



Comparative & Historical Sociology

Summer 2003

Newsletter of the ASA Comparative and Historical Sociology section

Volume 15, No. 2

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A Note from the Editor

Rosemary L. Hopcroft
Department of Sociology, UNC-Charlotte

I would like to introduce myself, as I know very few of the section members personally. However, many of you have reviewed my work in the past, and I would like to thank those of you who have anonymously given me so many helpful comments and advice over the years. Until now I have been rather inactive in the public life of the section, mostly because of the demands of teaching, publishing and two small children. However, now safely tenured, I feel I can begin to contribute more to the "civil society" of the section. My path into comparative and historical sociology has been a little unusual. I was trained as a demographer at the University of Washington, but late in my graduate school career became drawn to the richness of thought available in comparative and historical sociology. I wrote a comparative and historical dissertation under the supervision of Pierre van den Berghe. This has been the basis for a variety of articles and one book on agrarian (and social) change in European history. Recently, I have branched out in my own research, but I will always be a comparative and historical sociologist.

Given my own past work, I have been pleased to see the invigoration brought to "the rise of the west" topic by Richard Lachmann's recent book *Capitalists in Spite of Themselves* (and the discipline's subsequent recognition of the book through the award of the 2003 Distinguished Scholarly Publication Award of the ASA).

In this issue of the newsletter, Jack Goldstone offers provocative comments on the discussion of Lachmann's book contained in the last newsletter.

Richard Lachmann in turn offers a rejoinder. I look forward to Jack Goldstone's forthcoming book *The Happy Chance: The Rise of the West in Global Context, 1500-1850*. I heartily applaud both his and Lachmann's refusal to let economists (and others) have all the fun.

In addition, in this newsletter Diane Davis (MIT) discusses the fascinating work of Eiko Ikegami, our section chair. Mike Sobocinski, an independent scholar from Lansing, Michigan offers thoughts on future trends in the world system. Last, there is the line up for the comparative historical section sessions and activities at this year's meetings of the ASA in Atlanta. This year our Section Day is Sunday, August 17th. One further note about the upcoming meetings: In 2000, Charles Ragin, then the chair of the section of Comparative and Historical Sociology, proposed a tradition that the chair of the section gives a keynote speech at the section day of the ASA meeting. Margaret Somers, the former chair, gave the first such talk last year. This year, Eiko Ikegami, the current section chair, will give the keynote address: "Bringing Culture into Macro Structural Analysis in Historical Sociology."

Please continue to send me and the section web master references for your forthcoming and recent publications. I also invite any who would like to contribute to the newsletter to send me their contribution (no more than 2,000 words please – to rlhopcro@email.uncc.edu).

I look forward to seeing you all in Atlanta!

Many Boundaries to Cross: The Comparative-Historical Sociology of Eiko Ikegami

Diane E. Davis

Massachusetts Institute of Technology

The field of comparative-historical sociology has gone through considerable soul searching in recent years, especially as the world has changed and scholarly paradigms have tried to keep pace. It does not seem that long ago that Charles Tilly, in the pages of an early 1990s Comparative-Historical Section newsletter no less, suggested that globalization processes and the rise of network theory – because they challenged the conceptual utility of the nation-state as the primary unit of analysis – posed a potentially mortal blow to the field as we had come to know it. Since and even before then, a growing preoccupation with culture in a field that for years examined political or economic institutions and structures further shook our sub-disciplinary foundations, although the results have been only positive. Some of the most vibrant, creative and path breaking work in the comparative-historical field, exemplified in writings from scholars such as William Sewell, George Steinmetz, Ronald Aminzade, Margaret Somers, Julia Adams, Philip Gorski, and Mabel Berezin (with sincere apologies to the multitude of other worthy colleagues not duly noted), has come from the efforts to marry a concern with culture to such standard comparative-historical subjects as revolutions, states, classes, regime type, etc. It is in this tradition that I situate Eiko Ikegami's work.

To a certain extent, the sub-disciplinary boundary crossing that informs her scholarship and that of a growing number of other like-minded sociologists has become so common – if not conventional -- that one is almost shocked to see studies of states or revolutions or classes in which culture is not invoked in some way or another, even if there still may be controversy over what exactly culture means. In recent years, comparative-historical scholarship on state formation has especially flourished by taking the cultural turn,¹ edging out the preoccupation with

¹ George Steinmetz must be singled out and recognized as a leading proponent of this perspective, and his *State/Culture*

class formation that characterized the previous decade(s). Ikegami's first book, *The Taming of the Samurai: Honorific Individualism and the Making of Modern Japan*, published in 1995 (Harvard University Press), played a central role in establishing this shift in focus. Not only did *The Taming of the Samurai* put essential questions of the inter-relationship between culture and state formation squarely on the disciplinary map, it did so with a focus on identity formation in which class actors and class conflict were surprisingly absent. Much of this was due to the historical period she chose to study. Given her interest in understanding state formation in Tokugawa Japan, Ikegami initiated her study through examination of the medieval and late-medieval periods, long before the establishment and deepening of capitalist development and at a time when warriors were central protagonists in struggles between feudal lords and an ascendant (or what she calls "neo-feudal") nation-state. It was the focus on the samurai, however, that enabled her close attention to culture, primarily because questions of honor and trust were central to samurai identity formation. These attributes were not just intrinsically interesting from a cultural sociology perspective, then; they also linked the samurai to feudal lords – and later to the state – in ways that directly molded Japanese state formation.

To be sure, the book's originating preoccupation with the samurai made eminent sense even from a conventional comparative-historical perspective, as the link between war-making and state-making had long been a central concern in the sub-discipline. Introducing culture into this narrative was important but not necessarily revolutionary in any paradigmatic sense. What made Ikegami's take on this dynamic so rich, however, and what made her book so compelling and ground-breaking as a piece of historical scholarship, was the emphasis on the dialectic of culture and state formation (fueled by samurai identity) as constitutive of nation-building. That is, Ikegami attempted to do much more than introduce a cultural perspective into the study of the relationship between war and state-formation. By tracing the historical processes through which samurai culture produced a certain path of state formation, which in turn established the conditions

under which certain cultural orientations and practices linked to honor persisted in subsequent periods, she also provided an account of the making of modern Japan as a nation and as a "culture," if you will.

