

Political Sociology

States, Power, and Societies

Summer 2007

Volume 13, Number 2

Symposium on Discipline and Development

Middle Classes and Prosperity in East Asia and Latin America, by Diane Davis. Cambridge: Cambridge University Press, 2004.

Showing how Class Matters for Development

Patrick Heller

Diane Davis' book, *Discipline and Development*, re-opens what many in the comparative political economy field thought was a closed debate. Between the works of Evans (1995), Wade (1990), Amsden (1989) and Kohli (2004) we thought we knew that the developmental state could essentially be explained by its institutional form, that is the more or less Weberian characteristics of bureaucratic authority, rooted in particular histories of state formation, that gave the state, working in conjunction with an emerging bourgeoisie, the wherewithal and the relative or embedded autonomy to jump-start industrialization. But just when we thought we had this all sorted out, along comes Davis with her impressive, sweeping and

Discipline and Development and the Development of the Discipline

Andrew Schrank

Discipline and Development makes at least three important—if by no means uncontroversial—contributions to contemporary political sociology. First, it brings the experiences of the early developers to bear on the achievements of their successors and thereby puts paid to the idea that “late development” is *sui generis*. Second, it brings rural social organization back into the study of industrial transformation more generally and thereby underscores and transcends the “urban bias” (Davis 2004, p. 3) of contemporary development theory. And, finally, it illuminates the “disciplinary” influence of the “rural middle class” and thereby offers a novel interpretation of both Northeast Asia’s late twentieth century growth

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Message from the Chair

Ann Shola Orloff

We finish this academic year in good fiscal and organizational health. Our budget is in the black, and our membership is a healthy 768 members, putting us at the upper end of ASA sections. Our newsletter has gone electronic, which we hope is as helpful for you as it is for our budget. The substance remains lively, thanks to newsletter editor Gianpaolo Baiocchi. Our website has been updated and improved, thanks to webmaster John Dale. I know both Gianpaolo and John continue to welcome suggestions and submissions for both newsletter and website.

This year our section will be sponsoring several sessions that attempt to expand the boundaries of the political, investigating topics often understood as outside the mainstream of our subdiscipline. (And to celebrate, we're going to push the boundaries of reception-giving, too, by meeting outside the hotel, at the beautifully-rehabbed Puck Building, home of the NYU Sociology Department, which has graciously allowed us to use their space... it's a great spot, and very close to all sorts of excellent downtown eating, drinking and entertainment – about which, more later.) Note that, by the luck of the ASA schedule (which gave us roundtables and business meeting in the last session of the last day last year), we will have roundtables and business meeting in the first slot on the first day – so we begin our section day, Saturday, August 11, with roundtables at 8:30 am, followed by the business meeting at 9:30.

As for our sessions, first, we have something of a miniconference on gender, sexuality and politics, with two sessions of papers on that theme, and an “author-meets-critics” panel on a book that takes up the politics of feminism in ways that should provoke and stimulate sociologists of politics, social theorists and students of gender and sexuality.

Gender, Sexuality, and Politics: Rethinking Citizenship and Nation

From a variety of historical and national cases, these papers examine how political categories and subjectivities become gendered and sexualized—often in unexpected ways. From discourses of tradition and modernity in the Islamic Middle East and Singapore to conceptions of universal citizenship and obligation in France and the United States, the papers reassess the role of gender and sexuality in key political and legislative processes.

Time: Sun, Aug 11 - 8:30am - 10:10am; place: Hilton New York

Gender, Sexuality, and Politics: Comparative Perspectives on the Welfare State

These papers all offer reconceptualizations of how and why welfare states develop in gendered and sexualized ways—examining the role of legislative discourse, state structure, and patterns of work/family in the production of welfare provision and social policy in a range of national contexts.

Time: Sat, Aug 11 - 10:30am - 12:10pm; place: Hilton New York

Author Meets Critics:

FEMINISM AND THE ABYSS OF FREEDOM (2005) by Linda Zerilli –

Critics: Julia Adams, Myra Marx Ferree, Andreas Glaeser and George Steinmetz

In both contemporary sociological and feminist theory, the problem of identities and subjectivities persists as the site that grounds most discussion of feminism and other social movements. In *Feminism and the Abyss of Freedom*, political theorist Linda M. G. Zerilli argues that the persistence of this subject-centered frame severely limits feminists' and social scientists' capacities to think about politics creatively, particularly, in the case of feminism, a politics concerned with freedom. Offering both a discussion of feminism in its postmodern context and a critique of contemporary theory, Zerilli challenges feminists to move away from a theory-based approach that focuses on securing or contesting “women” as an analytic category of feminism, to one rooted in political action and judgment. She revisits the democratic problem of exclusion from participation in common affairs and elaborates a freedom-centered feminism as the political practice of beginning anew, world-building, and judging.

Time: Sat, Aug 11 - 4:30pm - 6:10pm; place: Hilton New York

Message, cont'd ...

Continuing our boundary-crossing mode, we take up a form of politics that has often exceeded the parameters of classical political sociology, but which concerns us as citizens and social scientists: terrorism.

Panel: A Political Sociology of Terrorism?

This invited panel will bring together social scientists and policy makers representing a range of viewpoints on the political phenomena now often called terrorism. Panelists will comment on what we know of the causes and processes of terrorism, for example, how humanitarian assistance or military interventions affect support for terrorist tactics or particular kinds of political goals; others take a more deconstructionist approach to the problem, seeking to understand how and why “terrorism” has come to occupy a central place in politics in the US and elsewhere.

Time: Sat, Aug 11 - 2:30pm - 4:10pm; place: Hilton New York

As has been our practice for some time now, we also have roundtables, but with a difference; organizers Barb Brents and John Myles have worked to enhance the possibilities for genuine conversation about the research being presented here by bringing in both senior and junior scholars as discussants. The roundtables feature lots of innovative and exciting research by new and established scholars on topics such as religion and political culture, outsourcing political activism, various studies of social capital, postsocialism and EU expansion and gay and lesbian ethnic enclaves.

Section on Political Sociology Roundtable Sessions (one-hour).

Time: Sat, Aug 11 - 8:30am - 9:30am; place: Hilton New York

Table 01. Sexual Politics/Morality Politics

Table 02. Suturing the Social: Parties, States and Movements in Turkey, India, and the USA

Table 03. Race, Class, Gender and Civic Culture

Table 04. Social Capital

Table 05. Social Capital and Networks

Table 06. Social Movements and Ideological Contests

Table 07. Social Movements and Politics

Table 08. Public Opinion and Politics

Table 09. Voting Behavior

Table 10. State Politics

Table 11. Civic Participation

Table 12. European Expansion and Cultural Change

Finally, please don't forget the business meeting and the reception:

Section on Political Sociology Business Meeting

Time: Sat, Aug 11 - 9:30am - 10:10am; place: Hilton New York

Section on Political Sociology Reception (co-sponsored with Section on Comparative and Historical Sociology)

Time: Sat, Aug 11 - 6:50pm - 8 pm; place: Puck Building, 4th floor, NYU Sociology Department, 295 Lafayette St., New York, NY 10012 (next to the Broadway/Lafayette subway stop on the D and F lines)

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The newsletter welcomes any and all submissions. Please keep letters under 300 words and articles between 750-1500 words. Submissions can be sent to polsoc@soc.umass.edu. All content is left to the discretion of the editor.

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SYMPOSIUM: *Class Matters*, cont'd ...

far ranging comparative and historical analysis of South Korea, Taiwan, Mexico and Argentina to tell us that it is really about the middle class and in particular the smallholder rural middle class. The basic argument is straightforward enough. Davis does not dispute the claims made in the state developmental literature about the key role of the state or the specific policies that made it possible for Taiwan and South Korea, but not Mexico and Argentina, to make the decisive shift at just the right moment from ISI to EOI. Davis also accepts the claim in the developmental state literature that the key to explaining the different paths taken resides in the state's capacity to discipline both labor and capital, in particular by breaking their habit of supporting the non-competitive and rent-seeking logic of ISI protectionism (Import Substitution Industrialization). But it is in the origins of this disciplinary capacity that Davis stakes out new territory by pointing to the pivotal role of the rural middle-class. She thus argues that under certain historical and cultural conditions when middle classes are sufficiently well formed to be aware of their distinct class interests they can provide the state with the political ally as well as the ethos necessary to "enable the state's use of a variety of measures to politically and economically discipline capitalists and laborers for the purpose of generating national prosperity and balanced growth." (2). Davis supports her argument by showing that developmental success (i.e. self-sustaining industrialization) in her four cases - Taiwan, South Korea, Mexico and Argentina - can be traced back to the conditions and circumstances of middle class formation.

