was right. What fascinated me was not the historical content per se, but rather historical data because they allowed me to figure out recurrent patterns in the ways that "things moved around" -- how they diffused over long periods of time and across diverse settings. This revelation brought me back full circle to history. I settled on a study of the diffusion and mobilization of the countless insurgencies that erupted throughout colonial North America in the late eighteenth century. For my dissertation, I ended up in New England courthouses to collect probate records from which I stitched together the credit networks that formed the economic foundation for political mobilization.

As others have stated in these pages, the story of becoming a historical sociologist is very much the story of becoming, and especially thinking like a sociologist in the first place. But only after the fact does it all resemble anything like a coherent narrative. At the time things happened, they seemed like a chain of mere accidents: from visiting the crypt of the Dukes of Saxony, the fall of the Berlin Wall, and coming to Columbia, to the study of political mobilization in Revolutionary Vermont. How I got from there to nineteenth-century Russian entrepreneurs and British and French privateers in the eighteenth century is yet another branch of that story.

In the end, it's all about "trying to figure out how things worked" as the historian Richard Pares once put it. Figuring out how things worked, it seems to me, is all about understanding what things mean in relation to other things that came before them and those that will happen in the future. And such understanding of the unfolding of events and actions over time is possible only with historical data. Herein lies the great attraction and relevance of historical sociology's enterprise. The much less desirable alternative is to be stuck in the illusion of a perpetual present where everything seemingly starts from scratch without any prior experience, and hence without any meaning.

Publications

Mounira M. Charrad. "Kinship, Islam or Oil: Culprits of Gender Inequality?"*Politics and Gender* (A Journal of the American Political Science Association). Vol. 5 (4), December 2009.

Leslie C. Gates, "Theorizing Business Power in the Semiperiphery: Mexico 1970-2000." *Theory and Society*. Volume 38, Number 1: 57-95. 2009.

Ho-fung Hung, editor. *China and the Transformation of Global Capitalism*. Baltimore: Johns Hopkins University Press. 2009.

Ho-fung Hung. "America's Head Servant? The PRC's dilemma in the Global Crisis" *New Left Review*. No. 60, Nov-Dec. 2009

Margaret R. Somers, *Genealogies of Citizenship: Markets, Statelessness, and the Right to Have Rights*. New York: Cambridge University Press. 2008.

Margaret R. Somers and Christopher N.J. Roberts. "Toward a New Sociology of Rights: A Genealogy of 'Buried Bodies' of Citizenship and Human Rights," *Annual Review of Law and Social Science.* 4:385–425, 2009.

David Stark. *The Sense of Dissonance: Accounts of Worth in Economic Life*. Princeton: Princeton University Press. 2009

Awards

Congratulations to **Margaret Somers** (University of Michigan), whose book *Geneaologies of Citizenship* was awarded the Giovanni Sartori Qualitative Methods Award by the American Political Science Association. The award is given to the best of "studies that introduce specific methodological innovations or that synthesize and integrate methodological ideas in a way that is in itself a methodological contribution; and substantive work that is an exemplar for the application of qualitative methods." Further information can be found at:http://www.apsanet.org/content_4129.cfm

Congratulations to **Leslie Gates** (SUNY-Binghamton) who won the best article award from the Political Economy of the World System section of the ASA, for her article "Theorizing Business Power in the Semiperiphery: Mexico 1970-2000." *Theory and Society*. Volume 38, Number 1: 57-95. 2009.

Calls for Papers and Applications

Call for Abstracts. Junior Theorists Symposium. August 13, 2010, Emory University.

The Junior Theorists Symposium is a special one-day conference for up-and-coming theorists, organized by the Theory Section of the American Sociological Association. This conference features scholars at a relatively early stage in their careers, and brings together sociologists who are engaged in original theoretical work as part of their ongoing research.

The forum will include up to 12 presentations organized into three sessions. There is no pre-specified theme for the conference. Instead, accepted papers will be grouped based on how they speak to one another. Past thematic groupings have included: Knowledge; Practice; Politics; Selves and Social Situations; Institutions and Organizations; Identity, Race, and Interaction; and What is Social Theory?

Neil Gross (University of British Columbia), Michèle Lamont (Harvard), and Andreas Wimmer (UCLA) will comment on the presentations.

We invite all ABD students, postdocs, and Assistant Professors up through their 3rd year to submit abstracts.

Complete information for submitting the abstract will consist of: (1) name and contact information of the author; (2) title of the presentation; (3) a 250-word abstract; (4) three or more keywords descriptive of the presentation.

Please send submissions to the organizers: Claire Laurier Decoteau, University of Illinois (decoteau@uic.edu) and Robert Jansen, University of Michigan (rsjansen@umich.edu).

The deadline for submission is December 15. Invitations to present will be extended by January 31. Please plan to share a full paper by July 1, 2010.

Information on the previous incarnations of the JTS (2005, 2008, 2009) can be found at: http://www.asatheory.org by clicking on the Junior Theorists Symposium link.

