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Abstract

This paper takes on the apparent paradox of India's combination of durable democracy, capable bureaucracy, but a deficient development trajectory. It begins by outlining the nature of the problem. Then it summarizes and compares some significant approaches to drawing connections from governance institutions to development outcomes. Next, it reviews some of the contributions to understanding India's political economy in the last few decades. Finally, it attempts to draw some lessons from India's experience for the application of different theories of governance and development. The central lesson is that the paradox recedes when attention is paid to the subnational level, where India's states encompass considerable variation in initial societal and economic conditions and in development outcomes.

Keywords: democracy, embedded autonomy, governance, limited access orders, India, inclusive institutions, political economy

JEL Codes: H1, O10, O43, O57, P26

Theories of Governance and Development: How Does India's Experience Fit?

Nirvikar Singh

1. Introduction

India's combination of durable democracy and low levels of development make it an important case study for the role of the state in development. Evans and Heller (2018), in surveying the Asian experience and its lessons, label India as a "paradox" (p. 10) for this literature, and the "most analytically challenging" (p. 1) of the Asian states. Acemoglu and Robinson (2012), in their sweeping analysis of the interaction of political and economic institutions in shaping development, have relatively little to say about India, but Subramanian (2013), in reviewing their work, points out that India (as well as China) is a significant outlier in the cross-country relationship between a democracy index and GDP per capita: it is "too economically underdeveloped, given the quality of its political institutions." Subramanian concludes that the specificities of the history of India (and China) make them unsuited for parsimonious theorizing. Can one do better?

There is a literature that starts from the specificities of India. Sinha (2016) identifies Frankel (1979), Herring (1983), Bardhan (1998 [1984]), Rudolph and Rudolph (1987), and Kohli (1987, 1990) as early contributors to conceptualizing a political economy of India. Evans and Heller (2018) also remind us of Myrdal's (1968) analysis of India in the context of looking at Asia, and Evans (1995) pays considerable attention to India, although his analysis of South Korea is mostly what receives attention – again because its story seems clearer than India's.

This paper seeks to distill and unify some of the vast literature on the state and development with the express purpose of trying to evaluate how well parsimonious theorizing can explain India's experience. Evans and Heller (2018) and many others begin with the "state" and unpack that concept (for example, separating out politicians and bureaucrats, or different layers of government). An alternative perspective makes groups (often left abstract, but commonly defined by class, caste or possibly regional boundaries) the building blocks of coalitions that define particular kinds of social order (North, Wallis and Weingast, 2009). To the extent that a dominant coalition has a monopoly over the "legitimate" use of violence, this is the "state," as conceived by Weber (1991): "a human community that (successfully) claims the monopoly of

Theories of Governance and Development: How Does India's Experience Fit?

Nirvikar Singh

the use of physical force within a given territory” (p.78). Nevertheless, the different starting points are indicative of different methodological approaches. In the title, we have chosen to use the term more amorphous term, “governance,” chiefly to avoid any reifying connotations of the “state.”

The next section examines some of the relevant literature on the interaction between political and economic institutions in driving development. The approaches considered are those of the “developmental state” (for example, Evans, 1995), “access orders” (North, Wallis and Weingast, 2009), inclusive and extractive institutions (Acemoglu and Robinson, 2012) and “political settlements” (Khan, 2010).¹ This is not meant to be a comprehensive survey, but rather, a quick overview of the similarities and differences of approach. This seems useful in a situation where the different strands of thought may not fully engage with each other.² Section 3 summarizes some of the analyses of India's political economy, including the works called out by Sinha (2016) and listed earlier, but also more recent efforts such as Joshi (2017), as well as comparative studies such Evans and Heller (2018), where India is given significant attention. In much of the writing of economists, the focus is on normative economic policy (for example, Panagariya, 2008), with limited explicit attention to underlying political factors, but even neglect of politics in the analysis is worth pointing out in some cases. Again, we will not attempt to be comprehensive in treating a large literature. Section 4 then tries to offer some evaluation of how to make progress in understanding the “paradox” of India, perhaps to the point of removing the paradox entirely. This section further draws on studies of India, but emphasizes the subnational level much more. Rao and Singh (2005) was a relatively early attempt at this approach, but much has happened in India since then, and a large literature on decentralization efforts in India has emerged. Section 5 offers a summary conclusion for this paper.