The great strength of the book is that the historical analysis presented of each case leaves little doubt that state-rural middle class relations were critical to shaping developmental trajectories. Taiwan is presented as the emblematic case of a state-rural middle-class developmental alliance. Davis documents the symbiotic ties between the KMT state and the small holding rural middle-class and shows how this relationship not only promoted agrarian accumulation, but also eventually created the economic and social basis for Taiwan's small-firm export model. In Korea, land reform and anti-communism also set the stage for a robust smallholding class. The Park Chung Hee regime - which broke decisively with the cronyism of its predecessor - was able to draw on middle class support (buttressed by the regime's own social origins

in this class) to discipline capital and control labor. But because the rural middle class had a precarious economic position based on uneconomical landholdings it ultimately provided less support to the disciplinary regime than in the Taiwanese case. As a result, Park and his successors became increasingly dependent on big business (the powerful Chaebols), which Davis argues explains the increased corruption and declining competitiveness of the South Korean economy. The Mexican case is one of lost opportunity. Because the Mexican revolution did mobilize the middle peasantry and push through land reforms, Mexico might well have developed the sturdy middle class of the East Asian developmental states. Politically however, the rural middle class was marginalized within the PRI and state policies did not promote rural middle class formation. In Argentina, an ethnic divide between rural and urban populations pushed the rural middle class into an alliance with ranchers. In both cases, the relative weakness of the rural middle class opened the road to urban-dominated populist politics. These accommodating regimes, as Davis labels them, were "more likely to accommodate the demands of capital or labor in ways that allow rent-seeking, short-term profit maximization, higher wage rates, and/or protectionist measures that in the long run limit firms' capacities to compete domestically and abroad." (2-3).

This is all very compelling, since there is indeed a clear relationship between the size and robustness of the rural middle class and long-term developmental paths. The argument also makes two important theoretical contributions. First, it resurrects one of the great but often forgotten findings of macro comparative sociology - most famously represented by Barrington Moore - that the modern was born in the agrarian and that if we really want to understand economic transformation we have to first understand agrarian relations. Second, Davis not only resurrects the middle class from the ignominious anonymity to which the devastating critique of modernization theory (that had mounted the middle class on a pedestal) had confined it but also from conventional Marxist analyses that sacrifice the middle class to a dichotomous capital-labor class structure. In recovering the middle class as a protagonist of development Davis restores analytical complexity and historical sensitivity to class-based theories of development. She specifically does this by showing how different patterns of rural middle class

SYMPOSIUM: *Class Matters*, cont'd ...

formation shaped the state's disciplinary capacity.

Where Davis' analysis is less compelling is in spelling out how "a rural middle-class embeddedness produces a state committed to the imperative of capital accumulation." Her treatment of this question is necessarily complex, but also a bit muddled at times. She in effect makes three different kinds of argument about why MC formation would matter so much – its cultural ethos, the political coalitional possibilities it offers state actors and its intrinsic class interests. Of the three, the first is the most novel, but also the least persuasive. Davis claims that the rural middle class has a particular cultural predisposition (attributed in part to its vulnerable material position) towards frugality, hard work and self-control and can as such provide the societal sources of discipline needed for economic success. But is such discipline really a cultural attribute? Davis for example contrasts the famed market productivity of "yeomen farmers" to subsistence-oriented peasants and unproductive landlords. But don't these differences just reflect varied class-relational dynamics? If offered the market access, the incentives and the capacity to use non-family labor, would not all peasants accumulate and become farmers? Similarly, when landlords no longer have a ready supply of squeezable labor or access to extra-economic sources of power, don't they of necessity turn to capitalist (wage-base) farming (Brenner 1985)? Moreover, in her own account of South Korea, where the middle class ethos is developed to its fullest, the rural middle-class itself was so much a product of state intervention that it is no longer clear who is doing what to whom. Indeed, Davis' account of the state's "New Community Movement" (Saemaul Undong) underscores the plasticity of the middle class ethos: within a decade this class went from community-oriented and subsistence-based ethos to market-oriented and competitive one (p. 135).

Davis is on much firmer ground when she emphasizes the class coalitional possibilities that emerge with the rise of a rural middle class. As Davis shows, the middle class in South Korea, itself a product of previous state interventions going back to Japanese colonialism, provided Park with just enough electoral support and legitimacy (though its not entirely clear how this worked) to give the state the leverage it needed during a critical period to force march the economy towards export-oriented industrialization. And in Taiwan the rural middle class was the social basis of its successful

economic take-off. In both cases moreover Davis shows just how deeply interlinked the state and the rural middle class became (though less so in South Korea), giving real meaning to the notion of embeddedness. The fact that a small rural middle in Argentina aligned itself with landed oligarchs and failed to develop ties to urban class interests, and that rural middle class formation in Mexico was thwarted by the resilience of large landed interests and that in both cases these constellations of middle class exclusion shifted the balance of power in favor of urban alliances that favored ISI makes for a compelling comparative argument. Yet, for all the detail that Davis provides in her analysis of these constellations there is still something missing. Even if we accept that an alliance with a middle class gives the state leverage against capital and labor, it is still not clear why that leverage necessarily translates into policies that are good for industrialization. There is a working assumption here (even beyond the ethos argument) that rural middle classes have a pro-accumulation logic. But even if one accepts that because the rural middle class owns property and it may indeed have a capitalist ethos (property =embourgeoisement), this is not the same thing as generalized support for accumulation. Davis provides bits and pieces of an argument for why a strong rural middle class might be good for accumulation – such as the more encompassing nature of the middle class and the greater demand for backward and forward linkages – but nothing quite as compelling as the argument made for the relationship of working class power to democratization (Rueschemeyer, Stephens and Stephens, 1992) or the welfare state (Esping-Anderson, 1990). Again, the relationship between the middle class and industrialization appears to be more contingent and relational (compare British and French small farmers) than intrinsic.

But then of course development is about much more than industrialization, and here Davis' focus on the role of the rural middle class may actually have lessons that carry well beyond her focus on growth. In her conclusion, Davis argues that the early formation of a rural middle class not only ensures better urban-rural economic linkages, but also a greater degree of state penetration into the countryside, including more decentralized institutions. This could really go either way with a high-powered state being able to successfully

SYMPOSIUM: *Class Matters*, cont'd ...

reform agrarian relations and give birth to a propertied middle class (e.g. the impact of Japanese colonialism on South Korea), or an emergent smallholder class that draws the state in (or strengthens the local state) by making demands for rural public goods, enforcement of property rights or social services (Tocqueville's America). Either way, the notion that state-rural linkages are critical to shaping more inclusive forms of development holds sway for explaining Scandinavia (where farmer demands were critical to building the modern welfare state) as well as Kerala, Mauritius and Costa Rica (Sandbrook, Edelman, Heller and Teichman), all cases of inclusive development before

industrialization. So while the middle class may only have a contingent relation to growth, it might very well have an intrinsic relation to the modern state, that is a state that can provide public goods, including the rule of law. Davis has reopened the debate on the comparative political economy of growth, but maybe more importantly she has pointed to new ways in which comparative political sociology might explain development trajectories.

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SYMPOSIUM: *Discipline and Development*, cont'd ...

miracle and Latin America's concurrent malaise. The first two contributions are likely to be overshadowed by the third, however, and I'll therefore begin by contextualizing and calling attention to the book's profound—if at times understated—challenge to the sociology of developing societies as a whole.

Sociologists have engendered and embraced three different approaches to the study of developing societies since the end of the Second World War: modernization theory; dependency and world-systems perspectives; and state-centered alternatives. While modernization theorists assumed that economic development would not only occur but would in all likelihood follow the same formula (i.e., industrialization, urbanization, redistribution, etc.) throughout the developing world, albeit at different paces in different places, their critics portrayed late development as a singularly daunting task and therefore issued a markedly different prognosis. "Somewhere near the outer periphery of Europe," argued the late Sidney Pollard, "there is the watershed between societies that on contact with the new industrialism were capable of imitating it and becoming part of it, and areas which, at least for a long period, were transformed away from it, becoming specialized as 'colonial' economies and facing an ever wider gap between themselves and the advanced economies" (Pollard 1973, p. 644).

By underscoring the myriad differences between the early developers and their imitators, however, critics like Pollard not only dethroned modernization theory but simultaneously opened the door to a

factious debate over the origins and implications of the gap itself. While dependency theorists assumed that growth in the proverbial core of the world economy presupposed stagnation in the so-called periphery, and therefore anticipated the inexorable "development of underdevelopment" (Frank 1969), their critics acknowledged not only the reality but the significance of the Asian miracle, and therefore underscored the theoretical importance—as well as the public sector underpinnings—of "dependency management" (Gereffi 1989, p. 510). Thus, elite bureaucrats and state managers assumed pride of place in the late twentieth century sociology of developing societies.

Figure 1 collapses the three perspectives—modernization, dependency/world-systems, and state-centered—into a two dimensional space. The first dimension captures two different attitudes toward the process of economic development. While modernization theorists maintain that the underpinnings of development are always and everywhere the same (*semper idem*), and therefore draw little or no distinction between first movers and latecomers, their successors believe that late development is *sui generis*, and therefore disdain the quest for Western European development models. The second dimension captures two distinct attitudes toward the likelihood of economic development. While modernization theory and the dependency/world-systems approach offer deterministic—if discordant—predictions, and thereby leave sufficient space for neither human agency nor historical contingency on the periphery, their state-

SYMPOSIUM: *Discipline and Development*, cont'd ...

