On Recent Comparative Historical Studies

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gendering varies by race and class, they provide a refreshing willingness to combine economic and cultural accounts.

References


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The Historical Turn in Democratization Studies: A New Research Agenda for Europe and Beyond, by Giovanni Capoccia and Daniel Ziblatt. A special issue of Comparative Political Studies 43 (8-9) 931–1176 (2010).
I.

One may ask, not unreasonably, whether comparative historical analysis is not a rather esoteric side-field of social science. Moreover, given the extremely limited access to the subjective world of people living in distant historical periods, how can one arrive at more than imaginative reconstructions of the past, which inevitably reflect our present-day expectations? Finally there is the inclination of many to consider historical developments as unique sequences and configurations of events that cannot be reasonably compared with other patterns; in this view, comparative historical analysis founders on ineluctable historical uniqueness.

Instead of a rejoinder to these doubts, I will point out that the classic scholars of social and political analysis—from Vico and Condorcet through Marx and Spencer to Durkheim and Weber—all engaged in comparative historical work; and they laid the foundation for modern social and political theory. Comparative historical studies experienced a revival in the twentieth century, and similar to the works of the modern classics, this later research generated again powerful insights and understandings.

A review of this revival demonstrated that, for example, our views of the causal conditions of social welfare policies (Amenta 2003), of democratization (Mahoney 2003), and of revolutions (Goldstone 2003) would be radically impoverished if it were not for renewed comparative historical studies.

The stream of important comparative historical publications continues unabated. The publications discussed in the following represent only a fraction of the major works that have appeared in print since the beginning of this century. I have selected a few that deal with power and states in different causal inquiries: two works that address major transformations over long periods of time, three studies about empires, and two contributions on democratization that stand out for their methodology.


II.

Richard Lachmann’s *Capitalists in Spite of Themselves* (2000) opens the analysis of economic transitions in early modern Europe with words that directly invoke the classic tradition: “Something happened in Western Europe in the fifteenth through eighteenth centuries. Sociology’s founders believed the task for their discipline was to define that something and to explain why it happened when and where it did. Karl Marx, Max Weber, and Emile Durkheim each dedicated their lives to that project” (p. 1). Lachmann reviews research on the slow emergence of capitalism in Europe done by several generations since then, and he sets out a major causal hypothesis to be tested and demonstrated in the analysis of specific phases in European history.

For his analytic framework he borrows a fundamental idea from Marx and Weber—the conception of social class grounded in modes of production. On this he grafts his central proposition that it is elites and elite conflict which are the essential proximate causes of economic and social change. Elites represent interests embedded in organizations and institutions, and they enjoy sufficient power to extract resources from the producing classes. Non-elites have transformative power only when allied with such power holders. Significant economic and social change is prevented or promoted by dominant elite alliances, permitted by elite conflict that cancels the power of opposing elites, possibly advanced by alliances of elites with non-elites.

Lachmann first turns to changes in medieval agriculture and its varied feudal power relations. Much attention had been given to the impact of the Black Death of 1348. But the consequences of this demographic disaster were more ambiguous than simple extrapolations of decreasing population/land ratios suggested. Most tenants in England and France did gain more freedom; but these developments had begun already before 1348. And other areas saw re-enslavement. Close regional and temporal comparisons lead Lachmann to his conclusion: where elites stood in unresolved conflict, peasants gained freedom; where elite conflicts were...
Movements toward exclusive property rights and "free" labor in agriculture can be seen as partial moves toward capitalism, though this outcome was neither foreseen nor intended at the time. The same is true of other partial developments Lachmann analyzes. The rise and decline of European city "states" and their leagues are discussed with an extensive focus on Florence. The subsequent rise of territorial states, which curtailed the autonomy of cities, is demonstrated in chapters on England and France. Elites and their alliances are shown to be decisive for outcomes of these developments that in retrospect tell us much about contingent and reversible phases in the emergence of capitalism, even though the goals pursued by elites were mostly short-term in character and ordinarily defensive of current interests—"capitalists in spite of themselves" indeed.