¹ The references given in each case are synthetic works of these authors, and each builds on earlier work of their own and others.

² In particular, Evans and Heller (2018) reference none of the other approaches, and other authors mention alternatives mostly in passing.

Theories of Governance and Development: How Does India's Experience Fit?

Nirvikar Singh

2. Theories of Governance and Development

The explicit concept of a “developmental state” is often traced to the analysis of Japan’s post-World War II economic success (Johnson, 1982), with further refinements based on other East Asian experience (Amsden, 1989; Wade, 1990). The modern generalization and conceptualization of this approach can be attributed to Evans (1995), with his treatment of South Korea, Brazil and India, and the introduction of the notion of “embedded autonomy.” This modifies the Weberian idea of a capable, autonomous bureaucracy as the key implementer of state development policies, to emphasize that this group must also be “embedded” in society in ways that enhance information flows and “negotiation and re-negotiation of goals and policies” (Evans, 1995, p. 12). The intellectual influence of Weber also reminds us that there has been a much older tradition of recognizing the role of the state in development, in European cases such as France (for example, Loriaux, 1999) and Germany (Boldrin et al., 2012), as well as the earlier history of Japan (Horie, 1937).

The key role of autonomy in this narrative is familiar, that of avoiding political capture by the private industrial elites who were needed for implementing crucial aspects of economic development. Evans and Heller (2018), in their summary, emphasize the other preconditions for a developmental state, namely, a balance of power that included weak civil society, and landed elites that had been wiped out by war or revolution. The final piece of the story is the perception of national elites that development was necessary for national survival amid geopolitical threats. The balance of power and perceptions of survival threats were differentiators from other countries with bureaucracies that appeared to satisfy the condition of embedded autonomy, such as the Philippines (Kang, 2002; You, 2005).

One might even veer toward the view that what constitutes effective “embedded autonomy” depends on other, contextual and historical elements, and, furthermore, that the concept itself needs broadening (Evans, 2010; Routley, 2012), to the point where it cannot be a fundamental building block in understanding the developmental state. Indeed, the developmental role of the state may be too complex to be amenable to parsimonious answers (Bardhan, 2016).

Theories of Governance and Development: How Does India's Experience Fit?

Nirvikar Singh

Nevertheless, attempts at such theorizing abound, and we turn to alternatives that have received considerable recent attention.

Acemoglu and Robinson (2012) frame their understanding of what drives development in terms of economic and political institutions. Their key distinction is between “extractive” and “inclusive” political institutions. Political institutions are the “rules that govern incentives in politics” (p. 79). They determine who has power and how that power is used. Centralization is necessary to allow for basic functioning of “the state” – which is conceived of again in terms of the Weberian “monopoly of legitimate violence,” but also as a coalition of interests. If political institutions distribute power broadly in society then Acemoglu and Robinson view them as “pluralistic.” Political institutions that are “sufficiently centralized and pluralistic” are “inclusive.” Otherwise they are “extractive.” Of course, there is a connection between economic and political institutions: extractive political institutions will tend to concentrate economic power, so that economic institutions are also extractive. The two go hand-in-hand, and one cannot expect inclusive institutions in one category to work with extractive institutions in the other category.

Acemoglu and Robinson would presumably frame “embedded autonomy” as part of the synergy of inclusive political and economic institutions. But in the case of countries like South Korea, political institutions were arguably not inclusive when economic development took off. Political power was concentrated, and perhaps so were economic institutions, in areas such as domestic competition policy. The discipline on rent extraction came from external competition and geopolitical threats and rewards. While Subramanian highlights China and India as outliers in a cross-section regression of GDP per capita on a democracy index, there may be other examples that do not fit well (Boldrin et al., 2012). So it may be that the framework of inclusive and extractive institutions, while identifying and labeling an interaction between politics and economics that is needed for development (the creation and distribution of economic rents³),

³ Economic rent is surplus value accruing to a resource beyond what can be obtained in the next best use. It may be the result of natural (talent, for example) or artificial (legal barriers, for example) restrictions in supply. The process by which economic rents are created and shared is one way to think in the abstract about economic and political institutions and how development proceeds.

Theories of Governance and Development: How Does India's Experience Fit?

