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The political economy of development: an assessment

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Abstract Research in the field of economic development is increasingly engaged with questions of political economy, of how political choices, institutional structures, and forms of governance influence the economic choices made by governments and citizens. We summarize recent developments in the field and introduce a set of papers that illustrate key themes and methodological innovations associated with the 'new' political economy of development.

Key words: political economy, economic development

JEL classification: D70, O10

I. Introduction

Why did economic growth in the East Asian tiger economies so dramatically outstrip that in most South Asian and African economies in the second half of the twentieth century? What explains the recent resurgence of the economies of Latin America? Why have some resource-abundant economies, such as Botswana and Norway, been able to manage their endowment successfully while others have so manifestly failed to reap the same benefits? Why do some states fail and why does it appear to be so hard for many such states to escape from failure? What lessons can be learnt from these experiences, both positive and negative, to inform policy in other regions and countries? These are huge questions and it is no surprise that understanding the patterns and processes of economic development across the world, of why some countries grow while others stagnate, and drawing relevant lessons for policy remains one of the enduring preoccupations of economics.

In the almost 20 years since Robert Barro's ground-breaking paper on cross-country patterns of growth (Barro, 1991) launched the vast research programme on the empirics of

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growth, perhaps only one broad conclusion emerging from the wealth of growth regression results commands universal support. This is that 'institutions matter' for growth and development and that they matter decisively (see, for example, Hall and Jones (1999) and Easterly and Levine (2003) and, especially, Rodrik *et al*. (2004)). The implication is that to understand the historical and spatial patterns of growth and development it is necessary to understand the role and functioning of the 'deep' determinants of development, those institutional or political factors that ultimately shape the proximate determinants of growth: factor accumulation, technology adoption, and policy choices.

Research on economic development has thus become increasingly engaged with questions of political economy and, in particular, with how political choices, institutional structures, and forms of governance influence the economic choices made by governments and citizens, and how, in turn, these structures reflect deeper forces, such as the patterns of colonial settlement and conflict, physical geography and natural resource endowments, the disease ecology of societies, and ethnic diversity, as well as a host of other cultural factors. While the ultimate concern is how these choices shape patterns of economic development—which may be taken to mean the enjoyment of a range of human freedoms including the freedom from disease, hunger, and economic want, the freedom from insecurity of person and property, from political or religious tyranny, and the positive freedoms of thought, cultural expression, and the enjoyment of leisure (Sen, 1999)—many of the deep issues associated with development are firmly rooted in the narrower economic challenge of promoting and sustaining high and inclusive economic growth.[1] It is this growth of incomes that allows individuals and societies to enjoy and afford the freedoms that Sen describes. The political economy of development is thus inextricably tied to the narrower notion of the political economy of economic growth. It is with this that the papers in this issue of the *Oxford Review of Economic Policy* are concerned. As we note below, the papers collected here represent an eclectic set of contributions to the field chosen to illustrate the scope of contemporary research into the political economy of development and to highlight how the methods of modern economics are being used to deepen our understanding of how political constraints shape economic development.

II. Theoretical foundations

In the last two decades, questions of political economy have moved from the margins back to the centre of all branches of economics, including the study of economic growth and development. This revolution has had a profound impact on how economists approach their subject nowadays: by bringing to the fore political choices and the role of institutional forms in shaping societal decisions, the study of political economy has forced economists to engage much more closely with disciplines such as economic history, politics and political science, decision theory, geography, and, increasingly, psychology (while at the same time it has brought some of the theoretical and empirical rigour of economics to these fields). The roll-call of recent Nobel laureates illustrates how these related disciplines are being integrated into modern economics. From Robert Vogel and Douglas North's Nobel prize in 1993 for

[1] Many economists, in fact, would start from this narrower perspective. Robert Lucas (1988, p. 3), for example, describes the problem of economic development as 'simply accounting for the observed pattern, across countries and time, in levels and rates of growth of per capita income', adding that 'This may seem too narrow a definition . . . but thinking about income patterns will necessarily involve us in thinking about many other aspects of societies too'.

their work on institutions and long-run economic growth, through Daniel Kahneman and Vernon Smith's work on decision making (2002), Leonid Hurwicz, Eric Maskin, and Roger Myerson's on mechanism design (2007), to Paul Krugman on location and trade (2008), pioneering economists are radically altering the discipline and re-building the theoretical basis on which modern political economy rests.

