



Notes on a Historical Institutional Approach to the IPG Agenda

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PREFATORY NOTE

Generally speaking the historical institutionalists whose work is discussed here have followed North in defining institutions as sets of rules, whether formal or informal, that actors follow and organisations as durable entities with formally recognised members, whose rules are also amongst the institutions of the political economy.

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1. WHAT IS 'HISTORICAL INSTITUTIONALISM'?

This term may be used to embrace both the considerable number of scholars described as the 'old institutional economists' and a significant body of research in comparative politics. The latter includes a lot of work on 'varieties of capitalism', showing the distinctiveness of the sets of institutions that characterise different advanced capitalist economies, and how they influence firms' strategies; and a significant literature on the characteristics of states in developing countries and their relations with trajectories of economic growth and distribution. What distinguishes all these scholars from mainstream economists working within the rational choice framework, who also study institutions, is that they question the basic assumptions of 'rational choice'. It has always been recognised that the model of human behaviour assumed in mainstream economics, that of individual preference maximisation – though it has given rise to some parsimonious and therefore powerful theory is a caricature. It does not account for a wide range of human behaviour – much of which is habitual – and what unites historical institutionalists is the view that the assumptions of rational choice are unduly confining. Essentially, historical institutionalists hold that 'unless something is known about the context, broad assumptions about 'self-interested behaviour' are empty' (Thelen and Steinmo 1992: 9) – and hence the 'historical' in their label, whereas the analysis of institutions within mainstream economics proceeds deductively, historical institutionalism works rather by an inductive logic, with much of the most outstanding research involving detailed historical comparison of relatively small numbers of cases. From such comparisons it is possible to develop a 'middle range theory', about the characteristics of state structures that are conducive to successful intervention in the economy for example, rather than attempt to generate grand empirical generalisations such as is the aim of a lot of research by mainstream economists.

2. WHY DOES HISTORICAL INSTITUTIONALISM MATTER FOR THE IPPG?

Several of the founding propositions of the IPPG imply that we will take an historical approach; we stated (citing, variously, research by Rodrik, Kenny and Williams, Hodgson and Bardhan):

1) there is widespread recognition of the limitations of both growth theory and cross-national econometric analysis for the understanding of growth, and the consequent need, given that economic growth 'concerns a complex of interlocking, circular and cumulative changes', for substantial historical analysis of 'the inner workings of actual economies', in addition to formal analysis [Kenny and Williams];

2) another reason for advocating the importance of history is that institutions are embedded in politics and in culture. The same formal institutions have different consequences in distinct political contexts – effective outcomes do not map onto unique institutional designs (Rodrik) and formal institutions, like legal rules, are always and necessarily embedded in deep, informal social structures, involving such factors as trust, duty and obligation, 'which have to be studied substantively' (Hodgson);

3) the history of the evolution of institutions (formal and informal) reflects the changing bargaining – power of different social groups. Societies may become 'locked' into institutions that are dysfunctional for large sections of their populations, because these institutions satisfy the interests of power-holders; but awareness of historical-path-dependence must be balanced in institutional analysis by recognition of the possibilities for human agency to bring about change (Bardhan).

Given these commitments, what can we learn from existing research within an historical institutionalist perspective and can we identify points of departure – researchable questions – for the IPPG programme?

3. HISTORICAL INSTITUTIONALIST PERSPECTIVES ON THE ORIGINS, REPRODUCTION AND CHANGE OF INSTITUTIONS

Historical institutionalism offers important insights into the over-arching questions that we have to address in the IPPG Programme, if we are to make progress towards the goal of identifying the institutional conditions that enhance the prospects for PPG, and of advising on how they may be encouraged in different contexts. These are the big questions of how the institutions that affect economic growth are established, sustained and changed, and (relatedly) what determines their effective functioning. I shall refer in particular to answers to these questions that are contained in Kathleen Thelen's recently published work on *How Institutions Evolve: The Political Economy of Skills in Germany, Britain, the United States and Japan* (2004).

3.1 Explanations of the origin of institutions that have been arrived at in much of the literature of the New Institutional Economics (NIE) (see Toye 1995) are functionalist, and use the following logic:

'This institution exists because it serves to reduce transactions costs and to facilitate cooperation. It was because of the need to reduce costs (etc) that these institutional arrangements were established.'

A more sophisticated version of the same reasoning holds that the institution exists because it serves the interests of a powerful group of actors. However, Thelen shows that – for instance – the institutions of skill-training in Germany, which have latterly served the interests both of employers and of unionised workers, go back to legislation passed in 1897 that protected and regulated artisanal producers. The

legislation was passed for political reasons; the authoritarian government of the period sought to shore up artisanal production as a way of stemming the mounting strength of unionised labour, the legislation was actually opposed, at that time, by the unions and by the Social Democratic Party and where it was not opposed by them, it was also not driven forward by large skill-intensive firms. However, over time, as a result of negotiation and struggle between capital, unionised labour and artisans, an institution originally set up to serve one purpose, came to serve other interests. The point is that there is not necessarily a direct connection between the later/current effects of an institution and the original intentions that lay behind its establishment; and methodologically, there seems to be no substitute for substantive historical analysis.

