Articles

Party-System Nationalization and the Scope of Public Policy: The Importance of Cross-District Constituency Similarity

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Abstract

Party-system nationalization is supposed to result in the provision of nationally focused policy, including spending priorities with widespread benefits. Conversely, democracies characterized by parties with geographically narrower patterns of support are suspected of parochial policies, including targetable spending. The authors show that party-system nationalization alone is not sufficient to generate national benefits. In addition, governing parties' constituents must be similar across districts. Nationalization can occur because parties are making the same appeal to similar constituents across different electoral districts, but it can also occur because parties are skillfully tailoring different appeals to diverse constituencies across districts. In the latter case, the authors expect to see the targeted spending priorities typically associated with party systems that are not nationalized. The authors test for the conditional effect of party-system nationalization in 36 elections across 20 countries using a Bayesian multinomial model and find support for their reasoning regarding the importance of cross-district constituency similarity.

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Party nationalization is the degree to which a party's level of electoral support is homogeneous across the country.[1] A consensus seems to exist around the conclusion that *high* levels of nationalization are *normatively desirable* because when nationalization is high, politicians are expected to respond to a broader constituency of supporters (Hicken, Kollman, & Simmons, 2008). Narrow constituencies prompt politicians to target goods and services to geographically concentrated and narrowly defined social groups rather than providing "public goods"—such as welfare expenditures—to the country as a whole (Bueno de Mesquita, Smith, Siverson, & Morrow, 2004; Cox & McCubbins, 2001; Hicken & Simmons, 2008; Lago-Peñas & Lago-Peñas, 2009).

The underlying logic is well developed in a broad literature. Rose and Urwin (1975) noted that parties with geographically broad support should have an integrative impact on the state, shunning regionally based policies. In very different contexts, both Schattschneider (1960) and Jones and Mainwaring (2003) argue that high levels of party-system nationalization are tied to party-voter bonds predicated on national-level factors and, therefore, patterns of spending based on those same national-level factors. Caramani (2000) argues that as nationalization increases, candidates will no longer represent local constituencies—bringing parochialism to an end. Morgenstern, Swindle, and Castagnola (2009) cite Katz (1973), Rose and Urwin (1975), and Stokes (1967) in support of their claim that "nationalization reflects both a country's political cleavages and realignments, and it influences such critical aspects of politics as the ways governments target spending" (p. 1322).

The reasoning outlined above assumes that high levels of party-system nationalization (as observed in electoral data) can be achieved only by promising the same policy program across districts and receiving roughly equal levels of support for that program everywhere. Implicit in this line of thinking is the idea that a party is able to court support from many districts with a single, nationally oriented platform because groups of party supporters across districts are of *similar makeup*—and therefore react similarly to the platform. Parties capitalize on those characteristics shared by supporters across districts to build national patterns of support.

However, there is a second route to party-system nationalization. High levels of nationalization will also occur when individual parties are skilled at marketing *different* policy promises to *different* areas of the country, thereby generating constituencies that vary substantially from one district to

the next (Katz, 1973; Rose & Urwin, 1975).[2] It was this type of party system that, in part, motivated calls for "responsible party government" in the United States (Ranney & Kendall, 1954; Schattschneider, 1942). This classic literature focused on the decentralization of American political parties and the strength of local bosses. Proponents of reform argued that rather than provide clear programmatic choices regarding the important issues of the day, the major parties were merely the patchwork of parochial interests stitched together. Stokes (1965, 1967), for example, in his work on the highly localized politics of the United States, noted the trade-off between "pork-barrel" expenditures that are required to pacify local constituents and national-based spending. According to Ranney and Kendall (1956), scholars in the area generally agreed that American parties could be characterized by "decentralization, boss-control, lack of discipline and unity on matters of public policy, and a consequent general irresponsibility that makes them incapable of accurately expressing the popular will and faithfully translating it into governmental action" (p. 153). Given their powerful electoral machines, they were capable of generating support at roughly equal levels across much of the country—but they did not do so by articulating national policy programs that they then implemented once in office.

In sum, party nationalization and cross-district constituency similarity need not go hand in hand. Although instances in which nationalization and constituency similarity are either both low or both high make immediate sense intuitively, there is nothing precluding the possibility of observing situations in which one of them is high and the other is low. Instances of high nationalization but low district similarity can ensue when parties tailor their appeals to different types of voters across different districts. The American example above is one such case. Conversely, the realignment from the national to the local arena as the locus of electoral politics in many advanced democracies (Scarrow, 2007) can result in party constituencies that look similar across districts but parties themselves that are not nationalized.

The possibility of observing high levels of party nationalization but low levels of cross-district constituency similarity led Morgenstern et al. (2009) to point out that high levels of party nationalization, as measured with vote distributions, may indicate two observationally equivalent states that have highly divergent policy implications. Specifically, the types of government spending programs that it would take to cultivate this level of nationalization across these two scenarios are very different—as would be all other public policies devised with electoral goals in mind. The two roads by which nationalization can be achieved (and all the gradations between them) are obscured if no attention is paid to the similarity—or lack thereof—in politically salient characteristics of parties' constituencies across districts. Given

that the two possible roads to high levels of party nationalization create divergent incentives for how governments craft policies, therefore, simply accounting for the level of nationalization should yield null results when trying to explain policy outcomes.

To distinguish between the effects of nationalization achieved by either of the two possible roads outlined above, we estimate a conditional model that interacts a measure of party nationalization with a measure of sociodemographic similarity of party constituencies across districts. We use this conditional measure to explain the extent to which national governments choose to spend more on nontargetable categories than on geographically targetable ones. In this way, we join a burgeoning group of scholars who—in the process of shifting the locus of analysis to the *effects* of party nationalization—are capitalizing on earlier studies of the *causes* of party system nationalization (Hicken et al., 2008; Lago-Peñas & Lago-Peñas, 2009; Simmons, Hicken, Kollman, & Nooruddin, 2011). Like the other authors, we attempt to shed light on the general but primordial question of *whether* and under what conditions party nationalization should be expected to matter for policy.