This 'new' political economy has two defining characteristics, the first relating to the use of economic theory and the second to empirical validation. In terms of theory, Besley (2004) notes that for much of the post-Second World War period, mainstream public economics in general and economic policy-making in particular turned away from the fundamental ideas of political economy articulated by Smith, Mill and the classical economists of the eighteenth and nineteenth centuries. Under the influence of Paul Samuelson, among others, economics became rooted in what Besley labels a 'Pigouvian paradigm' which stressed the design of optimal policy interventions in the presence of market failure by benevolent social-welfare-maximizing governments. Government in this paradigm was generally conceived of as a benevolent *deus ex machina*, disembodied from its social, historical, and political context that had fully internalized all relevant conflicts of interest. Notions of government failure or institutional deficiency had no coherent meaning within this paradigm. The 'new' political economy can be seen as a direct response to this limited technocratic characterization of government: it is an attempt to re-focus attention back towards earlier considerations of how politics and the institutional structures emerging from different forms of political competition shape policy choices and ultimately economic outcomes. Thus, Besley argues, while established traditions in political economy, such as rational political economy, associated most strongly with the work of James M. Buchanan and the Public Choice school, remain strong, contemporary researchers tend to draw more directly from mainstream economics. The economics of imperfect information, agency theory, and dynamic consistency thus figure prominently. The books by Drazen (2000), Persson and Tabellini (2000), and Dixit (2004) exemplify these new theoretical approaches.

Within the field of economic development, the narrative described by Besley is much less sharply drawn. There has long been a powerful tradition of viewing questions of economic development through an explicitly political or political economy lens. This is most obvious in the Marxian tradition in development theory which stretches back to Lenin's *Imperialism* (1916) and probably reached its apogee with Walter Rodney's *How Europe Underdeveloped Africa* (1972), but can also be seen in the structuralist economics tradition that dominated the intellectual debate on economic development in Latin America from the end of the Second World War until around the late 1980s (see, for example, Lance Taylor (2004)). In this tradition, economic development is deeply and ineluctably rooted in the politics of power. Power relations in this perspective are, however, essentially class-based. Moreover, the class relations and institutions they entail are invariably shaped by external rather than domestic political competition.

In what might be called 'mainstream' or neo-classical development economics, however, the emergence of an explicit 'new' political economy of development is more readily discernible. This can be seen most clearly in the evolving intellectual diagnosis of the 'development problem' by the World Bank, the IMF, and other international agencies, and in how this diagnosis has translated into policy on official development assistance to low-income countries. For much of this period, and drawing inspiration from the pioneering work of Rosenstein-Rodan (1943), Rostow (1960), and, from within the World Bank itself (Chenery and Strout, 1966), the diagnosis had elements of the 'Pigouvian paradigm' where the relevant failure was a market failure which led to socially inefficient capital accumulation in the face of 'big push' externalities. This 'capital shortage' diagnosis thus identified slow growth and underdevelopment with conditions of low incomes and a large wedge between the private and social returns to capital. Governments lacked the tax instruments to raise revenue domestically to finance the required infrastructure investment and were unable to raise capital on global markets (they could not issue bonds in their own currencies—the 'original sin'—and lacked the creditworthiness to float foreign currency debt). At the same time, foreign private capital was unable to internalize the social returns to investment. In this environment, temporary official capital inflows channelled through governments appeared to offer the prospect both of augmenting low levels of domestic saving and directing resources into high social-return investments, such as the transport infrastructure and education, thereby raising returns to private investment.

This approach to development assistance persisted from the 1950s to the late 1970s but with only limited success. The success stories were concentrated in East Asia, where large capital injections from aid did combine with rapidly rising private savings to place countries on the growth trajectory associated with the 'East Asian Miracle' (see the papers by Gill and Kharas and by Brady and Spence in this issue). In low-income countries, however, though individual aid projects frequently posted high returns, the persistent failure of aggregate growth rates to catch up with the developed world and, more telling, the growing divergence within developing countries saw this capital-shortage diagnosis give way to one which viewed low levels of growth and development as symptoms of deeper concerns about the political and institutional foundations on which societies are built and hence on the economic policy choices that emerge. In the context of aid, this diagnosis resonated with those of long-time critiques of foreign aid—most notably Peter Bauer (1974)—who saw the unconstrained state as being prepared, for a variety of reasons, to sacrifice broad-based economic development for more venal objectives. But this 'institutional failures' diagnosis links more generally to traditions in African political economy which embed institutional failures in systems of personal and group rule, work that is probably most closely associated with Robert Bates (1981, 1986) and Richard Sandbrook (1985), in which the heavy use of patronage, the discouragement of agencies of restraint, and the emasculation of competing centres of political power as 'rational' strategies of African leaders in the context of weak political legitimacy and tenuous bureaucratic control. Two decades on, this diagnosis suffuses Paul Collier's influential book *The Bottom Billion* (2007) and is the dominant theme of his current book *Wars, Guns and Votes* (2009*a*). The Collier paper in this volume (2009*b*), which previews some of these ideas, sits firmly within the 'institutional failure' school.[2]

Despite the very serious challenges involved in transforming this positive analysis into a normative political economy, a point to which we return later, the 'institutional failure' diagnosis has led international engagement towards a focus on the use of the range of (external) policy instruments, of which aid flows is but one, aimed as much at shifting the

