Symposium on

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

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

Also in this issue: Arthur Stinchcombe, William Sewell, Jr., and Charles Tilly review each others’ recent books; and Amy Kate Bailey, Rebecca Emigh, and Richard Lachmann reflect on why and how they entered Comparative Historical Sociology.
India and the Myth of the Anti-Developmental State

Jeffery Paige
University of Michigan

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

First, the book is based on extensive research on archival materials including both state documents and personal papers for the Indian post-colonial period. These materials have never before been exploited by either sociologists or historians. These primary sources are not simply deployed in Korea. The Korean case is based on secondary materials but is considerably more than simply a shadow comparison. The review of the secondary literature appears exhaustive and Chibber even develops a novel explanation of the success of business-state relations in Korea—self-interest. While most accounts emphasize the relative autonomy of the Korean state and its power to discipline a weak capitalist class, Chibber argues that it was the turn to export-led industrialization aided by Japan’s transfer of its light manufacturing to Korea that made cooperation with the Korean state both necessary and profitable for Korean capitalists. This comparison adds to our understanding of the Indian case because the absence of a turn to export-led industrialization (and the absence of Japan) further limited prospects for a successful developmental state there.

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

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

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

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

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

Still for a book explicitly concerned with the class base of the developmental state one class receives relatively little attention especially in regard to its immense numerical size—the peasantry. Diane Davis in her recent Discipline and Development (2004) argues that the peasantry or, more accurately, the small farmer class forms the social base of the developmental state in Korea and elsewhere. The influence of the business elite in the Congress party is not only a result of the successful demobilization of the working class but also the relative absence of independent political mobilization on the part of the Indian peasantry. Although Chibber notes that Gandhi succeeded in incorporating the peasantry into the Congress while advocating conservative positions in regard to wealth and property, the absence of the peasantry from the Indian development political equation is striking. In Japan and Taiwan as well as Korea land reform created a small farmer class that became reliable supporters of developmentally oriented political elites.

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

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

Finally, prospects for the social democratic developmental state may be limited by precisely those structural changes in the global economy that neo-liberal ideology did so much to promote. The current unprecedented rise of left parties and social movements throughout Latin America may provide a potential test of the prospects and limits of the social democratic or any kind of developmental state. Although the rise of the Latin American left is very much a project in process, the preliminary results are not encouraging. Socialist and populist parties such as the Worker’s Party in Brazil, the Uruguayan Broad Front, Néstor Kirchner’s Peronists in Argentina and the much tamed social-
ist party in Chile have promoted neo-liberal policies despite surprisingly radical antecedents and, sometimes, radical rhetoric. The developmental results of the much more radical, self-described revolutionary movements in Bolivia, Venezuela or post-Fidel Cuba remain to be seen. But so far Fernando Henrique Cardoso’s famous observation that “within neo-liberalism there is no alternative, and outside neo-liberalism there is no salvation” has yet to be disproved.

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

The Lessons of Failure

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

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

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

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

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

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

This piece of the argument exemplifies Chibber’s theoretical commitment to “unlock the black box” of group preferences and their consequences within policy processes (p. x). If South Korean capitalists, operating in an unusual geographical and historical conjuncture, developed rational preferences for state-led economic development, Indian firms operating in a different context had good reasons both to pay some lip service to economic planning and then to resist the implementation of a central planning agency with the capacity to discipline economic behavior. Faced with a wave of popular protest and labor mobilization during the struggles for independence, business had good reason to expect that the leadership of the Indian National Congress would support some model of state economic planning and, therefore, a number of business leaders sought to control what that would involve. Once again geography and history are important to the explanation – with a large domestic market and the withdrawal of British firms at the time of Independence (a decision that receives little in the way of explanation), both business and political leadership embraced import substitution as a developmental model rather than an export-led strategy. Consequently, Indian business resisted the “installation” of a centralized state capacity for economic planning and, once a relatively weak and poorly coordinated “planning” apparatus was established, their rational economic preferences led firms to adopt strategies that undermined the overall performance of the Indian economy. The shortcomings of this first-generation of poorly implemented state economic planning then created the condition for the failure of reform even in the face of weak economic performance. And, when pressure for reform finally did break through in the 1980s, it took the form of deregulation rather than the creation of the theoretically exemplary state-led development strategy credited with the economic successes of East Asia.

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

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

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

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

Unlocking the Shackles of the State versus Market Dichotomy

Leo Panitch
York University

The importance of Vivek Chibber’s Locked in Place lies especially in the enormous contribution it makes to overcoming the false state versus markets dichotomy that has plagued political economy during the neoliberal era. Writing from the perspective that states are indeed constitutive of (varieties of) capitalism but that state actions are always determined by their relation to the balance of class forces in any given society, Chibber has given us the definitive critique of the institutionalists’ state autonomy approach to explaining economic development (or the lack of it) in the South in recent decades. And he has done this in a way that brings that critique into a dialogue with the state debate concerning the advanced capitalist countries. In this respect, Locked in Place does more than any other book to reverse the unfortunate direction taken by political sociology away from class analysis after the advances made in the 1970s by the neo-marxist work on the capitalist
state. Insofar as those who led this shift in direction were in good part motivated by proving that the East Asian NICs and European social democracies were exemplary alternatives to Anglo-American neoliberalism, the strategic as well as the social scientific implications of Chibber’s accomplishment is considerable indeed.