Our analysis of party nationalization as a potentially conditional determinant of the scope of government policy proceeds as follows. First, we elaborate on the two routes by which a country's government might arrive at a high level of nationalization of its member parties. In the process we illustrate how what may appear to be straightforward means of accounting for the conditionality of nationalization's effect come up short.[3] We then extrapolate from the nationalization of individual parties to the notion of "government nationalization," employing this concept in interaction with cross-district constituency similarity to make specific predictions about the scope of public policy in general. Finally, we describe the various data sources we have brought together to test our hypotheses and report the results of our empirical investigation. We find convincing support for the hypothesis that broad, nationally oriented spending priorities result from government nationalization *only in combination* with party constituencies that are similar in important sociodemographic terms across the country's districts. Government nationalization achieved where cross-district heterogeneity of constituencies is present, on the other hand, leads to the same parochial spending patterns previously hypothesized to result from party systems that had failed to achieve nationalization.

Challenges Posed by Accounting for the Two Roads to Party Nationalization

As we noted above, parties can achieve homogenous vote shares across districts using two very different strategies. Either they can run on truly

national platforms, receiving relatively similar levels of support across districts because like constituents react similarly to that platform, or they can develop skillfully tailored platforms, catering to heterogeneous sets of constituents without developing a nationally oriented program. It is because of these two paths that we argue that party-system nationalization is, therefore, a necessary but insufficient condition for putting an end to pork-barrel or parochial politics.

Although previous works on the *causes of* party-system nationalization have diligently pointed out that there might be an observational equivalence problem based on the two routes through which it can occur, the existing literature on the *consequences of* party-system nationalization fails to account for the possibility of very different underlying constituencies. Simply accepting a vote-based measure of party-system nationalization as an indicator of the incentives a government has to provide certain types of goods will lump together cases where we would expect very different types of governmental priorities.

Coping with the problem of observational equivalence at the high end of the party nationalization metric is no easy task. Two seemingly straightforward methods of doing so—namely, controlling for national-level demographic heterogeneity and controlling for the size of electoral districts—both fail to adequately address the crux of the problem. Accounting for demographic heterogeneity at the level of the country as a whole, for example, does not take into consideration important nuances that would prompt parties to employ different electoral strategies. On one hand, it is possible that districts are demographically homogenous *internally* but at the same time very dissimilar *to one another*. On the other hand, it is possible for districts to be demographically heterogeneous *internally* but at the same time very similar *to one another*. In both cases, a measure of demographic heterogeneity at the national level will register a highly fractionalized electorate. However, we would expect that parties would respond quite differently in these two scenarios. In the former scenario, for example, a party that is highly nationalized becomes so by skillfully tailoring its electoral messages to the *different* types of constituencies in each district. In the latter, a highly nationalized party has clearly become so by offering the same message to the *same* demographic constituency in each district. More generally, as Morgenstern et al. (2009) make clear, careful examination of the district-level composition of the electorate is necessary to sort out the precise source of nationalization.

The second potential approach would be to use the size of electoral districts, measured in terms of population or district magnitude as a proxy for constituency heterogeneity. This approach inappropriately assumes that bigger districts will be more similar to one another, while perhaps more *internally*

heterogeneous. By contrast, when a country's national electorate is divided into more districts (of smaller magnitude), this line of reasoning assumes that districts become less heterogeneous *internally* but resemble *one another* much less. As a byproduct of this reduction in magnitude, then, it is argued that cross-district constituency similarity will possibly decrease (Bochsler, 2010). But this is not necessarily the case, as models like the one proposed by Alesina and Spolaore (2003) would suggest. In addition, as we argued above, this logic fails to account for the fact that a party has a specific *constituency* of voters in a district that (at least most of the time) is *not* synonymous with the district itself. What truly matters is cross-district similarity or dissimilarity of a party's supporters, not the internal homogeneity or heterogeneity of each electoral district or even how different districts are from one another as a whole.

This last point is important because it suggests that even cross-district measures of heterogeneity evaluated at the district level may not be enough if parties carve out particular types of constituencies within each district. By carving out niches of support, a party may appeal to similar types of voters within districts that, as a whole, look very different from one another. In such cases, concluding that party constituencies are different across districts because districts as a whole have different compositions would constitute an inferential error. For these reasons, then, district size (itself a proxy for cross-district heterogeneity) cannot serve as a good proxy for the concept of actual interest.

The best strategy to correct for the observational equivalence at the high end of the nationalization spectrum is to measure a party's cross-district constituency similarity directly. Recent survey work allows us to capture constituency similarity along several electorally relevant dimensions, which we then use to gauge how similar constituencies are across all these dimensions. Before we move on to how this measure can be constructed, we first explicitly state the hypothesized relationship among government nationalization, cross-district constituency similarity, and the scope of national policy.

## The Conditional Effect of Party-System Nationalization

We have reasoned that the nature of the effect of party-system nationalization on the scope of policy is not independent of the extent to which parties' cross-district constituencies do or do not resemble one another. The necessary-but-insufficient form of the effect of nationalization means that it *interacts* with the level of cross-district constituency similarity to produce more nationally oriented policies. The conditional nature of this connection must

therefore be appropriately accounted for if one is to avoid the problem of observational equivalence we have discussed.

