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From the Chair

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"On critical theory and the historical sociology of the present": that is the column I would have liked to have written for this newsletter, tapping it out on the old mojo wire, as Hunter S. Thompson called his field-reporting teletypewriter connection during those fateful days of the early 1970s United States, when pathos and bathos vied for supremacy. Dr. Thompson's "gonzo" journalism – never clearly defined other than by the exemplars of his wildly honest political reporting for Rolling Stone – would offer a template for a gonzo critical theory, itself infusing a gonzo historical sociology. A quote would emerge, as if out of the blue, from Adorno's Prisms essay on cultural criticism: "There are no more ideologies in the authentic sense of false consciousness, only advertisements for the world through its duplication and the provocative lie which does not seek belief but commands silence." I would deploy historical comparisons and mental experiments – drawing on histories from wars of the ancient Greeks to the present – to make the point that comparative and historical sociologies are centrally to the public sociology that ASA president Michael Burowoy has thematized for our upcoming meeting in San Francisco.

The flow of present history keeps running out ahead of that column so quickly that I dare not try to write it. But I know that the requisite historical sociological imagination runs as strongly as ever among our section's members – as needs it must. And so, suffice it to say that we will have much to discuss when we gather this

In this issue…

August. As you'll see elsewhere in this newsletter, we have an exceptionally strong series of sessions – on "states, critical turning points, and world history," on "religion and the state," and on "historical studies of economic processes." In additional, there will be a terrific series of roundtables, just before the business meeting, where the Bendix and Moore Awards will be announced – selected by the awards committees from what I am told are strong contenders. Finally, our reception will be held jointly with the Theory Section, in the bar and tea room of the King George Hotel (chosen not for any historical significance of the king, but for the venue's plush, gently decadent postmodern décor, and close proximity, at 334 Mason Street, to the Hilton Hotel). I take this opportunity to thank the organizers of the sessions – Rosemary Hopcroft and James Mahoney, Philip Gorski, and Viviana Zelizer – and the organizer of the roundtables, Brian Gran. I hope you will join us for what promises to be an important and exciting meeting.

John R. Hall

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The Passion of the State

Julia Adams
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Sociologists have thought about state formation in two main ways during the past quarter century. States were formed either by class dynamics, for those inclining to a marxist view, or, for those with a bellicist eye, by the vicissitudes of interstate competition and war. Much excellent work has been conducted under these rubrics. These were not the only ways of analyzing state-building, to be sure, but they've predominated – a byproduct of the Marx plus Weber combination that dominated historical sociology's "second wave" of the 1970s and 1980s (see Adams, Clemens and Orloff 2004). Philip Gorski doesn't repudiate these ways of seeing but construes them as too limited – not because they are incomplete, since all models are that, but because they fail to account for just the cases that they should explain best. Gorski aims to incorporate religion into comparative-historical theories of how European states were made. He explores the ways in which popular energies were politically harnessed and people were disciplined, capturing the variable relationship among religion, social discipline and states. The Disciplinary Revolution: Calvinism and the Rise of the State in Early Modern Europe is a terrific book.

Let me begin with a short plot summary. There were important differences among Christian salvation religions in medieval and early modern Europe – in their messages about ethical action, and in their organizational equipment for motivating and enforcing ethical dicta. Representatives of medieval Catholicism cared more about ritual observance and priestly intercession – while the Protestant Reformations emphasized the conduct of individual believers as the key to salvation. Gorski argues that the new ideas and regulative organizational practices associated with confessionalization – especially Calvinism – massively boosted social discipline, which in turn both strengthened and structured states. Not always in the same way, he shows -- for as confessional cleavages deepened, they either reinforced or upset existing political divides in various social settings. Sometimes, as in the Netherlands, Prussia or for that matter England, the upshot was positively revolutionary. Gorski particularly focuses on how the Reformation created new styles of governance that were picked up and used by political elites, including Catholic ones (p. 19). In some cases this happened in a direct way, by energizing broad swathes of the population in service of projects of rule; but the states with more pacified subjects were also indirectly strengthened, because people who police themselves are cheaper and easier to control. Gorski works this out elegantly over a series of cases – notably the Dutch Republic (which experienced a disciplinary revolution from below) and Brandenburg/Prussia (which was disciplined later on and from above). These historical experiences generated modular mechanisms of social disciplining that diffused more widely, in ways that reflected confessional dynamics – like systems of poor relief, or bureaucracy, which differed in Protestant and Catholic states. "Religion may not have been the driving force behind administrative development in early modern Europe, but it did serve as a switchman that helped to determine the path which those developments took" (p. 154).

I should say right off the bat that Gorski's argument persuades me on the most important points – that the Reformation was important to restructuring discipline and states (p. 33), and that social disciplining was more intense in Calvinism than elsewhere. He has also breathed new life into Weber's Protestant Ethic thesis by effectively linking it to the state, thereby illuminating early modern European politics in a new way. The Disciplinary Revolution should make marxists and bellicists think twice about claims to having mounted exhaustive explanations or come up with the primary motors of history. Furthermore, as a generous integration of the insights of historians, second wave historical sociologists, Weber, Foucault and others – revised and extended – The Disciplinary Revolution is another blow in favor of the "culturalization" of state theory. And I do mean the "revised and extended" part, for Weber never really incorporated the Reformation into his writing on state organization and political legitimacy, and Foucault neglected it while circling around it obsessively.

