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Comparative & Historical Sociology

The newsletter of the Comparative and Historical Sociology Section of the American Sociological Association.

Contents & Essays

A Note from the Chair

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The Section of comparative and historical sociology continues to strengthen its roots and to branch out. This progress has only been possible through the dedicated efforts of its many members and especially the officers who made it all happen. On behalf of all the members, I want to thank Anne Kane (University of Texas, Austin) for her tireless and efficient service as Secretary-Treasurer for the last three years. In every `crisis' in the day-to-day operation of our Section, Anne was there to help us to untangle the situation. We are pleased to report that Ming Chen-Lo (UC-Davis) agreed to take over this challenging responsibility for the coming three years.

Another significant transition was a change in the editorship of the Section Newsletter. J.I. (Hans) Bakker (University of Guelph) has been the editor of the Section Newsletter in the period 2000-2002. Hans showed his superb editing skills in the six issues of the Newsletter that he produced. He further turned the Newsletter into a well-read intellectual forum for the Section through style as well as content, inviting a diversity voices from the field to contribute. We are grateful to Rosemary L. Hopcroft (University of North Carolina - Charlotte), for agreeing to take over his position. This issue of the Newsletter is the first one under her editorship. As you can read below, she has collected several noteworthy discussions presented in the panels from the last ASA annual meeting.

Amidst all these transitions, I am happy to note that Mathieu Delflem (University of South Carolina) is continuing to serve us as the able webmaster of the Section's Homepage. Thank to his dedicated efforts, the section Homepage has grown into a full-fledged communication center. It now includes awards and history, an online Newsletter, notices of meetings, a publication corner, a member's area and a student center. This well-constructed homepage is becoming an envy of other sections. If you have a new publication or award you would like to be posted, please send it to him; see http://www.comphistsoc.org/.

Since this issue of the Newsletter is already very rich in content, we will include in the next issue, in June, information on the Section activities at the coming ASA meeting in Atlanta, August 16-19, 2003. For now, I would like to add a brief personal note, given the relevance of that meeting's general theme "The Question of Culture". Obviously, this theme directly speaks to our main concerns in comparative historical sociology. Recently, I have noticed how more and more section members are attempting to incorporate culture into their research programs on social change. While I encourage this trend in general, I also would like to voice some caution. There is always the danger of presenting an over-generalized notion of culture as coherent morals and values, at the risk of being interpreted as viewing the world at large in terms of "clashes between civilizations".

I am looking forward to meet many of you, and to have lively conversations among Section members in Atlanta.

Commentaries from the 2002 ASA Author Meets Critics session on: Capitalists in Spite of Themselves: Elite Conflict and Economic Transitions in Early Modern Europe, by Richard Lachmann. New York: Oxford University Press, 2000.
Winner of the 2003 ASA Distinguished Scholarly Publication Award.

Comments by Julia Adams, Samuel Clark, Rosemary Hopcroft, and Edgar Kiser, with a reponse by Richard Lachmann.

Materialists in Spite of Ourselves?

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Richard Lachmann's Capitalists in Spite of Themselves: Elite Conflict and Economic Transitions in Early Modern Europe is a superb book. It's tempting to assume that "we all know that" – it's won important prizes, after all! – and to leap directly into critique, as historical sociologists are wont to do, so I want to begin with an explicit appreciation of Lachmann's achievement. Capitalists in Spite of Themselves synthesizes and extends elite theory and Marxian class analysis in a remarkably inventive way. It provokes debate by proposing a new “elite driven” motor of macro-historical change, and this in an era of increasingly timid sociological claims. The book engages the perennially challenging question of the causes of transition from feudalism to capitalism in early modern Europe by means of detailed accounts of differences among and within regions of Europe. The geographic span includes the-countries-afterwards-known-as France, England, Spain, Italy and the Netherlands. Capitalists in Spite of Themselves is historically rich, theoretically rigorous and architecturally elegant. It even has a certain dry wit. Small wonder that it’s been laden with laurels.

Take, for example, the exemplary chapter on state-formation in England and France, which begins “Something began to happen in the sixteenth century in the most unlikely places” (p. 93), and ends with the conflagration of the French Revolution. Lachmann argues that elite – not class – conflict was the prime mover in the transformation from feudalism to capitalism and a modern state system. He agrees with Max Weber (1958) that the Protestant Reformation was the key turning point in this double development, but not because of the roster of reasons, including the Protestant Ethic, that Weber offers. Rather, Lachmann argues that the Reformation “opened a new cleavage in elite interests, transformed elite capacities” and thereby shaped state and class formation in Europe. (An elite, by the way, is “a group of rulers with the capacity to appropriate resources from non-elites and who inhabit a distinct organizational apparatus” (p. 9). Elites are important because they, and not classes or individuals, are the beginnings, the first link, in the “chains of opportunity” (White 1970) in medieval social structure. And not just any elite! Only some elites can act to extend their power and autonomy. Elites are rather like Marxian ruling classes (the book draws this analogy explicitly), but elites must also defend their organizational base from other elites. Their capacities to act are similarly organizationally conditioned. Lachmann contends that the structure of elite relations in France and England affected both the emergence of their respective bourgeoisies, and their monarchs’ abilities to build different kinds of absolutist states – eventually a “vertical” absolutism in France and “horizontal” one in England, to use the book’s catchy concepts. He makes an excellent case that key chains of action began with organizationally based elites and that the relational matrix among these actors also mattered for their step-by-step transformations. One might disagree about which organizationally based elites mattered – I for one would include early modern elite families as a key platform of economic and political action -- but strongly agree with his contention that social scientists and historians should pay more attention to the organizational basis of elite action and reproduction. Lachmann’s compelling argument shows that the elite conflict plus Marxian model improves upon a “just plain Marxist” model; the former is clearly better at deciphering shifts in the interests and capacities of class fractions in early modern France and England, and also in retrospectively predicting the mechanisms by which capitalist forms and modern states are created.

Capitalists in Spite of Themselves also illustrates the great virtues of an analytic narrative approach to history. Lachmann proposes a new “motor of history,” but this is not the same as extolling an essence or master logic of history in the grand Hegelian manner. He is instead trying to emphasize one among many intersecting contingent chains of cause and effect that in principle could have happened otherwise. It is for him the principal one, however. The main agents of change in the model are aggregates of rational actors, situated in organizations, whose strategic actions are heavily conditioned by other actors, by setting, by intersecting causal chains, by the unintended consequences of previous actions. This nouvelle utilitarian argument folds into a second claim about the causally dominant role of the elite mechanism in the temporal ordering of social-structural development. “My fundamental finding is that the chains of contingent change began with elites, not classes or individuals” (p. 9). In early modern England, for example, elite conflict destroyed the capacity of the clergy to control the levers of the relations of production; only then did the actions of the English gentry shift agrarian class relations in a capitalist direction. Establishing capitalism had not been their intention when they challenged the clergy: in that sense they were capitalists “in spite of themselves” (although whether Lachmann thinks that key actors were state builders in spite of themselves is admittedly less clear to me!). Elite action is not the sole source of change in this story, for the role of English monarchs in deposing the magnates also fostered the triumph of the local gentry, and hence the rise of horizontal rather than vertical French-style absolutism. But it is the most important one, and Lachmann makes a good case for its theoretical centrality.