In other words, our theorizing suggests that high levels of nationalization are *not* always "good"—in the sense that they may not always generate nationally oriented policies. High levels of party system nationalization in conjunction with party constituencies that are demographically similar to one another should produce a "better" (i.e., less parochial) outcome than high nationalization paired with constituencies that are dissimilar from one another. The logic behind this is that parties that achieve homogenous electoral returns across dissimilar constituencies are expected to have done so by appealing to the local concerns of their support base in each district—a procedure that is not necessary when the same set of appeals resonates across districts in similar ways, which is more likely to occur the more indistinguishable each parties' supporters are across districts.

On the other hand, low levels of nationalization are always "bad" in the sense that they lead to regionalized politicking and particularistic budgeting. In other words, if party system nationalization is low, there is no expectation of a conditional relationship with cross-district constituency similarity. Regardless of cross-district constituency similarity or dissimilarity, parties with regionalized patterns of support are likely to pursue spending they can target on their constituents. Our observations of (government) nationalization and cross-district constituency similarity are distributed widely across values of each, with a very low correlation between the two (viz., .17; also see the scatterplot with triangles in Figure 1). In other words, empirically we do observe cases of low nationalization combined with both high cross-district constituency similarity and low cross-district constituency similarity (not to mention cases of high nationalization paired with both high cross-district constituency similarity and low cross-district constituency similarity).

Of course, not all parties figure equally in policy outcomes. In trying to capture how parties' patterns of support and cross-district constituency similarity ultimately influence policy outcomes, we focus on parties in power. In both parliamentary and presidential systems, policy outcomes depend greatly on the composition of the executive and its support in the legislature. Given the executive's prominent place in the budget-making process, this is especially true regarding spending priorities. For example, when seeking to explain the impact of parties on the size of government, Blais, Blake, and Dion (1993) argue that examining the parties that compose the executive is "predicated on what we take to be a well-established proposition, which is that in advanced democracies political power is basically exercised through the cabinet" (p. 49). Similarly, Austen-Smith and
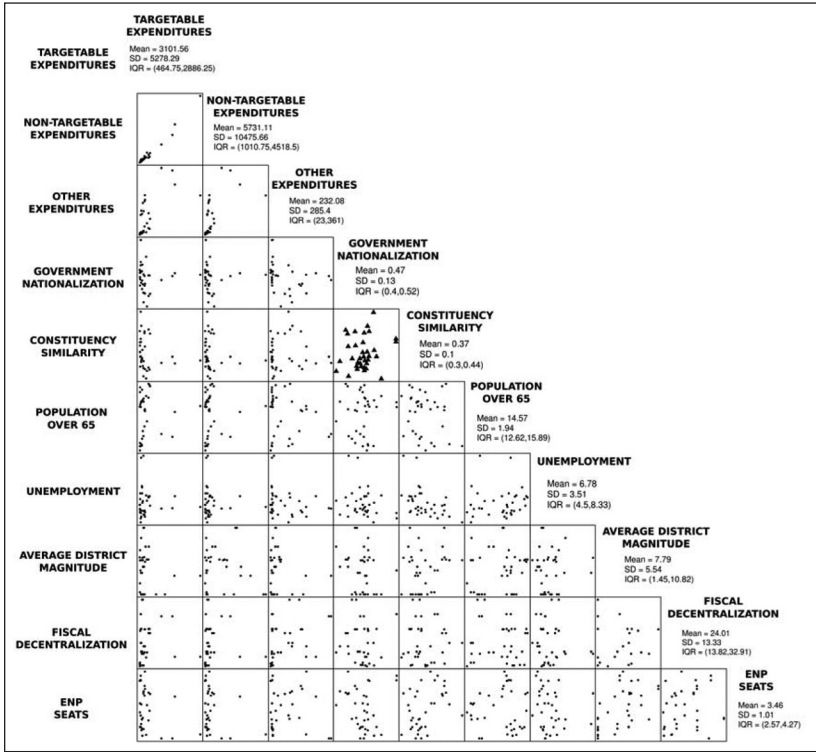
Figure 1. Descriptive statistics and bivariate scatterplots for response variable categories and for explanatory variables.
All expenditures are collected in hundreds of millions of the country's national currency.

Banks (1990) argue that "government policy . . . will be directly related to the underlying allocation of portfolios" (p. 891). Hence, parties in the government are expected to play a central role in shaping national policy, so long as their legislative participation can help them translate this advantageous position into actual policy-making power.

Given this logic, we create what we call a *government nationalization* measure. First we aggregate the votes of all the parties in the government coalition to generate a single government vote share distribution across districts. This allows us to calculate the nationalization of the coalition as a whole, thereby accounting for situations in which each individual party in the government coalition might have a low level of nationalization but

combined their patterns of support mean many districts are effectively represented by the coalition as a whole (because parties draw support from different parts of the country). As we describe in more detail below, we use the Gini coefficient–based measure developed by Bochsler (2010) to capture nationalization. Finally, we multiply our measure of government nationalization by the total legislative seat share held by all parties in the government. This has the effect of weighting the government's level of nationalization by its strength in the legislature. We would not expect minority governments, or even governments with slim majorities, to be as capable of pushing through their spending priorities as a government with a large majority of seats.

Having shown that not all party-nationalization is created equal and that we need to move from a party-system measure to a government measure, our theorizing leads to the following hypotheses:

Targetable Expenditures

> *Hypothesis 1a:* Increasing the level of government nationalization will *decrease* the proportion of expenditures allocated to *targetable* budget categories only when cross-district constituency similarity is high.
>
> *Hypothesis 1b:* When cross-district constituency similarity is low, increasing the level of government nationalization should actually *increase* the proportion of expenditures allocated to *targetable* budget categories.