Do I have gripes? Sure! To clarify some of them, I'll focus on the Dutch Republic (and I hope this is more than the usual scholarly tendency to grump about the case that one knows best). But in fact the Dutch state has been something of a symptomatic stumbling block for marxists of all stripes – whether orthodox or of the world-systems variety. Immanuel Wallerstein (1980), as Gorski mentions, understood how weird it was for the first hegemonic power to have such an apparently weak state. Bellicists have found the early modern Netherlands equally hard to decipher – Gorski points out how the Dutch case is a key one that just won't square with Thomas Ertman's (1997) theoretical typology. For his part, Gorski contends that the Dutch state (1) wasn't as fragile as it looked (it was locally rather than centrally developed and it wasn't as overridden by patrimonial venality as the French and other states); (2) was immeasurably strengthened from the bottom up by Calvinist discipline, via the indirect and direct mechanisms I've already mentioned. This is a double argument, about state structure and capacity.

I agree that the Dutch state wasn't as weak as it looked, first of all. It rested on many pillars: 50-some cities; the stadholderates (eventually unified in what Simon Schama (1987) calls one hereditary "presidential patriarchal" office), and the associated sovereign corporate bodies. But to say that that meant that there wasn't a center confuses form and function. There was a seat from which sovereign decisions radiated in the 17th century, the Dutch Golden Age, and that was the merchant regency of the province of Holland and its powerhouse city, Amsterdam. This wasn't the official center, but it was the actual one -- the effective nodal point that articulated the worldwide network of Dutch governance. In the 18th century, the so-called Periwig Period, this nodal point collapsed and could no longer organize the network. Then the capacity for making credible commitments to, for example, keep up the navy gave way to an ongoing, unresolved struggle among provinces and cities, overdetermined by the resurrection of the princely House of Orange and the unitary stadholderate it eventually controlled. So -- no more effective center. Plus, there was not only no bureaucratization of the absent center – there was none to speak of anywhere else, either. Family representatives of the regents and House of Orange held the offices that mattered, and passed them down and around with formal and informal inter-family arrangements in the 18th century. These Contracts of Correspondence, and other such deals, were also a nice functional substitute for venality (of which there was already enough, by the way, for the Dutch States-General to resign itself to taking a big cut of the proceeds!).

Now for Point Two. Calvinism certainly mattered in the rise of the Netherlands and (though this isn't Gorski's preferred language) in the construction of the network of Dutch hegemony. But this was not because Calvinism was institutionalized as the articulating principle at the core of the emergent state, because it wasn't. Theocratic tendencies were in turn disciplined – held at bay and controlled by elites who objected to the subordination of profits and power to strictly religious goals. The divorce of Saint and Regent actually happened fairly soon in the Dutch ascension, in the early 17th century. This was a momentous moment of differentiation – of modernization (though of course the Dutch elite didn't call it that) – and in my view it made possible what we think of today as the rise of the Netherlands, the Golden Age, and other key aspects of modernity.

The upshot of both these points is that my response to Gorski's summary sentence, that is: "We must shift our focus from the central to the local and we must broaden it to include a wider range of institutions" (p. 85) – is "no!" First, federated bodies can function as one, nowadays thanks to articulating notions of majority rule. True, that particular cultural recipe wasn't much in evidence in early modern Europe. Patrimonial states were messy networks of overlapping sovereignties. But even there what we think of as "the local" can constitute a center, a collective principal capable of commanding political agents. This happened in many sites in early modern Europe, for example, when elite patriarchs managed to unify themselves under the sign of monarchical father-rule. Neither Weber nor Foucault can help us too much here, for this also hinges on practices of signification and even Weber's helpful ideal type of patriarchal patrimonialism installs biology, not what people made of it, at the core. Second, should we really broaden our analysis to include a wider range of institutions? Maybe we need to narrow it instead, focusing on the syncretic institution that mattered most, the patrimonial package of elite family/state/monopoly niches characteristic of early modern Europe. Religion – Calvinism – was tied up with this package in the Netherlands, and it was extremely important, as Gorski has shown. But it was also institutionally subordinated in the 1620s, its significance and forms of organizational recruited to the emergent familial state as the axle of discipline.

So I find myself in the peculiar position of agreeing with many of Gorski's core claims about the Dutch state, but not on the grounds that he gives. This is surely a sign of our theoretical differences – for I would insist on the familial dimension of patrimonial power as a primary one as political signifier and organizational form. Regent and Saint, and Stadholder and Saint, may have been sundered, but Regent, Stadholder and Fatherhood were cosily coupled up even as the regents and Princes of Orange were fighting about who got to be "ubervader" in the Dutch Republic. I would make analogous claims for other early modern European states, too – France, England, even Prussia, where the logic of bureaucratization was most advanced.

