The developmental state is dead: long live the developmental state!

Stephan Haggard

Chalmers Johnson’s (1982) *MITI and the Japanese Miracle* rests on a big empirical puzzle – Japan’s extraordinarily rapid growth – and two core claims that have driven the surprisingly resilient research program on the developmental state ever since.¹ The first is that Japan’s high postwar growth could be traced to industrial policies that differed from both the “plan ideological” systems of state socialism and the “regulatory state” of Anglo-Saxon capitalism. This branch of the research program attracted by far the most attention because it directly challenged liberal orthodoxy in the economics profession and development policy community. Led by outsiders to that community – Johnson, Alice Amsden (1989), Robert Wade (Wade 1990, 2004; White and Wade 1984), and Ha-Joon Chang (1994) – this line of thinking was subsequently brought into the economic mainstream by economists such as Dani Rodrik (1995) and Joseph Stiglitz (2001), who reiterated the microeconomic logic of state intervention.

The second strand of the developmental state approach probed the political foundations of rapid growth. Industrial policy in the developing world was ubiquitous, but not ubiquitously successful. What accounted for successful industrial policies and the institutions capable of conducting them in the first place? The developmental state literature is typically identified with an institutionalist approach to politics, focusing on the autonomy or insulation of the government from rent-seeking private interests, delegation to lead agencies, and coherent bureaucracies. But Johnson was acutely aware of the centrality of business-government relations to the Japanese model, and subsequent contributions by Peter Evans (1989, 1995) refocused debate on the social foundations of rapid growth.

This second face of the developmental state research agenda developed a particularly strong comparative-historical component. The literature gradually moved beyond Japan and the paradigmatic Northeast Asian cases of Korea and Taiwan to the rapidly growing countries of Southeast Asia and to comparator cases that were distinctly “nondevelopmental.” This comparative-historical research agenda sought to identify the historical sources of development and underdevelopment through close consideration of a small number of cases.

What was theoretically and methodologically distinctive about the developmental state literature? The first point to note is that this literature took an adversarial stance toward orthodox models over an empirical puzzle of pressing practical import: why the East Asian countries had grown so fast. Johnson situated his plan rational model between state socialist and Anglo-Saxon economic models. But the exercise was not simply typological. Johnson, Wade, and Amsden believed that orthodox interpretations of East Asia’s rapid growth were deeply flawed, descriptively, theoretically, and methodologically. Neoclassical models were rooted in the claim that rapid growth was caused by the adoption of market-conforming policies, most particularly with respect to the external sector, thus the moniker “export-led growth.” But close empirical analyses of cases — what we might now call process tracing or causal process observation — suggested that these policies were not adopted to the extent believed, were combined with other interventions that did not fit prevailing theoretical models, and rested on other causal factors such as institutions and underlying social coalitions. In retrospect, the developmental state literature resembles the Varieties of Capitalism approach to comparative-historical research, in which the identification of types provided the basis for a critique of more generalized models.

The second feature of this work was deep engagement with particular cases and a configurational approach to causal explanation. In her well-known manifesto, Barbara Geddes (2003) challenges one claim of the developmental state literature — that the weakness of labor facilitated export-led growth strategies — by showing that the more general relationship between labor weakness and economic growth does not hold. According to Geddes, the developmental state literature was guilty of selection bias: drawing faulty inferences from cases selected on the dependent variable. And, of course, the small-N comparative-historical work considering whole countries always faced the classic degrees-of-freedom problem, with potentially significant variables far outnumbering the cases under consideration.

But scholars working on the developmental state had relatively modest presumptions about the ability to generate lawlike statements. Johnson (1987)
cautiously extended his work on Japan to Taiwan and Korea because he believed the Japanese model diffused organically within the region (see also Cumings 1984). Although he offers a summary statement of the Japanese model in the conclusion to MITI (315–24), he explicitly warned that, although “it may be possible for another state to adopt Japan’s priorities and its high-growth system without duplicating Japan’s history...the dangers of institutional abstraction are as great as the potential advantages.” Rather than seeking to isolate the influence of discrete causal variables, Johnson and his followers showed how features of these polities and societies combined or were configured in ways that promoted economic growth. Put differently, claims about the effectiveness of any given component of the developmental state – such as the weakness of labor – could not be isolated in the way Geddes sought to do; any given factor was conditional on other features of the model.

A final characteristic of most of the developmental state work was that it took history seriously. A strong theme in Johnson’s book is that historical analysis was required because successful strategies emerged only through a process of trial and error and learning by doing. Alice Amsden (Amsden and Chu 2003) and Dani Rodrik (2008) elevated this observation, that governments, societies, and firms need to learn in order to grow, into a virtual dictum about successful development more generally. Historical work on the origins of developmental states often found them in deeply rooted historical factors. These observations raised quite fundamental questions about sequencing (Faletti and Mahoney, Chapter 8, this volume) as well as the replicability of historical findings to other settings.

This chapter is divided into two major sections: the first on the relationship between intervention and growth; the second on the political sources of rapid economic development, including the question of origins of developmental states. In each section, I start with an outline of the theoretical rationale of the developmental state approach followed by a discussion of the evidence and methodological issues of interest to comparative-historical analysis. The conclusion takes note of some of the issues in the current revival of interest in the developmental state and the renewed focus on state capacity, in particular.