Nirvikar Singh

may leave out important factors, or not be analytically sharp enough. While Acemoglu and Robinson bring more focus on the “primitives” of the distribution of power, they may not sufficiently address the specifics of institutions such as the bureaucracy, and may overemphasize the priority of inclusive political institutions over economic institutions – the reverse causality from economic development to political inclusiveness may not be acknowledged enough.

North, Wallis and Weingast (NWW, 2009) take an approach that is similar in many respects to that of Acemoglu and Robinson. They frame their analysis in terms of “social orders.” A social order is a stable coalition of groups, each with its own leader. Leaders within the dominant coalition, in particular, have privileged access to resources, and their group members are their clients. Privileged access creates economic rents, which are distributed by leaders. A primary characteristic of any social order is the control of violence within a society: this explicit emphasis on control of violence is one differentiator from other theories of governance, but is consistent with the Weberian view of the state, and goes all the way back to Thomas Hobbes in the 17th century.

NWW distinguish between limited access orders (LAOs) and open access orders (OAOs). In the former, access to resources and rents is limited and personalized. An open access order access to resources is competitive and more impersonal. This translates into three “doorstep” conditions for an OAO: consolidated political control of organizations with violence capacity, support for perpetual “elite” organizations,⁴ such as government entities and private corporations, and rule of law for elites. LAOs are further graded into fragile, basic and mature orders. The first of these has difficulty controlling violence, and violence capacity is dispersed – organizations outside the dominant coalition (which is unstable) are not viable. A basic LAO does better on both fronts: violence control and institution-building, and the mature LAO does better still, but personalization and exclusion still matter. The OAO represents a difference in kind from LAOs, compared to the differences in degree among different gradations of LAOs.

⁴ The elite here is the dominant coalition, but membership in the elite is not fixed by personal identities and ascriptive characteristics, but by characteristics that can be acquired. For example, a meritocratic bureaucracy would be an elite group.

Theories of Governance and Development: How Does India's Experience Fit?

Nirvikar Singh

The NWW focus is on coalition formation among groups in society, and the impacts of the distribution of power for the type of order that emerges and evolves. In that sense, it is related to Acemoglu and Robinson's conceptualization, but it also seems to have more commonalities with ideas of "weak" and "strong" states, in the literature on the state and development, or the interpretation of that literature in terms of institutional economics concepts such as credible commitment (Bardhan, 2016). Arguably, NWW also have a sharper notion of political competition, especially in leadership contests, than the inclusive political institutions of Acemoglu and Robinson (Kotwal and Roy Chaudhuri, 2015). Both NWW and Acemoglu and Robinson have, in the background, models of market competition based on dispersed and assured property rights, something that is less firmly in the state and development writings of non-economists. On the other hand, the NWW emphasis on control of violence (Gray, 2015) seems to limit their view of wasteful (Directly Unproductive Profit Seeking, or DUP – Bhagwati, 1982) rent-seeking, which could involve other socially costly behaviors besides violence. Gray also critiques the narrow perspective on human behavior associated with neoclassical economics, just when economics is incorporating behavioral and evolutionary considerations in models of social order and dynamics.⁵ She also highlights the underlying transaction cost perspective that has shaped the NWW approach and which is a hallmark of the new institutional economics.⁶

A final related approach to be described here is the "political settlements" concept of Khan (1995, 2010). Khan sees this as related to, but more precisely defined than, the concept of a "social order." Specifically, his definition is (Khan, 2010, p. 4), "A political settlement is a combination of power and institutions that is mutually compatible and also sustainable in terms of economic and political viability." Noting the difficulty of defining power in any kind of general manner, Khan (2010, p. 6) narrows it down to a game theoretic context: "the capability of an individual or group to engage and survive in conflicts." He calls this "holding power."⁷ In

⁵ This is obviously something less present in the "developmental state" approaches of non-economists. Exploring these issue further is beyond the scope of this paper.

⁶ Again, Bardhan (2016) is a good reference for how one might bridge the different modeling approaches and perspectives of economists and non-economists.

⁷ In more formal game theoretic models, one could further distinguish between discount factors, threat payoffs, punishment technologies, and so on, in isolating different facets of "holding power." Khan also discusses sources of power, such as determination, mobilization strategies, and capacity for absorbing costs of conflict.

Theories of Governance and Development: How Does India's Experience Fit?