Figure 1: Rival perspectives on economic development

Economic development: the prognosis	Economic development: the process		
		<i>Semper idem</i>	<i>Sui generis</i>
	<i>Deterministic</i>	Modernization theory: Economic Development is inevitable.	Dependency/world systems approaches: Late development is all but impossible.
	<i>Conditional</i>		State-centered approaches: The prospects of late development rise and fall with the developmental state

centered successor is historically conditional, and thereby accommodates distinct outcomes in different times and places.

Davis finds fault with all three perspectives. While she finds determinism unacceptable, and therefore rejects both the modernization and dependency approaches, she finds state autonomy improbable, and therefore pursues a society-centered alternative rooted in the nature and strength of the middle class. “One should not fall into the tautological trap of assuming that states which discipline capital and labor are autonomous,” she writes. “States may be able to cultivate policy-making independence from capital and labor, or discipline them, precisely because they count on middle classes as their principal source of legitimacy or political support” (Davis 2004, p. 8, note 18).

In fact, Davis portrays South Korea’s pursuit of export-led industrialization not as a “technically expedient calculation made by autonomous state elites” (Davis 2004, p. 151) but as a fortuitous by-product of Park Chung Hee’s political commitment to a specifically rural middle class. “With modest farmers backing him at almost every turn,” she writes, “Park became a heavy-handed disciplinarian of bankers and large industrial capitalists, who were soon goaded (if not forced) to generate sufficient industrial export earnings so the South Korean state might foster the growth of a dynamic agricultural sector and a strong rural middle class of farmers” (Davis 2004, pp. vii-

viii).

Nevertheless, the benefits of “rural middle class embeddedness” (Davis 2004, p. 11) extend beyond the political to the cultural and include the propagation of values that are deemed essential to growth and development. According to Davis, the rural middle class not only depends upon but fosters disciplinary attitudes like prudence, thrift, and industry. “When this type of disciplinary ethos infuses both society and the state,” she continues, “the developmental gains are enormous” (Davis 2004, p. 11).

Davis defends her “rural middle-class-based ‘model’ of disciplinary development” (Davis 2004, p. 341) by invoking not only the experiences of South Korea and Taiwan, where the Kuomintang encouraged the growth of a particularly dynamic class of commercial family farmers in the 1950s and 1960s, but the importance of urban-rural middle class linkages in Europe and North America as well, and thereby concludes that the underpinnings of national development are “historically resilient” (Davis 2004, p. 341), if not quite *semper idem*. “Only with this rural middle-class-based interactive dynamic at play,” she concludes, “will countries be able to achieve self-generative developmental gains that spread relatively equally across city and countryside, thereby eliminating the market and political obstacles to national development identified

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SYMPOSIUM: *Discipline and Development*, cont'd ...

with rural impoverishment and attendant patterns of overurbanization” (Davis 2004, p. 341).

Latin America provides proof positive. According to Davis, the region’s rural populations are divided by region, class, and culture and therefore offer their respective public sectors neither powerful political allies nor disciplinary pressures and values. The consequences are entirely too predictable. Latin American parties and politicians compensate for the absence of rural support by soliciting—and paying a high price for—urban support. And the populations of countries like Argentina and Mexico suffer the myriad consequences of their costly populist campaigns.

In short, Davis maintains that the character and likelihood of national development are ultimately circumscribed by neither the timing of industrial takeoff nor geography per se but by the weight and influence of the rural middle class—and she thereby breathes new life into the currently underpopulated southwestern quadrant of Figure 1. Neither the importance nor the timeliness of her contribution should be underestimated. After all, Davis not only underscores the urban bias of the existing literature but simultaneously illuminates the ostensibly autonomous developmental state’s hard-earned rural support. Her book therefore complements a small but growing body of literature on the mutual interdependence of allegedly autonomous authoritarian rulers and their peasant supporters (see Turits 2003); calls the regnant state-centered approach to development theory into question; and provides a plausible society-centered alternative.

Davis is modest to a fault, however, and admits that her argument is only as persuasive as her sources (Davis 2004, p. 361). Do she and her sources offer a compelling account of East Asia’s divergence from Latin America? On the one hand, I think that differences in rural social structure have all of the hallmarks of a compelling explanans. First, they are clearly and quite literally exogenous variables. Northeast Asia’s atypically egalitarian rural sectors are products of Japanese colonial practices and externally imposed or supported land reforms. Second, they successfully differentiate the cases in question: Northeast Asian societies combine relatively egalitarian rural social structures and prosperity; and Latin American societies combine decidedly inequitable rural sectors and crisis. And, finally, they are consistent with theoretical expectations. A

substantial and growing body of scholarship portrays commercial family farming as the *differentia specifica* of modern economic growth.

On the other hand, I remain agnostic about the mechanism in question. While Davis traces the emergence and achievements of the so-called East Asian development model to the cultural and political influence of the rural middle class, and thereby underscores “significant commonalities” (Davis 2004, p. 350) between early and late developers, she devotes less time and attention to a related body of scholarship that traces the roots of industrial transformation not to the presence of family farmers but to the absence of landed oligarchies. After all, John Sheahan makes a compelling case that “the identification of export-led growth with landowner interests” served to discourage industrial as well as agricultural export promotion in late twentieth century Latin America (Sheahan 1987, p. 76). And Alice Amsden holds the “quasi-rents” captured by rural oligarchies responsible for the relative dearth of intersectoral capital mobility in latecomer societies more generally (Amsden 2001, p. 18).

Is the Asian miracle a product of the growth of the rural middle class or the elimination of traditional landlords? My point is neither to answer the question nor to criticize *Discipline and Development* but instead to spark further debate and discussion. The relevant variables are obviously collinear. The two perspectives are broadly compatible. And the theories they engender are clearly located in the southwestern quadrant of Figure 1. If sociologists want to make meaningful contributions to the study of national development in the years to come, as well they should, they would do well to spend less time arguing in (or across) the other quadrants and more time discussing the relative merits of the various ideas and perspectives found within the southwestern zone—and if they do so, they will have Diane Davis and her agenda-setting book to thank for getting the ball rolling.

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SYMPOSIUM: Rural Middle Classes, the Disciplinary State and the Disciplined Capital and Labor?

Hsin-Huang Michael Hsiao

Diane Davis' main thesis in her impressive book, *Discipline and Development* (Cambridge University Press, 2004), is quite straightforward and inspiring. She argues that the core issue is whether or not the rural middle classes, rather than the urban middle classes, have sustained the developmental state's determination and capacity to discipline the capitalists and the working class. Put differently, developmental regimes in Taiwan and South Korea enjoyed the relative autonomy (in Peter Evans' term) in the embedded class configuration to exercise necessary disciplinary measures over capital and labor because the rural middle classes provided political loyalty and stability in the rural areas, which was essential to the maintenance of state power. Such a stable power base turned out to be the prerequisite for what Davis has termed the "disciplinary regime of development" in East Asia. In contrast, Argentina and Mexico the rural middle classes failed to push the state to discipline both capital and labor. Therefore, Latin American states were too compromising /accommodating to the class demands of the capitalists and workers to enforce "balanced" industrialization and national development.

Davis has first developed her argument in a chapter for my edited book, *East Asian Middle Classes in Comparative Perspective* (Academia Sinica, Taipei, 1999) where she claimed that middle classes should not be simply seen as the product of industrial development, and on the contrary, rural middle classes could in fact define and delimit the course of industrialization. The rural middle class should be located in the center of the equation of both state-class embeddedness and state-facilitated development course. Her claim can be challenged by scholars who might take the urban new middle classes as the focus of research and therefore they see middle classes virtually as dependent variables rather than independent variables of industrialization and economic development. What Davis has called attention is the role of those mostly ignored segment of rural old middle classes in influencing the state's development policy-making by balancing the class pressures from capital and labor. To me, such assertion is quite illuminating as I have myself taken state-class relations seriously for both the initial and later phases of economic development in postwar Taiwan and South

Korea. The rural middle classes certainly have played an important role in state's industrial development process especially in the initial industrialization and right after land reform. In fact, it was because of land reform that a class of small independent farmers emerged out of a previously tenant class. Only in the context of state-enforced land reform policy can rural middle classes be correctly understood. In this sense, if a rural middle class may later turn out to be the cause of state-backed capitalist development, but they are also a direct consequence of prior state-initiated mega-policy of land reform. Such clarification seems to be crucial in correctly assessing Davis' line of argument.

If I were to rephrase Davis' assertion of the state-rural middle class dynamics, taking Taiwan and South Korea as two East Asian cases, the following theoretical-empirical questions are of immediate attention. First, what is the nature of power relations between states and rural middle classes after land reforms? Are rural middle classes such as small farmers and rural merchants independent of authoritarian state dominance? Is disciplinary development in fact the act of authoritarian state domination over economic activities? Second, how have states politically maneuvered with the rural middle classes against the capitalists and the working class? To what extent are rural middle classes are successfully pacified and manipulated so that states can enjoy the necessary autonomy in dealing with capital and labor in both making development policies and developing class alignment and realignment?