The sources of economic growth: policy

The developmental state literature posed itself as an alternative to an emerging neoclassical consensus in development economics. Prior to the appearance of Johnson’s book and continuing through the appearance of Wade’s, a
succession of studies by Anne Krueger (1978), Jagdish Bhagwati (1978), and Bela Balassa (1981), among others, had offered up a neoclassical interpretation of East Asia’s rapid growth. This work emphasized stable macroeconomic policies, market-oriented reforms, and trade and exchange rate policy, in particular. The implications of this advocacy of export-led growth were hardly limited to the academy. The neoclassical canon on export-led growth influenced the international financial institutions and provided the key empirical referent for the so-called Washington consensus.² Written a decade prior to the collapse of the Soviet Union, Johnson was responding to an earlier variant of this literature on Japan (e.g., Patrick and Rosovsky 1976).³ Johnson framed MITI and the Japanese Miracle in terms of a distinction between the plan-ideological systems of state socialism and two varieties of contemporary capitalism: market-rational and plan-rational systems. The fundamental distinction between the latter two was that market-rational economies took a regulatory approach to economic activity, whereas the plan-rational or developmental state was defined as purposive and goal-directed, seeking to achieve high growth through the allocation of resources to designated economic activities.⁴ Johnson is cautious in making causal claims with respect to industrial policy, and he notes the checkered history of state control of industry. Nonetheless, his core theoretical insight is that the process of economic development is characterized by a myriad of market failures that can be solved only by governments that are capable of coordinating private sector activity, intervening in markets, and providing selective and conditional support to firms.

The logic of industrial policy


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² When the World Bank (1993) finally did its own review of the East Asian miracle – at Japan’s urging – the report downplayed the role of industrial policy, setting off a heated debate over both the substance of the report and the process through which its conclusions were reached (Amsden 1994; Aoki, Kim, and Okuno-Fujiwara 1996; Wade 1996).

³ The Patrick and Rosovsky book was titled Asia’s New Giant; Amsden’s book on Korea, Asia’s Next Giant.

⁴ Throughout its life, the developmental state literature has been accused of a kind of tautology: that developmental states were little more than those that grew rapidly. But Johnson was rightly cautious on this point, arguing that whether such efforts were successful is not given and requires explanation.

⁵ Citations to Wade are to the second edition.
broad approach more structure by outlining the policy elements of the “governed market,” or GM, theory in more detail; in doing so, Wade also provided a more expansive definition of the developmental state. The GM approach, Wade (1990, 2004) argued, “emphasizes capital accumulation as the principal general force for growth, and interprets superior East Asian performance as the result of a level and composition of investment different from what FM [free market] ... policies would have produced, and different, too, from what the ‘interventionist’ economic policies pursued by many other LDCs would have produced” (29). But sheer accumulation was not the centerpiece of Wade’s story, which rested more fundamentally on market failures (11–14, 350–8; see also Chang 1994). In effect, the GM theory interpreted the challenge of growth to reside in coordination problems that private actors cannot resolve efficiently through private contracting and exchange.

Three kinds of coordination problems received particular attention: those that arose in moving from agriculture to industry and within industry; those associated with financial markets; and those that surrounded the transfer and adoption of technology. Wade’s dominant line of argument parallels Johnson’s and a group of Japanese analysts’ (for example, Aoki, Kim, and Okuno-Fujiwara 1996; Rodrik 1995 for a formal model) in focusing on coordination problems in the industrial sector. Efficient investment can be deterred by small market size and the absence of complementary suppliers or customers. Current market prices do not adequately convey information about future growth, and countries thus forgo investments that would lower production costs through larger plant size and learning effects; thus, Alice Amsden’s (1989) infamous dictum that the East Asian countries succeeded not by “getting prices right” but by “getting prices wrong.” If information is appropriately shared between government and the private sector and firm behavior is appropriately monitored, interventions such as subsidies, protection, and other direct supports to private business could overcome these collective action problems and thus push an economy from a bad to a good equilibrium (see Khan and Sundaram 2000 on the positive role of subsidies or “rents” in this process).

Such coordination problems often are assumed to arise primarily in the heavy and intermediate sectors such as steel and petrochemicals, but coordination problems also emerged in light industries seeking to enter world

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6 The GM theory thus anticipated an important set of arguments that emerged in the mid-1990s that capital accumulation – rather than either liberalizing reform or industrial policy – was at the core of the East Asian miracle (Krugman 1994).
markets and in more technology-intensive industries. In a widely read PhD dissertation, sociologist Thomas Gold (1981) documented the role of the government in Taiwan in coordinating complementary investments in the textile industry. Kuo (1995) and Noble (1999) offered comparative-historical analyses of Taiwan and the Philippines and Japan and Taiwan, respectively. Both used detailed industry studies to examine these coordination issues at the sectoral level.

A second theoretical rationale for intervention centered on failures in capital markets and provided one area where heterodox economists – including Nobel Laureate Joseph Stiglitz (Hellman, Murdock, and Stiglitz 1996) – entered the debate. Meredith Woo-Cumings’s (Woo 1991) book on Korea is exemplary of comparative-historical work in this vein. Woo-Cumings argued that the financial sector allowed the government to both steer investment and exercise political control over the private sector.

A third rationale for industrial policy centers on technology. Alice Amsden (1989, 1991; Amsden and Chu 2003) and Peter Evans (1995) were most closely associated with this focus, but it is visible in more heterodox economists as well (L. Kim 1997). Explicitly following Gershenkron, Amsden draws a distinction between the early developers, which grew as a result of their own innovation, and latecomers, such as Korea, that grew through technological borrowing and learning. Amsden frontally attacked the idea that latecomers can successfully borrow by focusing on comparative advantage in labor-intensive industries alone. Developing country firms lack full information on technological alternatives, function with imperfect information on the technologies they do acquire, and are subject to variable, unpredictable, and highly path-dependent learning processes. Incomplete appropriability leads to underinvestment in research and development (R&D), forgoing the many externalities that arise around R&D activities.

