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Explaining Subnational Variation in Voter Coordination and Party Entry in Electoral Competition

by

Joshua David Potter

A dissertation presented to the
Graduate Schools of Arts and Sciences
of Washington University in
partial fulfillment of the
requirements for the degree
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Abstract of the Dissertation

Explaining Subnational Variation in
Voter Coordination and Party Entry in Electoral Competition

by

Joshua David Potter

Doctor of Philosophy in Political Science

Washington University in St. Louis, 2013

Professor Brian Crisp, Chairperson

This dissertation focuses on electoral politics in a comparative perspective, specifically subnational variation in strategic party entry and strategic voter coordination. The dissertation studies how within-country variation both in the demographic composition of electoral districts and also in the supply-side composition of ballots can impact: where parties choose to contest elections, which and how many parties are successful, and why some voters in a country might not have the option of casting a ballot for all parties on election day. The first and third papers concentrate specifically on the supply side of electoral competition and demonstrate under what circumstances parties can be expected to compete across a broad range of electoral districts. I find that parties strategically enter those electoral districts where they believe that the outcome of the previous election indicates the “electoral market” is out of equilibrium. I also find that certain types of political institutions can induce parties in some countries to enter more broadly across electoral districts. The second paper explores the interaction between supply and demand to assess the impact of electoral constituency diversity on electoral outcomes. In short, I find that there exists a statistically robust relationship between driving up diversity in an electoral district and subsequently increasing the number of party offerings voters have access to on election day. This relationship, however, is substantially qualified by the permissiveness of electoral institutions.

Introduction

There's no point for us in working districts we know we'll lose. We know that blanketing districts with a bunch of paper candidates isn't going to get us anywhere. We achieve dramatically better results when we target a specific district.
– Party Strategist, Welsh Green Party

We tend to stand where we've stood in the past, regardless of how we performed there. Having the activists already in place in a constituency is a huge help.
– Executive Committee Member, Socialist Labour Party

Increasing the size of the district means more proportionality, sure. But it also means more voters live in the district. It means there's a more complicated political landscape at work. For a party as small as ours, reaching out to voters in a larger district is a lot more complicated.
– Party Leader, Scottish Christian Party

From the earliest days of the party, it was important for us that every Scottish person had the opportunity to cast a ballot for Scotland's nationalist party. For that reason, the SNP has always stood in as many constituencies as possible.
– Member of Parliament, Scottish National Party

The metaphor is loose, but common: elections are like markets. On one hand, there exists a supply of political parties – or products – that offer their platforms to voters on election day. This supply side of the electoral market can be comprised of parties that are large or small, famous or unknown, liberal or conservative, regional in their focus or more nationalized in orientation. On the other hand, there exists a demand for political parties and this demand emanates from the preferences of mass electorates. The voters that populate this landscape are typically arranged across the country in a constellation of electoral districts. Within each district, the balance of voters is either more liberal or conservative; some districts are more ethnically or socioeconomically diverse than others; and voters may be more or less predisposed to hazard their vote on less well-known ballot offerings, such as independent candidates, new parties, or parties catering to a very specific niche interest. Sometimes these markets have been in place for many, many elections. In other, nascent democracies, electoral markets are unsettled, in flux, and susceptible to upheaval.

For a long while now, scholarship of political markets has sought to understand the latent dynamics that drive the supply of political parties and the demands of voters, as well as, ultimately, their congruence with one another. Due to an early lack of cross-national data – and during the time of its intellectual infancy – the field of comparative electoral politics initially sought to develop and test hypotheses at the level of the country in the aggregate. Assuming away district-level market dynamics, early scholars in this field were able to determine that there are, for example, systematic differences between the supply and demand sides of electoral markets in those countries that rely on proportional representation and those countries that rely on single-member districts. They established the mechanical relationships between electoral outcomes and institutional variables such as minimum-vote thresholds, average magnitude, and seat allocation formulae. They explored the implications for representation in national-level legislatures when we move from a “majoritarian” system to a “consensual” system and they speculated about the more moderate effects of adopting so-called “mixed-member” systems that draw on aspects of both the majoritarian and consensual models of elective representation.

Many of these intuitions are deep and well-settled. But a more recent vein of scholarship has rigorously pushed these intuitions to the micro – or the district – level, where substantial variation in electoral outcomes can exist across districts falling within the same country. Indeed, scholars have begun to ask, when we treat elections not as one giant market, but rather as many (sometimes several hundred) district-level markets that are linked together, what does that change about our fundamental understanding of representational democracy? To this end, scholars of comparative politics have recently begun to explore within-country (or subnational) variation in voters’ abilities to successfully coordinate their voting decisions around a subset of truly viable party offerings; of geographic variation in the homogeneity of parties’ support across different districts; of the stability of voters’ preferences over time; and of the emergence and success of region-based parties that cater to the interests of only a limited sample of the country’s overall population. The conclusion from this vibrant strain

of scholarship is clear: focusing on subnational, district-level voting decisions can infuse our understanding of the demand side of electoral markets with much richer nuance.

In comparison to this focus on voters, their preferences, and their support for political parties, our theoretical and empirical understandings of the other side of the electoral market – the party-based, supply side – lag well behind. Across countries, we do not have a great handle, for example, on why some parties choose to field candidates or party lists in, say, 50% of a country’s available electoral districts while some other parties choose to contest every district. We are unfamiliar with the strategic considerations that drive a smaller party to choose to field a candidate in one district at the expense of fielding a candidate in one of any number of other districts. How do parties assess the marginal costs of these unrealized opportunities? On what criteria do they discriminate between districts? Finally, we know very little about how party leaders weight tradeoffs in the proportionality of electoral districts with the demographic composition of these districts. How many “types” of voters must exist in a district before a party is willing to undertake the work to craft a bundle of policies that might successfully target that type of voter? Does the permissiveness of the institutional environment qualify this calculation? To what extent?

The main thrust of this dissertation, then, lies with the parties, with their strategic calculations about when, where, and why they give certain subsets of a country’s voters the opportunity to evaluate and, hopefully, cast a vote in favor of their platform. The preferences and coordination efforts of voters are implicitly present in the analysis – after all, parties must pay attention to electoral outcomes if they are to succeed – but these will take a backseat to my considerations regarding the supply side of the market. In an anecdotal capacity, this dissertation is about why the Welsh Green Party targets its resources specifically in a district it thinks it can win while the Scottish Nationalist Party fields candidates in every district due to its party leaders’ fervent belief in the cause of Scottish independence. This dissertation examines, for instance, the difficulties encountered by leaders of the Scottish Christian Party in evaluating how district-level voter diversity should come to bear on their

strategic calculations. This dissertation is about why the Socialist Labour Party in England returns again and again to the districts it has previously contested, regardless of its previous levels of success with voters in these districts. In short, this dissertation drills down to the comparative tradeoffs parties face when weighing factors such as: the likelihood of their success in a district, the previous infrastructure they have built up in a district, the diversity of the district's population and how easy or difficult this makes pitching a unique party platform, and the marginal cost of contesting elections broadly rather than intensively.

The first paper – “Selectively Entering Parties” – begins the analysis by asking a basic question: given that a party chooses to enter some, but not all, electoral districts, how does it choose in which districts to enter? My explanation focuses on parties' ability to target those districts where there was a breakdown in the electoral market in the last election. Relative to districts where the supply and demand sides of the market were in balance, those districts with evident coordination failure call out to selectively entering parties as opportunities to make inroads with unsettled electorates. Together with the level of proportionality in the district and whether the party previously invested resources there, I am able to demonstrate that *win potential* – or a party's odds of winning a seat, given the distribution of votes in the last election – can substantially determine a party's district-by-district patterns of entry. This paper is the first to model individual parties' relative evaluations of the tradeoffs involved in entering one district at the expense of entering others.

To step a bit out of sequence, the third paper – “Linkage as Ballot Composition” – takes a slightly wider view of parties' entry decisions and, rather than exploring the motives behind a party entering *any one district*, this paper ventures explanations for why parties enter *a certain number of districts*. That is to say, this paper offers both marco-level institutional and party-level logistical explanations for why parties in some countries, on average, tend to contest more districts than parties in others and, similarly, why parties within the same country exhibit variation in the extent to which they field candidates across districts. I find that the centralization of political power – through the adoption of various constitutional

arrangements – leads to higher average levels of party “linkage” across districts. However, I am also able to show that, at the party level, previous levels of success and notoriety can mitigate these macro-level institutional effects. Put differently, the incentives emerging from most political institutions are more salient predictors of the extent of linkage among smaller parties than among larger ones. By way of concluding this paper, I make the case that once parties are sufficiently large, they simply contest virtually every district in the country, regardless of the expected district-by-district outcomes. This is a luxury that is not afforded to smaller parties (who must use their resources wisely and adhere to the institutional contexts in which they operate). Furthermore, this homogenous entry strategy adopted by larger parties can make voters’ job of selecting viable ballot offerings more difficult.

Finally, the second paper – “Latent Diversity and Party Systems” – altogether moves away from parties as the unit of analysis, yet draws extensively on a theory framed in terms of party-level decision-making. Specifically, this paper explores the ways in which voter diversity *within a district* as well as *across districts* combine together with electoral rules to determine how many parties – at the district level – receive substantial support from voters. The classical argument in this respect is that increasing voter diversity will open up more opportunities for parties to present unique and differentiable policy platforms to voters (so long as the electoral rules are sufficiently permissive). A long track record of mixed findings in this literature led me to argue that “diversity” as a concept should be disaggregated into two components and, furthermore, that these components would work at cross purposes. The first, diversity within a district, will drive up the number of parties competing and receiving votes. The second, diversity across districts, will drive down the number of parties due to the fact that it creates impediments for parties when they attempt to recycle policies tailored to a specific district in other – socially distinct – districts. This paper is the first to test the relationship between diversity and party system size at the district level. It is also the first to postulate and find evidence in support a more nuanced understanding of the link between voter diversity and party’s efforts at fielding candidates across districts.

Paired with the extant literature that focuses on the demand side of the electoral market, these three papers help contribute to our evolving understanding of the cross-district (but within-country) dynamics that govern both sides of the electoral market. This dissertation sheds new light on the strategic decisions underlying *which* and *how many* electoral districts parties choose to contest. It shows specifically how constituency-level characteristics such as previous coordination failures, district magnitude, previous resource investments, and voter diversity inform these decisions. Most importantly, it couches these explanations in relative terms, so that we can now understand what sorts of opportunities parties pursue and what sorts of potentials they are comfortable forsaking.

By way of closing, I should point out the implications of my findings for the demand side of the electoral market are worth noting. Voters should be wary for several reasons. When they fail to coordinate sufficiently around viable ballot offerings, for example, in the next election they can expect to be subjected to increasingly lengthy rosters of ballot offerings populated by newer and smaller parties desperate to pick up seats wherever they can. Having fielded candidates in the district previously, these parties are much more likely to return, regardless of how they fared in the tabulation of votes. When parties meet with more substantial electoral success, they break from the strategic tactics they employed when they were smaller. Instead, they pay less attention to the incentive structures put in place by constitutional designers and they place less emphasis on rooting out the specific electoral districts where, based on previous results, they can rationally expect to win. The result is uniform contestation across all districts – regardless of prospects – and this shifts the burden of assessing viability almost entirely onto the shoulders of the voters. When they fail, newer and smaller parties again take notice and again target those districts in future elections, adding to the clutter and making coordination more difficult. And the cycle continues.

– Paper 1 –

Selectively Entering Parties: Subnational Variation in Voters’ Choices

Abstract: Given that a party chooses to selectively contest some – but not all – districts in an election, how can we explain *where* it chooses to enter? This is a question that has profound consequences for the menu of ballot offerings to which voters in different areas of a country have access when they go to the polls on election day. However, previous scholarship on elections has not examined the tradeoffs that such *selectively entering* parties face in choosing to enter one district at the expense of another. In this article, I build the first cross-national model of intranational variation in voters’ menus of party offerings. I argue that selectively entering parties should aim for the electoral districts in which voters previously exhibited poor levels of coordination. I test this argument with data on more than a half-million party-entry decisions in thousands of districts across hundreds of elections in a diverse subset of 40 democracies.

Introduction

Given that a party chooses to selectively contest some – but not all – districts in an election, how can we explain *where* it chooses to enter? Because parties that universally contest *all* districts in a country are actually comparatively rare, this is something of an important question that affects most parties around the world. Indeed, a rather broad swathe of the types of parties studied in the field of electoral politics fall under the heading of what we might refer to as *selectively entering* parties. These might be newer and smaller parties, these might be parties with few resource endowments or regionalized parties that cater to the political interests of only certain districts; these might be parties that are simply transitionally small *en route* to broader electoral success or perhaps these might be niche parties that espouse platforms that appeal to the needs of specific demographics such as rural communities.

But these are only a few of many possible examples. The unifying conceptual characteristic of selectively entering parties – regardless of their “type” – is the fact that these parties

face real tradeoffs in electoral competition. For any one of a number of reasons, these parties assess their electoral prospects differently across different constituencies and ultimately conclude that some constituencies are simply not worth the time, resources, and effort of fielding a candidate or a list of candidates. Instead of contesting all districts, they forgo potential opportunities in some districts in order to more specifically chase opportunities in others. By contrast, parties that are able to contest every district in a country are not, by definition, faced with these tradeoffs. Perhaps a universally entering party will allocate greater or fewer resources to particular districts or take steps to ensure that high-quality candidates run in certain localities; but these parties do not feel the bite of completely forgoing opportunities to stand for an election in some areas of the country.

Previous studies of electoral politics have not yet addressed this question, which might most effectively be framed as an additional strategic calculation that selectively entering parties must undertake (and that universally entering parties need not). Studies of the emergence of new parties (Kitschelt, 1988; Meguid, 2005; Mudde, 2007) or of parties with platforms grounded in regional social cleavages (Chhibber and Torcal, 1997; Chhibber and Kollman, 2004; Brancati, 2008), for example, only address this question indirectly and are unable to provide systematic explanations that operate across *all* types of selectively entering parties. While previous studies of resource barriers to new party emergence (Harmel and Robertson, 1985; Hug, 2001; Tavits, 2006) tell us something about the impediments that parties face in deciding whether or not to contest an election at the *national* level, they have nothing to say on a district-by-district basis at the *intranational* level.

This is a gap that requires filling because the theoretical implications of selective entry range across research questions focusing on party system consolidation, party nationalization, voter choice, and preference aggregation. Understanding the choices of selectively entering parties can tell us something about important variation in the consolidation of party systems *at the district level* (Tavits, 2007). Stability in the number and type of party offerings have long been indicators of a party system's consolidation (Bielasiak, 2002; Sartori, 1976;

Tavits, 2007). Selectively entering parties – by virtue of never entering all districts – might potentially either reinforce or upend consolidated political competition to differing capacities within the same country. These elite-level, district-by-district entry decisions may leave intact preexisting patterns of party support in some areas of the country at the same time that they undermine patterns of support in other areas of the country. In short, until we are able to understand selective entry, we run the risk of committing an ecological fallacy when discussing party system consolidation at the national level.

Such district-level variation in support for parties results in varying levels of party system nationalization, or the extent to which parties draw homogenous levels of support across districts (Jones and Mainwaring, 2003). Party systems with low levels of nationalization have long been viewed as normatively distressing because these are systems where political competition centers on divisive regional issues, representation is predicated on particularistic rather than programmatic ties, and budgeting decisions result in resources being disproportionately focused on targetable (rather than public) goods (Caramani, 2000; Jones and Mainwaring, 2003). To the extent that these patterns of support across districts hinge on variation in the menu of party offerings rather than different groups of voters responding variably to the same party offerings, then studying selective entry stands to make an important contribution to the growing literature on nationalized politics. For example, we might learn that in some cases, a low level of nationalization is an artifact of many selectively entering parties neglecting a large number of electoral districts (in which case nationalization is *not* indicative of the concepts it is thought to measure), while in other cases, a low level of nationalization is resulting from voters' disparate assessments of the same (universally entering) parties (which is what we typically argue when studying geographical dispersion of party support).

Selective entry's most proximate implication for voters, however, is that voters in different districts within the same country can potentially be faced with radically different menus of party offerings on election day. This is a conundrum that has long received attention in the methodological and formal theoretical literatures on voter choice and district-level election

outcomes. At the level of the individual voter, studies drawing on survey data have tried to correct for violations of the assumption of the independence of irrelevant alternatives: certainly the fact that a voter is *not* presented with a particular party on the ballot when she might have actually preferred to vote for it will impact the quality of efforts to predict her vote choice (Alvarez and Nagler, 1998, 2000; Glasgow, 2001). These measurement problems also aggregate up to the district level, where vote totals for individual parties in a district are biased for want of information on parties that did not enter there, but did enter elsewhere (Jackson, 2002; Katz and King, 1999; Tomz, Tucker and Wittenberg, 2002).

Aside from methodological nuisances, this variation in the menu of party offerings also has profound normative implications borne out of the formal theoretical literature (Austen-Smith and Banks, 1999). Violations of the independence of irrelevant alternatives assumption results in deeper problems with the process of social choice. Aggregating up from districts (or smaller contests) to the national level (or larger contests) will not ensure that basic principles like path independence (Arrow, 1963; Plott, 1973) or choice consistency (Chernoff, 1954) are satisfied. When different constituencies are offered incomplete subsets of the broader national set of parties, then vote choices – and their aggregation – may simply not be fully informative measures of a society’s deeper preferences. By inducing variation in the menu of party offerings within a country, then, selectively entering parties may well pose a serious challenge to meaningful articulations of social choices at the national level.

For all of these theoretical considerations, the question of selective entry is germane empirically as well. Figure 1 illustrates the average share of districts – for each party across all elections it contested – in which the party chose to field candidates.¹ Each observation in gray depicts one party’s average share of districts entered and the observations are arrayed within the countries they reside. Heavier, black dots indicate the average share of districts entered by *all* parties within each country and the countries themselves appear in descending

¹ The data underlying this graphic were organized by the author from various sources. The particulars of these data will be explained in great detail in the empirical section.

order of average percent of districts entered. In a few settings at the top of the figure, the question of variation in party competition at the district level is moot by definition when a single at-large district is used. These cases are followed by a smattering of other countries with very few districts overall, making entry by parties in all districts more likely, but by and large, there is an astonishing amount of variation across countries. There is also a great deal of variation within countries, with individual parties' gray dots arrayed virtually from one district to all districts (in most cases). The overwhelming preponderance of parties across these elections chose to enter a *selection* of districts rather than *all* districts. About 75% of the party-election observations in my data set entered districts selectively. On average, these parties entered a nontrivial share of a country's districts (about 25%) and, more importantly, these parties garnered more than 60% of all votes cast across all elections in the data set.

Cross-nationally, we know something about the black points in Figure 1, but virtually nothing about the gray points. In countries where the value of national office is high (as in presidential systems, for instance), scholars have argued that parties will link their efforts across districts and enter across a broader range in order to increase their odds at gaining representation at the national level (Cox, 1999; Morgenstern, Swindle and Castagnola, 2009). We also know that decentralized fiscal or political power or the presence of region-based social cleavages might prompt parties to intensively enter smaller numbers of districts rather than flooding all districts with candidates (Brancati, 2008; Morgenstern and Swindle, 2005; Morgenstern, Swindle and Castagnola, 2009). Even within countries, we can think of convincing and commonsense reasons why some parties contest few districts and others many: parties have differing resource endowments, especially in countries with very few campaign finance restrictions², and some parties – especially of the niche rather than the catch-all

² In surveying the recent literature related to party systems and electoral competition in Latin America, Morgenstern and Vazquez-D'Elia (2007) observe that “it appears reasonable to assume that the same rules may pose different challenges and opportunities to *differently endowed parties*” (p. 155, emphasis added).

Subnational Variation in Entry

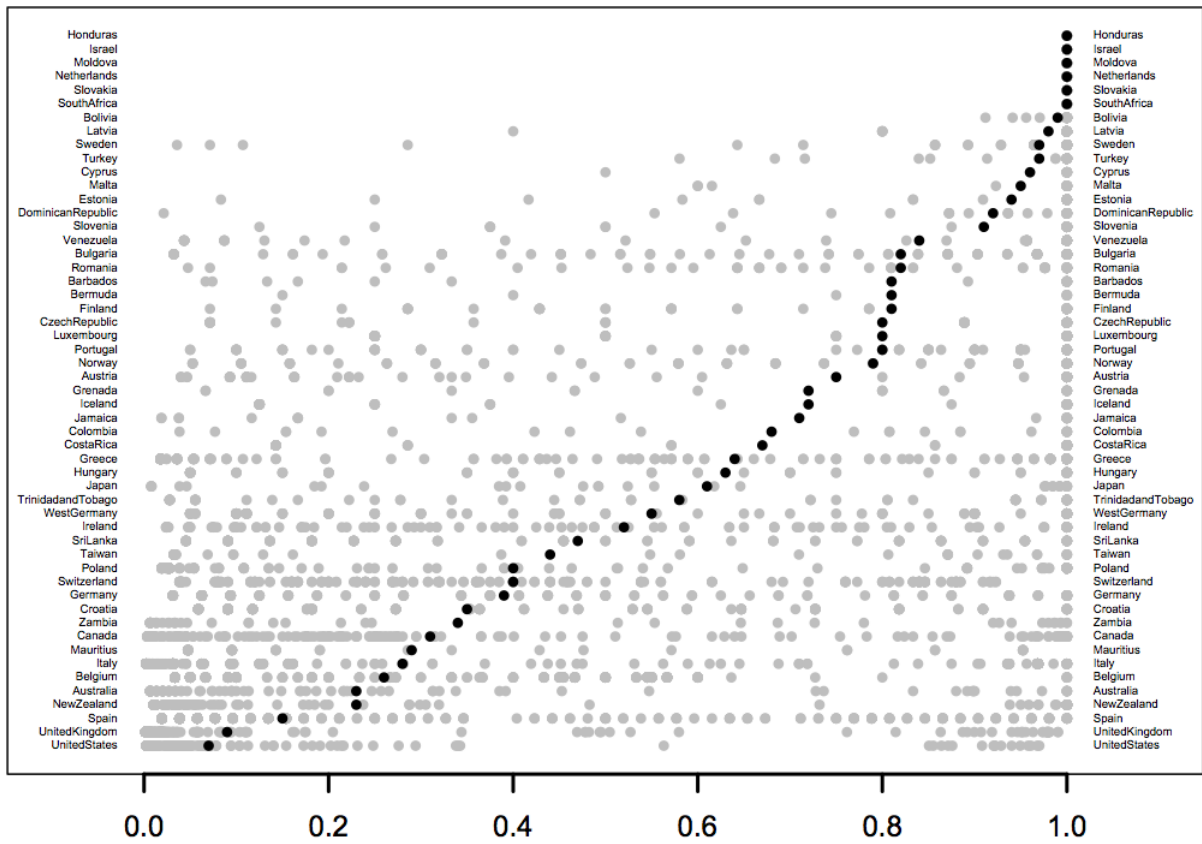


Figure 1. Average Share of Districts Entered by Party (Gray Points) Within Each Country (Country-Level Averages Depicted in Black)

variety – will espouse platforms that fail to gain traction in certain types of districts.³

Rather than explaining variation across parties in the national-level *incidence* of entry, however, I will endeavor to explain the *specific entry decisions* parties make on a district-by-district basis. That is to say, for every observation in Figure 1 that falls below universal entry across districts, I put forward a district-level model that explains variation in entry decisions. To my knowledge, my work is the first to treat selectively entering parties’ strategic election decisions as a string of dichotomous outcomes across all districts within a country. I define a concept called *win potential* that captures two dynamics that parties care about when choosing where to field candidates: first, the number of voters who failed to identify a winning party in the last election and, second, the fewest number of votes it would take for a selectively entering party to become a winning party. In other words, *win potential* measures whether there are enough voters coordinating on losing parties who, taken together, could make a new party a winner. By accounting for the cost of the “cheapest” seat *and* the votes most easily available to “buy” it, *win potential* provides a summary measure not only of the threshold of representation for a new entrant, but also the number of votes it might be able to mobilize in an effort to clear this threshold. I explain how win potential becomes a less salient factor for selectively entering parties as they grow in size and situate my explanation alongside other intuitive predictors of selective entry – namely, district magnitude and whether or not the party previously invested resources in a district.