All this leads me to a still broader and less settled question, one that The Disciplinary Revolution explicitly raises for all of us to ponder and discuss. What, as historical sociologists or sociologists tout court, are we trying to do? During the second wave of historical sociology, in the late 1970s through the 1980s, historical sociologists generally presented their collective project as finding a better if not the best way of coming up with the necessary and sufficient conditions to explain state formation and the Great Revolutions. States and revolutions are just as interesting as they ever were, but the notion that we might come up with a menu of necessary and sufficient variables is far less credible now than it was then. At least I think so - others disagree (those who are interested should take a look at the exchange in last year's Comparative Historical Sociology section newsletters about Richard Lachmann's Capitalists in Spite of Themselves: Elite Conflict and European Transitions in Early Modern Europe (2002), this past year's ASA Best Book winner). I wonder where Gorski stands on these issues. He uses the language of necessity and sufficiency, but is at the same time very careful to place his study as a problematization of such accounts rather than as a candidate to replace other single theories or explanations; instead, he emphasizes that he is making a case for religion as yet another important factor or variable. Admirably modest, if perhaps a shade too deferential to second wave formulations. And perhaps this modesty or politeness restrains him from exploring the more hermeneutic side of the Reformation – including the political importance of textuality – and the bloody struggles over interpretations of The Book, the Bible – at the nexus of Reformation, politics and religion. To give just two excellent examples, David Zaret's The Heavenly Contract (1985) investigates the dynamics and organizational consequences of key tensions within Puritan theology, while Steven Pincus's Protestantism and Patriotism (1995) explores the role of internally complex ideologies in the struggles over mid-17c English foreign policy. For religions are also networks of signs, and the Reformation was a time and place in which people would kill or die to defend interpretations of the bread and wine of communion as either signifiers of Christ's body and blood or the thing itself. This is an unstable language that can authorize discipline and self-discipline -- and also, even simultaneously, individualization; sacralization of a moral self; transcendance. We at least need to be very careful not to replicate the flattening of meaning and language characteristic of second wave accounts, and we need to try not to extirpate the self. I know that Gorski thinks that such matters of meaning are historically important – he himself remarks that the book is less hermeneutic than it might be – but it is also true that if taken seriously, they make even the compromise language of "one more factor or variable" still harder to sustain.

Today's historical sociologists are less sure-footed than were the second wave scholars about the methodological character of their analytic project(s) -- I include myself in this stricture. Nevertheless this is also, I think, a sign of new vitality and growth in the enterprise of historical sociology. The opening to new optics beyond marxism, bellicism and positivism is making for a renaissance of historical sociology right now. Heretofore settled questions are now unsettled and renegotiated. Philip Gorski's The Disciplinary Revolution is one of the best examples of this – a wonderful book of third-wave historical sociology – and I strongly recommend that you read it, teach and learn from it. I certainly will.

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Gorski and the Military-Fiscal Theory of State Penetration

Randall Collins
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Gorski's The Disciplinary Revolution is a wonderful work of historical sociology. Anyone who reads his chapter on Prussia, for example, is unlikely to forget it. Here I will concentrate only on some theoretical points of contention.

Gorski sets up his analysis as a critique of 'bellicist' theory of the modern state. He uses Thomas Ertman's formulation as a foil, but this is not the most general or significant version of the theory. Ertman's statement is rather particularized, giving weight to the destruction of local government by the Roman Empire, and to whether geopolitical competition occurs before or after 1450. Gorski doesn't have much trouble in finding exceptions to these points.

But the major line of theory, which may also be called the military-fiscal theory of state change, still stands. To reiterate the main points, which have been well documented by Tilly, Mann, Skocpol, Goldstone and others: the state originates as a military organization, and expands by military conquests (e.g. Prussia) or alliances (e.g. Dutch); military costs are the biggest item in the state budget; the 'military revolution' in size and expense of troops, weapons and logistics leads to creation of administrative apparatus (bureaucracy) to extract revenues. From here on several historical pathways can be followed: resistance by aristocrats and populace to revenue burdens and administrative encroachment can lead to state breakdown and revolution, or alternatively to authoritarian restoration, or to state disintegration; what happens to states which take the latter pathways is usually a fatal geopolitical weakness that ends the independent history of that state. In the long run, the states which survive are those which successfully expand their tax extraction and administrative organization; and this penetrates into society, breaking down patrimonial households, inscribing individuals as citizen-subjects of the state, and thereby creating mobilizing conditions for modern mass politics, and for state welfare administration.

There is nothing in Gorski that contradicts this model. It should be noted that democracy per se is not a universal feature of state development via military-fiscal organization and state penetration. Parliamentary democracy triumphed where resistance to state penetration, led by aristocrats organizing themselves through medieval collegial bodies, was successful -- and then was followed by resuming the pathway of state penetration but with parliamentary forces in charge of the state apparatus. Democracy is not the central feature of modern state development, but a side-effect of the main dynamic.

Similarly, it is anachronistic to define the modern state, as Gorski tends to do, by the extent of its provision of welfare, order, and control. Modern states, having developed a large administrative apparatus for tax extraction, could use it for provision of order and welfare. The geopolitically successful states generally took this path; but often it is a late development (the U.S. is a good example). Gorski, especially in his Dutch case, argues in the opposite order: that the establishment of disciplinary surveillance is the establishment of the core features of modernity.

Is there, then, a disciplinary path to modernity, contrasting with the military-fiscal path? The Calvinist practices that Gorski documents so vividly (especially in Holland) are an Orwellian horror. If this is the image of modernity, we would hardly have wanted to go there. But in fact, the military-fiscal path to state penetration did not in the long run result in increased oppressiveness of everyday life, but in opening up what was widely regarded as freedom. This is because state penetration and its formal bureaucracy led to decline of the patrimonial household, and of local corporate governing bodies; remote, impersonal and relatively ineffectual bureaucracy --- and indeed often bureaucracy run by self-styled liberals and reformers --- took over from petty despotisms of father, master, and local oligarchs. The extreme forms of discipline that Gorski describes seem to be a hybrid of the worst features of the two types: medieval-patrimonial local despotism, and modern-impersonal bureaucracy.