Method in the analysis of industrial policy: the role of history and counterfactuals

A surprisingly common research design in the literature on industrial policy is to pick a successful (or unsuccessful) industry, demonstrate that policy support existed, and conclude that the case for the significance of industrial policy is made (or rejected). Wade (2004, 29–33, 71–2, 109) recognized this problem explicitly and outlined a counterfactual strategy that other comparative-historical work explicitly or implicitly emulated. First, sector-specific policies must not only be plausibly associated with the success of the industry in question but must yield outcomes equal or superior to a more market-conforming
policy counterfactual. Moreover, Wade goes further by arguing that intervention must not have been the result of private sector demands (what Wade calls “followership”); if they were, then the investments in question might have taken place anyway. Rather, intervention must reflect “leadership” by the state that puts the industrial structure or a particular industry on a different path than it would have otherwise taken.

The alternative to this complex counterfactual analysis would appear to be straightforward: to examine industry-level data within a given country to determine whether those that received policy support surpassed those that did not on some metric, such as total factor productivity (TFP), export growth, or profitability. The World Bank (1993) *Miracle* report purports to conduct some tests along these lines, although they are hard to follow (Amsden 1994). Noland and Pack (2003) provide a skeptical overview drawing on the surprisingly small number of econometric studies in this vein.

Rodrik (2007), however, outlines clearly why these efforts do not escape the fundamental dilemmas of the counterfactual analysis undertaken by Wade:

The almost insurmountable flaw in this literature is that the key estimated coefficient [on industrial policy] . . . cannot discriminate between two radically different views of the world: (a) the government uses industrial policy for political or other inappropriate ends, and its support ends up going to losers rather than winners; (b) the government optimally targets the sectors that are the most deserving of support, and does its job as well as it possibly can in a second-best policy environment. Under (a) governments should commit to a hands-off policy. Under (b) a hands-off approach would leave the economy worse off. . . . The empirical analysis leaves us no better informed than when we started. (17–18)

These irreducible problems help explain why causal claims about industrial policy in the comparative-historical literature are necessarily posed in a more contingent and contextual way. For Johnson, industrial policy worked because of sequencing (Faletti and Mahoney, Chapter 8, this volume), identifiable path dependencies, and learning. Moreover, growth rested on political relationships and institutions that coordinated, monitored, and disciplined rent-seeking. For example, Haggard (1990) places more emphasis on the effect of neoclassical reforms than others do in the developmental state tradition. However, he argues that these reforms had effect only because prior interventionist policies – some going back to the Japanese era in Korea and Taiwan – had developed firm-level capabilities, a theme in Amsden (1989) and Chang (1994, 2002) as well. Sequencing, in short history, mattered. Moreover, these reforms and successful interventions rested on the political power of the state
and its independence from business interests. In short, politics mattered as well.

From policy to politics: institutional, coalitional, and historical foundations of the developmental state

The developmental state literature not only challenged neoclassical economic orthodoxy but also developed a line of reasoning about economic growth that ran counter to prevailing political economy models as well. The dominant political-institutional model of growth in economics and political science focused on property rights, the rule of law, and checks on state power (Barzel 1997; Haggard and Tiede 2011; Weingast 1995, 1997). The growth miracles in East Asia occurred during relatively long periods of political dominance by conservative parties or elites that might have mitigated property rights challenges emanating from the left. But the nature of these political regimes suggests the deeply anomalous quality of Asia’s growth: it is hardly a region characterized by strong checks on state power, well-developed rule of law, independent judiciaries, or codified property rights. Either informal institutions protected property rights or altogether different political mechanisms were at work (Chan 2000; Ginsburg 2000).

An integrated statement of the political model undergirding the developmental state is surprisingly hard to find. Nonetheless, the research tradition rested on two, interlinked political observations, one centered on the state, the second on the relationship between the government, the private sector, and other social forces, including labor. On the one hand, developmental states were characterized by substantial delegation to executives and in turn from executives to capable and appropriately incentivized bureaucracies. Johnson’s view of the Japanese polity, for example, rested on the controversial assertion – which generated its own revisionist backlash – that politicians reigned but that bureaucrats ultimately ruled through well-developed ministries such as MITI (the Ministry of International Trade and Industry).

On the other hand, the developmental state was politically insulated from social forces that might distract from its accumulationist and industrial policy aims, including not only the Left and working class but also the private sector.

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7 These include the Liberal Democratic Party (LDP), Kuomintang (KMT), and People’s Action Party (PAP) in Japan, Taiwan, and Singapore, respectively; Park Chung Hee, Soeharto, and Mahathir in Korea, Indonesia, and Malaysia; the British colonial administration in Hong Kong; and a surprisingly stable alliance of king, military, and bureaucracy in Thailand.
itself. In Alice Amsden’s felicitous phrase, a key feature of the developmental state was its capacity to “discipline” the private sector.

What is the underlying theory that would justify a focus on a state that is centralized, internally coherent, and politically insulated? The answers trace back to the core mechanisms that generate long-run growth. In a simple accounting sense, economic development can result from productive inputs – capital and labor – or from greater efficiency in their use. Early growth accounting suggested that virtually all of Asia’s growth could be explained by sheer accumulation (Krugman 1994). This view is still contested, but any model of East Asia’s growth must offer an account of the extraordinary level of investment during the high-growth period.