It was the latter objectives that served as Ikegami's passport to true boundary-crossing prominence. After all, it was one thing to insert an appreciation of culture into political processes and structures; it was quite another to understand the dynamic, historical interaction between culture, state, and nation. Still, it was not even this aim that helped Ikegami make her mark so much as the fact that in order to do so she developed an approach to culture that itself was inclusive and far-reaching enough to appeal to scholars whose work stood outside or on the margins of the most popular comparative-historical sociology of her times. This was clear in her effort to understand samurai identity formation through the lens of honor and trust, concepts that more traditionally found elective affinity in scholarship focused on the development and social integrative function of norms and values. Put another way, *The Taming of the Samurai* was a book that found elective affinity with the aims of scholars such as Robert Bellah as much as Charles Tilly and Michael Mann (and did so by way of Reinhard Bendix). In this sense, Ikegami's boundary crossing can be seen as theoretical (by marrying new and "old" perspectives in comparative-historical sociology) as much as it was disciplinary (i.e. bringing culture into state formation).

In the years since *The Taming of the Samurai* was published, Ikegami has continued her quest to marry different sub-disciplines while also deepening and extending her appreciation of culture and its role in the "making of modern Japan." Starting in the late 1990s, she turned her attention to aesthetics and fashion, and further cemented her place as a creative scholar of culture in its own right. Her 1997 article in *Political Power and Social Theory*, entitled "Protest from the Floating World: Fashion, State and Category Formation in Early Modern Japan," linked the rise of a certain aesthetic sensibility in fashion (as manifest in the use of color and texture) to social movements and thus identity formation, and then theorized this dynamic as central to Japanese state formation, thereby taking her samurai view of the latter in new directions. In article form, her argument

volume stands as a seminal marker leading the development of this subfield of inquiry.

offered a bold new perspective on identity and social movements that was both structural -- at least in terms of its focus on the political economy of artistic production -- and agency-centered, by virtue of its focus on individuals' appropriation of the artistic aesthetic. Yet it did so while also keeping one foot firmly planted in the more conventional work on state formation, by virtue of the focus on social movements (and protest) in the development of aesthetic identities and practices.

Unlike many scholars who continue to deepen their interest in culture and politics, Ikegami also continued to develop an appreciation for the economy and its role in state and cultural formation. This was evident not only in her analysis of the political economy of cultural production in "Protest from the Floating World;" it also was a clear in an article published earlier this year entitled, "Military Mobilization and the Transformation of Property Relations: Wars that Defined the Japanese Style of Capitalism."² In this article Ikegami's concern with the relations between war-making and state-making persists, but is marshaled in the service of understanding a key element of economic sociology -- property relations -- and how this in turn contributes to Japan's unique form of capitalism. In all her work, in fact, Ikegami shows a careful and nuanced appreciation of economy and capitalist dynamics, whether they are posed in the study of art worlds or cities as hosts for these worlds. By using a political economy sensibility to link culture and state formation to other large structures and processes, like urbanization and the rise of arts institutions, Ikegami is able to perform the impressive feat of multiple boundary-crossings. And perhaps her most ambitious work in these regards is her latest, tentatively entitled *Civility and Aesthetic Publics: The Political Origins of Japanese Culture* (Cambridge University Press, in production). This book, which is the second of a projected three volume work on the making of modern Japan, brings together the threads of previous scholarship but weaves them into luxurious new cloth that finds its texture in the language of civility and publics and its shape in the form of network theory. The shadows of Norbert Elias, Harrison

White, and Jürgen Habermas hang over this work, and by so doing strengthen its boundary-crossing potential in entirely new ways.

Ikegami has the uncanny ability to appropriate the latest "ideas in good currency" but to recast if not subvert them in the service of exposing their historicity. In *Civility and Aesthetic Publics* this is manifest in her emphasis on civility rather than civil society, which is presented in a chapter entitled "Civility Without Civil Society." Her logic draws from deep historical understanding of the limits inherent in the use of ideas, concepts, and theories drawn from the "West" -- that is, from European experiences with state formation, democracy, and identity formation -- in the study of non-western societies like Japan. She argues that although in western democratic theory, civil society "implies a domain of private citizens that has a certain degree of autonomy from the state," such that "the empowerment of civil society has been closely associated with the historical development of modern democracy," this clearly was not the case in Japan. But still, this does not mean that certain dimensions of these developments were entirely absent, or even that the early experience of the "West" is not somehow sociologically instructive for the study of the "East." Specifically, Ikegami suggests that despite the fact that in Tokugawa Japan the firm hand of the neo-feudal state "prevented the development of a civic associational domain that fit the Western notion of civil society," it did experience a form of civility -- which she defines as the "cultural grammar of sociability that governs interactional public spaces," a "civility without civil society" if you will -- that itself set the contours of Tokugawa cultural development and thus the making of modern Japan.

Clearly, this is an accounting of historical developments that uses as its spring board several leading theories or ideas about state formation and the role of civil society drawn from the West, but that reshuffles, critiques, and reformulates them (often by proposing new concepts and categories) in ways that both acknowledge and transcend their historicity. It is a form of boundary crossing that is relatively exceptional, moreover, not to mention innovative, compelling, and highly provocative especially given the weight given the Western experience in the corpus of works comprising the sub-discipline of

² This chapter appeared in Diane E. Davis and Anthony W. Pereira (eds.) *Irregular Armed Forces and their Role in Politics and State Formation*. New York: Cambridge University Press, 2003.

comparative-historical sociology. And as if that were not enough, the book further widens Ikegami's already mind-boggling cultural reach by focusing on infrequently studied cultural forms like poetry (developed in a chapter aptly entitled "The Haikai, Network Poetry: Border Crossing and Subversion), which is seen as an art form that expresses ("western" notions like) freedom as well as protest and other forms of communication that eventually lead to the development of Japanese national identities.

