

# **Understanding and Modeling Comparative Democratic Policymaking: How Institutional and Strategic Settings Shape the Incentives for, Capacities to, and Natures of Political *Budgeteering***

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[An Introduction to a New Project and a Guide to How to Read the Two  
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Two recent, important books, Persson and Tabellini's *Political Economics: Explaining Economic Policy* (2000) and Tsebelis' *Veto Players: How Institutions Work* (2002), are setting an exciting, ambitious, and very productive research agenda for modern comparative and international political economy. In different ways, and for somewhat different audiences, they each offer a truly *comparative* approach to studying democratic policymaking, i.e., an approach in which most of the key explanatory concepts *travel* across democratic contexts in a connected, systematic, and parametric way rather than in a nominal, classificatory way. The present research project, just begun, builds self-consciously and with great appreciation from these foundations, seeking however equally self-consciously to understand better and, hopefully, at least occasionally, to redress certain *productive* intellectual frustrations that arise from the occasional clashing of these recent efforts with the intuitions, evidence, and stylized facts collectively accumulated in the preceding years of the scientific study of comparative democracy and comparative political economy.

In Tsebelis' veto player approach, one identifies the veto actors in a political system—those whose approval is necessary to change the status quo—and their policy preferences. The number

and polarization of these veto actors then determine (specifically, reduces) the range of magnitudes of possible policy changes from the *status quo*, and thereby, most likely, the probability, average magnitude, and variance of policy change.<sup>1</sup> In short, policymaker fragmentation and polarization retard policy-adjustment rates.<sup>2</sup> One of the great advantages of this veto-player approach is that the numbers and polarization of policymakers are political features that can be determined empirically and compared smoothly across political systems. Three of the more important limitations, on the other hand, appear in the exaggerated claim of the book's subtitle. First, political institutions only affect, they do not determine, the numbers and polarization of policymaking actors. The socio-economic structures of interests in societies, for example, also strongly affect the number and polarization of political actors. Second, political institutions have other effects well beyond helping determine the number and polarization of veto actors; for instance, they also shape the nature and efficacy of political representation (and so the preferences of democratic policymakers). Third, the number and polarization of policymaking actors have other effects besides deterring policy change; they also induce common-pool and collective-action issues and can produce bargaining effects other than deadlock, such as compromise.

One part of this new project (see “Fiscal Policy with Multiple Policymakers”) seeks to explore more broadly the implications for policy outcomes (especially budgetary policies) of the dispersion of policymaking authority across multiple actors, considering simultaneously—and attempting to distinguish theoretically, empirically, and substantively its potentials to privilege the status quo and thus retard policy-adjustment rates, to raise collective-action and common-pool issues, and to induce bargaining and compromise.

Persson and Tabellini's *Political Economics* (2000) offers textbook compilation (and frequent

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<sup>1</sup> The ensuing implications do require assumptions beyond the base theory, but these assumptions are generally plausible.

<sup>2</sup> E.g., policymaker fragmentation and polarization likely induce “gridlock” (see, e.g., XXXX) and “delay stabilization” (see, e.g., Roubini and Sachs 1989ab; Alesina and Drazen 1991; Spolaore 2004).

considerable advancements) of a school of political-economic research, largely spear-headed by macroeconomists, which one might aptly call “positive macro political economy.” Broadly speaking, this positive macro political-economy asks in what mix and how extensively do strategic, self-interest-maximizing policymakers use four classes of policies—public-good provision, redistribution (broadly targeted), distribution (narrowly targeted), and rent-extraction (corruption, graft, stealing)—to pursue personal, electoral, and/or partisan goals under varying democratic institutional designs, emphasizing variations in electoral system, executive-legislative and central-government-local-government relations.

One limitation in these studies so far has been the tendency to dichotomize these variations—proportional vs. majoritarian, presidential vs. parliamentary, federal vs. unitary—and then to build separate theoretical models for each, rather than having the important concepts that vary across contexts to explain policymaking be parameters that vary within the same theoretical model. This sort of categorical theory building<sup>3</sup> has the unfortunate consequence of producing as its knowledge-accumulation output an ever-growing set of unconnected relations from binary treatments or combinations thereof to particular outcomes rather than a functional form with reduced parameterization that enables the analyst to understand more of a complex reality with less. A broad comparativist wants, in other words taking a metaphorically apt phrasing, comparative concepts that *travel* across contexts to produce theories that *travel* smoothly (rather than discretely, *i.e.* bumpily). Thus, ideally, one would build theoretical models that explored directly variations in the *degree* of proportionality, of executive-legislative fusion, of authority decentralization, etc., if these are the important aspects of the variation in electoral and governmental systems being studied.