Nontargetable Expenditures

> *Hypothesis 2a:* Increasing the level of government nationalization will *increase* the proportion of expenditures allocated to *nontargetable* budget categories only when cross-district constituency similarity is high.
>
> *Hypothesis 2b:* When cross-district constituency similarity is low, increasing the level of government nationalization should actually *decrease* the proportion of expenditures allocated to *nontargetable* budget categories.

We submit our line of reasoning to an empirical test by focusing on a particular type of policy that can easily be compared across many cases and

periods—namely, the national budget. In the following sections we describe our data and then the results of our analysis.

Data

Government nationalization is a measure of the static nationalization score of the government coalition as a whole weighted by the coalition's share of lower-house seats. The government coalition *nationalization* is calculated using the standardized, Gini-based Party Nationalization Score measure developed by Bochsler (2010), applied to the sum of all coalition members votes within districts (so that the coalition as a whole is treated as a single party in Bochsler's terms).

The literature on how to measure a party's level of electoral nationalization has developed three main types of instruments—namely, those based on (a) inflation factors, (b) variance estimates, and (c) the Gini coefficient.[4] Inflation-factor-based measures capitalize on the differences between the effective number of parties in the national party system and the effective number of parties at the district level. It was largely developed after Cox's (1999) work connected elite strategic behavior to nationalization through a phenomenon he called "linkage." Variance-based measures, on the other hand, attempt to capture nationalization through an estimation of how much cross-district variation there is in the vote shares obtained by a party. Most recently, these measures have been advocated by Morgenstern et al. (2009), who suggest using a random-intercept multilevel modeling approach. Finally, Gini-based instruments make use of the well-known inequality measure by generating the cumulative distribution function of a party's vote shares across districts (i.e., the equivalent of a Lorenz curve) and comparing it to the line that would ensue if all districts provided a party with exactly the same share of its total vote.

Each type of measure has some type of shortcoming, including a lack of upper limits, lack of scale invariance, and insensitivity to the number and size of both districts and parties (Bochsler, 2010; Morgenstern, Polga-Hecimovich, & Siavelis, 2011). With these problems in mind, however, Bochsler (2010) designed a Gini-based measure that, in addition to being easily understood given its longstanding use in the income inequality literature, is also easily modified to correct for the majority of issues with previous measures. In particular, it takes into account both the within-country differences in the sizes of districts and the across-country differences in the number of districts into which a country is partitioned—making it the most

appropriate measure of static nationalization for comparative work (Morgenstern et al., 2011).[5] Bochsler's measure is defined by,

$$
PNS = \left(2 \frac{\sum_{d=1}^{D} \left(v_d \left(\sum_{j=1}^{d} p_{j-} \frac{p_d}{2} \right) \right)}{\sum_{d=1}^{D} v_d \sum_{i=1}^{D} p_i} \right)^{\frac{1}{\log(E)}} ,
$$

for a party (or, in our case, for the superparty that is formed when considering all government coalition members at once) p in system with D districts (ordered according to the increasing vote shares of p) and E *effective* districts.[6] Each district d has a total of vd voters, pd of whom vote for the coalition p. After obtaining this measure for the coalition as a whole, we multiply it by the share of legislative seats held by all parties in the government coalition to obtain a final measurement of government nationalization at the level of the country.

Our second explanatory variable of theoretical interest is *cross-district constituency similarity*. This is a measure of how similar or different a party's (or, in our case, a government's) supporters are across electoral districts in terms of demographic characteristics usually associated with political preferences. Using data from the first two waves of the Comparative Study of Electoral Systems (CSES), we obtained information on the age, income quintile, education level, employment status, urbanness of the place of residence, and union participation of survey respondents who, in any given electoral district, reported supporting each of the parties in government included in our study.[7] By obtaining the median or modal category of a party's supporters in each district (depending on the measurement level used for each variable), we constructed a district-level summary of each of the six sociodemographic characteristics describing a party's constituency in each district.

For every party in government, we calculated Krippendorff's alpha—typically used as a measure of intercoder reliability (Krippendorff, 2004)—across the country's electoral districts. To aggregate up to the level of analysis at which we have posed our hypotheses, and to account for the importance of each party's size *within* the government, we calculated the weighted average of the alpha measures for each party using share of portfolios as the weighting variable.[8] This results in a single measure of sociodemographic constituency similarity across districts for each government in a given election year. Krippendorff's alpha is best known for its flexibility in handling different

levels of measurement—which is certainly true of the dimensions used to capture the various sociodemographic traits listed above.

Although it is perhaps not obvious at first glance, on a number of dimensions the problem of measuring cross-district constituency similarity is akin to the problem of gauging intercoder reliability. As an analogy, let each district represent a coder, and let each of the six demographic dimensions be one of six variables being classified or coded. High levels of cross-district constituency similarity would then be equivalent to high levels of intercoder agreement or reliability.[9] Krippendorff's alpha ranges from –1 to 1, with –1 indicating minimal similarity across districts and 1 indicating perfect similarity.

Our outcome variable is *the targetability of government spending*. Although it is hard to establish which portions of the budget are used as geographically targeted "pork," the literature has isolated categories that are most and least susceptible to being used as such (Keefer, 2007; Lago-Peñas & Lago-Peñas, 2009; Milesi-Ferretti, Perotti, & Rostagno, 2002). Specifically, investments on capital and on fixed capital (e.g., roads and bridges) have been identified as being the areas of budgets most amenable to being used as targeted pork.