It is ironic, in view of the popular connotations of both the terms 'bureaucracy' and 'Prussian', that neither of these is as harshly regimenting and anti-liberal as we often assume. The world of bureaucracy, once it triumphed over patrimonial local despotism, has turned out to be a place with unprecedented space for individuals to maneuver. And Prussia, once it got past its Calvinist beginnings, by the turn of the 19th century was the liberal and indeed leftist part of Germany (above all in its universities and in the free-thinking cosmopolitanism of Berlin); the reactionary parts of Germany were in more traditional states such as Bavaria. What Gorski has shown is that a religiously-based disciplinary movement, building upon both patrimonial and bureaucratic features at the time that military-fiscal problems were forcing a transition between them, generated a peak intensity of disciplinary surveillance such as the world has never seen, before or since.

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Comments

David Zaret
Indiana University

This outstanding piece of historical sociology is gracefully written, with carefully qualified arguments and abundant signs of Philip Gorski's masterful command over the relevant primary and secondary sources. Most impressive is how Gorski handles the perennial problem of conflicting historical accounts. He sifts through these based on his knowledge of substantive issues, and he avoids the all-too-common strategy (among sociologists) of selectively citing secondary historical accounts that support a thesis and dismissing or ignoring inconvenient accounts. When Gorski discounts writings with near-canonical status (e.g., Carsten on Prussia; Strauss on the Lutheran Reformation), he does so, authoritatively, as a historian with substantive arguments.

Of course, Gorski's principal concern is with synthetic accounts and analytic models of state formation in early-modern Europe. His criticism of Marxist and fiscal-military models refers, in part, to discrepancies in the application of these models to specific cases, with regard to predictions over the type of state regime, the timing of its origin and the strength of a regime. But because discrepancies are inevitable, Gorski fairly notes that it is unhelpful to dismiss a model simply because it cannot be fitted to all cases. He therefore champions his account of the importance of the disciplinary revolution with the added claim that it avoids major discrepancies and is more parsimonious than the other models. Simply put, Calvinism and constitutionalism go hand in hand. Discrepancies arising from the application of Marxist and fiscal-military models vanish when confessional dynamics (something larger than Calvinism per se) are factored into accounts of state formation in Brandenburg-Prussia and the Low Countries (Gorksi's principal cases) and elsewhere.

Gorski's analysis of the relevance of the disciplinary revolution formented by Calvinism has three important elements. First, he does not deny the causal relevance of factors cited by other models of state formation, e.g., geopolitical competition, economic forces, antecedent regime type, and the 16th-century "military revolution." The analysis is additive; that is, it resolves discrepancies arising from the application of these models by citing the religious (or "confessional") factor. Second, Gorksi argues for the relevance of confessional politics. It is not disembodied doctrine, but its institutionalization—the creation of state churches—that matters. Creating state churches requires local enactment, which introduces a host of contingent possibilities that can push religious reform in unanticipated directions. Third, this last point indicates why Calvinism, for Gorski, is not unique, but, rather, a more extreme version, compared to Lutheranism and Catholicism, of the religious wellsprings of the disciplinary revolution.

These three elements pave the way for Gorski's general claims about state formation. First, state formation involves more than political and administrative centralization. Equally important is the religiously-inspired disciplinary revolution, which increased state capacity in terms of regulatory power over citizens and administrative efficiency. Second, state formation cannot be understood solely as a top-down process. It is also influenced by developments in localities, and the shifting relation between the center and peripheral regions. For the Dutch case, Gorski describes a disciplinary revolution from below, as Calvinism's direct effects on the citizenry created a nation populated by disciplined, watchful, obedient and hardworking individuals. For Prussia,Gorski describes a disciplinary revolution from above, led by monarchs who rationalized the executive administration.

Any assessment of Gorski's synthesis should begin by acknowledging that he succeeds in marshalling evidence to support his claim about the causal relevance of the religious factor. But how should we assess this achievement in terms of his criticism of prior models of state formation? That is, what is the analytic consequence of including confessional politics in models of state formation? Contrary to Groski's claims, it is not simply a more parsimonious explanation. For some issues Gorski's argument is indeed very parsimonious (e.g., his account of the prevalence or absence of venality in office holding, which he explains in terms of proximity to the Papacy and to precisionist movements of religious reform). But for many other issues, the analysis moves to a less parsimonious footing, as the religious factor is added to a long list of other causal factors cited in Marxist and fiscal-military models. At these points the analysis is an additive exercise: e.g., "To fully understand what made the Dutch state so strong … we need to look at another factor: religion" (p. 40); "Prussia's divergent [compared to other central European states] path and its rise to power cannot be understood without regard to its religious situation" (p. 80). Indeed, in Gorski's analysis the Prussian case ceases to be one case, as Groski demonstrates how differences in relations between the Crown and local estates in different regions had important consequences for state formation in, for example, Cleve-Mark compared to Brandenburg.

This historicizing tendency is embedded in Gorski's model, due to the diffuse nature of "confessional politics." Confessional politics include all the social and political entanglements of religion—a point noted by Gorski when he observes that, in early-modern Europe, religion has dense links to virtually all aspects of life. If this is correct, then invoking the centrality of "confessional politics" invariably leads in the direction of more attention to historical complexities at the expense of parsimonious explanations and general models. While I don't think Phil Gorski would agree with this conclusion, I see this as one of the principal virtues of his excellent book.