So I like Capitalists in Spite of Themselves very much indeed, but sometimes – in spite of myself. My resistances and hesitancies fall under two main headings, theory and epistemology, as both bear on the historical tales that sociologists tell. To start with theory, the affinities and similarities to some unworkable elements of Marxist theory run deep in this book. Elites, like classes, are deemed to have “interests,” which the author seems to take as unproblematic, and those interests are said to derive from their “structural location,” in organizations as well as modes of production. Lachmann also assumes that those interests yield individual and (when aggregated) collective goals. No significant process of mediation or translation disturbs the book’s narrative on these points. Furthermore – and here the account goes even further than most Marxian analyses – members of a specific elite “in itself” know that they’re members of that elite “for itself,” and therefore also know, when they have the requisite information, what they should think and do about advancing their so-called interests. The causal predictions in the book rest squarely on this series of conceptual assumptions. To me, however, these assertions seem tenable only in certain historical circumstances and limited cases. People at a given historical juncture may act as part of such an organizationally-anchored elite, but only under certain conditions, including cultural conditions – such as whether they are successfully appealed to and recognize themselves as belonging to the elite category in question, and are moved and able to take concrete steps on the basis of those symbolic identifications. When they do take those steps, people may engage in a huge variety of social actions – they may designate representatives, or join their voices to a crowd, or sign onto an organizational project, give money or other resources, and so on. But the character and membership of such mobilized groups do not simply emanate from a “material base,” whether we are dealing with elites, occupations, state organizations, or peasant communities. Many sociologists will probably applaud precisely these parts of the book, because they are so rigorously argued, but the associated reifications made me tear out my hair in frustration.

My second big theoretical beef is with the utilitarian cast of Lachmann’s historical narrative. "Individuals are rational maximizers" (p. 239), he flatly states, and this belief constitutes the core of his challenge to Weber's (1958) and other arguments that religion has an independent ideological role in early modern economic and political history. In my view, utilitarian assumptions are – or should be construed as – conceptual building blocks in a model – not transhistorical truths about what makes all actors tick (Adams 1999). I suppose I could (grudgingly) imagine a compromise rational-choice evolutionary argument, of the sort that would argue that those elites that did deploy religion solely as a tactical device in forwarding their economic and political interests were more successful than others that were more ideologically committed, but Capitalists in Spite of Themselves takes a more hardline utilitarian stance. Religion figures here as a tool of preformed a-cultural interests, rather than a structured system of signs that people interpret and enact in various ways. The possibility that some people actually cared about saving their own and others' souls, or believed in and were moved by the authority of the sacred, and that those sorts of stances and associated desires and anxieties might have formed their vision of politics or business, is not genuinely admissible here, and that refusal is reflected in the straw-man version of Max Weber's (1958) argument given in Chapter 7, "Religion and Ideology". This form of argument also assumes that medieval and early modern religious, economic and political styles of thought and action were far more differentiated than they actually were. "Early modern Europeans were rational about their this- and otherworldly spiritual interests in the same way they were rational about their economic and political interests. Elites and others were able to determine their immediate and local interests and were capable of identifying which allies – temporal and spiritual – and which magical or "rational" modes of action would help them to sustain their positions against their enemies" (p. 227). The historical theoretical problem – still open for consideration, I believe – involves not belief versus interest in early modern Europe, but the patterns and rhythms of their mutual constitution, relationship and disaggregation.

Finally, and be assured briefly, let me turn to epistemology. Capitalists in Spite of Themselves conveys a striking sense of security in its own causal argument, and about determinist causal argument in general. To me these parts of the book seem to be written in a foreign language – a language I used to speak fluently (as a structuralist Marxist of yore) but at which I've now become hopelessly rusty. Historical evidence is first of all inherently fragmentary and provisional. What don't we know, including about elite structure and action? What can't we know, given the available evidence, which is particularly spotty for feudal and early modern Europe? These gaps inflected the historians' work that Lachmann is discussing and deploying, but the resulting fissures are neatly bridged in his text. If I were searching for an exemplary anti-text to Umberto Eco's The Name of the Rose, Lachmann's Capitalists in Spite of Themselves would be one candidate. The clash of interpretations of history, some quite thoroughgoing, is also downplayed. (Just two examples would be historians' debates about the practices and roles of Protestants and Catholics in the Reformation, or the endless controversy over the character and workings of the eighteenth-century English state.) Capitalists in Spite of Themselves mentions such disagreements and debates, but the depth of the contention is not reflected in moments of hesitation, or any sustained consideration of theoretical counterfactuals, in the ensuing argument. I would also have thought that today's pervasive epistemological unease over the status of causal argument, over narrative analysis, or over what counts as explanation and understanding would be registered here, especially inasmuch as the book deals with longue durée processes rife with interacting vectors, variables and shifting meaning systems (Abbott 2001). In this unsettled context, I do not understand how Lachmann can so confidently assert that anything is the definitive motor of history. I eagerly await his response, in hopes that it may put paid to my own epistemological queasiness.

These large anti-foundationalist objections may sound as if I'm dismissing the book, and that is definitely not the case. All historical sociologists are contending with these same sorts of theoretical and epistemological problems. I would have preferred that the author at least acknowledge these difficulties, in part because such acknowledgments are openings to conversations with other scholars, and to ongoing interactions with the traces of history, textual and otherwise. But I can happily read and teach Capitalists in Spite of Themselves in my own way, learning from it as a brilliant comparative historical development of elite organizational mechanisms in early modern Europe. It is for me a "local" argument -- a theoretical and historical analysis of one important mechanism in one important time and place -- and one of many possible motors of history. That is the version of the book that I find most exciting and inspiring. There is a text in this class, but it's not necessarily the author's.

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Actors and states in European history

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Capitalists in Spite of Themselves is a major tour de force, which will lead scholars to think very differently than they have until now about the making of modern Europe. Lachmann effectively uses his elite conflict model to explain a number of processes in the evolution of modern Europe. Time and time again he employs this model to account for processes and chains of events that have until now been attributed to underlying conditions or forces. This book falls into the eminently respectable tradition of Barrington Moore's Social Origins of Dictatorship and Democracy. In this tradition one tries to understand social developments in terms of the complex matrix of relations among social groups and their interaction.

So impressed with this book was I that I thought for a while that I would be unable to come up with the criticisms that I have been asked to provide on a panel of this nature. Eventually, however, I thought of a few things, none of which detract seriously from the overall quality of the book.

First let me note that the Moore tradition, celebrated though it may be, is not without its problems. Richard raises to new heights the long-standing question it faces as to whether social groups - be they elites or non-elites - can be treated as actors. Richard has no hesitation in doing so, repeatedly making bold statements about merchants doing this or the gentry doing that. Indeed, he is critical of earlier works for not specifying the collective actors that determined historical developments. He explicitly rejects individuals as playing such a role. He also argues that the state was not an actor, at least not in Early Modern France and England (pp. 9, 131). For him elites were the consequential actors, more precisely elites "for themselves" as opposed to elites "in themselves". In other words, the elites that shape history are those which were collectively organized and conscious of their common interests and purposes.

In this kind of analysis there is the obvious danger of treating groups as collectively organized which are not collectively organized. In reality collective action rarely extends beyond several hundred persons, occasionally several thousand. For the most part when we assert that a social group - say the peasantry - does something what we mean is that certain people in the peasant population did something and we think that what was significant about them was that they were peasants, as opposed to other categories in which they might be placed, such as Catholics, or women, or persons in a certain age category. These and many other alternative categories have been used by Early Modern historians to help make sense of the history they are studying. Still other historians would divide Early Modern society up into networks linked together by ties of kinship, friendship, or clientelism. Thus the story one tells comes to depend very much on how one divides a society up into interacting groups.