As comparative-historical sociologist with an interest in a non-European country, I find the boundary-crossing embodied in this latest book -- in which time and developmental experience are trespassed to a certain extent -- to be the most imaginative and original. The question of what to make of theories and concepts drawn from a particular historical experience when applied to another, and the challenge of using them without being constrained by the historicity from which they emerge, still has not been adequately addressed in our field. This may be so even in comparative-historical work on the usual Western subjects, European and American countries. After all, is it really possible to analytically separate time, or better said "historical moment," from the sum total of a nation's cultural, political, and economic experience? But if not, how can we make sociological observations that sustain some form of general claim-making? Another way to pose this question is to think about the larger rubrics that have developed to account for what we have come to theorize as distinct experiences -- East versus West, feudal versus early modern, early modern versus modern, early developers versus late developers, and so forth. Should these categories hold, such that we need entirely different theories and concepts to analyze different times and places; and/or would it depend on what we are examining? Conversely, should we look for universal structures and processes that transcend these categories; and if so, could this be done without making the errors of modernization theorists or so many of our predecessors whose so-called universalism was merely a proxy for the Western (and often, privileged white male) experience? Or should we take the Ikegami approach, and read these experiences off each other without seeking either complete replicability or distinctiveness? The work

offered by our new section president could serve as a starting point for beginning such a dialogue. Where we end up is hard to know, to be sure. But with the great variety of countries, time periods, and subjects studied by our section membership already, why not engage in such an enterprise? I for one look forward to the process, no matter the outcome.

Lachmann, Capitalism, and the Rise of the West: A Comment.

Jack A. Goldstone, *University of California, Davis
and George Mason University*

I am happy to congratulate Richard Lachmann on winning the ASA Distinguished Scholarly Publication Award for *Capitalists in Spite of Themselves* (Oxford 2000). Although his arguments are clearly controversial, it is a measure of the value of this book that it evokes such detailed and thoughtful reflections on social change in late premodern Europe as appeared in the last issue of the CHS Newsletter. I think we should call Richard's award a victory for comparative historical sociology, and be delighted for Richard and all of us in the field.

However, I also have some bones to pick with Richard's argument. Building in part on the previous comments, I wish to take the debate even further afield, introducing some new issues and broader comparisons.

On Capitalism and the Rise of the West

I think Julia Adams is quite right that despite its attacks on Marxism, Lachmann's book itself is built around a basic, long-established Marxist theme, or at least a Barrington Moore theme -- that the key turning point in the transition from pre-modern to modern life is the establishment of English commercial farming under the domination of the English elite. Lachmann argues that in England, uniquely in Europe, capitalist landowners became the dominant political elite. Establishing conditions for capitalism to flourish, first in agriculture and then more broadly, they paved the way for the transformation of economic life. Lachmann's main contribution is to point out that they did so not through intentions to establish capitalism as such, and not mainly through the Revolutions of 1640 and 1688, but largely "in spite of themselves," and centuries earlier through their efforts to gain and maintain wealth and status at the expense of the pre-Reformation clerical elite.

However, I believe the argument that the advent and triumph of capitalist farming in England marks a key event in the rise of modern economies, or even

the modernization of England, is mistaken, on two counts.

First, I think it is too facile to assume that because any country's elite starts farming for profit and selling goods that it will, within a few hundred years, invent electricity, automobiles, jet engines, or nuclear reactors. Especially since elites in India and China, as we now know, were engaged in just the same kind of renting land for profit and long-distance large-volume commodity trade as European merchants, only on a much larger scale and for a much longer time. This notion that once capitalism develops in one sphere (whether agriculture or merchandizing) it will inevitably spread and transform production through automatic technical innovation is an old confusion, present not only in Marxist accounts of the transition from feudalism to capitalism, but also in Weber's treatment of rationality and efficiency in the "spirit of capitalism." This idea is rooted in the contrast between a dynamic "capitalism" and a technologically and productively stagnant "pre-capitalism." Yet we now know that non-European economies were not static, but achieved considerable gains in productivity, trade, and technology in the pre-modern era. I think one cannot avoid asking why, of several distinct regions in Europe and Asia that developed highly productive, market-oriented agriculture, mainly Britain, Holland, and China, only Britain spontaneously developed a modern industrial economy. I believe that if you really want to explain *modern* capitalism, you have to talk about science and technology, and the cultural context that led to its specific development in certain directions in Europe.

Second, the notion that Britain had a single, unified, elite who transformed agricultural AND merchant AND industrial capitalism is just as wrong. The landed elite in the House of Lords, Commons, JPs, the Anglican Church and the state leadership who triumphed in the Reformation were not all capitalists. Most of them, in fact, let their lands out at a very non-capitalist leasing for multiple lives or 99 years at *fixed* rents. This was not only a lousy way to seek a profit, it was a recipe for failure in the years of rapid inflation from 1500 to 1650. By 1650, the gentry elite had tripled in size, and most of the additions were individuals who had indeed gained by capitalist use of land, but were drawn from the

independent farmers and yeomanry in regions where communal lands and strong manorial control had *never* been important (precisely as Rosemary Hopcroft insists). Indeed the "triparte" structure of large landlord, commercial farmer, and wage laborer was built more from the bottom up, with yeomen becoming larger landholders, through buying and renting more land, and employing more labor, than from the top down by large landlords dispossessing small farmers. The very high up-and-down social mobility of the years from 1500 to 1650 strongly argues that the gentry elite of 1650 and of 1500 were NOT simply the same class enlarged. Indeed the struggles of the British Civil Wars were not merely commercial landed elites vs. an aggressive state (as Lachmann insists analysts of these events treat them), but struggles within an elite that was itself deeply divided at every level over issues of state power, religion, and the importance of economic precedence. That's certainly how I treated the revolution in my book (Goldstone 1991), whose argument regarding the role of intra-elite conflict in the events of 1640-1660 Richard declines to mention.

But even putting aside the divisions in the landed elite, the *merchant* elite was something else altogether. These elites -- part allied with the Crown, engaged mainly in international trade, and receiving royal favor and contracts; part Puritan and engaged mainly in colonial and domestic trade and manufacture, intersected in a variety of ways with different segments of the landed elites of the countryside. Indeed, if not for the city of London, it is likely that the conservative landed interest allied with the Crown would have won the Civil Wars, instead of the Puritan landed interest in conjunction with urban elites and the rural sub-elites. Richard notes this, but suggests that English elites only underwent a temporary split in the 1640s due to the popular uprisings in London. It's certainly true that the uprisings in London and Ireland precipitated a split in the gentry, but the cleavages were already there, reflecting intra-elite cultural, political, and social competition.

What happened in the 1640s and 1680s was the increased assertiveness and eventual triumph of commercial trading elites in conjunction with Puritan and Dissenting landowners, who together opposed the expansion of Royal and Anglican Church authority and French and Catholic influence, and

sought more aggressive foreign policies to promote overseas trade. They triumphed over a more conservative faction of the elite more interested in maintaining the authority of King and Church over all aspects of society, and more inclined to tolerate Catholics in the elite and as foreign allies. So it's a real oddity to say the revolutions of 1640 and even 1688 simply ratified the dominance of the "same" capitalist elite who benefited from the Reformation. In other words, I think Richard is certainly right about the importance of elite competition as key to social change; but there was more intra-elite conflict involved in the events of the 1640s and 1680s than he seems to allow.