Lachmann offers detailed, often acerbic and sometimes quite single-minded critiques of earlier work (one case of the latter is his treatment of Weber on Protestantism and capitalism). Yet in their totality these assessments are themselves a major contribution. His main achievement, however, is to establish a large role for power in the proximate causation of the slow emergence of capitalism.¹

Statistical work of Louis Putterman and his associates (see, e.g., Chanda and Putterman 2005; Bookstette, Chanda, and Putterman 2002) throws a different light on the role of organized power in long-term economic change. They find that the age of states is positively correlated to economic growth in the second half of the twentieth century. This relationship holds despite controls for level of GDP/cap in 1960, extent of schooling, and rate of investment. Yet some of their findings create difficult puzzles. There are two measures of state antiquity: one that covers (with appropriate adjustments) all 2000 years of the common era, and one that breaks off in 1500 CE; surprisingly both are correlated with growth after 1960. This suggests that the correlations reflect very ancient conditions indeed. Equally puzzling is the finding that state age is stably related to economic growth but not to current levels of economic production. Furthermore, state age is not consistently associated with the institutional quality of today’s states, even though measures of state quality have been found related to economic growth (e.g., Evans and Rauch 1999).

One possible interpretation of these puzzling findings is that ancient states transformed societies in ways that only much later contributed to economic growth. Many varied systems of rule that reigned over rural societies supported some division of labor and trade responding to the demand of elite groups; many created a high culture often allied with religion above the local and tribal level; and most encouraged social discipline in the population through religion and family structures built on hierarchies of age and sex. While some of these developments may much later have proved to be favorable to economic growth, "patrimonial" and similar forms of agrarian rule were inherently averse to structural change. They typically encouraged exploitation by elites and discouraged innovation at the point of production. If that two-sided face of agrarian systems of domination is a reasonable idea, it would explain the long delay as well as the ultimately positive correlation between state antiquity and economic growth. The disruptions of agrarian structures of rule through colonialism, social revolutions, massive warfare, and accelerated technical change during the nineteenth and twentieth centuries opened up possibilities for economic transformation in many countries, though these openings were shaped diversely by very old but also more recent legacies of the past. Exploring the complex effects of ancient and younger historical

¹ This is true even if some of his premises will be and have been critically discussed: Can elite groups be treated as historical actors with homogeneous or at least predominant interests? Is his rational self-interest model adequate and adequately developed at the micro- as well as at the macro-level? Does it make sense to identify clear-cut causes in the face of the many varied circumstances that may be causally relevant, even if the ones identified are qualified as proximate, i.e. mediating others? See for instance the lively and instructive conversation between Julia Adams, Samuel Clark, Rosemary Hopcroft, Edgar Kiser and Robert Lachmann himself in the Author Meets Critics session on Capitalists in Spite of Themselves in: Trajectories, the Newsletter of the Comparative Historical Section of the ASA, Spring 2003, vol. 15, no. 1.
state/society relations would be of considerable help in devising and assessing today’s developmental policies.

III.
Three recent comparative historical studies have transformed our understanding of empires and colonial relations, their functioning and their long-term consequences. They offer clues about the longevity of historical forms of rule and they examine long-term effects on social and economic development exerted by contrasting structures of rule.

Karen Barkey’s *Empire of Difference* (2008) investigates the 600-year duration of the Ottoman Empire. Building on her earlier work (Barkey 1994), she examines several critical phases: the emergence of empire, its institutionalization, its dealing with rising dissent, its slow transformation beginning in the eighteenth century, and its transition to nation state forms of rule. Going back and forth between immersion in Ottoman history and theoretical reflection, which is informed by comparisons (above all with the Russian and the Habsburg empires), she develops—following Max Weber’s precedent—an “ideal type” of empire, a construction that is at once a complex concept, focusing in pure form on features that seem most important, and an embryonic theory or theory frame that begins to offer explanation. Her concept of empire sees the political center dominating over diverse component units, which do not become more similar over time nor are they strongly related to each other. It can be visualized as the hub and spokes of a wheel, the rim of which is missing. “The segmentation principle at the heart of empire remains the vertical integration of elites at the expense of horizontal linkages... empires remained in control of segmentation” (p. 18).