Nirvikar Singh

somewhat of a parallel to LAOs and OAOs, Khan distinguishes between clientelist and capitalist political settlements. However, he argues that the dimensions of variation in clientelist political settlements do not correspond to the gradations of LAOs posited by NWW. Furthermore, he argues that his pre-conditions for transition are not the doorstep conditions of NWW, but instead (p. 58) a sufficient level of “development and dispersion of productive ‘capitalist’ organizations.” There is much additional richness in Khan’s formulation, and in some ways it can also be seen as a bridge between the “developmental state” literature and the “institutional economics” rooted in game theory of Acemoglu and Robinson and NWW.

If Khan’s approach is a useful hybrid, it is also worth noting that there are many other contributions that are relevant as building blocks for theories of governance and development, beyond the four encompassing approaches outlined here. These include Olson’s (1965) pioneering work on collective action, Ostrom (1990) at a more micro level, and numerous formal models of social conflict or state-society interactions, such as Skaperdas (1992), Besley and Persson (2009), and Acemoglu et al. (2011), that provide parts of an overall understanding of the interaction between political and economic institutions.

3. India’s Political Economy

In this section we provide a quick tour of some of the salient analyses of India’s political economy, trying to relate them to the general theories presented in Section 2. Evans and Heller (2018) highlight several important themes with respect to Indian experience in Myrdal’s (1968) sprawling work: the importance of human capital and India’s failure to invest in it sufficiently, the problem of social inequality and lack of rural infrastructure, inadequate social protections, and ultimately a democratic apparatus with more form than substance – a “soft” state. There are many areas where Myrdal’s analysis was less accurate, particularly with respect to other parts of Asia, but this distillation of his diagnosis of India’s challenges seems to have held up well 50 years later.

Theories of Governance and Development: How Does India's Experience Fit?

Nirvikar Singh

Frankel (1979) offered a pessimistic view of Indian politics, as unable to accommodate the social change needed for significant economic development. Her earlier work on the Green Revolution had turned out to be prescient in predicting the negative consequences of increased inequality, as can be seen in contemporary Punjab (Singh, 2016). An updated version of her more general analysis, bringing it up to 2004, reprised her earlier themes, but ultimately can be seen as description in search of a theoretical framework.

To varying degrees, Herring (1983), Rudolph and Rudolph (1987) and Kohli (1987) address the problem of agrarian transformation in India, and their political economy analyses indicate the constraints of changing an unequal distribution of key resources such as farm land through gradual means. These approaches treat the “state” as an actor that is more or less autonomous, though Rudolph and Rudolph describe “demand groups,” somewhat similar to the standard interest groups of economists’ analyses,⁸ and Kohli views the state as captured by private capital to the detriment of the interests of the poor and disadvantaged. Of course, Kohli and Rudolph and Rudolph go beyond agrarian issues to tackle broader questions of political economy, and the latter, in particular, offer a theoretical framework that Sinha (2016) describes (p. 267) as having “married class analysis to Weber.” However, as Sinha points out, they are missing a concrete framework of the workings of the “state,” which remains an abstract third entity like capital and labor. Ultimately, the India-driven specifics of the Rudolphs’ analysis may not have been transferable enough to other contexts to be durable.

Bardhan (1998 [1984]) and Kohli (1990) have offered more general insights into India’s political economy. Sinha (2016) summarizes Bardhan as combining a class analysis to public choice theories of the state, and his idea, that a balancing of varied class interests to keep the dominant coalition in charge creates a situation of multiple vetoes and obstacles to change, has been a powerful one for understanding the workings of India’s political economy. Perhaps one can relate this characterization to the mature LAO of NWW, or the clientelist political settlement of Khan’s analysis. Kohli offered a pessimistic view of governance in India – indeed, a “crisis of governability,” which he traced to the organizational weaknesses of political parties (often

⁸ Sinha highlights that demand groups in the Rudolphs’ conception are more fluid and “agitational” than interest groups.

Theories of Governance and Development: How Does India's Experience Fit?