However, accumulation should not yield high growth if allocated inefficiently. Even advocates of industrial policy argued for policies that were in conformity with “dynamic comparative advantage.” The second political economy problem is thus one of reform and transition: how to move policy from an economically and politically distorted low-growth equilibrium to a high-growth path. Features of both states and social structures were implicated in achieving these two core objectives of rapid capital accumulation and relatively efficient allocation of resources.

**Political institutions, big and small: regime type and bureaucracy**

Johnson was clear that Japan’s autonomous developmental state was forged under semidemocratic and authoritarian rule, but he was ambivalent about whether authoritarian rule was a necessary condition for rapid growth. After all, Japan was at least nominally democratic in the miracle years of the early postwar period. However, as the developmental state literature migrated away from Japan to the newly industrializing countries – Korea, Taiwan, Hong Kong, and Singapore – the question was quickly joined (Cheng 1990; Haggard 1990).

Several causal arguments linked authoritarian rule to growth. Since capital accumulation demands that resources be diverted from consumption to investment, voters (or citizens, the poor, or labor, depending on the precise specification) are likely to oppose such a reallocation; as Huntington and Nelson (1976) had argued much earlier, participation generates trade-offs. The second route, and the one I took (Haggard 1990), focused on economic reform. Dictators can overcome collective action problems inside and outside the government that hinder the formulation of coherent policy, override rent-seeking and populist pressures, and thus push the economy onto a more
efficient growth path. I was particularly interested in underlining the irony that all of the governments that undertook reforms in the region – the darlings of the neoclassical approach – were authoritarian, a finding some intellectuals in the region actually embraced.

The finding of a link between authoritarianism and growth clearly didn’t generalize. In 2000, Adam Przeworski and his colleagues found that controlling for income and a number of other variables, regime type had no effect on investment, the growth rate of the capital stock, or overall income growth; this finding also held when limiting the test to a sample of developing countries. But Przeworski et al. also found that the standard deviation of growth in the sample of dictatorships was much larger than in the democracies, confirming that autocracies encompassed both high-growth miracles such as those found in East Asia and low-growth debacles. The failure to directly address the puzzle of why some authoritarian regimes grew rapidly while others crashed and burned was a drawback of the method of focusing on a high-growth region; this shortcoming subsequently motivated the cross-regional work by Evans (1995), Kohli (2004), Doner, Ritchie, and Slater (2005), and others on historical origins that I take up in the next section.

The other feature of the developmental state construct – and the one that preoccupied Johnson – was the bureaucracy. Regime type and politicians more generally mattered much less to Johnson than the core bureaucratic agencies, such as MITI, that he believed drove the growth process. Internally, such agencies were motivated by clear missions and were run on meritocratic principles with strong internal systems of both rewards (such as competitive pay and long-term career tracks) and controls (sanctions for corruption). These characteristics also served to insulate the bureaucracy from political or private sector manipulation.

In an innovative study drawing on expert evaluation, Evans and Rauch (1999) found that the “Weberianness” of the bureaucracy was associated with growth in a cross section of thirty-five developing countries. Case study work finds that economic reforms in the region were typically preceded or accompanied by major bureaucratic reorganizations that concentrated economic decision-making authority in one or several lead agencies, strengthened the role of technocrats in formulating policy, and reformed internal bureaucratic routines (Cheng, Haggard, and Kang 1998; Haggard 1990; Kim 1988). Reform of the bureaucracy did not typically reach the entire administrative apparatus. Pockets of bureaucratic efficiency coexisted with ministries that dispensed pork and political favors even in the Northeast Asian developmental states (Kang 2002a, 2002b). Such payoffs may even have been the price for wider
reforms. Nonetheless, all of the high-growth Asian countries had at least some core economic ministries and/or specialized agencies with important policy-making powers that looked relatively Weberian in form.

The relationship between politicians and bureaucrats in Japan’s economic decision making ended up being one of the most disputed features of John-
son’s book. Virtually from the moment it was published, critics pointed out the role that either politicians or private sector actors played in policy for-
mulation (Calder 1993; Krauss and Muramatsu 1984; Noble 1998; Okimoto 1989; Samuels 1987). The most blunt challenge was posed by Ramseyer and Rosenbluth (1993), who argued that Johnson’s much-vaunted bureaucrats were little more than agents of the Liberal Democratic Party (LDP).

With the benefit of hindsight, the debate seems somewhat stilted. Johnson was hardly a culturalist and sought to outline the political and administrative rationality of the plan-rational state. And it doesn’t take much work to show that asymmetric information and a host of other imperfections can upset the just-so delegation story advanced by rational choice institutionalism. However, the rational choice critique was clearly onto something important. As the developmental state model was extended to authoritarian regimes beyond Japan, it seemed particularly odd to think that bureaucrats enjoyed independence from autocratic political elites. In these cases, bureaucracies were reformed not to ensure their independence but to ensure their loyalty to authoritarian leaderships in the pursuit of some broader political project. This crucial observation led quickly back to state-society relations.

