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Symposium on

Locked in Place: State-Building and Late Industrialization in India

Vivek Chibber

New York University

Princeton University Press
2003

Why did India, despite a democratic framework and a state commitment to economic growth, fail to reach the levels of economic development that South Korea reached in the 1950s and 1960s? Vivek Chibber's *Locked in Place* revives the comparative historical study of economic development and argues for the central role of capitalists in sending India's developmental state awry. In this issue Jeffery Paige, Elisabeth Clemens, and Leo Panitch examine Chibber's claims and Chibber responds.



Also in this issue: Arthur Stinchcombe, William Sewell, Jr., and Charles Tilly review each others' recent books; and Amy Kate Bailey, Rebecca Emigh, and Richard Lachmann reflect on why and how they entered Comparative Historical Sociology.

India and the Myth of the Anti-Developmental State

Jeffery Paige
University of Michigan

Vivek Chibber's *Locked in Place* is in many respects an exemplary work of comparative historical sociology fully deserving of the many awards it has received. There are a number of features that make this work stand out.

First, the book is based on extensive research on archival materials including both state documents and personal papers for the Indian post-colonial period. These materials have never before been exploited by either sociologists or historians. These primary sources are not simply deployed in

Challenges the conventional wisdom on all sides, addresses problems of fundamental theoretical and practical importance, and proposes novel solutions with broad application to the global South. This is what sociology should be at its best.

a historical narrative but used to develop and test generalizable sociological propositions. Chibber moves easily from the particular to the general (and back) even though a substantial literature in comparative historical sociology has denied that such a combination is desirable or even possible. Detailed descriptions of historical events and personalities are linked to theoretical propositions of the widest possible historical and comparative scope. Debates in Delhi in 1947-1951 illuminate the development state not only in India but in South Korea, Japan, post-war France, Latin America and indeed in the world of "late-late developers" generally.

Second, this is a genuinely comparative work in two senses. First there is an explicit comparison with the paradigmatic developmental state—

Korea. The Korean case is based on secondary materials but is considerably more than simply a shadow comparison. The review of the secondary literature appears exhaustive and Chibber even develops a novel explanation of the success of business-state relations in Korea—self-interest. While most accounts emphasize the relative autonomy of the Korean state and its power to discipline a weak capitalist class, Chibber argues that it was the turn to export-led industrialization aided by Japan's transfer of its light manufacturing to Korea that made cooperation with the Korean state both necessary and profitable for Korean capitalists. This comparison adds to our understanding of the Indian case because the absence of a turn to export-led industrialization (and the absence of Japan) further limited prospects for a successful developmental state there.

Furthermore, the book is comparative in the sense that Marc Bloch announced long ago—a hypothesis developed in one context is tested in another. Indeed the comparisons of this kind both explicitly, with other attempts at late-late development, or implicitly, with counterfactuals, are one of the book's strengths. Both kinds of comparison are not static parallel descriptions but theoretically productive and generative.

Third, as Achin Vanaik has observed in his review of *Locked in Place* in the *New Left Review* (2004, p. 154), it represents "a powerful assault on the intellectual assumptions, arguments and claims on which the prevailing neo-liberal consensus in India rests." And not only in India. The dominant neo-liberal narrative portrays India as one of the principal examples of the failure of socialist inspired state planning in which a "license-permit-quota-raj" inhibited the development of a dynamic Indian capitalist class and a high-growth free market economy. The neo-liberal remedy of privatization, deregulation, liberalization, and globalization is then seen as an antidote to the maladies of Indian development planning and indeed of development planning generally.

It is difficult to believe that anyone could continue to hold to this view after reading Chibber's book.

Despite Nehru's socialist principles and a genuine devotion to development planning in the Congress party's political leadership, the Indian Planning Commission was a relatively powerless agency that was subordinate to other ministries. Its efforts at organizational embeddedness in the capitalist class consisted of little more than talking things over. The Commission was never able to do anything more than offer special permits and other benefits to the Indian business elite who successfully resisted all state efforts to direct or discipline their activities. The business elite, as Chibber shows, contrary to conventional historiography, was hostile to the very idea of state planning from the very beginning. Ultimately the weakness of the planning agency rested on Congress's ties to the business elite and Congress's successful efforts to demobilize an independent labor movement. India is an example not of the failure of state-led development but rather of its absence!

Finally, the book begins to develop a model of the political base of the developmental state. In the conclusion Chibber generalizes his findings and argues that the state must avoid capture by the capitalist class either by having the good fortune to have a very weak capitalist class (the case of Taiwan) or a strong political base in some other class. Chibber's preference is obviously for a developmental state based in a strong working class party and he makes a convincing argument that post-war France represents just such a case. In the end he is forced to conclude that the putative social democratic development state in the Third World remains a theoretical possibility only. Nevertheless the attempt to theorize the social base of the developmental state goes far beyond most accounts and has implications far beyond the case of India.

Still for a book explicitly concerned with the class base of the developmental state one class receives relatively little attention especially in regard to its immense numerical size—the peasantry. Diane Davis in her recent *Discipline and Development* (2004) argues that the peasantry or, more accurately, the small farmer class forms the social base of the developmental state in Korea and elsewhere. The influence of the business elite in the Congress party is not only a result of the success-

ful demobilization of the working class but also the relative absence of independent political mobilization on the part of the Indian peasantry. Although Chibber notes that Gandhi succeeded in incorporating the peasantry into the Congress while advocating conservative positions in regard to wealth and property, the absence of the peasantry from the Indian development political equation is striking. In Japan and Taiwan as well as Korea land reform created a small farmer class that became reliable supporters of developmentally oriented political elites.

If India is an example of a failed developmental state and the neo-liberal orthodoxy is wrong, how do we account for the extraordinary recent Indian economic growth rates?

Furthermore if India is an example of a failed developmental state and the neo-liberal orthodoxy is wrong how do we account for the extraordinary recent Indian economic growth rates? Did the Indian developmental state accomplish something after all? Or were the neo-liberal proponents of globalization right all along? Perhaps in his next book--or more briefly in the discussion--he can explain how India became "unlocked."

Finally, prospects for the social democratic developmental state may be limited by precisely those structural changes in the global economy that neo-liberal ideology did so much to promote. The current unprecedented rise of left parties and social movements throughout Latin America may provide a potential test of the prospects and limits of the social democratic or any kind of developmental state. Although the rise of the Latin American left is very much a project in process, the preliminary results are not encouraging. Socialist and populist parties such as the Worker's Party in Brazil, the Uruguayan Broad Front, Néstor Kirchner's Peronists in Argentina and the much tamed social-

ist party in Chile have promoted neo-liberal policies despite surprisingly radical antecedents and, sometimes, radical rhetoric. The developmental results of the much more radical, self-described revolutionary movements in Bolivia, Venezuela or post-Fidel Cuba remain to be seen. But so far Fernando Henrique Cardoso's famous observation that "within neo-liberalism there is no alternative, and outside neo-liberalism there is no salvation" has yet to be disproved.

These are some of the issues raised but not addressed in *Locked in Place*. Still there is only so much one can do in a single book and Chibber has accomplished a great deal--a genuinely comparative study, based on original archival sources, that challenges the conventional wisdom on all sides, addresses problems of fundamental theoretical and practical importance, and proposes novel solutions with broad application to the global South. It is also clearly, even elegantly, written with a refreshing absence of sociological jargon and a clear analytical line that runs throughout the book. This is what sociology should be at its best.

The Lessons of Failure

Elisabeth S. Clemens
University of Chicago¹

A common mistake of novice teachers is to focus only on mistakes. But if students vow never to write that particular sentence or make that specific argument, how can they learn from failure? More experienced teachers regularly point out success in the hope that students will revisit a successful essay when they turn to write on a different topic. Yet, as Vivek Chibber argues, we can also draw the wrong lessons from success. In *Locked in Place*, he engages theories of economic development informed by the impressive accomplishments of East Asia in order to better understand the trajectory of Indian economic development.

Chibber asks two questions of the Indian case. First, why did state-led strategies of economic de-

¹ I have benefited from the insightful discussion of *Locked in Place* by members of Sociological Inquiry, Autumn 2006.

velopment fail to be adopted and – to an even greater extent implemented – in post-independence India? Second, once this failure was recognized, why were economic planners and politicians unable to reform the situation and fix the problem? These questions are framed in an interesting double move, both against theory and against the case of South Korea as an exemplar of state-led economic development. The theoretical foil for *Locked in Place* is provided by "statist" accounts that became the dominant explanation in economic development research published in the 1980s and 1990s. In stylized form, these arguments contended that state-led planning was the key to the successes exemplified by certain industrializing East Asian economies with their emphasis on export-led growth. The key question was what made it possible for states to lead capitalist development and the answer was located in the qualities of state bureaucracy and bureaucrats.

Against this argument, India appears as a striking failure. Statist analyses would highlight both the political support for economic planning and the endorsement of centralized planning by capitalists, exemplified by the Bombay Plan (published in two parts in 1944 and 1945). Given this combination of political intention and capitalist support, the only account for the failure of India's project of economic development would seem to lie with the competence of Indian bureaucrats themselves. Within this theoretical framework, India is the case that could have had it all but blew it.

Locked in Place challenges the facts of both cases in order to redirect analytic attention away from the competence of state bureaucrats to the preferences of business.² First, Chibber adopts a strategy from the comparative literature on welfare state development in order to focus on the sequence and timing of events in South Korea. The turn to export-led industrialization, he argues, was not an effect of, but a condition for, the construc-

² By "bringing business back in" to historical sociology, Chibber contributes to a growing re-engagement of sociologists with the questions of economic history central to classical sociological theory. For an extended discussion, see Bruce G. Carruthers, "Historical Economic Sociology: Actors, Networks, and Context" in Julia Adams, Ann Shola Orloff, and Elisabeth S. Clemens, eds., *Remaking Modernity: Politics, History, and Sociology* (Duke University Press, 2005).

tion of a state-led developmental strategy. Exogenous factors – strong relationships with Japanese industrialists who sought to relocate industrial capacity to Korea as well as U.S. military provisioning – provided South Korean industrialists with a windfall inheritance of networks of international trade. Capitalists, however, required state aid to secure adequate financing and materials. Consequently, South Korea was able to construct a developmental state because the preferences of business supported the establishment of this policy regime. Whereas statist analyses of economic development focus attention on the competence and capacity of government agencies, Chibber underscores the importance of the consent of firms to be governed, to be disciplined.

This piece of the argument exemplifies Chibber's theoretical commitment to "unlock the black box" of group preferences and their consequences within policy processes (p. x). If South Korean capitalists, operating in an unusual geographical and historical conjuncture, developed rational preferences for state-led economic development, Indian firms operating in a different context had good reasons both to pay some lip service to economic planning and then to resist the implementation of a central planning agency with the capacity to discipline economic behavior. Faced with a wave of popular protest and labor mobilization during the struggles for independence, business had good reason to expect that the leadership of the Indian National Congress would support some model of state economic planning and, therefore, a number of business leaders sought to control what that would involve. Once again geography and history are important to the explanation – with a large domestic market and the withdrawal of British firms at the time of Independence (a decision that receives little in the way of explanation), both business and political leadership embraced import substitution as a developmental model rather than an export-led strategy. Consequently, Indian business resisted the "installation" of a centralized state capacity for economic planning and, once a relatively weak and poorly coordinated "planning" apparatus was established, their rational economic preferences led firms to adopt strategies that undermined the overall performance of the Indian economy. The shortcomings of this first-generation of poorly implemented state economic planning then created the condition for the failure

of reform even in the face of weak economic performance. And, when pressure for reform finally did break through in the 1980s, it took the form of deregulation rather than the creation of the theoretically exemplary state-led development strategy credited with the economic successes of East Asia.

As a work of comparative political economy, *Locked in Place* persuasively links the questions that inform studies of economic development with the strategies of comparative welfare state studies. The latter document just how hard it can be to establish a redistributive social spending state and Chibber uses this style of close analysis of policy conflicts to contend that the failure of the Indian economic planning project lay with the configuration of business interests rather than the ideological commitments or competences of Indian bu-

In Chibber's universe of comparative politics, everything happened except the cultural turn.

reaucrats or politicians. In the process, however, the competence of capital is largely taken for granted. Chibber's strategy is to engage in a close analysis of the structural location of Korean and Indian firms and then to read preferences off of location. Relatively little attention is paid to the mobilization or collective identity of business. For example, we are left to wonder about how the Korean case would have unfolded in the absence of the coordinating capacity of the chaebol, the dominant business groups. Instead, the ability of business to control political outcomes is understood as the consequences of the absence of a strong labor movement allied with state bureaucrats that could overpower capitalist interests. (Chibber documents the decision of the leaders of India Congress to divide and domesticate the labor movement; labor is largely absent from the discussion of South Korea.)

The result of this analytic strategy is both reassuringly familiar and slightly unsettling. For anyone steeped in the comparative politics of the 1980s and engaged in the theoretical debates that have followed, *Locked in Place* appears to have been written in an alternative but neighboring universe. Centered on two nations-as-cases, it bears the hallmarks of the comparative research of the late 1980s and adds attention to fine-grained historicity, conjuncture, and eventfulness that have figured in more recent theoretical accounts of historical change. Chibber problematizes the state in important ways, understanding it as an institutional accomplishment that is sustained by specific feedback processes and potentially undermined by exogenous process that change the distribution of actors' rational economic preferences. Thus in Chibber's universe of comparative politics, everything happened except the cultural turn.

Because of this, *Locked in Place* forces us to consider just what has been added by that influential intellectual move. The challenge is to identify where *Locked in Place* suffers from life in this alternative universe. Which elements of the analysis would have benefited from closer attention to processes of interpretation and cultural construction? Perhaps ironically, the potential for a fruitful engagement between *Locked in Place* and the cultural turn may be greatest with regard to the concept of economic rationality itself. As Chibber explains his focus on the influence of import-substitution and export-led industrialization, "in generating bourgeois preferences, these models serve to set the terms on which politics are conducted" (p. 233). But, in the theoretical language of John Meyer and his colleagues, these models are theorizations or scripts that circulated in a transnational discourse on economic development. Thus business preferences were not only read off of specific structural locations at a specific moment; preferences were also read through cognitive templates that related firm behavior to expected benefits. In this sense, historically-specific rationalities are culturally constituted. Events might have unfolded differently given a third way, an alternative theorization of economic development strategy.

A second point of fruitful engagement centers on the processes of group formation. To the extent that actors share an economic location and prefer-

ences are read off of location, then the capacity for collective political action does not seem to require further investigation. Yet rational economic calculation can also provide reasons for defection, betrayal and schism. Did the "consent" of Korean business to state-led economic discipline rest on the denser networks of a small nation, on the foundation of organized business groups, on the experience of occupation and war, or on the interpretation of the threats posed by organized labor and the risk of an alternative path of economic nationalization? In rejecting forms of cultural explanation rooted in ideology, Chibber may have truncated his analysis prematurely and missed an opportunity to contribute to a richer institutional account of economic rationalities. But this would have been the version of *Locked in Place* written in my universe. Within the framework of his own forceful analytic commitments, Vivek Chibber has produced a notable success that demonstrates the potential of harnessing rigorous comparative politics to questions of economic development.

Unlocking the Shackles of the State versus Market Dichotomy

Leo Panitch
York University

The importance of Vivek Chibber's *Locked in Place* lies especially in the enormous contribution it makes to overcoming the false state versus markets dichotomy that has plagued political economy during the neoliberal era. Writing from the perspective that states are indeed constitutive of (varieties of) capitalism but that state actions are always determined by their relation to the balance of class forces in any given society, Chibber has given us the definitive critique of the institutionalists' state autonomy approach to explaining economic development (or the lack of it) in the South in recent decades. And he has done this in a way that brings that critique into a dialogue with the state debate concerning the advanced capitalist countries. In this respect, *Locked in Place* does more than any other book to reverse the unfortunate direction taken by political sociology away from class analysis after the advances made in the 1970s by the neo-marxist work on the capitalist

state. Insofar as those who led this shift in direction were in good part motivated by proving that the East Asian NICs and European social democracies were exemplary alternatives to Anglo-American neoliberalism, the strategic as well as the social scientific implications of Chibber's accomplishment is considerable indeed.