3.2 This particular case-study also helps to make a nice point, about the notion of 'path dependence' – the argument that once an institution has become established powerful actors will have an interest in its reproduction in order that it becomes more or less 'locked' or frozen in time. This is an argument that is compatible with the common view that institutional change is generally of the kind described in evolutionary theory as that of 'punctuated equilibrium' – long periods of stable equilibrium, punctuated by changes brought about by exogenous factors. The case of the German system of skill-training, however, shows that while the 1897 legislation exercised a long-running influence, which helps to account, for instance, for the major differences between Germany and Britain (where a stable system of apprenticeship failed to develop and institutions concerned with skill training have often not functioned very well at all), it did not remain unchanged; rather was there a series of incremental changes coming about as a result of political realignments amongst the social groups concerned, and renegotiation of the implicit or explicit settlement between them – such as happened in Germany in the 1920s. Thelen sums up: '*institutions are the object of on-going political contestation, and changes in the political coalitions on which institutions rest are what drive changes in the form institutions take and the functions they perform in politics and society*' (2004: 31, my emphasis). Thelen's is a power-distributional argument, such as Acemoglu, Johnson and Robinson (2004) sketch in their model, but she is critical of those power-distributional perspectives 'that view institutions as straightforward reflections of the interests of the powerful, and thus as responding automatically to changes in the balance of power or the preferences of the powerful' (2004: 33). As the case of the 1897 legislation in Germany shows, institutions designed to serve one set of interests often become 'carriers' of others as well – and institutions inevitably have unintended, as well as intended, effects. They are not necessarily – indeed, probably rather rarely are they – 'reflections of the interests of the powerful', but must rather be seen as the objects of ongoing political negotiation; institutional evolution and change is the outcome of such negotiation between contending actors.

4. HISTORICAL INSTITUTIONALIST PERSPECTIVES ON STATES AND DEVELOPMENT

4.1 The substantial historical and comparative literature on patterns of state authority and organisation, and development includes, notably, the seminal study by Chalmers Johnson (1982) on the positive role played by the state in Japan's very rapid economic development – which gave rise to the concept of the developmental state; and work by Amsden on South Korea (1989) and by Wade on the East Asian model of development, grounded in an analysis of industrial policy in Taiwan (1990). Their research contributed to the retreat, in the 1990s, on the part of the World Bank and of other development agencies, from the more extreme forms of criticism of the role of the state in development that had been very powerful in the 1980s. Essentially what they shown is that in countries that have sustained notably high rates of economic growth, there are states that have intervened in the economy, both to provide incentives to private capital and to discipline it, often by ensuring that firms are subjected to the disciplines of market competition; these are states that are 'capital friendly' without necessarily being 'market friendly'. What these writers, and others have also pointed out, was that these successful 'developmental states' have common characteristics, grounded in their particular histories, that are not shared by the majority of developing countries. They are states with leaders who are dedicated to economic development as a national goal, usually in the interests of national security, and legitimated through a nationalist vision; they are states with economic bureaucracies staffed by relatively small groups of highly competent people, insulated from the pressures of special interests, and able to intervene selectively in the economy in a 'market-conforming' way to mould the incentives of private producers; their bureaucracies, at lower levels, function sufficiently well to implement the instructions of the political leadership and the senior bureaucrats; they are states that have the capacity of repressing popular protest; and they are states in which there is a consistently high level of co-operation with private sector business.

4.2 In recently published work on *State-Directed Development: political power and industrialisation in the global periphery* (2004) (based on comparative historical analysis of the development experiences of South Korea, Brazil, India and Nigeria), Atul Kohli has compared the characteristics of the successful 'late late-developers' – such as South Korea and Taiwan – with those of other developing countries, developing a useful typology, the basic argument is that state intervention in support of investor profits has proven to be a pre-condition for industry to emerge and flourish among late-late developers, and that the states that have done this effectively are '*cohesive capitalist states*': they are 'characterised by centralised and purposive authority structures that often penetrate deep into society. For a variety of historical

reasons these states have tended to equate economic growth with national security and thus defined it as a priority... (in particular these states have a...) close alliance with producer or capitalist groups. An important corollary of this political arrangement is a tight control over labour. The main political instrument of these states is... a competent bureaucracy' (2004: 10). They are states, that in fact, have many of the features of the European fascist regimes of the 1930s. A very large number of developing country states, however, are much more like Nigeria which is an instance of the type of '*neo-patrimonial states*'¹ (with which Bangladesh corresponds pretty well), in which the distinction between public and private realms is not well established, they are characterised by 'weakly centralised and barely legitimate authority structures, personalistic leaders unconstrained by norms or institutions, and bureaucracies of poor quality... despite the façade of a modern state, public office-holders tend to treat public resources as their personal patrimony...' (2004: 9); they 'fail to promote development mainly because of their failure to generate purposive power' (2004: 393). A third type, of which India is the example, is that of the '*fragmented multi-class state*'; these '...are real modern states. They command authority, and a public arena within them is often well enough established that leaders are held accountable for poor public policies and performance... (but)... public authority in these states tends to be... fragmented and to rest on a (broad) class alliance – meaning that these states are not in a position to define their goals as narrowly or to pursue them as effectively as are cohesive-capitalist states (exemplified by South Korea)]' (2004: 11). In spite of the undoubted commitment of India's leadership after independence to goals of national development through industrialisation, its capacity to implement these intentions was always compromised by the need to accommodate different interests and social groups (and Kohli notes that the regime that has been most successful in securing high rates of economic growth, at the turn of the century, deployed an ideology of religious nationalism in mobilising support and dispelling opposition).