The social foundations of developmental states

From its inception, a central critique of the developmental state approach was its excessive focus on political institutions and its relative neglect of business (Chan, Clark, and Lam 1998; Doner 1991; Evans 1995; Fields 1995; Jayasuriya 2005; E. M. Kim 1997; MacIntyre 1994; Moon and Prasad 1994). Recall that the concept of the plan-rational state assumed a mixed economy and the presence of a private sector that had to be incentivized to invest. Also recall that the property rights model did not appear to comport with the East Asian experience on its face. Authoritarian regimes backed by strong bureaucracies should lack credible checks on executive discretion and therefore suffer from credibility problems in protecting property rights and eliciting investment from the private sector. How could we square a state that was strong, but not predatory, credible to the private sector, but not captured?
Alice Amsden stated the general political problem most clearly. Just as rent-seeking could distort the efficient allocation of resources in neoclassical models, so private sector capture could distort the allocation of subsidies from their developmental purposes. The solution – and for Amsden the defining political feature of the East Asian developmental states – was the ability of strong states to “discipline” business:

In slower-growing late-industrializing regions, subsidies have tended to be allocated according to the principle of giveaway, in what has amounted to a free-for-all. In East Asia, beginning with Japan, there has been a greater tendency for subsidies to be dispensed according to the principle of reciprocity, in exchange for concrete performance standards with respect to output, exports, and eventually, R&D. For example, the government in South Korea disciplined its big business groups by means of price ceilings, controls on capital flight, and incentives that made diversification into new industries contingent on performing well in old ones. (Amsden 1991, 284)

If rents are extended to help solve any of the coordination problems outlined in the previous section, they must be conditional. Governments must define the objectives of the rent in terms of some discernible market failure or externality and monitor rent recipients; clearly, bureaucratic capacity matters in this regard.

But they must also credibly commit to withdraw rents for noncompliance or nonperformance, a politically charged task that goes far beyond bureaucratic monitoring to the nature of business-government relations (Amsden 2001, 8; Aoki, Kim, and Okuno-Fujiwara 1996; World Bank 1993). How could strong governments send credible signals to private actors while simultaneously constraining their rent-seeking behavior? Grabowski (1994, 1997) and Huff, Dewit, and Oughton (2001) formally modeled relations between a developmental state and the private sector as a signaling game in which the government proves its credibility to the private sector by complementary investments that elicit private investment. The point was extended to the distribution of subsidies or rents in an underappreciated study by Khan and Sundaram (2000). Kang (2002a, 2002b) talked about a “mutual hostage” relationship in Korea that bounded both state predation and private corruption. In all of these models, industrial policy is seen not only through the lens of its economic effects but as a crucial political signal of government intent.

The idea that institutions could help solve these signaling, credible commitment and monitoring problems between the state and the private sector was at the heart of the developmental state approach. At least partly
motivated by the East Asian experience, Schleifer and Vishney (1993) provided an influential model of corruption that outlined why centralization increased efficiency; the approach was subsequently picked up by both Kang (2002a, 2002b) and MacIntyre (2003) in their “second-generation” analyses of the Korean case and Southeast Asia’s response to the debt crisis, respectively. In Schleifer and Vishney’s model, rent-seekers demand a range of complementary government-supplied goods. If the state is highly decentralized, different branches of government, ministries, or bureaus pursue their own interests, pushing the cost of government-supplied goods to a suboptimal level and introducing uncertainty over property rights in those rents. Political centralization does not eliminate corruption but bounds it by solving these collective action problems, pricing rents at an “appropriate” level, and providing security of property rights.

Somewhat similar arguments were made about the internal coherence of the bureaucracy. Meritocratic recruitment and promotion, adequate compensation, and career paths with clear rewards at the top not only were important for performing the tasks outlined in the previous sections but served to control rent-seeking as well. Evans (1995), in particular, argued that the internal coherence of the bureaucracy was the key factor separating efficient “embeddedness” from inefficient rent-seeking. The case study work on industrial policy repeatedly returns to the significance of independent bureaucratic capabilities and information as a condition for effective governance of the private sector.

Yet, this focus on the state still left open the question of how political actors interacted with the private sector and to what effect. A significant strand of research on East Asian development, including Campos and Root (1996), Root (1996), and the World Bank’s (1993) own Miracle report, developed the argument that “deliberation councils” linking business and government played an important role in resolving credibility problems associated with authoritarian rule and building trust between the public and private sectors. Evans (1995), Maxfield and Schneider (1997), Weiss (1998), and Moon and Prasad (1994) all cast the argument in more general terms, claiming that a complex of both formal and informal networks between the public and private sectors played a role in economic growth. In Evans’s (1995) felicitous phrase, the East Asian state was characterized by “embedded autonomy”: strong political and bureaucratic institutions that simultaneously maintained dense ties with the private sector but had adequate independence to control the business-state nexus. Root (1996) explains how it worked by reference to an implicit model of costly signaling:
Tying the fortunes of many groups to the continued use of the cooperative decision-making structures raises the cost of altering the system ex post. Once councils permeate an economy, a government that unilaterally imposes its will on an industry or sector will risk undermining the value of councils for other groups, thus subverting the entire system of cooperative decision-making. Government, then, is unlikely to abide only by those decisions it prefers, overturning those it opposes... By institutionalizing deliberative councils, government reduces its discretionary power but gains the confidence of business in the stability of agreed upon policies. (12)

The existence and operation of such councils are thoroughly documented for Japan by Johnson (1982), Okimoto (1989), and others in the developmental state vein. The early Korean experience of export-promotion meetings chaired personally by Park Chung Hee is also a frequently used example. Yet, as we move beyond these two Northeast Asian cases, the evidence thins. The Southeast Asian cases also experienced rapid growth but did not appear to have similar levels or types of state intervention as their Northeast Asian counterparts nor the institutions of coordination (Jomo et al. 1997; MacIntyre 1994). Moreover, empirical studies of the region raised serious doubts about the capacity of Southeast Asian states to “discipline” their private sector constituents. In Malaysia, consultative institutions at the federal level did not appear until 1991, when Mahathir’s “Look East” campaign sought to emulate the Northeast Asian newly industrializing countries (NICs). Yet, in a succession of outstanding studies, Gomez (1991; Gomez and Jomo 1997) detailed how this period was the high point of corruption, cronyism, and the interpenetration of government, state, and party. In his 1994 study of business-government relations in Asia, MacIntyre (1994) stated the claim more generally: the Southeast Asian countries simply do not fit models of business-government relations derived from the Northeast Asian cases.