Of course even as good a book as *Locked in Place* has its limitations as well as its virtues. One of its great virtues is an argument advanced with a rigor that matches the best of game theoretic/rational choice analysis. But Chibber refuses to just rely on deductive logic for causal explanation, and amasses plenty of what Gramsci called "empirico-historical evidence" to sustain his class analysis, including evidence that pertains not only to the salience of capitalist class pressures on the state, but also to the salience of state actors' own "readings" of the balance of class forces. This concern with marshalling such evidence led Chibber, as he recounts in a fascinating passage in the Preface, to various Indian Ministries. But finding their historical records of policy-making either destroyed or denied to him, he struck on the brilliant idea of turning to the U.S. State Department, where indeed he found a wealth of data about Indian policy making in the 'Memoranda of Conversation' between US embassy officials and prominent Indian state officials, politicians and businessmen. That there was more of use to him regarding Indian economic policy making in the U.S. State Department's records than in the British Foreign Office ones tells us something important about the remarkable, even if informal, imperial capacity of the American state. But although Chibber has many references to imperial influences scattered through the book, the role of the imperial state is not theorized or analyzed here.

Is this then a book that, in its search for explaining the historical roots of the differences between the South Korean and Indian varieties of capitalism, continues to reflect some of the limitations of what Martin Shaw has appropriately called the 'social science as stamp collecting' comparative method, one which compares two nation states while paying insufficient attention to the overarching imperial carapace in which they are both imbricated? Such a criticism would be unfair. For instance, in addressing the central question the book poses -- that is, why Korean capital was receptive to disci-

plinary planning by the state whereas Indian capital was not -- Chibber's explanation points to the important role of Japanese capital in penetrating the Korean economy and the effect of this in making Korean firms recognize that they were dependent on state planners to make their turn to production for export possible and effective. This is a very persuasive argument, especially when combined with his awareness of the US role in the 1950s in promoting Japan's own export-led industrialization, and its extension to South Korea (indeed it is instructive to read Chibber's book in conjunction with the chapter on Korea in Chalmers Johnson's *Blowback* (2000)).

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This is an argument that is broadened and further enriched by the emphasis Chibber gives to showing how US foreign aid policy became by the 1960s increasingly oriented to promoting the restructuring of economies in the South so they would be less reliant on US government loans and more receptive to foreign direct investment and more export-oriented. That said, the role of the US imperium in relation to the South Korean state and capitalist class remains insufficiently analyzed. This especially so in relation to the emergence of the Park dictatorship under which Korean planning was inaugurated; in relation to the ability of the state to repress labor and other democratic forces for so long; and, not least important, in relation to the confidence of South Korean capitalists that state developmental planning would not entail a fundamental challenge to private property.

Chibber's argument is utterly compelling as he turns, in the main part of the book, to showing that, if one thinks it is state capacities that determine developmental possibilities, the post-independence Indian state initially evinced far more potential for this than the South Korean state. And he certainly demonstrates very well the opposition of the Indian bourgeoisie to the state playing any kind of disciplinary developmental role in terms of holding private firms accountable for the public funds doled out to them in line with the requirements of the economic plan. That said, it is not entirely clear whether Chibber is arguing that the Indian state really tried and failed to install a developmental planning process, or whether it never seriously even tried to do so. Was it capitalist pressures that really determined that effective state planning was killed in its infancy? Or was it the Congress leadership itself that aborted such planning not long after it was conceived? Chibber vacillates between these positions. On the one hand, he suggests (p. 126) that while 'much of the Congress leadership visualized state-led development' of the disciplinary type, it was the fact that India's capitalists wanted 'nothing at all' to do with this that determined the outcome. And yet he also suggests (p. 125) that the conservative, older generation dominant within Congress's "High Command" itself exercised "the most influence" in terms of the opposition to such disciplinary planning. My reading of the evidence Chibber presents supports the latter interpretation, at least in the sense of the pragmatic anticipatory opposition to developing effective planning capacity on the part of Indian political leaders and senior bureaucrats in light of their 'reading' of the resistance that would arise from both Indian and imperial capitalist forces.

Chibber rightly puts a lot of emphasis on how this accommodation to capital entailed a split between government and party, between the "High Command" and the activists. And, in an especially important chapter, he demonstrates how the demobilization of the labor movement after independence weakened the left inside the party. But more systematic attention might also have been paid to how imperial relationships steered the Congress leadership in their opposition to effective developmental planning. This is richly suggested, for instance, by "the barrage of letters" that Chibber uncovered written in 1949 from London by the

powerful Indian industrialist G. D. Birla to Deputy Prime Minister V. B. Patel in Delhi "reporting on the worries that British and American capital evinced about the 'investment climate' in India" (p. 244). But it would be very interesting as well to explore this in relation to what John Saville has termed "the mind of the Foreign Office" during the onset of the Cold War, and also in relation to the 1945 Labour Government's own abandonment of effective planning in the immediate post-war years. In terms of the American imperial relationship, Chibber himself provides good evidence (often in his footnotes which are worth the price of admission themselves) of how astute American officials in Delhi could be in terms of recognizing the grounds for the split between the government and the party on economic policy (pp. 286-7), but more might have been adduced from this about what this suggested about US imperial state capacities.

Both imperial and domestic capitalist opposition to effective capitalist planning in India in the early post-war decades suggests there is something fundamentally wrong with the conventional division of the era since World War Two into a period of national state autonomy and interventionist reform followed by a sharply contrasting period of the loss of state autonomy amidst globalization and neoliberal reform. Indeed, there is a strong case to be made that the seeds of neoliberalism were planted in the early post-war decades. Chibber's evidence can be read as showing that state policies in these decades were the incubators of the enhanced power of private capital. Just how much the fight against disciplinary planning in India set the stage for the greater reliance on market forces that has been the hallmark of neoliberalism is well-revealed in Chibber's quotation of Congress's Deputy Chairman, D.R. Gadgil telling a U.S. embassy official in 1967 that he was 'in favor of greater liberalization because successful state intervention "required much more administrative effort and sophistication than were available... Detailed planning of the production effort and investment can benefit the whole economy appropriately only if accompanied by meticulous price and distribution control. If, because of a variety of circumstances, such a regulatory regime cannot be operated, must not larger reliance be placed on market forces and competitiveness?" (p. 215).

What could have been done differently? And what might now be done? In this respect, Chibber is refreshingly open in affirming that his contribution is strategic as well as social scientific. Building on his demonstration of the significance of the exclusion of trade union representation from Indian post-war planning, and indeed Congress's deliberate demobilization of the labor movement, Chibber makes a case at the end of the book for mobilizing anew and enhancing the power of labor so its representatives might be able to exert greater influence within a genuine social democratic cross-class economic planning coalition. But this is quite misleading. What was followed in India in the post-war era epitomized social democratic politics in terms of the split between government and party, the demobilization of labor and the abandonment of effective planning. In some cases, this came about relatively quickly in the late 1940s (e.g. in the UK as well as India); in others it took rather longer (e.g. in Sweden or Austria). But in all cases capitalist planning of the social democratic corporatist type ran up against the impossibility of reconciling effective planning with capital's assertion of its right to privately determine what is invested and where, and what is produced and where. The ultimate demonstration of this was the sorry fate of the Swedish wage-earner's fund proposals as part of the labor movement's unsuccessful attempt to keep corporatist planning going there by the mid-1970s.

The attempt to rehabilitate social democratic corporatism in the hope that a newly mobilized and strengthened labor movement will make social democratic corporatist planning work in the South (without even addressing what this would have to entail in terms of changing government-party relations) was an unfortunately weak way to conclude such a very strong book. This much may be forgiven, however, in light of the book's great contribution in terms of rehabilitating class analysis and further developing state theory within political economy and political sociology, inviting their extension to the analysis of the imperial state, and demonstrating the dead end to which the state autonomy approach has led as an alternative to neoliberalism.

Response to Clemens, Paige, and Panitch

Vivek Chibber
New York University

The heyday of state-led development passed more than two decades ago, but some of the basic questions regarding its politics still remain understudied. In this respect, the developmental state has enjoyed a rather different history than has the welfare state, its counterpart in the advanced capitalist world. Whereas there is a very rich literature on the institutional variations within, and historical lineages of, welfare states, the study of developmental states fares rather poorly in comparison. Particularly weak is the scholarship on the origins of the latter, the politics behind its variations, and its relation to social forces.

When I conceived of the project that culminated in *Locked in Place*, the fierce debates on the origins of the American welfare state during the New Deal were still raging. They were triggered, in large measure, by Theda Skocpol's critique of, and challenge to, Marxist state theory, and soon generated a vast literature on the role of classes and political elites in the formation of social-democratic states. I was struck by the near total absence of careful analysis regarding these same issues when it came to developmental states. One of the ambitions motivating the book was to bring analyses of developmental states "up to speed," as it were, in light of the advances made in the study of social democracy. This was not the only, or even the main, inspiration behind the project – I had already decided as an undergraduate that I wanted to write a dissertation on the formation of the post-colonial state in India -- but it did play an important role in my framing of the issue.

By the time the dissertation morphed into a book a decade later, the concerns animating the welfare state debates had already receded, and even seemed a distant memory. Elisabeth Clemens is therefore entirely right in noting that the book seems to have come out of an alternate universe – not only because of the "cultural turn," to which Clemens points; but also because of the general fading of class analysis as a major force in American sociology. It is therefore something of a sur-

prise – and may all surprises be so pleasant! – that the book has been received warmly by the discipline.

Locked in Place is a book that asks a very precise question: if the success of state promotion of industrialization depends in large part on the state's institutional capacity, then what explains the success of some states in building this capacity, and the singular lack of success in other cases? It is a book about state-building – as its title declares. To this question, I offer the answer that the critical condition for state-building is the reaction of domestic capitalist classes. Korea exemplifies a case where domestic capital supported state-building, hence allowing for its success; India, in contrast, experienced a massive campaign against such a state by its business class, hence forcing state managers to retreat on their agenda, and leaving the state with a relatively feeble planning apparatus.

By no means should the book be taken as part of the chorus – so loud in Indian academic circles these days – that the four decades of planning were a gigantic mistake.

Bringing capitalists into the picture as a central actor went against the scholarship of the 1980's and 1990's which, in the study of developmentalism, had become increasingly state-centered. In much of the literature, it was simply assumed – this needs to be stressed, for it was rarely demonstrated – that capitalists in the developing world at mid-century were simply too small to have mattered as a political force. I try to argue that while it is certainly possible that capitalists in particular historical settings can be too small and too dependent on the state to be a formidable political force, this was so neither in India, nor Korea – and by extension, it may not have been so in other

countries at comparable levels of industrial development. The book therefore not only seeks to propose a class analysis of state-building in these two countries, but also its relevance to other cases in the South.

It is gratifying that this argument appears satisfactory to my three colleagues. But Panitch raises an interesting question: was it that the Indian state tried to install an effective planning apparatus and retreated, or was it that, anticipating a business attack, the state retreated in an attempt to keep business within the ruling coalition? Panitch thinks that I argue the latter, but, in my view, I quite clearly argue the former: the Indian state tried to push through its reforms, but retreated in the face of a capitalist offensive. I think what Panitch has in mind is a scenario in which the proposed state institutions might have actually been passed and put into place, but then would have been dissolved. Perhaps this is what he would take as a case of the state actually trying something and retreating – as opposed to a case in which institutions are proposed and never actually put into place. But in reality, the evidence is closer to his hypothetical scenario than he might think. The planning institutions that might have made the Indian state more effective got all the way into Parliamentary committees and even as draft legislation; moreover, some key institutions were actually installed, but then whittled down in clear response to business pressure. This may not be as far as he'd like, in order for it to merit the appellation of "state retreat"; but we might be quibbling over words here. What is clear is that measures were proposed, there was an attempt to implement them, and they were either shelved or broken down, in response to direct political pressure.

For the most part, the concerns that my interlocutors raise are not about the role of domestic business per se, but about the relevance of other potential actors and forces. Jeffery Paige wonders if the South Korean state's base in the middle peasantry might have contributed to its political stability and as well to the legitimacy of the whole planning enterprise. Here he draws upon Diane Davis's excellent new book on the political bases of de-

velopmental states.³ Leo Panitch, while quite happy with my book on its own terms, laments that it gives insufficient attention to the role of imperial influences. He is careful to note, correctly, that the book does point to the influence of core countries on state-building; his concern, if I understand him correctly, is that the analysis is not systematic enough. Imperialism comes in through the back door, as it were, instead of being theorized as a core element in the process itself.

Both of these concerns have considerable merit. It is certainly true that I do not pay sufficient attention to the rural sector in my analysis of Indian state-building. This is perhaps the biggest lacuna in the book. Paige suggests that Indian business's influence on the states was not only because of the demobilization of the labor movement (as I argue), but also because of the parallel absence of the peasantry as a mobilized actor: "The influence of the business elite in the Congress party is not only a result of the successful demobilization of the working class but also the relative absence of independent political mobilization on the part of the Indian peasantry." This suggests the following counterfactual: if the peasantry *had* been present as a mobilized force, they would have pressed for policies that pushed the state in another direction, perhaps in a more developmental direction. The state might have been more willing to resist business pressure for de-fanging the planning apparatus, and perhaps had a deeper commitment to pushing its agenda.

While this is certainly suggestive, I would offer the following cautionary note. The state's relationship to a social group can affect its policy outcomes in two ways: by affecting state managers' *intentions* and by affecting their *capacities to act*. My argument about the Indian state's retreat in the face of business pressure does not rest on any putative lack of commitment on the part of Nehru and his colleagues. I don't, in other words, think that the Indian National Congress was weak in its intentions – indeed, it is likely that there was no other political elite in the South at mid-century that was more committed to building a developmental state. The reason it retreated was that the

combination of capitalist pressure and labor's retreat reduced the political elite's *capacity* to push through its measures, because it lost leverage against recalcitrant capitalists.

Would a mobilized peasantry have directly affected the state's leverage with capitalists? Perhaps. But we ought to resist drawing a parallel between mobilized urban workers and rural peasants in this regard. Mobilized labor directly affects the state's leverage against business because it quite directly hits business operations, and hence business costs – thereby inclining industrialists to measure the relative worth of their resistance to state initiatives against the costs being imposed on them by ongoing strikes, job actions, etc. But a mobilized peasantry does not hit business operations as directly – it can hit business indirectly by affecting the flow of inputs of various kinds, but this is a contingent matter, depending on facts about the industrial structure of an economy, the specifics about which regions and sectors are mobilized, etc. Hence, I am wary of advancing, as a theoretical argument, that peasants ought to be taken as an actor parallel to workers in matters of the state's leverage against business. Paige is probably right in suggesting that a strong base in the middle peasants can more firmly incline political elites toward an effective developmental state – this is, I think, where he draws upon Diane Davis. But this speaks to state managers' intentions and commitments – which, in the Indian case, were not lacking. What was lacking was leverage, and here, I am somewhat skeptical about the potential opened up by the peasantry – in matters relating to *industrial capitalists*.⁴

I should also clarify that the failure of the Indian state was not absolute. I was careful to note in the book that India was a *relative* failure – relative to a case like Korea, and to India's own ambitious agenda. On an absolute level, import substitution in India yielded some remarkable achievements – a diversified industrial base, a highly trained engineering corps, managerial expertise, and a public sector that managed to survive and set up key infrastructural industries. Thus, I would not at all disagree with Paige in his observation that the efforts of the whole developmentalist period were

³ Diane Davis, *Discipline and Development: Middle Classes and Prosperity in East Asia and Latin America*, (Cambridge: 2004).

⁴ Of course, if the issue is the state's leverage against landed classes, this might be a different matter.

admirable; and further, that they laid the groundwork for the recent successes in industrial growth. By no means should the book be taken as part of the chorus – so loud in Indian academic circles these days – that the four decades of planning were a gigantic mistake.