Conversely, transfers made on social security have been identified as the budget category with the greatest national scope, making them the best example of nontargetable expenditures. Based on the categorization of Milesi-Ferretti et al. (2002), we have calculated a measure of both targetable and nontargetable portions of the budget for every observation in our sample, as proportions of the total amount spent by the government in that year's budget, according to the Organisation for Economic Co-operation and Development (OECD). Specifically, our measure of nontargetable spending is defined as social security benefits paid by the government, whereas our measure of targetable spending is defined as the sum of current and capital spending on (fixed) goods and services—or the sum of government consumption and capital spending (Milesi-Ferretti et al., 2002, p. 629), measured *on the year following the observed election*.[10] We introduce this lag to ensure we are observing the budget proposed and approved by the government for which we have obtained nationalization measures and for which the electorate is most likely to hold the elected government accountable.

Following Lago-Peñas and Lago-Peñas (2009), we use as our control variables the percentage of the population older than 65 (*age*) and the unemployment rate (*unemployment*) given their likely impact on spending priorities. The first two of these variables are a standard battery of controls in the public spending literature. The OECD measures them annually, and

we have collected the data corresponding to every observation in our sample. We expect age and unemployment to positively affect our measures of nontargetable spending, since it is mostly composed of social security transfers. Unemployment should have a positive impact on our measure of targetable spending because it is possible that governments use fixed capital investments to reduce unemployment in certain areas of the country, whereas age should indirectly affect targetable spending by making nontargetable spending more pressing, thereby reducing the amount of money available for pork.

We also include controls for the average district magnitude (*magnitude*), the effective number of parties measured in seats (*ENP legislative*), and the degree of fiscal decentralization (*fiscal decentralization*) under the assumption they may affect both government nationalization and spending priorities. The average district magnitude was taken from the Electoral Systems and the Personal Vote data set (Johnson & Wallack, 2007). The link between spending priorities and electoral institutions—in particular, district magnitude—is well studied (e.g., Lizzeri & Persico, 2001; Persson, Roland, & Tabellini, 2007; Persson & Tabellini, 2005). Political parties subject to majoritarian electoral rules and in systems with smaller district magnitudes face incentives to opt for targetable, pork-barrel-type allocations. The winner-take-all nature of the systems force politicians to seek swing votes in marginal districts rather than swing votes in the population at large. Conversely, as districts increase in size, parties seek out the support of more diversely composed coalitions of voters. This has led to the expectation that higher average district magnitudes should increase nontargetable expenditures and decrease targetable ones (Milesi-Ferretti et al., 2002; Persson & Tabellini, 2005). We control for the effective number of legislative parties in the legislature to account for the possibility that there are simultaneously more potential veto players in the legislature and a greater number of non-nationalized parties in the government, which would effectively result in a spurious relationship between some of our variables of interest. Finally, when the authority to collect and allocate tax moneys is delegated to the subnational levels of government, we would expect the balance of targetable and nontargetable goods in the *national* budget to be quite different from instances in which the central government is primarily in charge of administering public resources. Because it is reasonable to expect this level of fiscal decentralization to be related both to party-system nationalization (see, e.g., Chhibber & Kollman, 2004) and the homogeneity of party constituencies across districts, we include a measure of fiscal federalism (*fiscal decentralization*) devised by Treisman (2007).

Figure 1 presents summary statistics for the variables included in our analysis as well as bivariate scatterplots among them. The data amount to 36 country-year observations of 20 countries across Australia, North America, Western Europe, and Eastern Europe,[11] displaying significant variation in all the explanatory variables of interest despite the fact that data availability represented the main constraint when selecting cases. The measures of government nationalization were constructed using more than 20,000 district-year observations of electoral results, and the constituency similarity measures were calculated using data from almost 64,000 CSES survey respondents. Although the final number of observations is small, we are able to isolate robust statistical results, which we present in the following section.

Analysis

As a reminder, our argument is that party-system nationalization will lead to more nationalized policies (specifically, lower levels of targeted spending) only if the sociodemographic similarity of the government's constituencies across electoral districts is high. This amounts to a conditional relationship between government nationalization and the scope of policy, which is best captured with an interaction between nationalization and our measure of cross-district constituency similarity (Kam & Frazese, 2007).

The data on the policy we are interested in modeling, budgetary allocations, are compositional in nature—that is, they are bound to add to a given quantity, namely, the total primary government expenditure for the year under study. In general, the government chooses how to distribute the money it intends to spend among three mutually exclusive categories: targetable, nontargetable, and other types of expenditures.[12] The most direct way of modeling the process by which this type of data is generated is through the use of a multinomial distribution, governed by probabilities defined to be a function of a linear combination of our variables of interest, and with an n parameter equal to the amount spent by the policy maker (in national currency).[13] More specifically, we model the vector of moneys going to each category $\mathbf{Y}$ for country c during year y as,

$$Y_{cy} \sim \text{multinom}(\mathbf{p}_{cy}, n_{cy})$$

with a probability vector $\mathbf{p}_{cy}$ such that,

$$p_{cyk} = \frac{\exp(\mathbf{X}_{cy}\beta_k)}{\sum_{k=1}^{3}\exp(\mathbf{X}_{cy}\beta_k)}$$

$$\beta_1 \equiv 0 \text{ (for identification purposes)}$$

where $\boldsymbol{\beta}_k$ is a column vector of category-specific coefficients and $\mathbf{X}_{cy}$ is a vector with values for the interaction between our measure of constituency similarity and government nationalization (and its main effects), along with values for all control covariates.

We fit this model using Gibbs sampling,[14] and we obtained posterior densities for all the coefficients of interest.[15] The model fits the data well, with a deviance test statistic comparing it to the null model well into the tail of a χ^2_9, and model estimates are not generally sensitive (with an average Cook's distance of 0.04) to removing any of the observations from the data set—a matter of particular importance given the relatively small number of observations in our study.[16] For ease of interpretation regarding the results of the interaction term, and to improve Markov chain mixing, all coefficients have been mean centered. Table 1 summarizes the posterior densities by providing their medians (as point estimates) along with their 0.025 and 0.975 quantiles (as their 95% credible intervals).