The paper proceeds as follows. First, I will articulate in greater detail my theory of *selective entry* before, second, describing the data set I employ in the course of testing my hypothesis. Next, I estimate a logistic hierarchical model of district-level entry decisions for parties that do not enter all districts in a given election and substantively discuss the model’s results. A consistent empirical story emerges. Where there is variation in district magnitude within a country, parties that are forced to selectively enter a subset of districts consistently opt for districts with larger magnitudes. Having made the decision to enter a district in the

³ See Rodden (2010) for a discussion on the geographic clustering of political preferences.

first place, most parties persist in re-entering that district in subsequent elections, perhaps drawing on the organizational apparatus they constructed there in the last election. Despite these powerful and predictable trends, however, a district's *win potential* also consistently exerts a positive and statistically significant impact on the probability that a party will enter a given district: as a district's win potential increases relative to other districts, any given party is more likely to enter that district. This result is robust to alternative specifications of the model that I present in two appendices.

A Theory of Selective Entry

Even in party systems where several party offerings are universal, selectively entering parties still contest elections (Blais et al., 2011; Morgenstern and Vazquez-D'Elia, 2007; Tavits, 2007). While uniformly entering parties simply “enter” the race at the national level, selectively entering parties – having made the decision to enter at all – must then enter a subset of districts rather than the full field. Typically in studies of strategic party entry, only the first decision is given much consideration while the *intranational*, cross-district variation in entry is neglected (see discussions by: Lago and Martinez, 2010; Tavits, 2006). Indeed, scant research has addressed the question: given that a party chooses to selectively contest some – but not all – districts in an election, how can we explain *where* it chooses to enter? Effectively answering this question requires disaggregating the discussion about strategic entry from a national perspective to a district perspective. If political elites determine that their party's electoral prospects differ across districts, then we need to seek out determinants of these elite assessments that vary at the district level. Previous research in the field of strategic party entry, however, does not offer much in the way of precedent on this point.

Cox (1997) discusses the costs of entry as well as the benefits of potentially obtaining office. Simply put, he argues that parties enter a race when the benefits outweigh the costs. Following Hug (2001) and others, Tavits (2006) employs a comprehensive test of numerous hypotheses pertaining to the costs of entry and finds that parties are more likely to enter

when registration costs are low, when average district magnitude is high, and when the electoral histories of incumbents are short. Other work has explored the emergence and success of “niche parties” in particular (Meguid, 2005; Mudde, 2007) and an older literature investigated the emergence of new parties of specific ideological orientations such as Christian Democratic parties (Kalyvas, 1998), parties of the left (Kitschelt, 1988), and parties of the far right (Kitschelt, 1995).

While more general studies in the line of Hug (2000) have explored cross-national variation in generic party entry, these studies reach divergent conclusions. Harmel and Robertson (1985) conclude, for example, that the emergence of new political issues invite new party entry, while on the other hand, scholars such as Hino (2006) argue that electoral systems are the chief determinants of entry. These are all, as Lago and Martinez (2010) call them, “inter-variation” studies that take as their unit of analysis an election in a particular year in a particular country. Thus, parties either do or do not enter the election dependent on the characteristics of their country and these are characteristics that vary across national cases, but not within them. Put differently, entry anywhere and entry everywhere are conceptually identical in these studies.

In actuality, however, the strategic landscape of selectively entering parties never looks like this. Rather, for these parties, strategic entry “in an election” is actually an aggregation of many (sometimes literally hundreds) of dichotomous yes-or-no entry decisions. What district-level characteristics might influence the probability that a selectively entering party chooses “yes” for a particular district? Although different parties contest elections with different motivations, their aspirations can usually be best realized by winning or by coming as close to winning as possible. Winning a seat in a district or coming close to winning a seat are both roads to increasing visibility: whether a party is motivated to change policy, to promote a pet issue, or to push a like-minded establishment party to change its platform, coming as close to winning as possible is a straightforward manner to call attention to the party’s goals. Thus, I argue that parties require some sort of informational signal about

the potential of winning a seat within each district (which is conceptually distinct from arguing that they actually *expect* to win a seat). This assumption has its roots in a long line of literature beginning with Leiserson (1968) and Riker (1962) and continuing through to Laver and Schofield (1990) who argue that – whatever their underlying motivation – parties seek office. A more recent review of party motivations argues that although different parties are motivated by different incentives, “it is almost always better to be in office than not” (Müller and Strom, 1999, p. 6). Selectively entering parties, in particular, should look to a type of signal about the potential for gaining a seat that varies across districts within the same country.⁴ In particular, selectively entering parties should look to districts with evident previous electoral “market failures” as prime targets for successful entry.

Cox (1997) likened elections to markets and posited an equilibrium concept that balances both the supply of and demand for party offerings. This equilibrium point is structured by the well-known psychological and mechanical effects of party competition articulated by Duverger (1954): a given set of electoral rules (of which the choice of magnitude is oftentimes the most consequential) governs the mechanical translation of votes into seats and voters, not wanting to waste their votes on nonviable parties, strategically abandon parties that have entered in excess of this equilibrium. When this equilibrium point is exceeded and a larger number of parties net substantial vote shares, then the electoral market has failed (Lago and Martinez, 2010): many voters have cast ballots that, by virtue of going toward nonviable contenders, will not be included in the mechanical translation of votes into seats. In very new democracies, this failure can be attributed simply to unfamiliarity with democratic rules (Duch and Palmer, 2002; Dawisha and Deets, 2006; Tavits and Annus, 2006; Vander Weyden and Meuleman, 2008). But if we continue to observe variation in electoral

⁴ It is important to note that many of the informational inputs available to larger parties, such as detailed campaign survey data, are generally unavailable to smaller, perhaps resource-strapped parties. The measure of *win potential* that I describe below has the advantage that elites of all parties – regardless of resource endowments – should be readily able to calculate this measure based simply on previous electoral returns.

market failure across districts within *established democracies* (which, empirically speaking, we often do), then the persistence of electoral market failure might rather be construed as voters expressing their discontent with current party offerings (Lago and Martinez, 2010). By persistently casting ballots for nonviable parties, voters are, in a sense, giving voice to their dissatisfaction with current party offerings (Lago and Martinez, 2010; Hirschman, 1970).

When selectively entering parties observe electoral market failures, they see a set of voters whose support should be comparatively easy to win (especially vis-à-vis voters who have settled into established patterns of support for consistently seat-winning parties). Given that extant parties have been unable to mobilize support in this non-coordinating section of the district's electorate, the door remains open for a selectively entering party to offer a new option on the menu of party labels. New potential entrants do not have to pry votes away from already winning parties; rather, they just have to tie up the losing parties' loose ends. In an attempt to capitalize on voter dissatisfaction, then, selectively entering parties will enter districts where there has been a breakdown in voter coordination around a viable set of party offerings. Empirically measuring this "breakdown" however, can be difficult and, as previous studies have argued, the choice of metric is of some consequence (Cox, 1997; Crisp, Olivella and Potter, 2012; Singer and Stephenson, 2009; Tavits and Annus, 2006).

I create a new measure of coordination failure because, as I will argue, I am interested in a very specific type of calculation on the part of party elites.⁵ What is needed is a measure both of (1) the *cost*, measured in votes, of gaining a seat in the district (i.e. the hurdle over which a party must pass if it is to gain office) as well as (2) the number of *poorly coordinating voters* who might be easily convinced to throw their support behind a new entrant (i.e. the electoral materials at a party's disposal with which it might cross the hurdle of representation). Furthermore, the metric must be standardized to account for the fact that selectively entering parties are making entry decisions in each district by

⁵ And this calculation is not captured by statistics such as the effective number of losing parties, the SF ratio, hopeless votes, or the coordination product, which are reviewed in Crisp, Olivella and Potter (2012).

evaluating their prospects in that district *relative* to their prospects in all other districts. Thus, I propose as my explanatory variable of theoretical interest a measure of *win potential* which I define at time t in a given district d as:

$$win\ potential = \frac{hopeless\ votes_{t-1}}{\min[party\ votes_{i,t-1} / party\ seats_{i,t-1}]}, \forall i \in \{1, \dots, N\} \text{ in } d$$

Where i indexes all parties that won seats in the last election within a given district and *hopeless votes* is the sum of all votes going to the second-largest party that failed to win a seat as well as the votes going to all lesser vote-getting parties.⁶ This measure is then standardized (mean-centered) across all districts *within a given country-election* to reflect the fact that each district is endowed with a new value of win potential in each election and each party in the country makes a new round of evaluations across these districts.

In single-member district systems, the denominator simply collapses to the number of votes going to the party that won that district's single seat. In multimember systems, the denominator represents the idea that the cheapest seat to win is not always previously won by the smallest party. Imagine a hypothetical multimember District A. Perhaps the smallest seat-winning party in District A won only one seat, but won it soundly. Suppose that the largest seat-winning party in District A won five seats, but just barely cleared the threshold to win the fifth seat. In this example, it is this number of votes needed to gain that last seat of the largest party that a potential entrant into the district should be keeping its eye on. Thus, the denominator in this multimember context simply divides all seat-getting parties' vote totals by their seat totals and registers the minimum of all these values as the threshold which any new potential entrant would have to clear in order to win a seat.

The unstandardized district-level *win potential* statistic is bound on the low end by 0

⁶ See Tavits and Annus (2006) for a discussion of the advantages of using "hopeless" votes over "wasted" votes, which is simply the sum of all votes going to *all* non-seat-winning parties, rather than just those votes going to the second-largest non-seat-winning party and all other smaller parties. By and large, the empirical results hold when I replace "hopeless" votes with "wasted" votes in the numerator.

(i.e. a scenario where all votes go to winning or first-losing parties, meaning the district is in perfect electoral equilibrium) and can assume values upwards of 10 (in the improbable case of, say, an exceedingly diffuse distribution of votes in a single-member district with a very low margin of victory).⁷ Across the observations in my data set, however, unstandardized *win potential* has a mean of 0.31 and an interquartile range from 0 to 0.42. There are two things that can increase the value of *win potential*: either the number of votes going into the awarding of the cheapest seat in the district decreases (thereby signaling that winning representation in a district has become, in a sense, cheaper) or the number of hopeless votes increases (thereby signaling that a new potential entrant has a larger number of poorly coordinating – or disaffected – voters at its disposal with which it might cross the seat-winning threshold). Having made clear the operationalization of the main explanatory variable, then, I explicitly state my first hypothesis:

H1: Selectively entering parties are more likely to enter any given electoral district as its win potential increases relative to other districts.

When presented with the option of entering either of two districts, say, a selectively entering party will choose – other things being equal – the district with a higher win potential. It is in that district, when compared with the other, that the cost in votes of winning a seat or coming closest to it is lower and/or the supply of easy-to-mobilize voters who have been previously disappointed with extant party offerings is sufficiently large so as to make fielding a candidate in the district probabilistically worthwhile.

The relationship between win potential and the probability of entry should not be unconditional, however. As parties increase in size and contest more districts, they should

⁷ Empirically, the vast majority of observations fall between 0 and 1, with an additional, but very small, cluster of observations falling between 1 and 2. There is then a long tail of scattered observations assuming values as high as 13.83, which is the maximum. In the original data sets, values higher than this were reported but, upon closer examination, appear to have been generated by data entry errors in the original sources. When an error was located, that district was deleted. These deletions amount to less than 0.5% of the data set and the regression analyses presented below hold even if these erroneous districts are included.

depend less and less on win potential as an informative signal about their prospects in a given district. Entry in more districts indicates greater *party capacity*. As a party's capacity increases, the tradeoff between entering one district at the expense of *not* entering another drops off until, at the extreme where a party enters all districts, the tradeoff disappears completely. When capacity is low, win potential will constitute a crucial informational signal to selectively entering parties because the stakes are higher; with fewer districts to enter in the first place, entering just one of them precludes entering potentially many others.⁸

Empirically, then, as a proxy for *party capacity*, we can use the proportion of all districts that a party entered in the previous election. As a party's capacity increases – whether as a result of more resources in the party's coffers or an interest in creating a more national presence or in response to previous electoral successes – the importance it places on win potential as an informative signal should decrease. Put differently, selectively entering more districts overall necessitates a higher probability of entering any one district in particular. Thus, I formulate a second hypothesis:

H2: As party capacity increases, the utility of win potential as a signal to selectively entering parties should decrease.

There are two other potentially important determinants of selective party entry for which I need to control. The first determinant is *district magnitude*, which creates very different competitive environments for selectively entering parties. Where district magnitude is high, the translation of votes into seats is more proportional and electoral competition is more permissive. That is to say that winning a seat is mechanically “easier” in large magnitude districts. This is somewhat different than the idea of “ease” of winning a seat embodied in the win potential metric. There, parties are weighing both extant party strength as well as

⁸ Additionally, parties may extend into other districts over time in an effort to increase their profile (thereby appearing more “nationalized” in character) or out of a belief that voters throughout the country are entitled to cast a vote for the party, even if the party has no hope of winning a seat in the district. Both of these dynamics would work against the impact of win potential as party capacity increases.

the psychological nuances of voter coordination.⁹ Although irrelevant in countries without intranational variation in magnitude, district magnitude could nonetheless exert a positive influence on the probability of a party selectively entering a district. This idea is rather similar to district-level studies of entry and voter coordination, but recasts it in terms of relative tradeoffs that party elites face in deciding where to enter.

The second factor for which I must control is whether or not a party *previously invested resources* in a district in the last election. Certainly by having gone through the process of investing resources in a district once, the party may be able to make use of those resources in subsequent elections. This idea is similar to the emerging literature linking party organizational development to variation in electoral performance across districts (Tavits, 2012). While not being able to make use of detailed party organizational data in a cross-national test, controlling for previous entry in a district should serve as a rough proxy for previous resource investment. If previous entry is found to be a positive predictor of current selective entry, this will indicate that parties place a valuable premium on the past either as an organizational placeholder (Tavits, 2012) or as a learning heuristic (Gschwend, 2007). The reasoning regarding my controls leads to the following hypotheses.

H3: Selectively entering parties are more likely to enter any given electoral district as its district magnitude increases relative to other districts.

H4: Selectively entering parties are more likely to enter an electoral district where they previously invested resources.

A possible objection to my focus on these four hypotheses is that they do not take into account interactions between parties such as mergers, coalitions, or stand-down agreements. Empirically, there is little that can be done to account for this level of nuance in a large, cross-national study. Theoretically, however, I would argue that individual parties will not engage in these sorts of mutual agreements with other parties if they evaluate their own

⁹ The two measures are clearly tracking different concepts as magnitude is correlated with (unstandardized) values of win potential at only $r = 0.35$ and with (standardized) values of win potential at $r = 0.08$.

prospects in a district sufficiently positively. Put differently, if a party likes its odds in a given district, it would prefer not to share its success with other parties. It will not stand down in a district or merge with another party if it expects to do well there. The fact remains that if a party sees a high potential for doing well, it should be expected to enter; if it does not see this, then it should not be expected to enter. Any specific inter-party dynamics that govern this decision of “non entry” (which include, but are certainly not limited to, engaging in a stand down agreement or merging platforms and changing party labels) enter the story at a different theoretical stage and should not bias the results of the analysis that follows. Keeping this in mind, I now move onto a discussion of the nature of the data set, the statistical model, and its results.

The Data Set and Variables

In order to test the hypotheses, I require *complete* vote and seat data at the *district level* for *all* parties that contested a national lower-house election in a given democratic country in a given year.¹⁰ As I am interested in selectively entering parties – and as these are often the very parties that get lumped together into an aggregated “other votes” category in many cross-national databases – I need to be particularly certain that the vote data records *all* parties individually. Therefore, I take as my starting point the *Constituency-Level Elections (CLE) Dataset* compiled by Brancati (2007), which was gathered with the specific intent of avoiding the grouping of smaller parties’ votes in an aggregated “other votes” category. Working with this baseline data set, I then merged in a number of elections from the *Constituency-Level Elections Archive (CLEA)* compiled by Kollman et al. (2011). This data set is not as explicitly oriented toward avoiding the reporting of votes in an “other votes” category and, thus, I only took from this data set those elections for which the authors had complete votes for all parties. In both the CLE and CLEA cases, there were

¹⁰ I take as my definition of “democratic” a score of 5 or higher on the Polity IV measure of democracy, though almost every country included in the data set clears this hurdle by a rather healthy margin.

some countries that had complete vote data, but incomplete seat data; for those countries, I filled in seats from other sources wherever possible.¹¹

Employing these criteria, I created a data set with a large number of elections; however, I pared down the number of observations based on additional criteria. First, I was only interested in *continuous series* of elections due to the fact that I have argued that election outcomes in the immediately *previous* election should inform parties' entry decisions in *this* election. Second, I dropped observations for countries (such as Malta and Jamaica) during periods of time in which they were not sovereign, independent countries.¹² Despite these fairly restrictive inclusion criteria, I have a data set that far exceeds the number of observations included in most cross-national studies of electoral politics. Indeed, I retain district-level electoral data for more than 3,700 districts in a diverse set of 40 democracies.

But the unit of observation is not the electoral district; rather, it is each party's entry decision (whether yes or no) in each electoral district. In terms of the available data, I define *entry* = 1 for a given party in a given election's district when that party received at least 1 vote in the data set. I define *entry* = 0 for those districts where the party received no

¹¹ In the cases of Grenada, Jamaica, and Zambia – in which seats are assigned in single-member districts based on simple plurality rule – I simply assigned the district's sole seat to the party with the most votes. For elections in the Dominican Republic in 2002 and 2006 and Sri Lanka in 2000, 2001, 2004, and 2010, I relied on seat data reported by Adam Carr in his online Election Archive. For elections in Colombia, I aggregated individual list-level seat totals to the party level as reported by Arend Lijphart in his online Elections Archive. I relied on Lijphart as well for seat data pertaining to the 1995 election in Turkey. For elections in Finland in the same year, I pulled seat data from the Finnish electoral commission.

¹² Occasionally, other institutional peculiarities (such as partial renewal in Argentina and multiround elections in France) resulted in observations being dropped because dependably tracking vote totals across partial and multiround elections proved to be too difficult. In some countries that employ mixed-member systems – like South Korea, Mexico, and Germany – available electoral data only covered one tier of electoral results. These countries were also dropped from the data set because, without data from both tiers, we would have no way of understanding how parties' deliberations at these levels affected their entry decisions overall.

votes.¹³ I work from the fundamental assumption that, if a party stood for election in a given district, then it received at least one vote in the district.¹⁴

Expanding the data set to the party-district-entry-decision creates a huge number of observations, but not all of these are relevant. On a party by party basis, I drop observations related to two additional types of parties. First, by election, I drop observations for parties that entered all districts in a given election (otherwise, there is no variation on the outcome variable for these parties).¹⁵ Second, I drop observations pertaining to independent candidates as, by definition in legislative elections, an independent candidate can never contest more districts than her own. Due to the need to include lagged values as explanatory variables, no party enters the regression analysis until its second election appearance. Despite culling the data set in this way, I am left with more than 500,000 entry observations.¹⁶

I control for a number of variables related to the hypotheses articulated above. First, I

¹³ Practically speaking, the data sets that I draw on for electoral returns treat non-entry in systematic ways that make the distinction between entry and non-entry even clearer. The CLE data set simply leaves blank cells in the data for districts where a party did not field a candidate and the CLEA only includes party-district observations where the party received at least 1 vote.

¹⁴ This is not a terribly heroic assumption, given the fact that it would require very few resources for a party to net one vote in any given electoral competition (indeed, we might expect that the candidate who stood in the competition would at least vote for herself or himself). Still, it is worth conceding the point that a party's banner might have appeared on a ballot on election day, yet the party received no votes.

¹⁵ However, and importantly, the empirical results of the statistical model do not change when I include *all* parties, including those that entered all districts uniformly. Thus, even if one argues that uniformly-entering parties simply decide "yes" on a district-by-district basis for *all* districts in a country (a decision calculus which I find unlikely), accounting for this empirically does not affect the results presented here.

¹⁶ This data also encapsulates more than 4,000 party-elections, which will be discussed in greater detail below. The countries included in the analysis are: Australia, Austria, Belgium, Bermuda, Bulgaria, Canada, Colombia, Costa Rica, Croatia, Czech Republic, Dominican Republic, Estonia, Finland, Greece, Grenada, Hungary, Iceland, Ireland, Italy, Jamaica, Japan, Luxembourg, Mauritius, New Zealand, Norway, Poland, Portugal, Romania, Slovenia, Spain, Sri Lanka, Sweden, Switzerland, Trinidad and Tobago, Turkey, United Kingdom, United States, Venezuela, West Germany, and Zambia.

control for *logged district magnitude* because of the well-known implications of this variable for party competition (*H3*). Second, I control for *previous resource investment* (or lagged entry) on a district-by-district basis because parties that have previously invested resources in constructing a campaign infrastructure in a district can be expected to re-enter that district in subsequent elections (*H4*). Although the structure of the data is hierarchical in nature, I do not control for variables at the country level. While it is easy to argue that many country-level variables (such as federalism, say, or whether the country is a presidential democracy) might impact the *incidence* of selective party entry, it is rather more unclear how any of these variables might affect the *specific entry decision in any given district*. So while there might be a discernible difference in the average percentage of districts entered by parties in Sweden when compared to parties in Spain (as, indeed, there is if we consider again Figure 1), it is very difficult to see how Sweden’s national-level institutions vis-à-vis Spain’s national-level institutions would differently impact a generic party’s decision about which of two districts to enter. I do present a model in Appendix A that includes country-level covariates (and demonstrate that my results do not change in this context), but I maintain that we derive the most explanatory power for subnational variation in selective entry by focusing on variables that vary across districts and across parties within the same country.

At the level of the party, we can imagine a number of variables that would impact not only the party’s incidence of selective entry, but that might also inform its specific district-by-district entry decisions. Unfortunately, in a cross-national study, many of these variables (like the extent to which the party’s platform is regional in nature) are impossible to obtain.¹⁷

¹⁷ However, in a robustness check presented in Appendix B, I attempt to control for some of these potentially confounding party-level factors. In results not reported here, it is worth pointing out that the model’s results hold on two additional theoretically relevant subsets of the data: first, on those parties that entered two or more districts and, second, on those district entry observations where parties did not previously enter. These two tests drive home important points: the inclusion of single-district entrants or “nuisance” parties is not driving the results of the model presented below and, additionally, despite lagged entry’s strong role in predicting entry, it is not sufficient by itself to drive the results of the model.

For the time being, I focus on one party-level attribute that is readily available based on electoral returns – *party capacity* – because I expect that this attribute will qualify the relationship I have hypothesized exists between win potential and the probability of entry (*H2*). I operationalize party capacity as the share of districts a party entered in the previous election.¹⁸ For the same party across different elections, then, this variable should take on different values. In the model that follows, I interact win potential with party capacity to demonstrate the diminishing marginal utility of win potential as an information source as parties contest increasingly more districts. I now turn to a discussion of this model.