4.3 What accounts for such different experiences of state formation as are reflected in this typology? According to Kohli 'colonialism has proved to be a major architectonic force' (2004: 417); and he shows that the legacies of Japanese colonialism in South Korea and of the differing practices of British colonial rule in India and Nigeria, have been profound. He argues that 'it is probably difficult to create... a pattern of state-led development (such as that of South Korea) in a developing country without a decisive external input' (2004: 388). In the case of Brazil, he suggests, such an external input came in the 1930s from the model of the German and Italian fascist movements. Implicitly, therefore, the model of institutional change that he offers is one of 'punctuated equilibrium', colonial (or other) intervention reshaped institutional structures in colonised territories (a moment of 'punctuation'), and these structures 'are made to endure because the powerful in society discover that they can pursue their own interests through them' (2004: 411). The equilibrium that was established is hard to break – though it *may* have been, as say in the case of India in the 1990s, the outcome of the long and complex history of political negotiation and struggle that led to the emergence of the Bharatiya Janata Party (BJP) as the party of government.

4.4 There is no doubt that the work of Kohli, and of the other writers whose work has been referred to (and of others still – reviewed in Kohli 2002), has contributed greatly to the understanding of how and why some developing countries have experienced very high rates of economic growth, while others have stagnated. It leaves unanswered questions, however, as well as presenting an apparently bleak prospect for the majority of developing countries that have not had the 'advantages' (and the disadvantages, it should be said) of a history that has made them into 'cohesive capitalist states': questions about the origins, functioning and especially the change of institutions. As I have suggested, Kohli's theory about institutional change seems to be one of 'punctuated equilibrium'; yet some of his own material actually suggests a more subtle and nuanced analysis. In the Indian case, for example, he stresses the importance of the shift in attitude on the part of India's leaders towards private capital in the 1980s, well before the celebrated economic reforms from 1991. His account recognises the significance of ideas and, in this case, of the existence of an influential intellectual critique of India's development policies over the first thirty years or so of independence. He also refers to the possibility that there was a real regime change in the late 1990s – if he had extended his comparative net (practically difficult though this would have been) he might have found examples of countries that have experienced comparable or possibly even more marked changes of trajectory (such, perhaps, as Indonesia or Thailand). In other words, whilst appreciating the achievements of comparative research, like Kohli's, on developing country states, it seems that it can be further advanced through finer-grained research on institutions and the ways in which they are changed through on-going political contestation, in which ideas have a part to play. There is a further, emerging literature on this, that draws on research examining the relationships between ideas, interests and institutions in explaining policy choices and their implementation, with a focus on the gaps between policies and implementation. An interesting study by Poteete (2003) explains how and why it has come about in Botswana that policy on rangeland privatisation has remained unchanged, in spite of the changing ideas of rangeland specialists, while the implementation of policy has changed, because of the way in which institutions are involved in the implementation of rangeland policy function. It is another

1. There is a very large literature on neo-patrimonial states in Africa. A fairly recent summary is found in Bratton and van de Walle (1997).

case, incidentally, in which it is clear that the elementary power-distributional model of institutional formation and change is far too 'clunky' to be of high explanatory power.

4.5 There is also, in the existing literature, one rather finer-grained analysis of patterns of state authority and organisation, and development, for example Peter Evans' *Embedded Autonomy: states and industrial transformation* (1995) tells us a little bit more about the institutional factors that have influenced successful industrialisation. The oxymoron of 'embedded autonomy' refers to his finding, from a comparative study of the electronics industries of Korea, Brazil and India, that in the more successful case of Korea, the state bureaucracy concerned with fostering a new industry was characterised by both autonomy in relation to particular pressures from within society, whilst also being well 'embedded' in policy networks within private capital. It was capable of being entrepreneurial, while in both India and Brazil, though the parts of the bureaucracy that were concerned with industry were neither corrupt nor characterised by venal rent-seeking – as might have been expected in the light of neo-liberal theory about the state – they were not embedded in policy networks with private industry in the way that was true of Korea; there were problems in Brazil to do with the fragmentation (or 'segmentation' in the words of one author) of the bureaucracy, and in India with the relations of bureaucrats and businessmen and their influence on the approaches taken to fostering the new industry. Evans refers to points of detail in all cases, highlighting such questions as the availability of technical capability and the institutions supplying it – in all cases the quality of bureaucracy, influenced particularly by whether or not recruitment and promotion are meritocratic, was significant. In later research Evans and his co-author James Rauch (1999) showed that 'several relatively simple, easily identifiable, structural features constitute the key ingredients of effective state bureaucracies'; of these, meritocratic recruitment was shown to be the most important, followed by internal promotion and career stability (the importance of competitive salaries not being clearly established). It is unfortunate that the benefits of meritocratic recruitment can easily be undercut by the operation of political preference.