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

Is this then a book that, in its search for explaining the historical roots of the differences between the South Korean and Indian varieties of capitalism, continues to reflect some of the limitations of what Martin Shaw has appropriately called the ‘social science as stamp collecting’ comparative method, one which compares two nation states while paying insufficient attention to the overarching imperial carapace in which they are both imbricated? Such a criticism would be unfair. For instance, in addressing the central question the book poses—that is, why Korean capital was receptive to disciplinary planning by the state whereas Indian capital was not—Chibber’s explanation points to the important role of Japanese capital in penetrating the Korean economy and the effect of this in making Korean firms recognize that they were dependent on state planners to make their turn to production for export possible and effective. This is a very persuasive argument, especially when combined with his awareness of the US role in the 1950s in promoting Japan’s own export-led industrialization, and its extension to South Korea (indeed it is instructive to read Chibber’s book in conjunction with the chapter on Korea in Chalmers Johnson’s *Blowback* (2000)).

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This is an argument that is broadened and further enriched by the emphasis Chibber gives to showing how US foreign aid policy became by the 1960s increasingly oriented to promoting the restructuring of economies in the South so they would be less reliant on US government loans and more receptive to foreign direct investment and more export-oriented. That said, the role of the US imperium in relation to the South Korean state and capitalist class remains insufficiently analyzed. This especially so in relation to the emergence of the Park dictatorship under which Korean planning was inaugurated; in relation to the ability of the state to repress labor and other democratic forces for so long; and, not least important, in relation to the confidence of South Korean capitalists that state developmental planning would not entail a fundamental challenge to private property.
Chhibber’s argument is utterly compelling as he turns, in the main part of the book, to showing that, if one thinks it is state capacities that determine developmental possibilities, the post-independence Indian state initially evinced far more potential for this than the South Korean state. And he certainly demonstrates very well the opposition of the Indian bourgeoisie to the state playing any kind of disciplinary developmental role in terms of holding private firms accountable for the public funds doled out to them in line with the requirements of the economic plan. That said, it is not entirely clear whether Chhibber is arguing that the Indian state really tried and failed to install a developmental planning process, or whether it never seriously even tried to do so. Was it capitalist pressures that really determined that effective state planning was killed in its infancy? Or was it the Congress leadership itself that aborted such planning not long after it was conceived? Chhibber vacillates between these positions. On the one hand, he suggests (p. 126) that while ‘much of the Congress leadership visualized state-led development’ of the disciplinary type, it was the fact that India’s capitalists wanted ‘nothing at all’ to do with this that determined the outcome. And yet he also suggests (p. 125) that the conservative, older generation dominant within Congress’s “High Command” itself exercised “the most influence” in terms of the opposition to such disciplinary planning. My reading of the evidence Chhibber presents supports the latter interpretation, at least in the sense of the pragmatic anticipatory opposition to developing effective planning capacity on the part of Indian political leaders and senior bureaucrats in light of their ‘reading’ of the resistance that would arise from both Indian and imperial capitalist forces.

Chhibber rightly puts a lot of emphasis on how this accommodation to capital entailed a split between government and party, between the “High Command” and the activists. And, in an especially important chapter, he demonstrates how the demobilization of the labor movement after independence weakened the left inside the party. But more systematic attention might also have been paid to how imperial relationships steered the Congress leadership in their opposition to effective developmental planning. This is richly suggested, for instance, by “the barrage of letters” that Chhibber uncovered written in 1949 from London by the powerful Indian industrialist G. D. Birla to Deputy Prime Minister V. B. Patel in Delhi “reporting on the worries that British and American capital evinced about the ‘investment climate’ in India” (p. 244). But it would be have been very interesting as well to explore this in relation to what John Saville has termed “the mind of the Foreign Office” during the onset of the Cold War, and also in relation to the 1945 Labour Government’s own abandonment of effective planning in the immediate post-war years. In terms of the American imperial relationship, Chhibber himself provides good evidence (often in his footnotes which are worth the price of admission themselves) of how astute American officials in Delhi could be in terms of recognizing the grounds for the split between the government and the party on economic policy (pp. 286-7), but more might have been deduced from this about what this suggested about US imperial state capacities.

