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Despite an efflorescence of work, much of it in this journal, the study of comparative political economy suffers from two conspicuous lacunae. The field continues to display the marks of its birth in an era when manufacturing still lay at the core of most economies. Although attention gradually shifted towards services, seen as a growing source of employment if a drag on productivity, with a few exceptions, the field has been slow to consider how a 'fourth industrial revolution' in information technology and biotech might be changing the nature of employment and the basis for growth in the developed democracies.

The eyes of the field also remained fixed on producer group politics, seeing it as the motor for developments in the political economy. The study of trade unions and employers' associations has often been its bread and butter. As a result, there has long been a gulf between studies in comparative political economy and studies of electoral politics, as Kitschelt *et al.* (1999) observed with some regret. Recent years have seen an 'electoral turn' in research on the welfare state, but works that link developments in electoral politics to those in the political economy remain scarce (cf. Gingrich and Häusermann, 2015; Iversen and Soskice, 2015).

Pablo Beramendi and his collaborators have produced a bold, immensely informative and important book which, in greater or lesser measure, addresses both of these gaps. A sweeping introduction to an excellent set of chapters pulls them together in a 'model of constrained partisanship' designed to integrate electoral politics into analyses of the political economy.

This commentary concentrates on that introduction because its architectonic theory sets research agendas for the field, even if it contains some limitations by virtue of its very ambition.

This model of constrained partisanship is designed to explain the social and economic policies pursued by governments, and by extension the economic outcomes they induce. Those policies are said to emerge from interaction between a demand side and supply side of politics. The demand side is rooted in an electoral politics structured by prevailing cleavages, along both a left–right spectrum and values-oriented divide. The constraints facing politicians on this demand side are generated largely by the distribution of voters across those cleavages.

The supply side is given by the policy options politicians can feasibly pursue, based on constraints on state capacities, which are conditioned by the programmes or institutional arrangements that emerge from previous rounds of policy-making. Some of those constraints arise because the constituencies that social programmes generate inflect the political demands made in subsequent elections. Others stem from the fiscal capacities governments develop in the course of prior policy-making.

This is an elegant and plausible model. It addresses the intuition that economic growth in knowledge economies will require higher levels and new forms of investment. The principal distinction the authors draw between the policies of different countries turns on the extent to which public spending is devoted to 'investment' aimed at long-term returns (on tertiary education, child care, active labour market policy or research and development) versus 'consumption' seen as social expenditure on old-age pensions, unemployment insurance and disability benefits which are said to compensate individuals for the loss of income without improving the productive potential of the economy. These formulations are clearly influenced by the literature on social investment.

Although conceptualized in broader terms, state capacity is measured here by the total share of GDP spent on both investment and consumption. This yields a set of categories across which the distribution of countries is reassuringly (or some might say disconcertingly) familiar. The Nordic countries (Denmark, Finland and Sweden) emerge as those with the highest levels of state capacity and investment. The liberal market economies (plus the Netherlands and Japan more narrowly) have high levels of investment but low levels of state capacity. The coordinated market economies of continental Europe (Germany, Austria, France, Belgium and Italy) have high levels of state capacity but low levels of investment, while the Mediterranean economies of Greece and, more narrowly, Spain and Portugal have low levels of state capacity and investment. This last finding is especially illuminating for those thinking about the roots of the Euro crisis and how growth can be restored in southern Europe.

This emphasis on investment is apposite for a knowledge economy, but assessing it via levels of spending on these programmes raises a host of unresolved issues. How high are the returns on each of these types of investment? Can we extrapolate from the sums spent on investment to its quality without looking at how specific programmes are designed? Active labour market policy, for instance, is a catch-all category often used to cover subsidies to employment with little value for human capital as well as training programmes with higher returns (Bonoli, 2005). It is not clear why spending on tertiary education should be considered investment, while spending on vocational training is not. Although it is treated here as consumption, spending on unemployment insurance might be crucial to inducing workers to acquire skills (Iversen and Soskice, 2001).

A more complete analysis would also have to take into account the tax expenditures some governments use to promote investments by firms and workers. We cannot expect the authors of this book to have undertaken all those analyses. Their principal contribution is to point towards the research that still needs to be done. In the course of that research, however, finer lines will have to be drawn between what counts as 'consumption' and as 'investment'. There is ample evidence, for instance, that much of the gap in cognitive or social skills between children from affluent and less-privileged families is already visible at the time children enter school and persistent throughout schooling. Therefore, support to poor families via programmes that some might see as consumption, such as unemployment benefits, can also be seen as a form of investment with the potential to promote skill acquisition more widely across the population.