4.6 Another significant recent contribution to the literature on the role of the state and of institutions in industrial development is Aseema Sinha's *The Regional Roots of Developmental Politics in India: A Divided Leviathan* (2005); which this addresses a question which is relevant in other polities as well, including those which are not federal states like India: what accounts for marked regional variations in development performance (in this case, variations in the performance of three of India's regional states: Gujarat, West Bengal and Tamil Nadu, which were comparably relatively well-developed at the time of independence) even in the context of uniform central policy interventions? Sinha's emphasis on variation within a single state is a valuable corrective to Kohli's (and others') focus on the national level and it perhaps shows the existence of at least a little 'room for manoeuvre', and for institutional innovation, in a way that Kohli's analysis rather precludes – though it is a possibility that figures strongly in his earlier writing (1987). The particular problem for which Sinha seeks an answer is that of why Gujarat has consistently attracted a higher share of all-India investment over time whilst West Bengal has with comparable consistency, failed to capitalise on its relatively favourable initial conditions; and Tamil Nadu has seen a fluctuating performance – investment per capita in Gujarat was two and half times the all-India average by 1994, while in Tamil Nadu such investment stood at 0.85 of all-India levels, and in West Bengal at 0.47. Sinha's answer is that it was as a result of local regional politics in Gujarat that the regional state there developed 'strategic capacities' that have consistently made that state attractive to private capital – so here her argument is entirely in line with Kohli's – while in West Bengal and Tamil Nadu local politics have tended (at least until recently) to have the opposite effect. The politics of Gujarat, where there is no strong sub-nationalist sentiment or regional party – unlike in the other two states – have made for an 'integrationist' policy in regard to the central government and regional (state-level) institutions have been created that have, in a sense, worked with the grain of central government in such a way as to reduce uncertainty for investors, to improve flows of information to and from them, and to ensure credibility. In West Bengal and Tamil Nadu, on the other hand, the electorates have rewarded anti-centre actions and so anti-centre political strategies have emerged in both states, which in the perceptions of private capitalists policies, have lacked consistency – partly because of the friction with the centre (see Sinha 2005: 227). Sinha's argument, therefore, posits that regional elites bargain with central rulers over the extent and terms of state intervention; and she proposes that 'the national framework that governs industrial change in India (is) a combined product of central rules of the game, sub-national strategic choices, and regional institutional variation.² Regional politics, a product of sub-nationalist movements and electoral compulsions, emerges as a salient factor explaining both centre-regional and horizontal interactions' (2005: 5).

5. PERSPECTIVES ON STATES AND PRO-POOR GROWTH

5.1 There is rather less in the comparative historical literature on the political and institutional determinants of specifically pro-poor growth. The state's distributive role has received attention particularly

2. Chapters 5 and 7 in Sinha's book provide quite rich documentation of the institutional frameworks for industrial development in each of the states, drawing attention – for instance – to the significance of the role of Industrial Extension Bureau (iNDEXTb) in Gujarat.

in regard to Brazil – one of, if not the most unequal countries in the world – and in India; Weyland (1996) ‘analyses why even democratic governments in Brazil have not been able to pursue policies that may benefit the lower classes’. His answer focuses on the organisational fragmentation in Brazil, including the absence of well-organised political parties that could aggregate the interests of the poor, as well as fragmentation within the state (cf. Evans, above 4.5, on bureaucratic fragmentation) and the resulting weakened capacity of state elites to push against the interests of dominant classes... democratisation does not necessarily translate into enhanced political power for the poor; the latter requires deliberate re-crafting of political parties and of the state apparatus’ (Kohli’s account of this work, 2002: 108). It is possible that since the time of Weyland’s research the ascendance of the comparatively well-organised Workers’ Party in Brazil – the PT – which connects up with some effective mobilisation of the rural and urban poor in the country, has brought about greater concern for conditions of poverty and inequality.³

5.2 Work in India, comparing the performance of different Indian states in reducing poverty again by Kohli again (1987) and myself (2003) and research by Heller on Kerala (1996), is informative and corresponds with the recent indications of change in Brazil associated with the rise of the PT. This research on India also connects up very nicely with the comparative analysis of the poverty-elasticity of growth across Indian states by Besley, Burgess and Esteve-Volart (2004) in the OPPG study of India. They found the highest elasticities in Kerala and West Bengal, which is what Kohli and I would have predicted; we found the ideology and organisation of ruling parties, and the social base of the parties’ power to be key variables explaining distributive outcomes. Kohli concluded that a well-organised social democratic party in power with a middle- and lower-class base (like the CPM in West Bengal) was the most successful at prioritising and pursuing anti-poverty policies; my own work suggested that even relatively well-organised parties of a more populist character, when they are based on long-standing political mobilisation on the part of lower castes and classes, can be successful in poverty-alleviation; but both analyses (and they are echoed by Besley, Burgess and Esteve-Volart) highlight the cases of Kerala and West Bengal. The interesting question, worthy of further analysis, is that of how and why it is that one or two states without the characteristics identified by Kohli and me – notably Punjab – have also performed well in regard to the poverty elasticity of growth.