Finally, the industrial elite was yet another group again, drawn mainly from Dissenting merchants and middle class urban families, and quite often seriously at odds with the landed elite (over the Corn Laws, for example, and voting rights in the 1830s). Part of the reason that this group became capitalists was not "in spite of themselves," but precisely because that was the only avenue open to them. As a result of the complex outcome of 1689, certain kinds of non-Anglicans were secured in their property and legal rights, but excluded from positions in the state and church and military. So they often chose business. Insofar as Britain had the only state in which this odd cultural/economic compromise persisted (even Holland's toleration caved in as the Dutch Reformed Church gained power and exerted its monopoly after 1700), with a large minority of economically powerful elites excluded by law from the political sphere, we have an important factor that again disappears in Lachmann's "single elite."

If I start to sound like a "culturalist" I guess I'm becoming one. Better late than never, no?

On Elite Actions and State Power

Here Samuel Clark and Rosemary Hopcroft make terrific points, that I would amplify even further. Hopcroft observes that the main impetus to capitalist farming in the 16th and 17th centuries was coming mainly from small farmers in the old-enclosed areas of England, not the dominant landed elite. And Clark points out that the politically dominant elite was in fact the peerage, as reconstituted after the Civil Wars, not the middling gentry or the MPs in the Common, as the entire political elite was deeply tied to the Peerage by webs of patronage. But Rosemary

then asks why this politically dominant landlord class didn't simply set things up for them to enjoy life as a 'rentier' class, instead of pursuing agrarian capitalism. I would argue that the dominant political elite in England *was* precisely a rentier class. So I'd say Rosemary does not go far enough! If we ask why the landed gentry who gained so much from the dissolution of the monasteries didn't just secure their position using their power -- I think they did!

In fact, the gentry in 1500 was very tiny, no more than 5,000 families, and so even the relatively small British state structure did allow them to secure their position. Until challenged by upcoming commercial farmers in the very late 16th and early 17th centuries, they wholly dominated the positions of MP, JP, and military office. They owned huge estates (wealth was incredibly concentrated in England, more so than in France, because of both the dissolution of the monasteries and then the sale of Crown lands), not only in England, but also in Ireland and Scotland, where it was precisely the extension of English state power that was used to secure sinecures for the English elites. The independent political power of magnates in their territories may have been lost by the late 16th century, with JPs and MPs dominating local politics. But the great lords continued to dominate central government, through their wealth and patronage in Parliament, and their dominance of the King's council. Given the degree to which members of the Commons were relations or dependents of peers, or future peers themselves, it is anachronistic to draw too bold a line between peers and "commoners" in the lower chamber. Kudos to Sam for pointing this out!

It should have been clear that in the late 16th and early 17th centuries, when the English elites were sending their children to the universities in record, indeed exploding numbers, they were not being sent to get MBA's. They were getting clerical, law, physician, and bachelors' degrees, leading to jobs in the professions -- again precisely so they could sustain their positions without having to go into business.

If we ask *who* were the English capitalists, it was not these large landowners, but the commercial farmers to whom they leased their estates, and the independent yeomen and middling farmers whose farms far outnumbered (in quantity, if not acreage)

the holdings of gentry elites. It was also the urban merchants, who were distinct and often opposed to the landholding elites, and later the dissenting industrialist/entrepreneurs, who likewise were distinct and often opposed.

Richard responds to the above critique by saying that the critical element of his argument is that it was the post-Reformation elite who established the tripartite landlord/commercial farmer/wage laborer relationship, and that this relationship then survived into modern times (and presumably was key to ushering in modern times). So it didn't matter that the landlords came to fill the rentier/landlord role in this system; it was the structure itself that determined the outcome.

The problem with this view is that this structure in fact didn't do very much for English agriculture, as opposed to English landlords! Recent studies of labor and land productivity (Overton 1996, Hoffman 1996) suggest that England was no more productive in the 18th century than the equally commercialized farms of France's Seine valley or Flanders. It's true that France had larger areas of less productive farming in the south and west, like the British crown had in Scotland and Ireland. But if you compare likes with likes -- e.g. commercial core areas, not outlying areas under Crown control -- you find that yield per acre and per laborer were very similar in the core areas of England and France. For that matter, peak yields per acre in 18th century England didn't much exceed peak yields in 13th century England (Campbell 1983) -- higher yields were just more widespread in the later period. By the late 18th century, yields per laborer were getting higher, but this was largely because the tripartite structure reduced labor on farms, securing a higher portion of the product for farmers and landlords. But this did not boost output for the population as a whole. In fact the productivity of English agriculture, compared to the growth of English population as a whole, was negative after 1750, and incomes were falling (Overton 1986).

Again, Rosemary's claim that landlords did not use state power to enrich themselves is probably too mild. After 1750, when the tripartite system had already been built up from below mainly by the mobility of property and families, and the adoption of Dutch farm technology had rendered certain patterns of landholding more valuable and desirable

for elites, English agriculture *was* restructured by the elite (through the Enclosure acts) to improve their share of output. But this did not improve productivity any further. Almost all the productivity gains were made prior to 1750 (Allen 1999; Beckett, Turner, and Afton 2001). Yet after 1750, population continued to grow and agriculture failed to respond. When industrial capitalism arose in the 19th century, it did so not on the back of a uniquely productive agriculture, but in the teeth of declining agricultural output per capita of population.

To really put nails in the coffin and bury the notion of the exceptional productivity of English agriculture, I have new data, drawing on the research of Li Bozhong (1998), showing that China underwent an "agricultural revolution" of its own in the 17th century, pushing up yields both per acre and per laborer, and that as late as 1800, by any measure of productivity, Chinese agriculture was still performing as well or better than that of England (recently corroborated by independent estimates made by Robert Allen, 2003). In the 18th century, Southeast England and Holland indeed had agricultural productivity that was exceptional compared to other European nations. But Southeast England was not an entire Kingdom – the UK included Scotland and Ireland and Wales, regions of much lower agrarian productivity and income than England. At other times, other parts of the world – such as Sicily and Egypt in classical times – had such high agricultural productivity per capita that they served as breadbaskets for even larger regions. England's peak productivity in agriculture in 1800 was not something new and unprecedented, but the return to cyclical peaks reached in parts of Asia, in Europe in the Middle Ages, and even in classical times (Grantham 1999). This work is forthcoming in papers and a new book, so it's new (for a preview please see Goldstone 2002). But it suggests that the value of the 'triparte' system for English productivity is greatly exaggerated when placed in too narrow a comparative context, and its real impact may simply have been to secure a larger share of output for the landed elites.