Nirvikar Singh

reduced to collections of loyalists with no internal organization to speak of) combined with weak leaders, along with class conflict and a proliferation of demand groups. In retrospect, governance in India has proved more resilient, surviving many minority governments, managing a slow but rational process of economic policy reform, and achieving some decentralization without jeopardizing national unity or stability.⁹

Bardhan updated his original 1984 analysis in 1998, after India had embarked on a process of “economic reform,” which has been marked by loosening of government control of the domestic economy, particularly industry, and opening up to international trade. This has also been seen as a “retreat” of the state, but that perspective probably needs qualification. Bardhan in 1998 noted that subsidies for various interest groups remained mostly in place, and did not see a major shift in the political equilibrium he had described 15 years earlier. While he was cautious about the continued progress of market-oriented reforms, he noted changes in the dominant coalition (more diversity, greater fluidity) and in bureaucratic attitudes (some acceptance that the state had overextended itself, beyond its capacity). He also suggested a connection between the two: traditional elites pushed out of government because of broader reservations would favor policy changes that would give them more opportunities in the private sector.¹⁰

The thirtieth anniversary of the publication of Bardhan's book provided a further opportunity for examination of changes in India's political economy. Bardhan (2015) emphasizes the difficulties of collective action in the face of social heterogeneity and inequality, especially for long-term public investment. The apparent lack of state capacity is then potentially traceable to the systemic problems of inequality, rather than a shortage of expertise. He also highlights the increasing mismatch between aspirations and opportunities, especially for young people in India, and perceptions of illegitimacy of capitalism when corruption and unproductive rent-seeking flourish at the expense of more inclusive economic growth. The cost of competing in elections has also increased, attenuating the force of political competition. Again referring back to the

⁹ Singh (2018) offers an institutional analysis of the role of India's federal structures and institutions in holding India together.

¹⁰ Another possibility is that emigration among elites provided a way out in the face of broader caste-based reservations in the public sector. Furthermore, this has also created a positive feedback loop for economic reform.

Theories of Governance and Development: How Does India's Experience Fit?

Nirvikar Singh

models of section 2, this is very far from a situation of doorstep conditions for a transition from LAO to OAO, but the framing of India's political-economic equilibrium and changes in that equilibrium, can be easily accommodated in that approach.

Evans' (1995) model of embedded autonomy, inspired by East Asian experience, was applied to India as well. Evans characterized India (along with Brazil) as an "intermediate" state along an axis running from "strong" (or autonomous) to "weak" (or captured). He viewed India's bureaucracy favorably from a Weberian lens (though recognizing its imperfections), and relatively autonomous, but lacking the right kind and level of embeddedness. It lacked the means to garner useful information from the private sector that could guide policy interventions, and those interventions lacked selectivity and focus. Ultimately, those problems were linked to India's size and the heterogeneity of its society. In this respect, "embedded autonomy" sits uneasily as an explainer of the Indian experience, a judgement reinforced by an expansion of the meaning of the term in the context of understanding the case of Kerala (p. 237): "it was in a position to put the relatively well developed bureaucratic autonomy that characterized the Indian system as a whole together with its own brand of embeddedness to produce a ... an idiosyncratic version of "embedded autonomy," extremely well suited to accomplishing a transformative project aimed at increased levels of welfare." We return to the "idiosyncratic" case of Kerala in Section 4, along with the more general re-evaluation by Evans and Heller (2018).

As noted in the introduction, most of the debate among economists with respect to India's development trajectory has been about technical issues of policy reform, along with the goals of policy – what constitutes development? Bhagwati and Panagariya (2013) offered an implicit response to critiques of a policy focus on GDP growth (Dreze and Sen, 2013) by distinguishing between "Track I" and "Track II" reforms. The former are "conventional" market-oriented, growth-enhancing policy changes, such as trade liberalization. The latter are more redistributive in nature, aiming to reduce poverty and enhance the capabilities of the poor. Perhaps what is missing, though, from both sides of this debate is the nature of the political economy equilibrium that prevents these two tracks from being designed and implemented effectively.

Theories of Governance and Development: How Does India's Experience Fit?

Nirvikar Singh

Kohli (2006a, 2006b) provides an emphatic political economy answer, characterizing Indian economic policy reforms as more “pro-business” than “pro-market.” He explicitly relates his analysis to the work on East Asia of Johnson, Amsden, Wade and Evans cited earlier.¹¹ The implications of his position for other theories of governance and development are also taken up in the next section. For now, it is worth noting that Kohli identifies the impetus for change as coming from the national political leadership, rather than any change in the embedded autonomy of the bureaucracy, or other aspects of state-society relations. This, in turn, was driven by the accumulated experience of the decade of the 1970s, during which economic growth was slow, and political unrest increased. A mix of leadership motives, including intrinsic factors along with calculations of electoral success were at work in this narrative.