In sum, the developmental state literature innovated not only with respect to policy but also in thinking about how politics and institutions might incentivize growth. In contrast to political models that emphasized checks on state power, the rule of law, and property rights, the developmental state model emphasized strong or “insulated” states, coherent bureaucracies, and institutionalized business-government relations that checked rent-seeking and made government support conditional on private sector performance. But as case studies proliferated they revealed tremendous variation across countries, including not only between the Northeast and Southeast Asian cases but also among Japan, Korea, and Taiwan. Was it possible that the taproot of growth did not lie in institutions but in longer-run features of state-society relations? And even if institutions mattered, where did they...
come from? These questions were raised by an important second generation of work on the origins of developmental states.

Origins: the historical foundations of developmental states

Much of the pioneering work on the developmental state took the form of country case studies. The small-N, cross-regional comparisons that characterized the “origins” literature had a crucial methodological advantage: the studies compared the high-growth East Asian cases to countries that had more mixed records or that had failed to grow over long periods of time (Doner, Ritchie, and Slater 2005; Kang 2002a; Kohli 2004; Vu 2007, 2010; Waldner 1999). The historical literature had broadly the same agenda as the political theories we have outlined in the foregoing sections, looking at the role of strong states and the social foundations of long-run growth. However, the cases raised the crucial question of where these states and social relations came from in the first place. In doing so, the debate on origins raised a number of central questions in comparative-historical analysis: the role of more distal versus proximate causes; questions of sequencing and path dependence; and issues of case selection and causal inference.

Colonialism was a natural place to start. Cumings (1984) had noted the Japanese colonial origins of the developmental states in Taiwan and Korea. Kohli (2004) generalized the argument and tied the literature on the developmental state into the wider debate on state formation. State structures, Kohli argued, were the product of unusual concentrations of power and coercive capability; they did not suddenly appear in response to the functional demands of some development strategy. Once put in place, “core institutional characteristics acquired during colonial rule have also proved difficult to alter. Anti-colonial nationalist movements were one potential organized force capable of altering the basic state forms inherited from colonialism,” but for the most part such movements in Asia and Africa “were too superficial and/or fragmented to alter the inherited state forms decisively” (Kohli 2004, 17). Subsequent state forms – what Kohli called cohesive-capitalist states, fragmented-multiclass states, and neopatrimonial states – were thus the products of different types of colonial rule. At least in Korea and Taiwan, the developmental state could be traced to Japanese colonial policies of building coherent bureaucracies, strong ties to private actors, and a massive repressive apparatus for dealing with class challenges from below.

At the other extreme, Nigeria was ruled by the British “on the cheap.” Indirect rule resulted in the persistence of personalist and patrimonial relations and weak states that failed to develop even the basic capacity to
extract taxes. Kohli’s analysis thus stood in stark contrast to Acemoglu, Johnson, and Robinson’s (2001) widely cited work on the adverse effects of predatory states. Kohli argued that European colonialism in Africa had adverse long-run effects on growth not because it predated on native populations and failed to protect property rights but because it never developed adequate state capacity or the bureaucratic institutions required for development; this theme has recurred strongly in the recent developmental state revival.8

Kohli’s story raised classic questions of the nature of historical explanation. How, for example, do we square the disadvantages that Brazil appeared to inherit with its very high growth in the half-century prior to the debt crisis of the 1980s? And how do we square the developmental state institutions implanted by Japan during the colonial period in Korea with the weak performance of the Korean economy in the first postcolonial decade (Haggard, Kang, and Moon 1997)? Were we searching too far back in history to locate sources of economic performance that appeared to be more proximate? The answers go to the configurational nature of causal inference in the developmental state literature. Institutional legacies were a necessary but not sufficient condition for subsequent growth. As with Johnson’s account of Japan, the analysis of the “longue durée” must be coupled with an understanding of how inherited institutions are subsequently reoriented or reconfigured to achieve developmental aims that would be impossible to achieve in their absence.

Not all of the literature went back to colonial origins; other cases focused primarily on developments in the postwar period. The literature by economists on the East Asian experience drew a comparison between import-substituting and export-oriented growth paths. As a result, the comparison between the East Asian cases and the middle-income countries of Latin America was a natural focus (Evans 1989; Gereffi and Wyman 1990; Haggard 1990). Why did import substitution persist and “deepen” in Latin America while the East Asian countries shifted course toward greater reliance on exports?9

8 As with Evans, Kohli notes intermediate cases, including India and Brazil. The Indian nationalist movement altered British colonial structures to a certain extent. The Estado Novo (1937–45) and period of military rule (1964–84) reflected periods of state “hardening” in Brazil. But Kohli stresses the persistence of inherited political and social structures. In Brazil, for example, the power of landed oligarchs, local authoritarianism, and a weak central government lingered for at least a century after decolonization, and traces of these historical residues can be found to this day.