Modeling Selective Entry Cross-Nationally

Selectively entering parties make a set of dichotomous yes-or-no entry decisions – one decision per district per election. Because these decisions are nested in broader contexts, the appropriate modeling strategy is a multilevel logistic regression where each yes-or-no entry decision at the party-district-election level is nested within a particular party’s set of decisions in a given election. At the same time, the attributes of each district that do not change from election to election (like magnitude) should comprise their own separate level in the data’s hierarchy. In the analysis that follows, I specify a hierarchical logistic model as:

$$\begin{aligned} \Pr(\text{entry}_{ped} = 1) &= \text{logit}^{-1} \left(\gamma_d + \gamma_{pe} + \beta_1 \text{winpotential}_{ed} \right. \\ &\quad + \beta_2 \text{investment}_{ped,t-1} \\ &\quad \left. + \beta_3 \text{winpotential}_{ed} * \text{capacity}_{pe,t-1} + \sigma_{ped}^2 \right) \\ \gamma_d &\sim \mathcal{N}(\alpha_d + \delta_1 \log(\text{magnitude}_d), \sigma_d^2) \text{ for } d = 1, \dots, D \\ \gamma_{pe} &\sim \mathcal{N}(\alpha_{pe} + \xi_1 \text{capacity}_{pe,t-1}, \sigma_{pe}^2) \text{ for } pe = 1, \dots, PE \end{aligned}$$

Here γ_d and γ_{pe} are, respectively, district-level and party-election-level random intercepts that are modeled, again respectively, by the log of district magnitude (which, in general,

¹⁸Using the share of districts entered in the *current* election would be endogenous to the outcome variable.

should not vary from election to election) and lagged party capacity (which, in general for selectively entering parties, *should* vary from election to election). The β s in the model correspond to coefficients for, respectively, win potential (which varies at the district-election level in *this* election based on voter coordination in the district in the *past* election), a party's lagged entry decision in the district, and an interaction between win potential and lagged party capacity. In the data set, the districts are indexed by country (such that two similarly-named districts in different countries are not, in the model, included in the same country) and the parties are indexed by country and election year (such that a party in the United States in 2008 is not also faced with entering districts in Spain in 2008). In the models that follow, then, I will present estimates for the coefficients β_1 , β_2 , β_3 , δ_1 , and ξ_1 in addition to random effects at the district-level (γ_d) and the party-election level (γ_{pe}) together with measures of variance across these random intercepts at each level.¹⁹

Recall that win potential and logged district magnitude are standardized within each set of country-election observations to convey the relative tradeoffs that parties face in selectively entering one district at the expense of another. To aid in making the regression models' coefficient estimates across explanatory variables more directly comparable, I have similarly mean-centered party capacity. The regression model in Table 1 includes all observations in the data set for selectively entering parties. Coefficient estimates appear in the second column and the bounds of the 95% confidence intervals appear in the third column.²⁰ The model overall is a good fit to the data. A log likelihood ratio test indicates that the model performs significantly better than a null baseline model that includes only a constant, a lagged outcome variable predictor, and the random intercepts. In addition, the model's error rate (i.e. predicting $y_i = 0$ when $\text{logit}^{-1}(X_i\beta) > 0.5$ or predicting $y_i = 1$ when $\text{logit}^{-1}(X_i\beta) < 0.5$) across more than a half-million observations is just 4%. As expected, the coefficient on win potential is positively signed. Party capacity exerts a strong and

¹⁹ In estimating the logistic model, variance at the level of the observation (σ_{ped}^2) is set to 1.

²⁰ All coefficient estimates in Table 1 are highly statistically significant at the $p \leq 0.001$ level.

Table 1. Hierarchical Logistic Regression Model of the Probability of Entry

| | Estimate | 95% C.I. |
|--|-----------------|------------------|
| Win Potential | 0.10 | [0.08 , 0.12] |
| Party Capacity _{<i>t</i>-1} | 0.65 | [0.60 , 0.70] |
| Potential x Capacity _{<i>t</i>-1} | -0.07 | [-0.08, -0.06] |
| Previous Investment | 3.25 | [3.20 , 3.30] |
| Magnitude (Logged) | 0.51 | [0.47 , 0.55] |
| Intercept | -2.77 | [-2.83, -2.71] |
| $\hat{\sigma}_d^2$ | 0.18 | |
| $\hat{\sigma}_{pe}^2$ | 2.22 | |
| $\hat{\sigma}_{ped}^2$ | 1.00 | |
| <i>N</i> districts | 3,750 | |
| <i>N</i> party-elections | 4,069 | |
| <i>N</i> obs. | 522,198 | |

Notes: All coefficient estimates are statistically significant at the $p \leq 0.001$ level. All coefficients have been mean-centered to aid in comparability. The bounds of the 95% confidence intervals were generated from 1,000 simulated draws of the model's parameters.

statistically significant impact on the probability of entry and – as values of this variable increase – its interaction with win potential indicates that the strength of win potential as a predictor decreases. Previous investment in an electoral district and logged district magnitude, as expected, both exert sizable positive effects on the probability of entry.²¹

The substantive impact of win potential, party capacity, and their interaction on the probability of entry is difficult to assess without graphical aid. Figure 2 depicts win potential’s multiplicative impact on the odds of entry for different values of party capacity. When this multiplicative effect is greater than 1.0, then the effect of increasing win potential increases the odds of entry. When it falls below 1.0, the the effect of increasing win potential is a decrease in the odds of entry. The black line is the predicted multiplicative effect and the dashed lines represent 95% confidence intervals.²² In support of *H1*, we can clearly see that increasing win potential positively increases the odds of entry across the entire range of party capacity values (and the uncertainty around this predicted effect is bounded well away from 1.0). Also, in support of *H2*, we see that the magnitude of the positive effect of increasing win potential on the odds of entry is decreasing as party capacity increases.

To assess the effect of win potential on the odds of entry in more concrete terms, let me discuss it in terms of underlying probabilities. Table 2 provides a summary of how each explanatory variable impacts the predicted probability of entry. These probabilities are based on 1,000 simulations of the model’s parameters, with the predicted value appearing at the top of each row and the bounds of its corresponding 95% confidence interval in brackets below. In all cases “low” indicates the first quartile value of the variable and “high” indicates the third quartile value. In support of *H2*, we see that increasing win potential across its interquartile range increases the probability of entry by about 8% for low capacity

²¹ An objection to the modeling strategy in Table 1 might be that parties that selectively enter only one district in an election are not necessarily doing so with an eye toward sustained and strategic electoral competition. As noted previously, the results of the model do not change when I drop single-district entrants.

²² The confidence bounds run parallel to the predicted effect due to the almost completely uniform distribution of party capacity values over the range depicted on the x -axis.

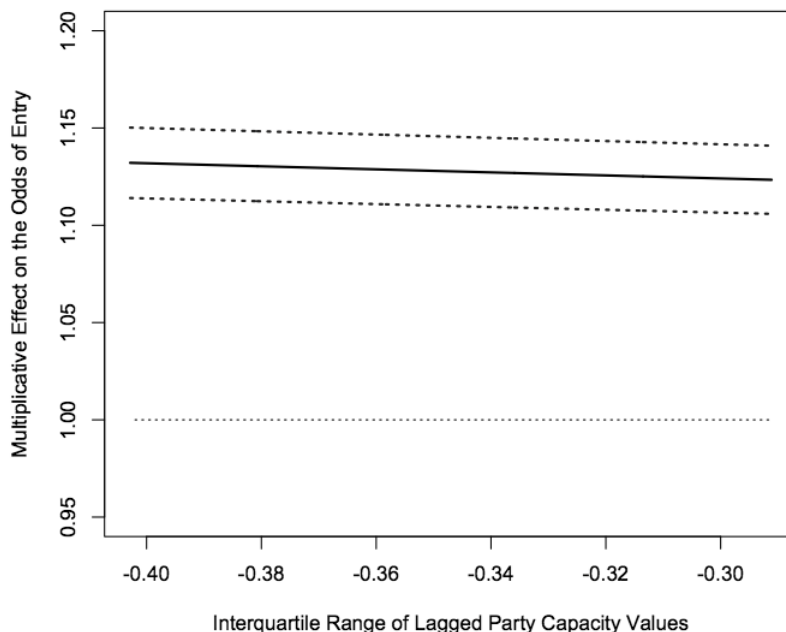


Figure 2. Win Potential’s Effect on the Odds of Entry Across Different Values of Lagged Party Capacity

parties, but increases the probability of entry by only about 3% for high capacity parties. Additionally, the confidence intervals on these two predictions indicate that we can be surer about a statistically discernible difference at the lower end than we can at the higher end.

The other explanatory variables also exert statistically discernible impacts on the probability of entry. First, in support of $H3$, we can see that increasing logged district magnitude across its interquartile range increases the probability of entry by about 21%. In support of $H4$, we see that when a party has previously invested resources in an electoral district, it is markedly more likely to enter that district in the next election. Indeed, previous investment jumps to the fore as having the most important impact on entry decisions. After having previously invested resources in a district, it seems that parties have an infrastructure in place that they can return to in subsequent elections. That the value of this infrastructure might outweigh the party’s actual electoral performance in the district is a potentially interesting finding that deserves greater exploration. This result points to the relatively static nature of party competition among those entrants that do not uniformly contest all districts. By

Table 2. Predicted Probabilities of Entry for Each Explanatory Variable

| H1 & H2 | Low Win Potential | High Win Potential |
|--------------------|----------------------------------|--------------------------------|
| Low Capacity | 0.51 [0.49, 0.53] | 0.55 [0.53, 0.57] |
| High Capacity | 0.58 [0.56, 0.60] | 0.60 [0.58, 0.62] |
| H3 | Low Magnitude | High Magnitude |
| “Average” Case | 0.56 [0.53, 0.59] | 0.68 [0.65, 0.71] |
| H4 | Previously Uninvested | Previously Invested |
| “Average” Case | 0.05 [0.04, 0.06] | 0.61 [0.59, 0.63] |

Notes: Low Capacity and High Capacity examples include setting logged district magnitude to its mean and setting previous investment to 1. Average Case examples include setting other variables at their means. Random district-level and party-election-level intercept values were held at their means across all examples. In all cases “low” indicates the first quartile value and “high” indicates the third quartile value.

examining a selectively entering party’s choice of entry decisions in its first election, we can learn quite a bit about its long-run patterns of entry.

Of course, these probabilities of entry will look different for different parties and herein lies the utility of hierarchical models. The hierarchical modeling strategy allows for random intercepts at the level of the party-election. As indicated graphically in Figure 1 and by the variances reported in Table 1, we see important variance in these random intercepts both within and across countries, indicating that the underlying probability of entry for differently-endowed parties and more-or-less regionalized parties are affected in different capacities by the model’s explanatory variables. There is obvious evidence that different parties assign varying importance to different factors when making their entry decisions. As is the case in the United Kingdom – where regionalized parties like the Scottish National Party and Plaid Cymru compete next to more nationalized parties such as Labour and niche parties like UKIP and the Greens – parties employ diverse strategies in pursuing their electoral goals. A hierarchical model employing data disaggregated to the district level is sufficiently flexible to allow parties to retain their diversity. In a sense, the modeling approach makes far fewer assumptions about parties’ behavior than do studies that focus on national-level data or do not make adequate use of the natural hierarchies in the data structure.

Conclusion

I began this paper by pointing out a theoretically important gap in the literature on strategic party entry and, furthermore, by demonstrating that this gap in our understanding of electoral politics has left us without an explanation for important empirical variation at the subnational level. Investigating the entry decisions of parties at the aggregated national level obscures a vitally important additional strategic calculation that these parties must conduct: how do selectively entering parties decide on a subset of all possible electoral constituencies in which to enter? Due to the fact that *most* parties around the world are selectively entering parties, this is a question in need of answering. Prior to this study, the question of selective

entry has never been addressed at the district level in a comparative fashion. In those terms, this paper marks the first effort to frame district-by-district entry decisions in a hierarchical data structure that directly models the implicit tradeoffs these selectively entering parties face in deciding where to field candidates or party lists.

I argue that *win potential* – a ratio that encapsulates both the marginality of the district’s cheapest seat as well as the extent of poor voter coordination – serves as an important informational cue to selectively entering parties. As win potential increases, *ceteris paribus*, parties should enter a district with higher probability. This relationship should be qualified, however, depending on party capacity or the amount of resources a party has with which to field candidates in multiple districts. Indeed, as party capacity increases, the tradeoffs inherent in entering one district at the expense of entering any one of a number of others should diminish. At lower levels of party capacity, where a selectively entering party can only enter a very small subset of all possible districts, these tradeoffs make themselves felt and parties in such positions rely more heavily on the information carried by the win potential ratio. Increasing win potential does indeed have a positive and statistically discernible impact on a party’s probability of entry in a given district. The same holds true for party capacity.

The analysis also confirms previous findings on electoral competition, but frames them in a new perspective. That district magnitude should consistently turn up as positively impacting a party’s probability of entry is not at all surprising. Indeed, a long line of studies has demonstrated that district magnitude is one of the main determinants (if not the primary determinant) of the raw number of entrants in a district as well as the effective number of parties. The value added of the findings presented in this study, however, is the explicit modeling of the tradeoffs selectively entering parties face as they deliberate simultaneously over a number of dichotomous entry decisions. Rather than simply understanding electoral coordination as a district-level phenomenon that structures generic numbers of parties with generic vote distributions, the findings of this paper point to a party-level causal story about why this might be the case. I am able to demonstrate that selectively entering parties enter

districts with larger magnitudes *specifically at the expense of* entering districts with comparatively smaller magnitudes. The same is true for those districts where parties previously invested resources: parties clearly prefer to reenter districts they are familiar with rather than strike out into new territory.

Beginning to understand the determinants of selective entry is an important step in refining what we know about party system nationalization and social preference aggregation. Nationalization is typically studied as a demand-side phenomenon: when voters uniformly support parties across districts, then a party system is thought to be nationalized in nature. But this conceptualization takes as given the fact that all voters are presented with the same party offerings. The same is true when thinking about ballots as choice sets and when treating the outcomes of elections as articulations of meaningful social choice: without meeting the prerequisite of uniform ballots, we have to be wary of collective preferences. Whenever a party enters one district, but not another, it inserts a complication for voters – in the aggregate – when it comes time for them to choose some parties over others or support some parties more broadly than others. Indeed, “nationalization” in vote distributions and “choices” on ballots in the aggregate do not mean what we think they mean when most parties are entering districts selectively. Broad-based party support and collective preferences, as concepts, are most informative either when most parties universally enter all districts or – as this is rarely the case – when we can accurately account for patterns of selective entry. This study is an important first step, then, in deepening the conceptual usefulness of party system nationalization and electoral balloting choices.

Avenues for future research are manifold. A subset of countries from this cross-national data set could be integrated with data on party resources, party organization, and campaign finance laws in an effort to model more explicitly the *party capacity* concept developed in this paper. Although I control for previous entry decisions, the modeling strategy in itself is not evolutionary in nature and, on this front (especially with additional data collection that might extend the time series within each party) explicitly modeling the process by which a

new party first enters a few districts and then strategically develops over time into a more major political player could evince important evolutionary dynamics. In considering how any one party pits itself against a range of competing parties, we might also begin to understand how parties jockey against similarly-oriented parties (either in ideological or financial terms) for district-by-district holds on different constituencies within the same country.

Appendix A: Modeling Country-Level Covariates

I have argued that the best predictive model of selective entry – a phenomenon that varies at the subnational level – should be predicated on variables that can assume different values below the level of the country as a whole. While certain types of national level institutions may incentive a party to enter more or fewer districts *overall*, it would be difficult to argue, say, that the fact that a country is a presidential democracy should have any bearing on a party’s decision to enter *any specific district* at the expense of entering another. Indeed, the model without country-level covariates presented above exhibits a strong fit to the data (showing dramatic improvement over a null model that just includes a lagged dependent variable and intercepts at the district- and party-election-level) and correctly models about 96% of more than a half-million observations.

However, drawing on the full scope of the data set’s inherent hierarchy, in this appendix I include a model with several country-level covariates. This robustness check demonstrates that the statistically significant impact of win potential, district magnitude, and previous investment on a party’s probability of entry hold in the face of controlling for the extent of ethnolinguistic *fractionalization*, whether or not the country is *federal*, whether or not the country is a *parliamentary* democracy, whether or not the country’s electoral system is a *proportional* one, and the total *number of districts* in the country as a proxy for its size. The first of these controls is taken from Fearon (2003), the next from Treisman (2007), and the remainder are taken from the World Bank’s *Database of Political Institutions*. Due to limited data availability, the number of countries included in the analysis is 34.

As can be seen in the table below, the highly statistically significant relationships between the original variables and the probability of entry all maintain. Of the country-level covariates, the only variables that come close to this level of significance are *federalism* and the *number of districts*, which are signed in intuitive directions: in more federalized countries, selectively entering parties are less likely to enter broadly and as the number of districts increases, these parties are also less likely to enter any given district.

Table A1: Hierarchical Logistic Regression Model of the Probability of Entry with Country Covariates

| | Estimate | 95% C.I. |
|-------------------------------------|-----------------|------------------|
| <i>Original Variables</i> | | |
| Win Potential | 0.05 | [0.03 , 0.07] |
| Party Capacity _{t-1} | 0.53 | [0.49 , 0.57] |
| Potential x Capacity _{t-1} | -0.06 | [-0.07, -0.05] |
| Previous Investment | 3.61 | [3.56 , 3.66] |
| Magnitude (Logged) | 0.48 | [0.44 , 0.52] |
| <i>Country-Level Variables</i> | | |
| Fractionalization | -0.16 | [-0.38, 0.06] |
| Federalism | -0.81 | [-1.32,-0.30] |
| Parliamentarism | -0.21 | [-0.63, 0.24] |
| Proportional | -0.70 | [-1.46, 0.03] |
| Number of Districts | -1.13 | [-1.57,-0.68] |
| Intercept | -2.80 | [-3.40,-2.21] |
| $\hat{\sigma}_d^2$ | 0.14 | |
| $\hat{\sigma}_{pe}^2$ | 1.32 | |
| $\hat{\sigma}_c^2$ | 0.55 | |
| $\hat{\sigma}_{ped}^2$ | 1.00 | |
| <i>N</i> districts | 3,703 | |
| <i>N</i> party-elections | 3,919 | |
| <i>N</i> obs. | 520,046 | |
| <i>N</i> countries | 34 | |

Notes: The estimates for all of the *original* variables remain statistically significant at the $p \leq 0.001$ level. Of the country-level variables, the only two that come close to this level of significance are *federalism* and the *number of districts*. All coefficients have been mean-centered.

Appendix B: Selective Entry in Regionalized Systems

A straightforward objection to the theory I presented above would be to argue that selectively entering parties are overwhelmingly regional parties and that these parties enter specifically (and simply) in “their” region’s electoral districts and nowhere else. In this case, knowing whether or not a district falls in a given region should be sufficient to predict whether the party entered the district and this consideration should override the other explanations I have already advanced. While I have previously argued that the conceptual label of “selectively entering party” actually incorporates a much broader range of “types” of parties beyond only regional parties, I grant that the argument has merit and I attempt to address it in this appendix. Accounting for the extent to which the content of individual parties’ policy platforms are “regionalized” in a cross-national study is nearly impossible. Instead, I demonstrate that my results hold *in particular* in countries with marked regional cleavages and with patterns of regional competition.

Brancati (2008) demonstrates that the extent of political decentralization in a country is positively linked to the formation and success of regionalized parties. When freestanding regional parliaments exist, for example, region-based parties have additional incentives to compete in elections. I select a subset of “decentralized” (as identified by Brancati) countries from my data and run the same regression models as above. These countries include Australia, Belgium, Canada, Finland, Italy, Spain, Switzerland, the UK, the US, and Venezuela. As can be seen in Table 1 below, the coefficients remain statistically significant, similarly signed, and retain their relative magnitudes. Party capacity plays a slightly less substantial role in determining entry, perhaps because while a party’s capacity in a particular *region* may be quite high, its capacity in the *country* more generally could be rather low. This makes party capacity (which is measured at the national level) a less informative predictor of entry for regionalized parties that do not necessarily have an interest in contesting all of a country’s districts.

Table B1: Hierarchical Logistic Regression Model of the Probability of Entry (Regionalized Country Subset)

| | Estimate | 95% C.I. |
|-------------------------------------|-----------------|------------------|
| Win Potential | 0.08 | [0.06 , 0.10] |
| Party Capacity _{t-1} | 0.59 | [0.51 , 0.67] |
| Potential x Capacity _{t-1} | -0.10 | [-0.12, -0.08] |
| Previous Investment | 3.93 | [3.88 , 3.98] |
| Magnitude (Logged) | 0.52 | [0.46 , 0.58] |
| Intercept | -3.27 | [-3.35, -3.19] |
| $\hat{\sigma}_d^2$ | 0.14 | |
| $\hat{\sigma}_{pe}^2$ | 3.92 | |
| $\hat{\sigma}_{ped}^2$ | 1.00 | |
| <i>N</i> districts | 2,496 | |
| <i>N</i> party-elections | 2,733 | |
| <i>N</i> obs. | 467,922 | |

Notes: All coefficient estimates are statistically significant at the $p \leq 0.001$ level. All coefficients have been mean-centered to aid in comparability. The bounds of the 95% confidence intervals were generated from 1,000 simulated draws of the model's parameters.

Appendix C: Correcting for Spatial Autocorrelation

Spatial autocorrelation in outcome variables arise from the fundamental proposition that units of observation (in this case, electoral districts) that are spatially closer together are more similar than those districts that are spatially further apart (Tobler, 1970). When it comes to selective party entry, it is reasonable to assume that, having made the choice to invest resources in contesting a given district, a party might more cheaply also contest a *contiguous* district rather than a district that is further removed. This is due to a number of reasons. The constituents in two districts that are located in close proximity to one another will, with higher probability, share more characteristics in common than constituents living in two districts that are further apart (Johnston, 1992; Chen and Rodden, 2009). This is especially the case if two districts fall within the same region in a country where regions are politically salient. It might also be the case that a campaign office in a given district or a set of volunteers or employees can easily be shuttled to a contiguous district. Media markets, like those in the United States, may also subsume multiple contiguous electoral districts, thereby making it easier for a party to cover contiguous districts with television advertisements after they have already paid to do so in one. The presence of such spatial autocorrelation in entry decisions poses a potential challenge to the theory developed in this paper. After all, the relationship between win potential and the probability of entry may be significantly weakened after accounting for the simple fact that parties prefer to cluster their entry decisions so as to make use of increasing returns to scale on their investment of resources. Adding an additional district is comparatively cheaper if it is proximate to preexisting campaign resources than if it is much further away.

Recently, spatial autocorrelation has received greater attention as a methodological issue in the field of political science (Franzese and Hays, 2008), especially in studies of policy diffusion (Beck, Gleditsch and Beardsley, 2006), and electoral geography (Rodden, 2010). However, the methodological toolkit is still somewhat limited. A potential approach to accounting for autocorrelation would be a post-estimation correction on the coefficient es-

timates produced by regression analysis. The first step toward taking this approach would be to calculate Moran’s autocorrelation coefficient (Moran, 1950) which – if it indicated evidence of spatial dependency – could then be employed in adjusting the standard errors of regression coefficients. Calculating Moran’s I becomes rather complicated in hierarchical models, however, and there is some debate as to whether it is an appropriate tool for addressing autocorrelation in dichotomous outcomes.¹

As an alternative to post-estimation adjustments in the standard errors of the regression coefficients, then, I construct an intuitively appealing measure of *contiguous clustering* that provides a measure of a party’s general proclivity to selectively enter districts in such a way that it would capitalize on the increasing returns to scale arising from geographic proximity.² Consider a given district d_i for i, \dots, n districts that a party p entered in a given election and any district d_j that is contiguous to d_i .³ Let $Entry(d_j) = 1$ when the party entered the contiguous district d_j and 0 if it did not. We can then define:

$$\text{contiguous clustering for } p \text{ in } d_i = \left(\frac{\sum_{j=1}^J Entry(d_j) = 1}{\sum_{j=1}^J Entry(d_j = 1) + \sum_{j=1}^J Entry(d_j = 0)} \right)$$

$$\text{contiguous clustering for } p = \frac{\sum_{i=1}^N \text{contiguous clustering for } p \text{ in } d_i}{N}$$

¹ A second potential approach would be to calculate a clustering coefficient of the type seen in network analysis (Holland and Leinhardt, 1971; Watts and Strogatz, 1998). However, given the structure of the data, an application of network theory would involve bipartite (two interdependent) networks – one of district contiguities and one of party entry decisions – and the complexity of calculating this value across multiple networks extends beyond the scope of this study.

² I choose to cast this measure at the level of the party rather than the district to account for the fact that some parties will rely on geographic returns to scale more than others. While a measure of clustering at the district level would encapsulate some aspects of clustering in entry decisions, it would not embody different parties’ varying reliance on geographic clustering.

³ In the case of Spain, i ranges from 1 to 52 while j ranges from 0 to 8 contiguous districts.

In words, for every district that the party enters, consider the full set of contiguous districts that surround it. Calculate the proportion of potential contiguous districts that it actually entered. Average across these district-level proportions to arrive at a measure for a given party in a given election of the extent to which geographic proximity informed its selective entry decisions. Because the resulting metric is an average proportion, it ranges from 0 (in the case where a party entered only one district or a subset of districts with no contiguous connections) to 1 (in the case where a party entered all contiguous districts in a country). Consider Figure C1 below for a graphical depiction.

Each panel depicts the mainland districts in Spain, with shaded districts indicating hypothetical patterns of selective entry. In the upper right panel, we see a party that selectively chose to enter only in those districts in the region of Catalonia. These districts are obviously clustered together and, to that extent, necessitate a high *contiguous clustering* value. However, there are still contiguous districts that the party could have *potentially* entered, yet did not. To this end, the only way for a party to have a contiguous clustering score of 1 would be to enter every mainland district.⁴ Now consider the drop in the metric's value in the upper left panel, where the party chooses to enter the same number of districts, but shifts one of its entry decisions to a non-contiguous district. Below this, in the bottom left panel, is a party that again entered the same number of districts, but without taking into consideration the increasing returns to scale offered by contiguous districts. Finally, in the bottom right panel, we see an instance of selective entry with two poles. Here is a party whose entry decisions are informed somewhat by geographic proximity, but also by other considerations.