The English and French States – How did they Differ?

I have to wonder that Samuel Clark has found so useful Lachmann's distinction between vertical and

horizontal elites. Was the English state really so decentralized? Maybe under Elizabeth and in the early 1600s. But after Oliver Cromwell -- who put the entire country directly under military rule from London through major-generals, something never even attempted in France -- English government was far more centralized than in France prior to the French Revolution. The tax/excise men formed a huge bureaucracy that reached everywhere in Britain, and England taxed windows, cards, stamps, and almost everything else that could be enumerated. Indeed, indirect taxation far outpaced direct taxation on the land after 1700, and the central government took a far larger percentage of national income than did the French Crown. It may be true that this centralized regime was held in check by local interests through Parliament, but those local interests had very little local independent power to challenge the decisions of the central government made by King in Parliament. There was nothing in the way of important and independent local government, as at least survived with the Estates and *Parlements* of France until 1789.

By contrast, although Louis XIV sought to undermine the complete independence of the provincial estates and *parlements* by the system of *intendants* and royal patronage, it remained a dual system of provincial governors and estates alongside *élus* and *intendants*. Louis' absolutism was so limited by the various liberties, privileges, and rights associated with various towns, provinces, and status groups that the state was perpetually short of funds, overtaxed its peasantry (the only group unable to secure exemptions), and later suffered a revolution at the hands of its elites when it tried to really jack up taxation to British levels.

I'm not saying that there were no differences between English and French states -- just that the states and their differences changed a lot over time, and that absolutism vs. decentralization is not always the right contrast. From 1500 to 1650, *both* France and England were rather decentralized, and absolutism was weak. From 1660 to 1789, *both* France and England were rather centralized, and the authority of the centralized state was stronger, and indeed considerably stronger in England.

Certainly after 1689, the King of England had rather less latitude than the King of France, and even less after 1714 when foreign princes from Hanover

were brought in to rule. But to mistake the "King" alone for the "state," and to confuse the discretion of the King with the power of the central government, is an error that I hope most of us will usually avoid. I fear that what happens all too often in comparisons of England and France is that the Tudor state of the late 16th century is taken as "typical" England and contrasted with the Sun King's regime of the late 17th century as "typical" France. This kind of selective comparison is misleading at best, as it suggests that "France" was generally more centralized and absolutist than "England," a statement that is simply false unless heavily qualified as to specific periods and institutions.

Of Straw Men

I have to agree with Edgar Kiser that many of Lachmann's strongest blows seem to be aimed at straw adversaries. Lachmann certainly shows that class conflict is not the motor of history; and that even rational self-interested action often leads to unintended outcomes. But for many decades, which important thinkers have held otherwise? Perhaps I am just sensitive to how arguments are made in my own field: revolutions. On p. 232. Lachmann chides theorists of revolution, telling us that "the comparative study of revolutions will stagnate (and it will continue to misinterpret the structural import of recent historical studies of specific revolutions) as long as the Marxist strawman of class war is challenged only by state-centered theorists who counter by viewing five hundred plus years of European history as a struggle between state and civil society and revolutions as victories for one side and the other." I can't tell whom he's talking about here; certainly neither Theda Skocpol nor I use the term "civil society" as a protagonist. Theda talks about marginal elites as well as states and elites; and much of my work stresses the role of social mobility, elite recomposition, and intra-elite conflict in causing revolutions. Maybe the "500 years" is taking on Charles Tilly's (1993) book on European revolutions from 1492 to 1992; OK, but Tilly's story is more about the growth of state power over time than a causal theory of revolutions, and of course his emphasis on state vs. society is just one view.

Finally, Agreement

Let me close on a note of agreement (and, OK, an advertisement). Richard closes his response in the CHS Newsletter by saying that "The riddles of how social relations produce historical change need to be addressed, ultimately, on structural, ideological, and epistemological levels." I couldn't agree more! In fact, I have a new book almost out (I hope next year), entitled *The Happy Chance: The Rise of the West in Global Context, 1500-1850*. It argues that structurally, the differences among European and Asian nations didn't amount to much in regard to economic progress. All kinds of different class and political systems were, at different times, capable of improvements in productivity and welfare. Circa 1750, levels of consumption, trade, agricultural productivity, and population growth were quite similar in the core commercial areas of many advanced preindustrial societies -- England, France, Holland, China. In fact all of these areas had enjoyed various intervals of growth and decline in a cycle of long waves going back for centuries.

What created a disjuncture c. 1850 in the West was an ideological/ epistemological shift. In Britain, an empirical and instrumental science produced an understanding of vacuums and steam technology that was unique. Harnessed to a Newtonian/Baconian ideology, Britain created integrated networks of engineers, natural philosophers, and entrepreneurs who radically changed production technology, bursting through pre-industrial barriers on the use of energy and materials. It was the ideological shift, in part brought by a critique of Aristotelian philosophy on the basis of Arabic science all across Europe, and in part a unique British result of the Anglican/Dissenting stalemate and settlement of 1689 and specific trends in British empirical science and technology, that produced the radical break. To sum it up in a formula, it was new conditions facilitating systemic innovation through scientific engineering, and NOT higher accumulations of wealth from agriculture, imperialism, or any other source, that produced modern industrial society (for previews of this argument, see Goldstone 1998 and 2002).

Obviously, I agree with Richard that intra-elite conflicts, transformations of state-elite relationships, and long-term change and comparisons all need to be understood if we are to make sense of the long and

complex journey from pre-industrial to modern industrial economy and society. Thus I find much to admire in his work. Yet, however unfair it may seem, I think we must demand more – that the story is grievously incomplete without explicit attention to the development of modern science and technology, to the interactions of cultural frameworks and political power, and to broader comparisons with regions outside of Europe. I hope to provide some movement in that direction in my future work – which no doubt will be taken apart by critics as well. Perhaps the lesson is that whatever our hopes to settle such issues, we do well simply to stimulate as much controversy and fruitful discussion as Richard's claims have done, in spite of themselves.