5.3 The Kerala case-study, now well known because of the attention paid to it by Amartya Sen, is of a state that has done extraordinarily well in regard to poverty reduction and improvement in human development, even in a context of low growth; as Evans has argued – drawing on Heller’s research – Kerala appears to represent another instance of ‘embedded autonomy’, but in this case the critical relationship is not between a relatively autonomous state machinery and industrial capital, but rather between the state and historically mobilised working classes; it should be noted, however, that not all scholars share this judgement on Kerala (see Tornquist, e.g 2004)

5.4 One of the lessons of all this research is that of the political importance of the organisation of subordinate groups as a bulwark against the degeneration of state-society connections to elite clientelism, but this must seem rather like ‘assuming the tin-opener’ (thinking of the parable about the economist, amongst different specialists’ reactions when they are marooned on a desert island, considering the cans of food that constitute their possible means of survival). Public sector reform, difficult though it is to bring about, is probably more tractable than the problem of establishing an effectively functioning party system. Which brings me to ...

6. INSTITUTIONAL ANALYSIS OF ‘THE TYPICAL DEVELOPING COUNTRY’

6.1 Thus far I have referred, in particular, to work on the most successful developing economies, and to the intermediate cases of Brazil and India; while making the point that the great majority of developing countries, and certainly those of Africa, have very different political and organisational characteristics. It is probably fair to say – as Mushtaq Khan has done (2005) – that a ‘typical developing country’ is actually what Kohli and others describe as a ‘neo-patrimonial’ state, with the characteristics briefly outlined above. ‘Patrimonialism’ is a concept elaborated by Max Weber, in opposition to the idea of rational-legal authority, to refer to political systems in which an individual rules by dint of personal prestige and power: ‘Authority is entirely personalised, shaped by the ruler’s preferences rather than a codified system of laws. The ruler ensures the political stability of the regime and personal political survival by providing a zone of security in an uncertain environment and by selectively distributing favours and material benefits to loyal followers who are not citizens of the polity so much as the ruler’s clients’ (Bratton and van de Walle 1997: 61). This is a model of a form of authority that corresponds well with what was observed historically in Africa and elsewhere, yet it is also clear that very similar features are found in many modern political regimes, especially in sub-Saharan Africa – where this patrimonial logic is incorporated into bureaucratic

3. Even if President Lula’s PT government has not proven as effective as some hoped in bringing about redistribution and poverty reduction, it has had some success. One poverty index records a modest fall in the incidence of poverty from 27.3 per cent of the population in 2003 to 25.1 in 2004; and it is said that inequality is now at its lowest level in the past thirty years, and still falling. See *The Economist* 4 March 2006, p.58.

institutions that should be operating according to the logic of rational-legal authority: hence the idea of 'neo-patrimonialism'. 'Neo-patrimonialism' actually undermines formal bureaucratic institutions, but is itself institutionalised, providing 'essential operating codes for politics that are valued, recurring and reproduced over time' (Bratton and van de Walle 1997: 63). Informal political institutions that have typically been stable and predictable in Africa's neo-patrimonial regimes are those of presidentialism and clientelism which may be embedded – according to Chabal and Deloz (1999) – in distinctive values, thus it is possible that while petty corruption is despised, the much more significant abuse of power by 'big men' may be seen as serving larger and legitimate 'moral' purposes. Ostentatious displays perhaps demonstrate the reality – the substance of the power of the 'big man' - for power, though it is usually understood by social scientists as a relational concept, may be seen in some societies as a kind of a substance that some individuals actually possess (e.g Anderson on the concept of power in Java, and Sukarno: 1990[1972]).

6.2 The 'typical developing country' then, operates not according to Weberian rational-bureaucratic principles, but in terms of personalised exchanges between 'rulers' at different levels of the pyramidal hierarchies of their factions. Bureaucratic rules are regularly broken and private interests are deeply imbedded in the public sphere represented by the bureaucracy: the question is why should this be so? The neo-Weberian argument holds that the neo-patrimonialism of a state, like that of Bangladesh for instance, is not based on traditional structures and ideas of legitimacy – as some scholars have suggested (though the point is clearly arguable) – but rather on modern forms of exchange between patrons and clients. The argument then is that neo-patrimonialism will be undermined by democratisation, which will help to consolidate rational-bureaucratic forms of rule and so assist the deepening of rational capitalism; the problem with this argument, Khan says, is that there is abundant evidence showing that democratisation (as in Bangladesh) does not undermine the dominance of patron-clientelism: neither democracy nor authoritarianism radically affect the personalised structures of patronage, but only modify the ways they work.