The coefficient distributions summarized in Table 1 reveal the effects of each covariate on the amount of money spent on either targetable or nontargetable categories vis-à-vis other types of expenditures. Because we have mean centered the covariates, these can be considered average effects. The different intercepts indicate that, for an average country, more resources are dedicated to nontargetable than to targetable categories, when compared to other types of expenditures. These results also indicate that, again, for an average country (and, most important, given an average level of district similarity), increasing government nationalization leads to a discernible increase in both targetable and nontargetable expenditures, when compared to other categories. Similarly, holding all else constant and given a country with an average level of government nationalization, increasing the level of constituency similarity of parties in the government leads to a decrease of targetable expenditures and to an increase of nontargetable expenditures (although the probability of this last effect taking place is less than 0.80)—once again vis-à-vis other types of expenditures. These results are an *unconditional* snapshot of the effects of nationalization and constituency similarity—that is, they speak of these effects only when the values of the other variable are held constant at a given value. The theorizing discussed above derives richer expectations, however. To fully test their validity, we need to show what happens to the levels of expenditures when government nationalization is high as cross-district constituency similarity, the conditioning variable, spans its observed range.

Table 1. Bayesian Multinomial Estimates of Coefficients in Models of Targetable and Nontargetable Spending, Along With 90% Credible Intervals.
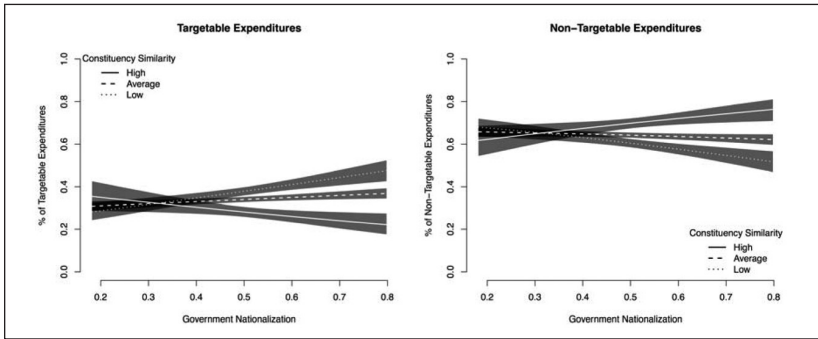
	Targetable	Nontargetable
	Estimate	Estimate
	(95% credible interval)	(95% credible interval)
Intercept	2.740	3.380
	(2.711, 2.770)	(3.351, 3.410)
Nationalization	2.149	1.766
	(1.903, 2.396)	(1.526, 2.007)
Similarity	−1.243	−0.220
	(−1.584, −0.904)	(−0.551, −0.114)
Nationalization × similarity	−3.653	1.956
	(−6.329, −0.896)	(−0.652, 4.637)
Age 65	0.277	0.240
	(0.256, 0.299)	(0.219, 0.261)
Unemployment	0.128	0.109
	(0.115, 0.140)	(0.096, 0.121)
Average magnitude	0.011	0.000
	(0.006, 0.017)	(−0.006, 0.005)
Fiscal decentralization	−0.027	−0.040
	(−0.029, −0.024)	(−0.042, −0.038)
ENP legislative	−0.554	−0.540
	(−0.585, −0.521)	(−0.571, −0.508)

$N = 36$. P(D): D ~ chi-square(9) = 0.000.

To evaluate our claims regarding the *conditional* effect of government nationalization, we obtain the predicted shares of an average country's total expenditures that are destined to targetable and nontargetable categories as both government nationalization and cross-district similarity of a government's constituencies span their observed ranges. The two panels in Figure 2 present these predicted shares as they vary with increasing government nationalization for three different levels of average constituency similarity—lowest observed value, average observed value, and highest observed value—along with 95% credible intervals around the predictions.

In general, all our expectations are borne out, although our evidence suggests that the conditional effects are stronger for nontargetable (right panel of Figure 2) than for targetable expenditures (left panel of Figure 2). Given an

Figure 2. Predicted shares of targetable spending (left) and nontargetable spending (right) by level of government nationalization, given different values of district similarity, with 95% credible intervals.

average level of government constituency similarity, the effects of government nationalization are almost nonexistent—that is, the dashed line in both panels is nearly flat. This is evidence that when similarity is not properly taken into account, the real effects of nationalization are effectively "averaged out," explaining previous null findings (Lago-Peñas & Lago-Peñas, 2009).

Targetable Expenditures

As suggested in Hypothesis 1a, and holding all else constant, increases in the level of government nationalization result in less money going to targetable categories of the budget *only when* constituency similarity is high (i.e., the solid line on the left panel of Figure 2 has a negative slope throughout the observed range of nationalization). More specifically, increasing nationalization from 0.19 to 0.8 (i.e., its observed range) is expected to bring down the percentage of targetable expenditures from 38% to about 20% of the total amount spent by governments.

This effect is reversed when similarity of constituencies across districts is low, as predicted in Hypothesis 1b. As government nationalization increases, so does the share of expenditures dedicated to targetable categories—from about 28% to almost 50% of the total amount spent by governments. Furthermore, these effects are statistically discernible from one another for higher levels of government nationalization (i.e., the probability that they are different is 0.95 or greater, given that their 95% credible interval bands do not overlap at high levels of nationalization).

For lower levels of nationalization (i.e., below 0.45), the predicted proportion of expenditures dedicated to targetable categories is expected to be the same across levels of district similarity, changing only slightly as nationalization increases. This can be inferred from the fact that at such low levels of government nationalization the credible interval bands for the three effects almost fully overlap with each other, indicating that the conditioning effects of district similarity operate only once a certain level of government nationalization is achieved. This is also the case with nontargetable spending categories.