What accounts for the longevity of such empires? Fifty years ago, S. N. Eisenstadt in his breakthrough work *The Political System of Empires* (1963) pointed to an inherent dilemma of historic empires: both central rulers and elites in subordinate units had interests that stood in tension with each other, above all cultivating stable cultural legitimation for their rule and mobilizing flexibly usable resources. From this fundamental insight he identified policies that seem most conducive to longevity of empire. Policies of special importance enabled different groups “to work out their own norms and regulations, to establish their own interrelations with other subgroups, and to maintain some scope of autonomous activity and creativity” (p. 315). Eisenstadt referred here to urban elites in education and business as well as to traditional agrarian elites and the population they controlled.

Barkey’s explanation of the Ottoman Empire’s longevity follows a broadly similar direction, but it focuses on the center’s varied relations with diverse peripheries, often defined by ethnicity and above all religion. Her claim that imperial rule is “negotiated rule” rather than imposed transformation corresponds to Eisenstadt’s emphasis on internal flexibility and differentiation as a condition for lasting imperial rule. Yet Barkey’s single case analysis of Ottoman history can be far more specific than Eisenstadt’s confrontation of Weberian and Parsonsian theory, with information about many cases scanned from the historical literature. It also offers more specific theoretical gains. Concentrating on critical phases in the long life of the Ottoman Empire, she can tell us how particular actors and elites acted in changing situations and how their actions fitted into the peculiar ideal typical structure of imperial rule. She is able to use network theory and historical institutionalist approaches to make sense of the situations in which these actors found themselves. And this in turn allows her to elucidate varied developments in center/periphery relations mediated by elites. She makes clear that religious toleration was largely based on organizational considerations and practices rather than on attention to religious tenets, even though religious tenets acquired greater importance in the empire’s dealing with heterodox Islamic movements. She succeeds in explaining how in the last 150 years of the empire, power and initiative shifted from the center to the peripheral elites, who also got into closer cooperation with each other. But in this account she does not fall into simplistic older notions of decline. Notably, the analysis of the critical role of mediating elites also highlights what is often overlooked: that the continuation and change of macro-structures is frequently best understood.
by looking at meso-processes in structural and situational context. 2

Both Eisenstadt and Barkey agree on the fundamental insight that the longevity of historic systems of rule has been supported by internal heterogeneity and looseness rather than by increasing structural and cultural homogeneity. This is an important lesson for anybody who is inclined to expect the opposite based on homogeneous models of society that are derived from modern nation states. The interpretation of the puzzling findings of Putterman and his associates, which I sketched above, presupposes that the variety of historic forms of rule exhibited a tremendous degree of longevity.

Matthew Lange’s discussion of British colonialism (2009) deals with a sharply focused question: What were the consequences of more direct and more indirect colonial rule after independence had been attained? His answer is dramatic and surprising. Contrary to a widespread belief that indirect rule was more friendly to the countries ruled, Lange finds strong evidence that indirect rule was associated with later outcomes of lower per capita GDP, worse health, lower school attainment, and negative features of postcolonial political development. He attributes these outcomes to the facts that direct rule created more effective states with greater bureaucratic efficiency, greater infrastructural power (the capacity to create and to induce the creation of collective goods), and greater inclusiveness, while indirect rule empowered ineffective and often self-interested rule by chiefs and other indigenous elites.

In support of this conclusion, Lange offers first a sophisticated quantitative analysis of different degrees of direct/indirect rule in the British Empire. Four chapters then present instructive case studies of two countries that exemplify clearly the overall statistical pattern of colonial legacies: Mauritius, a case of direct rule versus Sierra Leone, a case of indirect rule, and of two countries that deviate from the overall pattern, shifting during the phase of independence in opposite directions; and Botswana, from indirect rule toward an efficient and inclusive state versus Guyana, from direct rule toward despotism. In the concluding chapter, Lange compares British colonialism to the Japanese, French, Portuguese, and Spanish overseas empires as well as to internal colonialism within Europe. While this final discussion goes beyond the differences between more and less direct colonial rule, it strongly suggests that the colonialism of all powers tended to affect deeply the institutional features of later states, above all their levels of bureaucratization, infrastructural power, and inclusiveness.