Panitch's argument that the imperial role – not just of Great Britain but also the United States – needs to be theorized more explicitly is, I think, basically correct. But I would caution here against assuming, as Panitch might be, that the imperial core consistently opposed developmentalism. He refers to “imperial and domestic capitalist opposition to effective capitalist planning in India,” partly from the evidence I offer in the book regarding some British firms that mobilized against a powerful planning apparatus in India. But opposition from some firms does not amount to imperial opposition *tout court*. On this, I am agnostic. To answer whether there was something that deserves to be called imperial opposition, we need to uncover two facts: first, what was the sentiment within the larger business community, or discrete segments of that community, to developmentalism; second, what was the imperial state's position on the matter – which we cannot prejudge, whatever the opinion of business might have been. I say nothing about this in the book, and Panitch is right to castigate me on this. For what it's worth, this is precisely the project in which I am immersed right now – the reaction of hegemonic powers to developmentalism from the 1930's to the 1970's, and their role in its rise and fall. I hope to have something to say about this soon.

Elisabeth Clemens raises some very far-reaching points with regard to the possibilities that might have been opened up had I paid greater attention to the role of culture. There would be no way to address them adequately in the short space provided here. So let me offer some thoughts to her specific points. I argue that capitalist responses to state-building were generated by underlying structural – in particular, economic – conditions. Indian industrialists fought a developmental state because the adoption of import substitution made it rational for them to do so; Korean capitalists accepted such a state because the adoption of an export-led development model generated incentives for them to do so. Clemens offers that I might have adopted a different attitude to the rela-

tion between structures and capitalist strategies; instead of assuming a straightforward causal chain leading from structures to action, I might have allowed for great variation in capitalist responses – a variation resulting from cultural factors that might have filtered the very perception of the structures by key actors. Hence, in different cultural settings, the same structures might have generated varying responses.

I wish to register my agreement that culture can be relevant, and my resistance to the injunction that we insist on its relevance in every case.

Perhaps, but I'm skeptical. I am perfectly happy with the notion that culture filters the perception of economic actors. What I hesitate to accept is that “historically-specific rationalities are culturally *constituted*,” as Clemens says. There is no doubt that such actions are culturally *mediated*; but for them to be *constituted* by culture is a much stronger claim, and cannot be taken for granted. This is not to say that culture is secondary in all matters economic. Certainly, in many activities that are properly economic, cultural factors play a critical role – in the constitution of certain normative codes at the workplace, in the setting up of what a “fair wage” means for labor, in the manner in which capitalists choose to spend their wealth, etc. But there is a range of activities in which, I would argue, cultural norms are forced to adapt to economic circumstances: the compulsion to work for a wage if you are a proletarian, the resistance to shop floor despotism, and – for our purposes – the acceptance of the profit motive by firms and their managers. In the latter domain, culture might *color* the perception of economic pressure, but I do not believe that it *constitutes*. Of course, this is far too serious a matter to be settled here. I merely wish to register my agreement that culture

can be relevant, and my resistance to the injunction that we insist on its relevance in every case.

On the other hand, for some of the dynamics that Clemens points to – especially the willingness of Korean capitalists to ally with their political elite around state-building – there is good reason to pursue the cultural angle. There was certainly more homogeneity within the Korean power bloc than there was in the Indian counterpart. Perhaps that did allow for an easier state-building agenda. But that is an empirical matter and any verdict will have to not only show it, but also contend with the more properly materialist explanation offered here. I am quite confident that the very cultural turn to which Clemens makes reference will produce some analyses to this effect. We will then be in a better position to adjudicate between the contending approaches.

In the current neo-liberal age, it might seem quaint to call for more research on the politics of developmental states. As Paige notes, it might be that their age has passed. But I would urge that developmentalism is still of relevance for two reasons. First, and most generally, developmentalism was just one form of a dynamic that, within capitalism, is more or less constant, as Polanyi argued long ago – the pressure for states to intervene in markets, to bend them, to block their spontaneous effects. Hence, while a particular state form might no longer be dominant, the impulses behind remain very much present. Hence, the study of how they took root over the course of the twentieth century will be bound to have continuing relevance for future efforts at state intervention. Second, the forces that have made national development strategies are not natural or physical – they are the effects of legal and institutional changes, and are thus liable to be changed or even rolled back. It has happened before, when the last great globalization – stretching from the late Victorian era to the 1920's – was reversed after the Great Depression. Today, with the abject failure of neo-liberalism and its rejection in large parts of the Global South, there is again a call for returning to national development projects. Néstor Kirchner of Argentina has explicitly called for such a turn, and it has found an echo across political elites in South America. As long as such calls persist, the lessons of the past will certainly have continuing relevance for building a better future.

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Author Meets Author Meets Author

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Arthur Stinchcombe
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Charles Tilly
Columbia University

The 2006 meeting of the American Sociological Association featured an “author meets author meets author” panel, in which William H. Sewell, Jr., Arthur Stinchcombe, and Charles Tilly discussed each others’ recent books: Sewell’s *Logics of History*, Stinchcombe’s *The Logic of Social Research* and Tilly’s *Trust and Rule*. This symposium presents their original comments, plus responses by each author.

Sewell Reviews Stinchcombe and Tilly

It’s a privilege to be able to engage in a three-way dialogue with Art Stinchcombe and Chuck Tilly. I’ve been going round with both of them for some time. Art and I taught a joint course on social history and historical sociology at the University of Arizona in 1980. Chuck’s first book, *The Vendée*, was the intellectual model I took into the archives when I began my dissertation work in 1967, and our paths and our work have intersected and overlapped many times since.

I’ll begin with *The Logic of Social Research*. This is an unusual and on the whole a very attractive book. Theda Skocpol once wrote that *Theoretical Methods in Social History* was essentially a publication of Art Stinchcombe’s reading notes, but that in Art’s case it’s a privilege to be able to read over his shoulder. Art has now decided to publish his *lecture* notes from many years of sociological methodology classes; and once again the notes definitely repay a close reading. The book is a kind of methodology *un-text* book, one that both supplements and subverts typical sociological methodology. Rather than providing the reader with a set of standard procedures for taking sam-

ples, testing hypotheses, and modeling causes, Art dares his readers to craft different methods for different problems, to design samples that over-represent the extremes of the distribution, to tack back and forth between clarification of theory with data and clarification of data with theory, to engage in Levi-Straussian bricolage, to combine ethnography with surveys – in short, to be as unconventional and inventive sociological craftsmen as Art Stinchcombe himself has always been.

The book is full of wonderfully astute off-the-cuff observations and bons mots. Let me cite a couple of them.

On methodology: “Our job as a methodologist is to turn what it takes a genius to do the first time into something that all of us can recognize and work on” (86-7)

On methodological individualism: “Methodological individualism in its rational action variety counts individuals only as arenas in which changes in the situation, operating through stable preferences in the individual, affect other variables...Rational action...is in some sense an ‘individualism’ without any individuality in it” (173)

He’s witty and self-deprecating: He says “I think, in general, that a sociologist pontificating on epistemology is a sign of weakness: I’m guilty again as charged.” One hopes that some of Art’s originality and deadpan wit, as well as his methodological acumen, will rub off on the students who read this book.

The odd thing is that although I agree with almost all of Art’s practical methodological judgments, I disagree profoundly with his fundamental premises. Art sees all methodology as being about causation. “Almost all sociological theories,” he points out at the very beginning of the book, “assert that some social condition or conditions cause or produce one or more other social conditions” (1). This, as far as it goes, is a perfectly acceptable statement. But the devil’s in the detail, because “causation” can mean a lot of different things. Initially it appears that Art *must* have a broad definition of cause, since he specifies that

the methods of addressing causal questions in social science include not only quantitative and experimental methods, but also *ethnographic* and *historical* methods. And it's well known that ethnographers and historians tend not to see eye-to-eye with experimental psychologists or statistical sociologists when it comes to conceptions of causation in social life. But when, in chapter 2, Art actually lays out his account of causal reasoning, he adopts a purely quantitative conception of cause, one that would seem to leave most historians and ethnographers out in the cold. "All causation," he says, "is a relation between a *distance* of some sort on a cause, and a *distance* of some sort on an effect...the minimum piece of causal information is two distances" (22). Stinchcombe's thinking about cause, then, is avowedly *geometrical*: the world of the sociologist is made up of variables related to other variables in a Cartesian space. Cause is detected by an increase in distance on one variable that results in some measurable change in distance on another variable. In spite of the quirky originality to be found scattered through this book, Art paradoxically begins by planting himself squarely in what Andrew Abbott has trenchantly analyzed as the "general linear reality" of quantitative sociology. Art's claim – one I don't think he manages to sustain in practice – is that all forms of thinking about cause, even in historical or ethnographic research, can be assimilated to the problem of plotting distance on one axis against distance on another.

I can't claim to give a competent evaluation of Art's arguments about quantitative method. But I think I'm reasonably well placed to evaluate his thoughts on ethnographic and historical method. For lack of time, I'll just discuss ethnography. Art's treatment of ethnography seems to me distinctly peculiar. Two examples of ethnographic research are elaborated intermittently in the book. The first has to do with herding societies; the main question is the saliency of land versus herds in inheritance practices. The causal claim is that the more nomadic the society, the more salient are herds and the less salient is land – although there are many interesting side claims about horse-herders vs. cattle-herders vs. goat-herders, and about the variations in kinship structures between differently organized herding societies. All of these claims about causal relations are reasonable and interesting, and it certainly is true that we

could not test these causal claims without good ethnographic studies that tell us about how, for example, the Nuer or the Mongols organized their kinship and economic life. But note that Art's methodological remarks about herding societies are not actually about how to do ethnographic research effectively. Rather, they're about how a clever macro-sociologist can *use* the research of

The odd thing is that although I agree with almost all of Art's practical methodological judgments, I disagree profoundly with his fundamental premises.

ethnographers to develop a "distance-based" method to test theories about the relationships between property forms, kinship, and forms of production in herding societies. (The method is distance-based because herding societies can be ranked according to how fully dependent they are on herd resources as opposed to horticulture and on how salient inheritance of herds is in their property/kinship systems.) The problem is that scholars who actually carry out such ethnographies face a very *different* set of methodological questions, none of which Art addresses – about identifying good informants, analyzing rituals, grasping the semiotics of language use, classifying kinship structures, and the like. Although Art pays lip service to ethnography as a constitutive methodology of sociological research, he addresses none of the distinctive methodological problems facing the ethnographers – mostly anthropologists rather than sociologists – who carry out this sort of study.

The second salient mention of ethnography in *The Logic of Social Research* has to do with the work of Erving Goffman, who is clearly one of Stinchcombe's heroes – the only sociologist Stinchcombe hails in the book as a "genius" (86). The

discussion is in a chapter on “refining concepts of distances between units of analysis,” but distance figures in the discussion of Goffman only in the question of at how many feet apart people lower their eyes upon contact in order to maintain “civil distance” – the work in question here is Goffman’s *Behavior in Public Places*. Art observes, appropriately enough, that the distance at which people lower their eyes when they meet on the street can only be answered by ethnography, but it would be a stretch to claim that measurement of this kind of distance was crucial to Goffman’s method. In Art’s discussion of Goffman, there are some interesting observations about ethnographic method: he argues that Goffmanian ethnographers work by finding what he calls “contrary instances,” cases when people seem not to be following the general norm. But how this or anything else about Goffman articulates with Art’s fundamental principle of distance in a variable space remains unclear, at least to me. As I read him, Art officially grants recognition to ethnography as a form of research, but fails to grasp its logic, which rarely has much to do with Stinchcombian “distance.” (Although I don’t have the space to argue the point here, I think the same is true of his treatment of historical method.)

How, then, might we grasp the methodo-logic of ethnographic research? I think we have to begin from a very different starting point. For a Stinchcombian sociologist, the world is made up of a set of abstract laws and transportable mechanisms about distance relations; the job of the sociologist is to ferret these laws out from the messy concrete social relations in which they are enmeshed – very much as the Newtonian physicist shows that the fall of an apple to the earth and the orbiting of Jupiter around the sun are instances of the same law of gravity. The actual social relations in which people engage are of no interest for their own sake, but only to the extent that they can be made to provide evidence for the roving sociologist, who is always looking for new instances of his or her posited law or looking for new laws to posit. Ethnographers or historians begin from a very different starting point. They conceptualize the world not as a complex matrix of intersecting social laws, but as a concrete space- and time-bound complex of human interactions. Ethnographers (and historians) want to find out how the world is experienced and understood by the people

who inhabit it; the question of the *meaning* of peoples’ experiences is primary. They certainly look at comparative cases in order to gain perspective on their own, but their ultimate interest is in how the intertwined lives that make up their own cases fit together and get transformed.

This contrast between ethnography and Stinchcombian sociology is, of course, a restatement of issues as old as the late-nineteenth century *Methodenstreit* – which asked whether the human sciences, because their object was an intelligent and culture-bearing humanity, required distinct methods, or whether the methods of the natural sciences were adequate to the human sciences as well. Art and I both argue in our books for methodological eclecticism, endorsing both quantitative and interpretive methods, but we do so from fundamentally different positions in the never-ending *Methodenstreit* debate. Art attempts to *stretch* his natural-science-based geometrical model to embrace the interpretive methods of ethnography and history. As I have indicated in my discussion of ethnography, I don’t think this effort is successful.

In the final chapter of *Logics of History*, I conceptualize the social world as fundamentally made up of articulated streams of semiotic practices, or “language games,” and try to show that it possible to build up from this starting point a conception of social science that includes something like Stinchcombian mechanisms and quantification. In some ways this is the reverse of Art’s effort in *The Logic of Social Research*. But there is an important difference. My position is that there is one underlying social reality: human beings engaging in interconnected semiotic practices; but that the social life elaborated on the basis of this ontological starting point creates complex patterns, some of which can be grasped by interpretive methods but some of which also require quantitative methods and mechanical reasoning. Although these distinct methods get at aspects of the same underlying reality, I definitely do not claim, as Art does, that the *methods* are therefore fundamentally the same. In the end, I think we are on firmer ground if we recognize that sociological methods are *irredeemably* diverse. Instead of what Art calls “*the logic of social research*” I think we need to recognize “*logics of social research*,” with logics emphatically in the plural.

Charles Tilly's *Trust and Rule* lacks the grand ambitions of either *Logics of History* or *The Logic of Social Research*. (Mercifully, it's also only about half as long.) Chuck takes as his task in this book to theorize and make us aware of the importance of a particular form of social relation that he thinks is quite consequential for understanding the historical dynamics of states and societies, and especially for understanding democratization. Chuck does have a brief metatheoretical moment, on page 25, where he distinguishes three types of accounts of social life: the *systemic*, the *dispositional*, and (the one he prefers) the *transactional*. *Systemic* accounts posit large, coherent, self-sustaining entities (most often societies, but sometimes world-systems) and account for events by their location within these systemic wholes. *Dispositional* accounts posit a different kind of coherent, self-sustaining entity: individuals endowed with preferences, culturally determined beliefs, or dispositions whose motivated actions aggregate into events at the social level. *Transactional* accounts, in Tilly's words, "take interactions among social sites as their starting points, treating both events at those sites and durable characteristics of those sites as outcomes of interactions." Interactions, not individuals or societies, are primary. Transactional accounts have the advantage, Tilly adds, "of placing communication, including the use of language, at the heart of social life." Anyone who has read my book can imagine that I certainly strongly agree with Tilly's metatheoretical preference, although I would add that transactional accounts must be able to account in their own terms both for the emergence and durability of such quasi-systemic entities as nation states or global capitalism and for the relatively stable dispositions of actors. Tilly does not elaborate a detailed argument in favor of his metatheoretical preference. Rather, he tries to show the value of transactional analysis by working out the logic of one particular sort of transactional entity: the trust network.

