Empirical Implications of Theoretical Models of Effective Democratic Representation: Electoral- and Party-Systemic Institutions, Structures, and Strategic Contexts

1. Project Overview

Political-economy theories note that policymakers have two goals: (a) to obtain and retain power (office seeking) and (b) to enact policies and foster outcomes they favor (policy and outcome seeking). Furthermore, in stable polities (*i.e.*, excluding coercion), policymakers have four broad classes of policy they can direct toward these goals: public-good provision, broadly targeted redistribution, narrowly targeted distribution, and rent extraction. In democracies, finally, policymaking representatives use these tools to pursue these goals in the strategic context of partisan electoral competition. Substantively, this project will leverage and advance theories of comparative democracy and of comparative and international political economy to build powerful, estimable, and interpretable empirical models that directly, *but manageably*, reflect the multifarious and complex interactions of electoral institutions, of party and party-systemic structure, and of the strategic shape of particular partisan-electoral contests in determining the relative weight of these four classes of policy in policymaking output. Party- and electoral-system institutions, structure, and strategic context interact in quite complex ways to shape the *effective representation* of societal interests in the ultimate policies, *but*, as this project seeks to demonstrate, in ways that a nonlinear empirical-modeling strategy can encompass effectively.

One venue where the effects of such complex contextual interactions in shaping effective representation should be more-observable is in the composition of policymaking activity, particularly in the redistributive (broadly targeted) *versus* distributive (narrowly targeted) share of budgetary activity. Institutional, structural, and strategic contexts that induce more partisan, interest-based representation foster greater redistributive emphasis, and those that induce more particularistic, geographically-based representation favor distribution. I have outlined such a "comparative democratic political economy of *budgeteering*" more fully elsewhere (Franzese and Nooruddin 2003), arguing: *(i)* that the degree to which parties are able to act as strategic units (strategic party-unity) should determine the *capacity* of democratic policymakers to *budgeteer*, *i.e.*, to manipulate the budget for political (*i.e.*, electoral and partisan) purposes, *(ii)* that national and district-level electoral competitiveness should determine the *magnitude* of their incentives to *budgeteer*, and *(iii)* party-system polarization, electoral-system and district magnitude, and the degree to which parties receive their electoral support as units (representational party-unity) should determine the *nature* of the *budgeteering* that serves policymakers' goals of gaining and retaining power and of producing their desired policies and outcomes. The combination of capacity and incentive size and nature, therefore, should determine the degree and character of the *budgeteering*–*i.e.*, the relative shares of public-good, partisan-redistributive, geographic-distributive, and rent-seeking activities reflected in public policies–observed across countries over time.

I have also begun to outline an estimable nonlinear empirical model that could shed light on the multiple, complex interactions among these many causal factors in shaping fiscal policies. In this theoretical description of a substantive situation, *(i) strategic capacity, c,* times *(ii) incentive magnitude, m,* determines the amount of *budgeteering* I expect, while *(iii)* the *nature, n,* of the *budgeteering* that I expect in pursuing these goals depends on several party, part-system, and electoral-system factors that shape whether effective representation has more partisan-interest or particularistic-geographic basis and so whether policy has more redistributive or distributive nature. Thus, taking (for now) the ratio of redistributive to distributive budgetary activity, *R*, as the dependent variable, theory predicts that $R=\ldots+c(X_c)\cdot m(X_m)\cdot n(X_n)+\ldots$, and empirical models should reflect that proposition (EITM). In previous work (Franzese 1999; Franzese 2003a; see Franzese 2002, 2003b for further discussion), I showed how nonlinear empirical modeling can gain powerful yet interpretable leverage on the multiple, complex interactions implied by such specifications to the degrees that theory enables specification of the functions and their arguments—here: $c(X_c)$, $m(X_m)$, and $n(X_n)$—and that these functions and/or arguments differ empirically in the empirical sample. I have so far begun to specify $c(X_c)$, $m(X_m)$, and $n(X_n)$ in this new context as *(i)-(iii)* of the previous paragraph sketch, and I have demonstrated preliminary empirical plausibility and found some hope for empirical success by showing a role of partisan strategic capacity in shaping the relative (re)distributiveness of postwar US fiscal policy that supports the theory and showing a pattern of coefficients on partisan- and geographic-representation independent variables in explaining the policy mix in subsamples of developed democracies divided according to my own qualitative evaluation of conditions *(i)-(iii)* in those countries. This proposal would fund continuance of this project: (a) specifying further and in tighter connection to comparative-politics and comparative political-economy theories, $c(X_c)$, $m(X_m)$, and $n(X_n)$, (b) gathering, operationalizing appropriately, and disseminating the data components of R, X_c, X_m, and X_n, (c) writing and disseminating the necessary software algorithms to estimate such models, and (d) estimating such models, and disseminating the theoretical, substantive, and methodological advances produced thereby.

The intellectual promise lies both in advancing and demonstrating an EITM-style modeling technique for bringing theory that previously evaded empirical evaluation due to its interactive complexity under direct empirical scrutiny and in whatever substantively and theoretically new light is thus shed. Given that complex multiple interaction is inherent in most classical (social-structural) and modern (institutional) political economy and, indeed, political theory, empirical techniques that offer powerful, estimable, and interpretable leverage on such complexity are crucial. Indeed, this particular approach of integrating theory more closely into the empirical specification to garner such added leverage should offer much promise for usefulness across (at least) the social sciences, where such complexly interactive causality is rife. The research project will also involve the next generation in these advances directly through RA-ships (indeed, the primary RA, Karen Long, has already been chosen and is already making progress as part of this proto-project and on her own). It also seeks to broaden their use in this and subsequent generations through the public provision of transparent software tools to implement such nonlinear modeling.

In the next section, I work from Franzese and Nooruddin (2003) to introduce the broader research project from a more-substantive angle. The subsequent section works from Franzese (2003a) to offer an analogous in-road from a more-methodological angle. Section 4 gathers these two strands together to itemize the proposed research, and Section 5 lists results of prior support.

2. The Effective Constituency in (Re)Distributive Politics: Partisan versus Geographic Bases of Democratic Representation

2.a. Overview: Franzese and Nooruddin (F&N 2003) begin by noting that, while scholars generally agree that democratic policymakers are at least somewhat responsive to their constituencies, theoretical and empirical heterogeneity abounds regarding exactly what defines the constituencies to which policymakers respond. (See, classically, Fenno 1978 regarding the positive, Pitkin 1969 and Burke 1774 regarding the philosophical and normative, side of this question.) Simply and solely as practical-positivist first cut, F&N suggested conceiving the potential bases of representation as being roughly a continuum, from the geographic constituency of the policymaking representative, the members of her electoral district, d, to her partisan constituency, p, who are the set of people whose interests align behind and so support her party. The *effective constituency, c,* to which policymaking representatives respond could then be expressed as a convex combination of these two extremes, with the weight on partisan/interest representation relative to district/geographic summarized by the degree of party unity, u, or lack thereof, $(1\text{-}u)$, giving $c=u\cdot p+(1\text{-}u)\cdot d$. Considering this rubric, F&N re-examine the familiar Weingast-Shepsle-Johnsen (WSJ 1981) *law of 1/n* model of distributive politics and pork-barrel spending, which holds that distributive (*i.e.*, pork-barrel) over-spending should be proportional to the number of *constituencies*, which in turn are viewed as identical to *districts*. Postwar public spending across developed democracies, however, does not support a pure-electoral-district *WSJ 1/n* model, nor does the conventional wisdom in comparative politics regarding the emphasis on personalistic relative to strongly-partisan politics across developed democracies (*US>>Italy>>UK*) accord with their numbers of districts (*435>>32<<651*). In the U.S., however, where key data to test the theory using an *effective-constituency* concept exist, they show that public spending does seem to support a *WSJ* model as modified to reflect that *effective-constituency* concept. That is, using party unity as measured by partisan cohesion in legislative roll-call voting (Cox and McCubbins 1993) for u, the effective number of parties in government for p, and the number of electoral districts for d,[1] the *effective number of constituencies*, c, in the US varies significantly appreciably over the postwar era (as p and d do not), and this variation in c aligns statistically significantly with various measures of total spending and its (argued-*cum*-assumed) more-distributive components as the *WSJ law of 1/n* would suggest. The present proposal for research essentially begins from F&N's concluding section, which offers several ideas for expanding both the theoretical and the methodological approach to the (re)distributiveness of public policy (a) beyond geographic and partisan bases of representation, (b) to incorporate theoretical models that purport to explain party unity and other relevant political-economic contextual conditions explicitly and directly into this empirical specificationm and (c), more generally, to incorporate theoretical models that purport to explain relative policymaking emphasis (pub goods, rents, and) (re)distribution explicitly and directly into a broader empirical specification.