The herculean efforts of the authors to model the demand and supply sides of the polity are inspiring without being entirely convincing. I have qualms about treating social spending (on a limited set of programmes) as a measure of state capacity. In one way, this does make good sense: if growth in a knowledge economy requires higher levels of public spending on investment, existing levels of public spending may be an indicator of the general willingness of national electorates to countenance higher spending—and that is arguably a dimension of state capacity. Seen in these terms, the USA and UK may indeed lack state capacity.

However, others have argued that higher levels of social spending actually reduce the capacities of states to address contemporary economic challenges, because spending on entitlement programmes reduces the scope that governments already burdened by debt have for discretionary spending on the kinds of investments highlighted in this analysis (Schäfer and Streeck, 2013). I am not convinced that those constraints are as tight as some might think, especially in an era of historically low interest rates, but this counterargument must be taken seriously.

Moreover, equating state capacity with public spending ignores a rich vein of work in comparative political economy. Early analysts argued that state capacity turns on the institutional arrangements available to support different types of state intervention (Shonfield, 1969; Zysman, 1983). A subsequent literature on neo-corporatism observed that the capacities of states to implement various kinds of policies often depend not only on how the state is organized but also on how key social interests are organized (Schmitter and Lehmbruch, 1979). It may be misleading to posit a sharp separation between the capacities of states and the capacities of other social actors. Even in a knowledge economy, the implementation of appropriate policies may well depend on the organization of producer groups (Ornston, 2012).

Of course, this takes us back into the realm of producer group politics, which these proponents of the 'electoral turn' leave out of their model, even though some chapters (by Hassel, Jackson and Thelen) make valuable observations about it. We should not demand more from a book that already accomplishes a lot. But its argument raises an important question for the field. Relative to each other, what are the roles played by electoral politics and producer group politics in socioeconomic policy-making and the development of the political economy? This is a 'division of labour' question that the field has yet to confront. Are issues of redistribution associated with social policy largely determined by electoral politics, for instance, while matters of investment in R&D or infrastructure are resolved primarily through producer group politics? Do producer group politics and electoral politics play different roles across countries based on the organization of producer groups and the political system there?

These issues have been discussed in the literature, but they are far from resolved (cf. Lynch, 2006; Hacker and Pierson, 2010; Culpepper, 2011).

How well does the image of electoral politics at the core of this model work? It is ingenious and elaborated through a series of case studies. In broad form, the approach is somewhat Rokkanian: economic changes first give rise to shifts in occupational structures, and the subsequent policy preferences of voters on both material and values issues are determined by their positions within those structures, albeit not entirely. The analysis focuses on four groups of voters: sociocultural professionals, business-finance professionals, low-skilled wage earners and petty bourgeoisie. The book makes the useful point that the relative size of each group conditions the political coalitions most feasible in a country. Sociocultural professionals emerge as pivotal, because they compromise more than 40% of the electorate in many countries; and it is notable how numerous and electorally important the petty bourgeoisie are in southern Europe.

However, the analysis is not entirely persuasive about why electoral coalitions in support of investment arise in some countries, but not others. The most telling claim is that governments can either increase their overall level of social spending or direct more of existing spending towards investment, but they cannot do both at the same time. That is eminently plausible, if bad news for the southern European countries that are low on both dimensions. But I wonder whether an analysis based on these four electoral groups adequately captures the relevant coalitional possibilities. In particular, the catch-all category of sociocultural professionals may be too crude to capture the variegated nature of the pivotal groups to whom parties seeking to promote investment in a knowledge economy will have to appeal. The analysis often posits governments that rest on a coalition between sociocultural professionals and business-finance professionals. That formulation captures something distinctive about the British governments led by Tony Blair, but does it really identify their full bases of support with enough precision?

The problem is that differences among people in the intermediate strata of this large group of sociocultural professionals, who are neither strictly speaking professionals nor low-wage earners but are potentially median voters, tend to disappear in this analysis. The political preferences of those who might fall into this group—including policemen, lab technicians, college professors, lower-level managers and others—may not be as homogenous or entirely given by the circumstances of their occupation as the authors suppose. As a result, the real political challenge facing parties interested in investment may escape this analysis. It can be seen as one of devising appeals capable of forging coalitions among people within this diverse middle group—by drawing on multiple dimensions of their life circumstances as well as their aspirations for their children. This point suggests that electoral politics may be a more creative process than even this agent-centered analysis allows.