The defining features of trust networks are that they undertake "valued, high-risk, long-term activities" that are exposed to "malfeasance, mistakes, or failures on the part of network members." The key feature of such networks, as I understand it, is that their members rely on people whom they don't know intimately to carry out potentially costly actions and in turn are willing to carry out

such actions for them. Trust of this sort is only possible when there is a very strong boundary between members and non-members of the networks – trust networks are, as Chuck puts it, "segregated" and have "high costs of entry and exit." Chuck does a nice job of spelling out commonalities in an extremely diverse set of examples, including the medieval Waldensian heretical sect; the Jewish community of Johnstown, Pennsylvania (as studied by Ewa Morowska); "intentional communities" such as communes (as studied by Benjamin Zablocki and Rosabeth Moss Kanter); Provençal confraternities (studied by Maurice Agulhon); an assortment of trade diasporas and migration chains; a sixteenth-century English Parish (studied by Eamon Duffy); and the al-Qaeda terrorist network. For my money, any concept that enables us to recognize the common features and social processes of Waldensians, migration chains, and al-Qaeda is definitely worth adding to the sociological vocabulary.

Chuck points out that trust networks have typically stood in a very guarded relation to states, which, from the point of view of the trust networks, are usually regarded as at least potential predators. States want a piece of these networks' sequestered economic resources as taxes or protection money; they often want allegiance as well, and sometimes adherence to specific religious belief and practices. Segregated trust networks nearly always seem at least vaguely threatening to states. Typically, trust networks respond with downright concealment (as for the Waldensians or al-Qaeda), with dissimulation (by not making clear to the outside world crucial portions of their activities), or by seeking patronage. Becoming the client of some patron (parishes or confraternities linked to the Church, guilds chartered by the king) had the advantage of regularizing the trust networks' status, but this always came at a cost in terms of supervision, financial demands, and the like. There was also the possibility for the network itself to engage in predation, like pirates or bandits, but this was highly risky. Trust networks tended to proliferate in premodern states, which, because they had limited infrastructural power (to use Michael Mann's term), were willing to settle for patron-client relations with trust networks. But modern states, whether democratic or totalitarian, tend to look with less favor on segregated trust networks.

One of the themes Chuck treats in this book is the relationship of trust networks to democracy and democratization. I'm afraid I didn't find this aspect of the argument very satisfying. What I think works best in this book is Chuck's elaboration of trust networks as a kind of transhistorically valid category of sociological analysis, parallel, one might say, to Weber's discussions of bureaucracy or patrimonialism as sociological categories. The discussion of democratization and trust introduces into the analysis a diachronic, historical dimension that seems to me less adequately developed.

Chuck's chief claim here is that democratization can be understood, at least in part, as the *integration* of trust networks into the state. I certainly agree that trust is an important issue for democratic states. As Chuck points out, in order for democratic states to function effectively they must be able to gain the trust of the citizenry. Citizens must be willing to pay taxes, honor conscription, and, eventually, contribute to state managed pension and medical schemes. They must, in other words, assume that the state can be trusted to use wisely or at least honestly the valuable human and economic resources that they render up to it. Citizens believe this in part because they are able to influence governmental decisions by means of voting and freely expressing their opinions in the public sphere. But, for the most part, the big story here seems to be not the *integration* of pre-existing trust networks into the state, but rather the creation of a *new* form of trust that is universal between citizens rather than segregated and tightly bounded like the trust networks Chuck has so vividly described earlier in the book. It is certainly true that trust networks persist in modern democracies. For example, many of the intentional communities and migration chains discussed in this book occur in the context of modern democracies. I would say that extensive proletarianization and bureaucratization of social life, together with the vast increase in the infrastructural power of states, has undermined the conditions that made segregated trust networks so ubiquitous in pre-modern societies. Citizens of modern democratic states do in fact trust states to pay their pensions and (except in the United States) their health costs.

Rather than pursuing the question of how trust networks are integrated into modern democratic

states, I think Chuck might better have used his theorization of the trust network as a sociological category to explain what it is about social relations in modern democracies that has either enabled preexisting segregated trust networks to remain in operation or has given rise to new social niches in which such networks are invented anew. I'm confident that this question can be answered by using Chuck's general analysis of trust networks. As this final remark should make clear, even if I'm not convinced by Chuck's account of democratization, I certainly *am* convinced that his book has introduced an important new category into the sociological lexicon.

Stinchcombe Reviews Sewell and Tilly

My main methodological argument in this commentary is a simple one, namely that the central methodological canon for historical methodology is: **Know a Lot**. This sounds as if I was trying to resist having any epistemology, as T. S. Eliot no doubt meant when he said the analogous, approximately "The only method for literary criticism is the application of a very great intelligence." Not so for historical sociology.

First I will illustrate this by using two examples. I have been corrected by each of my co-panelists in a reckless theoretical statement, because they knew more about it than I did. Then I will argue that the central requirement of all methodology is that one be able to refute theories that are false, and the quicker and cheaper that refutation is, the better the method is. Knowing a lot makes it much easier and cheaper to refute theories with facts one already knows.

Once I was giving a sketch of a mathematical model of how it might happen that unions and labor or socialist parties start being organized at around a given time in the history of one country, later in another, and how it might happen that the labor movement declined in vigor, especially in new organizing and conquest of higher votes in elections, at later times, also sequenced by country. The basic idea of the model is built around

the model in physics of the decay of heavy radioactive atoms into an intermediate radioactive isotope of another element, then on into a stable lighter atom. It is obvious that if there is one rate of probability of the heavy atom to decay, and then a different one, perhaps slower, for the decay of the intermediate isotope into the lighter stable one (for instance the metal lead atom is stable and “lighter” than the uranium one), then the number of atoms of the intermediate might first grow as the heavy atoms decay but the isotope doesn’t decay as fast, and then finally decline as all of it decays into the stable atoms. So much for the mathematical model of labor movement growth and decay, with farmers taking on the heavy role, proletarians the intermediate isotope, and services workers the lighter stable ones. So I recklessly set the equations for England so that they would produce lots of workers in the late 19th and early 20th centuries, then declining, reset them so for Spanish ones so there would be lots of workers in the 30s, in time for the civil war, and so on. Bill Sewell raised his hand and said something like, “Back there where you have England having 3 or 4 % workers, they were actually somewhere from a third to a quarter of the work force. Does that undermine your main argument?” Well the answer I gave was that it didn’t really, except that one could tell which was the model, which was the truth.

Then one time I sent a paper to Charles Tilly in which I said something casually about how French nationalism allowed Napoleon to successfully raise a much larger army for foreign conquests than the other European countries could raise. Tilly sent me what is, for him, a stiff note that said something like: “You should find out what actually happened before you write about it.” He referred me to a book that told what a difficult time the Napoleonic government had getting conscripts into that army. They tried this and that, until finally they structured it so that the whole village was punished if they did not deliver enough young men. So I rewrote that part. I had recklessly adopted what was a more or less conventional interpretation of Napoleon’s success, in which en-

thusiasm played a bigger role than coercion. It wasn’t worth my twisting around to explain successful coercion as nationalism, though being a model builder, I imagine I could do it, to be knocked down by another fact Tilly knows.

My main methodological argument is: Know a Lot.

Now I am pointing to these things not because they are an encouragement to my modesty. I’m not going to be any good at modesty. Instead I want to make the point that *if* I am going to set out to explain why the advance of the labor

movement to massive organization and power came and went, and that it wasn’t nearly as strong in the 1790s or so that starts E.P. Thompson’s *Making of the English Working Class*, as in the last chapter in the 1840s or so, I’d better do it with more subtle tools than intermediate isotopes. And I will do better if I know that I can’t start with no working people when and where E.P. Thompson started his book. Similarly if I have a theory about enthusiasm building an army for the Bonapartist empire, I should know what actually happened in the recruitment for national glory.

I will now argue that Sewell and Tilly have a good clean epistemological advantage by knowing more than I do, at least about these particular things. In experiments in social psychology with control groups, and in surveys with attitude scales with high and low values, we have a way to disprove and throw away hypotheses. A researcher then can plan ahead how to test them. And if they get lost, they can look for new theories almost as fast as Tilly remembers a book to recommend to me, and I can read a correlation matrix even faster than Tilly can read a paper of mine, because I’m really quite good at matrix algebra. But it’s a lot harder to build historical research in advance so that, *if* it was really for Napoleon hard to make young men go to the army, *that* disproves enthusiasm as a cause of military discipline. That “if” came into Tilly’s mind because he knew the answer, and not into mine because I didn’t. So my methodological sub-point is, if you don’t know a lot, have friends that do. And if you are stuck to get a theory of survey data when your first one didn’t work, send the relevant correlation matrix off to me.

Other things being equal, the more investigators know about a subject, the more likely they won't build a foolish theory about it, because beforehand they can disprove in their minds a bunch of theories that don't work. Historical workers don't have to tell their audiences about all the null hypotheses that were really null, that only stayed in their minds until they remembered a scrap from somebody else's book or some document they ran across last summer. Now comes the crucial point: *because* investigators won't know very far in advance what it is that they are going to theorize about when they start their research, they will want to know a lot so that they will know when they are being foolish, more or less whatever theory they stumble across in looking over the facts. That is why it is important that history itself is divided by times and places in the first place, so that it is easier for a historian to know a lot about the place and time he or she is investigating. I once tried to show in my *Economic Sociology* that I knew a lot about the economies of 18th century France, the Karimojong herding tribes of Uganda, and the United States in the 1960s, so I make less mistakes there.

Now let me turn briefly to the specific books of my critics and colleagues. First I will address some things that I think are missing in Sewell's fundamental concept of an "event" that transforms old structures into partially new ones, such that in the new ones different causes operate, and the same causes have different effects. This seems to say that little causes have big effects, like an acorn becomes a big tree, changing the carbon dioxide from the air into wood ready to be installed on living room floors. The acorn has to grow for some time before it can start making wood, and for a lot of time before it can become a living room floor.

This logic of "events" and "conjunctures" and changes in what causes what over time is not specific to human history. But by thinking of how an acorn grew into a huge cause of living room floors, we may get an idea of how events become forces. I will take Mark Traugott's lovely demonstration that Marx was wrong about the lumpen-proletarian composition of the National Guard and the proletarian composition of the national workshops. Sewell's own recounting shows, I would maintain, that there was a very rapid change in the internal culture of what became the two con-

tending sides, including the buildup of the charisma of the leaders of national workshops, their culture of organization that made them more like an army, without the guard's devotion to the conservative side. This new culture had been nowhere to be found before, and was not, as Marx thought, a prior division within the working class of the *lumpen* and craftsman subcultures. And of course the new national guardsmen had not been armed before, had not been supervised in an organizational culture developed by army officers, and so on. That is, it was the culture changing rapidly, but by small changes per day, using a variety of elements in the repertoire of French rebellions and revolutions, that created the *preconditions of the events* that Sewell is so interested in. But that event became important because it first manifested a rapid change of culture on the two sides, starting with the same social materials, and one of those sides could start being disassembled once the "event" left the other newly grown side still on the field.

The same is, I would argue, even more true of the event of Captain Cook's death in Hawaii from Sahlins, that Sewell gives such a good summary of. Sahlins argues that there was a repertoire of what might be called the King versus church (against priests, anyway) conflicts in the Hawaiian culture, played out in the year or so between Cook's first contact in Hawaii and the second one in which he was killed. In that time Cook had become a totem in the priestly culture as a God come to Earth. After the death, Cook's bones were to become a totem instead in the royal repertoire of conquest of the other islands. The importance of bones to the Hawaiians was shown by their misunderstanding that the English who wanted Cook's body back to bury him as a Christian could make do with a few bones, that the priests apparently stole back from the King. While Cook had been in Alaska talking to the Russians, there developed in Hawaii a claim of priestly religious autonomy much like Thomas à Beckett developed in a short time, based on an interpretation of Cook's visit. As in England, a killing, the "event" gave the King the title: "Defender of the Faith," which is still Elizabeth's title, NOT that of the Archbishop of Canterbury. Sahlins's account of that rapid cultural change in Hawaii is nearly as exciting as T.S. Eliot's, or Traugott's. We just have no concept in sociology of how fast cultures

can change, except when a good historical sociologist or anthropologist knows more than we do.

The main general epistemological point here is that usually big things have big causes; if we think we have found big effects of small causes, we are well advised to look for massive changes in culture in a short period of time, and particularly big changes in its relation to (perhaps new) structures of institutionalized power, so that the new culture can be extended in time and space, as Giddens has taught us to think of institutionalization of culture.

So the second big methodological principle is that *when the facts you know so well tell you that culture sometimes changes faster than social organization and resources, listen.*

Now to Tilly: only by knowing a lot about several countries could Tilly even tackle the big question he is after in this book. He wants to locate those networks of loyalty and trust that enable people to trust their fates to each other: kinship groups, mafias, religious underground movements, and the like. Such networks have to consist entirely of relationships such that each person is monitored for trustworthiness by many others in the trust network. Each then may be entrusted with bringing up the children, not betraying fellow jihadists, and the like. When it's really serious, a matter of life and death, only strong ties at each node will do. The strength of weak ties can only come about if nobody knows enough to betray you to the secret police; with weak ties you get dominated by the Nazi neighborhood bully, the *Gauleiter*, who has strong ties within the Party and to the Gestapo.

Tilly then distinguishes these high loyalty networks according to whether they are in the political-military elite, the commercial and economic "second estate," or are trust networks that evade or oppose the elite, or ones that just live their lives in areas of low elite governance. The reason he needs this distinction is that *unless* these big things in the life of the general population (sex, children, care and subsistence in old age, security of the home and within the home from violence) are integrated into political life, democracy is both superficial and unstable.

Tilly then gives lots of examples of democratization that satisfy this criterion for a while, and de-

democratization when the governing elite invades to disable such loyalty and trust networks that guard the big essentials of life. And he skipped a few that we sociologists generally skip over, that the United States had a slave system and a Civil War that both violated a lot of the prerequisites of democracy, and that the decentralized or "federalist" genocide of the Native Americans invaded trust networks of indigenous communities. Many of the states that seceded in the Civil War had wider suffrage among white men than most Northern ones that opposed them. Where village democracy was the most developed on the frontier at that time, whites were often mobilized into democratic militias to kill many Indians and to take their land. I hold an emeritus professorship commemorating a Methodist layman who organized the slaughter of a peaceable Indian tribe when he was governor of Colorado. My point here is that there doesn't seem to be a very strong correlation between the formation of groups with very strong ties of deep loyalty, so others will trust their lives to them, and democracy. But they tend to become *big* causes of movement in whatever direction they take, because such subcultures can grow rapidly into structures of power with a great capacity to change social structure.

So the third methodological principle I will offer is: *go after the big things in peoples' lives, life and death and reproduction, that move people to change their culture and social organization.*

So the conclusion I take from this is, Know a lot; When the facts show that in fact culture changed rapidly, notice it; and Go after the big things in people's lives, because they can build strong structures that become big causes.

Tilly Reviews Sewell and Stinchcombe

Apple, orange, and kumquat? The three of us have taken on an intimidating task. The three books we are comparing and criticizing here include a collection of essays, a graduate level textbook, and a synthetic monograph. Having published essay collections, textbooks, and monographs of my own, I can hardly complain about our heterogene-

ity. But it does rule out the simplest strategy for such a comparison: an item-by-item scorecard with judgments as to who does what better. Fortunately, the outstanding books we are discussing come from Bill Sewell and Art Stinchcombe, authors whose work I have followed and admired for decades. Although I could certainly review them – and praise them -- for writing style, fresh insights, or challenging claims, another less obvious tack seems more likely to raise valuable points for debate. Let me assess the two books as contributions to the philosophies of history and social science. That means asking whether they supply viable visions of the presuppositions historians and sociologists must adopt in order to do work that reduces our collective ignorance. It means dealing with ontology, epistemology, logic, and method.

But don't worry: I have no intention of bludgeoning you with esoteric philosophical concepts. On the contrary, each of the two books, in its own distinctive way, broadcasts a strong message concerning the proper way to generate reliable knowledge of human affairs, taking history into account. For Sewell, relations between history and social science occupy the foreground of his analysis. For Stinchcombe, historical methods take their places along side quantitative, ethnographic, and experimental methods. But Stinchcombe, a seasoned historical analyst on his own, gives history plenty of attention.