9 This comparative work had the advantage of being able to draw on debates in Latin America about similar processes, particularly Guillermo O’Donnell’s (1973; Kaufman 1979) model of bureaucratic authoritarianism (BA), which bore a clear family resemblance to developmental state ideas (see, for example, Im 1987 on BA in Korea).
Haggard (1990) looked at the interaction of size, external shocks, and the nature of the state’s relationship with key social forces. In the face of external shocks, larger developing countries, and particularly those with a natural resource base, could continue financing import substitution industrialization (ISI) despite the constraints it placed on manufactured exports; they were more likely to move into secondary import substitution (“deepening”) than were smaller countries pursuing ISI that lacked natural resources and did not have similar domestic market opportunities. Haggard noted that the shocks of interest in the East Asian cases included a precipitous decline in aid in the canonical cases of Korea and Taiwan, a shock that triggered a scramble for new sources of foreign exchange in exports, foreign investment, and borrowing. Doner, Ritchie, and Slater (2005) similarly noted that foreign exchange and revenue constraints – including those associated with the absence of natural resources – were crucial determinants of efficiency-enhancing institutional developments.

A central issue of interest for Haggard was the interplay between the state and social forces. Labor was weak across the newly industrializing countries but appeared particularly restrained in the anti-Communist authoritarian regimes that were paragons of the export-led growth approach: Korea, Taiwan, Singapore, and colonial Hong Kong. Labor certainly benefited from rapid real wage growth; Campos and Root (1996) went so far as to argue that East Asian growth was “inclusive” and based on an implicit social contract. But, although governments invested in education and real wages rose, labor weakness and even outright repression appeared integral features of an export-oriented model that rested on low-wage labor, labor market flexibility, and managerial autonomy on the shop floor (Deyo 1989).

By contrast, longer periods of import substitution in Latin America had entrenched complex systems of unequal social entitlements that were ultimately rooted in employment in import-substituting activities; Haggard and Kaufman (2008) drew these comparisons in their historical analysis of the origins of welfare states in the two regions. Business interests were wedded to protection in both regions as well, but the sheer duration of import substitution in Latin America entrenched the strategy more deeply. It would have required a particularly powerful and independent state to shift policy in a more outward-oriented direction in the Latin American cases.

Over time, the “origins” literature widened from the East Asia–Latin America comparison to encompass the Middle East, South Asia, and Africa. Waldner (1999) argued that a key difference between Turkey and Syria, on the one hand, and Korea and Taiwan, on the other, was the breadth of the
coalitions elites forged at the time states were being formed. In Syria and Turkey, “premature” pressures to widen social coalitions gave rise to what Waldner called “precocious Keynesianism”: states that were committed to growth-inhibiting transfers. Kohli (2004) similarly noted that “fragmented multi-class states” – represented in his four-country comparison by Brazil and India – precluded pro-business policies. Evans (1995) similarly described Brazil as an “intermediate” case – between developmental and predatory states – in which clientelistic links to business and labor eroded the capacity to orchestrate a successful entry into global IT markets. By contrast, all four of these cross-regional comparisons emphasized the presence of the features outlined above in the East Asian cases: relatively autonomous states and coherent bureaucracies that were able to impose the short-run costs associated with major policy transitions (Haggard), limit transfers (Waldner), develop a distinctively pro-business environment (Kohli), or coordinate with the private sector to advance international competitiveness (Evans).

The findings of the work on the origins of developmental and other states raised one of the most vexing problems for comparative-historical analysis. The focus on the East Asian cases was driven by pressing pragmatic concerns: an interest in the sources of rapid growth and the concomitant improvement in human welfare. What were we to make of arguments that the success of these cases was rooted in historical factors that could sometimes be traced to the colonial era? Was history fate? Or could it be used to draw meaningful policy lessons?

**Conclusion: the developmental state is dead: long live the developmental state!**

Although the developmental state literature took off in the 1980s and 1990s, it was largely preoccupied with a much earlier period. Johnson’s book built up to the great industrial transformation in Japan in the 1950s. The subsequent literature on the newly industrializing countries of Asia looked back to the “takeoff” of the 1960s and 1970s. As the leading exemplars of the developmental state model gradually liberalized their economies, the concept appeared destined to become a largely historical construct: an explanation for an unusual period of very high growth limited to a small group of Asian countries.
However, the developmental state literature focused on questions that were central to the discipline – the relationship between the state and the market, the political and institutional foundations of economic growth – that were unlikely to fade. More important, ideological winds were about to shift as the market triumphalism of the Washington consensus and early post–Cold War period proved surprisingly short-lived. The financial crisis of 2008 also affected the tenor of the debate, setting in train a quest for a “post-Washington consensus” (Birdsall and Fukuyama 2011). It is worth tracing this intellectual history briefly, because the developmental state has enjoyed a surprising revival in recent years.

The onset of the Asian financial crisis in 1997–8 provided an opportunity for critics to underline the risks associated with industrial policy and restate the case for market-oriented reform. Rather than seeing strong states and well-ordered bureaucracies, critics noted the myriad problems of rent-seeking and moral hazard. Defenders of the developmental state model told a very different story about the crisis, emphasizing the risks of deregulation, and capital account and financial market liberalization, in particular (Stiglitz 2002; Wade and Veneroso 1998). It was only a short step from these observations about policy to the underlying political economy (Haggard 2000; MacIntyre 2003; Mo and Moon 1999). The weakness of regulation was not simply an accident of history, a sin of omission on the road to liberalization. These weaknesses were consistent with growing business influence over regulatory institutions and the declining ability of governments to check rent-seeking, the socialization of risk, and moral hazard. Rather than discrediting the developmental state model, the crisis confirmed some of its central tenets. In the wake of the crisis, reformers such as Kim Dae Jung sought to reverse these trends in business-government relations, in part through liberalization and in part through more robust regulation of the financial sector and corporate governance.