What the book does highlight, however, is the stark possibility that low-skilled wage earners may well be left out of the governing coalitions in a new knowledge economy. That outcome may not be inevitable—as the authors note, policies of social investment can be more or less inclusive and I am uneasy with a portrait that suggests the low-skilled and petty bourgeoisie will rarely be supportive of investment. But the image presented here resonates with contemporary politics in ways that are sometimes chilling: namely, of polities forced to choose between populist parties that unite the low-skilled and petty bourgeoisie behind programmes of social and economic protection and middle-class parties relying on support from sociocultural professionals or business-finance professionals to promote investment. This book has its limits, but it is also a great accomplishment. It provides a vision of how to integrate the study of electoral politics into comparative political economy in the context of an emerging knowledge economy with which everyone in the field can usefully conjure.

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1. The politics of investment

Over the past decades, the economies of Western democracies have experienced massive structural transformations. Advanced capitalist societies have turned into service economies through intensive deindustrialization processes, new employment structures have emerged, markets of labour and capital have globalized and integrated and a significant slow-down in economic growth has taken place. The authors of *Politics of Advanced Capitalism* ask how governments have coped with and reacted to these dramatic economic and socio-structural changes. In doing so, they provide us with an impressive book which takes up the legacy of its two predecessors (Goldthorpe, 1984; Kitshcelt *et al.*, 1999), both seminal for the literature on Western political economies. What makes this book such a fantastic book is that it—like its predecessors—works as a whole to provide an original assessment of the politics of advanced capitalist economies. At the same time, the individual chapters are impressive contributions to discussion around specific aspects of advanced capital economies. For instance, anyone who wants to know about the role of trade unions in capitalist economies or social policy can find chapters that provide an overview of research on the specific topic, but also an original assessment of this aspect of capitalist economies.

2. Consumption versus investment

If one focuses on the main claim of the book, it can be summarized by saying that the politics of advanced capitalism is the politics of investments. A central theoretical focus of the book is thus the alleged political conflict around consumption versus investment. The introduction of the book and the chapter by Beramendi clearly shows the importance of the distinction between consumption and investment for economic outcomes. However, showing that the distinction between investment and consumption is important for economic outcomes is not the same as showing the political importance of the distinction. Thus, one may ask if there really is an important political conflict around consumption versus investment. To answer this question, one needs to critically examine the conceptualization and argumentation presented by the authors on these points.

As a first, the book does not spend a lot of time discussing exactly what consumption and investment consist of in policy terms. The authors seem to suggest that investment includes social investment like education, family policies and active labour market policies (chapter by Gingrich and Ansell), but also public infrastructure and research and development. These are very diverse policies. For instance, their definition is very different from what has traditionally been understood as investment in national accounting and economic theory. Other literature does not even view investment as a specific set of policies, but as a paradigm that permeates (social) policies more generally (e.g. Hemerijck and Vandenbroucke, 2012; Janson, 2012). As such, the applied division between consumption and investment is far from being uncontroversial. Given the emphasis which the book—for good reasons—puts on the distinction, the lack of a more elaborate conceptualization is a shortcoming.

Further, the distinction between consumption and investment is not only a matter of policy categories. The authors hold that it constitutes a new fundamental political conflict that divides the citizens of advanced democracies. This realignment argument suggests that preferences towards investment and consumption are essentially driven by the broad universalism–particularism conflict. The authors argue that the well-educated prefer investments in the future because they have the cognitive capacities and material resources to put a high value on these future-orientated policies. While educated individuals might favour investments in education and research, it is not really clear whether this has to do with their universalistic values or it is simply a matter of material self-interest. Further, it is hard to see how the universalism–particularism divide is also relevant for political conflict over investments in infrastructure. Does a universal mindset lead to a stronger spending preference for highways? We do not see a clear connection here, and the book does not provide a detailed empirical investigation of this, which is probably due to the lack of suitable data. Whereas evidence on people's attitudes towards spending on policies like child care and education does exist, such policies are not simply investments in the future. They also provide material benefits to people right now, so whether preferences for spending on such policy areas imply preferences for investment as such remains an open question.