Finally, one of the most recent volumes on Indian economic reform (Joshi, 2017) includes an explicit analysis of India's political economy during this time. Joshi emphasizes increased political and economic fragmentation, greater salience of identity politics, and increased corruption. His view of state competence and accountability is quite negative, more so, at first sight, than the perspective on the bureaucracy offered by analysts such as Evans and Kohli. To some extent, this reflects viewing the state more broadly than just the elite bureaucracy, and when that is recognized, there is probably almost unanimity on the weakness of the state below the top rungs. Corruption and crony capitalism are also reflected in a decay in bureaucratic autonomy, and even in the institutional decay of political parties. This suggests a state moving backward along any normative ordering, in any of the conceptualizations of that ordering discussed in Section 2. This is another issue to be examined in the next section.

4. Governance Theories for India

India's development trajectory seems to present a challenge for theories that link the structure of governance to development performance, so much so that Evans and Heller (2018) pose the issue

¹¹ Kohli is also categorical in viewing the pro-business shift as anti-labor, in line with a view of the East Asian experience as involving significant labor repression. On the other hand, the “labor” here is only the small fraction of the workforce that are in the “formal sector.”

Theories of Governance and Development: How Does India's Experience Fit? Nirvikar Singh

as a paradox. Their own resolution of the puzzle has several components. First, they acknowledge the point made at the end of the last section, one that has been made by many, and one which does not accord with an emphasis on the embedded autonomy of elite bureaucrats: India's state capacity tapers off dramatically outside a narrow, centralized elite. Second, they highlight another common observation, that India failed to be sufficiently inclusive in its initial development strategy, whether it was effective broad-based investments in health and education, failure to reallocate productive assets such as land, or fostering access to financial capital for those entrepreneurs who could generate jobs for the masses. Third, they argue that India's size and heterogeneity make it difficult to achieve the right kind of embedded autonomy – there are too many different kinds of interests to be balanced. This is, of course, reminiscent of Bardhan's political economy analysis. Fourth, they draw attention to the problems of governance at the subnational level, particularly the local level, where effective democracy was absent for decades, and still lacks adequate funding, even after the creation of formal democratic structures through constitutional amendments.

We might abstract from these four components, and describe the situation as one of vertical as well as horizontal dispersion in characteristics and initial conditions. Furthermore, the emphasis on maintaining national political unity or integrity can be seen as resulting in sacrificing the building up of capabilities outside existing centers of power. In this narrative, India's elites made a tradeoff between development and stability, and, like any elite, erred on the side of choices that defended the status quo. One can also include non-instrumental factors, such as hubris and self-esteem: the derision accorded to “uncultured” *nouveau riche* small-town entrepreneurs by the successors to the colonial elite suggests this (Mishra, 1995).

Putting aside cultural factors, which can be surprisingly amenable to adjustment if economic incentives change, we would argue that concepts such as the “state” and “embedded autonomy” may not be the right starting point for a theory of governance that can have explanatory power for the Indian experience. The same applies, to some extent, to the “inclusive-extractive” dichotomy of Acemoglu and Robinson, which may be also be too hard to pin down in its specifics for a theory that works for India (aside from other sources of critique of their analytical

Theories of Governance and Development: How Does India's Experience Fit?

Nirvikar Singh

narrative). This leaves the approaches of NWW and Khan, which we examine further in the Indian context.

Kotwal and Roy Chaudhuri (2015) address the presumed paradox of India having a well-functioning democracy without commensurate developmental achievement by highlighting the absence of a (p. 52) “political meritocracy based on mass support.” Specifically, there is no “competitive process at the ground level able to throw up meritorious leaders.” This is an important observation, which Kotwal and Roy Chaudhuri trace to the access order framework of NWW, and which they see as distinguishing that framework from the less precise concepts of competition contained in Acemoglu and Robinson’s inclusive political institutions. This insight provides a theoretical underpinning for tracing out the consequences of the decline of party organizations, noted by many of the writers referenced in this paper. Indeed, there are two further aspects of this deficiency that we can make more explicit.