6.3 Khan's own argument is that it is economic factors that ensure that (the informal institutions of) patron-client relations are rational for both leaders and organisationally powerful constituencies in developing countries. His reasoning is (quoting him closely): whereas in advanced capitalist economies the welfare of most people depends upon maintaining the viability of the capitalist economy, so that democratic politics remains broadly pro-capitalist, in poor countries capitalism is not yet dominant and at least over the short-run decline in the capitalist sector will not affect most people. There are not the same incentives, therefore, for the establishment of broad coalitions. Second, and relatedly, in advanced capitalist economies the fisc is relatively large and politically active groups can address their redistributive concerns by seeking to influence or control the significant fiscal budget. Redistributive agendas in these circumstances are likely to be organised around broadly-based organisations representing the interests of large groups of individuals. By contrast in poor developing countries the fisc is small, and there are not the same incentives to organise around large and broadly-based organisations; the budget is not usually the primary focus of redistributive political activity. Political entrepreneurs (likely to come from the 'middle class' of 'intermediate classes' – educated people with college and university education, the petty bourgeoisie and richer farmers) who want to achieve redistribution in their favour have to look for other strategies.

'The only viable redistributive strategy for (such) developing country political entrepreneurs in the absence of any fiscal or regulatory space is to organise enough muscle to be able to capture resources through a combination of fiscal, off-budget and even illegal means... (and)... factional membership and activity is rational at all levels of the faction since the pay-offs available from the faction are always higher than those available from alternative types of political activity given the non-existence of budgetary resources.' (2005: 719)

Finally, whereas in advanced capitalist countries, because people can afford to pay for protection there are secure property rights, this is not true in poor developing countries; developing countries have weak protection for property rights, which opens a wide range of assets and activities to attempts at expropriation. Faction membership – participation in clientelist networks – may be the only way in these circumstances, of securing or trying to secure property rights. The conclusion is then that 'the economic structures of developing countries create strong incentives for the emergence of the very phenomenon that neo-Weberians identify as the variable explaining underdevelopment, namely the proliferation of patron-client networks and the domination of personalised politics'; and Khan suggests that to argue that neo-patrimonialism has to be addressed first – perhaps through democratisation – in order to assist development, sets developing countries an impossible target.

6.4 These arguments are extremely interesting and suggest *two points of great general importance* the first is that the argument that the dominance of personalised relations perhaps follows from the existence of social and cultural institutions that – like those in Bangladesh (according to Kochanek [2003] and others) – stress obedience towards 'highers' and indulgence on the part of these towards 'lowers', overplays the importance of culture in determining action. The second, of even greater significance, is

that institutional change (here from neo-patrimonial to rational-bureaucratic structures) follows from, rather than being the cause of, economic development. There is support for this view in certain of Bratton and van de Walle's findings about the transitions from neo-patrimonial to democratic regimes in Africa, amongst which they note that emergent middle classes, interested in more secure property rights, 'often take on political leadership roles in the opposition' to the incumbent neo-patrimonial regimes (1997; but the question then arises of how and why economic development comes about!

6.5 At least part of an answer to this question comes from a recent ODI paper (2005), about Bangladesh, which argues that (though) 'growth-oriented policies, functional institutions and probity in governance contribute powerfully to growth,... even major imperfections in aspects of these [as in Bangladesh] need not prevent growth at least for a time'. The same paper notes that 'Perceptions about the direction of change of policies and governance are important. In Bangladesh there was a growing belief, at least among indigenous entrepreneurs that policies were becoming more credible and that the environment was slowly improving. Savings and investment picked up'. This point recalls Atul Kohli's argument, referred to earlier, that the crucial changes in India's economic development were not to do with the economic reforms of the early 1990s (involving of course, lots of institutional change), but rather with the radical shift in the ideas and attitudes of the political leadership in the 1980s, towards the private sector.⁴ The point (which was made earlier) is: there is perhaps just a little more 'room for manoeuvre', even in poor countries with neo-patrimonial regimes, than an initial reading of some of the literature on state structures and economic development might lead one to suppose. In relation to the long-run objectives of pro-poor growth there are strong reasons for seeking to bring about changes in ideas and attitudes both towards private business, and *on the part of* private business; especially as they may be articulated through business associations, as well as working for changes in restrictive institutions, in order to help to bring about economic development such that weakens neo-patrimonialism, and ultimately helps to create conditions, for subordinate groups also, that is conducive to PPG (as observed in the research referred to in section 5 above).