[2] The 'capital shortage' diagnosis of the development problem continues to represent an important, if controversial, strand in modern development economics, particularly in regard to the condition of the lowest-income countries. It emerges in some of the empirical literature on aid effectiveness—for example Clements *et al*. (2004) but the argument is most forcefully reprised in the work of Jeffrey Sachs and his colleagues (e.g. Sachs *et al*., 2004) who seek to locate Africa's historically low growth and divergence in the presence of market failures—poverty traps—and to argue that large well-directed aid flows are both necessary and sufficient to lift poor countries over the relevant thresholds and put them on high-growth convergent paths. While the genesis of the relevant traps may be different—Sachs *et al*. place heavy weight on a combination of geographical disadvantage, a high latent disease burden, and a lousy political and historical legacy—the prognosis is pure 'big push' in the tradition of the early development economists such as Rosenstein-Rodan (1943) as re-interpreted by Murphy *et al*. (1989).

political equilibrium—though enforcing greater transparency and accountability on political elites—in ways that promote choices that deliver 'developmental outcomes' as at effecting pure resource transfers. Nowadays, 'conventional' questions of development assistance, such as macroeconomic policy choices, investment priorities, and trade reforms, compete with, and are cast within, a broader set of concerns about governance, regulation, corruption, and the institutional foundations of policy.

III. The empirical challenge

The second characteristic of the 'new' political economy, the emphasis on empirical validation, reflects both developments in empirical methods—particularly in the fields of micro-econometrics, programme evaluation, and the increased use of experimental methods—and an enormous investment in data generation (although the two are clearly related). Beyond the mere descriptive, the issue that preoccupies most of the work in this area is how to isolate (and understand) the causal effects of institutional or political factors on economic outcomes, and vice versa. A vast 'empirical political economy' literature has emerged, spanning traditional political-economy themes related to links between elections and business cycles as well as increasingly ingenious hypotheses tested using experimental or quasi-experimental methods (Green and Gerber, 2002). But much of the most exciting and innovative work is in the field of economic development. A first strand has been closely linked to the development and exploitation of comparable cross-country data sets on political and institutional structures. A key element of this strand has been the generation of data sets, such as the cross-country governance indicators produced by the World Bank (Kaufmann *et al*., 2008), or the aggregation of survey-based data such as from the Afrobarometer or the World Value Surveys (Alesina and Guiliano, 2009) which has breathed new life into cross-country empirical analysis. Related work has exploited institutional variation across states or regions within countries, such as the work building on the Ozler *et al*. (1996) state-level data set for India. A second strand has abandoned the cross-country focus to exploit more systematically variation between households or at least lower levels of administration, such as districts or villages. The resulting literature often reflects a far richer understanding of the specific context and, as a consequence, a vast array of locally relevant topics related to elections, bureaucratic processes, legal systems, conflict, property rights, and more (see recent reviews in Pande (2009), Banerjee *et al*. (2009), or Aldeshev in this issue).

Generating detailed data on political or institutional variables is necessary but not sufficient to exploit convincingly their variation to make causal inference. In recent years, economics, and not least development economics has experienced a seismic methodological shift in its ambition with commensurate scepticism about its past achievements in establishing causal relationships. The interpretative boundaries between associations, correlations, and causal effects had long been somewhat blurred in much empirical research in economics. That such debates came to the fore with more vengeance in development economics than in other parts of economic research should not come as a surprise. Development economics is a sub-discipline devoted to understanding how economic transformation can come about and has historically often been funded by public bodies with a clear objective of influencing and implementing policy.

The empirical work on the political economy of development had not been immune to this, particularly in aggregate cross-country regression work but also in the micro-level work taking place. At the same time, it is a field that has rapidly evolved to engage directly with the causality question between economic outcomes and political and institutional processes. The challenge is that political institutions and outcomes are shaped by economic conditions, and vice versa. At the aggregate level, such as in cross-country work, the statistical challenge when, for example, exploring the causal effect of institutions on economic outcomes is then to identify variation in these institutional variables that can effectively be considered random and not shaped by economic circumstances. In the literature exploiting aggregate cross-country data, this has typically implied the quest for a valid instrument, in our example a variable that strongly impacts institutions and only impacts economic outcomes via institutions. As there are no statistical tests that can test the validity of the choice of an instrument, persuasion is required in terms of a clear narrative to justify the use of the instrument. Well-known examples in this literature are Acemoglu and Johnson (2005), who argued for a causal link from institutional quality to growth, by using historical data on settler mortality and the legal system at the time of colonization as the instrumental variable for property rights and contractual institutions (see also Acemoglu *et al*., 2005). The argument is that settler mortality around 1500 may well have affected the nature of the colonization process, such as the nature and migrant-intensity of agricultural settlements, affecting the nature of the property rights system imposed by the colonizer, which in turn predict the property rights system in 2000. The other part of the argument is that legal systems tend to evolve gradually, so the system in 1500 predicts the contractual institutions in 2000. Using these variables as instruments allows them to disentangle which institutions matter most, and they find that property rights institutions drive the differences in economic performance, while differences in contractual institutions have no additional statistically significant effect once property rights systems are properly accounted for.