It is also very likely that voters and political parties *in general* favour education, child care and other investments in the future because a key feature of investment is the win–win solutions they foster over time. Long-term benefits from investing are not only reaped by a single group in society but are, to a large extent, positive-sum outcomes. Everybody benefits at some point from the increases in productivity and the higher growth rates that spring from future-orientated policy programmes. In this perspective, prioritization of investment should at the aggregate level not be particularly conflict-ridden, but rather resemble a type of consensus politics where the vast majority of voters support higher spending. Who can be against investing in future generations by spending more on education?

The question is whether or not the political challenges and potential controversies surrounding investment relate to another key feature of the consumption–investment divide; namely, the basic temporal dilemma which the distinction represents for politicians. Consumption makes some voters happy now. Investments make voters happier somewhere in the future but also mean that we sacrifice consumption now. So as with welfare retrenchment, the distribution of benefits and costs seems to speak against investments in the first place. Myopic politicians will be reluctant to impose short-run costs on voters to finance investments that only lead to gains in the long run. In a similar vein, it is not hard to imagine that investment policy could be an easy target of cuts because the consequences only arrive a long time after incumbents are to be reelected.

Even if the benefits of investments are back-loaded, investing in the future also generates small but crucial benefits in the short run. For instance, expanding school budgets not only boost the overall skill level of the labour force in the long run, but also provide immediate benefits for current teachers and students. In fact, these short-term benefits may turn out to be crucial for understanding the political choice between investment and consumption. Empirically, cross-national differences seem to partly be a result of such dynamics. For instance, the high levels of investment in the Nordic countries have convincingly been attributed to governments' short-term responses to the increasing unemployment caused by deindustrialization (Iversen and Wren, 1998). Similarly, the Scandinavian countries have invested in child care—not necessarily because they had politicians who cared more about the future—but because there was a short-term political demand.

3. The supply of politics

At a more fundamental level, these remarks raise a question about the whole theoretical approach of the book. As stated in introduction, the book represents an attempt to reintroduce 'electoral politics' into the study of advanced capitalism, an attempt which is very important and most welcome. The book also makes a very convincing argument why this requires us to understand both the supply and demand side of politics. However, in reality the book has surprisingly little to say about the supply side, at least central aspects of it. Thus whereas the book offers a chapter on trade unions (Hassel) and one on what voters want (Häusermann and Kriesi), only one chapter deals with political parties (Kitschelt & Rehm). This chapter focuses on the constituencies of the parties rather than on parties as strategic actors supplying possible political solutions to the challenges of advanced capitalist economies. To large extent, reintroducing 'electoral politics' into the study of advanced capitalist economies is thus translated into a focus on the electorate. Of course, the electorate is an important aspect of electoral politics, but there is much more to the supply of politics than what voters want.

If one thinks of for instance the Greek crisis, it seems like a case where the supply side has been crucial. The emergence of new political parties and the referendum are supply side dynamics. Of course the demand side is also crucial, but we need much more focus on the supply side of politics to understand where advanced capitalist economies are going. Here one could have wished the book had gone further. Of course, its claim about the need to re-introduce electoral politics in the study of advanced capitalism economies should be seen in light of the strong dominance of a focus on organized interests within the field. However, electoral politics in advanced capitalist economies cannot simply be understood by studying the electorate. Political parties have increasingly been detached from social groups like workers or farmers. Rather parties have become strategic actors with communication and branding strategies. How this type of party behaviour interacts with voters preferences to produce the policies shaping capitalist economies is a crucial question, which deserves much more attention than given in the book.

Exactly such a broader understanding of electoral politics is what is missing with regard to the politics of investments. One question regards clarifying to a greater extent which preferences voters actually have, but another thing is how political parties behave. Voters' preferences are likely to be important, but so are more intrinsic aspects of investment policies like time horizon relating to investment.

The *Politics of Advanced Capitalism* is a fantastic book, which takes up the legacy of the former two books with the same focus in a thought provoking way. One characteristic of books having a lasting impact on a field is that their impact is not just through the answers they provide to essential within question the field, but equally the questions for further discussion that they raise. Exactly, this is what *The Politics of Advanced Capitalism* achieves with its focus on the politics of investments and the call for a focus on electoral politics.