One of the most important actors in Richard's story is the English gentry. He boldly declares that there was "a single gentry elite". At one point he refers to it as a class whose interests were defined by its new monopoly over agrarian production (p. 130), but I was uncertain if this is what he means whenever he uses the term. It is not always obvious, nor was it at the time, who was a member of the gentry and who was not. In Early Modern England there was considerable confusion over the meaning of the terms gentry and gentlemen, which were not synonymous. This does not stop Richard from making sweeping claims about the gentry as a collective actor, for example, when he writes that during the reign of Charles I "county-based gentry came together to challenge each crown effort to aggrandize itself by appropriating gentry resources or by diminishing gentry authority" (p. 113). The evidence he then provides does not demonstrate that the county-based gentry did anything as a collective actor; he merely refers to decisions of the common-law courts. Often Richard implies that the gentry he is talking about were the justices of the peace, but in point of fact justices of the peace were a very heterogeneous lot and were also notorious for their mutual antagonisms; no small number of Englishmen and English women were the unfortunate victims of petty quarrels among justices.

In the case of France he uses the term aristocracy and nobility interchangeably, typically without distinguishing between different groups within the nobility - between robe and sword, between the Parisian and the provincial nobility, or between the rich nobility and the poor nobility. Even where he does make such distinctions, it is not clear that one can speak as generally about them as he does, for example in his claim that "provincial nobles incited popular riots against royal officials in 1788" (p. 143). All of them? Or just some of them? And if only some of them, what makes their being nobles the defining characteristic? The more research that is done the more it is clear that neither the nobility of the Old Regime nor large segments of it formed corporate wholes. Nobility was not much more than a socially attributed characteristic, which certain men and women carried around with them and which entitled them to special treatment. There is a presumption that the nobility behaved as a collective actor in the estates, but this is an error; only the composition of the Breton estates made this kind of collective action possible.

Richard also equates aristocrat and seigneur, which is wrong if by aristocrats he means nobles; many seigneurs were commoners and many more had recently been ennobled through office. He is also wrong in claiming that aristocrats - again if by this term he means nobles - were the main purchasers of venal offices. The noble population in France was not large enough to buy that many offices.

Unfortunately, Richard does not define other groups that he has acting - the magnates, the peasantry, the bourgeoisie, the merchants - any better than he does the French aristocracy; nor is there any more reason to believe these groups were collective actors. Furthermore, most of the time he provides no hard evidence of what he claims his collective actors were doing. The best we usually get is a citation to an historical work. As a result the actors that walk across his stage seemed to me very unreal and illusory. I admit that the kind of analysis he is doing would be very difficult without treating social groups as actors. Perhaps what I would have liked is some explicit recognition on his part of the problem and a little more precision in the language he used.

Ever since he published several articles in ASR I have always liked Richard's concepts of vertical and horizontal absolutism, but I do think he can get carried away with them. For those of you not familiar with what he is saying, his argument is essentially that the English monarchy tamed the magnates but at the expense of leaving the local gentry in charge in the counties - this is horizontal absolutism - while the French monarchy failed to tame the magnates but undercut their power at the local level - this is vertical absolutism. One of the effective things he does in the book is show how elite conflict kept the English gentry outside of and hence capable of destroying absolutism, while in France the interests of elite groups were embedded within a vertically organized state (p. 131).

Unfortunately, like many models that make complex history easier to understand, his constructs can also over-simplify it. For example, his argument underestimates the persisting power of magnates in England. There is no doubt that the Tudors curtailed the power of the great magnates, but not to the extent that Richard implies. No less than 43 percent of English peers served on the Privy Council under the Tudors. And some of the non-peers on the Council were related to peers. Sir Henry Sidney, a councillor from 1575 to 1586, was the husband of Mary Dudley, who was closely related to three peers who also sat on the Council - her father, the Duke of Northumberland, and two brothers, the Earl of Leicester and the Earl of Warwick. Sir Thomas Heneage, a councillor from 1587 to 1595, was the husband of Mary Browne, who was the daughter of Viscount Montague and widow of the Earl of Southampton. Although a British peer himself could not sit in the House of Commons, his sons (even the heir apparent) could, as well as other dependants. And some members of the British Commons were magnates who belonged to the Irish peerage. John Cannon calculates that in 1796 no less than 120 members of the British House of Commons were Irish peers or sons of English peers. In 1784 another 68 were related by blood or marriage to peers. Many other members of the British House of Commons were clients of peers. All this is in addition, of course, to the power of the House of Lords itself.

I have a different problem with his concept of vertical absolutism. It lumps together venal officials and intendants. The role of intendants was complex. They began as special commissioners assigned to advise governors. Under Richelieu the power of governors was curtailed. It was not until after this, beginning in 1634, that intendants were dispatched into the provinces in large numbers. The intendants were not sent into the provinces to undercut the power of governors. The people whose power the intendants really undercut was that of regional and local venal officials; this includes agents of the king, such as the trésoriers de France and the élus, as well as locally elected or appointed officials, such as mayors, échevins, or agents of the estates. It also includes the magistrates of the sovereign courts. Not surprisingly, a major demand in the parlementary Fronde was the revocation of the commissions of intendants, a demand that the crown pretended to meet, though in fact commissions continued to be issued under a different name.

Why Richard still thinks that the state was not an actor in Early Modern France escapes me. Indeed, I see the intendants as one of the few groups that qualify as collective actors in this whole show. True, the socio-political group to which intendants belonged was the very group whose power they were undermining, that of venal officials, but in their commissions the intendants were closely administered by the controller general. In some recent research that I have done on intendants, I have discovered that this control was much tighter than I expected.

I also think that Richard tries to explain too much with his concept of vertical absolutism. The "chains of opportunity" created by his two types of absolutism sometimes stretch the imagination. How is vertical absolutism responsible, as he claims (p. 200), for wars, looting, armed seizures, fiscal crises, high taxes, and divided control, as well as a lack of clear ownership of land? These conditions were caused primarily by other factors; and it weakens rather than strengthens the value of his concept to try to make it do too much for him. My personal view is that the concept is most useful as a way of describing the manner in which the French crown used intendants to extend its power into the provinces. French intendants were generally from robe families with a moderate level of status and power in the Old Regime. Many of the venal officials whose power they were undermining, particularly the parlementaires, enjoyed higher social status and so it is true that the French crown used intendants to undercut the power of higher status officials. This was one of the most important characteristics of the Old Regime and Richard's concept of vertical absolutism captures it very well.

In a study of the kind in which Richard has engaged, the selection of countries and the way in which they are used are crucial. In this regard I had several concerns. The first stems from his explicit rejection of inter-societal forces. For him, just as social groups form actors, countries form wholes, which he sees as more insulated from external forces than most scholars who have treated the subject. "The processes of state formation in early modern England and France were determined primarily by internal factors. Domestic elite and class conflicts, not foreign wars or international economic opportunities, moulded the two states' capacities and relations with civil society" (p. 145). His position stands in marked contrast not only with what Gorski calls the "bellicist" interpretation of the formation of the modern state, as represented in the works of Tilly, Downing, and Ertman, but also with the work of those scholars who believe that state boundaries did not mean as much in the Early Modern period as they do today, works such as the recent study of ties among European ruling families by Lucien Bély. What we need are more works that reveal the interrelationship between domestic forces and inter-societal forces.

For the most part I have no quarrel with his selection of cases. I do wish, however, that he had said more to justify it. If so much depends on the unique configuration of elites and elite conflict, can any region be seen as typical of a category? Can Florence be said to be typical of city states, Spain of imperial states, or the Northern Netherlands of commercial states? Maybe they can, or maybe it is not necessary to his argument that they do so. In any case he should have discussed his comparative methodology much more than he does.