That effective and inclusive states advance economic development is a finding in harmony with other research (e.g., Evans and Rauch 1999). That indirect colonial rule militates against institutional developments of bureaucratic organization, infrastructural power, and state inclusiveness is new; and it is an important addition to the repertoire of empirical instances of “path dependence”—patterns of long-lasting historical causation. For studies of imperial longevity such as Karen Barkey’s, it raises significant questions of how empires of difference affect the later quality of states and state/society relations.3

2 Following Weber in the dual use of “ideal types”—as identifying concepts, to which varied realities can be contrasted and compared, and as analytic frameworks or embryonic theories, which begin to offer causal mechanisms—has obvious attractions to those who are concerned about the antinomies between historical particularity and theoretical generalization, as Weber was and Barkey is. Yet maintaining this ambiguity has disadvantages. It makes it more difficult to distinguish between causal mechanisms claims about, for instance, mediating elite networks and other features that are “just” part of the definition of empire used.

3 I do not mean to imply the answer. Scope conditions of theoretically interesting findings—even historically rather than universally-defined scope conditions—are important to keep in mind. Comparative historical analyses can often come to persuasive causal arguments that are focused on a limited set of reasonably “comparable” cases. Matthew Lange offered a survey of other overseas colonial rule that reinforced his analysis of British colonies. Yet one would have to look closely at the Ottoman Empire’s history, especially its last century and a half, as well as compare the trajectories of rule and governance in different parts of the empire in order to reach expectations about the empire’s effect on the quality of later state institutions and the economic trajectories in different parts of the former Ottoman Empire.
James Mahoney’s Colonialism and Postcolonial Development: Spanish America in Comparative Perspective (2010) deals with the same fundamental questions as Matthew Lange’s book, inquiring about the economic and social consequences of colonialism. Yet it considers a much longer time span between causal conditions and outcomes. Mahoney takes off from the observation, noted by other analysts earlier, that the first colonies in Latin America, which were centered around the Aztec empire in central Mexico, the Inca empire in the Andes, and seventeenth-century Northeast Brazil, fared worse in postcolonial development than the initially neglected parts of the Latin American South. Another initial insight came from the fact that some former British colonies—the United States, Canada, Australia, and New Zealand—comprise a significant number of the richest countries in today’s world.

Grounded in his earlier work on historical causes creating path dependencies (Mahoney 2001 and 2003), this book lays out a comprehensive theoretical approach to the study of colonialism and its legacies. It gives greatest weight to long-lasting institutions that must be analyzed in their distributive and divisive consequences even if they also play coordinating roles for circumscribed interests. Among the variety of lasting institutional effects of colonization are setting subordinate ethno-racial segments of society apart, but also laying favorable foundations for capital investments. Such effects can often be traced back to their roots in colonial impositions.

For understanding these origins, Mahoney focuses first on how the political economy in the colonizing countries interacts with the institutional social structure of the area colonized. He points to the broad contrast between mercantilist and liberal colonizing interests and to the degree of institutional complexity in pre-colonial societies. Mercantilist policies focused on exploiting dense, complex and state-like pre-colonial societies, while liberal policies created rich investment opportunities after displacing less populous simpler social patterns.

That Habsburg and Bourbon Spain differed in their institutional outlook and colonizing practice made for special opportunities (though also for additional complexity) in understanding the effects of Spanish colonialism. Building on the differences just indicated, Mahoney further distinguishes levels of the intensity of a given type of colonialism and whether an area was at the core or the periphery of colonial practice in a given time and space.

This general model of colonialism and its economic and social consequences emerged from reflecting on the unusual pattern of the long-run impact of Spanish colonialism. It guided a profoundly knowledgeable analysis of the different areas and countries of Spanish America. And it gained in its sophistication from making sense of this complex historical evidence. However, while Mahoney also looks for and discusses features in the trajectory of different areas that require explanation outside his theoretical model, he is content that the overall pattern of postcolonial socioeconomic outcomes is consistent with his theoretical model, even though he does not deal with all postcolonial developments that might independently have shaped these outcomes as well. Given the historical complexity of Spanish colonialism, the broad range of his comparative analysis, and the innovative use of historical institutionalism for explaining path dependent outcomes, this is an outstanding contribution to the study of colonialism and its consequences.