Another example is Miguel *et al*. (2004), who showed the causal link from (poor) economic conditions to the outbreak of civil conflict, exploiting rainfall variability as an instrumental variable for economic growth. These appeals to plausibility, however persuasive the argument, will always be contested so that for many questions, the ability to identify causal effects remains debatable: indeed, the same data sets have often been used to argue for opposite conclusions, as is shown in the careful review by Toke Aidt in this issue (Aidt, 2009) on whether corruption is bad for growth.

When using below-national-level data, more opportunities arise for empirically convincing work on identifying links between political and other institutions and economic variables, addressing the underlying potential endogeneities. Three routes have been successfully used, even though all three have inherent problems. Similarly to the work on aggregate data, the first route has been instrumental variable estimation, in which institutions tend to be instrumented by some exogenous factor, often based on historical data or very detailed knowledge of local circumstances. For example, Banerjee and Iyer (2005) look at how different land revenue collection systems across India during colonial times led to differences in local cohesion and class conflict, the legacy of which impacts on contemporary public-goods provision many decades later. Another example is Goldstein and Udry (2008), who provide links from matrilineal rights and position in the local political hierarchy to land tenure security and investment. These examples are particularly careful studies: in general, however, and even with extensive longitudinal data, it is typically difficult to find suitable instruments, not least in terms of satisfying the exclusion restriction when researching the link from institutions to economic outcomes, i.e. that the selected instrument affects institutions only but has no further independent influence on economic outcomes.

An alternative, common in this literature, is to rely on natural experiments, which, when assessing the impact of institutions on economic outcomes, depend on some 'random' source of variation in institutions, rules, or regulations as if it was delivered 'by nature' so that the endogeneity question does not arise. An example is Chattopadhyay and Duflo (2004) who studied the impact of a change in political power towards women on local public-goods provision in India. They exploited a constitutional change in 1992 that reserved some positions of heads of village government for women. The villages in which these reservations were imposed were chosen at random at each election thereby offering a 'natural' experiment. In practice, however, few real 'natural' experiments are present, despite the titles of many published and unpublished papers referring to 'a natural experiment'. This is not least the case in political economy research: rule changes and their implementation are rarely independent of overall political and economic processes. The 'natural experimenter' is thus constantly on the look-out for random events and the laws of unintended consequence to throw up exploitable sources of random variation.

This leads to the final route for identification, increasingly influential in research in development in general and political economy in particular: field experiments, implemented as randomized controlled trials in which a population is divided randomly into a treatment and a control group, with the treatment group receiving or experiencing a particular 'treatment' or intervention. The random nature of the treatment allows the assessment of the causal link between the treatment and some outcome. Field experiments have been relatively rare in political science (Green and Gerber (2002) but are definitely on the increase in research on the political economy of development. Pedro Vicente and Leonard Wantechekon (2009, this issue) use their own experiments related to electoral processes and voting attitudes and behaviour in Africa to show the insights that can be gained. Fearon *et al*. (2009) offer a recent review of its potential and challenges and numerous examples.

This approach is not without its critics. One strand of criticism is concerned that previously careful attempts to understand the underlying forces of development are turned into a form of policy analysis (Bates, 2006). Others have argued persuasively that only relatively few relevant research questions for development can be turned into a field experiment (Ravallion, 2009). Deaton (2009) questions the statistical basis of the findings of many field experiments, including that the exclusion restriction may not always be valid even if randomization took place, as the intervention itself may not just affect the outcome through the variable whose causal impact is being researched. In all, the methodological challenge has not quite been settled, but it has led to more dynamic and creative, even if at times narrowly opportunistic, research in the political economy of development.

IV. Insights from the contributed papers

These words of caution emphasize that the new political economy research programme is very much a work in progress. But it is a large and very dynamic research programme. Our objective in compiling this set of papers, therefore, was not to be comprehensive but rather to use these papers—many of which are ostensibly on rather narrow and specific issues—to give a sense of the scope of field and a flavour of how the methods of modern economics are applied in this field. In the main, the papers are works of positive economic analysis but each takes steps in a normative direction—some less tentative than others. The papers split naturally into two groups. The first group, which adopts a broadly discursive approach, is fundamentally concerned with the political economy of aggregate economic development across a variety of settings: among the most rapidly growing countries of the world and among so-called 'failed states'; between the transition from low- to middle-income and the transition to high-income status; and in the presence of substantial natural resource dependence. The second group takes the analysis to a microeconomic and more empirical level. These papers are concerned with the detailed functioning of specific institutional forms and aspects of political and economic governance, dealing with the relationship between corruption and development, the impact of different legal institutions of economic outcome, and, finally, on political competition and vote buying.