Finally, I have to repeat an argument that some readers have heard me make before. The comparison of England and France, on which much of this book is based, is simply not, in my view, a valid comparison. I know that it is done all the time, but how can one legitimately compare France, including Brittany, Guyenne, the Midi, and all the territories annexed in the seventeenth and eighteen centuries with England, excluding Wales, Scotland, and Ireland? England, or more precisely southern England, was a core with an extensive and heterogeneous periphery over which it ruled - most of it by the sixteenth century and all of it by the seventeenth century. If we are going to compare England with the Continent then we should compare southern England with a broad region surrounding Paris but also including the French-speaking Southern Netherlands, which socially and economically was one with northeastern France and which experienced industrial capitalism as early as England.

Alternatively, we could take into account Ireland, Wales and Scotland, and doing so would change many conclusions that are now drawn in the comparative literature. Taking into account Ireland would have led Richard to modify his argument that land security and long leases were the principal conditions for the application of new techniques, since both conditions existed in Ireland without the same agricultural innovation, though I would not want to claim that no agricultural innovation took place in Ireland. Moreover, capitalist industrialization in Ireland occurred in a region of small farms in the northeast and not in the part of Ireland, the southeast and the midlands, where agricultural productivity was highest, where we find something of a yeomanry class, and where the gentry confiscated the most agricultural productivity.

Taking into account Ireland and Scotland would have led him to modify his argument about horizontal absolutism. The English crown did not establish horizontal absolutism in Ireland until the seventeenth century and not in the Scottish Highlands and Western Islands until the mid-eighteenth century. One of the dimensions that Richard’s concepts of vertical and horizontal absolutism do not capture is the geographical dimension. Central states resembled and differed from one another in their geographical makeup and in the way in which centres interacted with peripheries and the elites in those peripheries. One can distinguish between intensive expansion and extensive expansion. In the former case a centre begins with a comparatively tightly organized area and then expands that tight control; in the latter rulers have an extensive claim over a large area, which they gradually make good by destroying or undercutting those who have rivalled their authority. The English centre achieved intensive power over southern England and eventually northern England, but had great difficulty consolidating the extensive power it enjoyed over the British Isles as a whole. The French kings, in contrast, had great difficulty expanding their intensive power from the Ile de France but gradually became masters at consolidating extensive power over a much larger area. Ironically their very success in doing so has led historians and historical sociologists to underestimate their ability as state-makers by repeatedly comparing the power they enjoyed over all of France with only a part of the territory which the English crown governed. From at least the beginning of the seventeenth century there was no such process as English state formation, just as there was no such war as the English Civil War. It was British state formation and the British Civil War.

Actors, States and Institutions

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Richard Lachmann's book is a historically rich and well researched analysis of an always important question in sociology – why some parts of Europe (namely England) industrialized and got rich and powerful before other parts.

The book is a valuable corrective to teleological and functionalist accounts that posit that it was the outcome of some inevitable progression to ever greater productivity and efficiency. Rather, Lachmann notes that there was nothing inevitable about the rise of capitalism in England, and that the progression of economies can just as easily be away from greater productivity and efficiency as towards them. One aspect of the book I like is that it demonstrates how fragile socially constructed entities like economic prosperity can be, and how what rises can just as easily fall. Recent experiences like the current economic problems in Argentina – which was so recently highly prosperous – underline this point. Successful market economies delivering sustained economic growth are social constructions, and Lachmann tackles the difficult problem of how this was achieved in England in the early modern period, when it faltered in so many other places.

His answer is that it was social relations, in particular, social relations between elites, which created the social stage for the growth of the market economy in England. So this is quite different from a purely Marxian explanation of change, which regards change as stemming ultimately from the mode of production. However, it is not a neo-Marxist approach either, that regards class conflict as the prime-mover in bringing about social change. For what is central to Lachmann is not class conflict, but elite conflict. Lachmann regards social change as the product of the outcome of ongoing elite conflicts. The victorious elites make the rules and construct the states according to their own interests. Lachmann further suggests that in pursuing their interests some elites can promote new relations of production, which in turn, can usher in a new mode of production. The rise of capitalism occurred because the victorious elite group in England was a commercial elite, who promoted capitalist relations of production, which in turn led to the rise of the capitalist mode of production. According to Lachmann, this outcome was largely unintended by the elite group responsible for it.

So in this explanation it is crucial which elite corresponding to which interest group gets the upper hand in history. Which elite will gain the upper hand in history cannot be predicted a-priori; this depends on the historical structural situation of the different competing elites, and other relevant historical factors. Accordingly, in his book. Lachmann gives an overview of the myriad competing interest groups in a number of regions in western Europe, in various historical contexts, and how the waxing and waning of the fortunes of these groups helped determine the rise and decline of states, nations and economies. Lachmann does a good job of this, and his book is historically rich and detailed.

I like the way he goes about tackling the basic problem of social change in history, and I agree with his insistence on describing social relations and social conflicts between interest groups as key to understanding all social change. However I don't quite agree with all his answers. I have two primary criticisms. The first is that, despite his insistence that elite groups just pursued their economic interest, there is a gap in that he doesn't quite explain why first the English gentry, and later the English merchants, continued to participate in the market economy once they had emerged the victors of elite conflicts in English history. Once they were dominant, why didn't they change things so they didn't have to be so dependent on their commercial enterprises? Second, I think he downplays the importance of the relations of production – especially property rights, among the producing classes.

Let me deal with the first point. Lachmann notes that the English landlords and merchants didn't want to create a capitalist economy, but their self-interested actions inadvertently created the social relations of production that fostered a change in the mode of production. I don't entirely quibble with this part of Lachmann's argument. I don't doubt that the English commercial elite were self interested, nor that they did not fully intend what occurred. However, the idea that they became capitalists in spite of themselves doesn't quite gel with the fact that, as Lachmann points out, these landlords and merchants, were now the power holders in England. They took control of the state as much as they could and shaped it in a way that served their economic interests, as elites the world over have always done. Why didn't they just change the state so they could become a rentier class, and live the life of country lords without having to make the money to pay for it? Or why couldn't they have provided themselves with government offices and other official sinecures, the way the Dutch and French elite were able to do. That is, why did they continue to involve themselves in (status degrading) capitalist and commercial enterprises at all, when they could have lived comfortably simply from revenues gained by taxing the powerless?
You might argue, somehow, that in England the change to capitalism had gotten so far out of control that the commercial elite couldn't turn back the clock. Yet as Kenneth Pomeranz argues in his recent (2000) book, The Great Divergence, living standards in eighteenth-century England were not a great deal different to those in the developed parts of Asia at the same time, but Asia never saw the so-called take off into sustained economic growth. Further, as we have seen in our own century, prosperous commercial societies can and do stagnate.

The route away from commercial involvement toward a life of economic parasitism, is, after all, as Lachmann shows, the route taken by commercial classes all over Europe. This is what happened to the commercial classes in the great cities of medieval and Renaissance Europe: in Florence and the cities of the Netherlands. In Northern Italy local landlords became less involved in farming and commercial matters themselves, and became an absentee landlord class living comfortably in the cities on their rents from the land. In the Netherlands, as time went on the Dutch commercial elite drew less and less of their income from commercial ventures, and more from their control over city government offices and salaried positions in the powerful West Indies and East Indies Companies. As Lachmann notes, they became more concerned with preserving their local power and privileges than in tending their commercial enterprises and finding new markets or new sources of supply for their commercial enterprises abroad. This backfired when others (notably English merchants) were able to move in and take over Dutch territory and commercial interests in the New World.