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A Response to Goldstone

Richard Lachmann, SUNY-Albany

Jack Goldstone's important comments raise the key questions: What is capitalism, and what forces create capitalism? Goldstone is correct that I adopt a Marxist definition of capitalism that emphasizes the exploitation of wage labor. My book was written to address the classic Marxist problem of the transition from feudalism to capitalism, although my explanation highlights decidedly non-Marxist elite dynamics as the primary motor of historical change. My focus on English agriculture stems from my finding that that was the sector and the country in which wage labor, as Marx described it, first became dominant. The gentry created capitalism, but not (as Goldstone again rightly points out) because most gentry were themselves capitalists. Rather, the gentry created an institutional framework designed to guard their economic and political interests that had the unintended effect of facilitating the establishment of capitalist property and labor relations in agriculture (mainly by commercial farmers of yeomen origins). That structure also allowed the extension of capitalist social relations by colonial-interloper merchants and an urban middle

class into the commercial and, eventually, the industrial realms. I do not assume that farming for profit automatically leads to “electricity, automobiles, jet engines, or nuclear reactors.” I am not sure what conditions allowed for the European flowering of science and technology, and I don’t think sociologists have yet provided a satisfactory explanation. That is why I join many other historical sociologists in eagerly awaiting Jack Goldstone’s new book on that topic.

My elite model provides a foundation for understanding the emergence of full-fledged industrial capitalism marked by continued technological innovation by identifying the structural conditions that allow elites to redeploy productive assets without undermining their social positions. (Let me join Jack in a bit of self-advertisement: I expand this argument in “Elite Self-Interest and Economic Decline in Early Modern Europe,” forthcoming in *American Sociological Review*, June, 2003.) English commercial farmers and merchants differed from their Dutch counterparts or the Renaissance Florentine “new men” in that they were able to redeploy capital into new lines of investment and to transform the organization of production without thereby opening themselves to new challenges from rivals.

I do not offer full-fledged comparisons with India or China in my book. I argue, in a two-page Brief Note on the Asiatic Mode of Production (pp. 39-40), that neither the Marxist concern with centralized corvée labor nor the Weberian essentialist depiction of Asian stagnation are helpful in identifying the actual dynamics of Asian history. Instead, we need to ask whether the complex of elite and class relations created openings for transformative conflicts in each country, or city or locale, at particular historical moments. Without such openings, market-oriented agriculture and long-distanced trade did not give rise to industrial capitalism in India or China, just as the agrarian and commercial sectors of Florence and the Netherlands also failed, in distinctly different ways, to become original sites of industrial capitalism.

The importance of the structure of elite relations in shaping the opportunities open to future, elite and non-elite, actors is demonstrated by Goldstone’s observation that English agriculture was not noticeably more productive

than some parts of France or Holland (or, as we now learn from Goldstone, even China). England’s advantage came precisely in the capacity of its growing corps of gentry and commercial farmers to appropriate an increased share of national output and to deploy that output in commercial enterprises without endangering their political positions. That advantage stemmed in large part from the way in which landowners and merchants were represented, and their interests received institutional protections, within the state. It is not an issue of whether the English or French state was more centralized, as I tried to convey with my contrast of English horizontal and French vertical absolutism. Rather, the ways in which each state appropriated revenues, and the capacities of locally based officeholders to hold or spend some of those revenues without central oversight, reflected the capacities of each elite to institutionalize their interests in the fiscal organizations of their states. The overall level of tax revenues is less important for understanding the power of kings or other state elites than is the identity of who controlled those revenues. In France much revenue stayed in the hands of provincial elites while English kings were able to draw a far larger share of revenues to the Exchequer. Yet French provincial autonomy was undermined by each elite’s need to win crown recognition of its claims from challenges by rival elites. English “royal” revenues were spent under the supervision of a Parliament in which magnate-headed political parties needed to respond to gentry and merchant (and later manufacturing and middle class) interests as they competed with each other for national and county dominance. Tax collection and revenue allocation systems were artifacts of previous elite and class conflicts.

Similarly, the productivity of English agriculture mattered less for the future development of British industrial capitalism than did the identity and organizational bases of those who controlled the profits from farming. Goldstone is correct that there was much mobility into the English ruling elites in the sixteenth century. However those new commercial farmers entered elite positions, and assumed property rights, which had been defined in elite conflicts in the century from the Henrician Reformation to the English Civil War. That is what I

meant when I argued that the English Civil War ratified the results of previous elite conflicts.

If the English Civil War didn't change the balance of power or the institutional arrangements established in the previous decades, then how can we account for the intra-elite divisions which Goldstone (1991) and I, and numerous other analysts of this period, identified in our previous books. Goldstone is correct that a Marxist class analysis can't track political commitments in the Civil War. I agree, and that is what prompted me to attempt to analyze elite rather than class fractional differences in early modern European conflicts. The lines of alliance turned out to be more complex than I posited in *From Manor to Market* (1987). The Parliamentary bloc also was more (and less) than a middle class Puritan reaction to losers from below buttressing a gentry offensive against a grasping crown. The historians Goldstone (1991, p. 133) cites find support for Puritanism heaviest in particular ecological zones and among certain middle groups, and they are able to explain why people living under those conditions came to oppose the crown, but they can't explain how 'Puritan' ideology was translated into effective alliances that endured through the Civil War and molded the social structure of Britain in subsequent centuries. I built such an analysis on the work of Hill, Brenner, and Bearman who found that ideology was expressed in links of patronage and investment that endured because each participant tied their future prospects to joint religious, business and political ventures with their allies. As local networks were linked across counties, political actors were able to proceed through the shifting and dangerous terrain of the Civil War with the confidence that their allies were bound to them by long-standing ties, and that the allies of their allies were similarly bound.

My aim, in writing this study of the original development of capitalism, was to offer an explanation for both action *and* the lack of action. I wanted to explain why capitalism developed in varying ways in different parts of early modern Europe, but also why it hadn't developed earlier and why it was not an indigenous creation of most parts of the world. Knowledge about the qualities of actors and the resources of actors contributed to an explanation only when I was able to identify how those factors gain efficacy within specific institutional contexts. That is why the revenues

extracted by state actors, the information available to rational actors, and the future prospects of generations large and small had such different effects on the behavior and achievements of social groups in early modern Europe (and probably in other times and places as well). I found that the structure of elite relations is the most powerful predictor, and elite conflict the primary mechanism, of transformative social change. Ultimately, my model and its rivals will have to be evaluated by their capacity to identify and explain those rare moments when humans are successful in changing their social world.