Davis' theorizing of class- embedded state in Taiwan and South Korea is more or less class-centered and, therefore, class pressures must be taken seriously in understanding the process of policy- making. That assumption is used to explain how rural middle classes have been perceived by the state to be a priority for the class coalition in the initial ISI development strategies. But why examine small farmers and old middle class in the rural areas? To this question, Davis does not provide a consistent and satisfactory explanation. Davis seems to tell us that the Confucian states in both Taiwan and South Korea really regarded the rural-agricultural sector as the great cultural foundations and rural

SYMPOSIUM: *Rural Middle Classes*, cont'd...

middle classes that genuinely mattered in both states' development policies.

If that is true, why did both states later during the EOI industrialization phase betray and even "kill the rural middle class goose that laid the golden egg" in favor of the industrial capitalist interest? The plausible rationale, to me, lies in the state-centered reality all along in which rural middle classes such as small land holding farmers have been completely transformed into conservative and stable political base as the most salient result of state-dominated land reforms in Taiwan and South Korea. The state could then assume rural middle classes support and it could freely move to deal with both capital and labor by way of "disciplinary measures". Obviously, the power relations between states and post-land-reform rural middle classes were biased and unequal. The cultural explanation offered in the book that takes Confucianism and agrarian ideology as the key for the state to deeply respect for and allegiance to small farmers is far too simplistic. Moreover, such culturalist theorizing may also be too easy to provide a sophisticated explanation on the differences between East Asia and Latin America.

In other words, in Taiwan and South Korea, one has witnessed how the state has first successfully "disciplined" the emergent conservative rural middle class by changing its political character through land reform, and then used that rural class to "discipline" and "counter balance" the rising demands from the capitalists and working classes. Later, the state shifted its class alignment and sector coalition from rural/agricultural to urban/ industrial interests by enforcing an "agriculture squeezing strategy" to favor urban

middle and working classes and industrial capitalists. It is quite evident that the authoritarian state has always been the winner in both ISI and EOI developmental phases. The rural middle class, industrial capitalists, urban middle class and the working class are virtually the changing objects for the state at one time or another to direct and redirect its class alignment and realignment.

What Mexico and Argentina have differed from Taiwan and South Korea was that both states failed to take care of the rural middle class issue by means of land reform in the first place, due to the tremendous political resistance from the large landed class. Such comparative difference and its impact on the later disciplinary capacity on the part of the state over capital and labor in the following phases of industrial development in the four newly industrialized countries should be well explained by a sophisticated political economy analysis rather than a self-disciplined cultural interpretation.

Davis' challenge to the popular assumption on the cause-effect relations between middle class and development in East Asia and Latin America is well taken. Her claim to bring the rural middle class segment into the theoretical debate also certainly makes a contribution to the field of comparative sociology of development. However, the relations between the state, rural middle class, capitalists, and labor in different phases of state-led development remains to be fully disentangled.

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Article & Book Announcement:

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SYMPOSIUM: Charting New Territory in the Political Economy of Development: A Rejoinder to Shrank, Heller, and Hsiao

Diane E. Davis

What a sense of déjà vu enveloped me as I read these excellent commentaries from three important scholars in the field of development studies, whose work I greatly respect. Beginning with Andrew Shrank's lingering doubts about whether it was the presence of a rural middle class or absence of a landed oligarchy, moving to Patrick Heller's disquiet about the relative value of the cultural versus class versus political coalitional explanations, and ending with Michael Hsiao's concern about whether development progress was a consequence of the states' own power to mold the activities and orientation of the rural middle class versus their own *sui generis* actions, I felt thrown back into the same analytical soul-searching that dominated the long *duree* of archival and secondary research that led to the publication of *Discipline and Development* in 2004. Not for nothing did it take eight years to finish that book. Of course, immersing myself in the historical study of two new (for me) countries in East Asia and deepening an initially superficial knowledge of Argentina were quite time-consuming tasks in themselves. But what really extended the elusive goal of analytical closure was a constant engagement with the same theoretical and methodological dilemmas posed by these three authors, and the nagging sense that I had to take a firm position one way or the other before the manuscript could be completed.

Lengthening the personal agony, the only way I felt I could make progress in ascertaining a fixed stance on these issues was to return to empirical evidence drawn from the four cases at hand: to go deeper into comparisons both within and between countries and look for any "counter-factual" evidence that would sustain or preclude support for one position or the other. I think that was the right move, and still believe that the process was a valuable one for advancing the theory and documented trajectories of late industrialization. But over time, as going back to the evidence only made argument more dense and complex (if not "muddled," as Patrick Heller suggests), I would say that I am less interested in taking a firm position on each of these either/or framings or analytic entry points, and more interested in highlighting the dialectic or interactive relationship between these sets of competing antinomies and explanations.

This posture probably owes to my views on the other analytical oppositions that weave like a thread throughout all three commentaries, and that are so masterfully laid out by Andrew Shrank in his commentary: the structured versus contingent character of late development outcomes and the question of whether late industrializers follow their own unique path (*sui generis*) or a more universal (*simper idem*) process of development. As an historical sociologist interested in grounded empirical evidence and the interplay of agency and structure, I must admit having a hard time fully embracing a commentary that puts me in one corner of a four-fold box, despite my clear regard for Andrew's virtuoso analytical skills in placing *Discipline and Development* in a larger body of work, and despite his much appreciated point that this particular box lacks sufficient research and could use more rigorous debate. Still, my reluctance to put the arguments of this book into one corner of the box owes to my own rejection of these antinomies, as well as my sense that we as scholars will know more about development trajectories if we are willing to recognize the mix of the universal and unique in every country's process, and to tread the blurry and at times frustratingly ambiguous line between contingent and determined outcomes that can only come with good comparative and historical research.

Let me make these points with concrete evidence, and do so by starting with Andrew Shrank's important question of whether more emphasis should have been given to the absence of a landed oligarchy, rather than to the presence of the rural middle class. I pondered this issue long and hard, especially when reading the literature on the relative importance of land reform in the East Asian developmental success story (a point also raised by Michael Hsiao). Land reform could have been seen as the principal mechanism for eliminating the market distortions imposed by a landed oligarchy and providing the material basis for the development of a rural middle class. As such, more emphasis on land reform might have allowed me to sidestep the messy and more problematic question of whether it was rural middle class presence or landed oligarchic absence that determined outcomes, by offering a reason for their "collinearity," and at the same time quelled Andrew's concerns about my failures to identify a "mechanism"

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that catapulted East Asia to success. Still, I was reluctant to focus explanatory attention on such a straightforward and singularly powerful mechanism like land reform, and particularly this one, not just because I had no desire to resuscitate a well-worn theoretical argument from the past, but also because the empirical facts suggested that the role of land reform and its impact on rural class structure and developmental success were not nearly as straightforward as the literature suggested.

In particular, the development trajectory of the Mexican case -- where land reform also was introduced -- was quite different from Taiwan and South Korea, and more like Argentina, where land reform was permanently forestalled. Moreover, the relationship of land reform to the presence/absence of a landed oligarchy was much more problematic in Latin America than the literature focusing on the East Asian cases seemed to suggest. In Taiwan and South Korea, land reform did indeed eliminate the landed oligarchy, thereby allowing the growth of a rural middle class and developmental success (raising again the question of which of these two factors was determinant). But in Mexico and Argentina the relationship between land reform, the presence of a landed oligarchy, the emergence of a rural middle class, and development outcomes was much more complex and less linear. Moreover, there was no direct relationship (i.e. collinearity) between the presence of a rural middle class and the absence of a landed oligarchy -- whether produced by land reform or otherwise -- let alone between either of these forces and overall development paths. In Mexico, despite a serious government-supported land reform, the same economically vibrant rural middle class as emerged in East Asia did not materialize, nor did the same developmental success. [Truth be told, even Taiwan and South Korea differed considerably in terms of rural middle class vibrancy and developmental success, as noted in the book, despite the fact that land reform similarly eliminated the landed oligarchy in both countries]. In contrast, in Mexico land reform did not completely eliminate the landed oligarchy, which co-existed along with an economically weak rural middle class. And this reality (co-existence of rural middle class and a landed oligarchy) paralleled the case of Argentina, despite the absence of land reform.

The point here is not that land reform was universally unimportant. Nor is it that the absence of a landed oligarchy was completely insignificant. All

these factors were important in East Asia, and perhaps even necessary for pushing Taiwan and South Korea in certain developmental directions. But their impacts on development were neither direct, nor universally-given, nor understandable without a focus on the rural middle class. Indeed, in all four cases there were significant variations within and between rural conditions (land reform and agrarian class structure) and developmental outcomes, suggesting that any single mechanism -- either land reform, the presence or absence of a landed oligarchy, or even the presence of a rural middle class -- producing developmental successes in one case will not always produce similar outcomes in another country context. In terms of specific propositions, the comparative and historical evidence drawn from these four case studies showed that developmental outcomes had less to do with land reform or the mere presence or absence of middle class and/or oligarchic forces in the countryside, and more to do with the larger political, cultural, and state context in which rural middle class forces were able to prosper and politically insert themselves in the state. That is, it was not merely rural class structure materially given by land ownership policies and patterns that mattered most, but rather, the cultural, political, social and state orientations that enabled or disabled the rural middle class and, by so doing, made them the key protagonists of disciplinary or accommodating development, thus determining divergent outcomes.