While I argue that this contiguous clustering variable is, in the main, capturing the sorts of geographic dependencies we would like it to, I should also point out that there is a clear relationship between contiguous clustering and a previous concept I defined earlier, *party*

⁴Entry in Spain's island districts do not contribute to this score as they are not contiguous to any districts.

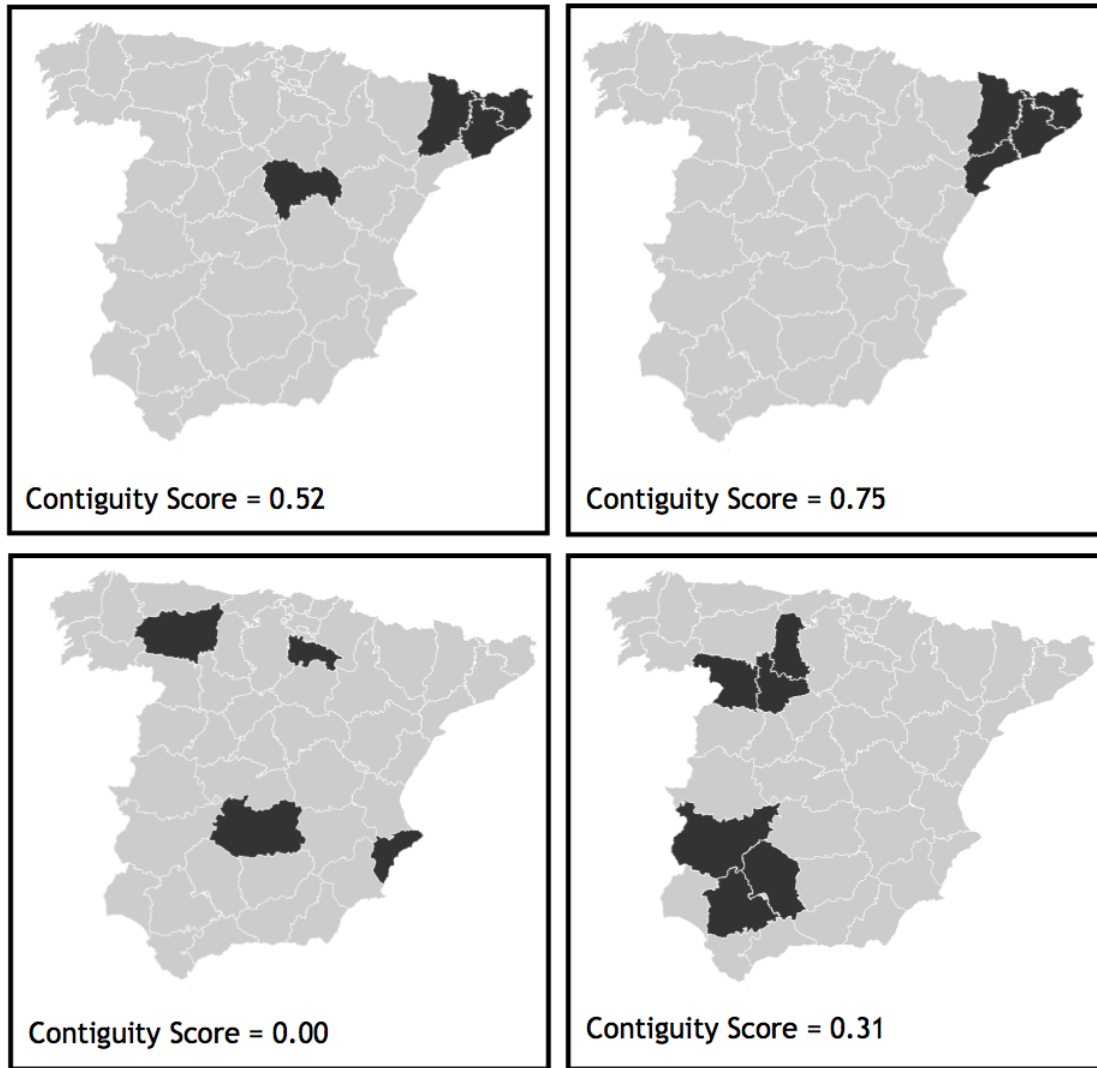


Figure C1: Four Representative Patterns of Entry and the *Contiguous Clustering* Value They Induce (Note: Catalonia-Only Entry Depicted in Upper Right Panel)

capacity. Their relationship is illustrated graphically in Figure C2 below.⁵ Intuitively, at low levels of party capacity, contiguous clustering can either take a high or low value. It is here that the clustering metric is at its most informative, because as party capacity grows it becomes harder for entry patterns to *not* look like geographic clustering. Up to party capacity around 0.5, however, we still see fairly dramatic variation in contiguous clustering, with many of Spain’s regional parties (such as the Catalonian example presented in the above figure) located in the heavy dark patches toward the upper left quadrant of the graphic.

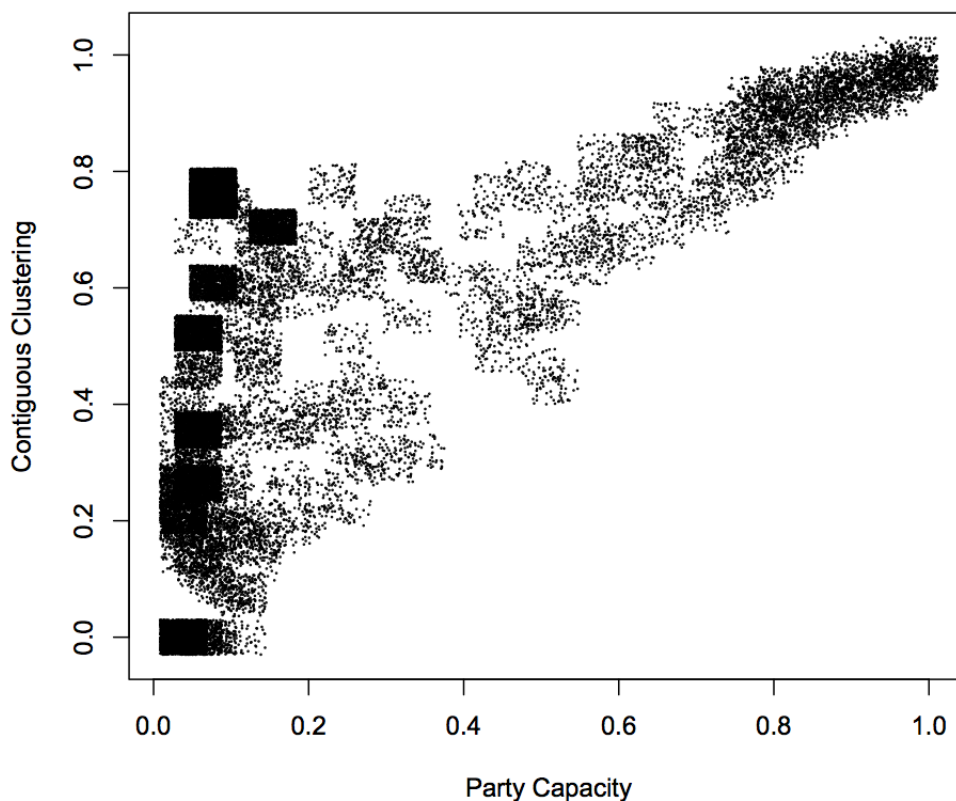


Figure C2: Plot Illustrating the Relationship Between *Party Capacity* and *Contiguous Clustering* (points have been jittered slightly to better illustrate areas of density).

⁵Recall that instances of party-election “no entry” and “total entry” are not included in the analysis, as they both indicate a lack of variation in the outcome variable.

To this extent, as party capacity was hypothesized to curb the effect of win potential on the probability of entry so, too, should contiguous clustering exert a less informative positive impact on entry at higher levels of party capacity. This is because, at a certain point, parties have the resources to enter so many districts in the country that they (a) no longer have to rely on the hypothesized increasing returns to scale and (b) begin to develop different goals in electoral competition, such as creating a national presence in the electorate. This is evinced fairly clearly in Figure C4, where most of the observations on party capacity fall either below 0.2 or above 0.8. At a certain point in a parties' growth, they simply seem to break in favor of broader, more national electoral competition.

Including the *contiguous clustering* variable in a hierarchical model of selective entry in Spain should provide a fuller explanation of the variation in entry we observe in the country. Like party capacity, this is a variable that would be cast at the party-election level. Initial modeling efforts indicate that controlling for contiguous clustering does, indeed, slightly weaken the other statistical relationships described in this paper: parties in Spain do seem to pay attention to district contiguities when making entry decisions. However, as examining this phenomenon in just one country is too limited for a true test of the theory and as constructing the contiguous clustering variable for every country in the data set would be an enormous undertaking, I set aside a cross-national test for future work. At a minimum, I hope that this appendix has illustrated some possible ways to think about (and eventually account for) the challenges posed by spatial autocorrelation.

Appendix D: Out-of-Sample Predictions in Malaysia in 1964

Another method by which we can evaluate the predictive power of a statistical model is to examine its efficiency in making “out-of-sample” predictions or, put differently, in making predictions in those countries and elections that were not originally included in the sample of observations that was employed to estimate the model’s parameters. In order to do this, I went looking for a country from a time period that was about as far removed as possible from the data that went into estimating the model that appears in this paper. I quickly settled in on Malaysia which, surprisingly enough, was a well-functioning democracy for about a decade in the 1950s and 1960s. In the middle of this time period, the country held two democratic elections using an SMD system with 100 districts spread across 11 highly distinct regions. As a check of the model’s predictive power, I pulled electoral data for two very different Malaysian parties – one small and one quite a bit larger – across these two elections. I will use the parties’ respective characteristics and previous levels of performance in the first election to model their probabilities of entry for each of the 100 districts in the second election, which took place in 1964.

In the absence of a very detailed district-level map of Malaysia in 1964, I was at least able to determine which electoral districts belonged in each of the 11 regions. A stylized representation of these districts overlaid on the actual boundaries of Malaysia’s regions appears in Figure D1 below. Although I do not know specifically where each of these districts fell relative to one another inside of each region, we can be sure about where they stand in geographic relation to districts in other regions throughout the country. Successfully predicting these entry decisions is going to be a difficult task, considering the fact that the model used to make these predictions is hierarchical in nature and thus includes random intercepts at multiple levels of the data. Due to being an out-of-sample prediction, these parties and districts, and this country cannot be assigned an appropriate set of random intercepts. Accordingly, I simply hold all of the model’s random intercepts at their means (a crude, but sufficient approach) and employ the rest of the model’s covariates as I normally would.

Figure D2 depicts the predicted probabilities of entry generated by the model for the People’s Progressive Party in 1964. Recall that these probabilities are being generated by each district’s win potential value from the previous election, the party’s capacity, and whether or not it previously invested resources in a district. Magnitude is held constant due to the fact that each of these districts has a single representative. In gradations, higher levels of probability of entry are darkly shaded. The actual entry decisions are overlaid with heavier black points. Clearly, the party missed some possible opportunities, but by and large, the party entered where the model would expect it to. The PPP only re-entered 40% of the districts it entered in the previous election, so previous investment is not single-handedly driving these results. Additionally, even though the PPP is clearly a regional party (focusing its efforts in Perack), within that region, it is distinguishing critically between districts. Holding constant magnitude and regional characteristics, then, as well as noting the comparatively low level of re-entry, we can conclude that variation in win potential is making a difference (even if only at the margins) in the successful prediction of entry.

Figure D3 depicts the predicted probabilities of entry for a much larger selectively entering party, the Malayan People’s Party. If we examine the actual entry decisions – again represented by black points – we can see that the model correctly predicts about two-thirds of the entry decisions of the MPP. Here again, the MPP only repeated its entry decisions from the previous election about 57% of the time. Although the model is not quite as effective in its predictions for this party, here is an additional – and interesting – aside: in the districts where the MPP entered “correctly” (in the sense that it entered where the model indicates it “should” have) it received on average 28% of the votes. In those districts where it entered “incorrectly” it received only 21% of the votes. This difference across the correct and incorrect districts is also statistically significant, as indicated by a simple difference-of-means *t*-test. So perhaps the model made a few mistakes in prediction for this party, but the party would have done well to pay better attention to the models results!

Malaysian Regions and "Districts" 1964

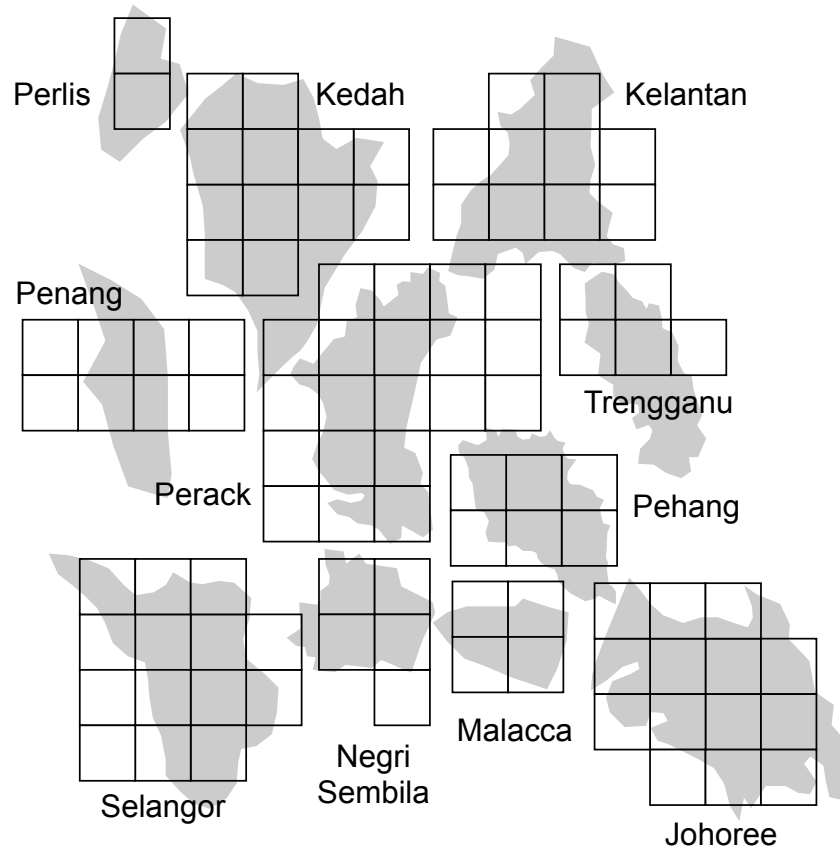


Figure D1: Stylized District-Level Representations of the Separate Regions of Malaysia in 1964.

Predicted versus Actual Entry: PPP 1964

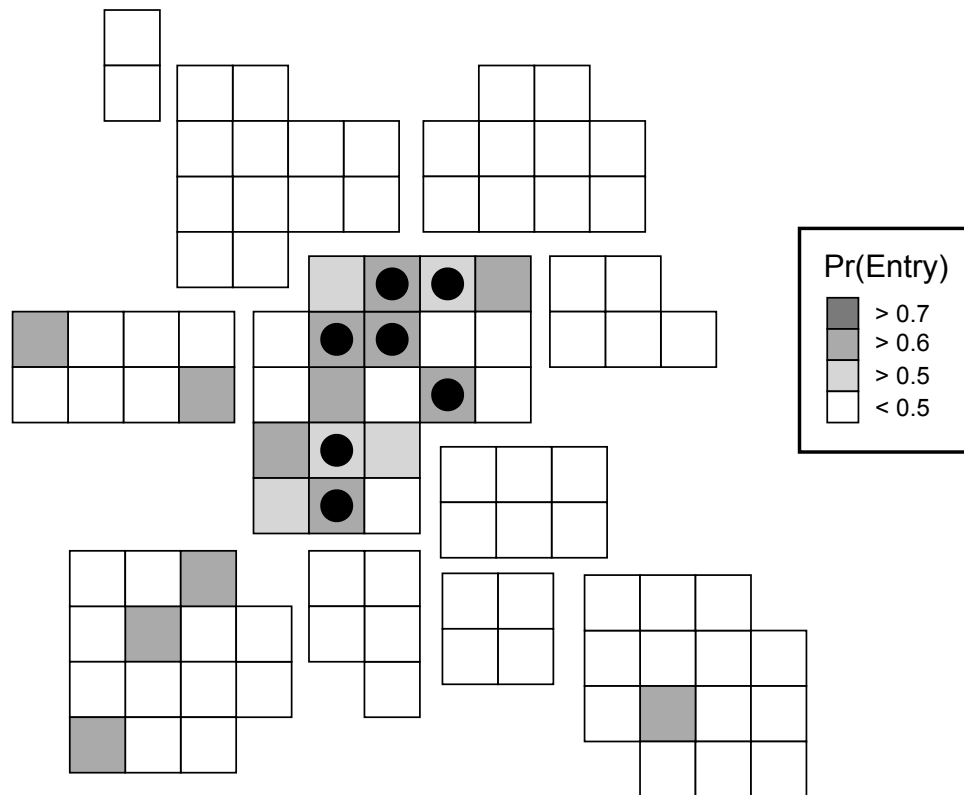


Figure D2: Predicted District-Level Probabilities of Entry and Actual Instances of Entry for the People's Progressive Party in 1964. Color gradations indicate increasingly high levels of *predicted* probability while black points indicate *actual* entry decisions.

Predicted versus Actual Entry: MPP 1964

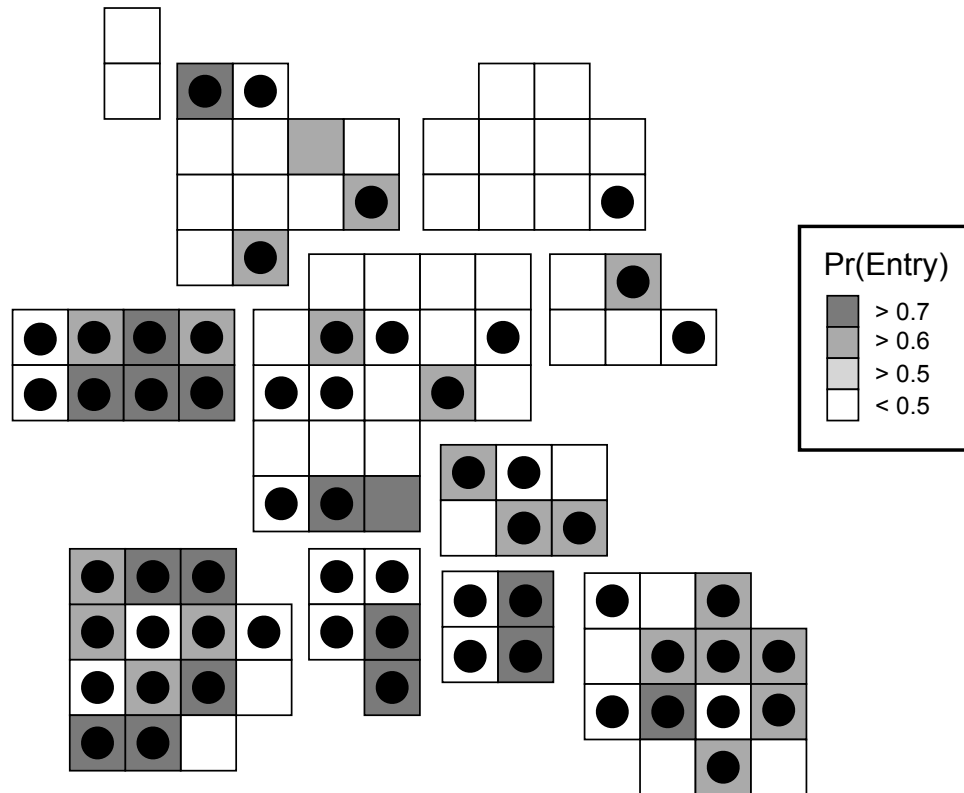


Figure D3: Predicted District-Level Probabilities of Entry and Actual Instances of Entry for the Malayan People's Party in 1964. Color gradations indicate increasingly high levels of *predicted* probability while black points indicate *actual* entry decisions.

– Paper 2 –

Within-District and Cross-District Latent Diversity and Their Impacts on Party System Size

Abstract: Scholars have long hypothesized that the size of a country’s party system is jointly determined by its latent demographic diversity and its electoral rules. However, in studies of this relationship, no clear consensus exists regarding the best operationalization of the latent diversity of a society. Furthermore, the relationship between latent diversity and electoral rules, on the one hand, and the effective number of parties, on the other, has never been tested at the level of the electoral district in a cross-national comparative context. This paper aims to fill both holes while, at the same time, introducing the novel concept of *cross-district diversity*. I find that the extent to which populations across districts are similar to one another exerts a substantial qualifying impact on the relationship between district-level diversity and the effective number of parties at the district level. This finding emerges from a statistical analysis that includes more than 2,563 electoral districts in a diverse set of 29 democracies.

Introduction

The number and relative sizes of political parties participating in elections have long interested scholars of electoral politics. This has largely been due to the fact that the effective number of parties in a political system is a powerful explanatory variable for many important outcomes of interest. Indeed, a large body of scholarship has demonstrated both theoretically and empirically that whether party systems are comprised of many (and smaller) parties or few (and larger) parties has profound implications for the extent of strategic coordination among both voters and political elites (Cox, 1997; Duverger, 1954), the extent of convergence in parties’ ideological positions (Downs, 1957; Grofman, 2004), the degree to which policy-making in the legislature is reflective of changes in the distribution of voters’ ideological positions (Powell, 2000; Przeworski, Stokes and Manin, 1999), the extent to which democratic representation is consensual in nature (Lijphart, 1999; Lijphart and Crepaz, 1991), the level of consolidation (or democratic maturity) in the party system (Bielasiak, 2002; Sartori, 1976), and even the nature of internal party organization (Mair, 1990; Katz and Mair, 1995).

Scholars have also devoted considerable time and energy to exploring the determinants of the effective number of parties in its own right. Early studies relying on both sociological (Lipset and Rokkan, 1967; Rae and Taylor, 1970) and institutional (Duverger, 1954; Rae, 1967) explanations for the effective number of parties have recently been updated by theoretical and empirical efforts to demonstrate that it is really social diversity *in interaction with* electoral rules that shape the contours of party system size (Amorim Neto and Cox, 1997; Clark and Golder, 2006; Ordeshook and Shvetsova, 1994; Stoll, 2008). While a country’s underlying pattern of social divisions might suggest a “natural” number of sustainable party offerings, the actual number of offerings may deviate significantly depending on the permissiveness or restrictiveness of the country’s electoral institutions (Cox, 1997). Thus, examining only one or the other variable can lead to incorrect estimates of the size of the party system (Clark and Golder, 2006).

However, as I will argue in this paper, aspects of the relationship between social diversity and electoral institutions, on the one hand, and the effective number of parties, on the other, remain under-studied or, even, unstudied. This is due to a number of reasons. First, studies of this relationship have tended to rely on a mismatch of data to theory (Stoll, 2008). While early theoretical arguments about voter and elite electoral coordination were very clearly cast at the district – and not national – level (Cox, 1999; Duverger, 1954; Singer and Stephenson, 2009), due to data limitations, scholars have typically relied on either national-level electoral data or national-level social diversity data (Amorim Neto and Cox, 1997; Ordeshook and Shvetsova, 1994; Singer and Stephenson, 2009; Stoll, 2008). Thus, it has only been in the past few years, with the emergence of large, district-level repositories of vote data and survey-based demographic databases, that correctly investigating this theoretical relationship has, strictly speaking, even become possible.

Second, a large and on-going debate exists about the appropriate methodology researchers should invoke when operationalizing social diversity (which is an inherently latent and difficult concept to measure). Indeed, as Stoll (2008) has convincingly argued, the choice of

measurement tool by itself can explain much of the variation we see across studies in support for the conditional hypothesis linking diversity and institutions to party system size. While scholars such as Posner (2004) and Fearon (2003) have relied on measurements of fractionalization (whether across ethnic, linguistic or religious groups), other scholars have advanced entropic metrics (Page, 2011), metrics of “cross-cuttingness” across two cleavage dimensions selected *a priori* for their relevancy (Selway, 2011), or measures of internal cohesion, like those which would be used to gauge inter-coder reliability (Crisp, Olivella and Potter, 2013). At the end of the day, then, even if the data-driven problems related to level of analysis were remedied, scholars still have at this point no clearly best methodology by which to operationalize social diversity.