7. LOCAL AND REGIONAL 'POLITICAL TOPOGRAPHIES' AND THE PROSPECTS FOR PPG

7.1 Most of the literature considered so far, with the exception of that on the comparison of the growth performances and poverty reducing experiences of different Indian states, deals with national states, but there is also important work that probes below the level of the nation state – including Catherine Boone's work on Political Topographies of the African State (2003). Boone's central argument is that there are geographically uneven patterns of state-building, even within a single country, that may be understood as the outcomes of regional variations in agrarian socio-political organisation and rural modes of production: 'Different configurations of rural authority have consequences for modern state-building strategies' (2003: 2) and shape possibilities for economic development, for decentralised democracy, and for the sustaining of the territorial integrity of post-colonial states in Africa. Conceptually Boone's approach, though her work refers to the very different context of West Africa, is very much like that of Kathleen Thelen, writing about Germany (to which I referred earlier): 'Institution building is viewed as a contested process, driven forward and undone by struggles (in this case) between regimes, rural elites and farming populations' (2003: 7) – and she aims to explain variation in the course and outcomes of these contests: 'In efforts to tax and govern peasantries, why did regimes' institution-building strategies differ?' (2003: 7). Boone's analysis provides an interesting example of how institution-building may be explained in relation to sociological parameters; it reminds us of the possible importance of regional variation even within the same state – and in doing so it suggests a possible basis for comparative research on institutional explanations for variations in the pro-poorness of growth.

7.2 Boone's analysis finds explanations of institutional choices and outcomes in a sociological framework, taking account of class and communal structure, and modes of production. Two key propositions are:

1) 'the greater rural elites' reliance on the market as a mechanism of surplus extraction and labour control, the greater their potential for political independence vis-a-vis the state and thus for confrontation with regimes bent on centralising power' (2003: 23) – while, on the other hand, 'rural elites which did not appropriate their own share of the surplus directly, relying instead on state intermediation, would be interested in aligning with new regimes' (2003: 29)

2) variations across West Africa 'in the extent of communal hierarchy determined notables' political clout in their dealings with governments' (2003: 27)

Where the two factors, of market-based surplus-extraction and strong social hierarchy, coincide then it may be expected that local elites will constitute strong rivals to state control; where neither factor figures then local elites will only be weak allies of the state. Boone's model for explaining variations in local 'political topographies' (or, more exactly 'patterns of spatial de-concentration of the state apparatus and the extent of devolution of authority to chiefs and other rural elites'), even within the same state (holding

4. See also Sinha's analysis of the immediate factors involved in accounting for differences in the growth performances of three Indian states (2005).

to the same administrative doctrine), proceeds according to the following logic:

1) In zones that are *not* cash-crop producing the state has little interest in penetrating the locality at all, this is the case of 'non-incorporation', in which local elites are left to wield authority.

2) In zones that are cash-crop producing, but which lack strong social hierarchy, then authority will be exercised by the central state, in a pattern labelled 'administrative occupation'.

3) In zones that are cash-crop producing, characterised by strong social hierarchy and in which rural elites rely economically upon the intermediation of the state, there will be 'power-sharing' locally between state and elites.

4) In zones that are cash-crop producing, characterised by strong social hierarchy, but in which rural elites are able to accumulate without relying on the state, the state is likely to try for 'usurpation' (of local power).

Boone shows how differences within West African states, in terms of the ways in which centre and periphery are linked, can be explained in terms of this model; and how these differences influence responses to the recent drive for democratic decentralisation. In cases of 'usurpation' and 'administrative occupation' regimes are likely to resist decentralisation, only in the case of 'power-sharing' are they likely very readily to acquiesce – but in these cases the effect of democratic decentralisation will probably be the further empowerment of the existing elite. The implications of variations in 'political topography' of the kind that Boone analyses, for pro-poor growth, have to be examined.

RESEARCH QUESTIONS

1. State-business relationships

Much of the historical institutionalist literature points to the vital importance of the relationships between the state and private business. The broad question is therefore: how does the character of the relationships between the state and private business vary across different developing countries, why, and what of it?

This question can be broken down into a number of more specific questions:

1) How is private business organised [business associations, chambers of commerce etc], across different sectors, and with what ideas and objectives? How much co-ordination is there between businesses in the same sectors, and across sectors? How are conflicts (e.g. between textile manufacturers and RMG manufacturers in a country like Bangladesh) managed? What are the relationships between individual businessmen, business groups and business associations with politicians?

2) How, on the other hand, is government organised in relation to business? Again, how much co-ordination is there? Is the bureaucracy relatively fragmented/segmented? Is there bureaucratic capacity to deal with technical questions? How is the capacity of the bureaucracy influenced by the institutions of recruitment, promotion and career development? What are the relationships between (different sections of) the bureaucracy and politicians? What are institutional mechanisms through which bureaucrats and business come together? Are there functioning policy networks? Is there, or what are the, prospects for the formulation of industrial policy? Or for 'industrial districts' (see Farrell and Knight 2003)?

3) What is the influence of party political competition on all of these questions?

4) What is the influence of ideas (and of the institutions that develop and propagate them)?