Nor did liberalization spell the end of industrial policy. Rather, empirical work shifted to the question of how to conduct industrial policy in a more open-economy context (Evans 1998; Hayashi 2010; Low 2004; Rodrik 2007; Weiss 1998; Wong 2004; Yusuf 2003). For example, Deyo, Doner, and Hershberg (2001) and Doner (2009) traced the evolution of Asian industrial policy across both natural resource and manufacturing industries over time. Not all of these accounts were success stories. Noble (1998) noted that “coordination” was difficult to distinguish from “collusion,” and Wong (2011) offered a cautionary tale on the biotech industry in Asia. Nonetheless, by the end of the 2000s a new round of debate had emerged on the merits of industrial policy.
based on fears of the so-called middle-income trap (Gill and Kharas 2007). Previously successful economies could not continue to grow through either sheer capital accumulation or further liberalization alone. Rather, the shift to a new development strategy rooted in higher productivity growth required new interventions, business-government relations, and political coalitions (for example, Paus 2012).

For countries at lower levels of development, the great boom of the 2000s saw high growth spread across the developing world and in countries pursuing diverse and heterodox developmental strategies. China was the most obvious example, and it was only a matter of time before efforts were made to explicitly assimilate the country into the broader East Asian model (for example, Baek 2005; Knight 2010). Although not without controversy, no less than the (Chinese) chief economist of the World Bank advanced a model of growth for low-income countries in which the state would “facilitate structural change by aiming to provide information, compensate for externalities, and co-ordinate improvements in the ‘hard’ and ‘soft’ infrastructure that are needed for the private sector to grow in a manner consistent with the dynamic change in the economy’s comparative advantage” (Lin and Monga 2011, 265; Lin 2009).

The debate over the future of the developmental state was not limited to the role of industrial policy; it addressed the issue of political change as well. The canonical Northeast Asian developmental states – Korea and Taiwan – democratized, followed after a lag by a number of the Southeast Asian countries. These changes raised the empirical issue of how growth might be affected by a more liberal political environment (for example, Rock 2013). In an important and forward-looking collection, a group of British development scholars and practitioners coined the term “the democratic developmental state” (Robinson and White 1998). The collection began by challenging the authoritarian hypothesis, noting the mixed record of authoritarian rule as a whole, and restating the classical liberal argument that democracy and growth were not only compatible but reinforcing over the long run (see also White 2006). The volume raised the issue of how the lessons of the East Asian cases could be adopted to a changed political environment in which the objectives were not simply to grow rapidly but to do so in the context of increased accountability and attentiveness to the provision of social services and the expansion of human capabilities (Evans 2011).

Yet the ligaments of earlier argument remained in place. If anything, such a state required even greater

10 Although it is beyond the scope of our interests here, a new literature emerged on the East Asian welfare state, focusing on its distinctive features during the developmental state era and its transformation following the transition to democratic rule.
capacity than the “classical” developmental state, with its single-minded focus on growth.

Africa became a somewhat surprising theater for this debate (Edigheji 2005, 2010; Meyns and Musamba 2012; Mkandawire 2001). In a scathing indictment, Thandika Mkandawire (2001) challenged “the impossibility theorem”: the idea that African states were too dependent, weak, incompetent, and corrupt to emulate the East Asian developmental states. Political scientists working on the region had long focused on the patrimonial nature of African governments, and some of the comparative-historical work cited earlier – including both Evans (1995) and Kohli (2004) – had explicitly drawn comparisons between developmental and predatory African states. From the 1980s, “governance” began to gain attention from the international financial institutions as well.

Mkandawire (2001) argued, however, that “most of the analyses about African states that have led to so much despondency about the prospects of development are based on invidious comparison between African states in crisis and idealised and tendentiously characterised states elsewhere” (290). On the one hand, neoliberal approaches to development in Africa had downplayed the significance of developing state capacity, a core feature of the Asian developmental states. On the other hand, the critics of African governance had overlooked the fact that the East Asian model hardly conformed with the new received wisdom on governance: policy was far from liberal, states intervened pervasively, rents were distributed to chosen firms, and government and business worked hand in hand. Rather than seeking to finesse Africa’s governance problems through weakening the state and focusing on liberalization, policy should aim to strengthen state capacity (Fritz and Menocal 2006) and “get interventions right.”

Most recently, these arguments have been generalized in an important debate about the role that accountability and capacity should play in the conceptualization and measurement of government performance. In a wide-ranging indictment of contemporary approaches to governance, Francis Fukuyama (2013) argues that both academics and the policy community have placed too much emphasis on the role of regime type, institutions of accountability, the rule of law, and procedural features of the bureaucracy and not enough on state autonomy and capacity. Fukuyama’s critique circles back to long-standing themes in the developmental state literature and is tribute to the fact that the questions raised by this research program are alive and well.

Some strands of the new institutionalism in economics (Acemoglu, Johnson, and Robinson 2001) reached implicitly pessimistic conclusions about
the capacity to escape poverty traps, noting the heavy weight of the dead hand of history. Yet the developmental state literature was from its inception deeply motivated by an engagement with policy debates; Wade, Amsden, Chang, Rodrik, and others were not simply historians of development but pragmatists wary of cookie-cutter models. Deterministic formulations on the origins of long-run growth – including some of those that lurked in the origins debate – were ultimately of less interest to them than mining history for lessons that could be adapted to different national contexts. Central to these lessons was the observation that growth was not simply the result of market-oriented policy or even appropriate interventions but of underlying state capacity. This observation remains the most simple and enduring of the contributions of the developmental state approach.

References


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