Third, and most importantly, I argue it is not only diversity *within* electoral districts, but also diversity *across* electoral districts that should matter for predicting a district’s effective number of parties. Stoll (2008) notes that variation in the extent of diversity across districts within the same country could potentially impact this relationship. Due to the fact that political parties and voters hardly ever participate in electoral competition in their district without also having some information about the broader electoral dynamics playing out in other districts, it seems necessary to situate the level of diversity in any individual district in the broader context of diversity across all districts. Given recent trends in the study of party system nationalization (Morgenstern, Swindle and Castagnola, 2009), party platform linkage or aggregation across districts (Chhibber and Kollman, 2004; Cox, 1999), and voter coordination (Singer and Stephenson, 2009), it is time to also treat diversity as something that might saliently vary within the confines of the same country and differentially impact voters’ and parties’ coordination efforts in any given individual district. Indeed, as I will argue theoretically and demonstrate empirically, when we take into consideration this broader context, we begin to see that district-level party system size is differentially affected by *within-district* and *cross-district* latent diversity. In order to begin thinking about this more complex multilevel relationship, however, we must first satisfy the data and

measurement prerequisites mentioned above.

Although these prerequisites have not been met in previous studies, I aim to address them in this paper. Employing district-level survey data about social diversity from the *Comparative Study of Electoral Systems* and district-level electoral data collected from various sources, I am able to correctly test the conditional hypothesis relating diversity and institutions to the effective number of parties. Perhaps more importantly than this, however, I develop a novel method of combining voters' differences across multiple social divisions and then demonstrate theoretically how *within-district* and *cross-district* diversity can work at cross purposes in determining the effective number of parties at the district level. My argument is that, while increasing diversity at the district level drives up the “natural” number of potentially differentiable party platforms, increasing diversity across districts makes it more difficult for parties to pitch the same platform (and its attendant campaign messages, resources, and infrastructure) across different districts. Thus, while increasing diversity *within* a district drives up the effective number of parties in that district, driving up diversity *across* districts should modify the strength of this relationship.

In order to appropriately demonstrate this differential effect between within-district and cross-district diversity, I first use a “topics model” or Latent Dirichlet Allocation (LDA) model to sort individuals into groups based on their responses to several demographic questions. This methodology has many advantages over previous metrics of latent diversity, the most important being that it makes no *a priori* assumptions about which and how many social divisions are the most salient. I then use the LDA latent number of groups and the number of individuals within each group to construct my main explanatory variables: the *effective number of groups* or (*ENG*) both within and across electoral districts. I am able to demonstrate that “high diversity” (which has previously only been measured at the national level) can either be a result of high levels of diversity within each individual district or, alternatively, large differences in composition between socially homogeneous districts. Using both within-district *ENG* and cross-district *ENG* as explanatory variables, I then build a

hierarchical linear model of the effective number of electoral parties. I find that distinguishing between types of diversity provides a more complete and accurate understanding of the determinants of party system size and also has important implications for our understanding of other electoral phenomena such as party system nationalization and party system consolidation. In the next section, I will first briefly review the extant literature on this question before then developing in detail my arguments regarding within- and cross-district diversity.

Diversity, Institutions, and Party Systems

In a recent summary of the state of the literature on diversity and electoral institutions, Stoll (2008) defines *social cleavage* as: “large-scale divisions within a society that are exogenous to the political system” (p. 1441). To drive home both the exogeneity of these social divisions as well as the importance of thinking about *diversity* in continuous terms (rather than as a sorting of disjointed groups, for instance) she adopts the terminology *latent diversity* which I will also employ in this paper. As a collection of individuals’ latent diversity increases, so too does the aggregated complexity of their behaviors, interactions and – most importantly for electoral politics – their political preferences (Page, 2011). As increasingly different types of individuals bring increasingly complex permutations of preferences to the table, the number of (potentially) differentiable party platforms increases (Lipset and Rokkan, 1967; Rae and Taylor, 1970). Obviously an electoral district with some wealthy Catholic voters and some poorer Protestant voters harbors a more complex set of preferences than a district that is homogeneously Protestant and wealthy. Accordingly, we would expect more party offerings in the former situation than in the latter.

Thus, as Cox (1997) has argued, the level of latent diversity in an electoral district suggests a “natural number” of potentially distinguishable party platforms (p. 140). Given an underlying distribution of voters across a variable number of social dimensions – and assuming that more similar people have more similar preferences – political elites will construct party platforms to cater to the needs of different groups, so long as they are substantial

enough in size (Lipset and Rokkan, 1967; Rae and Taylor, 1970). Less conducive to party system growth are constituencies that are homogeneous in construction, where the “natural number” of platforms is severely circumscribed and there exist fewer opportunities for parties to differentiate themselves from one another. Following from this discussion is a straightforward and well-known hypothesis:

H1: As latent diversity within an electoral district increases, the effective number of electoral parties in that district will increase.

A separate literature predicated on electoral institutions argues that district magnitude is the primary determinant of the effective number of parties at the district level. Beginning with Duverger (1954), who speculated that two-party systems arise from single-member district systems because voters are unwilling to waste their votes on non-viable party offerings, more recent scholarship has generalized this relationship to multi-member districts with magnitudes greater than one (Cox, 1997; Cox and Shugart, 1996; Taagepera and Shugart, 1989). As district magnitude increases, the overall level of proportionality in the translation of votes into seats does so as well, meaning that parties can reasonably hope to gain representation in the legislature with comparatively fewer votes (Anckar, 1997; Benoit, 2001; Gallagher, 1991). Additionally, voters in this setting face much lower probabilities of wasting their votes on non-viable parties. This greater permissiveness induces more parties to compete and thus drives up the effective number of parties:

H2: As an electoral district’s magnitude increases, the effective number of electoral parties in that district will increase.

However, as has been argued repeatedly, neither *H1* nor *H2* adequately account for the interplay between diversity and electoral institutions (Amorim Neto and Cox, 1997; Ordeshook and Shvetsova, 1994). Specifically, electoral rules that shape the proportionality of the vote-to-seat translation (most notably district magnitude) can perturb the number of party offerings from the “natural number” that we would expect given an electoral district’s underlying diversity (Clark and Golder, 2006; Cox, 1997). Given an underlying level of di-

versity, district magnitude must be sufficiently high if we are to realize the logic articulated above in *H1*. Otherwise, even if parties can potentially offer a large number of differentiable platforms, high levels of disproportionality resulting from low district magnitudes will compel voters to “defect” from their most-preferred party offering if they sense that it is electorally non-viable. In this way, Cox (1997) argues that district magnitude serves as an “upper bound” on the effective number of parties: increasing district magnitude mechanically increases proportionality, which should allow more parties to stake out electorally viable positions predicated on underlying social divisions. Should this “upper bound” be set comparatively low, however, potential avenues of political competition (as suggested by the level of diversity) will not be mobilized during electoral competition. Strategic voters will instead opt for larger parties with broader platforms (or with platforms that simply ignore less salient social divisions). This line of reasoning leads to the “interaction” hypothesis:

H3: Increasing an electoral district’s latent diversity will only result in an increase in the effective number of parties if the district’s magnitude is sufficiently large.

The “sufficiently large” qualifier in *H3* is somewhat vague, but different studies have placed differing levels of emphasis either on the overall conditionality of the relationship or the specific marginal effect of one or the other variable. Recent work has also simplified the relationship somewhat by positing that district magnitude exerts a “smaller impact” in homogenous districts than it does in heterogeneous districts (Singer and Stephenson, 2009). In an effort to examine these slightly different formulations of the hypothesis, in the discussion of the model’s results I will point out whether diversity’s impact on the effective number of parties is noticeable for all values of district magnitude (i.e. is always increasing and significant in district magnitude) or is noticeable only for certain levels of magnitude (i.e. is insignificant below a certain threshold of district magnitude).

However, within-district diversity is not the only sociological variable that should matter for the effective number of parties in any given individual district. Scholars of electoral politics are beginning to realize that studying the internal dynamics of any one district with-

out considering broader, cross-district considerations cannot provide a full explanation for district-level dynamics. Singer and Stephenson (2009), for example, argue that, in countries with variation in magnitude across districts, the presence of larger, multimember districts at the same level of vote allocation should drive up the effective number of parties in single-member districts. Having already gained a foothold in more permissive districts, the marginal cost of fielding a candidate or list of candidates in a more restrictive district should be lower. Additionally, scholars interested in partisan bias have argued that the distribution of voters *across* districts should result in individual districts being “safer” for certain political parties than they should be (Cox and Katz, 1999; Grofman, Koetzle and Brunell, 1997; King and Gelman, 1991). When geographic reality results in the concentration of like-minded voters in the same districts, for example, then voters with minority political preferences in such districts may lose out on meaningful representation (even if they constitute a substantial proportion of the electorate when summed *across* districts). Finally, studies of party nationalization argue that similarities between districts falling in the same geographic region of a country will determine which and how many parties contest any individual district within that region (Caramani, 2000; Jones and Mainwaring, 2003; Morgenstern, Swindle and Castagnola, 2009). A party that caters to regional separatist interests, for instance, should only enter districts in the region in question; to this extent, knowing whether or not an individual district is a part of a distinguishable “region” of districts should shed some light on whether the separatist party will appear on the ballot.

All of this indicates that no district is an electoral island, perfectly isolated from other districts in the country. But what specific role might variation in diversity across districts play in determining the effective number of parties in any one district? Imagine a hypothetical scenario of two electoral districts, A and B. In the first case, assume that the populations of both District A and District B are comprised of two groups and these groups might best be described as mainly urban-dwelling Catholics and mainly rural-dwelling Protestants. Suppose that a political party is laboring in advance of an election to construct a battery

of policies that it believes will be attractive to the urban-dwelling Catholic population in District A. Having built this platform, no doubt this party would *also* garner relatively commensurate levels of support in District B, given that its two groups are characterized in the same way. Now, as a second case, consider a similar population in District A, but split the Catholic population in District B evenly between urban-dwellers and rural-dwellers. The platform the party built to cater to interests in District A will fail to net commensurate support in District B. If the party has the resources, time, and energy to rework its platform, then it might enter both districts and achieve similar levels of support; if not, it will either abstain from entering District B or enter District B with a less-successful platform. Either way, under this latter scenario, the effective number of parties in District B will decrease by virtue of its relationship to District A.

Generalizing from this example, I argue that when the populations of different districts resemble one another – i.e., when cross-district diversity is low – then the marginal cost to a political party of fielding a candidate or a list of candidates in an additional district is comparatively small. Having worked out the campaign infrastructure, advertising strategy, policy prescriptions, and stump speech in *this* district, the same election apparatus is easily implemented in *some other similar* district. The critical idea here is that while similarly-comprised constituencies respond in the same ways to the same party platforms and messages, differently-comprised constituencies will require slightly different political messages. This need to specifically tailor campaign platforms across districts will drive up the marginal costs for a party in fielding a candidate in another dissimilar district in the country. Even if the party should choose to field a candidate *without* putting forth the extra work to refine its platform, this lack of effort will be met with smaller vote shares. In either case, the effective number of parties will be lower (whether by lack of entry or less successful entry, given the mismatch of platform to population) when districts are dissimilar.

A long line of electoral scholarship in both American and cross-national contexts has argued in support of the idea that different constituencies respond differently to the same

party platform. To begin, we have substantial evidence that a voter's partisan affiliation differentially predisposes her to accept information in campaign advertisements that challenge her ideology (Iyengar and Simon, 2000; Zaller, 1992, 1996). This is especially true with voters possessing a lower level of interest in the election (Ansolabehere and Iyengar, 1995). Additionally, Sides and Karch (2008) find that under certain circumstances, "issue publics" such as senior citizens, veterans, and parents respond differently to different content in political advertising and Abrajano (2005) finds that lower education Latino voters focus on non-policy, personalistic cues whereas higher education Latino voters do not. Furthermore, in the American context, it has been shown that the tone and content of negative ads are much more important in determining turnout among independent voters than among partisan affiliated voters (Kahn and Kenney, 1999) and in an experimental study in Brazil, Desposato (2007) concludes that the impact of negative attack ads dampen turnout in lower social classes, but not higher social classes. Finally, in a cross-national study of voters' abilities to "learn" about party platforms over the course of a campaign, Arceneaux (2006) finds that learning effects are more pronounced among individuals with lower levels of political sophistication.

The the extent, then, that electoral districts are comprised of varying numbers of independent voters, veterans, parents, young people, poorer people, better educated people, politically sophisticated people, and so on, parties must invest in tailoring their platforms, advertising strategies – and, really, the entire approach of their campaigns – to specific districts. In an effort to win the support of different types of people (thereby increasing their overall vote totals), parties will have to tailor different versions of their platforms. If not, different-looking constituencies will respond differently to the same (un-tailored) platform. As districts become increasingly dissimilar to one another, fielding candidates or candidate lists across numerous districts becomes increasingly burdensome. By contrast, when districts are similar in their composition, then the same platforms and communication mechanisms should work across districts. In the aggregate, then, countries with higher levels

of cross-district diversity might experience *systemically lower* district-level ENP figures because parties, in general, face higher costs of successful competition. This reasoning calls for a qualification of the traditional and more straightforward “interaction” hypothesis that focuses only on within-district diversity. Specifically, I argue that higher levels of cross-district diversity should actually dampen this interactive effect by virtue of making it harder for parties to link their platforms across districts.

H4: Increasing a district’s diversity will only result in an increase in the effective number of parties if its magnitude is sufficiently large and the level of cross-district diversity is sufficiently low.

Drawing the distinction between the two levels of diversity within a country allows us to consider the full range of nuanced electoral dynamics resulting from the composition of electoral constituencies. Consider two countries that have similar average levels of *within-district* diversity and similar electoral rules, but differ in their levels of *cross-district* diversity. The theoretical expectation is that, even though the two countries have similar district-level environments, the effective number of parties at the district level may still be systematically lower in one country than in the other. This could be the case for one (or both) of two reasons. First, because at least some parties in a country with comparatively high levels of cross-district diversity will be unwilling to tailor their platforms and, thus, will opt not to contest more districts. In this case, voters are presented with systematically fewer ballot options to which they might lend their support. Second, at least some parties in a country with high cross-district diversity will be unwilling to tailor their platforms *but* will still enter districts broadly. In this case, voters will be confronted with a larger number of ballot offerings, but fewer of these offerings will espouse policies that are relevant in their constituencies. To test these implications of *H4*, I will use a hierarchical regression model to estimate a triple-interaction between district magnitude and both types of diversity. But first, I next describe in detail the method I employ to operationalize both within-district and cross-district diversity and I also demonstrate that these are distinct empirical phenomena.

Concepts and Measurements

As Stoll (2008) points out, there are a number of important questions that must be addressed when thinking about an appropriate operationalization of latent diversity:

- Should we study one cleavage or many cleavages?
- On which cleavage(s) should we focus?
- Do we focus on “how many” groups or on “how much difference” between groups?
- Should we measure diversity in the aggregate or in individual districts?

Several other scholars have raised similar concerns about the ways in which latent diversity has been operationalized. In order to address the first question above, some scholars have included multiple separate cleavages in their analysis (Annett, 2001; Powell, 1982) or have attempted to explore the ways in which separate cleavages overlap one another (Selway, 2011). Focusing instead on answering the third question, other scholars have focused on the extent of polarization between groups along a single demographic dimension (Posner, 2004) as well as the relative sizes of these competing groups (Fearon, 2003). Recent research on diversity issues unrelated to electoral politics has discussed a number of more complicated entropic measures of diversity that attempt to capture both the number of “types” and the distribution of individuals in a population across those “types” (Page, 2007, 2011). Finally, a few scholars of electoral politics have paid close attention to answering the fourth question, with Jones (1997, 2004), for example, attempting to disaggregate diversity below the level of the country at large in order to show variation across units of analysis. Elsewhere, Crisp, Olivella and Potter (2013) argue that measuring the variance in diversity across districts – rather than simply the diversity of each district individually or the diversity of the country as a whole – can lead to better tests of many important electoral dynamics. They advance a measure of intercoder reliability predicated on Krippendorff’s α (Krippendorff, 2004, 2011) that evaluates the extent to which each district similarly “codes” a set of constituency-level demographic attributes.

What is really needed is a measurement tool that can measure diversity both within districts *and* across them. Furthermore, the method should make no *a priori* conjectures either about the number of cleavages that characterize the level of diversity or about which cleavage is most salient. Finally, related to the third question, an ideal measurement tool would not only sort individuals into distinct groups, but also take stock of the relative sizes of each of these groups and their internal cohesion (which are concerns raised by both Page (2011) and Selway (2011)). The “most correct” method, then, would be some sort of modeling procedure that examines a group of individuals’ characteristics across a variety of demographic traits, iteratively sorts these individuals into various constellations of groups, and then selects which sorting is the best fit to the underlying patterns in the data. Below, I will argue that a *probabilistic topic model* is one such method capable of generating probability distributions over several individuals’ group membership based on their demographic traits.¹ I first discuss the method as it is applied to within-district latent diversity before illustrating

¹ One might argue instead that clustering algorithms are a more logical starting point for the task of iteratively sorting individuals into groups. Indeed, clustering algorithms have been increasingly employed by scholars who work with questions of classification and taxonomy (Gordon, 1999; Hand, 1981; Huang, 1997; MacQueen, 1967; Venables and Ripley, 2002). Despite the many different algorithmic specifications this literature has developed, however, none of these are actually fit for the research question at hand. First, very few clustering algorithms are appropriate for categorical data (Huang, 1997) and the demographic variables under consideration are all categorical in nature. Second, many clustering algorithms either require the researcher to specify the number of clusters (or groups) before estimation (thereby demanding *a priori* the very thing we are interested in recovering from the method) or generate a vague partition of the data set (as in the case of hierarchical clustering) that does not lend itself readily to interpretation. Additionally, even if we were to run many iterations of a clustering algorithm with different pre-specified numbers of clusters, it is very difficult to assess which of the resulting outcomes is a uniquely “best” fit to the data (Venables and Ripley, 2002). Scholars such as Ray and Turi (1999) have discussed possible selection criteria based on model fit and the minimization of error, but error can always be made smaller by piling on more clusters and examining the marginal reduction in error of moving from, say, 2 clusters to 3 clusters is fraught with ambiguity and subject to the researcher’s arbitrary selection of a marginal threshold.

how it might be used to measure cross-district latent diversity.

Probabilistic topic modeling – in particular, Latent Dirichlet Allocation (LDA) – is a form of Bayesian hierarchical generative models developed by scholars of machine learning in the 1990s and continues to see broad application in a variety of research fields (Blei, Ng and Jordan, 2003; Griffiths and Steyvers, 2004; Hofmann, 1999). The methodology was designed as an iterative classification algorithm that examines the types and frequencies of words in a collection of documents and then returns a set of conceptual “topics” to which the documents belonged. As with other generative models, the aim of LDA modeling is to recover the set of latent variables that could have plausibly generated the pattern of observed data (Steyvers and Griffiths, 2007). In most applications, the “observed data are the words of each document” and the latent structure is “the topics themselves and how each document exhibits them” (Blei and Lafferty, 2009, p. 73). The LDA approach can work with highly dimensional, categorical data – such as that employed in survey research – and makes very few *a priori* assumptions about how words and their frequencies should structure the final description of the latent space. In fact, “because the number of topics is in general not known, models with several different numbers of topics are fitted and the optimal number is determined in a data-driven way” (Grün and Hornik, 2011, p. 7).

How is this done? As opposed to the results of other iterative algorithmic procedures (such as *k*-modes or hierarchical clustering) the explanatory power and model fit of LDA outputs can be evaluated with ready-made goodness-of-fit statistics like AIC or BIC. This is a major advantage over other procedures, because it allows the researcher to evaluate whether three or four or five, etc. topic groupings best describe the pattern of responses. The field of machine learning has developed a goodness-of-fit statistic that is uniquely tailored to the LDA process called *perplexity* (Blei, Ng and Jordan, 2003; Grün and Hornik, 2011; Steyvers and Griffiths, 2007). A technical description of this statistic can be found in Blei, Ng and Jordan (2003), but from a conceptual standpoint, the *perplexity* measure can be described as the “inverse of the geometric mean per-word likelihood” and gives an estimate of the

model’s generalized performance (Blei, Ng and Jordan, 2003, p. 1010). Lower perplexity scores indicate that the LDA’s selection of the number of topics is a better fit to the latent structure of the observed data.²

Newman et al. (2009) argue that LDA topic models are “among the most successful recent algorithms for analyzing discrete data” (p. 1802) and Blei and Lafferty (2009) extol the LDA approach’s flexibility and generalizability, noting its application in fields as diverse as image recognition, population genetics, and social networking. LDA topic models have even successfully been applied to survey data, which is particularly fitting given my approach (Erosheva, Fienberg and Joutard, 2007). For demographic survey data, the “topics” of LDA are conceptually repurposed as the number of “groups” we recover. Depending on the level of analysis, either the respondents (in the case of within-district diversity) or the electoral district (in the case of cross-district diversity) constitute the “documents.” Each respondent’s demographic attributes (or each district’s distribution of these attributes across respondents) constitute the “words” that, taken together, comprise the groups/topics.

Before demonstrating with a few examples how the LDA procedure classifies individual survey respondents, I should recapitulate for clarity. Table 1 breaks down how each of the LDA components can be applied as conceptual analogues in electoral politics. Within each district, individual survey respondents are represented as “documents.” Within each country, each district constitutes its own “document.” When measuring within-district latent diversity, each individual’s pattern of responses to several demographic questions stands in as “words.” Similarly, when measuring cross-district latent diversity, the responses from each individual in a district are pooled and the *distribution of their pooled responses* serves as a larger pot of “words.” The LDA modeling procedure is iteratively run n separate times within each district, where n is the number of respondents. Each of these n iterations is compared against one another and – using the *perplexity* goodness-of-fit criteria – the

² The LDA procedure and perplexity goodness-of-fit statistics are easily calculated with **R** by using the `topicmodels` package designed by Grün and Hornik (2011).

Table 1. LDA Topic Modeling Analogies

| | Electoral Concept | LDA Concept | Example |
|------------------------|----------------------------|---------------------------|--------------------------------|
| Within-District | Survey Respondent | documents | “John Doe” |
| | Demographic Attributes | words | “language:english” |
| | Group | topics | “high-income english-speakers” |
| Cross-District | Electoral District | documents | “New York 5th Congressional” |
| | Distribution of Attributes | words | “70% language:english” |
| | Group | topics | “mostly english-speakers” |
| Both Levels | <i>ENG</i> | weighted number of topics | 3.5, 1.3, etc. |

uniquely best-fitting number of topics (or groups) is selected as that district’s latent *number of groups*. The group membership of each individual respondent is then recovered so that each group can be weighted by its size. This weighted statistic is the *effective number of groups* or *ENG* within the district and constitutes one of my two main explanatory variables. The *ENG* at the national level (i.e. cross-district latent diversity) is measured analogously.³

Examples are telling. Consider two attributes, religion and the urban-rural divide, and two possible values within each attribute, { Protestant, Catholic } and { Urban, Rural }. When we are interested in measuring within-district *ENG*, we “unfold” all permutations of these attribute-values and examine the frequency of their appearance across survey respondents. Each of these permutations – for example, { religion:protestant, divide:urban } – is a possible “word” that could be exhibited by a survey respondent (or “document”). Not all possible permutations must exist in a district; indeed, if there were no Catholics in this district,

³ The literature on topic models and LDA modeling, in particular, is well-developed. My discussion here is simply intended to conceptually review the procedure and explain how it can be applied in studying diversity. Readers who are interested in the mathematical specifics of topic models should refer to Blei, Ng and Jordan (2003), Griffiths and Steyvers (2004), and Newman et al. (2009) for relatively accessible reviews.

then the LDA construction of topics would come down to the relative balance of urban Protestants versus rural Protestants. If we were to measure three values in the religion attribute (by adding, say, Islamic), then more word permutations would be possible and this more complicated demographic space can be easily handled by the flexibility inherent in the LDA approach. In such complicated spaces, we might, for example, recover a large group (or “topic”) represented by { religion:islamic, divide:urban, income:high, language:english } alongside a much smaller group represented by { religion:catholic, divide:rural, income:high, language:english }. The important point to be made is that the LDA approach selects the defining characteristics of each group (and, subsequently, the number of groups) in an effort to reduce the amount of error in classification as measured by the *perplexity* metric. The data, however complex, speaks for itself insofar as certain demographic dimensions are given more importance over others based on the ways in which all dimensions overlap one another.