The first two papers explore the political economy of economic success, with both drawing on work done by the authors for the Commission on Growth and Development. Established in 2006 under the chairmanship of Nobel laureate Michael Spence, the Commission sought to 'to take stock of the state of theoretical and empirical knowledge on economic growth with a view to drawing implications for policy for the current and next generation of policymakers'.[3] The main report of the Growth Commission (Commission on Growth and Development, 2008) took as its focus countries that had managed to achieve and sustain high growth for two decades or more and sought to provide a diagnosis of their success.[4] This diagnosis highlighted four key proximate determinants of high and sustained growth, factors around which country-specific growth strategies may be designed. In many respects, these factors are conventional. The first was a fundamental commitment to a market-based resource allocation combined with an openness to trade, both in goods and in technological know-how. The second was a policy framework that delivered a high degree of predictability in macroeconomic policy-making and a stable macroeconomic environment over the extended medium term; and the third a strong 'future orientation', in other words an environment which supports high domestic savings to fund high levels of public and private investment on the grounds that, at least in the early stages of development, the constraint to growth is limited principally by the rate of investment and hence saving. While the Commission recognized the role of foreign savings in supporting this future orientation—especially in the early stages of a growth take-off—sustainable growth requires domestic savings mobilization: foreign capital inflows, either in the form of FDI or concessional aid, in the case of low-income counties, are imperfect substitutes, liable to be associated with a range of adverse effects, from capital account volatility to problems of 'sudden stops' for domestic savings.

The final critical ingredient identified by the Commission was a 'capable, credible and committed government'. By this it meant a system of governance and leadership that had the flexibility to adjust policy and institutional structures to changing circumstances and opportunities but to do so in a manner that is credible and commands broad support. This latter requirement is essential: a common theme of all the successful countries examined by the Growth Commission is their ability to make growth tolerably inclusive. The paper by Indermit Gill and Homi Kharas is directly concerned with exactly this last issue. The starting point is the highly influential work of Hilton Root and Jose Edgardo Campos, *The Key to the*

[3] See www.growthcommission.org

[4] The Commission took as its sample all those countries that had averaged 7 per cent per annum growth for a quarter century or more since 1950, a rate being consistent with a doubling in size every decade. Thirteen countries satisfied this condition: Botswana, Brazil, China, Hong Kong, Indonesia, Japan, South Korea, Malaysia, Malta, Oman, Singapore, Taiwan, China, and Thailand. (As at the Commission's reference point, India and Vietnam were the closest 'runners up'.)

East Asian Miracle: Making Shared Growth Credible (1996), which articulated the notion that the success of the East Asian growth strategy was founded on the credible and coordinated belief that the benefits of growth would be spread widely. This entailed institutions that supported a close compact between government and producers, on the one hand, to generate growth, and between government and the economically vulnerable and politically opposed, on the other, to manage its distribution. That this compact was feasible and endured from the mid-1950s to the mid-1990s, the period which saw the countries of the region move from low- to middle-income status, was due to growth over this period being essentially generated through the combination of powerful improvements in agricultural productivity and rapid physical and human capital accumulation. Given this 'early stage' constant-returns-to-scale growth process, the shared growth model ensured that the market delivered both growth and equity, validating and reinforcing the growth strategy.

The East Asian environment is nowadays radically different to the era of shared growth. The region is almost uniformly middle-income, heavily urbanized, and no longer able to exploit a strategy of growth through factor accumulation and low real wages. Rather, to maintain growth and make the transition from middle- to high-income status requires a growth strategy which exploits increasing returns to scale. This radically redefines the role of the state in promoting the creation of economic rents—through strategic and targeted policy interventions—and there is strong evidence that this new model of growth is emerging in East Asia. But this model confronts the state with the now much more demanding challenge of creating inclusivity in the distribution of these rents. Growth based on increasing returns necessarily privileges some sectors and industries over others, some factors or skills over others (most notably those whose skills are valued in global markets), and some locations over others, most obviously urban locations over rural ones.

Picking up on the notion that 'nothing fails like success', Gill and Kharas argue that in these circumstances the policies and the political economy that were so successful in delivering the Asian Miracle era cannot be effective in the next transition from middle-income to high-income status. Without innovation, both technological and political, middle-income countries risk being becalmed, unable to maintain the momentum of growth through traditional means as other lower-income countries erode their cost advantage. But more importantly, when growth and equity are in conflict, and where the model of growth generates substantial rents, a growth-promoting government is increasingly likely to see itself aligned with narrowly based winners. The challenge of delivering distributional equity therefore becomes much stiffer and the role of government must change accordingly: managing the (mal)distribution of the proceeds of growth becomes a priority before damaging economic conflicts become unmanageable. Gill and Kharas suggest that a key element in this process is for the high-growth economies of East Asia to focus on strengthening institutions supporting better integration of domestic and regional markets (when previously the growth model demanded a focus on developing institutions to support integration with external markets such as export-processing zones). This requires a spatial focus to institutional development: on regional policy as conventionally understood in industrialized economies; on supporting greater flexibility in regional factor markets, in credit markets, and in education and training; and in developing the regulatory framework for network infrastructure.