The English commercial elite no doubt would also have liked to have become less involved in commercial matters themselves, and have become a rentier landlord class, or been able to obtain lucrative and secure positions in local government for themselves, their friends and family. This way they would have had more time for their beloved dogs and horses. But by and large they didn't.

This is a puzzle to which there are two possible answers. First, they were a commercial class, and were so imbued with the habits and ideologies of this class that they were unable to imagine, let alone bring about, a new existence as rentiers or officials for themselves. This seems unlikely, and I don't buy it for a minute. I concur with Lachmann that culture has a convenient way of not getting in the way of the material interests of the elite.

The second possibility is that the English elite were forced into pursuing commercial activities because it was the only way they could maintain the lifestyle they desired and their local social status in an increasingly affluent society. I think this is the real essence of the difference between England and other places on the continent. The English elites could not use their elite position to become a rentier class or a class of government officials, whereas in most other places on the continent, they could, and they did.

They couldn't because although they were indeed the dominant elite in England, first, there were comparatively few concentrations of power in English society for them to control, and second, they were never able to monopolize control over all of them. The English state at both the local and central level was comparatively small in the early modern period, and its powers comparatively slight. Second, as Barrington Moore (1966) argued, there remained in England a balance of power. Thus, for landowners and merchants, their local power was balanced by the powers of the crown. In other places on the continent, there was no balance of power. In some places it was concentrated in the central state (France), in other places it was concentrated in the localities (the Netherlands and Northern Italy).

Further, given the small central state in England, even when local landowners were able to exercise considerable control over that state, the state itself was limited in its size and power. Later on, merchant adventurers were also able to exercise considerable control over the state, but once again, given the relatively meager power of the state, their power over English society was much less than say, that of the Dutch merchants, who were able to control city government, and thus the most important part of Dutch society. Thus, the English elite, although they were able to provide themselves with many privileges, were never able to obtain a comfortable and secure existence for themselves outside of commercial ventures. They were forced to remain capitalists in spite of themselves.

Let me now address my second and more minor criticism of Lachmann's book. Despite his attention to regional and national differences in elite relations, Lachmann neglects the local social relations of production involved in the emergence of agrarian capitalism. Like so many analysts before him, Lachmann describes how a proto-capitalist elite, in the interests of greater efficiency of production, imposed private property rights on a largely communal peasantry. Yet as I and others have shown, while this was true in some places, in other places there were no, or very few, communal rights to eradicate (see Hopcroft 1999). In a number of regions across Europe, (including most of the Netherlands and eastern England) there had always been few communal rights to land, and elite control had been weak as far back as historical records go. In these places, property rights (and elite relations) conducive to agrarian capitalism were indigenous, and were not imposed by modernizing elites. These regions became a source of commercially-oriented farmers and traders, as well as a wellspring of innovative new agricultural techniques and methods, and hence provided an important component of social and economic change across Europe. Many of the up and coming commercial landlords, who pushed hard in England for political change, were from such regions (e.g. Edward Coke – 17th century). Without the presence of these innovative regions and the impetus for change they provided much of this change may never have occurred at all.

In sum, I think Lachmann is right when he notes that elite conflicts were crucially important in shaping the course of European history. But at least in England, I would contend with Barrington Moore that it was not so much the resolution of such conflicts, but the fact that there was no final victor to these conflicts, and instead a balance of power between competing interest groups prevailed. This prevented the rigidification of hierarchies of power and privilege that occurred on the continent, and forced the English elite to maintain their commercial focus. A secondary point is that Lachmann presents the European rural sector as somewhat monolithic, and overlooks local social relations of production, notably property rights systems, which differed from place to place. The less-communal of these regions were an important source of both efficient techniques of production and other capitalist behaviors. Thus, while elite relations and other historically specific factors are central to any explanation of long term economic change, it is also important to look at the sources of innovation and productivity (not to mention new elites) bubbling up from the bottom.

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The Role of Microfoundations in Historical Analysis

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Capitalists in Spite of Themselves is impressive in many ways – most prominently in the scope and detail of historical analysis. Lachmann goes from feudalism to the French Revolution, covers city-states, states, and empires, and all with very detailed historical analyses -- since it's hard to develop detailed knowledge about multiple times and places, this is very rare. I learned a lot from reading this book. My comments will focus on the theoretical model that shapes the historical narratives, primarily the micro-level assumptions.

Lachmann labels his theory a structuralist elite-conflict theory. Like most other structuralists in comparative-historical sociology, Lachmann assumes that actors are rational – that their actions were the result of instrumental calculations of costs and benefits – not by sentiment or tradition. He also assumes they are self-interested, and that their most general goals are to maintain and if possible increase their wealth and power relative to both subordinates and competing elites. These are the standard assumptions of most rational choice work.

Lachmann also modifies some of the auxiliary assumptions of rational choice theory. Most importantly, and like almost all institutional economists, political scientists and sociologists who use rational choice, Lachmann assumes that actors do not have perfect information. As a result of incomplete information and the complexity of the world, "no one could anticipate or control the ultimate effects of their actions" (2000:229). Lachmann (2000:229) concludes that "none of those groups [referring to elites] could foresee the consequences of their actions upon themselves or their heirs decades and centuries later". This is the foundation for the main argument of the book, signaled in the title – capitalism arose as an unintended consequence of elite actions.

Lachmann's second elaboration of his rational choice microfoundations is his claim that most elite action was defensive or reactionary, basically seeking to preserve the rights and powers they had. This too is commonplace (although far from universal) in contemporary rational choice theory, since Kahnemann and Tversky (1979) demonstrated that people do not treat gains and losses symmetrically – they generally want to avoid losses more than they want to achieve gains. Behavioral economists like Thaler (1991) have developed these arguments further, and Lindenberg (1989) has used this argument to good effect in explaining revolutions.

These are all quite reasonable, and in fact fairly standard micro-level assumptions in contemporary rational choice theory. But Lachmann does not conclude that his ability to provide a better explanation of the rise of agrarian capitalism in Western Europe shows the utility of rational choice microfoundations. Quite the contrary, the book is full of criticisms of it -- Lachmann (2000:8) even argues that one of the goals of the book is to "highlight the limitations of rational choice theory".

This really confused me – how can rational choice be both the source of his micro-level assumptions and the object of constant polemical attacks? His actual theoretical strategy and his rhetoric just don't match. I think the main reason is that Lachmann has a narrow and outdated view of rational choice theory – he seems to see it as neoclassical economics circa 1970. In the context of neoclassical economics a few decades ago, most rational choice models did assume that actors had perfect information, and this (often along with assumptions of strong selection mechanisms), assured that markets were efficient. Lachmann hauls this tattered old straw person out of the attic, and like generations of sociologists before him, he gives it a good beating.

He notes that actors don't have perfect information, and thus they often cannot foresee what the consequences of their actions will be "decades or centuries later". Reasonable enough, but other than that old, and now even more tattered straw person, it's not clear whom he's arguing against. Most contemporary rational choice theorists dropped the assumption of perfect information long ago – this is even true in economics, where the new institutionalism has ushered in work on property rights, agency theory, and transaction costs – all of which attempt to understand the consequences of incomplete and unequally distributed information. But Lachmann virtually ignores these contemporary rational choice theorists, even those who have focused on the political economy of early modern Europe – people like Doug North, Hilton Root, and Avner Grief are not even cited. Had he addressed this work, he would have found that his microfoundations are much closer to theirs than his polemics against rational choice theory suggest.