2.b. Bases of Democratic Representation: Who do democratically elected policymakers see themselves as, and, more critically, behave as if they are, representing? Can we develop more precise and more-widely applicable theoretical and empirical conceptualizations of *constituency*, which clearly manifests differently across representative democracies over time? How do varying democratic institutions, structure, and strategic situations explain the varying effective weights on the multiple potential constituencies in policymaking? These are longstanding, philosophically important, questions. As Pitkin (1969) notes:

> "[W]riters disagree on the appropriate role or conduct for representative: should he act on his own judgment of what is in national interest, or should he be a faithful servant of his constituency's will?" Notice that Pitkin means constituency as electoral district and contrasts it to a broader constituency of "the national interest" (p. 7).

Edmund Burke (1774), in his famous speech to Bristol electors, argues with himself over the same question:

> [First:] "The happiness and glory of a representative is to live in the strictest union, the closest correspondence, and the most unreserved communication with his constituents. Their wishes ought to have great weight with him; their opinion, high respect; their business, unremitted attention. It is his duty to sacrifice his repose, his pleasures, his

[1] All three measures assume government is simply 1/3 House+1/3 Senate+1/3 President.

satisfactions, to theirs; and above all, ever, and in all cases, to prefer their interest to his own" (p. 32), [but then later:] "Parliament is not a congress of ambassadors from different and hostile interests; which interests each must maintain, as an agent and advocate, against other agents and advocates; but parliament is a deliberative assembly of one nation, with one interest, that of the whole; where, not local purposes, not local prejudices, ought to guide, but the general good, resulting from the general reason of the whole. You choose a member indeed; but when you have chosen him, he is not a member of Bristol, but he is a member of parliament" (p. 33).

F&N declare their philosophical contribution decidedly modest. They merely suggest the potential theoretical and empirical utility of conceiving the potential bases of democratic representation as some continuum from electoral-district, d, or geographically defined, to partisan/interest, p, representation. Two assumptions are implicit in defining latter extreme: *(1)* the broadest interest any representative might serve would still reflect a partisan (*i.e.*, partial) conception of national interest; *(2)* partisan subsumes interest, ideology, and identity-group representation. They mention more-than-2 bases of representation; I would explore such more-directly.

One interesting positive question: what determines the relative weight of these differing representational modes across polities over time? Although F&N suggested several possible factors, including district- and national-level electoral competitiveness, partisan polarization, and other electoral- and party-systemic factors to be explored in this project, they isolated the degree of party unity, u, (more precisely: the degree to which legislative parties vote unitarily) as a potentially useful summary statistic. As noted, they used this u, the numbers of parties in government, p, and of electoral districts, d, to guage *effective numbers of constituencies*, c, in the US. This measure (a) actually varied over time and (b) correlated with postwar US total spending and presumed more-distributive (*porkier*) components as the *WSJ law of 1/n* model of distributive politics and pork-barrel spending. Indeed, this *effective-constituency* conceptualization arose from Franzese's (1996; 2002) unsuccessful attempts to explore empirically, comparatively the *WSJ* distributive politics and pork-barrel spending model, and its implications emerge most clearly in that theoretical/substantive venue.

WSJ demonstrated that, under certain conditions (below), overemphasis of distributive politics, generally, and pork-barrel overspending, particularly, increase with the number of constituencies. *WSJ* did not, however, distinguish *electoral districts* from *constituencies*, and they defined distributive politics and pork-barrel spending very narrowly, creating two mutually reinforcing problems for the comparative empiricist. (1) Data of precision equal to theory's distinguishing of pork-barrel/distributive from other spending/politics do not exist. Indeed, all politics and spending reflect some (varying) degree of distribution, redistribution, public-good provision, and rent seeking. (2) Policymakers will likely exhibit varying responsiveness across different democratic settings to their electoral districts relative to myriad other potential *constituencies* they might serve. Moreover, these two issues are inseparable because the definition of distributive spending hinges on identification of the politically-relevant constituencies, and, conversely, the number of relevant constituencies depends on the policy at issue. F&N suggested broader conceptions both of distributive spending and of the constituencies policy-relevant thereto. From there, extending *WSJ*'s logic was exceedingly straightforward yet offered considerable gains in empirical testability and theoretical insight.

2.c. "The Political Economy of Costs and Benefits" Reviewed and Reconsidered: WSJ ask why representative legislatures routinely pass budgets that manifestly over-emphasize distribution, or *pork-barrel*. Their answer stresses the division of democratic polities into electoral districts, noting that representation everywhere is based on "a districting mechanism that divides the economy into n disjoint political units called districts" (p. 643), and defining "*distributive policy* [as] a political decision that concentrates benefits in a specific geographic constituency and finances expenditures through generalized taxation" (p. 644). They thus isolate geographic location as the distinguishing characteristic of distributive policies and politics: "Programs and projects are geographically targeted, geographically fashioned, and may be independently varied" (p. 644). Assuming legislators follow some log-rolling or universalistic norm, *WSJ* then prove overemphasis (overspending) on distributive policies an increasing function of the number of electoral districts.

To be precise, first, index the n electoral districts $i \in [1..n]$. Assume benefits, B, of pork-barrel projects concentrate in district i and grow with project size or cost, $B_i=f(C)$, which, with diminishing returns, gives $f'>0$ and $f''<0$. By definition of distributive policy, costs accrue more uniformly across all n districts: $C_i=C/n$. An individual district then faces a utility-maximization problem, $Max_c\, f(C)-C/n$, with solution $f'(C)=1/n$. The optimal project-size from the individual district's view thus increases in the number of districts. If legislatures decide democratically, without log-rolling, universalist norms, or side-payments, then all pork-barrel projects lose legislative votes $(n-1)$ to 1 because only receiving districts derive net benefits, $f(C)-C/n$, while others only pay costs, C/n. *WSJ* argue, contrarily, that legislators adopt a universalistic norm where all legislators vote for distributive bills, implying the legislature passes the district-by-district optimal, leaving pork-barrel spending strongly proportional to the number of districts. Riker (1962) shows, however, that optimal coalition-building strategies in majority-rule legislatures involve side-payments sufficient to induce bare-majority support (*minimum-winning coalitions*) for distributive projects, meaning $(n-1)/2$ other legislators must receive $C/n+\varepsilon$, which also implies overemphasis on pork proportional to the number of districts, albeit more marginally so. (Under universalism, all projects with $B>C/n$ pass; under majority-rule with side-payments, only projects with

$B>[(n+1)/2n]\cdot C$ pass.) Later scholarship deduced several reasons super-majoritarianism may indeed govern legislative decision-making. Shepsle and Weingast (1981) note that, given uncertainty over membership of minimum-winning coalitions, legislators prefer super-majorities to insure against their omission. Luebbert (1986) and Strom (1990) argue similarly regarding parliamentary government formation that, with uncertainty over legislative support, which, e.g., secret balloting or lack of party discipline may induce, coalition builders would seek super majorities. Others stress that legislative procedures affect optimal-coalition size. Carruba and Volden (2000) show all coalitions, minimum-winning to universal, possible depending on amendment openness and other procedural rules. Baron (1991), e.g., finds universalism on distributive bills unlikely yet over-provision still prevails to a degree mitigated by procedural openness. McCarty (2000) and Bradbury and Cain (2001) argue that, respectively, presidents or second chambers dampen without eliminating the *1/n* effect by—F&N inferred—adding a legislative step in which veto or amendment may occur. F&N add to these that, if voters are rationally ignorant, *C/n* may easily escape non-receiving-district voters' notice, yet receiving-district voters readily appreciate their much larger net benefit, *f(C)-C/n*. Rationally ignorant voters thus allow legislators to forge universalist log-rolls or other super-majoritarian agreements more easily *via* cooperative solutions to their iterated-prisoners-dilemma game. Such cooperation is especially likely because legislators number relatively few, have relatively homogenous interests here, and interact repeatedly and indefinitely (Axelrod 1984). Furthermore, voters' rational ignorance also facilitates side-payment arrangements because legislators will demand smaller payments to support others' distributive proposals the greater is their voters' ignorance. In the limit, rational ignorance revives WSJ's universalist scenario. Plus, the total distributive inefficiencies or side-payment excesses about which voters may rationally remain ignorant also rises with the number of districts over which such costs distribute. Thus, distributive politics generally and pork-barrel spending specifically increase with the number of districts, more strongly so as legislative behavior tends more universalistic and less minimum-winning, which tendency, in turn, heightens as rational ignorance, winning-coalition uncertainty, or legislative-rule closure to amendment or veto rises.