There is a deep tendency within economics to think of 'capable, credible, and committed government' emerging from systems of checks and balances—constitutions, perhaps—which lay down the rules of political engagement and limit individual politicians' pursuit of narrow pecuniary self-interest. But this clearly does not do justice to the role of individuals or groups of individuals in shaping the economic trajectory followed by countries. How they do so and how they build and sustain support for their decisions, in other words how economic leadership is articulated, is examined in the paper by David Brady and Michael Spence (the Chairman of the Growth Commission). They also draw on the evidence from the high-growth countries studied by the Growth Commission to identify those elements of leadership that appear to be decisive in sustaining high and inclusive growth. By leadership they mean 'the making of fundamental choices about strategy, consensus building, and adapting the political institutions to support economic and social objectives'. They identify two key stages in successful economic leadership. The first is the process by which the political leadership chooses (or imposes) an appropriate economic model and builds a constituency of support for the strategy. The process of choosing a new model is rarely a simple story of progression and learning. More often there is a trigger, be it economic good fortune, such as resource discoveries, or moments of political opportunity such as often arise out of crisis. (In East Asia, these would include both the crises of conflict for Japan and Korea and also crises of political partition in the case of Singapore and Malaysia.) But triggers only create opportunities and the central question Brady and Spence seek to understand is how political leaders have seized these opportunities to establish and build a sufficiently encompassing coalition of support for the new model. The second stage is concerned with the capacity of leadership to adapt growth strategies—and to retain support for such changes—as circumstances change, both through exogenous events and in response to changes occurring endogenously as a result of the growth strategy itself. As the Gill and Kharas paper described, this latter case corresponds exactly to the situation the leadership of the 'East Asian miracle' countries have found themselves in since the mid-1990s. The low-wage, rapid-factor-accumulation model of growth that had sustained the region since the 1950s was beginning eventually to encounter diminishing returns but at the same time, the very success of the model meant that the political bargain that supported this model was changing. As Gill and Kharas (2009) state: 'Urban, middle-class people in middle-income economies engender a dramatically different political economy from that prevailing in the 1980s.' Brady and Spence have started to build a persuasive case that leadership matters and can be decisive in key settings. Ultimately, however, a definitive understanding of the centrality of leadership requires a counterfactual, a corresponding description of outcomes in circumstances where the political elite chose alternative paths or failed to respond to the trigger events.

Paul Collier's paper is concerned with countries at the other end of the spectrum, with failed states. These are countries either trapped in a vicious cycle of low incomes, weak states, non-consensual politics, low and inefficient investment, and low growth in incomes, or have slid back towards this state, principally through conflict, invariably civil conflict. In large measure these are the low-income countries of Africa. Collier (2009*b*) draws on the literature on state formation in Western society to argue that the global political settlement established at the end of the Second World War and enshrined in the international institutions of the UN, combined with a particular legacy of European colonialism, profoundly changed the dynamics of the state in Africa, endowing them with institutional structures that not only militated against the efficient developmental state—one that exhibited the characteristics identified by the Growth Commission—but also failed even to provide the basic security to its citizens that legitimates the nation state. The relevant 'market failure' in this case is the emasculation, by international agreement, of external threats to the nation state in Africa.[5]

[5] Article III of the Organization of African Unity Charter, signed in Addis Ababa in 1963 affirms the principles of 'non interference in the internal affairs of States' (Art III.2) and 'respect for the sovereignty and territorial integrity of each State and for its inalienable right for independent existence'.

But, it is argued, it was exactly this threat which elsewhere—most notably in Europe—had stimulated the political bargain of public security for taxation. As a result

> the typical post-colonial state did not face an external threat and so did not need to build an effective and hence expensive military. Pressure to raise revenue was lower and so there was less need to invest in either fiscal capacity or a legal system which would have assisted private prosperity. (Collier, 2009*b*)

But, argues Collier, the failure to ride this particular Darwinian wave was compounded by three related challenges: states were small, ethnically diverse and often established on abundant natural resources. The presence of a lootable prize—natural resources—and the dominance of ethnic or national identity both shortened and narrowed economic horizons and discouraged investment in the public state capacity. Economies of scale in the provision of public goods were not exploited, the environment for private investment remained hazardous, and growth stagnated which, in turn, served only to reinforce the returns to rebellion.

It is against this diagnosis that Collier offers a normative strategy for overcoming state failure, which involves institutional reforms at both the international and domestic level. These are not directed at re-establishing the conventions of inter-state war—indeed, much of the focus on the role of international agencies is directed towards the reduction and containment of the risk of a return to (internal) conflict. Rather the paper seeks to identify political and institutional reforms—at the level of the international community and at the level of the failed state—aimed at overcoming the adverse compounding factors of small economic size, ethnic diversity, and the latent resource curse.