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Reply

Philip S. Gorski
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It has become customary to periodize the field of comparative-historical sociology in terms of three distinct waves more or less as follows: a first wave that arose in opposition to structural functionalism and modernization theory during the late 1950s and early 1960s; a second wave during the mid-1970s that took its questions -- class, state and revolution – from Marx, but many of its answers from Weber; and a third wave, more heterogeneous and diffuse, beginning in the early 1990s.

From this perspective, my book can be seen as a third-wave revision to second-wave work on European state formation Collins' critique can be seen as a skeptical second-wave rejoinder; Adams' as the sympathetic dissent of a fellow third-waver; and Zaret's as the methodological queries of a fellow neo-Weberian Their critiques pose four questions: How much does The Disciplinary Revolution add to the second-wave orthodoxy? What is the causal significance of Calvinism, especially in the Dutch Republic? What model of explanation do I advocate – nomothetic or mechanistic? And is my account more theoretically parsimonious than its alternatives or just more historically grounded?

1. Collins: What's the value-added?

While Collins evidently enjoyed the book, he doesn't think it adds that much to our understanding. His position, if I understand rightly, is that the second wavers, fiscal-military model in hand, have made short work of early modern state formation – and of state formation tout court – and pretty well cleared it from our collective plate. My book is little more than a digestif: it leaves a pleasant taste on the palate and a warm feeling in the belly but has little weight or substance. His view is that the big questions – about the genesis, development and survival of states – can be easily and adequately explained in terms of the fiscal-military model. The disciplinary revolution was a cul-de-sac somewhere off the royal road to the modern state. Which is just as well, since it was "an Orwellian horror" that we can be happy to have escaped. Ironically it is the Prussian path of bureaucratization, rather than the Dutch path of constitutionalism, that ultimately created the greatest space for individual liberty by sundering the shackles of local patrimonialism.

I am happy to agree with Collins about the importance of geo-political competition for state formation. But I think that religion is also an important part of the story. And here are my reasons: a) If we extend our view beyond early modern Europe, to include the great states of Antiquity (e.g., China, India and Egypt) we see that state-making often went hand-in-hand with religion-making. Indeed, some of the earliest states were probably temple states. To say that "the state originates as a military organization" is an exaggeration. b) Turning back to early modern Europe, I would argue that the fiscal military model is not as powerful or complete as Collins implies. It has been unable to fully account either for the divergence between absolutist and constitutionalist regimes or between patrimonial and bureaucratic states. These outcomes cam be more plausibly and parsimoniously explained in terms of i) confessional conflict, especially as it intersected with conflict within political elites and between social classes and ii) social disciplining, especially as it under-girded the projects of social and political reformers. iii) In my view, geo-political competition and confessionalization are not two alternatives paths to modernity, but two complementary mechanism of state formation. One contributes to the monopolization of the legitimate means of violence and the extraction of material resources, the other to the monopolization of the legitimate means of socialization and the cultivation of human resources; one leads to an increase in coercive power, the other to an increase in regulatory power. And it is the combination of these two forms of power that distinguishes the early modern state from its medieval predecessors. Or so I argue. d) Today, we are apt to think of bureaucracy and discipline as obstacles to freedom. Collins is right to question our thinking about bureaucracy. But I would like to question his thinking about discipline. Inimical as it may be to the Zeitgeist of contemporary America, there is a long tradition in Western political thought, from Cicero to Kant, that views discipline as a pre-condition of, rather than an obstacle to, freedom, and in two senses: self-discipline frees us from the tyranny of our passions, while social discipline protects us from the passions of others. Disciplinary revolutions helped to create the characterological and institutional conditions for this type of freedom – freedom as rationally-grounded obedience to self-prescribed law. Where we should value this type of freedom, or aspire to other types – important questions that are too far afield.

2. Adams I: How much did Calvinism really matter?

Julia Adams shares my reservations about the fiscal-military model, but is skeptical of my interpretation of the Dutch case. She objects that the Dutch state was more centralized and less bureaucratic than I claim, and that its relationship with the Calvinists was fraught at best. She argues that the peculiarities of the Dutch state can best be understood in terms of the "patrimonial package of elite family/state/monopoly niches characteristic of early modern Europe." Without delving too deeply, let me clarify my position on three issues.

a) That there were centripetal as well as centrifugal forces in the Dutch Republic, and, more specifically, that the de facto power of Amsterdam and the pervasiveness of patrimonial discourses provided a certain degree of institutional centering and symbolic coordination among elites – on that I would certainly agree. But if one is interested in the mechanisms through which the everyday life of the common people was disciplined and regulated – these were quite de-centralized, especially in comparison to, say, France or Prussia. My claim is simply that the principle sources of social order in the Dutch Republic were local in character. So I am not sure that we really disagree about state de/centralization. Where we may disagree is about the peculiarities of the Dutch state. My account focuses on the intensity of disciplining, Adams' I think on the intensity of patrimonialism.

b) On the second count – understating the degree of patrimonialism – I would say that I adhered to the letter of the law, but maybe not the spirit. If we operationalize "patrimonialism" as "venal office-holding", and if we understand venality as the legal and public sale of offices for money – which is what I do in the book – then one can reasonably conclude that there was not a great deal of venality in the Netherlands as compared to, say, France or Spain. However, if we understand patrimonialism and venality more broadly and loosely, as Adams does, then there was plenty of it. The important question, for both Adams and myself, would concern the effects of these two different variants of patrimonialism, both on administrative efficiency and on social mobility. I would speculate that familism was more efficient, since it created a link between patrimonial honor and political probity, but that venality allowed for greater mobility, insofar it made money the only prerequisite to office.