As a second example, when it comes to measuring cross-district *ENG*, the process is virtually identical except that now the “document” becomes the electoral district and – instead of a survey respondent completely exhibiting { religion:protestant, divide:urban } – respondents are distributed across possible permutations, meaning that a district might have 70% of its respondents in the { religion:protestant, divide:urban } permutation. In the cross-district LDA approach, then, districts with similar distributions across word permutations will ultimately be lumped into the same group. If all districts’ distributions of survey respondents resemble one another, then the *ENG* figure will be quite low (around 1.0). If, however, about half of the districts have mostly urban inhabitants and the remainder have mostly rural inhabitants, then cross-district *ENG* will increase (around 2.0). If the addition of more demographic attributes overlap in such a way that they partition the districts even further (such as 25% of districts being mostly urban and Protestant, 25% of districts being mostly Catholic and urban, etc.), then *ENG* will increase even more (to around 4.0).

Along these lines, a final important conceptual point is that the LDA approach is able to sift through potentially highly dimensional spaces and – if warranted – return an *ENG*

figure that is comparatively low. After all, just because we have information along the same number of demographic attributes in Switzerland and Japan, this fact alone should not return the same effective number of demographic groups. Rather, we are interested in how all of these attributes overlap, reinforce, or fragment one another. This is a process that a researcher is decidedly *unable* to manually engage in, especially in a cross-national analysis involving several demographic attributes that can assume dozens of different values. The data employed in this study, for example, includes six separate attributes, each of which can assume anywhere from four to two-dozen values. As will be discussed below, within-district *ENG* figures range from 1.0 to 9.3 across the various countries under consideration. This level of nuance – though vitally critical for assessing the effect of diversity on the effective number of parties – could not previously be attained by extant methodologies.

Ultimately, conceiving of latent diversity as a weighted measure of the number of distinct groups within a constituency (or across constituencies) has an important advantage over measuring diversity as, say, a continuous scale of fractionalization. Specifically, it allows for a clearer theoretical linkage to the outcome variable: the effective number of parties. While “more diversity” has always been argued to result in higher *ENP*, the theoretical story driving this relationship is that more diversity allows for the possibility of more differentiable policy positions. But how much more fractionalization on a continuous scale, theoretically speaking, would we need in order to expect the addition of another party offering? The amount is unclear. The relationship between *ENG* and *ENP*, however, is more straightforward. For sufficiently permissive institutional contexts, an *ENG* of 2.5 should support an *ENP* of about 2.5, an *ENG* of 4.0 should result in an *ENP* of about 4.0, and so on. The theoretical mechanism driving this relationship is drawn into much clearer focus when we think about voters’ party support and voters’ demographic characteristics in the same way: if a demographic constituency is sufficiently sizable and the electoral institutions allow for

it, we should see the emergence of a party that will cater to the needs of that demographic.⁴

Data

To calculate both within-district and cross-district latent diversity, I required district-level data about the demographic composition of the population along six dimensions that have been previously argued to structure political competition (Stoll, 2008). These dimensions are ethnicity, language, religion, income, the rural-urban divide, and support for the democratic regime.⁵ I took individual survey responses – situated in individual districts – from the widely-utilized *Comparative Study of Electoral Systems* survey database. The CSES database asks respondents about their demographic attributes and vote preferences, and also includes identifiers for the specific electoral district in which the respondent resides. Although the CSES does not sample randomly at the district level, it is wholly unique in the level of district-level demographic detail it offers to researchers. Indeed, Stoll (2008) concludes from her review of electoral scholarship on measuring diversity that working with CSES data is

⁴This assumes, of course, that there are political entrepreneurs who are willing to activate the combination of social divisions that would allow them to carve out this electoral niche. While the topic modeling approach has many benefits, its one shortcoming is an inability to specify *which* of these effective groups are the most politically salient. Rather, it returns the group structure as a nuanced mapping of all *possibly salient* divisions that could potentially be mobilized by political elites. Measuring saliency is a complicated question that falls outside the scope of the present analysis, but I will return to the debate between the “sociology of politics” (i.e., social divisions exogenously determined) versus “political sociology” (i.e., social divisions emerge from political competition) in the conclusion.

⁵ Other district-level characteristics no doubt affect party competition and political representation more generally. Things like district-level unemployment, for instance, or the proportion of the population over the age of 65 will, of course, affect how many (and which types of) parties populate the ballot on election day. However, I choose to focus for the time being on the six attributes listed above both because previous studies of electoral politics have focused on these (Caramani, 2004; Lijphart, 1999; Jones, 2004; Ordeshook and Shvetsova, 1994; Powell, 1982; Rose and Urwin, 1969) and because data for these six attributes was more widely available in a comparative context.

the most promising way forward in constructing district-level measures of diversity. It has been previously demonstrated by Crisp, Olivella and Potter (2013) that the CSES can be used as a “best available” approach to examining within- and cross-district demographics.

Missing values were imputed on a country-by-country basis using Bayesian multiple imputation. This approach ensured that in countries where a specific question was not asked (such as a question on language in the American survey), responses were not being imputed based on response patterns in other countries (where the relationship between variables might be inherently different and, thus, uninformative in the country under consideration). So, for example, if four of the six demographic questions were asked of respondents in a given country, there would be *some* missing values in the four (asked) questions and *complete* missing values in the two (unasked) questions. Values were only imputed for the four asked questions, however, and the two unasked questions were left completely missing as there was no information from which to impute those values. Additionally, values were imputed on a country-by-country basis so that, for example, the underlying (latent) relationship between income and language in Switzerland was not informing the imputation of values along these two variables in, say, Croatia (where the underlying latent relationship might be different).

Appendix A provides more detail on which attributes were included by the CSES in which countries and also summarizes the average number of survey respondents that appeared in each electoral district. Not surprisingly, this number varies widely depending on the country. In an effort to increase the number of respondents within each electoral district – thereby feeding into the topic model calculations a larger number of observations – I pooled survey responses across waves of the CSES in those cases where a country had been covered by the CSES in multiple elections. Pooling survey respondents in this way has several distinct advantages, especially in countries employing single-member districts. To begin, the number of respondents is much lower in smaller-magnitude districts and if one leaves each individual CSES wave intact (rather than pooling) the end result is that several districts have very

few respondents.⁶ This low number leaves little room for the topic model to gain leverage on the latent amount of diversity in the district. Furthermore, anecdotal evidence would suggest that district-level demographics will not change dramatically in any given 8- or 10-year period. For the data employed in this study, the modal response category in the same district across different waves of the CSES exhibited significantly static characteristics: the modal “ethnic” response category remained constant 78% of the time, for instance, while the modal “language” category remained constant in 79% of cases. What this means is that, rather than observing shifting demographic distributions between iterations of the election surveys, what we have instead are simply different election-year snapshots of a more-or-less static underlying distribution of characteristics. It makes the most conceptual sense, in this respect, to pool observations across survey waves in an effort to provide the most complete picture (and the most statistical leverage for the topic modeling procedure).

A final issue with the use of the CSES data set is that, for countries with a very large number of electoral districts (like the United Kingdom), not all districts are sampled from in each iteration of the survey. The coverage of districts improves when separate waves are pooled, because while many districts are sampled multiple times, some districts only appear in one wave. Still, there is a systematic (but, perhaps, not substantial) under-sampling of electoral districts in those countries that have hundreds of single-member constituencies. In terms of examining the effect of within-district diversity on party system size, this sampling bias should not result in statistical bias: there are still several hundred single-member districts (in both plurality systems and some multimember systems) included in the analysis and these SMDs exhibit a wide range of within-district *ENG* values. A potentially larger problem emerges, however, when we consider the impact this under-sampling might have

⁶ As depicted in the appendix, pooling brings the average respondents per district in countries like Canada, the United Kingdom, and the United States to 9.8, 9.7, and 9.6, respectively. This is a large improvement over leaving the waves unpooled, which results in average response rates in these countries around 3 or 4. In most cases, however, the number of respondents in each district is much higher. The mean across all plurality system countries is 23 and the mean of the entire sample of countries is 43.

on garnering a “complete” picture of a country’s cross-district diversity: indeed, without sampling from all districts, how can we have faith in a national-level aggregation of diversity across districts? A solid rejoinder to this concern can be found in the fact that, while the CSES does not sample from all districts, it appears to sample from all areas – or regions – of a country. For example, in the United Kingdom, the CSES captures districts from Wales, Scotland, the length and breadth of England, and Northern Ireland. In the United States, there are Congressional districts samples from the Northeast, Midwest, South, West Coast and Pacific Northwest. The most substantial source of bias in cross-district diversity would potentially occur if, for instance, the CSES had omitted all districts in Scotland and Wales from their surveys. In the absence of this, we can have some faith in the data’s ability to pick up on the general level of cross-district heterogeneity: it is not clear that omitting one Scottish district and including another similar district (Scotland being a very internally homogeneous region) would unduly bias our national-level understanding of diversity.

The two main explanatory variables of interest, as discussed above, are the *within-district* and *cross-district* effective number of groups ENG , which are statistical approximations of latent diversity at both the district and national level. Because of pooling observations across waves of the CSES, I have one measure of within-district ENG per district and one measure of cross-district ENG per country. Within-district diversity ranges from 1.0 to 9.3 with a mean of 2.6 while cross-district diversity ranges from 1.5 to 11.3 with a mean of 4.6. For each country, if we compare the average within-district diversity against national-level cross-district diversity, it is immediately obvious that the two measurements are tracking distinct phenomena (the correlation coefficient between the two is $r = -0.18$). Figure 1 below provides an “eagle-eye” overview of how the two metrics relate to one another. Average levels of within-district diversity are plotted along the x -axis while levels of cross-district diversity are plotted along the y -axis. Although the graphic obscures some important variation in diversity across individual districts, it clearly indicates that the two statistics are not tracking the same concepts.

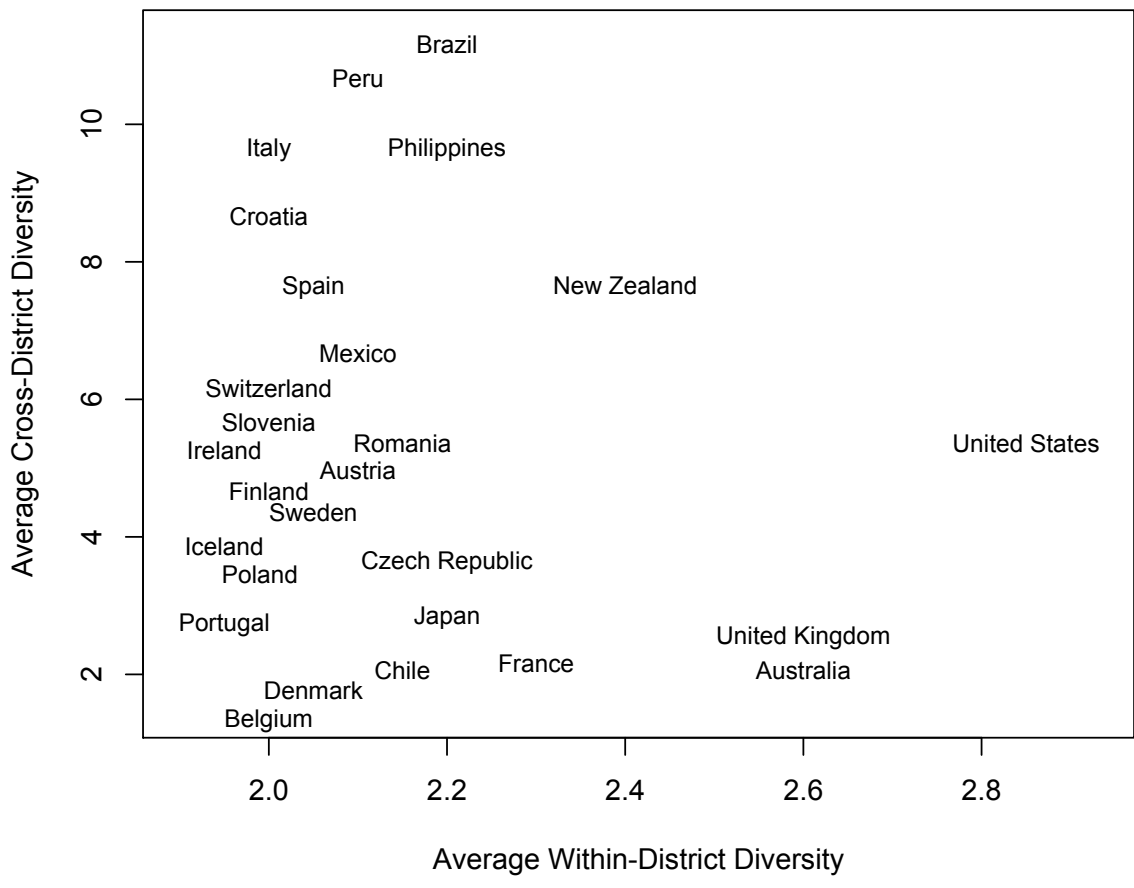


Figure 1. An Eagle-Eye View Relationship of Diversity. Plot of cross-district diversity (averaged across elections) Against within-district diversity (averaged across elections and electoral districts)

Furthermore, on their face, the statistics seem to be returning plausible estimates. Cross-district diversity is significantly greater in, say, Brazil (11.3) than in Australia (2.0) and is also much greater in the Philippines (9.7) than in Iceland (3.6). Within countries, as well, the topics model returns plausible values. In Switzerland and Spain – two infamously regionalized countries – average within-district *ENG* is 2.0 and 2.2, respectively, while national-level cross-district *ENG* is 6.1 and 7.1, respectively. This indicates the presence of internally-homogeneous regions that are also quite distinct from one another. In countries with more homogenous populations and no notable regional differences, these two figures are much closer in line with one another (as is the case in Poland, where average district-level diversity is 1.9 and cross-district diversity is 3.2).⁷

The outcome variable of interest is the district-level *effective number of electoral parties* (*ENEP*) or the number of parties weighted by their vote share. This operationalization of the number of parties has become standard in studies of this type (Singer and Stephenson, 2009; Stoll, 2008). The electoral data underlying the analysis come from a number of sources including: Brancati (2007), Kollman et al. (2011), the European Elections Database, Adam Carr’s online election archive, and various electoral commissions around the world. Every effort was made to collect *complete* votes data, meaning that smaller parties’ individual vote tallies were left disaggregated rather than pooled into an “other votes” category that could potentially obscure some important variation in the *ENEP* calculation. All told, in the data

⁷ Looking to other external sources of validity for the topics model’s results is a difficult endeavor, mainly because the approach differs widely from extant measures of fractionalization (Fearon, 2003), cross-cuttingness (Selway, 2011), and measures that take into consideration a broader range of demographic attributes, yet are cast solely at the national level. Country-specific tests could be applied, but would be inherently ad hoc in nature. For example, in the case of the United States, there is evidence that more diverse congressional districts (such as the Illinois 9th and the Missouri 3rd, which cover substantial areas of downtown Chicago and St. Louis) have considerably higher *ENG* values than districts in more homogenous areas of the country such as Utah, Oklahoma, and Colorado. But this type of detailed validity test in a cross-national study would far surpass the scope of this paper.

set there are 2,467 districts in 64 elections across 29 countries.

The size of the data set may seem comparatively small for a cross-national, district-level study of electoral politics. However, there were several limiting factors on inclusion in the study: (1) the country needed to be included in the CSES, (2) the CSES needed to have recorded in which electoral district the survey respondent resided (which did not always happen), and (3) the district names needed to match up with some external source of official electoral data.⁸ Additionally, with the intent of giving the topic model enough leverage to correctly recover the number of groups, in those districts where fewer than 10 survey responses was reported in a district, I dropped that district from the analysis.⁹ While other recent studies of the determinants of *ENEP* have omitted mixed-member systems, the CSES included survey responses from the SMD tier of a handful of mixed-member systems and I include these observations in the results reported below. Typically mixed-member systems are omitted due to concerns about “contamination” between tiers, or the idea that outcomes in the SMD tier will be biased due to the presence of an additional MMD tier. However, as recent research has demonstrated, this concern may be overstated, especially in more developed democracies (Crisp, Potter and Lee, 2012; Karp, 2009; Maeda, 2008; Moser and Scheiner, 2004).

Another primary explanatory variable – *logged district magnitude* – is measured at the district level (rather than as a national-level average) and is taken from the same sources of electoral data described above. I control for a number of variables that have been hypothesized to impact the district-level number of parties. At the national level, for instance,

⁸ The CSES includes some district-level electoral results, but only for a small subset of the parties that contested the election. In the interest of calculating the most accurate *ENEP* figures possible, I turned to more complete sources of electoral data and then merged these together with the CSES results. In some cases, the CSES database included district names that could not be matched to external data sources. Rather than biasing the results with rough *ENEP* approximations, these countries were dropped from the analysis.

⁹ The cut point of 10 is admittedly arbitrary. However, in Appendix B, I take up this issue in greater detail and demonstrate that my findings remain intact when I adopt different cut points of 15, 20, and 25.

presidentialism has been hypothesized to drive down *ENEP* systematically in all districts as parties strain to link their platforms across districts to increase their national profile in an effort to gain access to highly valuable national-level offices (Cox, 1999; Greene, 2007; Shugart and Carey, 1992). Similarly, the extent political concentration (or lack of *federal* power structures) is thought to drive down the number of parties, as devolving power to regions incentives more parties to contest limited numbers of districts (Brancati, 2008; Chhibber and Kollman, 2004; Samuels, 2000). Finally, a long line of scholars argues that as *democracies age*, their citizens and political elites become more familiar with electoral institutions and are able to coordinate more effectively (Duch and Palmer, 2002; Moser and Scheiner, 2009; Tavits, 2007; Tavits and Annus, 2006; Zielinski, 2002). As democracies age, then, we would expect that *ENEP* decreases. Values for these control variables are taken from Treisman (2007) and I use a running tally of the number of years a country has been democratic as my measure of “democratic age.”¹⁰

Analysis and Discussion

Given the hierarchical nature of the data structure – electoral districts which are nested inside countries – as well as the continuous nature of the outcome variable, a multilevel linear model is the appropriate modeling strategy. Due to the decision to pool survey respondents across waves of the CSES in order that the LDA model might have the greatest leverage in recovering latent demographic distributions, I have one value of within-district *ENG* per district and one value of cross-district *ENG* per country. Rather than trying to model election-specific *ENEP* figures (which vary within the same district across elections) with pooled (non-varying) institutional and social variables, I instead average *ENEP* within each

¹⁰ The results presented below are effectively the same when I substitute in different measures of democratic age, such as an operationalization of “new democracy” from Singer and Stephenson (2009) that takes a value of 1 if the country did not hold democratic elections prior to 1989.

district across all elections for which I have CSES survey data in that district.¹¹ In this way, the model can be specified as follows:

$$\begin{aligned} ENEP_{cd} &\sim N(\gamma_c + \mathbf{X}_{cd}\beta, \sigma_{cd}^2) \\ \gamma_c &\sim N(\mathbf{X}_c\xi, \sigma_c^2) \end{aligned}$$

where $ENEP_{cd}$ is the outcome variable of interest: the average effective number of parties (measured in votes) in a particular country’s district. The country-level random intercepts, γ_c , are modeled by a country-level matrix of predictors that includes variables such as presidentialism, federalism, and cross-district ENG . The β matrix is a battery of district-specific explanatory covariates such as within-district ENG and district magnitude. In this way, β and ξ are vectors of coefficients to be estimated and σ_{cd}^2 and σ_c^2 are measures of variance across each level of the model.¹²

The first model presented in Table 2 below constitutes a test of $H3$ – or the classic “interaction hypothesis” – which postulates that the effect of diversity on party system size is qualified by the permissiveness of the electoral rules.¹³ This first model offers some

¹¹ The results presented below hold well, however, when we think about the outcome variable slightly differently: if we leave intact each election’s separate $ENEP$ figure – as I do in Appendix C – the results do not change. This formulation has the conceptual benefit of allowing voters and parties to meet repeatedly in a fixed social-institutional environment, but it also has the empirical drawback of modeling an outcome variable that varies time-serially with explanatory variables that do not.

¹²The models were estimated in R using the `lmer` command, which is a part of the broader `arm` package written by Andrew Gelman and his collaborators.

¹³ A supplementary model testing the “additive hypotheses” $H1$ and $H2$ – which leave intact the separate effects of diversity and district magnitude on the effective number of parties – is not included here because, as argued above, this is not the correct way to formulate the relationship between diversity, magnitude, and party system size. Support for $H1$ and $H2$ has consistently been unearthed by previous studies and my supplementary model also indicates that increasing one or the other will drive up $ENEP$. I focus in the discussion of the results on the “interaction” model, however, as being the most appropriate.

disappointing – yet not surprising – results: when we correctly measure latent diversity at the district level, then the classic interaction hypothesis does not perform well. Rather, we see that district magnitude exerts a strong substantive influence on party system size while diversity’s impact is much small and statistically insignificant. Their interaction falls far short of either substantive or statistical significance.

This result is not unexpected because, as I have argued, thinking about diversity in any individual district without also considering the district in a broader context should not yield a full picture of the dynamics at play. The second model in Table 2 is a first illustration of why this might be the case. In interacting within-district diversity with cross-district diversity, we retrieve a statistically significant coefficient that indicates the marginal effect of within-district *ENG* on *ENEP* is not always going to be positive; rather, it seems that cross-district *ENG* is going to play an important modifying role. Specifically, the marginal effect of increasing within-district diversity will only be positive when the effective number of cross-district groups is less than about 4.5 ($\partial ENEP / \partial withinENG = 0.27 - 0.06 crossENG$). In the third and fourth models, I subset the data by plurality countries (of which there are 10) and multimember countries (19). The substantive effect of the relationship remains roughly the same (the marginal effect of within-district diversity is positive so long as the effective number of cross-district groups remains below 4.8 in plurality systems and below 4.5 in multimember systems), although within-district diversity is significant in plurality systems (and not multimember systems) whereas cross-district diversity is significant in multimember systems (but not in plurality systems).

A final point regarding Table 2 is that the second model fits the data more poorly than the first model (as evidenced by the AIC and log likelihood ratio test statistics). Dropping magnitude from the analysis clearly results in a poorer model. Indeed, appropriately modeling H_4 requires a more complicated triple-interaction model such as that depicted in Table 3. Recall that H_4 postulates that the extent of cross-district diversity will qualify the relationship between within-district diversity and district magnitude. Including this triple

Table 2. Multilevel Linear Regression Model of District-Level *ENEP*

| D.V. | Model 1 | Model 2 | SMD | MMD |
|---|--------------------------|---------------------------|---------------------------|---------------------------|
| Log(Magnitude) | 0.51 [0.00, 1.02] | | | |
| Within-District <i>ENG</i> | 0.05 [-0.01, 0.11] | 0.27 [0.12, 0.41] | 0.24 [0.08, 0.40] | 0.49 [-0.10, 1.07] |
| Log(Magnitude) × Within <i>ENG</i> | -0.01 [-0.26, 0.24] | | | |
| Cross-District <i>ENG</i> | | 0.30 [0.10, 0.50] | 0.10 [-0.05, 0.25] | 0.41 [0.15, 0.68] |
| Within <i>ENG</i> × Cross <i>ENG</i> | | -0.06 [-0.10, -0.02] | -0.05 [-0.09, -0.01] | -0.11 [-0.22, -0.01] |
| Presidential | 0.23 [-0.64, 1.09] | -0.46 [-1.65, 0.73] | | |
| Federal | 0.14 [-0.60, 0.89] | -0.13 [-1.09, 0.82] | | |
| Democratic Age | 0.01 [-0.01, 0.02] | 0.01 [-0.01, 0.02] | | |
| Intercept | 2.77 [1.90, 3.63] | 2.38 [1.09, 3.66] | 2.28 [1.49, 3.08] | 2.53 [1.04, 4.02] |
| $\hat{\sigma}_{cd}^2$ | 0.67 | 0.67 | 0.72 | 0.56 |
| $\hat{\sigma}_c^2$ | 0.85 | 1.42 | 0.28 | 1.00 |
| AIC | 4018 | 4077 | 2822 | 1228 |
| LL Ratio Test Statistic | 7466 | 7406 | 10 | 8 |
| <i>N</i> countries | 29 | 29 | 10 | 19 |
| <i>N</i> observations | 1,600 | 1,600 | 1,100 | 506 |

Notes: 95% confidence intervals were calculated from 1,000 simulated draws of the model's parameters and appear in brackets below coefficient estimates.