Both imperial and domestic capitalist opposition to effective capitalist planning in India in the early post-war decades suggests there is something fundamentally wrong with the conventional division of the era since World War Two into a period of national state autonomy and interventionist reform followed by a sharply contrasting period of the loss of state autonomy amidst globalization and neoliberal reform. Indeed, there is a strong case to be made that the seeds of neoliberalism were planted in the early post-war decades. Chhibber’s evidence can be read as showing that state policies in these decades were the incubators of the enhanced power of private capital. Just how much the fight against disciplinary planning in India set the stage for the greater reliance on market forces that has been the hallmark of neoliberalism is well-revealed in Chhibber’s quotation of Congress’s Deputy Chairman, D.R. Gadgil telling a U.S. embassy official in 1967 that he was ‘in favor of greater liberalization because successful state intervention “required much more administrative effort and sophistication than were available... Detailed planning of the production effort and investment can benefit the whole economy appropriately only if accompanied by meticulous price and distribution control. If, because of a variety of circumstances, such a regulatory regime cannot be operated, must not larger reliance be placed on market forces and competitiveness?” (p. 215).
What could have been done differently? And what might now be done? In this respect, Chibber is refreshingly open in affirming that his contribution is strategic as well as social scientific. Building on his demonstration of the significance of the exclusion of trade union representation from Indian post-war planning, and indeed Congress’s deliberate demobilization of the labor movement, Chibber makes a case at the end of the book for mobilizing anew and enhancing the power of labor so its representatives might be able to exert greater influence within a genuine social democratic cross-class economic planning coalition. But this is quite misleading. What was followed in India in the post-war era epitomized social democratic politics in terms of the split between government and party, the demobilization of labor and the abandonment of effective planning. In some cases, this came about relatively quickly in the late 1940s (e.g. in the UK as well as India); in others it took rather longer (e.g. in Sweden or Austria). But in all cases capitalist planning of the social democratic corporatist type ran up against the impossibility of reconciling effective planning with capital’s assertion of its right to privately determine what is invested and where, and what is produced and where. The ultimate demonstration of this was the sorry fate of the Swedish wage-earner’s fund proposals as part of the labor movement’s unsuccessful attempt to keep corporatist planning going there by the mid-1970s.

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

Response to Clemens, Paige, and Panitch

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

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

By the time the dissertation morphed into a book a decade later, the concerns animating the welfare state debates had already receded, and even seemed a distant memory. Elisabeth Clemens is therefore entirely right in noting that the book seems to have come out of an alternate universe — not only because of the “cultural turn,” to which Clemens points; but also because of the general fading of class analysis as a major force in American sociology. It is therefore something of a sur-
prise – and may all surprises be so pleasant! – that
the book has been received warmly by the discri-
pline.

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

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

Bringing capitalists into the picture as a central
actor went against the scholarship of the 1980’s
and 1990’s which, in the study of developmental-
ism, had become increasingly state-centered. In
much of the literature, it was simply assumed –
this needs to be stressed, for it was rarely demon-
strated – that capitalists in the developing world
at mid-century were simply too small to have mat-
tered as a political force. I try to argue that while
it is certainly possible that capitalists in particular
historical settings can be too small and too de-
pendent on the state to be a formidable political
force, this was so neither in India, nor Korea –
and by extension, it may not have been so in other
countries at comparable levels of industrial de-
velopment. The book therefore not only seeks to
propose a class analysis of state-building in these
two countries, but also its relevance to other cases
in the South.

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

For the most part, the concerns that my interloc-
tors raise are not about the role of domestic busi-
ness per se, but about the relevance of other poten-
tial actors and forces. Jeffery Paige wonders if the
South Korean state’s base in the middle peasantry
might have contributed to its political stability and
as well to the legitimacy of the whole planning
enterprise. Here he draws upon Diane Davis’s
excellent new book on the political bases of de-
velopmental states. Leo Panitch, while quite happy with my book on its own terms, laments that it gives insufficient attention to the role of imperial influences. He is careful to note, correctly, that the book does point to the influence of core countries on state-building; his concern, if I understand him correctly, is that the analysis is not systematic enough. Imperialism comes in through the back door, as it were, instead of being theorized as a core element in the process itself.

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

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

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

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

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

4 Of course, if the issue is the state’s leverage against landed classes, this might be a different matter.
admirable; and further, that they laid the groundwork for the recent successes in industrial growth. By no means should the book be taken as part of the chorus – so loud in Indian academic circles these days – that the four decades of planning were a gigantic mistake.

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

Elisabeth Clemens raises some very far-reaching points with regard to the possibilities that might have been opened up had I paid greater attention to the role of culture. There would be no way to address them adequately in the short space provided here. So let me offer some thoughts to her specific points. I argue that capitalist responses to state-building were generated by underlying structural – in particular, economic – conditions. Indian industrialists fought a developmental state because the adoption of import substitution made it rational for them to do so; Korean capitalists accepted such a state because the adoption of an export-led development model generated incentives for them to do so. Clemens offers that I might have adopted a different attitude to the relation between structures and capitalist strategies; instead of assuming a straightforward causal chain leading from structures to action, I might have allowed for great variation in capitalist responses – a variation resulting from cultural factors that might have filtered the very perception of the structures by key actors. Hence, in different cultural settings, the same structures might have generated varying responses.

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

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

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

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

References