Accordingly, I appreciate and want to underscore the importance of Patrick Heller's close reading of this book as offering three different dimensions of determination: cultural, class, and political-coalitional, as well as his suggestion that in my book "the relationship between the middle class and industrialization appears to be more contingent and relational than intrinsic." Unlike Heller, though, I see this as the book's strength, not a weakness. That is the point made earlier, and it is the reason I resist the urge to privilege either cultural, class, or political coalitional explanations in my narrative, a preference that is implicit in Shrank and Hsiao's calls for a direct focus on rural class structure and authoritarian state actions, respectively. To be sure, Heller's comment was not a demand for a singular determining variable so much as a reasonable call for more clarity in explaining the interactions between culture, political coalitions, and policies and how they are good for industrialization. I fully concur that this could have been better developed

 SYMPOSIUM: *Rejoinder*, cont'd...

in the book. Heller also is properly skeptical about the proposition that discipline is a cultural “predisposition” that may have explanatory power in and of itself, and his suggestion that it may be a product of changing material conditions (market access, the use of family labor, etc.) shows why. Again, I fully agree with this position, and concur that discipline as a cultural ethos does not exist independent of material/class condition. This, however, is one of the key claims of the book: that the “culture” of discipline is grounded in the material conditions of small-scale rural production, especially (though not exclusively) in the constraints on farming imposed by unpredictable environmental conditions and undercapitalized sweat equity.

But I also have taken Heller’s request to ground the cultural ethos in the material conditions of production one step further, or better said, beyond class, resource access, and labor process conditions, alone. Indeed, much of the book seeks to show that the disciplinary ethos is contingent on a variety of political and historical conditions, not merely “culture” or class; and only under certain political, historical, social, class-relational, and state conditions can anything presumed to be a “cultural predisposition” drawn from small-holder farming can become institutionalized as a discourse, a mind-frame, and a tangible reference point for citizen actions, for state programs, and for macroeconomic development policies.

This is perhaps where Michael Hsiao’s critique is most important. Hsiao also takes umbrage at my focus on cultures of discipline, especially as drawn from Confucianism or agrarian ideology, and suggests that this is overplayed if not simplistic because it does not take into account the ways a strong and authoritarian state helped created such conditions in the first place. By making this point, he properly argues that the state was key to developmental gains in East Asia because it “successfully ‘disciplined’ the emergent conservative rural middle class by changing its political character through land reform and then used that rural class to ‘discipline’ and ‘counter balance’ the rising demands from the capitalists and the working classes.” In this claim, Hsiao offers the “statist” equivalent to Heller’s “class” critique of the culturalist notion of discipline: he insists that state actions, much as Heller did with the focus on material/class conditions, nurtured the development of a disciplinary ethos. Yet this then begs the question of which is the most significant

material grounding for the rural middle class-based cultures of discipline identified in my book, state or class? And why must it be one or the other? Why not try to understand the dialectic of all these forces and conditions over time? After all, in his critique Hsiao articulates this position even better than I, by suggesting that the relationship between rural class structure, a disciplinary ethos, and state actions are interactive and mutually reinforcing, with the state’s forceful imposition of development policies (*vis-à-vis* land or workers or capitalists) acting as both cause and effect of changing conditions on the ground, whether seen in terms of the ascendance of a disciplinary ethos or the rural middle class that gives it discursive life.

Having suggested this for East Asia, it is a pity that Hsiao did not also recognize a similar set of historical interactions in Latin America, evidenced by the book’s claim that land reform did not produce a disciplinary rural middle class in the Latin American cases not merely because “of the tremendous political resistance of the big landed classes,” to use his words, but because the authoritarian nature of the state (and its comparable desire to be “a winner” no matter the development strategy employed) brought the Mexican and Argentine regimes to respond differently to rural and urban middle classes in terms of both land policies and disciplinary expectations, given the unique history, culture, and politico-coalitional dynamics of the country. Both contingent and determined, both *semper idem* and *sui generis*, again.

What has been made so evident in the give and take produced by these commentaries is how we as development scholars are wont to tie to ourselves to particular analytic points of entry and interpret complex stories through a singular lens of theoretical determination, selected because of its potential to amplify and deepen our outstanding of outcomes. I too am drawn by this approach; but I also have found that as time passes my repertoire of lenses keeps on expanding and my historical knowledge deepens, making such an aim almost impossible. To be sure, I am the first to admit that this is frustrating, because it necessitates serious comparative research and a deep immersion in history in order to tease out the contingent and determined, or the unique and the universal, in any argument about development. I am also the first to admit that in laying out this complexity in *Discipline*

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SYMPOSIUM: *Rejoinder*, cont'd...

and Development, the cast of characters I have presented are neither new nor novel. Even throwing the middle class and culture into the heap, along with the state, with working class-industry relations, and the focus on political coalitions, is hardly unusual. Thus, what I consider to be the most novel aspect of this book was hardly mentioned at all: the focus on the spatial and territorial underpinnings of late development, both successful and failed.

So why did this issue elude commentary? Is it also a result of the analytical straightjacket imposed by a search for singular explanations and a pre-occupation with the “usual suspects” of classes, politics, and the state? Indeed, with the exception of some attention paid to forward-backward linkages between agriculture and industry, there was little commentary on the book’s suggestion that the development trajectories in these countries also have been molded by such spatial configurations and geographic conditions as over-urbanization, country size, regional fragmentation, rural-urban networks, key protagonists’ location in the city versus the countryside, and so on.

With closer examination of these divergent spatial and geographic conditions in Mexico, Argentina, South Korea, and Taiwan, especially as they mold and are molded by class, cultural, political, and economic activities, we may have a better understanding of why certain political coalitions materialized, why land reform failed, why rural middle classes divided among

themselves, whether cultural discourses united or divided rural and urban middle classes, why the state supported certain political or economic strategies, and how successful states were in doing so, among other key things. In combination with the culture, class, politics, and state-centered analytic lenses, perhaps such spatially informed knowledge can move the field of development beyond constraining antinomies and inspire new theoretical and empirical propositions that focus on dialectics rather than singular determinations. Such a perspective has already been introduced in studies of the advanced capitalist world by scholars like David Harvey, who long ago argued that “space and the political organization of space express social relationships but also react back on them... Industrialization, once the producer of urbanism, is now being produced by it.” (Soja, 1980:1) Now it is time for us to seriously build on this insight in studies of the developing world.

Let me close, then, with a plea to introduce a spatial imagination into the study of the politics of late industrialization and national development, and to combine this with our historical imagination and our knowledge of classes, states, and culture to produce some new ideas to enliven the field.

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Symposium on The Politics of Free Markets, by Monica Prasad, Chicago: University of Chicago Press, 2006

Does Adversarialism Cause Neoliberalism?

Isaac Martin

1.

The *Politics of Free Markets* (Prasad 2006) is a major contribution to political sociology. It addresses a central mystery in the field: why did the rich democracies suddenly embrace neoliberal policies in the last decades of the twentieth century—cutting taxes, abandoning welfare commitments, and reducing the state’s control over industry? And it does not pin the blame for neoliberal policies on the usual suspects—globalization, the strength of business, the weakness of labor, or the power of an idea whose time had come—but instead on a most unlikely suspect: democracy. These countries adopted neoliberal policies because those policies proved popular with the electorate.

The book displays the results of a prodigious research effort on neoliberal initiatives, including primary textual research and interviews on three policy domains in each of four countries. Prasad follows the efforts of policy makers in the United States, Britain, France, and Germany during the period from 1973 to 1989 to make the tax system more regressive, decrease government control of industry, and cut public spending for major welfare programs. In the United States and the United Kingdom, she argues, these proposals succeeded early and often. In France and Germany, by contrast, successes were few, delayed, and partial.

Prasad traces this divergence to the institutional structure of the postwar political economy. In France and Germany, corporatist bargaining and the delegation of policy to non-partisan experts fostered cooperative decision-making (p. 23). Governments furthermore lavished generous subsidies on everyone, especially the middle classes. This model of “political economy as nation-building” proved resilient because its policies were popular. Politicians could not do much to limit the state’s role in the economy, because the voting public would not stand for it.

In the U.S. and Britain, the postwar political economy rested on adversarial decision-making

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The role of ideas in the rise of neoliberalism

Greta Krippner

In reading Monica Prasad’s accomplished new book, I was reminded of a comment Philip Abrams made in diagnosing the many obstacles to studying the state. Abrams wrote, “Any attempt to examine politically institutionalized power at close quarters is...liable to bring to light the fact that an integral element of such power is the...ability to withhold information, deny observation, and dictate the terms of knowledge” (1988, p. 62). Even more troubling, Abrams noted, is that when official secrets are finally revealed, they often “turn out to be both trivial and theoretically predictable.”