First, the problem with dynastic and personalized party organizations is not present just at the national level, but almost uniformly holds at the state level as well. The exceptions at the national level noted by the authors are the Bharatiya Janata Party (BJP) and the Communist Party of India (Marxist) (CPM). But the CPM was effectively present only in two states, Kerala and West Bengal, while the BJP, having provided an example of a state level leader rising through the ranks to become prime minister, is now moving to a more personalized model itself. Regional parties at the state level, and even state-level components of national parties, are run as dynastic, personalized entities. Second, the absence of local level politics as a training ground for up and coming politicians has vitiated a potential avenue for demonstrating competence in governance, rather than personal loyalty.¹²

To summarize, the insight of Kotwal and Roy Chaudhuri is that the nature of Indian politics, which can perhaps be seen as an offshoot of the importance of ties such as kinship, caste or feudal loyalty, is a major barrier to India’s governance structures moving toward the doorstep

¹² In other countries, being mayor of a large city would be a natural proving ground for politicians, but cities are barely beginning to establish independent governance institutions, even after constitutional decentralization, because of the continued absence of fiscal autonomy and the shadow of state and national-level administrative and political control.

Theories of Governance and Development: How Does India's Experience Fit?

Nirvikar Singh

conditions for an open access order. It is important to note another aspect of the authors' analysis. Like Evans and Heller (2018) and other analysts of India's governance, they note the diversity of outcomes at the local level, that have resulted from decentralization.¹³ This diversity can be seen as a function of heterogeneous initial conditions: for example, the political ideology of leftist parties in power in Kerala and West Bengal changed the distribution of local power prior to national decentralization legislation. In Kerala in particular, human capital in the form of health and education was more evenly distributed than anywhere else in India.

Mangla (2015) provides another subnational example, but focusing at the level of state bureaucracies rather than elected local governments (which in fact lack their own bureaucracies for policy design and implementation). He compares two states, Himachal Pradesh (HP) and Uttarakhand, which are similar in terms of size, geography and societal composition. He makes the case that HP has done better in delivering public education to its population because of a more effective bureaucracy, but that this effectiveness has not been determined by formal, Weberian characteristics, but instead by informal norms and organizational culture. In particular, he views HP as illustrating the positive impacts of deliberative governance, which allowed for a certain level of collective wisdom to emerge. The difference in Uttarakhand was a more legalistic approach, which might be considered more typical of Indian bureaucracy, and indeed, the state was only split off from the behemoth of Uttar Pradesh (with a population comparable to Brazil's) only relative recently. Some of Mangla's observations and analysis are reminiscent of the more well-known case of Kerala, which has been described by Evans (1995) as well as Evans and Heller (2018). The general lesson may be that it is not helpful to examine India in the aggregate, in trying to establish the line from governance institutions to development performance: there is too much local and regional variation. In addition, the evolution of local or regional norms may be an understandable and predictable phenomenon, as analyzed in Ostrom (1990).

¹³ References can be found in Kotwal and Roy Chaudhuri (2015), as well as Singh (2007). Important contributors to this literature include Pranab Bardhan, Timothy Besley, Esther Duflo, Dilip Mookherjee, Rohini Pande, Vijayendra Rao, and many others.

Theories of Governance and Development: How Does India's Experience Fit?

Nirvikar Singh

Another illustration of this point is the work of Khan (2010), echoed in Roy (2013). Khan's framing is in terms of clientelist political settlements, whereas Roy uses the concept of LAOs. The analysis compares the experience of Maharashtra and West Bengal as examples of evolving LAOs. The Maharashtra case was particularly interesting, because it described how control of growing and refining sugar by the political elite in that state allowed them to provide space for industrialists to invest and prosper, at least relatively to the rest of the country. West Bengal, on the other hand, saw a different type of LAO, based on control of agriculture and stifling of industry. Khan, in particular, describes the details of political coalitions, rent-seeking, and the influence and interplay of caste and class in two states that were perhaps not dissimilar when India became independent, but diverged dramatically over the decades following. The complexity of comparing even these two subnational cases is illustrated by the differing geographies: in particular, West Bengal was separated from much of its former rural heartland due to Partition.