Table 3. Multilevel Linear Regression Model of District-Level *ENEP*

| D.V. | Estimate | 95% C.I. |
|--|-----------------|------------------|
| Log(Magnitude) | -0.27 | [-1.45, 0.91] |
| Within-District <i>ENG</i> | 0.25 | [0.09, 0.40] |
| Cross-District <i>ENG</i> | 0.14 | [-0.02, 0.29] |
| Log(Magnitude) × Within <i>ENG</i> | 0.24 | [-0.34, 0.82] |
| Log(Magnitude) × Cross <i>ENG</i> | 0.13 | [-0.08, 0.34] |
| Within <i>ENG</i> × Cross <i>ENG</i> | -0.05 | [-0.08, -0.02] |
| Log(Magnitude) × Within <i>ENG</i> × Cross <i>ENG</i> | -0.04 | [-0.14, 0.06] |
| Intercept | 2.61 | [1.80, 3.42] |
| $\hat{\sigma}_{cd}^2$ | 0.66 | |
| $\hat{\sigma}_c^2$ | 0.76 | |
| AIC | 4002 | |
| LL Ratio Test Statistic | 94 | |
| <i>N</i> countries | 29 | |
| <i>N</i> observations | 1,600 | |

Notes: Control variables were included in estimation, but are not reported here as their effects remain consistent as those presented in earlier models.

interaction term in the model (and dropping all of the insignificant control variables) results in the best-fitting of the four models (it returns the lowest AIC value). Interpreting triple-interaction models can be difficult and recovering the marginal effect of one explanatory variable on the outcome variable requires taking the partial derivative with respect to the variable in question (Brambor, Clark and Golder, 2005). In assessing $H4$, I am interested in the marginal effect of increasing within-district ENG on $ENEP$ at different levels of *both* district magnitude and cross-district ENG . This can be derived as follows:

$$\begin{aligned}
 ENEP = & \beta_0 + \beta_1 \log(mag) + \beta_2 \text{within}ENG + \beta_3 \text{cross}ENG \\
 & + \beta_4 \log(mag) * \text{within}ENG + \beta_5 \log(mag) * \text{cross}ENG \\
 & + \beta_6 \text{within}ENG * \text{cross}ENG + \beta_7 \log(mag) * \text{within}ENG * \text{cross}ENG
 \end{aligned}$$

$$\frac{\partial ENEP}{\partial \text{within}ENG} = \beta_2 + \beta_4 \log(mag) + \beta_6 \text{cross}ENG + \beta_7 \log(mag) * \text{cross}ENG$$

Holding logged district magnitude at its average for the full data set, the marginal effect of increasing within-district diversity is only positive when cross-district ENG is less than about 5.0. When cross-district ENG is 1.0, for example, increasing within-district ENG by one group drives $ENEP$ up by about 0.6 parties. However, when cross-district ENG is much higher (for example, at 10.0, which is nearing its observed maximum) increasing within-district ENG actually drives down $ENEP$ by about 0.5 parties. In fairly broad terms, then, these results seem to offer tentative support for $H4$. Holding magnitude at its full-sample average is not terribly informative, however, and is obscuring some important variation between plurality and proportional systems. In Figure 2 below, I portray the marginal effect of within-district ENG holding magnitude constant at 1 (in the top panel), 3 (in the middle), and 5 (in the bottom panel).

For SMD systems, increasing district-level diversity increases the effective number of parties, but only when cross-district diversity remains below roughly 4 effective groups. When

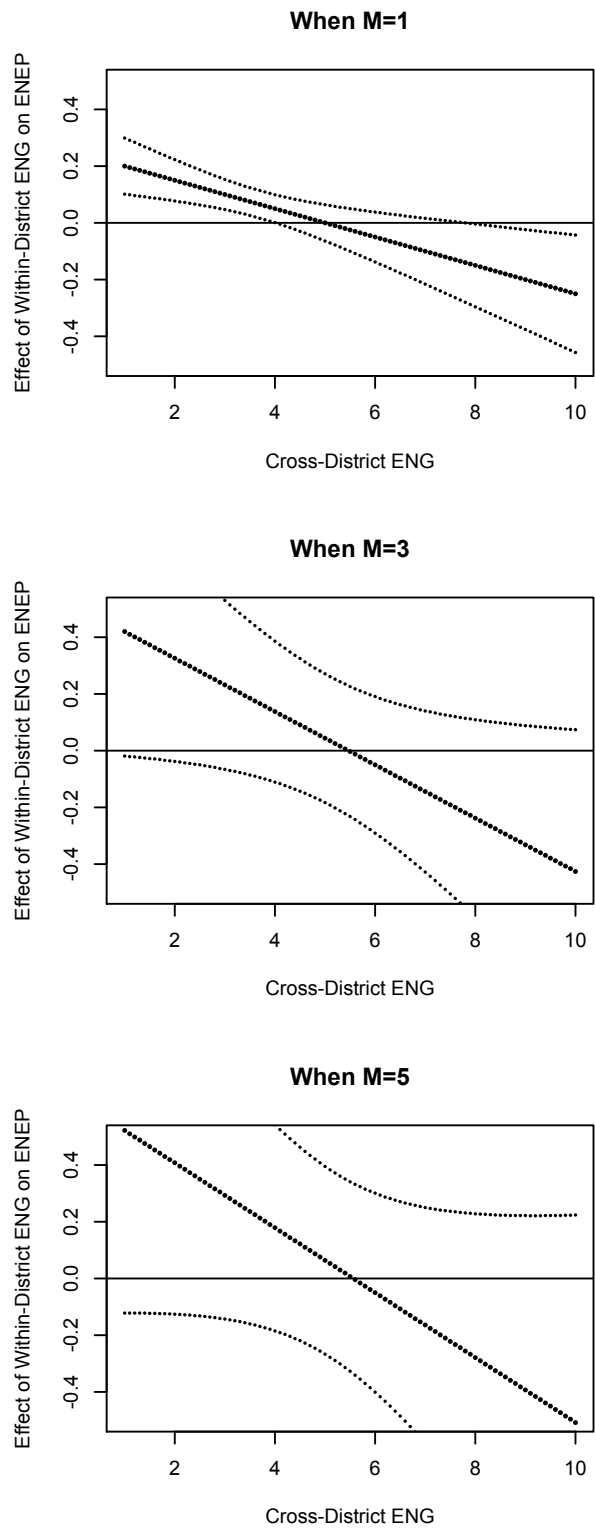


Figure 2. Marginal Effect of Increasing Within-District *ENG* on the Effective Number of Electoral Parties for Different Values of Cross-District *ENG* with 90% Confidence Bounds. The top pane holds district magnitude constant at 1, while the middle pane holds magnitude constant at 3, and the bottom pane at 5.

cross-district diversity increases (in particular, when it increases above 8) then the costs for parties of tailoring their platforms to radically different constituencies begins to drown out the electoral inducements of singling out diverse demographics in any one constituency. In other words, while Districts A and B might both be internally diverse enough to allow new party entrants into the electoral arena, if the two districts are radically different from one another, any potential new entrant will have a hard time entering both. This dynamic is going to systematically drive down *ENEP* figures across all districts in countries with higher levels of cross-district diversity. Although the data set does not apparently have a sufficient number of MMD observations to confirm that the same trend is taking place in countries relying on multimember districts, the case can be made that cross-district diversity’s impact on the conditional relationship between within-district diversity and magnitude appears to be operating in the same direction (yet might not be as highly consequential). The drop off in the marginal effect in these systems is more substantial, but statistically less certain.

Moving away from a discussion of marginal effects, I present Table 4, which more concretely discusses the model’s results by illustrating predicted values of *ENEP* at different levels of district magnitude and within- and cross-district diversity. Table 4 intuitively represents the various take-away points from the models testing each of the four hypotheses described above. The top set of figures correspond to a country with a low level of cross-district *ENG* (in this case, 2.0 or the lower end of the interquartile range) and the bottom set correspond to a high level (in this case, 6.5 or the upper end of the interquartile range). Each cell in the two tables indicates the predicted effective number of parties that correspond to a pairing of a value of within-district diversity and district magnitude. Drawn from 1000 simulations of the model’s parameters, 95% confidence intervals appear in brackets below each prediction.

In support of the idea that increasing district magnitude additively (i.e., by itself) drives up the effective number of parties, we see that increasing district magnitude always (and usually substantially) increases the effective number of parties (*H1*). Holding within-district

Table 4. Predicted *ENEP* Values Drawn from 1000 Simulations of the Model’s Parameters

| Cross-District <i>ENG</i> Low | Within-District <i>ENG</i> | | |
|----------------------------------|----------------------------|---------------------|---------------------|
| | 1 | 3 | 5 |
| Magnitude = 1 | 3.0 [2.5, 3.5] | 3.3 [2.8, 3.8] | 3.6 [3.0, 4.2] |
| Magnitude = 5 | 3.2 [2.5, 3.9] | 4.0 [3.2, 4.8] | 4.8 [3.0, 6.6] |
| Magnitude = 10 | 3.3 [2.3, 4.3] | 4.3 [3.3, 5.3] | 5.3 [2.5, 8.1] |

| Cross-District <i>ENG</i> High | Within-District <i>ENG</i> | | |
|-----------------------------------|----------------------------|---------------------|---------------------|
| | 1 | 3 | 5 |
| Magnitude = 1 | 3.4 [3.0, 3.8] | 3.2 [2.8, 3.6] | 3.0 [2.4, 3.6] |
| Magnitude = 5 | 4.2 [3.7, 4.7] | 4.0 [3.4, 4.6] | 3.7 [2.3, 5.1] |
| Magnitude = 10 | 4.6 [4.0, 5.2] | 4.3 [3.6, 5.0] | 4.1 [2.1, 6.1] |

Notes: “Low” and “High” cross-district diversity scenarios correspond to the lower and upper bounds of the interquartile range (2.0 and 6.5 effective number of groups). Country-level random intercepts were held at their means. 95% confidence bounds appear in brackets below each predicted value.

ENG at 3.0, for example, increasing magnitude from 1 to 10 in countries with low levels of cross-district diversity results in an increase in *ENEP* of 1.0 and an increase of 1.1 in countries with high levels of cross-district diversity. Furthermore, the 95% confidence bands on these predictions indicate that there is very little overlap in the uncertainty surrounding these predicted values. The evidence in support of the argument that increasing within-district diversity by itself drives up *ENEP* (*H2*) is very mixed: moving across any of the six rows in Table 4 (thereby increasing within-district diversity while leaving the other covariates constant) only increases *ENEP* in some cases and the uncertainty around these predictions is comparatively large. There is no evidence in direct support of *H3* or the “interaction hypothesis” and, as I have argued, this null finding can best be accounted for by including cross-district diversity in the analysis (postulated in *H4*). Here we see that within- and cross-district diversity work at cross purposes in SMD systems, but tend to reinforce one another in MMD systems. Whereas moving from a position of low within- and low cross-district diversity to a position of high-high in an SMD system will either reduce or leave constant the predicted *ENEP*, a similar move in a district of magnitude 5 will increase *ENEP* by 0.5 while doing so in a district of magnitude 10 will increase *ENEP* by 0.8.

Conclusion

This paper began by asking an age-old question: how do institutional and sociological factors combine at the district level to structure political competition? I noted that, although this question has been asked many times, scholars of electoral politics have never been entirely satisfied with the answers. For example, Jones (1997, 2004) argues that the district-level theoretical tenants of Duverger’s thinking have never been appropriately matched by empirical investigations cast at that level. Instead, studies have measured one or both of the explanatory variables and / or the outcome variable at the national level. Stoll (2008) convincingly demonstrates that scholars working on electoral politics need to devote more time to thinking about their operationalizations of “latent diversity” because the choice of

metric influences the conclusions that one draws from the analysis. Extant measures all suffer from one or more problems that bias results in different ways. Finally, the findings of scholars working in closely related topics like party system nationalization, partisan bias, and “contamination” effects all remind us that no electoral district is an island; rather, district-level dynamics need to be situated in broader, country-level dynamics that can have down-ticket implications for political competition in any given constituency.

I cannot argue that my manuscript is a definitive statement that will lay to rest lingering debates about each of these three points. Indeed, the analysis could be made more robust if there were more countries included and if there were more survey respondents located within each district. Both of these would result in greater statistical leverage that would perhaps bring the relationships I have discussed above into sharper focus. And taking an *a priori* agnostic approach to determining which demographic dimensions are “salient” when it comes to political competition – while being less laden with questionable assumptions than many methods that try to measure diversity – no doubt infuses my measurements of *ENG* with a bit of noise. While there may be 3.5 effective demographic groups, for example, when we consider survey responses on language, religion, and income, political parties competing in the district may only make resort to two of these three dimensions in the course of taking policy positions during campaigns. Additional work could study the nature of political competition within each district (by examining party platforms, campaign rhetoric, etc.) and then use this information to select a subset of demographic characteristics to feed into the LDA modeling procedure.

Despite these shortcomings, however, I am able to make a number of important contributions. Mine is the first true district-level test of the “conditional hypothesis” relating institutional and sociological factors to political outcomes. This is vitally important because, as many scholars have previously argued, this is the level at which many of our theoretical intuitions about electoral competition are supposed to operate. But thorough data collection is not, in itself, much of a contribution. Pushing further, I described how a generative

Bayesian LDA model could straightforwardly be applied to the measuring of latent diversity by treating voters and their demographic characteristics as analogies for the “words” and “topics” of LDA modeling. What results is an iterative sorting procedure that allows the researcher to make virtually no assumptions that might bias the outcomes. While so doing might allow non-salient demographic dimensions to creep into the structuring of the groups, one might argue that this is still important variation that needs to be modeled. Whether or not political parties make use of every distinct group in an electoral constituency is structured by a number of things, at least one of which is parties’ own ability to shape the rhetoric of political competition around issues that benefit them electorally. As an older literature on the spatial model of voting has demonstrated, “political entrepreneurs” and “architects of political change” are always looking for ways to transform non-salient cleavages into salient avenues of political competition when they stand to benefit from them electorally (Riker, 1986, 1990; Schofield, 2006). Allowing a measure of diversity to capture some of these (potentially) non-salient-but-certainly-real demographic divisions allows us to paint a more complete picture of political competition at the district level.

Finally – in an effort to explain the disappointing lack of support for the “conditional hypothesis” – I advance a novel theoretical contribution: parties are unlikely to view the political reality of an individual district in a vacuum. This is a point returned to time and again in the burgeoning literatures on party system nationalization and partisan bias and I argue in this paper that the general logic should apply to the question of diversity in a specific way; namely, that within-district and cross-district diversity should work at cross purposes. For an average level of district magnitude in the data set, this argument is borne out empirically: for countries with less than about 4.5 effective groups nationwide, increasing within-district diversity should indeed increase the effective number of parties. Above this number, though, increasing within-district diversity either has no impact or actually drives down the effective number of parties. This is because, when districts are populated with radically different types of voters, parties have a hard time re-branding their platform over

and over again. Even though internally diverse constituencies might offer potential new parties a wealth of unique policy positions they might assume in order to gain an electoral foothold, this benefit is quickly overwhelmed by the costs of being universally appealing to radically different constituencies. The conclusion is simple: for two countries with the same district magnitudes and the same level of within-district diversity, the country with greater cross-district diversity will have systematically fewer parties competing in each district.

Statistically speaking, this relationship is slightly weak. The logic expressed above holds particularly well in SMD countries, but appears to have less influence in MMD countries. This difference may be due, in part, to the fact that MMD countries have systematically fewer districts than SMD countries and the prospect of re-branding for parties in these systems is a less daunting proposition across, say, 29 districts (as is the case in the MMD country of Sweden) than in, say, 659 districts (as was the case in the SMD country of the United Kingdom in 1997). But even beyond this largely mechanical point, I think this dynamic points to a more important finding: district magnitude simply plays a larger role in structuring political competition than does district-level latent diversity.

As the predicted values in Table 4 indicate, moving from a district magnitude of 1 to a magnitude of 5 *almost always* results in a larger absolute change in the effective number of parties than a commensurate movement from 1 to 5 district-level demographic groups. In thinking about district magnitude as an upper bound on the number of parties sustained in political competition, the effective number of demographic groups needs to become quite large to reach this upper limit. Indeed, if we are to reach an *ENEP* that equals $M+1$ (Cox, 1997) in a district with a magnitude of 5 in a country with relatively low cross-district diversity, the results of this analysis indicates that we need almost 9 demographic groups. By contrast, if we examine a similarly-sized district in a country with *high* levels of cross-district diversity, then it is actually impossible to hit the district's institutional carrying capacity simply by increasing the amount of within-district latent diversity. This constitutes a significant qualification to our traditional understanding of the way that permissive institutions

allow sociological diversity to be translated into a diversity of party offerings. Indeed, the nature of the party system within an individual district *will not be the same* if it is removed from its country of origin and dropped into another country with a different level of cross-district diversity. This finding should be of interest not only to political practitioners, but also institutional designers and scholars of political representation.

Appendix A: *ENG* Attributes by Country

Table A1: Countries Included in the Analysis.

| Country | Avg. Resp. Per District | Ethnic | Language | Religion | Rural | Income | Regime |
|----------------|----------------------------|--------|----------|----------|-------|--------|--------|
| Australia | 34.9 | x | | x | x | x | x |
| Austria | 27.1 | | x | x | x | x | x |
| Belgium | 206.9 | | x | x | | x | x |
| Brazil | 130.1 | x | x | x | x | x | x |
| Canada | 9.8 | x | x | x | x | x | x |
| Chile | 29.3 | | | x | | x | x |
| Croatia | 100.4 | x | x | x | x | x | x |
| Czech Republic | 170.1 | x | x | x | x | x | x |
| Denmark | 117.7 | | | | x | x | x |
| Finland | 177.1 | x | x | x | x | x | x |
| France | 10.1 | | x | x | x | x | x |
| Iceland | 550.2 | | x | | x | x | x |
| Ireland | 84.5 | | | x | x | x | x |
| Italy | 53.3 | | x | x | x | x | x |
| Japan | 24.4 | | x | x | x | x | x |
| Mexico | 40.2 | x | x | x | x | x | x |
| New Zealand | 80.5 | x | x | x | x | x | x |
| Norway | 291.3 | | x | | x | x | x |
| Peru | 175.0 | | | | x | x | x |
| Philippines | 14.6 | x | x | x | x | x | x |
| Poland | 134.9 | x | x | x | x | x | x |
| Portugal | 266.4 | | x | x | x | x | x |
| Romania | 73.5 | x | x | x | x | x | x |
| Slovenia | 189.6 | x | x | x | x | x | x |
| Spain | 74.1 | | x | x | x | x | x |
| Sweden | 113.5 | | x | | x | x | x |
| Switzerland | 255.0 | x | x | x | x | x | x |
| United Kingdom | 9.7 | x | x | x | x | x | x |
| United States | 9.6 | x | | x | x | x | x |

Notes: The first column contains the average number of CSES respondents per district when pooling data across CSES waves while subsequent columns indicate which demographic components were utilized in the diversity calculations.

Appendix B: Testing Different Response Rate Cut-Points

The topic modeling approach employed in this manuscript should, in principle, be applied to a sufficiently large set of observations to allow it enough “degrees of freedom” for correct classification of individuals and districts into groups. Technically speaking, however, the model is capable of operating on *any* set of observations, no matter how small. This is especially helpful in the case of survey respondents situated in electoral districts, where the number can sometimes be quite small. In the CSES database I employ in this study, for example, the mean number of respondents in each district is 43 (pooling across waves of the CSES). In single-member districts, the average response rate is somewhat lower at 23. In order to provide the topic model with a more-or-less detailed portrait of each district, I dropped those districts with 10 or fewer respondents in the main line of my analysis.

Admittedly, this low-response cut point is somewhat arbitrary. In this appendix, however, I focus on one important model and show how applying different cut points leaves the result intact. I calculate three different hierarchical models, report the coefficients of interest, and calculate the marginal effects in those cases where we drop all districts with 15 or less, 20 or less, and 25 or less respondents. While I think the case for including districts with lower cut points is straightforward, these supplementary models demonstrate that the choice of cut point is generally not driving the results presented in the main line of the analysis.

Table B1: Multilevel Linear Regression Model of District-Level *ENEP*.

| D.V. | Drop ≤ 15 | Drop ≤ 20 | Drop ≤ 25 |
|--|----------------------------------|----------------------------------|----------------------------------|
| Within-District <i>ENG</i> | 0.31 [0.12, 0.51] | 0.41 [0.17, 0.65] | 0.55 [0.27, 0.84] |
| Cross-District <i>ENG</i> | 0.27 [0.10, 0.43] | 0.30 [0.13, 0.48] | 0.36 [0.17, 0.54] |
| Within <i>ENG</i> \times Cross <i>ENG</i> | -0.06 [-0.11, -0.02] | -0.08 [-0.13, -0.03] | -0.10 [-0.16, -0.04] |
| <i>N</i> observations | 1,189 | 981 | 899 |

In the paper, I calculated the marginal effect of increasing within-district diversity for different levels of cross-district diversity and concluded that this effect is positive so long as cross-district *ENG* remains below 4.5. The marginal effect of within-district *ENG* on the effective number of parties is $\beta_1 - \beta_3 \text{crossENG}$. For these three models, the marginal effect of driving up within-district diversity on the effective number of parties is positive so long as cross-district *ENG* remains below 5.2, 5.1, and 5.5, respectively. In conclusion, not only do all coefficients remain statistically significant at different cut points, but the substantive effect is remarkably consistent: increasing within-district diversity only increases the effective number of parties so long as cross-district diversity remains comparatively low.

Appendix C: Models Without Averaging Values of ENEP

In the paper, I argued that – since diversity data was pooled across separate waves of the CSES in order to capture the most complete picture – the outcome variable should be pooled as well. To that extent, one value each of pooled within-district diversity, pooled cross-district diversity, and district magnitude was paired with one value of average effective number of parties per electoral district. Random intercepts in the data hierarchy were only included at the country level. In this way, each district appears in the data set once, regardless of how many rounds of electoral data I had for the district. Conceptually, this makes good sense: if we are not measuring time-serial variation in the explanatory variables, nor should we be in the outcome variable.

Typically in district-level studies of electoral outcomes, however, researchers take a different approach with their modeling efforts: the underlying institutional and social characteristics of an electoral district structure a latent environment where voters and parties meet repeatedly to play out multiple rounds of electoral competition. Under this conceptualization of electoral competition, each electoral district’s institutional and social attributes are measured once and held constant across different iterations of the electoral process. In this way, in the hierarchical structure of the data, each district can be modeled with its own random intercept and the outcome variable (ENP, voter coordination, number of new parties, volatility, etc.) can be allowed to take on a new value in each election. In the models that appear below, I adopt this approach in an effort to demonstrate that my choice of averaging values of the outcome variable presented in the main line of the analysis are not influencing the results. I repeat the analysis for the “traditional interaction” model (my Model 1) and the “diversity model” (my Model 2). I have omitted insignificant control variables in an effort to focus on the relationship between district magnitude and latent diversity.

As opposed to the model included in the manuscript, we see here in the supplementary version of Model 1 that district magnitude and within-district *ENG* both return statistically significant coefficients in this formulation of the “interaction model” although the interaction

itself is far from being significant. By contrast, the findings on the interaction between within- and cross-district diversity are very consistent with the results reported in Model 2 in the manuscript. Not only do all coefficients remain significant, but they also generally maintain their substantive size. However, as evidenced by the goodness-of-fit statistics, this second model fits the data more poorly than a model that includes district magnitude, lending additional credence to my substantive conclusion that it is really district magnitude that drives party system size rather than the niceties of underlying social divisions.