In view of such difficulties, Prasad’s *The Politics of Free Markets* offers hope that sometimes persistence and tenacity pay off. This exhaustively researched book delves into the world of official secrets and produces findings that are far from theoretically predictable, and hardly trivial in their implications. The most remarkable feature of the book is perhaps its ambitious scope: this account of the rise of neoliberal economic policies examines four countries and three policy areas. Comparative historical researchers handling this many cases tend to shy away from primary research, letting insights come not from materials unearthed in archives, but from an elegant comparative design. The elegant design is here—Prasad skillfully works between her cases in order to draw attention to theoretical anomalies—but so too is an astonishing amount of primary research.

While Prasad’s cases are complex, the thread of her argument never unravels, but provides a coherent framework for this well organized book. Put simply, the thesis of the book is that divergent paths to neoliberalism are best explained by the intersection of politics and policies. More specifically, Prasad argues that the adversarial politics of the United States and Britain allowed a radical version of neoliberalism to take root; in the absence of similar cleavages in West Germany and France, political actors in those

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SYMPOSIUM: *Adversarialism and Neoliberalism, Cont'd...*

structures—including conflictual union bargaining and government by alternating single-party cabinets of the Left and Right. These states also implemented adversarial policies that pit business against labor and the middle class against the poor. This model of “political economy as justice” was sustainable only as long as the beneficiaries of redistribution were in the majority. But economic growth eroded this majority until most of the voting public no longer perceived itself to benefit from redistribution (p. 280).

Neoliberalism arose as an electoral strategy to appeal to this public. Political entrepreneurs and demagogues sought out and exploited the dissatisfaction of the newly affluent majority in order to get elected. Prasad’s argument on this point is not that the policy preferences of the median voter determined policy. The public’s policy preferences are often weakly held, incoherent, and unknown to policy makers (p. 268). But majorities were clearly dissatisfied with the status quo, and politicians sought out and exploited this dissatisfaction in order to get elected. Although voters did not spontaneously demand tax cuts, deregulation, privatization, or social spending cuts (see, e.g., p. 46), all of these policies proved to be popular. Neoliberalism is a tyranny of the majority.

This provocative argument challenges the assumption of political theorists from Aristotle onward that greater democracy will lead to greater redistribution. It also challenges a literature on globalization that sees neoliberal policy as evidence for the power of business to subvert democracy. At least in the first world, Prasad argues, neoliberalism succeeded where it was the democratic alternative.

2.

But what is neoliberalism? The book’s definition is unconventional, and perhaps also inconsistently applied. Consider tax policy. The book defines neoliberal tax policies as those that “favor capital accumulation over income redistribution” (p. 4). This definition seems to imply flat income tax rates, and low tax rates on capital income. But by this definition, France would seem to be a neoliberal success story: according to the data presented in the book, the average effective tax rate on capital income in France even before Jacques Chirac came to power was less than half of what it was in Britain after ten years of Margaret Thatcher’s neoliberal revolution (p. 30).

So maybe a neoliberal tax policy is a tax cut that reduces tax rates on high incomes and income from capital. This description certainly applies to Prasad’s American case, the Economic Recovery Tax Act (ERTA) of 1981, which cut income tax rates, indexed brackets for inflation, and filled the tax code with special tax breaks for specific industries and firms (p. 59). But it is hard to argue that ERTA had anything to do with free markets. It introduced new tax breaks that protected ailing industries from the market and favored inefficient investments. According to estimates at the time, the resulting waste would cost the economy \$3-5 billion per year (Gann 1985). The other major tax acts of Reagan’s presidency, particularly the Tax Reform Act (TRA) of 1986, were partly efforts to undo these market distortions and repeal the ERTA tax breaks. So was ERTA really neoliberal?

These matters of definition are important because a different definition—with correspondingly different cases—would suggest different inferences about the causes of neoliberal policy. Had the book examined the TRA, for example, it would have qualified the argument that resentment of progressive taxation was what won the middle class over to the side of neoliberalism. The public was not concerned about heavy taxes on the rich. To the contrary, policy-makers seem to have been more worried about the public perception that the rich were escaping taxation (Conlan, Wrightson, and Beam 1990).

3.

This brings me to my second question: what exactly is adversarial about “political economy as justice”? Many things get called adversarial in this book, including conflictual union bargaining, coalition-averse governments, means-tested welfare policies, progressive taxes, and punitive business regulations (p. 24). It is worth asking whether all of these things really belong together under a single heading, and how we might know adversarialism when we see it.

Prasad tells us that we may recognize adversarial policies by the fact that they create conflicting incentives for different groups (p. 35). But this describes most policies. Progressive income taxes, e.g., fall more heavily on the rich, who are in higher tax brackets; sales taxes fall more heavily on the poor, who consume a greater share of their income. In both cases we might predict conflict. In neither case is it

SYMPOSIUM: *Adversarialism and Neoliberalism, Cont'd...*

immediately obvious which side the people in the middle of the income distribution will take.

We may also recognize adversarial policies, Prasad tells us, by how they structure the flow of information. Sales taxes, e.g., are non-adversarial in part because they are “invisible”—meaning that taxpayers do not receive a notice reminding them of the total sales tax bill every year (p. 243). But are taxpayers really deceived? One recent study suggests that people are more likely to overestimate their sales tax burdens than their income tax burdens (Gemmell, Morrissey, and Pinar 2003). Another study finds that a sales tax increase is more likely than any other kind of tax increase to cost the incumbent party votes (Landon and Ryan 1997). And new comparative data from thirteen countries, including Prasad’s cases, suggest that sales taxes are, in fact, the most likely to provoke collective protest (Martin and Gabay 2007).

As this example illustrates, it is not always immediately clear which policies and structures are

adversarial—and so it is unclear whether adversarialism is really what distinguished the U.S. and Britain from France and Germany.

4.

These criticisms address Prasad’s argument about why neoliberal policies were perceived to be popular in the U.S. and Britain. But they do not touch what I take to be the core argument of the book: that politicians introduced neoliberal economic policies in the rich countries because they thought these policies were actually or potentially popular with voters. I am persuaded that this hypothesis is right, at least as applies to these policy domains in the rich countries. And I look forward to other scholars testing its application more broadly, in other policy domains and other countries.

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SYMPOSIUM: *The Role of Ideas, Cont'd...*

countries were unable to use neoliberal policies to mobilize electoral constituencies in their favor. As a result, the neoliberal policies implemented in the Continental economies were generally of a milder, less aggressive sort. These differences in political climate were reinforced by the nature of policy regimes in each country, with divisive policy legacies in the United States and Britain making those countries vulnerable to radical innovation, whereas corporatist traditions and a pro-growth bias in West Germany and France resulted in more stable social structures.

As a proponent of “state-centered” research, Prasad’s analysis of neoliberalism privileges factors such as the permeability of state institutions to new political actors, the structure of systems of party competition, and the design and implementation of specific policy programs. That these factors made an important difference in determining the shape of neoliberalism is utterly compelling; that they did so at the exclusion of broader societal factors, as Prasad wants to argue, is less so. Among the “society-centered” explanations that Prasad examines—and rejects—are the roles of globalization, business power, and national culture in shaping neoliberalism. But the most important

“society-centered” explanation—and the one that Prasad deals with in the most sustained fashion—is the rise of neoliberal ideas, particularly those associated with supply-side economics and monetarism. While Prasad’s argument merits careful consideration, the separation that Prasad imposes between ideas and institutional practices is not, I believe, well suited to an analysis of neoliberal politics.

Prasad makes three principle arguments in order to suggest that ideas were not central in determining the course of neoliberalism. She notes first that ideas associated with both supply-side economics and monetarism were not reputable or widely adhered to in the academy. Second, Prasad argues that ideas associated with both supply-side economics and monetarism were malleable, having gestated for many years before they were appropriated by neoliberal policymakers. Finally, Prasad observes that the same set of free market ideas produced quite different effects—huge deficits in one context, fiscal restraint in the other—in the United States and Britain. Thus, Prasad argues that supply-side economics and

SYMPOSIUM: *The Role of Ideas, Cont'd...*

monetarism did not cause the Reagan Revolution or Thatcherism; rather, these political movements used supply-side and monetarist rhetorics to justify and rationalize their objectives.

These are rather stringent conditions for asserting the causal efficacy of ideas in the rise of neoliberalism. Stating Prasad's propositions in the affirmative, she requires first that ideas be successful in the academy in order to be efficacious more broadly. If only it were so! In fact, academic ideas and popular conceptions often diverge—with popular notions frequently assuming greater weight in shaping social action. Prasad takes the fact that monetarist theory was never practiced in “pure” form in the British context as indicating that monetarist ideas were not central in shaping neoliberal economic policies. But the fact that academic ideas rarely enter policy streams in pure form, but are transformed as they encounter competing sets of ideas, is not an argument that ideas are inefficacious. There is no requirement that ideas be self-contained, coherent, or even “true” in order for them to have effects that we can trace and analyze (see for example Somers and Block 2005).