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Future Trends in the World System?

Mike Sobocinski
Independent Scholar, Lansing, Michigan
SobocinM@michigan.gov

Immanuel Wallerstein's World Systems Theory sorts nations into core, semi-periphery, and periphery status based on the nature of their relationship to the expanding capitalist system. Some interesting tests and applications of this theory might be useful and appropriate as they relate to incidents of current international turmoil.

One interesting hypothesis concerns the proportion of the population that is involved in this global stratification system. Some rough calculations that I have done suggest that the percentage of persons in the semi-periphery, periphery, and external areas will of course shift over time as a general pattern of economic development occurs. Today, this means that there are really no longer any "external areas" as there were when the world-system started, centuries ago. The number and size of peripheral nations has also decreased, mainly as China has become semi-peripheral. But what I notice is that for the last decade or two, the core countries have accounted for roughly 10-15% of the world's population. I wonder if this proportion remains fairly stable as a capitalist system develops. The new nations that get added to the core (such as Ireland or Spain, as the EU has continued to expand) seem to be only those for which there is "room" in which they can be accommodated within that 10-15% ratio. If this is indeed the case, then forecasts of global economic development would have to take this into account. A nation like Brazil, later Argentina, (and now China?) that has enjoyed "miraculous" growth rates can rise from peripheral to semi-peripheral status under favored conditions, but then stagnates or suffers economic problems that may seem to hinder its further growth indefinitely. The exceptions seem to be those that are small enough to be accommodated in the core (Taiwan, for example) or those that throw about their power to demand an exalted status. The World War II axis is now in the core, and it appears that parts of the old Soviet Bloc will eventually gain core status as well. A consideration of global stratification structure (and

population proportions within each world system classification) might be revealing when studying which countries are successfully able to achieve upward mobility in this system over time.

It also raises questions about concerns some have expressed about global capitalism being (again) in a state of crisis. Since capitalism needs to expand, the nature of this expansion logically needs to shift as external areas, and even peripheral areas, continue to disappear and become proletarianized. Once all populations are proletarianized, the world-system itself will doubtlessly change and new forms of expansion may necessitate a reformulation of the categories into which nations can still be sorted. The 10-15% ratio of persons in core nations is interesting in that it conveniently parallels the 10-15% ruling class and professional class proportions that are typical in so many countries. If hegemony is now being established by core nations over the semi-peripheral or peripheral West Asian area (or at least the suppression of anti-systemic movements) and China and India continue to be nurtured into at least semi-peripheral status, then a new period in which capitalist hegemony is effectively unchallenged may arise, and new divisions form in the global system. The social classes that are still noted within countries would become truly globalized in a mature world-capitalist system. The various factors (economic, environmental, technological) that permit and encourage a global system to emerge also are appropriate to the original Marxian analysis of socialism emerging from a more advanced stage of capitalism.

When all the social classes recognize themselves as part of a global stratification system, the framework will be in place in which progressive movements (i.e. toward greater equality) would finally be able to shape the entire system, without the apparatus of "separate" states to help conceal and defend exploitation and injustice. When production has thus expanded, a few decades from now, the Marxian precondition for socialism will exist, in which all of people's basic needs can be met with very little work expended in a given day. If enough people are satisfied with leisure and "getting by" then perhaps many socialist conditions will be seen without a revolution or continued movements for greater equality. Equal access to health care might be the key concern 20 or 30 years hence, and if that

sector of the economy has been socialized, the rest might remain fully stratified, and of course one of the pressing social dilemmas will revolve around how people can achieve meaning and set goals for their lives in a world that no longer requires much work to allow their survival. The divisions between nations may become more rooted in culture than in economics.

A problem that we already see as possible in this new century is whether new profits (for an expanding capitalist system) will come through non-productive, contradictory sectors of the economy such as a "War on Drugs" or increasing surveillance and controls over the populace. If a global economy creates an expansion of a correspondingly global anti-terrorist system, one that requires continual monitoring of individuals, then we may discover that ideals of socialist utopias are merely a progressivist delusion. It seems to me that some of these questions can begin to be explored in advance, however. The current administration's continued use of centuries-old realpolitik doctrine definitely deserves to be questioned and its repercussions explored in light of historical and geopolitical theory.

New Publications, Awards and Announcements of Section Members

Julia Adams has just finished a working paper entitled "Social Theory, Modernity, and the Three Waves of Historical Sociology." The paper is available at http://www.russellsage.org/publications/working_papers.htm.

Kum-Kum Bhavnani, John Foran, and Priya A. Kurian, editors. *Feminist Futures: Re-imagining Women, Culture and Development*. London: Zed Press. 2003.

Mounira M. Charrad, University of Texas at Austin, received the following awards for her book, *States and Women's Rights: The Making of Postcolonial Tunisia, Algeria, and Morocco* (University of California Press, 2001): the Distinguished Contribution to Scholarship Award for the Outstanding Book in Political Sociology, American

Sociological Association, Political Sociology Section, 2002; the Hamilton Award for the Outstanding Book in Any Field, University of Texas at Austin, 2002; the Award (co-winner) for Best First Book in History, Phi Alpha Theta International Honor Society in History, 2002; Honorable Mention, Best Book in Sociology Komarovskiy Award, Eastern Sociological Society, 2003.

John Foran, editor. *The Future of Revolutions: Rethinking Radical Change in the Age of Globalization*. London: Zed Press. 2003.

Julian Go and Anne Foster (eds.). 2003. *The American Colonial State in the Philippines: Global Perspectives*. Durham: Duke University Press.

John R. Hall, UC Davis, gave the keynote address for Comparativists' Day at the UCLA Center for Comparative Social Analysis on January 31. He spoke on: "Beyond the great divides: history, the social sciences, and the humanities as a domain of integrated disparities."

Regina Hewitt and Pat Rogers, eds., *Orthodoxy and Heresy in Eighteenth-Century Society: Essays from the DeBartolo Conference Lewisburg, PA*: Bucknell UP; London: Associated UPs, 2002.

William A. Mirola, 2003. "Asking for Bread, Receiving A Stone: The Rise and Fall of Religious Ideologies in Chicago's Eight-Hour Movement." *Social Problems* 50(2):273-293.

Joel Stillerman. 2003. "Space, Strategies and Alliances in Mobilization: The 1960 Metalworkers' and Coal Miners' Strikes in Chile" *Mobilization: An International Journal* 8, 1 (February): 65-85.