The final paper in the first half of the issue is by Ragnar Torvik and is concerned with the political economy of the natural-resource curse. The paper revolves around two key questions arising out of the powerful and robust stylized fact that on average resource abundance, however measured, and economic growth are negatively correlated. The first question is in large measure a statistical one: can a structural interpretation be given to this negative association? Is it causal? The second, and arguably the much more interesting question, is that even if the average effect is robust and causal, what can we infer about those countries that are 'off the regression line'? As Torvik (2009) puts it, why 'for every Nigeria or Venezuela there is a Norway or a Botswana'? Trying to understand this variation around the mean pulls the enquiry into two different areas of the literature on the resource curse, one theoretical and the other empirical. On the theoretical front, the paper examines recent contributions from economics and political science that seek to understand the mechanisms of the 'paradox of plenty' through which resource abundance can lead to immiserizing economic outturns, at least for substantial sections of societies. These include the mechanisms grounded in the interaction with poverty, ethnic diversity, and conflict, as discussed by Paul Collier; the 'conventional' politics of rent-seeking, 'voracity' (Tornell and Lane, 1999); and the incentives to allocate natural-resource revenues in ways that may secure electoral success but risk endowing the economy with disposition of resources which may be inimical to growth—what Torvik refers to as the so-called 'white elephant problem'. On the empirical side, and drawing heavily on literature inspired by the comparative success of Norway, Torvik reviews how empirical research has sought to explain the variation around the simple negative correlation between resource abundance and growth by controlling for historical and environmental factors, such as the nature of natural resources and the historical context in which they were discovered, and for the structure of the political institutions in resource-abundant economies. Do on-shore natural resources pose greater problems than those located offshore? Are countries better placed to take advantage of natural-resource discoveries if they are at an early or at a late stage in development? If they are democratic or autocratic?

This is a relatively new and exciting empirical literature but one where robust results remain contested. The step to a normative political economy is still, therefore, a tentative one. Nonetheless, public policy has moved rapidly to embody many of the insights emerging from this political economy literature (as well as from elsewhere). Two recent initiatives typify this move. The first is the Extractive Industries Transparency Initiative (EITI), which is a coalition of governments, natural resource companies, civil society organizations, and international organizations established to set and monitor verifiable global standards for the extractive industries sector. The EITI—which was mooted at the 2002 World Summit for Sustainable Development and is now supported by a secretariat in Oslo—is built on the principle that voluntary disclosure of the financial operations of natural-resource companies and by host governments underpins efficient natural-resource management. As at the end of 2008, approximately 40 of the world's major extractive countries have committed themselves to the EITI charter, while 25 low- and middle-income countries had achieved 'candidate country' status which recognizes commitment to the EITI charter (to date only one country, Azerbaijan, had achieved 'full compliance'). The second initiative, the recently launched Natural Resources Charter, shares much common ground with the EITI—the emphasis on transparency in particular—but moves decisively to reflect a powerful normative perspective on the successful economic management of natural-resource wealth.[6]

The second half of the issue is concerned with the microeconomics of political economy and, in particular, with questions of empirical validity. Toke Aidt dissects the evidence on whether corruption is really bad for development. Much of the standard evidence on the link between corruption and development has been based on cross-country comparisons. On the basis of such data sets, some have argued that corruption tends to be good for growth, as it helps 'to grease the wheels' of development. Aidt (2009) shows that the reported evidence is weak and definitely not robust. Nevertheless, the evidence in favour of the reverse view, that corruption is bad for growth, is not convincingly addressed in such data, as it cannot be easily ignored that views on corruption may well be coloured by the actual growth experience of the country. This does not mean that corruption is harmless, and Aidt reviews the growing micro-level evidence on the negative impact of corruption on economic activity. Furthermore, he shows that when using broader measures of development, the link with corruption appears both strong and robust.

The paper by Gani Aldashev offers a comprehensive review of the recent theoretical and empirical literature on the relationship between the legal system and economic development. It also offers a fascinating case study of how much of the initial research, exploiting crude aggregate indicators using cross-country data sets, is increasingly complemented by detailed empirical studies at the country or sub-country level. The legal system can be defined as a system of interrelated formal institutions with three main functions (Gray, 1991): the setting of rules and standards, mainly via laws and regulations, for the functioning of society; law enforcement; and dispute resolution. Aldashev (2009) organizes his review around three questions. First, do characteristics of the legal system matter for development outcomes?

[6] Details on EITI are available at www.eitransparency.org Established in early 2009, the Natural Resource Charter, has been developed by a group of economists and lawyers seeking to distil key lessons from the economics and political economy of natural resource management into a Charter for the effective management of natural resource wealth (see www.naturalresourcecharter.org).

Work using cross-country correlations, such as La Porta *et al*. (2008), confirms this link but it does not easily settle the causality between specific characteristics of the legal system to economic outcomes. Recent micro-economic work has started to shed more light on this, more convincingly overcoming the endogeneity concerns than the macro-level work could do. Aldashev offers diverse examples on the consequences of formal land titling, changing bankruptcy, and other commercial law changes, and the interaction between informal and formal legal institutions. Still, much of this work tends to focus on relatively simple empirical findings, and more work is needed to uncover more precisely when and why reforms of legal systems have an impact. This also leads to his subsequent two questions: how are legal institutions developed, and why do legal reforms (not) occur? This political economy of institutions is still in its infancy. Much inspiration can be found in a study of historical or cultural contexts, but obviously economic agents have clear incentives to shape the nature of legal institutions that govern their activities. Aldashev discusses some of the recent theoretical models exploring these aspects, pointing to possible directions for further empirical work as well as offering guidance for a normative analysis of legal reform.