Prasad's second proposition suggests that in order for an idea to be considered efficacious the direction of influence should be from idea to political actor, not from political actor to idea. In this regard, Prasad notes that politicians opportunistically appropriated ideas associated with supply-side economics and monetarism for their own purposes. As such, Prasad argues that ideas should not be regarded as causal, since they were themselves the object of manipulation. Here Prasad seems to be on solid ground: standard social science methodology asserts that statements of causation require establishing the directionality of influence. But lurking underneath this familiar formulation is an overly restrictive notion of the way in which ideas shape social outcomes. The fact that politicians deploy ideas strategically for electoral advantage does not mean that those ideas exert no constraints on policymakers. In short, ideas and political actors may influence each other in both directions.

This leads to Prasad's third and final point: in order to be considered efficacious, ideas should exert their influence in a uniform manner across different cases. This position is implicit when Prasad argues against the role of ideas by noting the fact that the

same set of free market ideas produced different outcomes in the United States and Britain. But this conclusion does not follow from the points I have just made. That is, if ideas do not exist in “pure” form but are transformed by their encounter with other ideas as well as with political realities on the ground, then we should not expect the “same” set of ideas to have equivalent effects independent of context. Rather, these effects will vary with distinct institutional conditions in historically contingent ways. This of course does not imply that ideas are unimportant in shaping outcomes, merely that their influence should not be understood as deterministic.

To summarize, Prasad's argument against explanations that place emphasis on the role of ideas in shaping neoliberal policies implies too sharp a separation of ideas from institutional practices. A similar observation might also apply to the broader separation between state and society that anchors Prasad's argument. There is an irony in the rigid distinction between state and society posed by Prasad's analysis: neoliberalism as a political logic is itself organized around this very distinction. As Mitchell (1999, p. 83) has argued, maintaining the boundary between state and society (as is also true of the analogous distinction between state and economy) is a critical source of power in contemporary capitalism. In this sense, one might argue that attempts to penetrate neoliberal politics should account for, rather than merely assume, the binary form in which state and society appear. While this is not a task that Prasad herself takes up, her careful analysis of neoliberalism provides many insights that could be useful for such an endeavor. Put simply, this important book is the most comprehensive account of neoliberal economic policies available in the current literature. As provocative in its arguments as it is systematic in its use of data, Monica Prasad's *The Politics of Free Markets* has much to offer students of neoliberalism.

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SYMPOSIUM: The Unexpected Ideas of Economists

Johanna Bockman

I found Prasad's book very refreshing. Prasad argues that national political structures and the policy process shape the realization of neoliberal policies. She examines three kinds of neoliberal policies – tax cuts, welfare spending, and industrial policy – that were implemented in the 1980s in Britain, France, West Germany, and the United States. By examining these three kinds of concrete policies, she dispels stereotypical images of countries, such as the image of the United States as neoliberal and pro-business and France as collectivist and egalitarian. Instead, Prasad finds that the United States has had progressive taxes, anti-business regulations, and targeted welfare policies, while France has had regressive taxes, pro-business regulations, and universal welfare policies. The United States and Britain implemented stronger neoliberal policies than France and Germany because of adversarial politics and policies. In the United States and Britain, the progressive taxes, anti-business regulations, and targeted welfare policies created a division between the beneficiaries and the victims of these policies, which encouraged political entrepreneurs to exploit the discontent of the growing numbers of upwardly mobile victims of these policies. In France and Germany, their less adversarial policies led to less adversarial politics and weaker neoliberal policies. Prasad's revelations about political structure and policy process make an important contribution to the history of neoliberalism.

While she argues against a variety of "society-centered" explanations for neoliberalism, such as national cultures, globalization, and business group pressures, Prasad's main opponent is the idea-centered explanation. According to Prasad, ideas are not the cause of neoliberalism because they are self-contradictory, malleable, and develop over time through the policy process. I find her approach an important corrective to scholars who accept the victorious declarations of the Right as reality. In her analysis, the ideas of the Right were not fully developed before Thatcher and Reagan, but rather developed through the policy process. Despite her rejection of idea-centered explanations, her approach in fact complements these explanations. Ideas (such as tax cuts), experts (including economists), and institutions (such as the Mont Pelerin Society or universities) where experts develop ideas are important

to Prasad's argument, though the analysis would have benefited from a systematic analysis of these ideas.

Prasad finds that academic economists did not support neoliberal policies. My research agrees with this view. I study economists in Hungary, Italy, and the United States. Why do I study economists? I study them because in the post-1945 period economics became the predominant training for government officials worldwide. I also study economists and their professions because many studies of neoliberalism assume that all economists particularly those in the United States shared neoliberal free market views of the Reaganite sort and this in fact is not true. Most economists in the Cold War period were not committed to being ideologues for the Right, but rather supported a range of political views and actually did academic research following a range of paradigms and traditions. The economists I study at top American universities knew their Marxism, Maoism, and market socialism. The view of the economics profession as unified in its support of neoliberalism is the result of factions within the profession making this argument to constituents outside the profession.

I study economists not only to reveal the weaknesses of simplistic arguments about American cultural imperialism, but also to show the importance of economists' research to policy. Prasad shows how politicians watched neoliberal experiments in other countries. They watched these experiments not only for strategic reasons, but also because these experiments created new knowledge about capitalism (Bockman and Eyal 2002). Evolving capitalism required new knowledge about capitalism: What were the essential elements of capitalism? What institutions do markets require? What does post-Fordist capitalism require? Knowledge about capitalism did not already exist, but has always developed over time, as Prasad suggests in her four case studies. The market experiments in Eastern Europe were particularly important because knowledge about Western capitalism and the Other – Soviet socialism – developed within a liminal space between and within these two categories. Even Milton Friedman (1998) spent time in Yugoslavia because he was interested in worker self-management as an alternative to state initiatives (pp. 423-424). Economists' knowledge about capitalism and socialism provided important information for politicians and their

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think tanks for exploitation and for understanding.

Prasad fascinatingly reveals wide ranging public discontent with the status quo. She interprets this discontent within a policy spectrum that moves from state intervention to free markets, and from left to right. I would suggest that the artificial Cold War dichotomies of right and left should also be dispelled. Each side in the Cold War worked hard to maintain the distinctions between each side, and after 1989 neoliberal politicians rewrote history stating that Western capitalism and Soviet communism were always polar opposites and Western capitalism was the victor. However, scholars have recently shown that the United States and the Soviet Union had many similarities, sharing similar “dreamworlds” arising from the Enlightenment ideals of progress and control (Buck-Morss 2002), a common fascination with planning (Engermann 2004), state support of big science, and the cooperation of science and the military (Bernstein 2001; Gerovitch 2002). I study politicians and intellectuals in Western Europe, who sought to create a space outside the Cold War dichotomies between state and market, Soviet communism and American capitalism. Italian anti-Communist socialists criticized the Soviet Union, state intervention in the market and in society more generally, and identified strongly with dissidents in Eastern Europe. The West European left had the interest, skills (language skills and experience working in the East Bloc), and ability to travel to the East Bloc that would allow them to work with East European dissidents. We can see the anti-state left inspired by Gramsci’s call for a self-governing society, the long-term interest in liberal socialism in Italy, and anarchist traditions. I would thus add to Prasad’s analysis that France and West Germany did not have the strong neoliberal policies of the US and the UK also because they had left-wing political alternatives to the state-

interventionist left and thus the left and the right could both shape the anti-state discontent of the public. While shaped through the policy process and political structure in all these countries, the neoliberal narrative was controlled by the Right at least in the United States, which could use it to convince the disgruntled public that only the Right could realize a true alternative.

Prasad is quite right to privilege the role of politics. Right-wing politicians interpreted the “evidence” and the anti-state public discontent from Eastern Europe, Western Europe, Latin America, and other countries as support for Thatcherist and other programs that erased their left-wing origins. The ideas, their promoters, and idea-generating institutions also remain important to the shape that neoliberalism took. I would argue that neoliberalism latently retains its left-wing origins – the hope for the end to oppressive states, the realization of a radical left liberated from the Soviet Union, and the empowerment of civil societies or more radical associations of individuals –, as well as the hopes of more conservative forces around the world. The heterogeneous origins of neoliberalism likely intensify the difficulties we find in defining neoliberalism.

Prasad makes an important contribution to the history of neoliberalism. Her broad-ranging knowledge of each of her cases makes the case for the centrality of the domestic political structure and policy process to the history of neoliberalism. Her work also powerfully complements idea-centered analyses. We are very lucky to have Prasad’s work, which offers and will inspire new ways of understanding the origins and the future of neoliberalism

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SYMPOSIUM: Response to Responses

Monica Prasad

It is a great pleasure to have three scholars as insightful as Isaac Martin, Greta Krippner, and Johanna Bockman give such a careful reading to my book.

Perhaps authors are always surprised by how their work is received; I was very surprised that these readers thought my “main opponent is the idea-

centered explanation.” My intention in the book was to give equal time to four alternative explanations of neoliberalism that one finds in the literature: the other three are that neoliberalism was the result of the power of business groups; that neoliberalism occurred in the U.S. and the Britain, but not in France and Germany, because these countries represent different “varieties of capitalism”; and that neoliberalism happened

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where a country's national culture happened to be favorable to it. I also discuss globalization briefly. I take pains to make this point because there may be readers wondering how a book on neoliberalism could ignore the recent discussion of the distinct varieties of capitalism (treated at length in chapter 3) or national culture (treated in chapter 4) or globalization (treated in the introduction). (1)

But, since it's ideas that have struck this particular group of readers, let's talk about ideas.