Another limitation of these studies so far, more-jarring and frustrating for many

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<sup>3</sup> ...and the not unrelated empirical approach of *matching* (*propensity-score matching* or otherwise)...

comparativists perhaps, but also potentially quite productively so, has been the isolation in studying each of these institutional designs a single aspect of these previously dense concepts: proportional and majoritarian, presidential and parliamentary, federal and unitary systems. In Persson and Tabellini (2000), for example, proportional vs. majoritarian electoral-system distinction involves only whether electoral contests occur, respectively, in a single or in multiple districts. The key difference, then, is whether one wins the entirety of policymaking power by gaining majority electoral support in the single, national district or one wins that same entirety of policymaking power by gaining a majority in a majority of the nation's many districts. In either case, the competition occurs between two parties; the parties may have differing policy preferences but are otherwise identical (e.g., equally unified); and the number and other characteristics of the parties, the informational context of elections and policymaking, voter participation, and all other aspects of the political system are the same regardless of electoral system. Similarly, the study of presidentialism and parliamentarism isolates for comparison the differing policymaking rules in the two systems and ignores associated, or at least correlated, differences in parties characteristics, informational environments, etc. The isolated aspects of these dense concepts are definitely important aspects, and, when so isolated in the theoretical models offered, they do produce the logical implications derived. Accordingly, if and insofar as the concepts' definitions and/or their substantive implications jar with the comparativists' sensibilities, this should be a productive jarring. We comparativists, if I may speak so, should be pushed by these jarring demonstrations to unpack our dense concepts, to consider more carefully and more closely the implications of the multifarious aspects of those concepts and of their various combinations. This precisely states the aims of the other part of the current research agenda.

The other part of this new project (see "The Effective Constituency in (Re)Distributive Politics," especially Section V) seeks to explore more broadly how the *many* (or, at least, *more* of the)

aspects of electoral, party, and governmental systems and political-economic strategic settings *interact* to shape effective representation in democratic policymaking and, so policy outcomes (especially budgetary policies).

In both parts of this new project, I intend more precise development of the underlying concepts, enabling sharper measurement, and more direct building of the theoretical propositions into empirical specifications, employing nonlinear empirical modeling strategies as illustrated in, e.g., Franzese (1999; 2003), to gain empirical leverage on the complexly interactive expectations of the resulting highly context-dependent comparative political economy. For example, in “Effective Constituencies” involves specifying how the institutional and strategic setting shape the magnitude of the incentives to *budgeter*,  $f(X)$ , the strategic capacity of the policymakers to respond effectively to those incentives,  $g(Z)$ , and the nature of the *budgeteering* that would be most effective in that context,  $h(W)$ . The product of these three functions, incentive-magnitude times strategic-capacity times incentive-nature,  $f(X)g(Z)h(W)$ , yields the predicted policy outcome. Insofar as  $X$ ,  $Z$ , and  $W$ , and/or  $f()$ ,  $g()$ , and  $h()$  differ theoretically and empirically in sample, we can gain efficient leverage on all the implied interactions. Indeed, even if some elements of  $X$ ,  $Z$ , and  $W$  are unobservable—for example, strategic party unity—we can gain leverage even on the values of these unobservables, conditional on the broader theory, if we can model those elements and their precise role in the combined function determining the outcome sufficiently distinctly, precisely, and accurately and the arguments to this second-order model vary sufficiently in sample. “Effective Constituencies” shows the current state of my thinking on specifying this model of *comparative budgeteering* and some very preliminary empirical results suggesting some fruit might come from pursuing that effort further. Similarly, “Multiple Policymakers” seeks to distinguish precisely which aspects of the numbers, policy-preferences, and polarization of policymakers matter in producing veto-actor, common-pool, and bargaining effects

and precisely how those effects shape policymaking outcomes. Veto-actor effects stem from absolute numbers and polarization (ranges) whereas common-pool effects derive from effective (size-weighted) numbers (and possibly size-weighted polarization: i.e., variation or standard deviations); bargaining effects stem from actual preference positions. Veto-actor effects do not make predictions about policy levels but rather about policy changes or adjustment-rates, whereas the other two effects make level predictions. I follow Franzese (2002) and Przeworski and Ghandi (2004) in modeling veto-actor effects, adding here a particular kind of common-pool effect that is theoretically distinct and empirically distinguishable. Furthermore, bargaining tends generally to produce policy predictions (equilibria) that are some convex combination of the preferred policies of the bargainers. I showed how to model such scenarios empirically in Franzese (1999; 2003). “Multiple Policymakers” offers some very preliminary empirical results following such strategies that suggests some empirical possibility of productively distinguishing veto-actor, common-pool, and bargaining effects of variation in the allocation of policymaking authority.