Sinha (2005) is also an important example of comparison across states. She chooses Gujarat, Tamil Nadu and West Bengal, and characterizes her framework as that of treating these cases as multilevel developmental states. It is difficult to distill the comparisons she makes as easily as the Maharashtra-West Bengal comparison in the last paragraph, but clearly the variations in social composition, and the distributions of political and economic power were important in driving the different development trajectories of the three states. Tamil Nadu's proximity to the software hub of Bangalore, even though it is in a neighboring state, also mattered.

A comparison that has not been systematically explored, but would also be useful, is that of the neighboring states of Punjab and Haryana, which were part of the same state until 1966, during the early part of the Green Revolution. Geography and social composition have mattered here as well, with Haryana having the good fortune to see the development of Gurgaon as an outsourcing hub, and to be spared the consequences of the violent religio-political conflict that has had continuing negative effects on Punjab's development. The emergence of a non-agricultural capitalist class in Haryana changed the room for how rents were created and captured in ways reminiscent of Maharashtra, while Punjab stagnated and fell prey to systemic, encompassing corruption without any meaningful political competition as a check (Singh, 2016).

Theories of Governance and Development: How Does India's Experience Fit?

Nirvikar Singh

These are not the only analyses and comparisons that can be made. Other states have been split and taken different trajectories. Some states are relative development failures, especially in the Hindi heartland of Northern India, but each in its own way. In other cases, states within this group have shown seemingly discontinuous changes in development trajectories. Whether an access order or political settlements framework is the best for considering these varied cases, or the developmental state or inclusive-extractive institutions models can do the job, is a question that can be resolved only by attempted application. The point to be made to close this section is that India's case is not paradoxical or contradictory – it is simply being tackled at the wrong level of aggregation for many, if not most, parts of the answer.

5. Conclusions

In this paper we have taken on the apparent paradox of India's failure to fit encompassing theories of the relationship between governance institutions and development outcomes. One part of the answer we provide is that beginning with the concept of the state, and its developmental or non-developmental characteristics may not be the ideal starting point. The theory of inclusive vs. extractive political and economic institutions may also not provide enough analytical sharpness for the case of India. Instead, theories (such as those based on different kinds of access orders or political settlements) that begin with societal groups and organizations (formal and informal) may be the appropriate primitives, allowing a better focus on the distribution of power that ultimately shapes outcomes. This ranking of theoretical approaches can still be debated. It must also be acknowledged that all four theoretical approaches discussed here do acknowledge the importance of underlying group formations and the inequalities inherent in them, but perhaps they do not bring these initial conditions into sharp enough focus, nor trace the links from inequality to institutional characteristics precisely enough.¹⁴

¹⁴ Bardhan (e.g., 1998 [1984], 2016), in his own analysis and in surveys of these issues, does bring out the importance of inequality in shaping institutions of collective action. Rajan (2009) and Acemoglu et al. (2011) both provide examples of formal models in which the inequality of initial conditions shapes the nature of institutions and the efficiency or inefficiency of economic outcomes. These kinds of models point the way to further analysis of India's case.

Theories of Governance and Development: How Does India's Experience Fit?

Nirvikar Singh

However, the deeper and more practical message of this paper is that the apparent contradictions or predictive failures of governance theories in the Indian context arise from a failure to consider India's subnational constituent units comparatively and systematically. We have provided some illustrative examples of what can be possible in such comparisons, and building up from a fuller set of these cases may be the way to understand how theories of governance institutions perform in explain India's development outcomes. A further complication, of course, is that India's states are not sovereign entities, but are influenced by, and influence the workings of the national government. This federal aspect of governance in India needs to be incorporated as well (Rao and Singh, 2005; Singh, 2007).¹⁵

We recognize that no theory will explain everything, nor should it. Yet it is useful to close by acknowledging that this paper has not considered all the historical and institutional factors that may be important in understanding the Indian experience, nor has it considered all the existing theories and their variations. Nevertheless, we hope it makes a contribution to the larger debates about an important topic, and stimulates further research along these lines.

¹⁵ For example, the models referenced in the previous footnote could be extended to allow for the interaction of different tiers of government, if they are to be useful in understanding the Indian development experience.

Theories of Governance and Development: How Does India's Experience Fit?

Nirvikar Singh

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Nirvikar Singh

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Nirvikar Singh

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Nirvikar Singh

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