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The Politics of Advanced Capitalism (PAC) is self-consciously designed to be a successor to two earlier volumes, *Order and Conflict in Contemporary Capitalism* (1984, edited by John Goldthorpe) and *Continuity and Change in Contemporary Capitalism* (1999, edited by Herbert Kitschelt, Peter Lange, Gary Marks and John Stephens). When they were published, these earlier works delivered close to the 'state of the art' regarding the comparative political economy of advanced capitalism. Each assembled an outstanding team of scholars. Moreover, each of these volumes was greater than the sum of its parts; they gained impact from a set of clear bottom-line claims about the main sources of (large) variations in important outcomes over time and cross-nationally. In *Order and Conflict* those claims centered on national capacities for successful tri-partite negotiation: countries that could successfully negotiate wage restraint could more effectively combine low inflation and higher growth rates. In *Continuity and Change*, the contributors echoed and built on both the Esping-Anderson (1990) and Hall and Soskice (2001) conceptualizations of relatively coherent policy regimes. The policy regimes existing within distinct clusters of advanced economies underpinned quite different political and economic equilibria.

That PAC has the same 'feel' is very much to its credit. Like their predecessors, the editors assembled a terrific group of scholars, with a combined expertise that makes a panoramic treatment of the vast subject matter possible. Moreover, despite some differences in perspective across chapters (as there were in the earlier volumes), *PAC* has a clear message about how to approach the subject of variation across nations and over time. In their introduction that frames the volume, the editors call this approach the 'electoral turn'. Because this is the volume's agenda-setting move, it is the merits of this turn that I wish to examine in this short review essay.

Precisely what 'the electoral turn' means is not completely clear. At a minimum, it entails an increased focus on the electoral and coalitional strategies of parties and the preferences of voters. Of necessity, turning *towards* also means turning *away*, and in this case that means turning away from the focus on organized groups (especially employers and unions) that characterized the earlier volumes. At times, the editors are cautious. They insist (p. 25) that 'We are not arguing that economic producer groups are unimportant. Instead, we are arguing that producer group politics *alone* is insufficient to account for the politics of advanced capitalism'. But elsewhere they move beyond that (unarguable) minimalist stance, describing 'a theoretical move toward what we consider essential for the electoral turn in political economy: The presence of cross-class parties does not reflect specific producer interests, but is *a purely electoral political phenomenon*'. And again (p. 59): '. . .there are other ingredients to policy formation than electoral politics. Nevertheless, understanding the operation of electoral democracy is *constitutive* in order to account for variance over time and across space in policies and quality of life outcomes delivered by postindustrial democracies'. With the electoral turn voters, and party appeals to the electorate, move to the middle. Groups move to the periphery.

The editors are right to highlight this electoral turn as a major analytical shift. It is also a shift that brings the field of comparative political economy into closer alignment with the approach that has long been dominant in the study of American politics. There, as Morris Fiorina (2009, p. xvii) has put it, the voter-centric Downsian framework long 'served as a kind of master theory'. That framework, with various permutations, had a number of attractive features: a clear set of predictions with plausible micro-foundations, evident normative appeal (because it suggested that governments frequently faced clear and powerful incentives to be responsive to core voter preferences), and a strong elective affinity with methodological trends that have drawn researchers to large and easily available troves of data (and, more recently, to phenomena that lend themselves to experimental treatment). Voters check all these boxes; groups do not.

There is a big irony here. The 'electoral turn' would bring comparative political economy closer to the traditional practices of Americanists, even as that electoral focus increasingly looks besieged. More and more Americanists (especially those most interested in political economy) doubt the wisdom of placing voters at the heart of the political process. Leading behaviourists now argue that voter 'preferences' are not thick and solid, but rather thin and relatively plastic (Bartels, 2008; Lenz, 2012). There is growing evidence of massive gaps between the preferences of ordinary voters and policy choices (Gilens and Page, 2014). Macro-outcomes in the USA, from mounting partisan polarization to a huge increase in top-end income inequality, are difficult to reconcile with bottom-up, voter-preference-driven accounts of modern politics (Hacker and Pierson, 2014).

In short, just when the editors counsel comparative scholars to make an electoral turn, many Americanists are making a different one: towards a recognition of the limited and highly mediated influence of voters and towards a greater appreciation of the role of what Bawn *et al.* (2012) call 'intense policy demanders'—highly motivated and organized actors who both care a great deal about the content of governance and are capable of playing a long game to achieve their goals.