The specific claim I am examining is that the causal force behind the rise of neoliberalism was the rise of a set of ideas about how the market works. This is what I understand as "the ideas explanation" of neoliberalism. So I first try to show that the ascendancy of these ideas was in fact limited, and was confined to the fringes of academia. I look at economists in particular because there is a folk theory of neoliberalism that sees it as emanating from economists. Indeed, a cottage industry is developing in sociology on the study of economists, on the assumption that economists have considerable influence on policy and the social world. I wish I could be as confident as Krippner is that the claim these scholars are making—that we need to take economists' influence seriously—is obviously false. To me it seems necessary to assess this claim empirically, and therefore it seems necessary to point out that, at least in this important case, economists were the puppets, and it was politicians pulling the strings.

But perhaps what really matters is the ascendancy of the ideas among the public; this would modify what I above called "the ideas explanation" to this claim: "the causal force behind the rise of neoliberalism was the rise of a set of ideas about how the market works in the public at large." We have recently seen the resurgence of the argument that neoliberalism occurred because the public at large became convinced of neoliberal ideas (Somers and Block, 2005). While there was indeed a shift in public opinion in favor of neoliberalism in some domains in the US and UK in the 1970s, we cannot conclude that this was the result of the power of neoliberal ideas: those ideas were also present in France and West Germany, as I show in the book, but they did not have the same power over the public. This means that the presence of the ideas alone does not dictate their success, as conceptualizations of the "ideas" explanation which focus on the internal power of ideas suggest. (2) The question is why

public opinion was more receptive to neoliberalism in some places than others, and that requires us to look at the broader context within which the attempts were made. The shift in public opinion, in other words, is the result of whatever it is that is actually causing neoliberalism.

But even if the ideas were not the original causal push, maybe the ideas were still necessary, in that if the ideas had not been available when the politicians sought them out, neoliberalism would have failed for lack of ideological cover. The claim is now that "the causal force behind the rise of neoliberalism was the existence [not necessarily the rise] of a set of ideas about how the market works." If this is the case, then when the ideas don't exist, neoliberal policies should not be possible.

To assess this, consider what happened in Britain in the case of privatization: in that case, the ideas didn't exist. There was no economic justification for the privatization of the large public utilities—indeed, there was justification for retaining them in the state sector—and what happened is that the politicians invented the ideas they needed. When the politicians saw how popular privatization seemed to be, they developed a rationale for it. Ideas don't really seem to be necessary either; if they don't exist, they get invented.

But perhaps an idea, even if it is not the source of the original push for the policy, and even if it is not necessary, can be constraining. Perhaps it can develop a *sui generis* existence and exert a causal influence that way. That is, "the causal force behind the persistence [not necessarily the rise] of neoliberalism was the existence of a set of ideas about how the market works." In the British case it is clear that the idea of monetarism did not exercise such a constraint. The Treasury, which had always privileged the idea of balanced budgets, continued to privilege the idea of balanced budgets, by simply redefining monetarism to mean balanced budgets—over the strong opposition of Milton Friedman and the original developers of the idea. Balanced budgets are what had been done in the absence of the idea, and balanced budgets are what were done in the presence of the idea. The idea did not do anything to constrain the policy.

Britain is only one case, of course; but it is the one where the "ideas" explanation of neoliberalism has been most commonly given, which is why I focus on

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it when considering this issue in the book.

I said I understood the “ideas” explanation as the specific claim that the causal force behind the rise of neoliberalism was the rise of a set of ideas about how the market works. Krippner and Bockman both give examples of other ways in which ideas might matter. I find this interesting, but I must admit that at this point, I begin to wonder if we are really talking about ideas or causation any more. For example, it seems to me that showing that economists participate in international networks, as Bockman does, is not the same thing as showing that economists’ ideas influence policymakers. Krippner thinks my conditions for showing causation are too strict; but the conditions are implicit in the claim. If we want to claim that “ideas...have effects that we can trace and analyze,” as Krippner does above, then that what’s we need to show: that ideas have effects that we can trace and analyze. That is, we need to show that an analytically identifiable thing called an idea can have a causal effect that we are able to identify on some other thing. (Nothing in this conceptualization excludes probabilistic effects or two-directional effects.) But if on the other hand we want to say that ideas cannot be separated from institutional practice, it is logically incoherent to then claim that ideas can have a causal effect separately from institutional practice. That neoliberal ideas in particular—ideas about how the market works—caused neoliberal policies is a very strong and quite precise claim; it should either receive a precise defense, or it should be abandoned.

If I understand it correctly, Krippner’s injunction to “account for ... the binary form in which state and society appear” means that the state is really controlled by (or is) a set of interests who manage to make it seem that the state is something separate from these interests. I address arguments about the behind-the-scenes power of capital in chapter 1. Krippner’s phrase may also mean that practices that no one really controls determine or influence policy. This does not seem to be in contradiction to my arguments, or in contradiction to a historical institutionalist perspective; for example, differences in the micro-practices of tax collection—one of the techniques by which the state is instantiated in everyday life as something separate from society—are important elements in my causal story. The history of these practices is fascinating, but it is a separate question from the question of their

consequences on policy. (Moreover, this history does not go in the direction Krippner expects: one of the people most responsible for introducing income tax withholding in the U.S.—a technique that makes the state’s revenue demands less visible, and therefore blurs the line between state and society that Krippner thinks neoliberals try to sharpen—was Milton Friedman).

Martin’s criticisms, briefly: I am looking at policy change, which is why the absence of large tax cuts in France during the period I investigate puts it in the low neoliberalism camp (this is, indeed, precisely the point: there was little neoliberalism in France in this period because France was already surprisingly neoliberal); I consider ERTA to be neoliberal because it was and still is the largest tax cut in American history, thus “favoring capital accumulation over income redistribution” (the TRA was intended to be revenue neutral, neither raising overall taxes nor lowering them (3); and opposition to progressive taxation waned in the 1980s because of ERTA, while the TRA arose partly in response to ERTA). Martin’s note that sales taxes in fact lead to greater protest is fascinating, but puzzling: the most sustained work on tax backlash to date (Harold Wilensky’s) finds the exact opposite, and politicians in these countries clearly believed that sales taxes were less likely to generate protest. Martin’s own continuing work on this will shed light on the issue soon. Martin rightly asks for a better a priori definition of adversarialism, so let me give one: adversarial policies are policies that create conflict between business and state, or that aim to redistribute from rich to poor.

Finally, Bockman wonders if the absence of an anti-state left in the US and UK helped to shape the outcome; chapter 1 of the book presents the case for the emergence of an anti-state left in the U.S. in the 1960s, but it is true that this faction may have been weaker in the US and the UK than in France and Germany. This does not seem in contradiction to my arguments: while my argument is that the left in France and Germany was not able to use the state against capital, it is possible that the left in these two countries was also less willing to do so.

The biggest surprise for me in the responses was that they passed over largely in silence what I thought would be the most controversial claim in the book, certainly the claim that was the hardest-won insight for me: that the US and UK have had policies that are

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more punitive to business and the wealthy than France and Germany. So while I look forward to the work that others will do on how ideas might matter, my hunch is that the greatest payoff in the study of neoliberalism will come not from questions of this sort, but from broader historical engagement with the curious fact that instead of loving the market, Americans and Britons have in fact seemed to be the ones who hate it the most, in the sense of passing punitive and adversarial policies on business and the wealthy. I hope that the book convinces readers that we ought to be paying attention to this phenomenon, and I hope also that in the next generation of scholars will be a few who are able to do a better job of clarifying it than I was able to do.

Notes

1) I would like to take this opportunity to apologize for a mistake in the introduction: in Figure 9B, the value of “0” for France 1968 reflects missing data, not absence of strikes. This was not explicitly labeled in the book, resulting in a misleading graph: in fact, of course, France experienced the largest wave of strikes in

its history in 1968, which did not make it into the official statistics precisely because of its size and political sensitivity. I am very sorry for this error.

2) See e.g. Somers and Block, 2005. Somers and Block do identify two structural conditions as necessary to the success of the idea: crisis in the existing regime, and a battle of ideas. Both seem to have been present in France and Germany at this time. (They mention a third structural condition—“the once ‘extremist’ market fundamentalist competitor has to gain a new mainstream legitimacy by establishing itself as the only possible solution to the now teetering old regime” (266)— but this seems in fact to be the dependent variable.)

3) Whether efficiency rather than capital accumulation should be what defines neoliberalism is an important question, but not one for which there is space here; briefly, I prefer the latter as it seems to capture our sense of neoliberalism being not only about efficient markets, but the withdrawal of the state in general, even if that withdrawal is uneven (and therefore market-distorting). Also see ch.4 for a discussion of the distinction between neoliberal policies that favors capitalists and neoliberal policies that punish them.

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