But this is more than a labeling issue, more than the common complaint that someone has depicted my favorite theory unfairly (although I'll admit it's partly that) – after all, even though this is a pretty extreme case, many of us are guilty of turning theoretical competitors into more easily defeated straw people. The main thing I want to suggest here is that Lachmann's argument could have been improved if he had taken the rational choice microfoundations he used more seriously. If he had used the large and growing rational choice literature on things like imperfect information and discount rates, he could have made his argument about unintended consequences much more compelling.

The main problem with his argument about unintended consequences as it stands is that it's theoretically undeveloped, and thus too general. As a result, Lachmann's claims about unintended consequences range from obvious to overstated. For an example of the former, who would deny that the elites he studies could not foresee the consequences their actions might have "decades or centuries later"? Of course actors can't see clearly that far into the future. An illustration of overstatement is his claim (2000:8) that "all long-term changes were inadvertent". Moreover, Lachmann (2000:8) argues that most of these unintended consequences were negative: "rational actions were as often to the detriment of their instigators as to their benefit". If this were true, if negative unintended consequences were so dominant, it is hard to explain the fact that most elites were able to reproduce their positions most of the time – if the world were like Lachmann depicts it, chaos and upheaval should be the norm.

In effect, the under-theorization of imperfect information and the unintended consequences that follow from it lead Lachmann to treat them as rough constants. This prevents him from answering the most important question raised by his argument: what explains variations in the amount and type of information available to different elites, and thus the extent to which and the particular way in which the consequences of their actions will be unintended? Once the assumption of perfect info is dropped, we need to know how much information exists in environments, and how that information is distributed. Knowing the structure of elite relations should help with this, so parts of Lachmann's model would be useful here. For example, information flows across elites will be very different in vertical patronage-based systems than they will in systems with stronger horizontal ties – such as those formed between elites who meet periodically in national or provincial legislative assemblies like the English Parliament or the French Estates General. More specifically, the distribution of information will differ in France after the Estates General ceases to meet in the 17th century, and depending on whether or not provinces had viable provincial estates. Information flows within the state will depend on whether it is more patrimonial or more bureaucratic in structure – and will differ across types of patrimonialism as well. Although there are hints at some of these differences in Lachmann's historical narratives, they are not theorized – his elite-conflict model focuses much more on distributions of resources and organizational capacities than on flows of information.

More generally, one of the most interesting problems in rational choice theory today is that there are now many different ways to modify and elaborate the microfoundations of the model. Since the use of multiple modifications at once makes it difficult to test the theory, scholars generally use a small subset of possible modifications or extensions in any one study. But the choice of which to use is currently more art than science: there are no clear rules about the conditions under which different subsets of modifications will be useful. We have a large toolbox containing many fancy hammers and screwdrivers, but we often can't tell whether what we're working on is more like a nail or more like a screw. This issue is not explicitly raised by Lachmann – he modifies the standard rational choice assumptions in particular ways without considering alternatives. The problem with this is that there is more than one way to account for the dominance of actions oriented toward short-term consequences. As Lachmann does, we could argue that it reflects a lack of good information, making the prediction of long-term consequences impossible. However, another possibility is that actors didn't care about the long-term consequences of their actions – they may simply have had high discount rates. But again, we would not want to assume that this is a constant, that nobody ever cared about the future – we can instead begin to model the ways in which structural conditions and relations shape discount rates.

For example, different political systems produce rulers with different discount rates. Weber ([1909]1976) gives a good example in his comparison of the Roman Republic and the Roman Empire. Consuls on one-year terms were the executives in the Republic. Their short tenure gave them high discount rates, so they didn't much care that tax farmers were ruining the agrarian tax base by taking illegal surcharges from farmers, since this cost would be their successors' problem. Since rulers in the Empire anticipated longer tenure in office, their discount rates were lower, and they thus had greater incentives to abolish and/or control the tax farmers. Many other examples could be cited. Within monarchies, different systems of succession produce different discount rates – when succession goes to brothers duration of rule is shorter (since on average rulers are older when they take over) and thus discount rates are higher than when succession goes to children.

These arguments about discount rates point to an alternative causal mechanism that could explain the short-term orientation that Lachmann finds among early modern elites. They could be short-sighted due to a lack of information, as Lachmann claims, or they could simply care little about the long-term consequences of their actions, focusing only on immediate results. Since Lachmann does not develop his arguments about microfoundations, we can't tell which causal mechanism (or what combination of the two) accounts for his results.

To sum up, I think Lachmann's book both illustrates the utility of a synthesis of structuralist comparative-historical sociology and rational choice theory, but also signals possible problems with this approach. The synthesis will work well only if both parts are equally developed – in this case, the structuralist part is, but the rational choice microfoundations are not. To be fair, this problem exists on the other side as well – structural conditions are often undertheorized and thus used in ad hoc ways in rational choice models. In part, this is a simple function of scholars on each side having much more detailed information about one part of the synthesis than the other. Unfortunately, in addition to this, the animosity between the two sides is an additional barrier – it causes strange and unproductive outcomes like Lachmann feeling compelled to criticize rational choice theory even as he uses it. Getting beyond the unproductive polemics that have separated structuralist historical sociology from rational choice theory is a necessary condition for a synthesis that could dramatically improve both.

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Author’s Response

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I can say, after hearing the comments, I wish I had had the panelists edit my book. They each identify ways in which I could have made my argument clearer, more precise, and better engaged with recent theory. The latter problem is especially acute with projects that develop over many years, as this book did.

My explanation begins with the understanding that most people in most times never need to think about altering their social activities. They can follow tradition and reproduce their current positions. My book looks at moments when that was not so; when people had to make decisions to preserve their positions. My model argues that elites are faced with such decisions more than non-elites because they face two potential sources of conflict: from rival elites and class conflict from non-elites.
How do actors decide what to do? How do they decide whom to oppose and with whom to ally? My answers to those questions are the ground upon which the panelists raise disagreements.

Do elites act locally, nationally, or transnationally? It depends on who is challenging them. I found that for England and France the key challenges to landlords came from kings and clerics making national claims. Thus, the national level became increasingly important as the site of conflict, even as sharp local differences in economies persisted, as both Rosemary Hopcroft and Sam Clark find in their own work. Why do I slight local conditions in describing elite and class conflicts in England and France? I do so because national elites themselves sought, and often succeeded in eliminating local bases of resistance and differentiation in their struggles to achieve dominance. Analysis should highlight and track those actors and factors that proved to be decisive ultimately.

To focus on the specific case raised by Hopcroft and Clark in their comments: Why did the English gentry do as they did? Why didn’t they try to become rentiers again? The gentry did become rentiers in the sense that they left the management of their lands to commercial farmers. Yet, the sort of land security and long leases that emerged in England differed, in form and political effect, from those which Sam describes as typical of Ireland. English landlords created their tripartite agrarian system of landowner, commercial farmer and wage laborer to sustain their political power against challenges from two directions. The gentry-created English agrarian system spoke simultaneously to the crown and church above and peasants below in a way that Irish landlord did not need to. Once, English land tenure had been transformed, it was not easily changed again. Peer dominance in the 18th century British court, noted by Clark, did not affect agrarian class relations. Neither the peers nor the crown possessed the capacity or the interest to do so. Despite the diversion of enormous resources by favored politicians at the royal court through the 'Old Corruption,' the underlying structure of land tenure remained intact.

I refer to kings and royal elites (which in France included the intendants) rather than the state or state elites because multiple elites inhabited state-like institutions. Kings, large landlords, and nobles of various sorts all exercised state-like powers in England and France, as Sam points out. At issue is which elites gained control over which portions of state-like institutions and exercised governmental powers. Up through the eighteenth century these various elites never combined to form a single state elite. The intendants of France were key to vertical absolutism since they were positioned where they could play multiple provincial elites against one another to the benefit of the king and his court retainers and the national financiers.