It is important to be clear about the main lines of dispute. All agree: *to endure, governments must sustain support of a majority of voters*. What is in question is the extent to which this constitutes an *explanation* for important phenomena. Put differently, do governments behave in particular ways because of the pre-existing distribution of voter preferences? The need to maintain electoral support might also be more like a tautology, something that is essentially true by definition, but does not explain all that much about what governments do or choose not to do. And of course, there is room for nuanced positions that recognize that voters can play more or less of a constraining role depending on context, e.g. whether a particular dispute is highly visible to voters or whether an election is approaching (Culpepper, 2011).

Indeed, that nuance is evident in *PAC* once you look below the headline. Groups come in to the analysis in important, but often under-analyzed ways. In the volume's introduction, for instance, organized interests are downplayed in the theoretical discussion, but they are actually pretty prominent in the case narratives. First, the editors place heavy emphasis, rightly in my view, on *legacies*—on inherited constellations that constrain the options of contemporary actors. Echoing the core themes of *Continuity and Change in Contemporary Capitalism*, they acknowledge *that groups largely constructed these legacies*. Organized interests pushed hard for their priorities over extended periods of time. Indeed, the existing distributions of voter blocs in different countries are themselves in significant part the consequences of these earlier policy outcomes.

Of course, that's the past. But why would not it be equally true of the present? Part of the editors′ reasoning is based on the demise of unions: 'The decline of labor has given more prominence to partisan electoral competition' (p. 26).[1] And labour certainly has declined, although it is clearly still quite consequential in many settings. But to say that the influence of one set of organized actors has declined is not the same as saying that the role of organized actors has declined in general. Maybe other groups have increased in relative prominence? Indeed, an argument is to be made that the weakening of middle class organization *diminishes* the capacity of electoral systems to provide decisive guidance to governance. Arguably, low-information voters need clear signals from attentive and trusted groups to hold policymakers accountable.

The decline of labour could represent not a diminution in the role of organized interests, but rather a shift in the balance of power resources among organized interests. Thus, a striking feature of *PAC* is the relative absence of attention to the policy priorities or political activities of business interests in general, and the financial sector and financial politics in particular. This absence may be related to the policy structures that the volume places front and center. What you see as the fundamental issues in a political economy is going to have an effect on whom you see as most consequential, and on which venues are perceived as salient for determining 'who governs' and with what effects. Thus, it is notable that *PAC* places heavy emphasis on fiscal issues—and in particular on the trade-off between budget allocations to 'investment' and 'consumption'. Much less attention is paid to topics like corporate governance or financial regulation.

Those who question the centrality of electoral politics have noted that organized interests will often seek to remove issues from the partisan agenda, shifting them to technocratic venues or to a different level of political authority where they can exert more influence. The most relevant illustration in the current context is the shift of authority over macroeconomic policy to the EU level. Revealingly, the editors treat the Eurozone crisis as 'a kind of testing ground' for their argument about the distinct political economies of the different regimes. The result feels a bit like a morality tale. 'Core' countries are well positioned and equipped to handle fiscal challenges and retain considerable autonomy; peripheral countries lack these capabilities and pay the price.

But the Eurozone is not some sort of randomized trial; it is itself a policy regime, put in place to promote some policies and constrain others, and to shift the locus of authority over critical macroeconomic decisions. Moreover, it would be hard to argue that the Eurozone's development was at its core a bottom-up, electorally driven affair. It was, and

1 The editors also offer an interesting argument (p. 61) that the profound uncertainty about the consequences of our policy actions leaves all the major actors groping in the dark. Arguably, this undermines the idea of group-led, policy-focused agendas, freeing up more space for purely electorally driven outcomes. This is an interesting speculation, but it is not really pursued. Moreover, the volume's own emphasis on the superiority of fiscal mixes that lean towards investment casts doubt on the authors' commitment to it. In any event, the case that groups play a vital role in shaping policies does not rest on their prescience concerning the consequences of policy; it rests on their capacity to get their way.

remains, an elite project. Thus, a plausible alternative account would run as follows: a set of powerful policy demanders pushed for a new policy regime that established very important new rules for policymakers—rules that would structure behaviour regardless of party or election results. These rules eliminated the traditional mechanism through which economies could adjust to economic shocks, leading to intense new external constraints for peripheral countries and the prospect of a lengthy and severe bout of austerity.

So I have my doubts about the big theoretical move that anchors this volume—the heightened focus on electoral dynamics and party systems. Yet the authors of this important and enormously stimulating research are to be congratulated for putting forward a serious and sophisticated argument. In doing so, they sharpen the debate while informing our understanding of such vital issues. They also clear the high bar set by their widely admired predecessors.

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