In a recent clarifying essay on the philosophical foundations of political analysis, Philip Pettit distinguishes five relevant branches of philosophy: philosophies of reason, of nature, of mind, of society, and of value. Although we could search out Sewell's and Stinchcombe's presuppositions in all five regards, their books commit them most explicitly on questions of reason and society. The philosophy of reason, according to Pettit, "explicitates and examines the presuppositions we make as to what follows from what when we reason on any topic whatsoever, whether of the kind related to deductive or inductive logic, epistemology, or the philosophy and methodology of science" (Pettit 2006: 36). The philosophy of society, he continues "deals with presuppositions about the nature of conventions, norms, and laws, about the possibility of joint intention, communal life, and group agency, and about the character of the citizenry, democracy, and the state" (Pettit 2006: 37).

Pettit also makes a useful distinction between two philosophical concepts of persons, one decision-theoretic, the other discourse-theoretic. Decision-theoretic persons act in response to interactions of their beliefs and preferences, discourse theoretic persons in response to interactions with other persons, mediated by the surrounding culture. In these terms, both Sewell and Stinchcombe engage the philosophy of reason; the word "logic," after all, appears in both their books' titles, although characteristically as a singular in Stinchcombe and a plural in Sewell. Yet on the whole Stinchcombe's book draws more heavily on the philosophy of reason, Sewell's on the philosophy of society. Stinchcombe, furthermore, stays much closer to decision-theoretic conceptions of persons than Sewell, whose persons mostly inhabit a discourse-theoretic world.

What problems is Sewell trying to solve with his discourse-theoretic philosophy of society? In a collection of ten disparate essays, more than one problem sometimes takes charge. Individual treatments of Clifford Geertz and Marshall Sahlins, for example, necessarily take up different issues from a chapter boldly labeled "Refiguring the 'social' in social science." Nevertheless, one immense organizing question recurs throughout the book: how should historians and social scientists represent interactions of time, culture, and social structure? Such a question might seem to cover the entire field. In fact, it leaves out a whole series of problems that occur to a philosophically alert reader as the book proceeds, for instance how exactly to detect cause-effect connections and what sorts of formal models plausibly represent social processes. Since Stinchcombe's book features just such problems, we see that Sewell has substantially narrowed his field.

Sewell takes up time, culture, and social structure separately at various points in his book, but brings them together in his grandest efforts at synthesis. Remember that bold essay on refiguring the social? It starts by teasing readers with a long exposition of semiotic approaches to social life, but ends by substituting social construction for language as its preferred metaphor. As Sewell puts it,

I claim that discursive or semiotic processes (that is to say, meaningful human ac-

tions) are conditioned by and give rise to structures or forces governed by built-environment logics (logics of spatial fixing, material instantiation, accretion, and duration) and that such built-environment logics condition semiotic processes (by stabilizing them, undermining them, or by subjecting them to transformative pressure). An adequate conceptualization of the social must recognize both the semiotic and the built-environment logics and trace out their dialectical interrelationships (Sewell 2005: 368).

Time enters this complex passage in the processes by which semiotic practices generate social environments and those environments subsequently shape semiotic practices. Culture enters as what Sewell elsewhere in the book calls “partially coherent landscapes of meaning” (Sewell 2005: 174), hence in the content of semiotic interaction. Structure enters in the very Giddensian form of negotiated outcomes to that interaction. Together, time, culture, and structure constitute history as meaningful lived experience.

“This is the point,” comment Don Kalb and Herman Tak, “at which Sewell’s formulations become confusing:

Either culture is autonomous and operates according to a logic of its own, roughly Saussurean if need be; or it is not autonomous and is operated within an interlocking set of institutional practices, including specialized institutions for culture production, set in motion by identifiable and interested actors, who may or may not face resistance. It is the one or the other, and it makes a difference for how we think and talk about social existence (Kalb and Tak 2005: 9).

Will this ambivalence do philosophically? Let me identify two large philosophical difficulties that Sewell’s book leaves unresolved. First, the time-culture-structure model of social processes implies that all social construction operates through the mediation of conscious, intentional human minds engaged in language games. Although Sewell makes concessions to unintended consequences, how can we reconcile his model with incremental effects, simultaneous effects, environmental ef-

fects, and unconscious effects? Second, to adopt a term that figures prominently in Stinchcombe’s book, what causal mechanisms produce the transformations of culture and structure so central to Sewell’s model? If, for example, we reject the teleological temporality typified by Immanuel Wallerstein and the experimental temporality typified by Theda Skocpol in favor of the eventful temporality Sewell recommends, how shall we specify the causes that make some events more consequential than other events? Must we, in fact, abandon serious efforts at explaining social processes? Since neither the word “cause” nor the word “explanation” appears in Sewell’s index, the book seems to leave the two big difficulties unresolved.

Sewell actually talks philosophy, deploying terms like epistemology and ontology repeatedly. Neither word appears in Stinchcombe’s index, although the word epistemology does sneak into the book’s main text. Stinchcombe presents himself as more of a cracker barrel philosopher than an Aristotle, self-consciously retaining the oral style of his lectures to graduate students. Nevertheless, his book’s very first sentence declares as its purpose “to analyze logically and practically various strategies sociologists have invented to explore for, develop, or test theories of causation in social life” (Stinchcombe 2005: 1). What’s more, David Hume makes his first appearance in the same first paragraph. That sounds a lot like philosophy, indeed like Philip Pettit’s philosophy of reason.

How does philosopher Stinchcombe do his work?

He posits a world of phenomena falling onto continua along which distances vary from small to big. Those phenomena also compound into units: persons, places, organizations, and more. To the extent that those units serve not merely as convenient points of observation but also as efficacious actors or sites of action, they must be real and causally coherent. If efficacious units are not persons, they must nevertheless possess boundaries within which causal interdependence clearly operates. Representations of distances along the continua occupied by distinct phenomena count as variables, but we must be careful to distinguish between the underlying ontology of phenomena and their representation, which takes place through the observer’s constructs. A Stinchcom-

Sewell's model implies that all social construction operates through the mediation of conscious, intentional human minds engaged in language games. How can we reconcile his model with incremental effects, simultaneous effects, environmental effects, and unconscious effects?

bian observer faces the problem of disciplining information drawn from the units to test theories about how the continuous phenomena affect each other. They affect each other by means of causal mechanisms.

Observers of social phenomena can choose among quantitative procedures, historical analyses, ethnography, and experimental intervention as they measure distances by means of variables. To do so, they must adopt or invent concepts. Concepts identify differences worth attending to – differences in one variable that cause differences in other variables. But observers must also shape their theories to the contexts in which cause-effect relations are operating; as Stinchcombe puts it, “Most causal processes in all sciences have boundary conditions” (Stinchcombe 2005: 16). Theorizing consists of producing verifiable statements about relevant cause-effect relations within appropriate boundaries. The wider the range of a theory's empirical implications, on the average, the more powerful the theory. Testing theory then involves two steps: deriving observable implications, and determining whether those implications are true or false. That sort of two-stage test takes on greater weight when competing theories, all at least superficially plausible, come into play.

Readers who have advanced beyond the first-year graduate methods course for which Stinchcombe originally wrote his lectures will of course find a

number of the arguments familiar (see e.g. Brady and Collier 2004, King, Keohane, and Verba 1994). Without using the term, it incorporates a correspondence theory of truth. But with greater lucidity and far more well-worked examples than the average textbook, it lays out a program for inferring causes – not simply correlations – from data. It stands out from conventional presentations not only by identifying practical ways of making causal inferences but also by showing the complementary parts that quantitative, historical, ethnographic, and experimental data can play in pinning down cause-effect relations. Nevertheless, the book's deep logic resembles that of standard social statistics. As Stinchcombe says:

I believe that these logics are basically the same, but that statistics textbooks do not ordinarily go into where the numbers they calculate with come from. We have gotten near the end of a long book on method before coming to this point, and I believe that most of what precedes ought to be at the beginning of a good statistics textbook (Stinchcombe 2005: 239).

Thus Stinchcombe's teaching qualifies as superior sociology.

But as philosophy? Like Sewell, Stinchcombe takes us to the edges of a pair of philosophical gorges, but doesn't tell us how to bridge them. First, what is our warrant for assuming that the observable world divides neatly into continuous phenomena we can plausibly represent as variables? What if everything out there actually consists of boiling plasma from which our puny attempts at measurement only capture occasional spurts of gas? Or what if every apparent continuum actually takes the form of a Moebius strip, forever turning back on itself? To what extent will the observational procedures Stinchcombe describes so alluringly then produce false positives: apparent verifications of theories that are wholly inadequate? Could it be, as Andrew Abbott asserts, that the general linear model, foundation of sociological statistical analyses, “has come to influence our actual construing of social reality, blinding us to important phenomena that can be rediscovered only by diversifying our formal techniques”? (Abbott 2001: 38).

Second, what about time and its compounding into history? Stinchcombe generously treats historical analysis as one of his four major methods, and provides many an example of effective historical work, including his own. But his causes seem to be instantaneous, reversible, and impervious to sequencing effects. At a minimum, one might have thought that the history of a social unit or social process would affect its behavior at a given point in time. Since Bill Sewell's whole book argues such a point at length, perhaps I should leave the question there. Can we possibly reconcile or at least adjudicate the confrontation between time-drenched Sewell and timeless Stinchcombe?

Rejoinder: Sewell

It's hard to disagree with the methodological conclusions Art Stinchcombe elaborates in his presentation. We are well advised to know a lot, to take notice when cultures change quickly, and to look for the big things in people's lives. I am less convinced by his claim, in his discussion of my book, that "usually big things have big causes." There are two problems here. First, I think the notion of bigness simply repeats in slightly different language the mistake of reducing all of social life to questions of distance. How can we compare the "bigness" of an event like the Parisian insurrection of the "June Days" in 1848 (about which Mark Traugott wrote) to the "bigness" of the cultural changes that differently affected the National Guard and the Mobile Guard in the weeks leading up to the insurrection? Or the "bigness" of the Hawaiian chief's assumption of the role of "defender of the faith" (about which Sahlins wrote) with the "bigness" of the cultural rift between the chief and priests over the previous year? Surely in neither case is there anything approaching a single metric that would allow one to compare the size of a cause with that of an effect. "Bigness," here, seems highly metaphorical and subjective. Second, to the extent that my notion of eventful temporality implies "little causes having big effects," the issue is really not about big and little causes and effects, but about the importance of *contingency* in social life. Thus, for example, the fact that Hawaiians took Captain Cook to be the God Lono (an identification that Sahlins shows had

very important ramifications for the future of Hawaiian society) was entirely dependent, according to Sahlins's account, on the utterly contingent fact that he arrived in Hawaii at the beginning of that God's festival. Had he arrived at a different time of year, he might have been identified as a different God (with different social entailments) or not have been thought a God at all. As far as I am concerned, the insistence on using the metaphor of "bigness" as if there were a common linear scale for all sorts of social facts distracts us from using effectively all that highly differentiated knowledge that Art wisely counsels us to obtain.

I think Chuck Tilly gets the contrast between my book and Art's about right – that mine is primarily discourse-theoretic and time-drenched and Art's primarily decision-theoretic and timeless. Chuck criticizes my book for being insufficiently concerned with causation, remarking that neither "cause" nor "explanation" appears in the index. But this is a matter of poor index construction on my part: the index does have quite extensive entries under both "mechanistic explanation" and "paradigmatic explanation." (I now see that the entries should have been "explanation, mechanistic" and "explanation, paradigmatic.") What is true is that I spend relatively little time talking about the standard sociological protocols of explanation: the positing and testing of causal generalizations. My neglect in this respect is purposeful. That such protocols are valuable, even essential, to good social science seems incontrovertible, but hardly news. Both Tilly and Stinchcombe reason in this way very effectively in their books. What I attempt in *Logics of History*, and most explicitly in my final chapter on "refiguring the 'social' in social science," is to work out an account of social life based on what is often called "interpretive method." Interpretive method, I argue, is based on very different explanatory protocols – what I call "paradigmatic explanation," which explains patterns of human action by specifying the paradigms or codes (for example, linguistic, aesthetic, scientific, rhetorical, or kinesthetic) that enable actors to produce them. I believe that the study of culture, in sociology as in other fields, is necessarily based on this form of explanation. I therefore think that making it explicit as a distinct explanatory method rather than trying to shoehorn cultural interpretation into standard sociological

methodological categories could be both clarifying and liberating.

But I also argue that paradigmatic explanation, which enables us to understand semiotic practices, is not fully adequate to the task of social analysis. This is because it is basically a synchronic method and therefore fails to capture the enduring temporal effects of semiotic action – among others, the “incremental effects, simultaneous effects, environmental effects, and unconscious effects” that Chuck claims I ignore. Far from ignoring these, I take them into account as “built environment” logics that are dialectically intertwined in real social processes with semiotic logics. I do not, as Chuck claims, “substitute” the metaphor of built environment for that of semiotic practice. Rather I claim that understanding the unfolding of social life requires us to operate on more than one causal register simultaneously. In short, I do not ignore the philosophical problem of explanation in *Logics of History*. But I treat the problem in ways that differ quite dramatically from the assumptions of standard sociological methodology. I continue to regard this not as a weakness but as a distinctive strength of my book.

Rejoinder: Stinchcombe

Unfortunately I learned about time first in calculus, where *dt*, an instant of time, went into the denominator. If things were changing fast, the numerator, though just an instant, was big compared to the denominator. So in my examples of cultural change, I pointed to cases like Traugott’s or Sahlins’s where culture was changing very fast. I have also used my favorite *longue durée* fact, Le Roi Ladurie’s presentation of the gradual evolution of varieties of grain, first adapted to the Mediterranean, evolving so as to tolerate colder and colder latitudes in France, until the Seine-Loire plain became the breadbasket and Languedoc had to go over to grapes and chestnuts: time drenched. But what Sewell is talking about is, it seems to me, time that is itself meaningful, such that different times differ because what happened over time changed basic meaning structures. The definition of the meaning of time, because of the events that could only happen in time, then has causal impact.

Thus when I used one of Sewell’s examples of the relation between the riot that came to mean liberation from the coercion of the Bastille and the reorganized Estates General that was a republican alternative government, to show how Giddens ought to have written the last half of his *Constitution of Society*, I was talking about how the crowd changed the legitimation process of the new government, but could never be an alternative government because crowds were not sufficiently extended in time and space. I was arguing that Giddens was casting away the value of his revisions of social theory inspired by Goffman and Garfinkel to produce structuration, because he was not looking for examples that would show its power. I argued in the discussion after the 3-way critique presentations at ASA that the introduction of the protesting crowd as a legitimator of governments was a big cause of a lot of things in the revolution, many of them wonderfully analyzed in Markoff’s *Abolition of Feudalism*. Many of them involved communication between crowds and people in the successor representative bodies to the Estates General.

So the argument takes the form of one observation of the *dt* form, that meanings sure changed fast, and that it was partly because the situation of a crowd is different than the situation of a legal and administrative bureaucracy topped by a specially organized crowd who used to be summarizers of the *Cahiers de Doleances*, and were becoming legislators in communication with protesting crowd of various descriptions throughout France: a France where the food supplies to the Paris were bread grains grown on the Seine-Loire plain because of a slow creep north of the genes of grains, a low numerator and big denominator, the *dt*, in miles per century.

The brief summary then is that I don’t see any conflict between my simultaneous holding to timeless causation (because the time is in the denominator, and the cultural change is in the numerator) and time-drenched causation. But you still need the causal mechanism that makes meanings go on into the future, that makes the conjoint meaning of a riot and of an alternative government give a definition of what a revolutionary government is all about.

Now briefly to Tilly's worry about whether there is anything systematic about the relationship of deep strong ties and their structure and democracy. I suppose the biggest democratic revolutions in the United States were, on the one hand, the rebellion of the whites-only democracies of the Southern States, resolving their differences in one state after another (leaving out a few border states), and the counter-revolution, and the second biggest the Abolitionist movement and its allies in the North, eventually turned into democratic support for a war to bring them back in as "democratic" non-slave provinces again. There were as many conflicting interests in the North as in the South, but there too Civil War policy was largely resolved with democratic trucking and huckstering among the interests, and anti-draft riots, and the rest.