The final paper, Vicente and Wantchekon (2009), is a good illustration of how experimental methods can shed light on pressing political economy issues in developing countries. They focus on elections, more specifically how candidates can attract votes, and show how ingenious field experiments can offer insights beyond basic theoretical models of voting. Using data from specific experiments in Benin and in Sao Tome and Principe, they focus on clientelism, which is the exchange of votes for favours conditional on being elected, and vote-buying as votes-for-cash or other immediate reward. Both seem to be common in many countries and are perceived as being harmful for elections and its broader development consequences. They suggest that clientelism is more efficient, as there are incentives for voters to respond to clientelism in their voting behaviour, while vote-buying does not have a clear vote-enforcement mechanism. Their findings suggest, nevertheless, that both strategies 'work' even though clientelism works particularly well for incumbents, while vote-buying is more effective for challengers. They argue for further replication in other settings of such experiments to generate stronger stylized facts, as well as more theoretical work for a richer understanding of the incentives and mechanisms by which such strategies may have impact.

V. Conclusions and policy implications

The papers in this issue of the *Oxford Review* give a glimpse of the breadth and dynamism of contemporary research in the political economy of development. At the same time, however, they highlight two deep tensions within the research programme. The first is between the positive and normative dimensions of political economy, and the second between the internal and external validity, or the generalizability, of research. Each paper highlights key normative questions. How should institutional and governance reforms and public policy interventions be structured to deliver sustained growth and better developmental outcomes? What is the best form of legal structure for a particular country? What actions lead to a durable exit from stagnation or state failure? What is the appropriate mix between the provision of traditional public goods, such as health and education, on the one hand, and the development of market-supporting institutions, such as legal and regulatory systems and institutions of financial and political accountability and transparency, on the other? The pressures to move rapidly from the positive analysis to practical policy advice on these issues are immense and come from all quarters. This tendency pervades all economic research, but it is powerfully present in the field of development, perhaps because the potential pay-offs are so great. Thus in today's research environment, for example, it is virtually impossible to obtain funding for any research in development economics unless some clear trajectory towards policy-making has to be promised to the relevant funding agency, be it the World Bank or bilateral donor agencies with very explicit mandates to demonstrate 'results' from their aid programmes, or even national research councils concerned with the 'policy relevance' of their research portfolios.

But despite the dynamism and innovation in the field and despite the pressures, developing a robust normative political economy of development remains an extremely difficult challenge and one that the economics profession is still a long way from successfully meeting. Formal models, though elegant and intellectually stimulating, remain highly stylized, while our empirically based understanding of key relationships remains very tentative. (It is still depressingly common, however, for carefully established statistical associations to be rather quickly interpreted as showing causal linkages and for statistical wizardry to be marshalled to give such results an unjustified semblance of truth.) Both continue to counsel a cautious and modest approach to policy recommendations. Robert Solow, writing in an earlier issue of the *Oxford Review*, celebrating the 50th anniversary of his famous model of economic growth, notes how easily the urge to draw specific policy recommendations from highly stylized models of growth can trivialize the debate. Thus he claims that some of the mainstream economics literature

> gives the impression that it is after all pretty easy to increase the long-run growth rate. Just reduce a tax on capital here or eliminate an inefficient regulation there, and the reward is fabulous, a higher growth rate forever. . . . But in real life it is very hard to move the permanent growth rate; and when it happens . . . the source can be a bit mysterious even after the fact. (Solow, 2007, pp. 5–6)

This concern is echoed by Avinash Dixit who concludes his Presidential Address to the American Economic Association on 'Governance, Institutions and Economic Activity' with a similar call for caution :

> before recommending any change, you should determine whether existing institutions are there for a good reason, and how your reforms would interact with them in the short run and the long run. I am not saying that everything that is there is there for a good reason, but it is better to start with a presumption in favour of what has existed for a while than the presumption that everything should be changed to match the successful formal institutions in advanced countries. (Dixit, 2009, p. 21)

These calls for caution reflect the second tension, the balance between internal and external validity. The research programme in the political economy of development is undeniably dynamic at present, and much of this dynamism is to be found in the micro-level empirical work, including in the use of experimental methods. There is a danger, however, that with the pressure to establish the 'internal validity' of precise identification and causality—the gold standard of contemporary empirical analysis in economics—micro-level research will become too narrowly data-driven and opportunistic. This pressure to find some suitably exogenous source of variation in the data carries with it the risk that the field becomes defined by a narrow notion of the measurable to the exclusion of more complex but no less relevant questions. The closely related challenge is that internal or statistical validity does not necessarily offer a more precise understanding of the mechanisms involved, beyond a narrow focus on impacts, nor does it help in moving from the specific to the general.

The key challenge, therefore, is to link the innovation of modern microeconomic research with the broader aggregate analysis of the style highlighted in the macro-contributions in this issue, both to improve our understanding of robust statistical findings but also as a means to fit this evidence into more general theories of institutional development. Both seem to be essential elements in the development of a practically valuable normative political economy of development.

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