IV.

Finally, I turn to research on democratization. Democracy is another aspect of the organization of power, one that came into its own only during the last three or four generations. In recent decades, research on democratization has zeroed in on a causal understanding of its conditions and of the forces deepening and stabilizing it.4 Here I offer only a few remarks on two publications that above all illustrate recent developments in the mode of inquiry, though they also present interesting substantive findings.

The first is a programmatic collection of papers, The Historical Turn in Democratization Studies, edited by Giovanni Capoccia and Daniel Ziblatt (2010). Building on earlier

4 See Rueschemeyer (2010) and two other papers in the same newsletter of the American Political Science Association’s Section on Comparative Democratization.
and current work by such authors as Thomas Ertman (1998), Daniel Ziblatt (2006), and Kurt Weyland (2007), Capoccia and Ziblatt propose that the emergence of democracy in Europe was not a single unitary process but rather a set of specific institutional developments that responded to different critical constellations. Much can be learned by detailed reexaminations—for instance about the role of political parties—that will be of value in studying different aspects and outcomes of democratization in contemporary non-European countries. What is remarkable is the fact that historical investigation and reinvestigation are presented as contributing to broadly applicable causal explanation.

The second publication is Thad Dunning’s (2008) treatment of the so-called “resource curse,” the fact that a country’s disproportionate reliance on natural resources and their export often has negative consequences for democracy. Often, but not always. *Crude Democracy* reviews the statistical cross-national evidence and finds good support for the claim that reliance on natural resources can also support democratic rule. Dunning’s analysis stands out for its multi-method approach to theory-oriented research. He combines game theoretic modeling, large cross-national regressions, and pattern tracing in five cases: Venezuela, Chile, Bolivia, Ecuador, and Botswana. These case studies seek to identify democracy-enhancing mechanisms. Their results are in turn integrated in his rational choice analysis.

I am happy to note that these two publications practice what is praised and advocated as an important trend in comparative social science in a recent publication by Ira Katznelson and Barry K. Weingast, *Preferences and Situations* (2005). In their introduction, the editors argue for a mutual rapprochement between historical and rational choice institutionalism.

V.

It will surprise nobody that the outstanding comparative historical studies briefly described here do not, as an ensemble, constitute one grand advance in our understanding of the role of power and states in human history. After all, they pursue quite diverse questions about these broad issues and are relatively eclectic in their theoretical presuppositions. Their results do on occasion link up and help us understand a little better how, say, bureaucratic state structures come about and how they affect economic well-being. But in concluding, I will not review such partial linkages and gains in substantive understanding. I will instead point to just a few analytic tools that proved useful in these studies and that can be put to good use in other social and political research.

Analyses at the meso-level often play a critical role in understanding macro-structures and macro-change. Barkey makes this point about her elite analyses in the center/periphery relations of the Ottoman Empire. Lachmann’s central hypothesis—that power constellations among different elite interests shaped the long and halting emergence of capitalism—is broadly related. And analyses of causal mechanisms shaping macro-outcomes often involve meso-level processes.

Of similar broad interest is the effective identification of path-dependent processes. At first sight, explanations via historical causes seem often surprising and perhaps even implausible, especially when the time distance between cause and outcome is substantial. Yet the research reviewed here offers quite a few convincing analyses of path-dependent development. In political science, Paul Pierson has given with his *Politics in Time* (2004) a new impulse to incorporate consideration of historic causes and path-dependencies.

Finally, among the most important features of culture and society that underlie long-lasting continuities in social life are institutions. Mahoney’s *Colonialism and Post-colonial Development* offers perhaps the most powerful recent example of this. Yet institutional analysis plays a strategically important role in all social and political research because institutions shape and constrain the extreme variability at the level of individual action, which has led some to give up on any scientific analysis of social life. For this reason, institutional analyses may well be one of the most promising ways to approach greater scientific precision. It is perhaps because of the unifying effect of institutions on individual, small-scale, and meso-aspects of social life that different theoretical perspectives have converged to
focus on institutional analysis, as I have argued in *Usable Theory* (2009).

**References**