The logic is elegant, intuitive, and profound; unfortunately, the comparative evidence simply does not correspond. F&N simply plotted two measures of public spending and two revenue measures against numbers of electoral districts in 20 OECD countries, finding nothing. Many others, e.g., Franzese (2002: ch. 2-3), have explored varied political and/or economic controls, other fiscal measures, and/or more sophisticated modeling techniques. Regardless, little or no significant relation between numbers of districts and virtually any fiscal-policy measure in postwar samples of developed democracies emerges; i.e., comparative public fiscal-activity just does not support a simplistic application of *WSJ's* model of electoral-district-based distributive politics. The model's implications for the relative prominence of distributive politics across democracies also receive no support. E.g., the UK House of Commons has *651* electoral districts, the US House of Representatives has *435*, and Italy's *Camera dei Deputati* had (pre-1994) *32* in its first tier. The *1/n* logic suggests that the UK should exhibit distributive, i.e., district-focused, politics most prominently, followed closely by the US, and more distantly by Italy. Students of comparative developed-democratic politics would generally agree to the contrary that the actual ranking is probably the US, followed closely or possibly preceded by Italy, with the UK a distant last. Regarding the UK, Rose (1986) states unequivocally:

> ...constituency [i.e., district] representation...is of little importance to government. MPs can devote time to…the concerns of individual constituents[, and] this relationship can flatter an MP who is a small fish in a big pond at Westminster[, b]ut an MP cannot gain government favors for his constituency by trading his vote in return for local benefits; *the whip* [i.e., party]*, not constituency interest, determines an MP's vote* (pp. 100-1; emph. added).

Contrarily, party-organized and -directed *patronage* and *clientelism*, complicated theoretical concepts that include strong distributive-politics and pork-barrel aspects *inter alia*, are long-acknowledged central features of Italian democracy (Banfield 1958; Powell *et al.* and Schneider *et al.* in Schmidt *et al.* 1977). Spotts and Wieser (1986), speaking of parliament's legislative role in Italy, clarify the extent to which MP's local-service pervades the legislative agenda (*n.b.*, local civil-service jobs are the preferred clientelistic currency in Italy):

> ...the Chamber and Senate have produced a flood of legislation that generally well surpasses the output of other Western European parliaments and the US Congress[, b]ut the product tends to be narrow in scope, clientelistic in nature, and fragmented in its treatment of national problems. The great majority of these...*leggine*, "little laws" [were] devoted to bettering the condition of government employees. Fully 37% of the legislative proposals between 1963 and 1972, e.g., concerned [various] civil service [...compensation and job conditions] (110-1).

In the US, meanwhile, district-oriented politics certainly plays a much larger role than in the UK and perhaps even than in Italy, though also perhaps less "clientelistically" so.

Thus, discrepancy from theory to comparative evidence is wide (cf. Bradbury and Crain 2001) and seems not to derive from deficient methodology or controls. Contrarily, Gilligan and Matsusaka (1995, 2001) find support in comparing US states; Levitt and Snyder (1995) find indirect support in the pattern by spending category of partisan effects on the district distribution of US spending; Lee (1998, 2000) finds US Senate malapportionment to affect distributive politics consistently with the *law of 1/n*; Alvarez and Saving (1997) find that US Representatives do derive electoral benefits from spending in their districts; and Bickers and Stein (1996) find that US district spending increases with challenger quality. These conflicting results do not suggest that the *1/n* logic only applies in the US; rather, they highlight an important substantive problematic that *WSJ* and many others (e.g., Burke) ignore: namely, conflation of *constituency* with *electoral district*.

2.d. "The Effective Number of Constituencies" Concept: WSJ's *law of 1/n* equates the empirical, physical, and geographic borders of electoral districts with the conceptual, theoretical, and substantive boundaries of constituencies. Consider, instead, conceiving the number of constituencies in a political system as lying on a continuum with only one of its endpoints, that corresponding to pure geographic representation, at the number of electoral districts. Representative policymakers certainly may see themselves as representing and so act legislatively in the interests of their electoral districts, implying identity of *constituencies* and *districts*. At the other extreme, though, they may see themselves and act legislatively as representatives of the entire nation—as, e.g., presidents often claim—implying that only one *constituency*, the nation, exists. More realistically, executives or legislators may be pure partisan actors, representing the interests and ideologies of their party's supporters, which equates *effective constituencies* and governing parties in number. Thus, the US case could have any number of *effective constituencies* from *1*, if presidents fully control policy and solely represent the entire nation or, more realistically, if partisan presidents and legislators of one party share policy-control, or *2*, if president and legislators act as purely partisan representatives in divided government, to *435*, if Congress completely controlled policy and each congressperson solely represented her own district. The UK has *651* electoral districts and *2* parties, *Tories* and *Labour* (ignoring small parties). Assuming each party represents some distinct group of people, the UK has minimally *2* constituencies, *Tory*- and *Labour*-supporters, of which government usually reflects only *1*. Conversely, if voters in each individual MP's district define constituency, the UK has maximally *651* constituencies. Where along this range lies the *effective number of constituencies* that will be represented in government policy is a function, F&N argued, of the degree of party unity.

To clarify the intuition, imagine varying the degree of party unity in the UK. The more apt is a unitary-actor characterization of parties, the more an individual MP's party label determines her legislative behavior. This being so, voters will also choose party-labels more than individual MPs. Therefore, individual MPs neither act as independent legislative actors nor have much to gain by abandoning party unity to make some localistic appeal in their electoral districts. Partisan constituencies come to the fore. Conversely, the party label becomes less meaningful as the independence of MPs as legislative actors increases. Absent meaningful party labels, as electoral draws and as prescriptions for legislative behavior, individual MPs' electoral districts become more relevant to them and constituency service (including distributive projects) becomes more important to them and their supporters. Thus, the *651* electoral-district constituencies become more dominant.

Therefore, the UK's *effective number of constituencies* lies between *651* and *1*, with extremes reflecting perfect party-disunity (i.e., legislative and electoral irrelevance of party label) and perfect party-unity (i.e., legislative and electoral irrelevance of any individual MP or district characteristics). More fully, *effective constituencies* lie on a continuum from pure partisan- to pure geographic-representation, therefore a convex combination of the numbers of governing parties, p, and electoral districts, d, gives the *effective number of constituencies*, c, in a political system, and the relative weight of p increases with the degree of party unity, u, characterizing that system. F&N adopt the simplest possible convex-combination, a linear weighted-average: $c=u\cdot p+(1-u)\cdot d$ with $u\in[0..1]$. Thus, given any two countries with near-equal numbers of parties and electoral districts, more (fewer) effective constituencies exist in the system with lesser (greater) party-unity. Thus, distributive politics may be much more prominent in the US than UK, despite their roughly equal numbers of governing parties (*1-2*) and electoral districts (*435-651*), because British parties exhibit far greater party unity, making the UK's *effective number of constituencies* radically lower.

To applying this conceptualization to the *1/n* logic of distributive politics and pork-barrel spending, first, redefine distributive policies as those that concentrate benefits within a single *effective constituency* but spread costs more evenly across all *constituencies*. Then, distributive/pork-barrel overemphasis/overspending so defined increases with the number of *effective constituencies* rather than *districts*. A trivial corollary is important to the empirics evaluating this re-conceptualization below: holding constant the numbers of parties and of electoral districts, distributive politics and spending decrease with party unity. Before proceeding, and to set the stage for the proposal of future research, consider several further thought-experiments to illustrate how *effective numbers of constituencies* depend on multiple factors beyond numbers of electoral districts and governing parties and degrees of party unity.