Policy History Conference
Call for Papers
Deadline for Submission: December 30, 2009

The Institute for Political History and the Journal of Policy History are hosting a Conference on Policy History at the Hyatt on Capitol Square in Columbus, Ohio from June 3 to June 6, 2010. We are currently accepting panel and paper proposals on all topics regarding American political and policy history, American political development, and comparative historical analysis. Complete sessions are encouraged, but individual paper proposals are welcome. The deadline for submission is December 30, 2009. Please see our website for further information: http://www.slu.edu/departments/jph/conf2010.html

2010-11 National Miller Center Fellowship Applications Now Online

The ***Governing America in a Global Era (GAGE) program*** at the University of Virginia's *Miller Center of Public Affairs* invites applications for the *2010-11 National Miller Center Fellowship*. The Miller Center Fellowship is a competitive program for individuals completing their dissertations on American politics, foreign policy and world politics, or the impact of global affairs on the United States. The GAGE fellows represent a new cohort of academics who seek to address critical issues facing our nation by engaging a larger public in a discussion about patterns in American political development.

The fellowship provides up to eight $20,000 grants to support one year of research and writing. Along with the fellowship grant, the Miller Center assists the fellow in choosing a senior scholar as fellowship "mentor" to make suggestions on the literature in which the fellow should frame the project, read the fellow's work, and give general advice on research.

The Miller Center also invites applications for the *Wilson Carey McWilliams Fellowship*. The McWilliams Fellowship supports a graduate student in political science or history whose dissertation combines the special blend of Political Theory and American Politics that characterized the late Wilson Carey McWilliams' extraordinary scholarship. The applicant must be a Ph.D. candidate who is expecting to complete his or her dissertation by the conclusion of the fellowship year. The McWilliams fellow will participate in the regular Miller Center Fellowship program and will also be paired with a fellowship "mentor."

The Miller Center encourages applicants from a broad range of disciplines, including but not limited to history, political science, policy studies, law, political economy, and sociology. Applicants will be judged on their scholarly quality and on their potential to shed new light upon contemporary developments in American Politics, Foreign Policy, or World Politics.

Requirements: An applicant must be 1) a Ph.D. candidate expecting to complete his or her dissertation by the conclusion of the fellowship year, or 2) an independent scholar working on a book. This is not a post-doctoral fellowship.

Residence is strongly encouraged but not required. All fellows are expected to participate in and contribute to the intellectual discourse at the Center, as well as attend conferences in Fall 2010 and May 2011. These two conferences will provide a forum for presenting research and findings to the scholarly community at the Miller Center and the University of Virginia.

To Apply: Please complete our online application at: http://www.millercenter.org/academic/gage/fellowship.

All applications must be submitted online by February 1, 2010. Applicants will be notified of the selection committee's decision in April 2010. Inquiries should be directed to Anne Mulligan atacm8k@virginia.edu or 434-243-8726, or Bart Elmore at bje5d@virginia.edu . Additional information is available at: http://www.millercenter.org/academic/gage/fellowship.

The Comparative and Historical Sociology Section would like to congratulate:

Karen Barkey, Columbia University
Author of *Empire of Difference: The Ottomans in Comparative Perspective* (Cambridge University Press, 2008)

And

Ivan Ermakoff, University of Wisconsin-Madison
Author of *Ruling Oneself Out: A Theory of Collective Abdications* (Duke University Press, 2008)
Co-Winners of the Barrington Moore (Best Book) Prize of the Comparative and Historical Sociology Section.

Cedric de Leon, Providence College
Winner of the Best Article Award of the Comparative and Historical Sociology Section for "'No Bourgeois Mass Party, No Democracy': The Missing Link in Barrington Moore's American Civil War." *Political Power and Social Theory*. 19: 39-82. 2008.

Honorable mentions:
Ho-Fung Hung, Indiana University "Agricultural Revolution and Elite Reproduction in Qing China: The Transition to Capitalism Debate Revisitied." *American Sociological Review* 73(4): 569-588. 2008.
Liliana Riga, University of Edinburgh, "The Ethnic Roots of Class Universalism: Rethinking the 'Russian' Revolutionary Elite." *American Journal of Sociology* 114: 649-705. 2008.

Ateş Altinordu, Yale Univeristy
Winner of the Reinhard Bendix (Best Student Paper) Prize of the Comparative and Historical Sociology Section for "The Politicization of Religion: Political Catholicism and Political Islam in Comparison."

Honorable mention:
Wesley Hiers, UCLA, "The Colonial Roots of Racialized Polities."

See the committees' comments on each of the papers, and links to the papers themselves, at:
http://www2.asanet.org/sectionchs/awards.html

In the next issue of *Trajectories*:

Book symposia on Mabel Berezin's *Illiberal Politics in Neoliberal Times* and Jeff Haydu's *Citizen Employers.*

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