I do not contend, in answer to Julia Adams' question, that the royal elites were state builders in spite of themselves. Rather, they were self-aggrandizers (as were their rivals) who often miscalculated the long-term consequences of their efforts to achieve absolute rule. Henry VIII didn't foresee that his appropriation of monastic powers and properties would redound to the ultimate benefit of the gentry. Conversely, Louis XIV's humiliating de facto bankruptcy in 1709 enhanced his power at the expense of the great financiers and prolonged the monarchy for eighty years.

Edgar Kiser shows how my argument can speak to ongoing debates among rational choice theorists. He suggests that "variations in the amount and type of information available to different elites" can be used to explain the choices made by each elite and their efficacy. I found that differences among actors in their access to information rarely affected outcomes. The one type of case where more information would have changed outcomes was in peasant rebellions. Peasants frequently made the mistake of assuming that peasants elsewhere were rebelling, and as a result staged isolated uprisings that were easily suppressed by crown or noble forces. Had they known how isolated they were, they never would have challenged their overlords.

Information had a different sort of effect among elites. Elites in early modern Europe, unlike the stock market investors of today who seem to be the archetype of the rational actor, rarely had the opportunity to use momentary information advantages to cash out their positions at the expense of less informed buyers. Elites were invested, socially and economically, for the long-term. Elites dealt with their lack of information by forming dense and enduring alliances that were hard to break, even when one party acquired new knowledge of possible strategic opportunities. The Florentines' mixing of marriage, business and office and the English gentry's linking of religious and political patronage and marriage and business ties were strategies to ensure that allies remained allies by so raising the costs of defection that information about new opportunities and better strategies could not and would not be acted upon. That is why I discount the value of information in explaining outcomes.

In finding that early modern Europeans valued allies over assets, and that they decided what to believe in part by settling upon whom to believe, I realized that learning operated differently than rational choice scholars, such as Avner Greif (1998), contend. Actors cannot necessarily embark upon more successful and rewarding strategies when they learn lessons about reliable allies, about the costs of conflict, and about the conditions of game equilibria. New strategies require structural openings. That is why I argue that elite conflict is the best predictor of changes in actors' individual choices and of transformations in the patterns of social actions and relations.

It is because elites were locked into such long-term social alliances that I make the strong claims about interests that Julia Adams, accurately, sees as paralleling Marxist contentions about class. Adams raises vital questions about how we can conceptualize both our understanding of historical change and the consciousness of the actors who actually made history. I did not believe I could address those issues in a worthwhile manner until I had established to my satisfaction the actual process of structural change. My conclusions about the processes of social change do provide a basis for reassessing the ways in which actors think about their social situations and choices. I find that historical actors think and act locally, building alliances with people they can see and with whom they can form personal ties. Far-reaching alliances and social institutions are formed by amalgamating such local networks, with the allies of allies who can cement such links gaining enormous power. Exploitation can be carried out impersonally and at a distance, which give exploiters a conceptual as well as geo-political advantage.

I recognize that the clear organizational lines that made European elites so easily self-aware of their identities and interests in the medieval and early modern eras might be much more blurred in other times and places. In such societies the cultural questions that Adams raises become much more central to historical explanation. I also am careful not to claim such clear identities and interests for non-elites. Any study of non-elites in any era must begin with the issues of symbolic identities Adams highlights. It is only because I find that elites are such a small part of a society's population, and that those elites are so clearly embedded within specific organizations, that I am able to make my very strong claims about their capacitates to identify and act on their interests (at least in the short-term).

The clear, locally based organizations within which they were embedded allowed numerically small elites to be rational about their religious interests. Certainly early modern Europeans held and acted upon powerful religious convictions. Those religious convictions were expressed through organizations that melded economic, political, familial, and other-worldly desires and interests. Elites, if not others, were able to take rational action in the pursuit of all their interests through the organizations that gave them their elite positions without having to chose among material and spiritual ends. That is the point on which I depart from Weber's view of religion as an exogenous shock to settled social relations and practices. It also is the way in which my elite model concurs with Adams' contention that "the historical theoretical problem…involves not belief versus interest… but the patterns and rhythms of their mutual constitution, relationship and disaggregation." I find the constitution and relationship in elite organizational apparatuses, and I find that disaggregation begins in elite conflict.

Finally, Adams expresses surprise that my book doesn't reflect "today's pervasive epistemological unease over the status of causal argument." Her characterization of my narrative is accurate. I believe that new theory as much as more empirical research is needed to resolve the often unsettled debates among historians. That is what sociology can contribute to history. While lack of data will always leave us with certain gaps in our understanding, new theories can reveal aspects of how people made and make history. The best response to doubt is to critically compare theories and to bolster analysis with new historical research. We always should probe the empirical and epistemological foundations of our historical knowledge, but we shouldn't despair about our ability to make progress in understanding moments of social transformation.
The riddles of how social relations produce historical change need to be addressed, ultimately, on structural, ideological and epistemological levels. I hope my focus on the first has provided a framework that can shed light on the latter two. Our growing understanding of each dimension of social change can illuminate the bases of exploitation and thereby identify ways that struggles for justice have been waged in the past and might be pursued in the future.

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Why Study Colonialism?

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In 2002, I was invited to organize a panel on Colonialism for our section at the ASA meetings in Chicago. Entitled “Colonialism, Domination, Identities,” the panel included four papers that suggest the importance of studying colonialism in sociology in general and from a comparative historical perspective in particular. This brief summary is intended to offer here some themes as “food for thought” in invitation of a more sustained exchange about the topic and to give members of the section a glimpse at the topics discussed at the 2002 panel. I then let the panelists speak in their own words by including their abstracts below.

Most of the world today in effect is postcolonial and has experienced some form of colonization in the last two centuries. This is the case in the Middle East, Africa, Asia, and parts of Latin America. Current political developments in many countries of the Middle East and Central Asia, for example, can hardly be understood separately from their colonial past. The big wave of decolonization in the world at large did not occur until after World War II, fairly recently on the scale of world history. One reason to analyze processes and structures during the colonial period is to understand the dynamics of colonialism itself as a form of domination. Historical sociology has much to contribute to this objective in telling us how systems of domination operate when an external force imposes itself on another society

Another reason and one that may be even more compelling has to do with the implications of the colonial experience for postcolonial states and societies. The continuing importance of the colonial legacy raises a host of questions with regard to former colonies such as the characteristics of political institutions, culture and cultural production, language and problems posed by bilingualism, issues of identity, ethnic nationalism and the struggles surrounding it, postcolonial diaspora communities in Europe and the US, or political configurations that are favorable or on the contrary obstacles to development and democracy. In a nutshell, an understanding of large areas of the world today requires close attention to how colonialism shaped power distribution and the development of institutions and culture. Different forms of colonization have left different legacies and thus have had a varied impact on postcolonial societies.

In my own work, I have found that the particular form of rule exerted by the French on their North African colonies had a lasting effect on political processes in each country. In States and Women’s Rights: The Making of Postcolonial Tunisia, Algeria, and Morocco (Univ. of California Press, 2001), I argue that much of what was distinctive about the polity of each North African country when it gained independence can be traced back to the different forms of colonialism. In particular, the greater centralization and weaker local structures that characterized Tunisia in the early postcolonial state had their origins in the way in which features of the precolonial polity combined with the effect of colonialism. So did the greater autonomy and power of local areas in Morocco and Algeria. The specific form of colonial rule depended in part on the characteristics of the colonized society coupled with differences in the timing of the conquest, the special economic interests of the colonizers in each colony, and the availability of French military and administrative personnel at that particular time. Drawing on Weber, I show how these factors came together in multiple ways to produce distinctive modes of administration with long-term effects on state organization.