One of the facts that seems to me to show the relevance of Tilly's argument about the important, but ambiguous, relation between democracy and deep strong social networks is that after the civil war, we find the college fraternities that had been of national scope breaking apart. The short histories of many fraternities show that they were organized in Virginia; Robert E. Lee was in charge of a college in the same state. Surely it was hard for defeated Southern young men (no sororities then, I think) to be brothers with victorious Northern young men. From Cincinnatus to the Post WWI *Freikorps* to the Four Insurgent Generals of the right wing half of the Spanish Civil War, such remnants of camaraderie have given trouble to many governments, not always "democratic."

I of course agree with Tilly that besides knowing a lot, one has to forget a lot. I argue that the chief value of getting the cause of something exactly right is that, for the effect in question, we can forget the rest of the facts that got us to it. After Newton's straightening out gravity, we can forget all Kepler's tedious facts about orbits. And in research it's a good writing method to get some parts of your argument organized into paragraphs, so you can arrange a subhead within the paper remembering only four or five topic sentences, and then a chapter or a paper can be organized by thinking only about five or six subheads, and a book or research program by organizing only five or six chapters. Nearly all of the facts in the non-topic-sentences can be forgotten.

I guess then what I really meant by the motto, "Know a lot," was that you should have a lot of half-digested facts, not so theorized that they can be used only one way. I had thought a lot about the problem of drovers' roads in Scotland before I could remember *and use* the fact that when the Scottish government was trying to create gun control a couple or three centuries ago, the central government insisted that drovers (they herded sheep south to be fattened and butchered in England) were permitted to carry pistols, while the farmers along their routes were not. How that relates to the theory of federalism takes about 15 or 20 pages to explain. Briefly if drovers have to drive the most valuable capital and consumption resource of a region through many villages and provinces of strangers, they need to have immediate response to raiders, and defense of their property rights in the cattle, regardless of village and provincial government interference. So the central government "had to" have the final say on legitimacy of drover property and its defense. Once I know that theory, many of the facts about who in the lower classes could carry weapons became irrelevant. Now I can bring up the fact from its theoretical pigeon hole to mystify the reader.

As I have commented elsewhere, an antelope needs good peripheral vision and eyes on both sides of its head to notice the lion that might attack; the lion needs its eyes to the front, so that it can aim exactly where it can break the deer's neck. Similarly knowing a lot with one's peripheral vision has very different virtues than knowing exactly the occupational composition of the contending organizations in a French Revolution, that Traugott needed to sort out. I hope he, too, doesn't remember the details. So knowing a lot *within reason* is the correct version of the motto.

Rejoinder: Tilly

The author-meets-author format works best – or at least most entertainingly -- when authors who visibly vilipend each other all qualify as sages. Hence my double apologies to the historical-comparative audience. First, as my original commentaries indicate, I have long held colleagues Sewell and Stinchcombe in high esteem; I'm not going to start

stomping on them now. Second, for a bit longer, I would like to hold off becoming a sage: "A person, usually an elderly man, who is venerated for his experience, judgment, and wisdom," says my desk dictionary. Please help me delay old age! Nevertheless, I appreciate the opportunity to respond one more time to the sage publications of friends Stinchcombe and Sewell.

In reply to Arthur Stinchcombe, can we be sure that knowing a lot is always better than knowing less? Remember Borges's wonderful character Funes el memorioso. He remembered everything, and as a result could do nothing. All of us have known specialists and enthusiasts who knew so much about their subjects that they paralyzed themselves and anesthetized their audiences. (I hasten to add that neither applies to Stinchcombe and Sewell.) We can be thankful that evolution built selective purging of memory into our nervous systems.

More important, all knowledge of the kind that Stinchcombe praises resides within mnemonic frameworks, sometimes including concepts. It took a long time, for example, before England's historians understood that much of the countryside de-industrialized during the 18th and early 19th centuries, hence that industrialization did not consist mainly of manufacturing's intrusion into previously bucolic regions. We can't remember such things without concepts such as protoindustrialization, proletarianization, and capital concentration.

But those concepts in their turn easily become blinders. My hard-won knowledge of the French Revolution and Napoleon helped me notice something fishy in Stinchcombe's original account of Napoleonic nationalism and military service. The Sewell book under discussion, however, complains that my fixation on the forms of collective claim making leads me to underestimate the eventful impact of the early Revolution on subsequent French political history. I deny underestimating the Revolution's impact, but admit that my favored concepts draw attention to different continuities and discontinuities from those spotlighted by Sewell's favored concepts. In my own work on the subject, I am trying to explain how, why, and when the prevailing means of making claims change, not how, why, and when prevailing understandings of the past make significant shifts. As

Stinchcombe points out, nevertheless, when Sewell turns to rates of change of prevailing schemas, he arrives at an argument resembling those that Ann Swidler offers for toolkits and I offer for contentious repertoires: All three of us see our objects of explanation as far more subject to rapid innovation and transformation in times of extensive political struggle.

Sewell, Stinchcombe, and I do not, however, agree about everything. Stinchcombe and Sewell offer contradictory evaluations of my approach to trust networks and democratization. For Stinchcombe, it looks plausible that segregation of trust networks from public politics inhibits democratization or even promotes de-democratization, while integration of those same trust networks into public politics promotes democratization. He generously interprets the argument as an application of his third methodological principle: "Go after the big things in people's lives, life and death and reproduction, that move people to change their culture and social organization." Yes, the junction between trust networks and public politics focuses network members' hopes and fears on the performance of governments and major political actors, which doesn't guarantee democracy, but promotes democratic participation when other processes favor it as well.

Sewell voices greater doubt than Stinchcombe on this point. In fact, Sewell agrees that untrusting publics undermine democracy. But he challenges the *Trust and Rule* account of the relationship in two regards. First, he claims that few trust networks survive proletarianization and bureaucratization, and thus even remain available for integration into democratic public politics. Second, he suggests that democratic trust does not pass through interpersonal networks, but consists of more generalized and impersonal forms of relationship between citizens and states.

By no means does *Trust and Rule* argue that the same trust networks (or even the same types of trust networks) generally survive democratization. On the contrary, it uses the cases of Ireland, Mexico, and major episodes of de-democratization elsewhere to demonstrate transformations of trust networks in both directions. But the book does make three claims on which Sewell and I appear to disagree:

- that in democratizing regimes people continue to pursue a wide variety of consequential long term activities within trust networks, with kinship and religious solidarities prominent among them
- that new and altered forms of trust networks – for example, the integration of workplace relations into trade unions and the creation of ties between providers and recipients of welfare – emerge in democratizing regimes
- that integration between both surviving and newly emerging trust networks, on one side, and public politics, on the other, promotes democratization

So much the better. Our disagreement establishes that the claims are not trivially true. Other scholars can thus bring their detailed knowledge to bear on the controversy.

As it happens, I wasn't completely satisfied with the demonstration of the relationship in the chapter of *Trust and Rule* that Sewell singles out. Partly for that reason, I have written another book containing a more extended treatment of democratization and trust networks. *Democracy* (Cambridge University Press, 2007) returns to Ireland, Mexico, and salient instances of de-democratization. But it also offers the United States, Argentina, and Spain as cases in point. My persistence, to be sure, does not prove that my arguments are correct. It may merely establish that my conceptual blinders have grown larger. Let other scholars – notably including comparative-historical sociologists – join the fray.

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Identities

Comparative-historical scholars reflect on how and why they entered the subfield.

On Becoming a Comparative-Historical Sociologist

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The invitation to contribute an essay to *Comparative & Historical Sociology* on my identity as a comparative-historical sociologist compelled me to reflect on something I don't usually think about: How does the *kind* of work that I do inform my *identity* as a sociologist? Is "doing" comparative and historical sociology akin to "doing gender" (West and Zimmerman 1987)? Is "comparative-historical sociologist" a role we consciously enact to assert the kind of work we do and distinguish ourselves from other kinds of sociologists? Or is being a comparative-historical sociologist an identity forged through a process of specialization (Becker 1981)? Do we invest in those questions and methods from which we expect, based on our comparative advantages over other sociologists, to reap the greatest reward in the academic marketplace? Or, do we become comparative and historical sociologists through bargaining with other sociologists – our partners in the production and reproduction of knowledge? Do we engage in a continual dance of negotiation with scholars who have *other* specialties, dividing up the work in a more or less egalitarian manner based on our own collective attributes and our competing options outside the walls of academia (Lundberg and Pollak 1996)? I think that the answer, for me, is all of these, and it is none of them. Frankly, I had more difficulty coming to terms with the "sociologist" part of this identity than the "comparative-historical" portion. After years resisting the requisite master identity, "academic," I now embrace it.

Rewind to the late 1980s. I was an undergraduate at UC Santa Cruz, a bucolic, radically left-wing campus 90 minutes from San Francisco. I bounced around academically, finally settling on a double major in women's studies and health – an explosively political combination during the hey-

day of street battles over reproductive rights and federal HIV policy. Although I enrolled in as many history classes as possible, and seemed always to orient my term papers to incorporate an historical perspective, in the final analysis I didn't want to *read about* history. I wanted to be in the thick of the fight, *making* history. Despite being inspired to embark on this work by my *own* professors' teaching, research and activism – including Nancy Stoller, Pam Roby, Gwendolyn Mink and Bettina Aptheker, and my thesis advisor R.W. Connell – I could not imagine "making a difference" within the walls of academe. Although these scholars' work passionately engaged questions of inequality, sexuality, race and gender – all issues about which I cared deeply – I did not yet sense that my place might be among them. I spent a decade working in family planning, reproductive rights, HIV/AIDS, and tobacco control, defining myself as a feminist and social justice activist. Not an academic. Not a sociologist. And certainly not an historian.

I began the process of becoming a comparative-historical sociologist during my first year of graduate school, before I understood there were ways to "do sociology" that did not concern themselves primarily with big ideas and big institutions, with change across time and space. It was early 2002, a few months after hijackers crashed into the World Trade Center, bringing what many claimed was the logical outcome of U.S. foreign policy crashing through our television screens and into our collective consciousness. It was also the historic moment when the first cohort of former AFDC recipients was at risk of losing their TANF subsidies, and Bush II appointee Tommy Thompson captained the domestic policy ship. The sense that we were living through a time of historic social change was palpable, and here I was, in my early 30s, diving headfirst into a second career. I had at last made peace with the part of me that yearned to orient my life toward the pursuit of knowledge. My new identity was being formed. I could finally envision myself as an intellectual and not "just" an activist. But how did I know what *kind* of sociologist I wanted to be? Was there a way to develop a research agenda that would be

scientifically rigorous and simultaneously allow me to maintain a sense of relevance?

Perhaps paradoxically, given my activist bent, I began taking traditional demographic coursework. Midway through the second class – Fertility and Mortality, taught by Stewart E. Tolnay – inspiration struck. We were discussing Demographic Transition Theory and fertility decline in the historic European context. I noticed that no country's fertility had declined before it experienced a democratic revolution (van de Walle and Knodel 1980). Given my training in feminist thought, this seemed to me a fruitful link, incorporating the Enlightenment's redefinition of the individual, feminist work interrogating the role of reproduction in women's lives – particularly the relationship between fertility control and women's social and political citizenship (thank you, Ann Orloff (1993) and Sheila Shaver (1993-4), among others) – and the Second Wave feminist mantra, “the personal is political.” It just seemed to make sense. I naively asked about the body of literature discussing the relationship between political structures and fertility regimes and was shocked to learn that it was rather limited. Hence, I identified the topic for my first piece of original research, my M.A. thesis, a much-revised version of which received last year's Reinhard Bendix Graduate Student Paper award.

I had also found my mentor and cemented my identity as a comparative-historical sociologist, at least in the eyes of my colleagues. In order to convince myself, I had to actually build a research agenda and reflect on the character of my work. Would comparative-historical work allow me to feel relevant? If I was to don this cloak, it needed to fit. Indeed, it does. My dissertation project explores the changing relationship between spatial and social mobility for U.S. veterans from 1950-2000. I am particularly interested in the mechanisms through which veteran status influences life chances under different staffing policy regimes – universal selective service and the All Volunteer Force – and varying levels of policy commitment to veterans. I pay explicit attention to the way that this relationship might diverge for black and white men, in light of the distinct processes that have historically selected members of different racial categories into military service. With Stew Tolnay and his original collaborator, E.M. Beck, I

In many ways, doing comparative-historical work is very much like “doing gender”: my contributions often reflect the things I know others expect from someone who does this kind of work.

am also creating a data source that identifies and incorporates census records for individuals included in the Beck-Tolnay inventory of lynch victims (Beck and Tolnay 2004). When completed, the database we are compiling will help us better understand racial violence and hate crimes in the United States, and restore individual victims' identities to the study of mob violence. Both projects seem to fit with both my residual identity as an activist and my new identity as an intellectual. They braid together theory, data, and methods from a variety of perspectives, spanning decades of social change. In short, both projects are firmly embedded in the practice of comparative-historical sociology and both seem to matter.

So how *do* I, as a still-developing scholar, understand my identity as a comparative-historical sociologist? Does that differ from the identity of someone whose work falls within another disciplinary subfield? In many ways, doing comparative-historical work is very much like “doing gender”: my contributions often reflect the things I know others expect from someone who does this kind of work. I am the one who raises questions of historical perspective or counterfactual examples when attending colloquia, reviewing manu-

scripts, or providing my colleagues with feedback on their work. Why else would *my* input have been solicited? In other ways, this identity feels like something I do because I know how to do it. As time passes and my experience grows, it is likely that my research trajectory will exhibit more than a degree of path dependence: my specialization with a certain set of questions and approaches will allow me to attain a higher level of productivity if I stick to what I know best. And of course, the process of peer review guarantees that our work and the parameters within which we must operate are defined through a process of negotiation. These are all nice, tidy, academic explanations. But when I get right down to it and am honest with myself, I chose to do this work and to claim the identity “comparative-historical sociologist” because it just feels right.

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The Historical Sociologist as Outsider

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A major debate in sociology is about the relative advantages of insider and outsider knowledge. On the one hand, an insider – someone from within the society – has an understanding of it that facilitates access and the interpretation of social action. On the other hand, an outsider – someone from outside the society – has insights that only a new and different perspective can bring. From a philosophical and methodological point of view, I could argue the advantages or disadvantages of either. However, from a personal perspective, I prefer the position of an outsider.

Some sociologists come to their subject from a desire to know about the forces that shaped their lives and thus they bring insider knowledge to their academic research. Their personal attributes shape their professional lives. Of course, my personal attributes shape my academic research as well, but my background (suburban working-class neighborhood north of Seattle) convinced me that I wanted nothing to do with the conservative, chauvinist, ant-intellectual climate where I grew up. All I wanted was out! I certainly did not want to study it.

Instead, I wanted to know about something else, anything else.... This has been formalized in my choice of sociological fields, historical sociology, and in my choice of research topics. I am interested in finding out about times and cultures that are different from my own. In a discipline that often takes for granted that whatever happened in the U.S. in the past five years is of utmost importance, I am drawn to the foreign. Since difference (and here Mill had it right; cf. Emigh 1997) is the best way to illustrate arguments, and since the past is always a referent for the future (Emigh 2005a), I believe we often learn more about ourselves by studying others. But my modest upbringing is still apparent: my work often pushes a view from below, the way that ordinary individuals, not elites, affect social outcomes. I often use microhistorical techniques or historical ethnography to try to cap-

ture these ordinary lives (Emigh 2003b, 2005b, Forthcoming b.)

Thus, my interest in historical sociology in particular stemmed from my long standing interest in medieval history, but it was crystallized in college in a particular way. At Barnard College, where I attended on financial aid, I debated for some time about my major. I was participating in a joint program with the Columbia School of Engineering to earn a joint liberal arts/engineering degree. I did fine; but the engineering classes interested me relatively little. I loved the classes where I did research papers and gravitated towards them over time. One semester, I ran out of money, was working too many part-time jobs, and got very sick. The double load of classes was too much and I gave up the engineering ones, with some, but not a lot of discomfort. Certainly, I have never been sorry about that decision.