Nontargetable Expenditures

Also, as expected under Hypothesis 2a, and holding other relevant covariates constant, higher levels of governmental nationalization lead to greater expenditures on nontargetable categories only when cross-district constituency similarity of parties in the government is high (i.e., the solid line on the right panel of Figure 2 has a positive slope). Nontargetable expenditures are predicted to compose more than 70% of a government's expenditures when both cross-district constituency similarity and government nationalization are high. Alternatively, when similarity is low, increasing government nationalization is expected to *decrease* the share of a government's expenditures dedicated to nontargetable categories—bringing it down to a little less than 50% of a government's expenditures (as expected under Hypothesis 2b).[17] Once again, there is enough evidence to suggest the probability that these effects are not different for high and low levels of similarity is less that 5%, given the lack of overlap in the credible interval bands.

The marginal effects of our control variables are relatively small when compared to the effects of nationalization and district similarity, but they are nevertheless statistically discernible from zero. Increasing the percentage of people older than 65 and district magnitude are predicted to increase the relative amount of money spent on targetable (and nontargetable in the case of the former) categories by relatively small amounts, whereas fiscal federalism and the effective number of legislative parties both result in decreases in both types of expenditures vis-à-vis other types of expenditures.

Conclusion: "Conditional" Does Not Mean "Irrelevant"

After obtaining nationalization scores for each government in our study, we tested the conditional statement relating these measures to parochial or

national policies. Being able to offer a better specified test of the claim usually found in the literature on party-system nationalization, we found compelling evidence to support our hypothesis that only *some* nationalized party systems will generate public policy outcomes that are national in scope. We found that although government nationalization increases the national scope of budgetary policy and reduces its parochialism, this increase largely depends on how similar government constituencies are across countries.

We have shown that party-system nationalization (or government nationalization, more specifically) alone cannot account for spending choices. From that, can we conclude that party systems are simply not analytically relevant and that we should instead focus on the incentives created by the composition of each party's constituency? In other words, is cross-district similarity or dissimilarity of constituencies all that matters when it comes to understanding the scope of representative government? The answer, as we show below, is no: Party systems play a significant role in making the res publica truly nonparochial.

Those who discuss the possibility of random districting (see, e.g., Ford, 1997; Polsby & Popper, 1993; Rehfeld, 2005)[18] point out that assigning voters randomly to an electoral district (regardless of geographic location) will result in districts that are as similar as possible to each other (on average). This, they go on to argue, should generate incentives for all politicians to engage in practices that reflect the "national will." This line of reasoning suggests that party nationalization should follow automatically from the fact that voters look roughly similar across districts—which in turn renders party nationalization superfluous as an explanatory variable. If this argument is correct, we should focus solely on cross-district *composition* of electorates, rather than on understanding the geographic patterns of support that bring parties to office.

Our empirical evidence, however, runs contrary to this argument. Not only do we find instances in which it is in fact possible for the parties in a government coalition (and therefore the parties that compose it) to be non-nationalized as a whole when their corresponding constituencies are similar to each other, but we also have statistical evidence suggesting that constituency similarity is not, by itself, enough to generate policies that are nonparochial in scope. If cross-district composition is all that matters, the effect of cross-district constituency similarity on nontargetable spending should always be positive and should not vary systematically with different values of party nationalization. Our results indicate that the marginal effect of average constituency similarity is never statistically discernible throughout the observed range of government nationalization values. In other words, simply having constituencies

that look like each other is *not* sufficient for encouraging politicians to adopt policies that are truly national. Both elements—party-system nationalization and cross-district constituency homogeneity—must be present to incentivize politicians to provide public goods.

The result emphasizes the importance of parties in the policy-making process. Party systems still play a significant role in the type of representative government that should be expected in a given political system, including the scope of policies the government typically enacts. In other words, parties and party systems continue to be key players in the representation of the "national will." Our findings should therefore encourage further research into the implications of party-system nationalization that is cognizant of nationalization's interplay with cross-district constituency composition. In particular, and now that we have a firmer grasp on *whether* nationalization affects policies, studies can focus on bridging this evidence with the findings in the literature on the determinants of nationalization. This would allow us to better understand the mechanisms through which more basic phenomena (such as regime type or party age) affect the scope of policy through their effects on party system nationalization.

In addition, future research could focus on the effects of party system (or government) nationalization on other areas of the political process. For instance, scholars can focus on different dimensions of public policies—such as their stability—as they relate to nationalized parties under conditions of high and low constituency homogeneity. Given our current findings, we would expect nationalized governments in polities with similar constituencies across districts to generate highly stable policies because in such cases policies not only would reflect broader electoral preferences (shared by voters across districts) but also would be promoted by internally consistent parties (i.e., parties whose current and future members are likely to share policy goals and preferences).

Nationalization might also be systematically related to dynamics between executives and legislatures in presidential systems. Even if presidents have a majority in the legislature, we might still expect to see stalemate when it comes to passing legislation if that majority is composed of copartisans whose party is nationalized, but drawn from very different constituencies (consider, once again, the case of the United States).

In general, having tuned the instruments with which we can measure the concept of party-system nationalization, including having reached a clearer understanding of how it is related to the composition of the constituencies parties represent, we can more confidently turn to exploring how party-system nationalization helps shape the dynamics of many dimensions of democratic representation.