John Torpey, editor, *Politics and the Past: On Repairing Historical Injustices* (Rowman & Littlefield, 2003).

Comparative and Historical Sociology Section Activities ASA 2003

Annual Meetings of the American Sociological
Association, Atlanta, August 15-18 2003.

Saturday 8/16 6:30pm-8:15pm Section on Comparative and Historical Sociology, Reception

Sunday, 8/17/2003 from 8:30 a.m. - 9:25 a.m. Section on Comparative and Historical Sociology Roundtables (one-hour)

Organizer(s): Paul D. McLean - Rutgers University

Table 1. Religion and Nationalism

Participant(s):

1. Ben Zablocki - Rutgers University (Presider)
2. Elif Andac - University of Washington (Presenter)
Title: Political and Economic Inclusiveness and
Islamic Opposition Movements: A Comparative
Look at Algeria, Iran and Turkey
3. Robert Michael Kunovich - Boston College
(Presenter) Title: Relative Group Size and Religious
Nationalism in Europe

Table 2. American Social Policy and Its Implementation

Participant(s):

1. Pamela S. Behan - University of Houston-
Downtown (Presenter) Title: The Politics of National
Health Insurance in the 'Liberal' Nations: A FS/QCA
Analysis
2. Susan Eachus - University of Pennsylvania
(Presenter) Title: Work First Implementation: Effects
of Agency and Worker Goals on Welfare Policies
3. Anthonette Andrea Rodriguez - Howard
University (Presenter) Title: Federal Child Labor
Policy in America 1912 to 2002: A Historical
Materialist View

Table 3. State Cultural Efforts and Local Resistance

Participant(s):

1. Paul D. McLean - Rutgers University (Presider)
2. Alexandra Hrycak - Reed College (Presenter)
Title: The Russian imperial state and the origins of
Ukrainian Theatre:

3. William K. Cummings - George Washington
University (Presenter) Title: Thinking about
Educational Revolutions and Reform

Sunday, 8/17/2003 from 9:30 am – 10.10 am Section on Comparative and Historical Sociology Business Meeting

Sunday, 8/17/2003 from 10:30 a.m. - 12:10 p.m. Section on Comparative and Historical Sociology Paper Session. **Integrating Non-Western Histories into Sociological Theories**

Organizer(s): Said Amir Arjomand -

J.I. Hans Bakker - University of Guelph

Participant(s):

1. Mounira Maya Charrad - University of Texas,
Austin (Presenter) Title: State, Islam, and Women's
Rights
2. Judit Bodnar - Central European University
(Presenter) Title: Uneasy Relations and Comparative
Strategies: Eastern Europe and the 'West'
3. Anna Da Silva - Rutgers University (Presenter)
Title: How Ideas Travel: Nihilist Reading of
Evolutionary Theory as a Case of a Palimpsest
4. Ho-Fung Hung - Johns Hopkins University
(Presenter) Title: Early Modernities and Contentious
Politics in China's Long Eighteenth Century

Sunday, 8/17/2003 from 12:30 p.m. - 2:10 p.m.

Section on Comparative and Historical Sociology
Session. **Section Chair's Keynote Address**

Session Description:

Organizer(s): John R. Hall - University of California,
Davis, Incoming Chair of the Section

Participant(s):

1. Viviana A. Zelizer - Princeton University
(Presider)
2. Eiko Ikegami - New School University
(Presenter), Chair of the Section Title: Keynote
Address: **Bringing Culture into Macro Structural
Analysis in Historical Sociology**
3. Richard G. Biernacki - University of California,
San Diego (Discussant)
4. Harrison C. White - Columbia University
(Discussant)
5. Viviana A. Zelizer - Princeton University
(Discussant)

Sunday, 8/17/2003 from 2:30 p.m. – 4.10 pm
 Section on Comparative and Historical Sociology
 Paper Session. **How History Shapes Events: Case Studies and Comparisons**

Organizer(s): William G. Roy - University of California, Los Angeles

Session Description:

How are events like 9/11, the French Revolution, the Assassination of Lincoln, or the Boxer Rebellion interpreted in consequential ways. It is not just that events make history, but that the historical process following events determine what consequences the events will have. Events become points of contention that actors use to pursue goals. This is patently clear with the "war on terrorism." What can we learn from events in other times and places to put the reverberations of 9/11 in historical context?

Participant(s):

1. James Mahoney –Brown University (Presenter) and Matthias vom Hau - Brown University Title: Indigenous People, Colonialism, and Social Development in Spanish America
2. Tom W. Smith - NORC (Presenter) Title: The Impact if the Cuban Missile Crisis on American Public Opinion
3. Virag Molnar - Princeton University (Presenter) Title: Tulips and Prefabrication: Modernist Architecture Bound by a Social Modernization Discourse in post-war Hungary
4. William G. Roy - University of California, Los Angeles (Presider)
5. Andrew Abbott - University of Chicago (Discussant)

also FYI

Saturday, 8/16/2003 from 10:30 a.m. - 12:10 p.m.
 Section on Political Sociology Invited Panel.
Explaining Politics: History, Culture, and Comparison

Organizer(s):

Ann Shola Orloff - Northwestern University

Participant(s):

Craig Calhoun - Social Science Research Council and New York University
 (Presider)

Elisabeth S. Clemens - University of Chicago
 (Panelist)

James Mahoney - Brown University (Panelist)

Ann Shola Orloff - Northwestern University
 (Panelist)

Jack A. Goldstone - University of California, Davis
 (Panelist)

Craig Calhoun - Social Science Research Council and New York University
 (Discussant)

Other Announcements

Introducing a new website, www.compass.org

It's a website describing a multiuniversity research center in Europe, dedicated to comparative analysis. Charles Ragin will be giving the inaugural lecture in September.

This will be followed immediately by the European Consortium for Political Research (ECPR) conference in Marburg, Germany, which will have more than 6 sessions dedicated to comparative methodology. There is a link to the ECPR conference on the compass website.

Comparative Historical Webpage

The webpage has the following: section information, awards & history, an online version of the newsletter, research tools, teaching aids, notices of institutes & meetings, a members area, a student center, a publications corner and an online library.

If you have a new publication or award you would like to have posted, please send it to the Webmaster (below).

Mathieu Deflem, Assistant Professor, University of South Carolina, is Webmaster:

<http://www.comphistsoc.org>