Several factors drew me towards sociology in particular. My then boyfriend (now husband of nearly 25 years) was a “history-sociology” major – a unique major that Columbia College offered. His major made me realize that there was a way to combine the two disciplines and he prodded me to take courses in sociology. Fortunately, this prodding landed me in Viviana Zelizer’s course, Introduction to Sociology. She taught at Barnard at the time and lectured to packed audiences in a particularly engaging and enthusiastic style. I was hooked. When I realized that she was a historical sociologist, I took a graduate course (though I was still an undergraduate) with Sigmund Diamond on historical sociology, which introduced me to documentary analysis. I loved looking up obscure documents and figuring out what they meant.

I wonder if I am the only living sociologist to have courses such as “Stochastic Processes” and “Reading Medieval Latin” on their graduate school transcript.

My decision to go to graduate school was easy (it was certainly better than working a real job, as I had had plenty of those already). I did an MA at Columbia and then went to the University of Chicago. But it took me somewhat longer to choose my first dissertation topic on delayed transition to capitalism in fifteenth-century Tuscany. The best thing for me about the University of Chicago was the lack of requirements. Since I had a BA and an MA in sociology, it seemed pointless for me to take a lot of Sociology courses. However much this damaged my reputation in the Sociology Department, I happily took History and Statistics courses and did whatever I wanted. I really did not need another MA in Sociology, so I did my MA in Statistics. I also started working with Robert Bartlett, a medievalist, on the “Frauenfrage” – the role of women in religious movements in medieval Europe, but soon realized that this was not going to be the easiest topic to combine historical sociology with quantitative methods. Since it seemed rather useless to have an MA in Statistics and write a completely qualitative dissertation, I began searching for other topics.

One day, while I was explaining this problem to Bartlett, he suggested that I get David Herlihy and Christiane Klapisch-Zuber’s (1985) book on the Tuscan *Catasto* of 1427, one of the first comprehensive cadastral surveys in Europe that collected information for the purposes of taxation. Their 1985 edition had just been translated from the earlier and longer French version. He told me, “there must be something that you can do with those numbers.” Indeed; I recognized Tuscany as an interesting case of delayed transition to capitalism – though it had a precocious economy in the late medieval and early Renaissance, the region did not experience early industrialization. Thus, it fit well into the sociological literature on “transitions to capitalism” and would be able to shed new light on this subject. My research shows how inequality between urban and rural sectors meant that when the urban capitalist market spread to rural regions, rural inhabitants no longer had sufficient economic resources to participate in what previously had been lively local markets that were linked to cultural and economic practices of property devolution and agricultural production. Thus, as capitalism spread in the presence of a high degree of inequality, it actually eroded the institutional support necessary for its maintenance (Emigh 2003a,

2005b, Forthcoming a). Though I had studied German all the way from junior high through graduate school, I began taking Latin and Italian and learning to read the handwriting in the original documents. I wonder if I am the only living sociologist to have courses such as “Stochastic Processes” and “Reading Medieval Latin” on their graduate school transcript.

Herlihy and Klapisch-Zuber’s book opened up the archives to social scientists, including myself. They created a machine readable file that contained some of the most important information from the *Catasto* of 1427, including individuals’ names and the locations where they lived. Although most projects require the researcher to return to the original documents for more information than is contained in their data, Herlihy and Klapisch’s data provide basic tools that make social science research feasible. Their data make it possible to sample systematically because it contains a list of everyone in the *Catasto* of 1427, to match other documents to *Catasto* records because it is relatively easy to search for names, and to compare and contextualize detailed research from a small area with patterns for all of Tuscany.

The rest is history....

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The Man Who Mistook Sociology for Marxism: An Intellectual Biography

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My original plan, as an undergraduate at Princeton, was to create an independent major in Marxist Studies. Politically the mid-1970s, when I was in college, were still much like the 60s. Student activism with the end of the Vietnam War turned to other foreign policy issues: the U.S.-backed coup against Allende in Chile, apartheid in South Africa, and (thanks mainly to the efforts of Noam Chomsky) Indonesia’s *anschluss* of Timor. Domestic issues absorbed less attention, although the fact that the Chairman of Princeton’s Board of Trustees was the CEO of the union-busting textile firm J.P. Stevens did receive a good bit of attention and protest from students. Princeton, surprisingly, was a fairly cozy place for leftist politics. Right wing students, of whom there must have been many, were for the most part intimidated by

the number and assertiveness of radical classmates and by the fact that the majority of students, while uninvolved in protests, held views that today would put them somewhere between Dennis Kucinich and Ralph Nader on the spectrum.

I joined in a minor way in political activities but mainly drew from the ferment a curiosity about how the bastards got away with it. Why did soldiers line up to die in imperialist wars? Why did workers put up with bad wages and alienating and dangerous labor? Even then, well before the pigishness of the Reagan and Clinton eras and still far from the unrestrained and boastful viciousness of the current administration, I was stunned at what I read in the *New York Times* (and even more so when I saw the fuller reality presented in small leftist outlets). On many days I would walk outside after reading about the latest outrages and wonder more than half seriously: Where are the guillotines?

My initial reading of Marx convinced me that somewhere in those tomes were the answers to my questions. What I found, even in *Capital*, were historical explanations. Hoping to learn more I looked, largely in vein, for history courses that addressed Marx's questions. Philosophy and anthropology were even worse. Instead it was sociology, a department well staffed with historical comparativists, that seemed more promising. Lacking sophistication, it took me a few years to understand that modernization wasn't the same as capitalism, and by then it was too late. I had graduated and was off to Harvard.

The great virtue of Harvard sociology during the late 70s and early 80s, in addition to some wonderful mentors, was the almost total freedom it gave graduate students to design and pursue their own research projects. I arrived there with the conviction that I needed to understand the origins and workings of early capitalism if I wanted to make sense of contemporary society. I embarked on a program of reading the largely Marxist debates on the transition from feudalism to capitalism. Most of that literature I found unconvincing even as it all was informative in some way. The best authors (Eric Hobsbawm, Perry Anderson, Immanuel Wallerstein) seemed to provide part of the answer, yet spoke past each other. I sought to focus my thinking by figuring out how and why

past contributors to the debate went wrong. When I found a way to understand where their explanations were incomplete or mistaken, I had a foundation on which to build an alternate analysis. At that point the substantive research and writing of the dissertation followed (relatively) quickly.

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Once I knew how I wanted to insert myself into the debate on the transition, it became clear that the first step should be a case study of the first site of sustained agrarian capitalism, England in the century between the Henrician Reformation and the Civil War. I also was able to identify a set of comparisons to failed, partial and delayed transitions: the Italian city-states, Spain, the Netherlands, and France. I first planned to include all those comparisons in my dissertation. Fortunately, my advisors eventually succeeded in convincing me to save the other cases for later publications.

My writing on the transition, which culminated in *Capitalists In Spite of Themselves*, published 17 years after I received my PhD, took me a ways from my original confidence that Marx could answer my questions. I concluded, in essence, that Marx and later Marxists asked the right questions but that the answers required a heavy dose of Weberian and elitist analysis.

I was not the first graduate student to find writing a dissertation all-consuming, and the following years of publishing coincided with raising two children. It became easy to remain single-minded in my devotion to that project and to shy away from topics with present-day political implica-

tions. I no longer was confident that my historical research spoke to the problems of contemporary capitalism that first sparked my interest in its origins. Perhaps more fundamentally, my withdrawal from active politics (except for some involvement in opposition to the US-backed Contra war in Nicaragua) stemmed from the almost unbroken series of defeats suffered by progressive forces in this nation throughout my entire adulthood.

*I still remain interested
in understanding how the
bastards get away with it.*

Once I had finished my book, I felt I had written all I had to say about the origins of capitalism. I didn't want to become one of those scholars who spends the rest of his career defending his old thesis from challengers. For some time I cast around for a new topic of research. Then George W. Bush came to my intellectual rescue. A joke of my graduate student years was that the Democrats and Republicans both were parties of capitalism; it was just that the former represented smart capitalists and the latter dumb ones. Bush seemed to be the embodiment of my old bon mot. What seemed remarkable and worthy of sociological inquiry was not Bush's own personal stupidity or viciousness but the lack until very recently of a credible challenge to his policies from any significant power base. I remembered from my historical work that previous hegemonic powers from Medicean Florence to Victorian Britain had had policy debates over how to address geopolitical and economic challenges from abroad. Most of those debates were decided by the self-interests of those elites with the structural capacity to protect their privileges, and as a result those polities followed paths that led to decline. Suddenly I had my new research agenda. I could address U.S. imperialism and the tightening grip of a tiny elite over power and wealth, the political concerns that had first brought me to sociology.

As with the origins of capitalism, I again am working on a well-studied and much debated topic. However, this time I don't feel that I need to begin by taking a stance in relation to all the debates swirling around this topic, although I am sure I will have something to say about a number of the triumphalist, culturalist, and world systemic interpretations of U.S. hegemony. Instead I can begin by engaging in comparative historical sociology, by systematically comparing recent American developments to the structural relations and causal processes I found in my historical studies of previous hegemons. The elite conflict model I developed to understand early capitalist development will inform my analysis of decline.

I have conflicting emotions as I work on this project. As a sociologist I feel enormously privileged to have a front row seat in observing a major historical transformation, and the academic articles I am writing, directed at social scientists and historians, will attempt to address the decline of the United States in rigorous analytical terms. At the same time as someone who has spent my life in a First World democracy and who would like my children to have that same option, I regard this country's present trajectory with horror. I feel a need to try to address a broader engaged public beyond academia along with my intellectual colleagues and think that can best be done in a book that presents the policy choices still open to citizens in this country. While I feel it would be intellectually dishonest to deny the structural forces that will propel U.S. geopolitical and economic decline, I do not think rising inequality and the atrophy of democracy are inevitable. The historical relationships among decline, public participation, and inequality are not automatic or unilinear. I still remain interested in understanding how the bastards get away with it, and with identifying the strategic openings for challenges to elite rule.



Work in Progress

Comparing Climate Change Policy Networks (Compon)

Jeffrey Broadbent
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Greatly reducing greenhouse gas emissions, not to speak of attaining a “sustainable society” with no net emissions, will require the radical global transformation of industrial civilization. To attain this goal, humanity will have to learn to cooperate like never before. Crisis brings opportunity. Social scientists can contribute to this transition by helping humanity understand how we have responded to this crisis, and how to respond better.

In the process of conducting extended field work on environmental politics and movements in Japan and later collecting and comparing policy network survey data, I became increasingly fascinated with networks, the policy network perspective, and the relational view of power and social processes. Accordingly, colleagues and I are designing the Comparative Climate Change Policy Network (Compon) project from that perspective.

We can only know about climate change through science. The Compon project will compare a range of nations on how they take in and use scientific information about global climate change from a common global source, the IPCC (Intergovernmental Panel on Climate Change). To be effective, issue framings, including science, must be carried by “advocacy networks,” which increasingly include global actors. However, theories of network governance raise questions about the relationship between networks, governance and democratic representation, noting possibilities of bias and co-optation. Neo-institutional and realist theories predict different impacts of global regimes upon domestic regimes and networks. Comparative research can clarify these complex causal chains.

To develop the Compon project, I received the SSRC/Abe Fellowship from the Center for Global Partnership of the Japan Foundation for 2007. I started organizing the project at the annual confer-

ence of the International Network for Social Network Analysis (Vancouver, April 2006), where several colleagues skilled in network analysis agreed to join. Spreading from that point, country-case investigators in Compon currently include researchers representing 17 cases: China, Taiwan, South Korea, Japan, New Zealand, India, United States, Canada, Brazil, England, Netherlands, Germany, Sweden, Austria, Greece, Italy, and Russia, plus global level networks as a distinct “case.” Investigators come from sociology, political science, anthropology, and mathematics. The cases represent important variation in contextual factors: institutional form, prosperity, “interest group,” social structural, and cultural. The Compon survey project will continue until at least 2010. The survey is modular, so researchers wishing to add new country cases are welcome to contact the organizer (broad001@umn.edu).

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Compon held its first conference on January 25-28 at the University of Minnesota. In the public conference, 10 speakers discussed their existing comparative social scientific research on global environmental issues, with a focus on the science-policy interface. In the following workshop, 15 network experts and country case investigators discussed how to build on existing research and design the Compon survey. Conference presentations can be viewed at:

<http://igs.cla.umn.edu/research/conferences.html>

Recent Dissertations

BECOMING A CIVILIAN: MAINLAND CHINESE SOLDIERS/VETERANS AND THE STATE IN TAIWAN, 1949-2001

Yu-Wen Fan

New School for Social Research
2006

When the Nationalists (or KMT) were defeated by the Communists in the Chinese Civil War in 1949, they retreated to Taiwan, followed by more than half a million Mainland soldiers. This dissertation explicates how the KMT state made Mainland soldiers/veterans a group of “exploited honored citizens” to consolidate its rule in Taiwan, where the economy had to be reconstructed and the majority Taiwanese were new to the KMT. I argue that it was the contradictory status of Mainland soldiers/veterans in economic and symbolic realms (poor but honored) that forged a unique trajectory for their role in state building and socioeconomic stability in post-WW II Taiwan.

The KMT’s budget-consciousness in the administration and settlement of Mainland soldiers/veterans aimed at preventing the enterprise from extracting too much from the society; by so doing, the KMT secured the support of the Taiwanese. I demonstrate this by examining marriage restrictions in the military and the entrepreneurship and thriftiness of VACRS, the government institute in charge of demobilization. As a result, Mainland veterans who lacked kinship, social networks and a common dialect with most of the people became the most economically disadvantaged group in Taiwan. Nevertheless, their economic grievances did not grow into social turmoil because of their strong emotional and ideological ties with the KMT. Dubbed as *rongmin* (honored citizens), they were the exalted group in the symbolic realm of the imagined nation created by the KMT state to claim sovereignty over the lost Mainland.

SPACE, IDENTITY AND INTERNATIONAL COMMUNITY: NEGOTIATING DECOLONIZATION IN THE UNITED NATIONS

Vrushali Patil

University of Maryland, College Park
2006

This work brings a transnational feminist perspective to the process of legal decolonization in the United Nations. Specifically, I examine colonialist and anti-colonialist debates on legal decolonization within the United Nations General Assembly (UNGA) from 1946-1960 (The UN passed its declaration initiating the onset of legal decolonization in 1960). Informed by a critical feminist perspective, my argument is twofold: 1) First, these conversations constitute the renegotiation of historic colonialist hierarchies of race, culture and nation. They are unique in that for the first time, beyond Euro-American colonialist perspectives, they also formally incorporate the voices of anti-colonialist Asian, African and other formerly dependent peoples. 2) However, occurring between elite groups of ‘colonialist’ and ‘anti-colonialist’ men, these conversations are profoundly gendered. On the one hand, colonialist speakers argue against the impetus for decolonization by insisting that still dependent and newly independent peoples are ‘childlike’ and require continued care and tutelage. On the other hand, anti-colonialist speakers respond that they are not children but grown men and that continued colonialism amounts to emasculation. They seek justice, democracy and decolonization, then, with an argument about the need to reclaim masculinity. Ultimately, I argue that these debates provide an important—yet neglected—frame for understanding the emerging masculinization of ‘postcolonial’ state- and nation-building projects and their problematic implications for women. Moreover, they also point to some of the gendered tensions in how ‘postcolonial’ states negotiate collective identity on the contemporary world stage.

“how the KMT state made Mainland soldiers/veterans a group of ‘exploited honored citizens’ to consolidate its rule in Taiwan” -- Yu-Wen Fan

“brings a transnational feminist perspective to the process of legal decolonization in the United Nations” -- Vrushali Patil

CULTURES OF SECURITY, CULTURES OF RIGHTS: SECURITY, RIGHTS ACTIVISM, AND THE GROWTH OF ANARCHISM IN CATALUNYA (1896-1909)

Suzanne H. Risley
New York University
2007

My dissertation analyzes and explains the transformation of local political culture and the legitimation and implantation of anarchism in Catalunya in the years 1896-1909. It accounts specifically for the defeat of state security projects and the emergence of a local culture of collective rights defense which coincided with and formed the basis for anarchist mobilization in the region. It offers a novel explanation for the success of anarchism which places practical action in the context of rights activism at its center. The study develops a new conceptualization of state security as a complex of discourses and practices characterized by three related dimensions: dangerousness, prevention, and exception. It finds that security measures in Catalunya in this era were not merely the province of the central state; rather, their development was embedded in civil society, in political struggles, and in the construction of local political identities. The research also speaks to current debates concerning the relationship between rights activism and collective projects of political transformation. Rights campaigns were central to the construction of a local culture of rights defense which both constrained the advance of security projects and was expanded upon by anarchists and used, in a radicalization of rights protest itself, as the basis for their own success in the region. The collective, direct defense of rights promoted by local republicans had the unintended effect of subverting their own legalist-constitutionalist rights project and laying the practical groundwork for the anarchists' radical rights vision and challenge to state-legal authority.