Consider, for instance, two hypothetical UK's, each with *2* parties, *651* electoral districts, *and* the same degree of party unity. These two UKs, however, differ in the ideological distance between their *2* parties. I expect the UK with the more-distinct party-ideologies to appeal less to the pork-barrel precisely because electoral competition in that UK will be more ideological (i.e., broader interest-based). In the polarized UK, representatives and candidates compete to considerable degree on partisan-interest bases as members of two opposing teams. In the UK with little ideological distance between parties, conversely, electoral competition has less ideological content; lacking broader teams on which to base competition, distributive politics and spending comes forward. The Irish party system may exemplify a case of such relative absence of ideological conflict between the parties (on economic dimensions) fostering greater emphasis on distributive politics.

Electoral competitiveness of the districts may also enter. Imagine two other hypothetical UK's, each with *2* parties, *651* districts, and the same degrees of party unity and of partisan ideological polarization. One UK, however, has *651 competitive* electoral districts while the other has *651 uncompetitive* districts. I.e., all districts in the competitive UK have on party expecting a 51% to 49% victory; in the uncompetitive UK's districts, one party expects 100%-0%. If voters reward pork-barrel district projects with votes, both parties will have greater incentives to allow their candidates to promise, and their MP's to deliver, district projects and services in the more competitive UK. Moreover, in the district-competitive UK, distributive overemphasis increases with national-level competitiveness also because winning a marginal district is more critical. Thus, distributive politics and pork-barrel spending increase with electoral competitiveness.

Notice the similarity of the role of partisan polarization and electoral non-competitiveness in dampening distributive-policy incentives to the role of party unity, u, in the *effective constituencies* defined above: $c=u\cdot p+(1-u)\cdot d$. As party unity, partisan ideological-proximity, and/or electoral competitiveness decline, electoral districts weigh more in determining *effective constituencies*. One could, therefore, replace the constant u in this heuristic model with a function reflecting the factors that push democratically elected policymaking to represent more their partisan than their geographic constituencies. These factors would include party unity (or, alternatively, some set of conditions that induce it) but also partisan polarization, ρ, and electoral competitiveness, e. The new heuristic would be $c=f(u,\rho,e)\cdot p+\{1-f(u,\rho,e)\}\cdot d$ with $0\leq f(\cdot)\leq 1$, $f_u>0$, $f_\rho>0$, and $f_e<0$. (To ensure $0\leq f(\cdot)\leq 1$ in the empirical-model implementation, $f(\cdot)$ could be a logit or probit function.) I return to this extension of the conceptualization in the conclusion and future research.

Because all of these hypothetical UK's conduct pure single-member-simple-plurality elections, the distributive overemphasis that electoral competitiveness heightens is geographical in nature. In systems with larger district-magnitudes, electoral competitiveness would likewise foster distributive overemphasis, but *constituencies* would likely have less geographic than partisan base. Thus, using the extended heuristic above, f_e should be conditional on the electoral system with $f_e>0$ in larger-magnitude systems and $f_e<0$ in smaller (see, e.g., Long 2002: the intended lead-RA on this grant proposal). Further, the substantive and theoretical context suggests national-level electoral-competitiveness enters interactively in a specific way (see below).

In sum, I contend more fully that *(i)* the degree to which parties are able to act as units should determine the *capacity* of democratic policymakers to *budgeteer* (call that *strategic party-unity*, u_s), *(ii)* national and district-level electoral competitiveness should determine their *incentive-magnitude* in *budgeteering*, and *(iii)* party-system polarization, electoral-system and district magnitude, and the degree to which parties receive their electoral support as units (call that *representational party-unity*, u_r) should determine the nature of the *budgeteering*, the relative shares of partisan-redistributive and geographic-distributive policies, that they will pursue. F&N concentrated on the effect of different levels of party unity on representation and spending; this proposal is to undertake the substantive, theoretical, empirical, and methodological research to finish the task.

2.e. Proof of Concept 1: Preliminary Empirical Exploration of US Fiscal Policy & Party Unity, 1956-94

To evaluate the base concept, F&N required data on amounts of distributive activity, degrees of party unity, and numbers of electoral districts and governing parties. Two difficulties emerged immediately. First, unavailability of comparable measures of party unity across many countries debarred direct comparative analysis. The future work proposed here may utilize cross-US-state data effectively, and will apply nonlinear techniques to enhance empirical leverage as discussed below. Few countries appear to have the necessary legislative-vote records to produce direct measures of legislative voting-unity. For fewer still have scholars compiled such measures, almost exclusively in the US. However, whereas numbers of US electoral districts hardly changed postwar, providing almost no leverage to test the pure-electoral-district *WSJ* model directly with aggregate US data, and whereas governing-party numbers also held relatively constant, party unity varied sufficiently *and measurably* across time (Cox and McCubbins 1993) to yield empirical leverage on our *effective-constituency* concept. Thus, the conceptualization offered a practical "testability" benefit at least. Second, deciding how to distinguish distributive from other categories of spending and whether to measure such activity as a share of GDP or of total public activity is difficult. F&N used final-consumption expenditure and non-transfers spending, as shares of both GDP and the total budget, acknowledging that the measures' relative weakness rendered the results highly preliminary indeed. Mildly alleviating the limited-degrees-of-freedom problem, they *jointly* estimated four equations, each regressing a spending measure on several controls and a variable capturing our conception of the *effective number of constituencies*. Given the strong serial correlation, they estimated these simultaneous equations in error-correction format (Beck 1992), controlling for real-GDP-*per-capita* growth and levels, unemployment rates, CPI inflation rates, a pre-electoral indicator (Tufte 1978), and government left-right partisanship (Hibbs 1977).

Regarding their operationalization of the effective number of constituencies, they note:

> By measuring the *effective number of constituencies* before and outside estimation of the empirical model, we set the null hypothesis as that this measure relates (positively) to spending and as alternative merely that it does not. A more direct and revealing test would allow the data to adjudicate whether numbers of governmental parties and of electoral districts affect spending in the manner hypothesized, *i.e.*, in a convex combination with weight a function of party unity, against a stronger alternative that these factors might affect spending linearly additively or not at all (as, *e.g.*, Franzese 1999, 2002, 2003 do in monetary-policy contexts). However, as noted, US electoral-district numbers do not vary and governmental parties numbers hardly vary in our sample, rendering such more-direct and -powerful empirical evaluation of our argument impossible [at this point].

Research that would enable "such more-direct-and-powerful empirical evaluation" is, of course, precisely this proposal. As I began to sketch above, and will continue below, the key is to apply theory to substantive context to uncover concepts like "electoral competitiveness" that travel, then to apply theory again to specify as exactly as possible *how* those concepts enter the empirical model, and then to estimate that (nonlinear) model directly. Here, though, specifically for the US case, F&N measured *ENoC*, the *effective number of constituencies*, directly and prior to empirical-model estimation as:

$$
\begin{aligned}
ENoC = 0.5 \cdot & \left[U_{HD} \cdot 1 + (1 - U_{HD}) \cdot N_d^h + U_{HR} \cdot 1 + (1 - U_{HR}) \cdot N_r^h \right] \\
+ 0.5 \cdot & \left[U_{SD} \cdot 1 + (1 - U_{SD}) \cdot N_d^s + U_{SR} \cdot 1 + (1 - U_{SR}) \cdot N_r^s \right]
\end{aligned}
$$

> where U_{JK} = party unity (Cox and McCubbins 1993) amongst House or Senate (J=H,S) Democrats or Republicans (K=D,R) and N_k^j is the number of House or Senate Democrats or Republicans. This assumes House and Senate equally important in policymaking and that the president's effective number of constituencies is fixed and so may be ignored…We can modify these simplifying assumptions if that proves theoretically or empirically necessary.

"Modifying these simplifying assumptions," i.e., exploring the political economy of authority allocation across multiple governmental actors, is addressed directly in Franzese (1999, 2003) using the sort of nonlinear empirical modeling that research under this proposal would advance. Long-term plans include integrating by such methods this effective-representation project with a broader agenda encompassing authority allocation.