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Notes

1. Morgenstern, Swindle, and Castagnola (2009) define two types of nationalization: static and dynamic. The former, the one in which we are interested, refers to the extent to which a party's level of support at any point in time is homogenous across districts. The latter refers to the extent to which a party's support levels increase or decrease in unison across districts over time—regardless of its underlying homogeneity of support. Because the arguments regarding the nationalization of policy are tied to the breadth of electoral support, we focus on static nationalization, for which we simply use the term *nationalization* or *party nationalization*.
2. Notice that we use the term *constituency* to signify something other than *district*, namely, the set of voters who support a given party *within* each district.
3. This disaggregation of party-system nationalization into that which is achieved through tailoring policies to different constituencies and that which is achieved through a blanket policy for all constituencies throughout the country has not, to the best of our knowledge, yet been undertaken in the study of the *effects* of nationalization. To that end, we posit that the findings of our article will be a useful contribution both to efforts connecting party-system nationalization to federal spending (Lago-Peñas & Lago-Peñas, 2009) as well as to the provision of public services (Hicken, Kollman, & Simmons, 2008).
4. For a more thorough review of this literature, see Bochsler (2010).
5. The measure's biggest drawback is that it concentrates on the static dimension of nationalization, effectively ignoring the dynamic side of the story. Because, as we

argued above, static nationalization is the relevant dimension to concentrate on when considering policy effects, this "shortcoming" is rendered less problematic.

6. The effective number of districts is calculated using the measure introduced by Laakso and Taagepera (1979), using shares of voters living in each electoral district v_d, or $E = \dfrac{1}{\sum_{d=1}^{D} v_d^2}$

7. We used Bayesian multiple imputation to complete survey responses. Missingness was most often found in the income variable—as expected: Lowest and highest quintile respondents tend to avoid answering this question. Although missingness at random (MAR) cannot be proved, we believe that, conditional on education level, urbanness of residence, and type of employment status, MAR is at least reasonable for the income variable. There is no expectation as to why other variables would be missing in some systematic way.

8. The position of the prime minister is counted twice in the count of how many cabinet posts are held by a party to account for the relative importance of the position. For a similar approach to accounting for such relevance, see Ansolabehere, Snyder, and Strauss (2005).

    When constructing a government-level measure of constituency similarity, we do not first aggregate demographic data for the supporters of every party represented in the government—as we do when constructing our measure of government nationalization. Aggregation of this sort would require that we assume that preelection identifiability, the ability of (sociodemographically similar) voters to know which parties would eventually end up in government together, was very high. Only then would it make sense to consider the impact of sociodemographic characteristics on vote choice to be operating at the coalition level. According to Shugart's (2006) impressionistic measure, most of the countries in our sample have low identifiability—and those for which it is high do not have coalition governments in the periods in which we observe them, mitigating any need for aggregation.

9. Once a square symmetric coincidence matrix with all values in all variables has been constructed, the measure is defined as: $\alpha = 1 - \dfrac{\sum PairableValues \delta^2_{Obs}}{\sum PairableValues \delta^2_{Exp}}$, where *Obs* and *Exp* are observed and expected (under independence across districts or coders) coincidence counts, and δ is a distance metric (usually an indicator function for nominal data). Krippendorff's alpha generalizes other previously used measures, and it corrects itself to accommodate previously problematic situations (such as small sample sizes and missing data). For a thorough discussion of the measure's computation and limitations, see Krippendorff (2004, chap. 11).

10. Using the acronyms of the OECD Economic Outlook data set, nontargetable = SSPG and targetable = CGW + CGNW + CAPEXP (or the sum of government consumption in wages, government consumption excluding wages, and government investment plus net capital transfers paid).

11. Namely, the observations are Australia (1996, 2004), Canada (1997, 2004), the Czech Republic (1996, 2002), Denmark (1998, 2001), Finland (2003), France (2002), Germany (1999, 2002), Great Britain (1997, 2005), Hungary (1998, 2002), Iceland (1999, 2003), Ireland (2002), New Zealand (1996, 2002), Norway (1997, 2001), Poland (1997), Portugal (1997, 2005), Slovenia (2004), Spain (1996, 2004), Sweden (1998, 2002), Switzerland (1999, 2003), and the United States (1996, 2004).

12. Expenditure types included in the "other" category each include both targeted and nontargeted spending. As a result, whether they serve the goal of catering to a national or targeted constituency is indeterminate.

13. The n parameter determines the level at which the model assumes allocation decisions are being made. In our case, therefore, we assume that budget allocation decisions are made at the intermediate level of hundreds of millions. Decreasing the level at which decisions are made only increases confidence in the results obtained, as would be expected.

14. Gibbs sampling is a Markov chain Monte Carlo algorithm designed to approximate samples from a posterior distribution by iteratively sampling from full conditional probability distributions. In a Bayesian inferential framework, such posteriors correspond to the sampling distributions of the statistical parameters on which inference is to be performed. For a discussion of the advantages of such a framework over likelihoodist or frequentist approaches, see Gelman, Carlin, Stern, and Rubin (2004), Gill (2002), and Jackman (2009).

15. The estimation involved two Markov chains, implemented in JAGS 2.2, each consisting of 3.5 million iterations, 1.5 of which were discarded as burn-in. Multivariate normals with mean vector **0** and variance 1000 were chosen as uninformative prior distributions. The chains appear to have converged on their stationary distribution, albeit slowly: Every Gelman–Rubin scale reduction factor is indiscernible from 1, and all z-scores for the Geweke diagnostic are below 1.96. Replication data and relevant code for both R and JAGS is available at http://solivella.wustl.edu/replication-data-and-code/

16. Removing the observation with the highest Cook's distance (viz., the Czech Republic in 2002, with a distance of about 0.5) does not substantively affect the results with respect to the coefficients of interest.

17. The fact that the models show evidence of consistently opposite trends for types of expenditure across levels of similarity is also evidence that the two types of categories are considered as alternatives to one another, rather than complements.

18. For a similar but broader discussion on the virtues of randomness in decision making, see Stone (2009).

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