There is an important research agenda here, and a number of the research questions (or 'hypotheses') identified by Paul Hare from his parallel review of the economic literature, might usefully be pursued in connection with this substantive agenda:

H2 Policy protecting incumbent firms

H3 Competition policy

H5 Policy governing engagement with the world economy

H7 Costs and complexity of doing business

H11 Influence of external relations

Here there is a substantial research programme that may be pursued through comparative research across a small number of countries; possible countries for comparison: Senegal, Kenya, Botswana/Mauritius, Bangladesh, Thailand, Vietnam?

2. Comparative research on neo-patrimonial regimes

Khan also argues that 'factional conflicts and contests over resources do not necessarily have to lead to either economic stagnation or the breakdown of democracy. In some cases democracy may be a viable mechanism of managing these conflicts, and factional conflicts can drive economic growth in some contexts ...' (2005: 705). Yet he does not, I think, embark on analysis or explanation of why

these differences should be so (indeed he says: 'The economic outcomes of democratic management of factional conflicts are... not well defined without *further examination of the specific patron-client networks and factions that are involved in particular countries*'[my emphasis], perhaps implying a view that there is scope for 'analytical deduction' from comparative case studies).

Possible research questions (to be addressed by means of analytical deduction from comparative case studies) might include:

1) How does neo-patrimonial/patronage politics work in practice (recognising the existence of different forms of these politics: see Khan 2000)?

This may be very difficult to research, because what is involved must often be of questionable legality, if it is not frankly illegal, yet it seems important to try, since much of the foregoing is based quite heavily on surmise. The existence in Bangladesh of the Citizens' Forum Against Corruption, which is said to involve quite a number of ex-bureaucrats, perhaps provides an opportunity; the IPPG team might volunteer support for the Forum (and similar organisations elsewhere) and in exchange actually learn a lot in detail about what goes on.⁵

2) In what circumstances, and why, is democracy a viable mechanism for managing factional conflicts and contests over resources (that constrain the capacity of the state to deliver to poor people); and, similarly, in what circumstances, and why, can factional conflicts drive economic growth?

3) In what circumstances, and why, have growth episodes occurred even in the presence of the corruption, rent-seeking and factional conflicts characteristic of the 'typical developing country'?⁶ In what circumstances, and why, have broad coalitions or class-based political parties emerged to overcome the effects of factional/patron-client politics?

4) How, why and in what circumstances do sections of the business community put pressure on state agencies to improve their performance?

Comparative cases, to be examined historically might include, in Asia: Thailand vs Indonesia vs Vietnam vs Bangladesh vs Pakistan vs Sri Lanka (not all of them); in Latin America: perhaps a comparison of Bolivia with Peru and/or Ecuador; suitable comparisons also need to be identified in Africa.

3. When and why does bureaucracy function well enough?

This is a research theme that is implicit or explicit in the first two questions described here; I say 'function well enough' mindful of Grindle's sensible argument (2004) that the 'good governance' agenda is impossibly vast and must be prioritised by asking what is necessary for governance that is 'good enough'. Grindle also makes the good point that so much of the literature on development is about why government does not work very well, and that there is a lot to be said instead, for studying cases of relative success (like Tendler in Ceara: 1997), but can we find some?

4. Comparative research on states/regions with high poverty elasticities of growth

At present this is an India-specific project, but the same principles might be applied elsewhere. The point is that there are substantial differences in terms of PPG between Indian states; some explanations for these differences have been advanced by Besley, Burgess and Volart, Kohli and myself (amongst others) and I have indicated the possible value in more detailed comparisons both between successful PPG states (Kerala, West Bengal and Punjab) and between successful and relatively unsuccessful PPG states (East/Indian Punjab and West/Pakistan Punjab? Or Punjab and Gujarat or Maharashtra).

The same principles might be applied elsewhere – in the context of Africa perhaps using, as a framework for analysis, Boone's work on the distinct 'political topographies' that obtain even within individual African states.

A SET OF PEGS FOR IPPG RESEARCH

Following from the above, and from reports on the exploratory country studies, I believe that we may have a set of three 'pegs' around which our research programme can be structured:

1. Institutions and making markets work more effectively

This is what much of the NIE is really about and what is meant when it is said in policy discourses that 'institutions are the answer' – we have a good number of possible research lines, those identified by Paul Hare and in some country studies (e.g the Tanzania study), that are in this vein of work.

5. Thanks to Adrian Leftwich for suggesting this possibility.

6. See, for example, Sinha's work; she comments 'In India, particularistic relations did exist between some business people and the national bureaucracy. However, at the state level – in some states and in some periods – these same modes of clientelistic interaction facilitated complementarities between high growth outcomes and state actions, and encouraged competition rather than monopolistic rent-seeking'. (2005: 231)

2. Institutions and allocating resources more effectively

Not necessarily distinct from 'peg 1' – given that though the interventions of the successful developmental states have sometimes been concerned with making markets work effectively, but they have also been concerned with allocating resources by non-market means. Research themes around industrial policy are referred to in my own suggestions under 1 (and 2) above.

3. Institutions and governing more effectively

Comparative work on neo-patrimonialism, on when bureaucracies work well enough, and on how and why some states/regions are so much better than others at PPG.

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