In this preliminary analysis, the higher the party unity, the fewer the constituencies because legislators appeal more to broadly-based ideological constituencies along party lines than to localistic interests of their electoral district. Conversely, leaders that use pork-barrel projects for their own individual district are, *ipso facto*, less responsive to their partisan and more responsive to their geographic constituency. Plotting the resulting series, *ENoC*, F&N revealed a notable upward-then-downward trend. The numbers of parties and electoral districts barely change in this period, so the pattern reflects a decline then rise in legislative party-unity. Peak party-disunity and so peak effective-constituency numbers occur in the mid-to-late 1960s, and both return to 1950 levels by 1990. If the re-conceptualized *WSJ* model is correct, distributive politics and spending should similarly rise then decline. Indeed, for all four measures of US government spending 1956-94, F&N found the coefficients on changes and levels of *ENoC* positive, as hypothesized, with the level-equilibrium relationship significant at minimally the $p<0.06$ level, and with the change-momentum and level-equilibrium effects both obtaining high significance in the final-consumption-as-a-share-of-GDP equation. Thus, the *effective number of constituencies* clearly relates positively to US federal-government final-consumption-spending as a share of GDP (*FC-GDP*); some, but less robust, evidence of positive relationships with final consumption as a share of total expenditures and with non-transfers spending as a share of either GDP or total expenditures also emerge. Substantively, the estimated fiscal-policy response to *ENoC* tracks the peaks and troughs of government consumption quite well, and the downward trend since about 1966 seems to have coincided with a rise in legislative party-unity over that time and the corresponding rise in the number of effective constituencies. In magnitude, the estimated effects are about *25%* those of the actual government-consumption path. That is, very crudely, changes in the effective number of constituencies over that period may account for about *1/4* of the developments since 1955 in federal-government final-consumption.

2.f. More "Proof-of-Concept" Regarding the Efficacy of the Proposed Nonlinear Modeling Techniques in Fruitfully Integrating Theory More Closely With Empirical Evaluation to Address Multiple, Complex Interactivity in Comparative and International Political Economy

Franzese (1999, 2003) offer further "proof-of-concept" regarding the empirical efficacy of these nonlinear modeling techniques, and the substantive and theoretical insights they enable. Franzese (1999) showed that, theoretically, any degree of central bank independence (CBI) implies that banks and governments share control over monetary policy *cum* inflation. I demonstrated that this implies that the inflation effect of *any* political-economic factor to which banks and governments would respond differently depends on the degree of CBI, and, conversely, the effect of CBI depends upon *all* such factors. I showed there how to model that convex-combinatorial theoretical prediction and the multiple complex interactions it implies empirically compactly, powerfully, and interpretably. And the evidence from the post-Breton

Woods (floating exchange-rate) era across developed democracies decidedly supported that specification over less-revealing standard linear-additive or less-revealing and highly intractable linear-interactive models. In Franzese (2003), I showed fixed exchange-rates and international monetary exposure likewise entail partial delegation: namely, from the domestic combination of central bank and government to foreign monetary authorities, respectively those controlling the peg-currency(ies) and the global set of monetary authorities. Now, the effect of all *foreign and domestic* political-economic factors generally depends on degrees of degrees of CBI, exchange-rate fixity, and global financial-exposure *at home and abroad*. Again, the nonlinear modeling techniques advanced here rendered this even more highly complexly interactive situation empirically tractable, with decidedly supportive and theoretically and substantively revealing results. In Franzese (1999), the empirical model specified a theoretically determined function reflecting what inflation would have been under direct central bank control, another reflecting the set of factors to which inflation would have responded under direct government control, and then estimated empirically the theoretically determined convex combination of those two. Franzese (2003) showed that one need simply nest that convex combination within two nested others. First, one would observe the domestic combination only insofar as exchange-rates float, to the remaining degree we would observe peg-currency(ies) inflation. Then, we would observe that nested pair of combinations only insofar as the domestic political-economy deviates from the theoretical ideal of an atomistic and fully financially-exposed country. To the remaining degree, we would observe global inflation.

2.g. Proposed Research: Extending the Effective Constituencies Concept by Using and Advancing the Nonlinear (Convex-Combinatorial) EITM Methodology

That this convex-combinatorial approach to modeling principle-agent and other shared-policy-control or diffused-responsiveness situations has proven effective in the monetary-policy context hardly guarantees that I will have any success in the present context. Moreover, while the evidence from F&N is far more suggestive than conclusive, the conceptualization of the *effective number of constituencies* seems, at the least, to have provided a means to test the *WSJ* model for the US case. The evidence from the postwar history of US fiscal policy seems to support the argument and suggests that as much as a quarter of the rising-then-falling path of US federal-government final-consumption might be attributable to a parallel path in the *effective number of constituencies*, which, in turn, stemmed from a mirror-image decline then rise in legislative party-unity. The argument and evidence above, to return to the present substantive context, also suggest that the *effective-constituencies* concept in general and, more narrowly, the argument relating it to distributive politics and pork-barrel spending might be usefully extended in several theoretically interesting and important ways. These extensions are the crux of the current research proposal.

First, F&N conceive the *effective constituency* to which policymakers respond as a simple continuum from geographic representation of electoral districts at most disaggregated to partisan representation of the sets of interests supporting political parties at most aggregated. One may alternatively conceive the endeavor as an attempt to describe the dimensions of the possible bases of democratic representation. From that broader view, F&N spanned partisan and geographic bases but may have omitted others such as functional, identity, interest, or social-cleavage representation. Their partisan endpoint may cover many of these; *i.e.*, *partisan* representatives may be seen as serving the set of interests that support their party, which may subsume interest and social groups. The sufficiency of a unidimensional continuum, though, is an empirical matter. Representatives may, *e.g.*, represent certain industrial interests in a way that cross-sects rather than reflects their partisan affiliations. For example, much comparative-politics research has suggested that *corporatist* bases of representation pervade many developed democracies (*e.g.*, Berger 1984; Lijphart1974, 1975, 1977, 1984; see Gallagher *et al.* 1995: ch. 14 and Lane and Ersson 1994: ch. 7 for textbook review).

The convex-combination approach proposed here remains useful in testing such propositions—*e.g.*, that industrial sectors act as bases of representation distinct from partisanship and geography. In that case, one would first estimate the *effective number of industries* (*i*) in the political economy using some standard approach: *e.g.*, $i=(\Sigma_j z_j^2)^{-1}$ where z_j is the j^{th} industry's share of employment or output. Then, the *effective number of constituencies*, *c*, would be given, as before, by some convex combination of the numbers of parties, *p*, of electoral districts, *d*, and, now, also of industrial sectors, *i*. Again, a linear weighted-average would be simplest, but party unity, *u*, no longer suffices to give the weight. Substantively, one possibility would be to adopt some measure of the degree of corporatist representation, *cr*, in a society from that literature; our concept of the *effective constituency*, *c*, then extends naturally: $c=cr\cdot i+(1\text{-}cr)\cdot[u\cdot p+(1\text{-}u)\cdot d]$. Another possibility would be to estimate a country-by-country constrained nonlinear least-squares regression of a distributive-activity measure on *effective numbers of constituencies* thus: $Y=\ldots+\beta[a\cdot i+b\cdot p+(1\text{-}a\text{-}b)\cdot d]+\ldots$ Then, β is the estimated impact of the *effective number of constituencies* on *Y* and *a*, *b*, and *1-a-b* are the estimated degrees of corporatist, partisan, and geographic representation, respectively, in that country. (As with the suggestion above that unit-interval bounded terms could be replaced with logit or probit functions, here multinomial logit or probit expressions could enhance accuracy of mapping from theoretical to empirical modeling.) Also, *b*/(1-*a*) here is the estimated degree of party unity in the country assuming the causal role attributed to party unity here is correct. This approach effectively assumes the degrees to which representation operates in various forms and of party unity (i.e., *a*, *b*, and *1-a-b*) are some country-specific constants to be estimated. Alternatively, one could model a and b theoretically as foreshadowed above and elaborated now.