c) Now for the third objection – one that I have heard often from Dutch specialists – namely, that the Dutch Calvinists were a beleaguered bunch, who were often at loggerheads with the urban regents, and that I greatly exaggerate their influence on Dutch society. Here, I will simply make two points: i) in my view, the main source of disagreement between orthodox Calvinists and "libertine" regents was not so much whether there should be religious discipline but who should exercise it: the town councils or the church consistories. The result of this disagreement was that discipline was exercised by church officials but confined to church members. ii) it must be added that while non-Calvinists were not subject to Calvinist discipline, the non-Calvinist churches and sects had disciplinary systems of their own – systems that were usually stricter than those of their sister churches outside the Netherlands, and sometimes even stricter than those of the Dutch Calvinists themselves. The Calvinists made religious discipline a sign of social respectability, and thereby left a deep imprint on the religious and social life of the Republic.

3) Adams II: What's your model of explanation?

Adams also raises some more general questions about sociological explanation. Her main question, if I understand rightly, is whether I advocate the quasi-nomothetic form of explanation advanced by second-wavers such as Theda Skocpol during the 1980s; or whether I advocate the mechanistic version of explanation that has been so vigorously championed by Jon Elster and Charles Tilly in recent years. My answer, I think, is "neither and both." (Which is why I vacillate between theses two different languages of explanation in the book). Let me try to clarify my position.

I say "neither", because I believe both models are problematic. Like many other historical sociologists, I have become increasingly leery of deductive forms of explanation, and the falsificationist philosophy of social science that underwrites them. The problem with Popper is that he a fails to distinguish between theory in the sense of a specific account of a particular event or class of events in the social world (e.g., "Perry Anderson's theory of absolutism") and theory in the sense of a general approach to the study of social life (e.g., "neo-Marxism"). I will refer to these as first and second order theories, respectively. Here's the problem: second-order theories do not logically entail any well-specified empirical predictions that could be unambiguously falsified; first order theories, meanwhile, can be falsified, but falsifying a first order theory does not falsify the second order theory from which it was derived, since the derivation is not truly deductive (On this point, see my essay on "The Poverty of Deductivism", forthcoming in Sociological Methodology).

But like many other historical sociologists, I am still loath to give up the quest for empirical generalization and theory-building. After all, that's what sets us apart from mainstream historians. The mechanistic program is often presented as a solution to this problem, i.e., as a form of non-nomothetic generalization. But while I understand the appeal of the program, I have become increasingly disenchanted with its current inceptions. On one hand, there are those, such as Charles Tilly, who argue that we should proceed inductively, by inventorying the recurring processes and sequences of events that we uncover in the course of our empirical research. On the other hand, there are those, such as Jon Elster, who argue that we should proceed deductively, by deriving such processes or sequences from a well-specified set of theoretical assumptions (i.e., the individualistic and utilitarian assumptions of rational-choice and game-theories.) Both of these positions seem problematic to me. Tilly's strategy leads to an endless profusion of mechanisms whose theoretical premises and interrelationships remain implicit or underspecified, while Elster's approach generates a small inventory of mechanisms that must be jerry-rigged to fit very heterogeneious situations. In short, I think the Tilly version contains too little second-order theory, while the Elster version is too disconnected from first-order theory.

Before the reader concludes that I am just a nay-sayer and a grump, I should stress that I also see considerable virtues in the nomothetic and mechanistic programs. The part of the nomothetic method that I accept involves deriving well-specified empirical predictions from first-order theories, not as a means of testing or falsifying second-order theories, but rather as a means of delimiting and refining first-order ones. An example: Skocpol's States and Social Revolutions predicts that all social revolutions will be preceded by state breakdowns. The fact that this (first-order) prediction does not obtain for a good number of social revolutions led subsequent scholars to delimit the scope of this claim (e.g., to "agrarian empires") and to refine its contents (e.g., by distinguishing different types of "state crisis"). The part of the mechanistic approach that I accept involves the attempt to concretize mechanisms "deduced" (loosely-speaking) from second-order theories and to generalize mechanisms "induced" (loosely-speaking) from first-order theories. Again, some examples may be useful. Robert Bates' deft use of the chain-store paradox and regulation theory to illuminate the dynamics of the International Coffee Organization provide a paradigmatic example of the deductive strategy; Bill Sewell's analysis of the storming of Bastille as a means of understanding how collective representations coalesce in moments of collective effervescence provides a fine illustration of the inductive strategy.

Let me conclude this discussion with a few caveats and recommendations. First, the caveats: 1) Mechanisms are not a substitute for second-order theories. Rather, they are an attempt to concretize and operationalize second-order theories in a way that makes them amenable to inclusion in first-order theories, and thus to empirical scrutiny (not "falsification", which is much too strong a word). The question is not whether one needs second-order theory, but rather, how explicit one's theoretical assumptions are. 2) Mechanisms are not a substitute for first-order theories. Rather, they are the raw materials out of which such theories are built. Most robust first-order theories invoke a number of mechanisms (e.g., "state breakdown", "peasant revolts"), and these mechanisms are often derived from very different second-order theories (e.g., Weberianism, Marxism). What I am suggesting is that we should see mechanisms as a form of theorizing that straddles the divide between the first- and second-orders. Now the recommendations: 1) We should try to ground our mechanisms in explicit, second-order theories, i.e., we should be clear about their constituent parts and laws-of-motion. A good second-order theory will specify the constituent elements of social life (e.g., "classes", "individuals", "signs"), their variable properties (e.g., "consciousness", "values", "meanings"), and the appropriate means of observing or measuring them (e.g., unified class action, opinion polls, content analysis). 2) The mechanisms we deploy in a first-order theory need not all be derived from the same second-order theory. Or, to put it more positively, we should willing and able draw on different repertoires of mechanisms rooted in different second-order theories. Not that I have followed both of these recommendations in The Disciplinary Revolution…But I plan to in the future!