“The collective, direct defense of rights [laid] the practical groundwork for the anarchists’ radical rights vision.” – Suzanne Risley

THE BOUNDARIES OF CONFLICT: NARRATIVE, VIOLENCE, AND DISPLACEMENT ON THE ITALO-YUGOSLAV FRONTIER

Tammy Smith
Columbia
2006

This dissertation explores the emergence of a social boundary within a formerly unified community at the end of the Second World War. My analysis provides evidence for how individuals' identity narratives have been shaped through narrators' interaction within state institutions and social groups. I examine these processes under both democratic and authoritarian regimes confronting similar challenges to conflict resolution at the dawn of the Cold War. My dissertation focuses on the emergence of two narratives about the same historical events in Istria, a region in the northern Adriatic. Ethnically based political violence following the Second World War prompted the flight of more than 200,000 inhabitants from the region, then under Yugoslavia. Approximately one-third of those fleeing settled in Trieste, Italy. The division of this population into two states produced two dramatically different narratives about the post-Second World War period. The differences in the narratives reveal the impact of formal institutions on the development of shared historical accounts and personal memories. While offering an examination of the development of a social boundary between former friends and neighbors, my work also proposes methodological innovations to the study of narrative and life histories. I apply a formal relational perspective more commonly associated with analyses of social groups to the analysis of narrative to investigate how micro-events relate to each other to form concepts that individuals use to describe their histories. Employing such a structural approach affords a view into gaps in the narratives that have developed around politically sensitive topics since the 1950s. These silences occur in patterned ways and have structures of their own. My work shows that an understanding of silences within the narrative structure is essential for comprehending overall narrative meaning, since silences alter the meaning of events to which they are connected.

CONTESTED INCLUSION: A COMPARATIVE STUDY OF NATIONALISM IN MEXICO, ARGENTINA, AND PERU

Matthias vom Hau
Brown University
2007

My doctoral dissertation is a comparative-historical analysis of the transformation of official national ideologies in these countries during the mid-20th century. It represents one of the first efforts to systematically compare different forms of nationalism in Latin America. Furthermore, the study theorizes changes of nationalism, by tracing and explaining how national discourses evolve over time. As such, the project provides a new theoretical framework and a corrective to the relative absence of theories that explain historical transformations of nationalism--as opposed to its emergence. Through an analysis of primary school textbooks, I show that the three countries exhibited liberal nationalism as a dominant state ideology during the early 20th century. This national discourse adopted a political-territorial understanding of the nation and depicted national history as driven by benevolent leaders. During well-defined periods in each of these countries, popular nationalism replaced liberal nationalism as official national ideology. This national discourse promoted a cultural understanding of the nation and portrayed popular classes as protagonists of national history. To explain the extent and the timing of these transformations of nationalism I employ an institutional approach that calls attention to conflicts and alignments between state elites and subordinate movements, and to the timing of state making.

“one of the first efforts to systematically compare different forms of nationalism in Latin America.”
-- Matthias vom Hau

News and Announcements

Stefan Bargheer received the Nineteenth Century Studies Association (NCSA) Article Award 2007 for “The Fools of the Leisure Class: Honor, Ridicule, and the Emergence of Animal Protection Legislation in England, 1740-1840.” *European Journal of Sociology* 47:3-35.

The editors of Choice magazine have named *The Social Construction of Free Trade: The EU, NAFTA, and Mercosur* (Princeton University Press 2006) by **Francesco Duina** (Bates College) an “Outstanding Academic Title” for 2006.

Joseph O. Jewell has recently been appointed as interim director of Texas A&M’s Race & Ethnic Studies Institute. His goals for the Institute include a comparative/historical look at race and ethnicity <http://resi.tamu.edu>

Carol Schmid was recently selected to participate in the Freeman Institute for Infusing Japan Studies into the Undergraduate Curriculum, a 3 week intensive seminar on Japan at the University of Hawaii, May 20th-June 9th.

Tammy Smith has accepted a tenure-track position at SUNY Stony Brook, which she will begin in Fall 2007.

George Steinmetz received the ASA’s Lewis A. Coser Award for Theoretical Agenda Setting and was named Corresponding member of the Centre de Sociologie Européenne (Paris). In March 2007 the Ecole des Hautes Etudes en Sciences Sociales organized a conference around the work of George Steinmetz on the relations between history and sociology.

Arafaat A. Valiani was selected for an award from the Summer Stipend Program of the National Endowment for the Humanities for research on a book project, ‘Formations of Militancy: Religion, Violence, and Political Mobilization in Twentieth Century India’

New Publications of Section Members

Agassi, Judith Buber. *The Jewish Women Prisoners of Ravensbrück: Who Were They?* Oxford: Oneworld, 2007, xviii+312+ cd-rom.

Almeida, Paul D. 2007. "Organizational Expansion, Liberalization Reversals and Radicalized Collective Action." *Research in Political Sociology* 15: 57-99.

Bloemraad, Irene. 2006. *Becoming a Citizen: Incorporating Immigrants and Refugees in the United States and Canada*. University of California Press.

Boswell, Terry, Cliff Brown, John Brueggemann and Ralph Peters. *Racial Competition and Class Solidarity*. SUNY Press.

Collins-Dogrul, J. 2006. Managing US-Mexico "border health": an organizational field approach *Social Science & Medicine*, 63(12): 3099-3211.

Cottrol, Robert J. 2007. "Beyond Invisibility: Afro-Argentines in their Nation's Culture and Memory was published in *Latin American Research Review* vol 42 No. 1 February.

Deflem, Mathieu. 2007. *Sociologists In a Global Age: Biographical Perspectives*. Ashgate, Aldershot, UK.

Eastwood, Jonathan. 2006. *The Rise of Nationalism in Venezuela*. Gainesville: University Press of Florida.

Ejiogu, E. C. "Historical Statistics as Text: Unusual Indicators of How Ordinary British Folk Reaped from the World-Economy, 1800-1960," in *Journal of Asian and African Studies*, Vol. 42 (1) Pp. 73-115.

Ejiogu, E. C. "Colonial Army Recruitment Patterns and Post-Colonial Military Coups d'Etat in Africa: The Case of Nigeria, 1966-1993". Forthcoming in July 2007 in *Scientia Militaria: South African Journal of Military Studies*.

Ejiogu, E. C. 2006. "Projecting the Future: Some Implications of a Transformed US Military for the Third World," in *The ISA e-Bulletin* # 3, spring.

Ejiogu, E. C. 2006. "State building in Pre-Colonial Sub-Saharan Africa: The Case of Yorubaland," in *Political Power and Social Theory* Vol. 18, spring Pp. 3-40.

Hopcroft, Rosemary L. and Dana Burr Bradley. 2007. "The sex difference in depression across 29 countries." *Social Forces*. Forthcoming in June.

Jansen, Robert S. 2007. "Resurrection and Appropriation: Reputational Trajectories, Memory Work, and the Political Use of Historical Figures." *American Journal of Sociology* 112(4): 953-1007.

Jewell, Joseph O. *Race, Social Reform, and the Making of a Middle Class: The American Missionary Association and Black Atlanta, 1870-1900* Rowman & Littlefield.

Johnston, Hank and Paul D. Almeida. (eds). 2006. *Latin American Social Movements: Globalization, Democratization, and Transnational Networks*. Lanham, MD: Rowman and Littlefield.

Kalberg, Stephen. 2006. "Ascetic Protestantism and American Uniqueness: The Political Cultures of Germany and the United States Compared." Pp. 231-29 in *Safeguarding German-American Relations in the New Century*, edited by Hermann Kurthen, Antonio V. Menendez-Alarcon, and Stefan Immerfall. Lanham, MD: Rowman and Littlefield.

Khalidoun Samman. *Cities of God and Nationalism: Mecca, Jerusalem, and Rome as Contested World Cities*. Paradigm Publishers.

Lopez-Alves, Fernando, and Diane E. Johnson. 2007. *Globalization and Uncertainty in Latin America*, Palgrave Macmillan.

Loveman, Mara. 2007. "Blinded Like a State: The Revolt Against Civil Registration in 19th-Century

Brazil" *Comparative Studies in Society and History*.*/ /*49[1]: 5-39.

Mukerji, Chandra. 2006. "Printing, Cartography and Conceptions of Place." *Media, Culture and Society*. 28 (5).

Mukerji, Chandra. 2007. "Stewardship politics and the control of wild weather: Levees, seawalls, and state building in 17th-century France" In *Social Studies of Science* Vol 37 (1) Feb.

Mukerji, Chandra. 2006. "Tacit Knowledge and Classical Technique in 17th C France: Hydraulic Cement as Living Practice Among Masons and Military Engineers." *Technology and Culture*, October, 47:713-733.

Mukerji, Chandra. 2007. "The Great Forestry Survey of 1669-1671: The Use of Archives for Political Reform." *Social Studies of Science*. Vol. 37 (2) April.

Pickel, Andreas. 2006. *The Problem of Order in the Global Age: Systems and Mechanisms*. New York: Palgrave.

Ram, Uri. 2007. *The Globalization of Israel: McWorld in Tel Aviv, Jihad in Jerusalem*. Routledge.

Roopnarine, Lomarsh. 2006. *Indo-Caribbean Indenture: Resistance and Accommodation*, Kingston: University of the West Indies Press 2006

Smith, Tammy. 2007. "Narrative boundaries and the dynamics of ethnic conflict and conciliation," *Poetics* 35: 22-46.

Steinberg, Marc W. 2006. "Unfree Labor, Apprenticeship and the Rise of the Victorian Hull Fishing Industry: An Example of the Importance of Law and the Local State in British Economic Change," *International Review of Social History*. 51, no. 2: 243-276

Stillerman, Joel. 2006. "The Politics of Space and Culture in Santiago Chile's Street Markets." *Qualitative Sociology* 29, 4 (December): 507-530.

Valiani, Arafaat A. 'Cultural Heritage and the Making of Militant Political Subjects in Twentieth Century Indian Nationalism-A Preliminary Outline' *Interventions*, forthcoming in 2007.

Valiani, Arafaat A. 'Violence', Entry in the *International Encyclopedia of the Social Sciences*, (series editor) William A. Darity, forthcoming in 2007.

Walters, Barbara R., Vincent Corrigan, and Peter T. Ricketts. 2006. *The Feast of Corpus Christi*. University Park, PA: The Pennsylvania State University Press.

Woodberry, Robert D. *forthcoming*. "Pentecostalism and Economic Development." in *Markets, Morals, and Religion*. New Brunswick, NJ: Transaction Publishers. Jonathan B. Imber (ed.)

Call for Papers

Political Power and Social Theory is a peer-reviewed annual journal committed to advancing the interdisciplinary understanding of the linkages between political power, class relations, and historical development. The journal welcomes both empirical and theoretical work and is willing to consider papers of substantial length.

Publication decisions are made by the editor in consultation with members of the editorial board and anonymous reviewers. Potential contributors should submit manuscripts in electronic format to ppst@mit.edu. Potential contributors are asked to remove any references to the author in the body of the text in order to preserve anonymity during review.

Email: ppst@mit.edu
<http://web.mit.edu/dusp/ppst/>

Diane E. Davis, Editor
 Professor of Political Sociology
 Massachusetts Institute of Technology
 77 Massachusetts Avenue #9-521
 Cambridge, MA 02139

Conference

The Thunder of History: Taxation in Comparative and Historical Perspective

May 3-5, 2007

Northwestern University
Harris Hall Rm. 108
1881 Sheridan Rd.

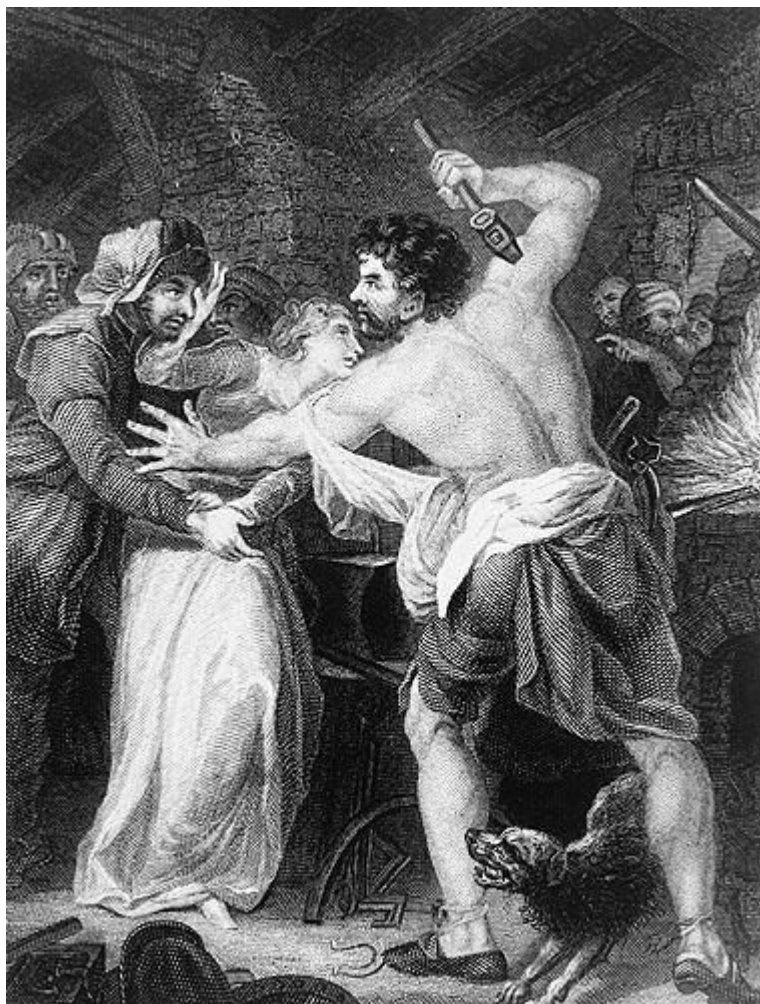


Image: Wat Tyler killing the poll tax collector.

Keynote Speaker: Charles Tilly

Fred Block
Elliot Brownlee
Andrea Campbell
Robin Einhorn
Chris Howard
Edgar Kiser
Evan Lieberman
Isaac Martin
Ajay Mehrotra
Beverly Moran
Monica Prasad
Joel Slemrod
Nancy Staudt
Joseph Thorndike

“The spirit of a people, its cultural level, its social structure, the deeds its policy may prepare -- all this and more is written in its fiscal history, stripped of all phrases. He who knows how to listen to its message here discerns the thunder of world history more clearly than anywhere else.” (Joseph Schumpeter, 1918)

<http://www.tgs.northwestern.edu/facultyandstaffinfo/facultyconferences/thunder/>

The Thunder of History is being held in conjunction with Northwestern’s Program in **Comparative-Historical Social Science (CHSS)**. For more information please contact Elisabeth Anderson at: elisabeth@northwestern.edu. Sponsored by: The Graduate School, the Tax Program at Northwestern Law School, Weinberg College of Arts and Sciences, Northwestern’s Institute for Policy Research, Northwestern Sociology Department, and the ASA Fund for the Advancement of the Discipline Award Supported by the American Sociological Association and the National Science Foundation.

In the next issue of the Comparative and Historical Sociology Newsletter:

**Nitsan Chorev
(Brown University)**

and

**Greta Krippner
(University of Michigan, Ann Arbor)**

take over as newsletter editors!