Relating to *effective constituencies* generally and to the political economy of effective representation and (re)distributive politics and policy particularly, I suggested above that lesser partisan-polarization and greater district- and national-level electoral-competitiveness may affect the relative prominence of distributive politics and spending in a way that depends on electoral systems and district magnitudes. I began to suggest how I would model such propositions empirically under this proposal. I now elaborate these arguments and their appropriate embodiment in an empirical-model specification somewhat, stressing aggregate-level electoral-competitiveness, characteristics of the electoral system (see, *e.g.*, Carey and Shugart 1995), and the number and relative importance of various levels of government (national, regional, local, etc.).

Holding constant the number of parties and of electoral districts, and the degrees of party unity, polarization, and district-level electoral-competitiveness; national-level competitiveness likely spurs (re)distributive spending (depending on the electoral system) also. Consider, again, two hypothetical UK's, alike in all the above respects; assume specifically that the partisan competitors expect a 55-45 split in the next election in each electoral district. In one UK, though, all the 55-45 splits favor Labour, and, in the other UK, half favor Labour and half Tory. The marginal value to the incumbent party of district projects is much greater in the second UK, swinging a few districts being much more critical, and so we should expect greater distributive politics and spending there. The logic is a simple extension of Tufte (1978) and follows directly from Schultz's (1995) demonstration of a similar effect—that pre-electoral manipulation of transfer payments occurs only to the degree the coming election is expected to be close—in the actual UK (1995). Empirical exploration of this hypothesis, relating it specifically to distributive politics and spending is for this project.

Furthermore, Carey and Shugart (1995) summarize the incentives deriving from the electoral system for representatives to cultivate a personal vote, which here would imply greater emphasis on district-oriented distributive politics, by four aspects of the system: (*i*) party-leader control over the ballot, (*ii*) vote pooling, (*iii*) type and number of multiple votes, and (*iv*) district magnitude (see also Long 2002; the intended lead-RA for this project). Once again, one can model u in the *effective-constituency* concept to reflect these arguments directly (see above), and I intend to do so as I extend this project comparatively.

I now outline such complicating considerations more formally and completely as the intended research. The broader and far more ambitious project toward which this research aims is to merge insights from current *macro-political-economics* (as, *e.g.*, compiled in Drazen 2001, Persson and Tabellini 2001, and Grossman and Helpman 2002) with those of *modern comparative democratic theory* (as, *e.g.*, cited throughout this section) to generate theoretical propositions regarding the relative weight in politics and public-policymaking of pubic-good provision (security, clean air, *etc.*), broadly targeted redistribution (welfare, health, *etc.*), narrowly targeted distribution (*pork*), and rent seeking (corruption, graft, stealing). Although I expect the arguments will apply to relative weights in politics and public-policymaking quite generally, almost certainly the primary empirical insights into, and likely the clearest theoretical illustrations of, the shares of public-policy activities in these four spheres will derive from government-budget component-size and composition.

Regarding this *comparative democratic political economy of budgeteering*, I have begun to outline more fully the following view: *(i)* that the degree to which parties are able to act as strategic units (*strategic party-unity*) should determine the *capacity* of democratic policymakers to *budgeteer*, *i.e.*, to manipulate the budget for political (electoral and partisan) purposes, *(ii)* that national and district-level electoral competitiveness should determine the *magnitude* of their incentives to *budgeteer*, and *(iii)* party-system polarization, electoral-system and district magnitude, and the degree to which parties receive their electoral support as units (*representational party-unity*) should determine the *nature* of the *budgeteering* that serves policymakers' goals of gaining and retaining power and of producing their desired policies and outcomes. The combination of capacity and incentive size and nature, therefore, should determine the degree and character of the *budgeteering–i.e.*, the relative shares of public-good, partisan-redistributive, geographic-distributive, and rent-seeking activities reflected in public policies–observed across countries over time. Moreover, I believe I can now leverage theory heavily enough to specify an estimable non-linear empirical model of these processes.

Focusing on the relative partisan-redistributive *versus* geographic-distributive character of public policy, the general theory, which adds the *effective constituency* concept to *WSJ's law of 1/n*, offers the empirical model of absolute and/or relative amount of (re)distributive spending, y:

$$y = ... + f(u,\rho,e)\cdot d + \{1 - f(u,\rho,e)\}\cdot p = ...$$

with $f(\cdot)$, the relative emphasis on geographic representation and distributive policy depending on strategic and representational aspects of party unity, u, party-system polarization, ρ, and electoral imperatives (described below), e. (Ellipses indicate controls and stochastic components.) In particular, party strategic-unity, u_s, which determines the *capacity* to budgeteer, should interact with electoral imperatives, e, party-system polarization, ρ, and representational party unity, u_r, which determine the *magnitude and the nature of the incentives* to budgeteer, to determine the relative emphasis in *y* of geographic representation and distributive policy, which specifies *f(·)* somewhat more explicitly:

$$f(u,\rho,e)=\Phi\left(\beta_{us}u_s+\beta_\rho\rho+\beta_e e+\beta_{ur}u_r+\beta_{use}u_s e+\beta_{us\rho}u_s\rho+\beta_{usur}u_s u_r+\beta_{\rho e}\rho e+\beta_{\rho ur}\rho u_r+\beta_{eur}eu_r\right)$$

where *f(·)*, being a weight, is bound *0-1*, which I could assure empirically via a logit or cumulative-normal function, *Φ(·)*. Although a linear-interactive specification of *Φ(·)*'s argument assumes the effects of u_s, e, ρ, and u_r depend on each other but not on combinations of each other, the model remains highly interactive and thus, likely, empirically very difficult to estimate. However, the theory suggests more specifically that strategic party-unity, u_s, determines a *capacity* to *budgeteer*, which suggests that the data may agree that we can constrain u_s to modify proportionally the impact of the other factors, which determine the *magnitude* and *nature* of the *budgeteering incentive*:

$$f(u,\rho,e)=\Phi\left(\beta_{us1}u_s+(\beta_\rho\rho+\beta_e e+\beta_{ur}u_r)+\beta_{us2}u_s\cdot(\beta_\rho\rho+\beta_e e+\beta_{ur}u_r)\right)$$

which greatly reduces the estimation demands of this part of the model. The term that recurs in parentheses reflects the model of the magnitude and nature of the incentive to budgeteer. The theory argues that the strategic (incumbent) party-unity should determine the degree to which the government can act effectively upon that incentive, and $\hat{\beta}_{us2}$ will estimate this degree. In principle, $\hat{\beta}_{us1}$ should be zero because the *capacity* to *budgeteer* is irrelevant if there is no incentive. If the data allow this constraint also, I would have:

$$f(u,\rho,e)=\Phi\left(\beta_\rho\rho+\beta_e e+\beta_{ur}u_r+\beta_{us}u_s\cdot(\beta_\rho\rho+\beta_e e+\beta_{ur}u_r)\right)$$

Next, I unpack the *electoral imperative*, *e*, which affects both the magnitude and the nature of *budgeteering* incentives. Policymakers seek some combination of goals: to retain office and to fulfill their policy and outcome objectives. As discussed below, part of the *electoral imperative* involves the relation between aspects of the electoral system and the *nature* of *budgeteering incentives*, *i.e.*, issues surrounding how the electoral system shapes *how* policymakers might use the budget to pursue these goals. First, though, recall that, as argued, the *magnitude* of policymakers' *budgeteering incentives* depend on the degree of district-level electoral competitiveness, ec_d, and national-level governmental competitiveness, gc_n, that they expect. When they expect more competitive districts (*i.e.*, higher ec_d), any given degree of *budgeteering* will buy greater increases in their expected probabilities of winning more seats and therefore of attaining greater policy and outcome influence. Likewise, the policy-and-outcome impact of winning a few seats increases with national-level governmental competitiveness (higher gc_n); therefore, for any given ec_d, greater gc_n sharpens these incentives and *v.v.*, giving an interactive model of *budgeteering* incentive magnitude, *bim*:

$$bim=\beta_{ec}ec_d+\beta_{gc}gc_n+\beta_{egc}ec_d gc_n$$

Pushing harder on the theory, notice that, whereas the effect of governmental competitiveness should be zero when district-level competitiveness is zero because policymakers could not *budgeteer* any more seats regardless of their greater desire to do so, the effect of district-level competitiveness need not be zero even when national governmental competitiveness is zero because *budgeteering* could win a few more seats and so pursue policymakers' office-retention if not the policy or outcome goals. Again, if the data agree, this would allow constraining $\hat{\beta}_{gc}$ to zero, reducing estimation demands and giving:

$$bim=\beta_{ec}ec_d+\beta_{egc}ec_d gc_n$$

The *capacity* to react to *budgeteering* incentives, $\hat{\beta}_{us2}$, and the *magnitude* of those incentives, *bim*, should jointly (*i.e.*, multiplicatively) determine the degree of policy response. The *nature* of these incentives, i.e., *how* policymakers will respond to the incentives, in the argument so far is a function electoral system, *es*, party-system polarization, ρ, and representational party-unity, u_r. This gives:

$$f(u,\rho,e)=\Phi\left(\beta_{bim1}\cdot[\beta_{ec}ec_d+\beta_{egc}ec_d gc_n]+(\beta_\rho\rho+\beta_e e+\beta_{ur}u_r)+\beta_{us}u_s\cdot[\beta_{ec}ec_d+\beta_{egc}ec_d gc_n]\cdot(\beta_\rho\rho+\beta_e e+\beta_{ur}u_r)\right)$$