Colonial rule lasted over a century in Algeria, from 1830 to 1962, about three quarters of a century in Tunisia, from 1881 to 1956, and less than half a century in Morocco, from 1912 to 1956. In Tunisia, the French maintained the administrative structure they found in place when they occupied the country and superimposed their own apparatus upon that structure by extending its scope and powers in local and regional administration. As a result, colonial rule furthered bureaucratic centralization while essentially extinguishing tribal politics in Tunisia. In Algeria, where the French had higher stakes than in their other North African colonies, they secured land for settlers by using military means. They then established direct rule by French military and administrative personnel as much as resources permitted at all levels, central, regional and local. Colonialism had a deeply destabilizing effect on Algerian society and fragmented tribes and local communities. Ending as it did with a decentralized guerilla anti-colonial war, it left independent Algeria with a heterogeneous, highly divided leadership and a lack of administrative infrastructure. One could argue that the bloody conflicts of the 1990s in Algeria were in part the legacy of social devastation suffered under colonial rule and accentuated by the political divisions inherited from the anti-colonial war. In Morocco, the French used a divide and rule strategy, relied on an indirect form of administration in rural areas, and coopted local leaders who maintained considerable autonomy over their own areas. Colonization left behind a strong tribal framework in rural Morocco. A challenge for many years following the end of colonial rule was the integration of local areas into the newly formed Moroccan nation-state.

Focusing on a range of countries and issues, the papers on the panel address two major themes, the effect of colonialism on the development of political institutions, and the relationship between colonial domination and culture. Hans Bakker shows how colonialism in Indonesia undermined the potential for economic and political development. Comparing Sierra Leone and Mauritius, Matt Lange analyzes the form of interaction between the colonial state and local populations as a determinant of information and resource flows, and thus of later trajectories. Focusing on Puerto Rico, Julian Go examines how culture “works” under colonialism and how the different cultural repertoires of the colonized and colonizer produce incommensurability in meanings. In a study of Trinidad, Marina Karides considers how cultural factors have been used as a justifying device to legitimate economic exploitation in a situation of cultural domination. All together the papers presented at the panel make a convincing case for the importance of studying colonialism and invite a renewed debate on the issue among comparative historical sociologists.

J. I. (Hans) Bakker, University of Guelph, “Involution versus Structural Transformation: the Colonial Legacy in Indonesia”

> Today Indonesia suffers from lack of sufficient economic and political development. Why is the Republic of Indonesia not a highly industrialized and completely democratic nation-state?

To answer that question we must distinguish between structural transformation and what is here called "structural involution." The concept of involution comes from Alexander Goldensweiser (1936) and was utilized by Clifford Geertz (1963) to describe Agricultural Involution in Java. This paper argues that Indonesian society as a whole has suffered from a general structural involution that can be heuristically understood utilizing Max Weber's Ideal Type Models, particularly the model of Patrimonial-Prebendalism in Economy and Society (1968 [1920]). Weber's final theory of the structural transformation that is associated with the rise of modern capitalism can only be evaluated through a careful reading of his whole oeuvre. This paper argues that critiques by the California School (Goldstone 2000) and Lachmann (2002) do not capture the whole of the Weber thesis. Nevertheless, all theorists agree that something happened, as Lachmann put it, in Northwestern Europe around the end of the seventeenth century. That structural transformation did not take place in Indonesia because whatever indigenous potential for rapid economic and political development there may have been in the archipelago was nipped in the bud by the way in which dualistic colonialism (Boecke 1945) reinforced prebendal, prijaji tendencies and a closed rigidly hierarchical society in Central Java.

Matt Lange, Brown University, “Structural Holes, State Capacity and Development: An Analysis of Colonial Sierra Leone and Mauritius”

This paper analyzes colonialism as an important determinant of the form of state-society relations and thereby social development. Recognizing that colonial rule either created or dramatically transformed state institutions, it focuses on the ties between the colonial legal-administrative apparatus and local populations, noting that the form of ties between colonial state and society shaped local power relations, information and resource flows, and thereby local developmental processes. To investigate these claims, the paper considers post-World-War-II development efforts in two former British colonies: Sierra Leone and Mauritius. While the colonial administration attempted to implement economic and human development programs in both colonies, the different network structures linking state and society caused vastly different degrees of success. In Sierra Leone, the indirect form of rule placed chiefs at key intermediary positions that empowered them to exploit their subjects and control information and resource flows between state and society. As a result, chiefs were rent-seekers extraordinaire while development programs were non-existent locally. Mauritius, on the other hand, experienced a direct form of colonial domination that established multiple ties to the local populations, thereby preventing any individuals from obstructing information and resource flows between state and society. As a result, broad segments of society were able to collaborate with the colonial administration in order to make late colonialism a period of rapid social development. Noting the continued power of chiefs in postcolonial Sierra Leone and the democratic developmentalism in postcolonial Mauritius, the paper suggests that colonialism must be analyzed as a potential determinant of long-term developmental trajectories.

Julian Go, Harvard University & University of Illinois "Culture in Colonialism: Making Meaning in the US Occupation of Puerto Rico"

Sociological research on culture examines cultural repertoires within a given society or compares cultural structures across different societies. The role of meaning-making during times of foreign occupation, when people of different cultural systems are put into sustained contact, remains elusive. This essay specifies how culture "works" in colonialism through a study of US colonial rule and elite collaboration in Puerto Rico. I suggest that culture in colonialism takes on a particular dynamic. Ruler and ruled interpret and act in the colonial situation according to their preexisting cultural structures, but as their structures are drawn from different cultural systems, the meanings they make of the same signs and structures will not be identical. Cultural difference between occupier and occupied makes for incommens-urability in meaning-making. I disclose this cultural logic at the level of political culture during the first years of US colonialism, when the US tried to "export democracy" to Puerto Rico (ca. 1898-1900). Ultimately, the cultural logic of difference and incommensurability contributed to the formation of a centralized colonial regime and lasting tensions between the US occupiers and the colonial elite. The study demonstrates that the analysis of culture and colonialism can be a fruitful "laboratory" for analyzing sociopolitical interaction in post-colonial situations of cross-cultural contact. I submit that an analysis of cultural difference and meaning-making in colonialism starkly reveals the workings of culture and power more generally. Finally, more than serving as a "laboratory" for understanding present phenomena, an analysis of political culture and colonialism is critical for understanding the weight of the colonial past on the postcolonial present. The case of US colonialism was one instance of a larger pattern by which western political forms were imposed upon non-western societies. The study demonstrates how such imposition has been mediated by local cultural structures to produce syncretic political cultures.

Marina Karides, Florida Atlantic University, "Race, Culture, and the Evaluation of Micro-entrepreneurs: Colonial and Post-colonial Influences in Trinidad"

This paper traces the racially biased evaluation of African-Trinidadians business capabilities currently utilized by state officials and international development professionals to the nation's colonial period. Examining colonial ideology and colonizers' racial organization of workers is essential for understanding the present circumstances of African

Trinidadians as well as the ideological basis of present government and development programs that claim to address their needs. Presenting an analysis of historical and interview data, this paper shows that recently established micro-enterprise development programs use descriptions and assumptions of African Trinidadian created during colonialism as their basis of legitimation. Rather than addressing the current economic and political features of the local and global economy (also an outcome of the nation's colonial legacy) to understand African Trinidadian circumstances, policy makers continue to rely on stereotypes and biased ideologies established through colonialism to explain the poverty experienced by these workers.