4. Zaret: Is this account really more parsimonious than its predecessors?

At various points in The Disciplinary Revolution, I contend that my account is more parsimonious than its competitors. Am I justified in doing so? That depends on what one means by "parsimonious." Let me propose a definition – or rather three definitions. Drawing on the foregoing discussion, I would suggest that we distinguish three different levels or types of theoretical parsimony: i) onto-logical parsimony: applies primarily to second-order theories and is measured by the number of analytical entities or assumptions in the theory. Example: a theory that posits the existence of "individuals" animated exclusively by "material self-interest" is more onto-logically parsimonious than one that posits the existence of individuals and classes, or one that posits individuals animated by both interests and norms. Economists like to invoke this definition, especially against sociologists. ii) processual parsimony: applies mainly to mechanisms and is measured by the number of parts and processes contained in the mechanism. Example: A mechanism that explains the generation of norms in terms of the relationship between a charismatic leader and her followers is actually more processually parsimonious than one the mechanisms of an infinitely-repeated nperson game (although the latter is more onto-logically parsimonious). Sociologists can – and should! – invoke this type against economists. iii) explanatory parsimony: applies mainly to first-order theories and is measured by the quantity of, and qualifications to, the mechanisms that are invoked in any given explanation. Example: An explanation that invokes two mechanisms without loss of explanatory power is to be preferred to one that invokes three; an explanation that invokes two mechanisms without temporal, spatial or typological scope conditions is to be preferred to one that invokes the same number of mechanisms with such conditions. Now, if these three types of parsimony moved in tandem, there would be no need to distinguish between them. But I don't think that they do. In fact, I think there are probably severe and unavoidable trade-offs between them -- especially between onto-logical and explanatory parsimony. Fewer assumptions means more qualifications, and vice versa. What is more, I am firmly convinced that there also tend to be severe and unavoidable trade-offs between theoretical parsimony and explanatory power. The fewer the assumptions, mechanisms or qualifications, the fewer cases, variations and details that will be explained. In light of this hypothesized tradeoff, I will propose a fourth type of theoretical parsimony: marginal parsimony. Example: assume a first-order theory that invokes two mechanisms with two qualifications to explain three units of variation; assume further that adding one additional mechanism to the theory allows us either to remove both qualifications or to explain two additional units of variation. I would argue that we should prefer the second theory to the first, on the grounds that it generates increasing explanatory returns to theoretical complexity. That is what I mean by "marginal parsimony." Obviously, these standards are more easily defined than applied.

After this long detour, I am now in a better position to answer Zaret's query. Zaret notes two instances in which I claim greater parsimony for my (first-order) theory: i) my argument that the type of state administration -- venal or bureaucratic can be explained in terms of network proximity to the Renaissance Papacy and precisionist coalitions, respectively.; ii) my argument that the type of political regime -- constitutionalist vs absolutist – can be more easily accounted for if we put confessional conflict into our models. In the first example, which Zaret accepts, I am (implicitly) making a claim for explanatory parsimony: my theory invokes fewer mechanisms and fewer qualifications than its competitors. In the second example, which Zaret questions, I am (again, implicitly) making a claim for marginal parsimony. By putting an additional mechanism into the model – "confessional conflict" – I am able to explain more cases with fewer qualifications.

In closing, I would like to reflect briefly on the three-stage periodization that I used to frame this essay. Some historical sociologists – including myself – have wondered whether the third wave is really a wave – i.e., a coherent movement – or just froth and spray– the last remnants of the second wave. I am still not sure what the answer is. "Third-wave" historical sociology certainly does not have the theoretical and thematic coherence of second-wave work; it draws on numerous theoretical approaches (everything from rational-choice to post-structuralism) and takes up very wide-ranging questions (that go beyond class, state and revolution). Whatever coherence it does have, or might attain is, I think, methodological. It involves a two-fold reaction, against the nomothetic vision that still animates many social-scientists, including a good number of sociologists, and against the radical interpretivism that has captured the humanities and even a few sociologists. If I am right the in/coherence of the third-wave will be determined by the success or failure of our search for a via media between neo-positivism and neo-historicism.

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ASA Comparative and Historical Sociology 2004 Sessions

***Business Meeting, Comparative and Historical Sociology Section, Monday August 16, at 11.30 am.**

****The section also will hold a gala reception, joint with the Theory Section, at the King George Hotel, just around the corner from the Hilton, 334 Mason Street @ Geary, from 6.30 to 8pm, also on Monday.**

1."States, Critical Turning Points, and World History."
Organizers: Rosemary L. Hopcroft and James Mahoney
Monday, 8/16/2004 at 8:30 a.m

Lawrence King (Yale University)
Paper Title: Does Neoliberalism Work: Explaining Postcommunist Performance

Elif Andac (University of Washington)
Paper Title: Out of Empire: Transnational Foundations of Nation-States

Cedric de Leon (University of Michigan) Paper Title: Radicals in Our Midst: the American Critique of Capitalism in the Chicago Two-Party System, 1833-1867

Richard Lachmann (State University of New York-Albany)
Paper Title: The Mismeasure of the State: Elite Appropriations and Fiscal Crises

Ann Orloff (Northwestern University) Discussant
James Mahoney (Brown University) Discussant

2. Historical Studies of Economic Processes
Organizer and Presider: Viviana Zelizer, Department of Sociology , Princeton University . Monday, 8/16/2004 at 2:30 p.m.