Conversely to a point made above, $\hat{\beta}_{bim}$ should be zero because the *incentive* to *budgeteer* is irrelevant if there is no *capacity*. If the data support this constraint also, that would leave:

$$f(u,\rho,e)=\Phi\left\{(\beta_\rho\rho+\beta_e e+\beta_{ur}u_r)+\beta_{us}u_s\cdot[\beta_{ec}ec_d+\beta_{egc}ec_d gc_n]\cdot(\beta_\rho\rho+\beta_e e+\beta_{ur}u_r)\right\}$$

The term in parentheses is the model (so far) of the relative geographic-distributive as opposed to partisan-redistributive *nature* of policymakers' *budgeteering incentives*; the term in brackets is the model (so far) of the *magnitude* of these incentives, and $\hat{\beta}_{us}u_s$ is the model (so far) of their *capacity* to respond strategically to these incentives. Analogous to previous points, the *nature* of the incentives is irrelevant absent *capacity* or *incentive* to react to them, so perhaps the data will allow us to drop the first term, leaving this model of the weight on geographic-distributive policy relative to partisan-redistributive policy, *i.e.*, on d, the number of electoral districts, relative to p, the number of governing parties in shaping the relative mix of these constituencies effectively represented in the budget:

$$f(u,\rho,e)=\Phi\left\{\beta_{us}u_s\cdot[\beta_{ec}ec_d+\beta_{egc}ec_d gc_n]\cdot(\beta_\rho\rho+\beta_e e+\beta_{ur}u_r)\right\}$$

At this point, I can only offer some ideas about how I might, with the funding of this proposal, operationalize, *i.e.*, conceptualize and model further or measure directly, the independent variable-terms of this model. Regarding how the electoral system shapes the *nature* of the incentive to *budgeteer*, this should depend primarily on the district magnitude, *DM*, in particular, with geographic representation and the manner in which policymakers win by assembling majorities of districts increasing proportionately as district magnitude decreases, suggesting the following, which can be measured directly, as an appropriate characterization of the electoral system for present purposes: $es=DM^{-1}$. Party-system polarization, ρ, too, can be measured directly, perhaps using data from the Comparative Manifestos Project (Klingemann *et al.* XXXX). Representational party-unity, u_r, recall, refers to the degree to which candidates (incumbents in particular) compete for and receive votes as a party unit, *i.e.*, by virtue of partisan reputation. This suggests that, if I can obtain it, a sufficient measure will be the coefficient on party-label in an appropriately specified vote-choice equation. Expected national-level governmental competitiveness, gc_n, in turn, increases (likely increasingly) as the incumbent-parties' control of policy approaches 50%. Easiest to conceptualize in a pure parliamentary system, this could be operationalized thus: $gc_n=\{E(GovtSeatShare)-0.5\}^{-2}$. Perhaps a simple ARIMA seat-forecast model might suffice to represent expectations formation here. Similarly, but *much* more tediously (*i.e.*, a job for the TA's), district-level electoral competitiveness, ec_d, would refer to how close the average competitor in the average party was to gaining representation in a district. For single-member districts, one could apply the previous equation by analogy, replacing the expected government-seat-share with expected (two-party) vote-share, but the concept is more complicated in multi-member districts. There, I would explore considering, first, how close the average competing party is to gaining another seat, which would be (a) the remainder of the expected vote-share divided by the district magnitude, *DM*, minus (b) the threshold, *i.e.*, *1/(2DM)*, then, dampening that measure of competitiveness for *one more* seat according to the share of the seats at stake in the district that one represents, i.e., multiplying by DM^{-1}. This gives:

$$ec_d=\frac{1}{DM}\cdot\sum_j\left\{remainder\left[\frac{E(VoteShare_j}{DM}\right]-\frac{1}{2\cdot DM}\right\}^{-2}$$

Again, I hope a simple ARIMA seat-forecast model will suffice to represent expectations formation.

Finally, consider how I might gauge the degree of *strategic party-unity*, which determines the *capacity* of policymakers to respond effectively to *budgeteering incentives* whose *magnitude* and *nature* is characterized as modeled and operationalized above. In the first parts of this paper, I used parties' legislative voting unity as a summary statistic for party unity. However, this direct measure is, first, inappropriate in the present context because the strategically effective legislative voting behavior from a party's view may not be to vote with unity. That is, the degree of legislative voting-unity need not reflect the degree to which parties can act in the manner the party as a unit would find optimal. Moreover, legislative voting-unity measures are not practically available in many democracies over much time and, indeed, are inherently unavailable in some over extended periods (such as in Italy, where legislative votes were secret and unrecorded until a mid-1980s legal change). The approach here will be to replace the term u_s with an estimable model of party unity: $u_s=h(\cdot)$.

For example, Carey and Shugart (1995) argue that party-leadership control over campaign funds, backbencher careers, and the ballot itself (who gets to run, in what districts or where on lists), as well as various forms of vote pooling, types and numbers of multiple votes, *etc.*, shape, in their terms, the incentives for representatives to cultivate a personal vote. (The other factors that may determine the degree of strategic party-unity may include some variables, such as district magnitude and governmental competitiveness, that enter the overall model elsewhere. I have not decided yet whether including such factors in the embedded model of strategic party-unity is beneficial and feasible.) In these terms, such factors relate more directly to the degree to which the party can act strategically in its unitary interest. Empirical measures of some of these institutional features of parties and electoral systems are available (*e.g.*, Katz and Mair 1994, Lijphart 1994). Embedding them in the proposed, estimable empirical model has the side benefits of (a) offering a measure of a concept, u_s, that is practically difficult or impossible to obtain otherwise and in some cases, and perhaps in all cases inherently unmeasurable, and (b) offering a test, albeit an indirect one, of comparative-democratic theories of the conditions that contribute to this perhaps unmeasurable, but theoretically important, concept. Of course, these side benefits will accrue correctly only insofar as the models from which they derive, i.e., the policymaking model, *y=...*, and that for party strategic-unity, $u_s=h(\cdot)$, are empirically accurate and powerful.

Virtually all of the arguments and discussion in F&N and here relate to representational aspects of democratic politics: how political-economic institutions, structure, and strategic conditions shape the effective representation in public policymaking of geographically or ideologically defined interests. Another part of the broader project, or perhaps a second broad project following it, will study how the allocation of policymaking authority across these representative and their appointees shapes policies and outcomes. Consider, for example, the number and relative importance of various levels of government. In a federal system, *e.g.*, two considerations suggest that decentralization of fiscal decision-making to local governments might mitigate the tendency toward distributive overspending that *WSJ* hypothesized. First, especially if federalism includes transferal of some fiscal authority to sub-national governments, decentralized decision-making may reduce the effective fiscal authorities' ability to externalize the costs of their locally desired spending to larger, aggregate decision-making units (see, *e.g.*, Del Rossi and Inman 1999, Jones and Sanguinetti 2000). At the regional level, the ability to concentrate benefits relative to costs diminishes simply because regions are both smaller geographically and less diverse in the interests they encompass. Second, decentralized fiscal-decision-making may induce a "race to the bottom" as localities compete for investment by lowering taxes (Peterson 1990). *I.e.*, whatever the impact of decentralized fiscal-decision-making on the *1/n* problem, it also introduces a coordination problem among regions that operates toward reducing distributive overspending. However, by reifying region and geography politically, federalism might also raise the salience of local relative to national concerns among the electorate and so among policymakers, which suggests larger pork barrels. Finally, one can distinguish between stronger and weaker unitary-states. One might well expect the relative weight of distributive politics and, thereby, distributive spending to rise the weaker the central state in this respect. These considerations, and the question of how they interact, remain open issues, but ones again that ultimately may be addressed using the broader theoretical and empirical strategy proposed here.