Richard Biernacki, University of California,
Paper Title: How Protestantism Created and Subverted Economic Contracts in Reformation Britain.

Claude Fischer, University of California,
Martin Ruef, Princeton University
Paper Title: Boom and Bust: The Effect of Entrepreneurial Inertia on the Evolution of Markets and Industries.

David Woodruff, MIT
Paper Title: Ideas, Institutions, and Soviet Industrialization.

Discussant: John R. Hall, Department of Sociology, University of California, Davis

3. "Religion and the State: Preconditions of Tolerance and Violence, Past and Present"
Organizer: Philip S. Gorski Monday, 8/16/2004 at 4:30 p.m.

Robert Mackin (Texas A&M University) Paper Title: Secularization and the Structuring of Progressive Catholicism in Mexico, Colombia, and Chile

Fumiko Fukase-Indergaard (Columbia University)
Paper Title: *'State Formation and Repression of Protestants in Meiji Japan, 1868-1912'*

Michael Indergaard (St. John's University) Paper Title: *'State Formation and Repression of Protestants in Meiji Japan, 1868-1912'*

Aysegul Kozak (University of Minnesota) Paper Title: *Islamic Parties and the State: Case Studies on Democratization of Turkey and Egypt*

Gulseren Kozak-Isik (University of Minnesota)
Paper Title: *Islamic Parties and the State: Case Studies on Democratization of Turkey and Egypt*

J. I. Bakker (University of Guelph)
Paper Title: *The Execution of Oldenbarnevelt: The 'Means of Coercion' (Weber) in Comparative-Historical Perspective*

4. Comparative and Historical Sociology roundtables
Organizer: Brian Gran Monday, 8/16/2004 at 10:30 a.m.

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New Publications and Awards of Section Members

Mabel Berezin and Martin Schain (Editors). 2003. *Europe without Borders: Remapping Territory, Citizenship, and Identity in a Transnational Age* edited by. Baltimore: Johns Hopkins University

Mounira Maya Charrad's book, *States and Women's Rights: The making of postcolonial Tunisia, Algeria and Morocco* (UC Press, 2001), recently received the Best Book on Politics and History Greenstone Award (co-winner) from the American Political Science Association, 2003..

Toby E. Huff. 2003.*The Rise of Early Modern Science: Islam, China and the West* 2nd edition (Cambridge UP)

Maryjane Osa, *Solidarity and Contention: Networks of Polish Opposition.* Minneapolis, MN: University of Minnesota Press, 2003.

Maryjane Osa, "Networks in Opposition: Linking Organizations Through Activists in the Polish People's Republic," in Mario Diani and Doug McAdam, eds., *Social Movements and Networks: Relational Approaches to Collective Action*. Oxford: Oxford University Press, 2003.

Maryjane Osa and Cristina Corduneanu-Huci, "Running Uphill: Political Opportunity in Non-Democracies," with Cristina Corduneanu-Huci. *Comparative Sociology* Vol. 2, No. 4 (December 2003): 1-25.

Also of Interest

Scott A. Hunt is the editor-elect for the Journal of Contemporary Ethnography. JCE publishes theoretically, methodologically, and substantively significant studies based upon participant-observation, unobtrusive observation, intensive interviewing, and contextualized analysis of discourse as well as examinations of ethnographic methods. Submissions from all substantive areas and theoretical perspectives are welcomed. Email manuscript submissions (in Word or WordPerfect format) may be sent to sahunt00@uky.edu. Hardcopy submissions and all other correspondence should be sent to Scott A. Hunt, Editor, Journal of Contemporary Ethnography, Department of Sociology, University of Kentucky, Lexington, Kentucky 40506-0027. A processing fee of US$10 must be submitted via a check or money order made payable to the Journal of Contemporary Ethnography.

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Position Available

The Department of Sociology at Vanderbilt is recruiting for two tenured senior faculty positions (pending final administrative approval). Areas of specialization are open, although we have particular interest in scholars with distinguished research and teaching records on race, class, gender; crime, law, deviance; health and mental health; or work. Applicants should submit a letter of interest in the position, curriculum vitae, examples of recent scholarship, information on teaching effectiveness, and three letters of reference. (Six letters will be required for finalists.) All materials must be received by October 1, 2004. Vanderbilt is an Equal Opportunity-Affirmative Action Employer and women and minority candidates are encouraged to apply. Send all materials to Search Committee Chair, Department of Sociology, 2301 Vanderbilt Place, VU Station B Box 351811, Nashville, TN 37235-1811. Information on the department, the College of Arts and Science, Vanderbilt University and e-mail addresses can be obtained on the Internet at http://www.vanderbilt.edu/AnS/sociology.

Congratulations to Richard Lachmann [SUNY-Albany], Section chair-elect. Congratulations also to Miguel A. Cedeno [Princeton] and James Mahoney [Brown], elected to 3-year council terms.

New Webmaster Announced
As of September, 2004,
Dylan Riley
of the University of California at Berkeley
will become new section webmaster.