Some questions demand answers even at this highly preliminary stage before proceeding. Does what stands to be gained theoretically, empirically, and/or substantively from this project warrant the complexity of the *EITM* model being constructed? If so, can I compartmentalize that complexity into understandable theoretical components that will travel to other contexts beyond (re)distributive spending? That the model I am constructing may offer even a first stage in the development of a unified theory of comparative democratic political economy of *budgeteering*, *i.e.*, of the relative shares of public-good provision, redistribution, distribution, and rent seeking in public policymaking seems promise enough to me to warrant further effort. The possibilities to gauge through this approach previously unmeasurable key concepts (like u_s) and test them (even if only indirectly and conditional upon other theoretical propositions) alone seem worth some effort. I also believe the conceptualization I am constructing around the model, once compartmentalized to the component models of policymaking capacity, incentive-magnitude, and incentive nature, is intellectually manageable (*i.e.*, comprehensible) and portable across substantive context. Indeed, a book emerging from this research would construct the broad model through chapters compartmentalized just so.

2.h. Proof of Concept for the Model of Effective Representation and (Re-)Distributive Politics

I offer, lastly, a quick glance at some relevant empirics below that suggest empirical leverage may exist on the precise questions I will ask, and that the substantive findings may prove interesting and important. That, too, is crucial even at this stage early stage because, while Franzese (1999, 2002, 2003) found strong empirical leverage for and much theoretical and substantive fruit from EITM models like this for monetary policies and outcomes, nothing guarantees any empirical success from such models applied in this different way in so different a context. The exploratory empirical analysis uses annual, 1962-95, budgetary data from 19 OECD democracies–US, Japan, Germany, France, Italy, UK, Canada, Austria, Belgium, Denmark, Finland, Greece, Ireland, Netherlands, Norway, Portugal, Sweden, Australia, and New Zealand–giving 566 usable observations. I model, in error-correction format, with country indicators, *Social Benefits and Other Transfers* (SB_o), as a reasonably defensible measure of redistributive spending, and *Property Income Paid by Government (PIPG)*, as a perhaps defensible proxy for distributive spending, each as a share of GDP (all data from *OECD National Accounts: Volume II, Detailed Tables: 2002 online edition)*. The independent variables (all from Franzese 2002) include controls for temporal dynamics; government-party polarization and its product with the lagged dependent variable (*veto actors*: see Franzese 2002, ch. 3); government partisanship (*CoG*); election-year indicators (*ELE*); unemployment (*UE*); real GDP growth (*dY*) and levels (*Y*: Wagner's Law); terms of trade (*ToT*), trade openness (*OPEN*), and their product; income inequality (*RW*); and percent of the population aged 65 or older (*POP65o*). The key independent variables are numbers of governmental parties, *NoP*, and numbers of electoral districts (natural logged, adjusted for spatial size), *NED*.

Table 1:

Independent Vars. →	*NoP*	*NED*
Dependent Vars. ↙	**Full Sample**	
Social Benefits (Redist.)	+.001251 (.000471)	-.001272 (.001313)
Property Inc. Paid (Dist.)	NA	NA
	Strong Geographic Representation/District Emphasis Countries: US, Jap, Fra, UK, Can, IR, Austral, NZ	
Social Benefits (Redist.)	+.001190 (.000980)	+.001608 (.002901)
Property Inc. Paid (Dist.)	$+8.09e^{-5}$ (.000577)	+.001577 (.001702)
	Strong Interest Representation/Partisan Emphasis Countries: Ger, Ita, Aus, Bel, Den, Fin, Gre, Neth, Nor, Por, Swe	
Social Benefits (Redist.)	+.001372 (.000592)	-.002718 (.001552)
Property Inc. Paid (Dist.)	+.000280 (.000518)	$+6.73e^{-5}$ (.000602)

Among democracies whose electoral and other systemic aspects tend to foster (*i.e.*, by my very rough judgment, considering factors discussed above) geographic representation, the number of districts is positive but insignificant in predicting social benefits and other transfers (redistributive spending), whereas that variable has negative and approaching significant coefficient among countries whose political systems foster more interest representation. Meanwhile, the number of governmental parties has positive and significant effect one social-benefits/transfers spending in interest-representation countries, but only insignificantly positive effects in geographic-representation countries. Conversely, the number of governing parties has no effect at all on property income paid by government in geographic/district-representation countries while it *may* (highly insignificant, though) have some positive effect in partisan/interest-representation countries. Essentially the opposite holds regarding the number of electoral districts: it may have positive effect, perhaps appreciably so, in geographic/district-representation countries while it more certainly has quite-near zero effect in partisan/interest-representation countries. Although these are *certainly **not** close* to the empirical specifications in mind ultimately for evaluating the theory, this pattern of coefficient magnitude and significance bodes well for the potential of those more-sophisticated specifications.

2.i. The Dependent Variables and Their Measurement: One final, but critical aspect of the proposed research has escaped thorough discussion so far: how do I propose to measure the dependent variable(s) of interest: the total amount and share of public-policy activity that is distributive, narrowly, and geographically targeted (what Persson & Tabellini 2002 call *special-interest politics*) and that which is redistributive, broadly, and partisan/interest targeted? The question is especially thorny because all real policies have aspects of at least two of the four classes of policy considered—public-good provision, redistribution, distribution, and rent extraction—so the operationalization question is inherently one of degree. One approach would be to borrow other's choices, thereby essentially ignoring the issue. The usual practice is to select some clearly redistributive policy area, like welfare spending, and analyze that, perhaps adding a policy conjectured to be somewhat *porkier* than most (as I just did in *2.h*). Another approach, due to Levitt and Snyder (1995), would be to obtain geographic incidence of spending outlays of sufficient spatial granularity to calculate which budget items have greater geographic variance, and call those items *porkier*. If such granularity lacks beyond their US case, I could assume that porkier line-items in the US are also porkier elsewhere, and proceed upon that assumption. A third approach would be to offer my own substantive judgments on the relative content by these four classes of various policy items. For now, my plan is to triangulate from these three, hoping to add credibility to an ultimately debatable but central question, while continuing to look for better options.

3. Results of Prior Support

The PI has received prior support in connection with, and as as co-PI of, NSF award # S137-245-01 (Amount: $174,687; Dates: 1/10/02-4/30-04) for "Empirical Implications of Theoretical Models (EITM) Summer Training (Political Science Program: EITM Competition IIIa," which funds a series of four summer training institutes (2002-2004) in EITM, hosted at Harvard, Michigan, Duke, and Berkeley, respectively, for about 25 advanced graduate students and early junior faculty. To be exact, the PI served as one of about fifteen faculty lecturers at the first institute and is serving as the institute director this summer at Michigan. Only in the latter role, representing approximately one-fourth of the total grant and only just begun the end of 2002, does he receive and manage any support directly. Thus, *results of funding* here will refer to the first and second year of the grant. In the five months from funding to the start of the first institute, roughly fifteen faculty lecturers were recruited, a very large number of students applied from which the PI's selected 25 participants. Instructors lectured, led discussion, and designed projects, all of which produced about five-and-a-half days of full time engagement for the participants for four weeks from mid-June through mid-July. The participant evaluations enthusiastically agreed on overall quality of experience, which concluded with a coast-to-coast (participants in Cambridge; several lecturers in Seattle and elsewhere) web-simulcast of student project proposals. All lecture, assignment, and related material has been recorded and will be made publicly available in various forms. The second summer session of this EITM institute at Michigan is very similarly organized, is just completing planning stages to begin instruction next week. During the time of receiving this funding, and certainly benefiting from participation in these institutes, the author polished Franzese (2003) for publication in the forthcoming EITM special issue of *Political Analysis* 11(4).