As a final point, it should be noted that district-level variance in this formulation of the hierarchical model is quite low. This is not surprising, given that the district-level predictors cannot take on different values in each election; by contrast, the outcome variable assumes a new value for each election year. Thus, the “value added” of modeling this district-level variance is not readily apparent. Accordingly, the main line of the manuscript’s analysis relies on averaging values of the effective number of parties and treating each district as its own (single) observation, regardless of whether or not the CSES administered multiple election surveys there.

Table C1: Multilevel Linear Regression Model of District-Level *ENEP*.

| D.V. | Model 1 | Model 2 |
|---|--------------------------|---------------------------|
| Log(Magnitude) | 0.60 [0.03, 1.16] | |
| Within-District <i>ENG</i> | 0.11 [0.04, 0.18] | 0.41 [0.22, 0.60] |
| Cross-District <i>ENG</i> | | 0.30 [0.11, 0.49] |
| Log(Magnitude) × Within <i>ENG</i> | -0.01 [-0.28, 0.27] | |
| Within <i>ENG</i> × Cross <i>ENG</i> | | -0.08 [-0.12, -0.04] |
| Intercept | 2.84 [2.43, 3.26] | 2.37 [1.38, 3.36] |
| $\hat{\sigma}_{cde}^2$ | 2.04 | 2.04 |
| $\hat{\sigma}_d^2$ | 0.05 | 0.07 |
| $\hat{\sigma}_c^2$ | 0.76 | 1.44 |
| AIC | 11404 | 11458 |
| LL Ratio Test Statistic | 76 | 22 |
| <i>N</i> countries | 29 | 29 |
| <i>N</i> districts | 1,605 | 1,605 |
| <i>N</i> observations | 3,160 | 3,160 |

Notes: Values for the outcome variable in different elections are left separate (rather than averaged), thereby driving up the number of observations. 95% confidence intervals were calculated from 1,000 simulated draws of the model's parameters and appear in brackets below coefficient estimates.

Appendix D: The Political Saliency of Social Divisions

One important consideration that the main line of the analysis fails to take into account is the political saliency of social divisions. In the conclusion to the manuscript, I note that while we should expect more parties in the presence of a larger effective number of social groups, this will only be the case when political entrepreneurs are willing to undergo the political machinations necessary to electorally mobilize these groups. If, for example, a population can naturally be divided into three groups based on a combination of religious and income divisions, but most parties run on economic platforms, then religion would not be politically salient despite the fact that – mathematically speaking – it *could potentially* be. Addressing the issue of salience in large- n , cross-national comparative research is difficult. Relying on in-depth knowledge of election- and country-specific contexts simply becomes too infeasible when we are interested in assessing empirical relationships across a large number of cases.

As a side-project for this dissertation paper, I developed the beginnings of a methodology that – together with Santiago Olivella and Brian Crisp – I will continue to work on in the near future. The basic idea is to apply the type of iterative, algorithmic classification procedures discussed in this manuscript with district-level electoral results in an effort to explain which demographic attributes were primarily responsible for structuring electoral outcomes in socially similar districts. First, at the district level, we will develop a computational clustering procedure that classifies districts into different “groups” based on many demographic dimensions and that is a priori agnostic both about the number of salient divisions in society and also which specific divisions are most important (call these groups *demographic partitions*). We will also use this same clustering procedure to similarly categorize districts into groups based on the nature of electoral outcomes in the district (call these *vote partitions*).

Second, we will compare both sets of partitions using a novel correspondence procedure that evaluates all possible matches of these taxonomies and selects the optimal match. The result of this process will not only be a measure of how many social divisions are needed to slot districts into a distribution that could support a matching electoral distribution, but

also which specific divisions are needed to produce such a match. Third, we will validate both the dimensionality and the social attributes that characterize the match by comparing the results to expert surveys and party manifesto codings. For example, if we find that vote-partitions and demographic-partitions in a given country are best explained by ethnicity and income, we might look to Comparative Manifesto Project codings or expert surveys for that election to see if the parties were, indeed, talking about ethnic and economic issues. With this type of external validation in hand, we will, finally, use the results as a powerful explanatory variable to evaluate a number of novel hypotheses related to party competition, government formation, and policy outcomes.

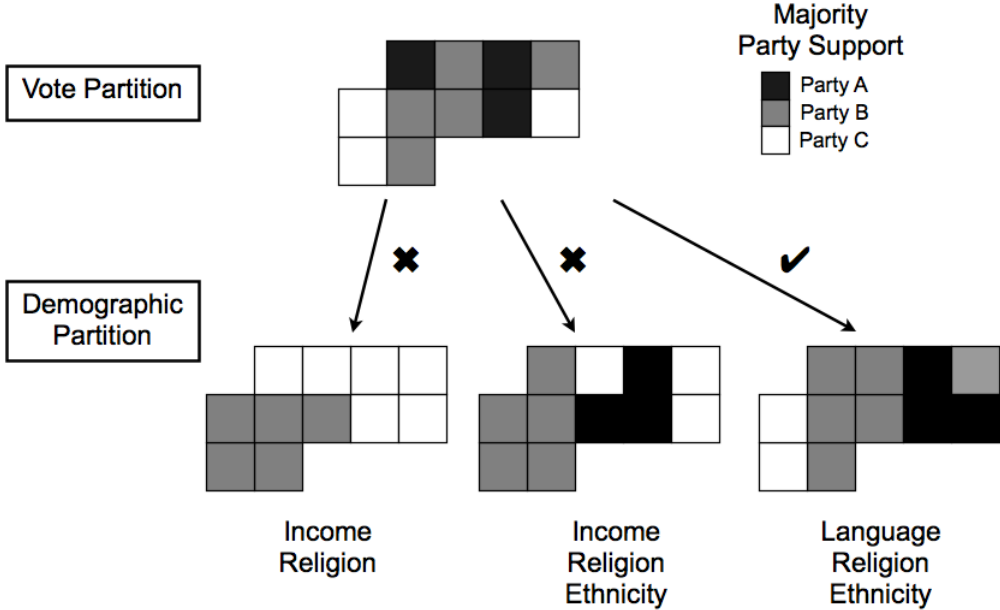


Figure D1: The Clustering Correspondence Procedure as a Method for Ascertaining the Political Saliency of Social Divisions

Before listing the sorts of research questions we hope to examine, I will first discuss in greater conceptual detail how we can use district-level electoral and demographic data to recover the salient social divisions underlying party competition. The Figure D1 intuitively illustrates this process. First, we use a clustering procedure to slot districts into similar groups based both on votes for parties and also for various demographic characteristics.

In the simplified illustration below, similar districts in the vote partition are merely those where the same party won the majority of votes. The demographic partition will entail many more complications because we intend to iterate the clustering procedure across all possible permutations of demographic variables. So, for example, at the bottom left corner of the diagram, the districts have been categorized taking only income and religion into consideration whereas at the bottom right corner, three demographic variables (language, religion, and ethnicity) have been used as the criteria with which to sort the districts. At any rate, we should be able to return a description of which demographic attributes combine to provide the closest clustering match to electoral outcomes. In the case of the figure above, it is clear that the electoral outcomes in the hypothetical arrangement of districts most closely matches a social taxonomy of districts predicated on linguistic, religious, and ethnic divisions. We would conclude, then, that these three social attributes were salient in this election. With this information in hand, we will be positioned to begin theorizing along a number of important dimensions. The range of potential research questions is large, but we have begun to think about the following:

- Are parties able to change the contours of their social support?
- Do countries with more salient cleavages have larger party systems? Are party systems that mobilize more social cleavages also more “nationalized” in nature?
- Does the nature of a party’s social support influence its probability of being included in a governing coalition?
- Does the social structure of political competition affect what types of policies and budgets are produced by the legislature?

– Paper 3 –

Explaining Party Label Linkage as Supply-Side Ballot Composition

Abstract: The extent to which political parties field candidates or lists across multiple electoral districts can profoundly impact the comparability of ballots voters in different parts of the same country have access to on election day. While previous studies have measured this “linkage” by focusing on the distribution of voter support across districts, I argue that measuring linkage – an inherently supply-side phenomenon – using demand-side information is empirically incorrect and obscures some important theoretical attributes of the electoral market. I demonstrate that linkage is an empirically differentiable concept from nationalization. I then examine which institutional factors shape the extent of linkage across nearly 2,000 parties in nearly 500 elections in 47 different democracies. I find that, while the centralization of political power in a country drives up the extent of linkage, this effect is more salient for smaller than larger parties. Indeed, as parties grow, they tend to link their platforms across almost all districts, regardless of the macro-institutional environment.

Introduction

Around the world and across all types of electoral systems, party elites make strategic decisions leading up to election day that structure the supply side of the electoral market. Indeed, before voters ever have the opportunity to cast a ballot, the ranks of this ballot must first be populated with party offerings. Scholars of electoral politics have long studied the determinants of ballot composition at the district level: various institutional and social factors come to bear in determining both the *number* (whether few or many) and *type* (whether centrist, extreme, niche, etc.) of party offerings in any given district (the literature in these regards is vast, but see, for example: Cox, 1997; Cox and Shugart, 1996; Downs, 1957; Duverger, 1954; Golder, 2003; Grofman, 2004; Ordeshook and Shvetsova, 1994). Voters living in larger magnitude districts, for example, especially in newer democracies with a number of salient social divisions, must sort through comparatively longer rosters of potential candidates or party lists. In addition, these offerings can be expected to occupy a broad swathe of the ideological spectrum, possibly including even the most extreme positions. By contrast, voters living in

single-member constituencies in more established democracies will face a severely circumscribed set of ballot options on election day. These offerings can additionally be expected to be more centrist in their ideological orientations.

Moving beyond thinking about ballot composition in *any one district*, however, scholars have also been interested in explaining cross-national variation in the extent to which ballots across different districts within the same country might resemble one another. This level of similarity in ballot composition has been examined indirectly through various lenses. For example, scholars have shed some light on this question by explaining the emergence of “regional” parties versus more “national” parties (Brancati, 2008; Hearl, Budge and Pearson, 1996). In the vocabulary of earlier studies, a political party is a “national party” if it is able to contest elections on equal terms in all areas of a country (Schattschneider, 1960) or if it is able to compete in “all or all but a handful of constituencies in every region of the country” (Urwin, 1982, p. 220). Other studies have more explicitly called attention to similarities in ballot compositions as a byproduct of elite-level decisions to “link” their party labels across districts (Cox, 1999), or as a result of the extent to which parties field candidates with a mind toward providing “territorial coverage” of the entire country (Caramani, 2004). More recently, scholars have turned to a battery of “inflation indices” (Chhibber and Kollman, 1998; Kasuya and Moenius, 2008; Moenius and Kasuya, 2004) predicated on voter support for party offerings in order to examine the extent to which the party system in any one district resembles the national party system in the aggregate. These comparisons have always been made using an electoral statistic – the effective number of parties – that draws on *both* supply-side ballot offerings *and* demand-side voter support.

But this focus on the size of the party system weighted by vote shares conflates the two (separate, albeit interrelated) stages of electoral competition: first, political elites make strategic decisions about contesting certain districts and then, second, voters make decisions about which subset of these offerings to support. Previous measures of party system linkage, inflation, and nationalization all draw on information garnered from the second stage –

specifically, the distribution of voter support across districts – to draw inferences about the strategic calculations undergone by political elites during the first stage. If we are interested in the question of what determines ballot similarity across electoral districts, then what we require is a renewed focus on the supply side of the market.

This is a question worth answering for a number of important reasons, most obviously because it can qualify our understanding of nationalization as conceptualized by demand-side voter support. Mechanically speaking, the level of party label linkage across districts (or the homogeneity of ballot composition across districts) will impose an upper limit on the level of party- and party-system nationalization (or the homogeneity of voter support across districts) that can plausibly be attained. If very few parties enter all or most districts during an election, then the extent of nationalization can never be high. To this end, it is worth studying the determinants of linkage as a *supply-side prerequisite* for nationalization. As Cox (1999) first argued years ago, we need to consider the set of factors that might differentially affect party elites and voters because our grasp of the two sides of the electoral market is uneven. While recent studies in the vein of Morgenstern, Swindle and Castagnola (2009) and Morgenstern, Polga-Hecimovich and Siavelis (2012) have explored the determinants of vote homogeneity both across districts and over time, the dearth of studies on linkage has left the supply side of the electoral market a “neglected topic” in this respect (Cox, 1999).

Pairing a separate measure of linkage that is isolated from vote outcomes with a separate measure of nationalization also allows us to paint a richer conceptual picture of what transpires across electoral districts. Lower levels of nationalization, for example, will mean different things in the presence of lower or higher levels of linkage. When there are very few similarities in ballot composition across electoral districts, we will necessarily see lower levels of nationalization. But this pairing of outcomes implies a completely different scenario than one where linkage is high, but nationalization remains low. In the former case, we know nothing about voters’ collective proclivity to uniformly support the same set of parties. In the latter case, however, we can clearly delineate a set of party leaders who are overly opti-

mistic in spreading their candidates thinly across a voting population that is more discerning in which ballot offerings it supports.

In this paper, I make a number of contributions to the study of party label linkage. First, I advance a simple but intuitive measure of linkage that focuses purely on a supply-side concept: the composition of ballots across districts within the same country. I argue theoretically and demonstrate empirically that linkage is a distinct concept from nationalization and, in so doing, make the case for examining its causes. In the second section, I build on the theoretical efforts of Cox (1999), which is, to the best of my knowledge, the only published work to speculate explicitly about the cross-national determinants of ballot composition. I introduce a number of novel hypotheses pertaining to both macro-level institutions and micro-level party strategies that are then tested in a multivariate, hierarchical framework. By merging district-level electoral data collected by Brancati (2007) and Kollman et al. (2011), I draw on what is perhaps the largest database of complete district-level electoral outcomes ever employed in a cross-national study of elections.¹ I show that the extent of centralization of political power in a country will tend to make the party *system*, as a whole, more linked. At the level of the party, however, different parties respond to political centralization in different ways, with smaller parties responding more predictably than larger parties. Indeed, it seems that most large parties – having hit some threshold of notoriety and resource endowments – opt to link their party label across nearly all districts, regardless of the macro-level institutional environment.

¹This database completely disaggregates the “other parties” category that plagues most repositories of electoral data. By virtue of possessing *complete* vote data for *all* political parties, I am able to study ballot composition in very fine-grained detail across thousands of districts in nearly 500 elections spread across 47 democracies.

What is Supply-Side Linkage?

The concept of “linkage” has been defined by numerous scholars as an elite-level, supply-side concept that is temporally antecedent to voters’ decisions about for which party (or parties) to cast a ballot. Cox (1999), for example, defines linkage as “whether the parties in one district are the same as those in another” (p. 146) and, elsewhere, discusses linkage in terms of parties’ decisions to compete across all districts, rather than in terms of the subsequent levels of support they receive from voters (Cox, 1997). At various points in his canonical book on electoral dynamics, Cox (1997) refers to linkage as a “national entry market” problem (p. 198) and a “nationally brokered multi-constituency entry game” (p. 202). Similarly, Caramani (2004) describes linkage as being synonymous with “territorial coverage” or “the number of constituencies in which the party is present” (p. 61), rather than the distribution of votes for the party. An earlier strain of this literature emerging from studies of American politics similarly defined linkage in supply-side terms, with Schattschneider (1960) referring to patterns of contestation (in other words, appearance on ballots) and Urwin (1982) focusing on whether or not a given legislative seat was contested or not (in other words, whether or not there were only one or two ballot offerings for voters to consider).²

But despite the conceptual clarity underlining “linkage” as an elite-level, supply-side dynamic, empirical indicators of linkage have almost universally drawn on voter-level, demand-side information. Cox (1999), for example, constructs a measure of party system inflation that compares differences in the effective number of parties (ENP) at the local and national levels. ENP is a weighted measure of the number of parties competing in the election and the weighting itself is based on the *vote shares* each party receives on election day. From a theoretical standpoint, this conflates supply with demand: rather than having a raw count

²While I am clearing drawing on the electoral institutions literature for my definition of “linkage”, I should also point out that the term has been used to indicate different concepts in other literatures. For example, Kitschelt (2000) and Kitschelt and Wilkinson (2007) use the term to discuss different ways of representationally “linking” voters to their elected legislators.

of parties competing in the district, we instead have a weighted count that internalizes the outcome of the election itself. We can think of any number of research questions that might require the disjuncture between supply and demand to remain intact, rather than merged. For instance, do voters have greater difficulty in coordinating when they're faced with more ballot options? And does the number of ballot options make it more difficult for major parties to dominate the distribution of votes? These are questions that we are fundamentally unable to answer if we cannot distinguish party offerings from voter preferences.

From an empirical standpoint, increasing the raw (unweighted) count of party offerings can have a positive, negative, or negligible impact on the effective number of parties (weighted by vote shares). After all, simply because an additional party enters a district does not imply that it will net a number of votes substantial enough to shift the overall ENP figure. If it does draw votes away from preexisting parties, its entrance may also – depending on the overall distribution – actually *reduce* the effective number of parties. This makes the inflation measure advanced by Cox (1999) and similar inflation measures advanced by Chhibber and Kollman (1998), Kasuya and Moenius (2008), and Moenius and Kasuya (2004) even more problematic as proxies for elite-level decision outcomes. More sophisticated vote-based work in party system nationalization can provide a more nuanced picture of the demand side of the electoral market, but not of the homogeneity in ballot composition across different districts (Bochsler, 2010; Caramani, 2004; Jones and Mainwaring, 2003; Morgenstern and Potthoff, 2005; Morgenstern, Swindle and Castagnola, 2009). What is needed is a measure of ballot composition that is free from the weighting biases incurred by considering (temporally subsequent) vote distributions.

This is not a difficult measure to construct and one has already been proposed by Caramani (2004), although it requires a slight refinement and has up to this point never been used in a broad, cross-national study of this type. I begin with his measure of *territorial coverage*, which is simply the number of constituencies a party entered in an election divided by the total number of constituencies in the country. I refine this measure slightly

by weighting each district’s contribution to this percentage by its district magnitude, which is a standard approach employed in previous studies of nationalization. As many scholars of nationalization have argued, variation in district sizes (whether considering magnitude or voting population, which correlate very highly with one another) can cause methodological and conceptual problems when aggregating up from individual districts to the level of a party (Bochsler, 2010; Ersson, Janda and Lane, 1985; Morgenstern, Polga-Hecimovich and Siavelis, 2013; Rose and Urwin, 1975). In the particular case of studying linkage, I weight entry decisions by district magnitude for the following reason: while political parties would like to offer their platform to as many voters as possible, “voters” do not come to parties as continuous units, but rather as discrete blocks that have been sectioned off into districts. To this extent, where within-country variation in magnitude exists, it is not the same for parties to enter a district with magnitude of five as it is to enter a district with magnitude of ten. The party has a chance to place its platform in front of more voters in the latter case and, thus, the decision to enter the latter district should drive up linkage levels more than entering the former.

Where there is no within-country variation in magnitude, the decision of whether or not to weight entry by district magnitude results in no change; where there is some variation present, this weighted measure appropriately accounts for the fact that the decision to enter a larger district is not the same as the decision to enter a smaller district.³ This measure, then, returns for each party in each election an estimate of the share of all possible ballots upon which its party label appeared. Across all parties within each election, we can then take an average of these party-level shares to construct a *system-level* measure of the average share of ballot homogeneity (or supply-side linkage) across all districts. As this measure increases toward 1.0, voters in different districts are encountering increasingly similar ballots when

³ In empirical fact, however, the choice of whether or not to weight this percentage by district magnitude is immaterial as the two versions (weighted and unweighted) are correlated with one another at $r = 0.98$ across nearly 5,000 party-election observations.

they head to the polls on election day.

While I am strictly interested in the question of entry and not the resulting distribution of votes across parties, I do have to make a few concessions to demand-side dynamics when aggregating up from individual parties to the level of the party system. Rather than weighting each party’s linkage score by its national-level vote share (which would, I believe, bring the concept too in line with various nationalization scores), I instead drop from the analysis those “nuisance” or “very small” parties netting one-percent or less of the national vote before calculating the average level of linkage across *all remaining* parties.⁴ This strategy has the dual conceptual advantage of, first, acknowledging that the analysis should focus on parties that are sufficiently sizable while also, second, leaving as intact as possible the pure supply-side dynamic of the electoral market.

The data set I employ in this study is constructed from very fine-grained data: complete vote data on all parties at the district level across all districts in a given election in a given country. My primary data source was the *Constituency-Level Elections Dataset* managed by Brancati (2007). This is a database that focuses specifically on collecting thorough and complete vote data for all parties that competed in an election, no matter how small. I supplemented this data with a subset of the elections presented in the *Constituency-Level Elections Archive* by Kollman et al. (2011). This latter repository is not as deliberately focused on collecting complete vote figures for all parties and often reports aggregated “other parties” categories wherein vote totals for very small parties are lumped together. For most research questions, this distinction is immaterial. Yet for the research question at hand – the composition of ballots – the appearance of a party name (regardless of how few votes the party will eventually go on to obtain in the election) on the ballot is my primary point of interest. Lumping together many smaller parties’ vote totals into an “other” category will

⁴The regression results presented later in the manuscript are robust to dropping those parties that netted *five*-percent or less of the national vote before calculating the cross-party average linkage level. The results of this robustness test are presented in the appendices at the end of this paper.

obscure much of the important variation I am interested in measuring.

I define $entry = 1$ for a given party in a given election's district when that party received at least 1 vote in the data set. I define $entry = 0$ for those districts where the party received no votes. Practically speaking, the data sets that I draw on for electoral returns treat non-entry in systematic ways that make the distinction between entry and non-entry even clearer. The CLE data set simply leaves blank cells in the data for districts where a party did not field a candidate and the CLEA only includes party-district observations where the party received at least 1 vote. Admittedly, without having original ballots on-hand, I cannot observe the possibility of an entry decision that resulted in absolutely no voter support. However, I work from the fundamental assumption that, if a party stood for election in a given district, then it received at least one vote in the district. This is not, I think, a terribly heroic assumption, given the fact that it would require very few resources for a party to net one vote in any given electoral competition (indeed, we might expect that the candidate who stood in the competition would at least vote for herself or himself). Still, I concede the point that a party's label might have appeared on a ballot, yet the party received no votes.⁵

The resulting indicator is empirically distinct from previous measures of linkage or nationalization that include various weighting schemes based on vote distributions. A glance at the correlation coefficients between my indicator and various other indicators in Table 1 is telling: isolating the supply side from the demand side results in a very low degree of covariation between the two concepts.⁶ In the next section, I will summarize the scant liter-

⁵ In correspondence with Prof. Dawn Brancati, however, she argues that this scenario occurs with negligible frequency in her data, which provides the overwhelming majority of observations in my analysis.

⁶In large part, this might be attributed to the simple fact that one measure is weighted by national-level vote share and the other is not. Although I have argued that there are analytical justifications for *not* merging the supply and demand sides of the electoral market in this way, in results not reported here, I weighted my linkage metric by national-level vote share and plotted it against the same battery of nationalization metrics. The correlation coefficients are stronger, but still comparatively weak and allow for a great deal of variation in nationalization values while holding constant linkage values. For example, for those country-election

Table 1. Correlations Between Average Share of Districts Entered and Various Linkage Indices

| Linkage Metric | Correlation Coefficient |
|--|--------------------------------|
| <i>Cox's Inflation Index</i> | 0.04 |
| <i>Chhibber and Kollman's Difference Index</i> | 0.04 |
| <i>Moenius and Kasuya's Inflation Index</i> | -0.37 |
| <i>Moenius and Kasuya's Weighted Inflation Index</i> | -0.41 |
| <i>Jones and Mainwaring's Gini Measure</i> | 0.57 |
| <i>Bochsler's Gini Measure</i> | -0.31 |

Note: Country-Election Observations, $n = 511$.

ature that has speculated on the causes of linkage and conduct the first cross-national test of these hypotheses alongside my own novel set of hypotheses. I will pay particular attention to different dynamics that might exist at the level of the party system in the aggregate as well as at the individual party level.

Explaining Variation in Linkage

Why might a political party decide to enter electoral competition in fewer or greater numbers of electoral districts? I will advance several hypotheses in response to this question, each of which falls into one of three broader conceptual arguments. The first set of hypotheses was advanced by Cox (1997, 1999) and can be characterized as *system-level centralization* hypotheses: as political control in a country becomes increasingly centralized, party elites face incentives to increase their national profiles and link their platforms across districts in observations in my data set with very high system-level weighted linkage (i.e., 0.95), Bochsler's measure of nationalization returns system-level values ranging from 0.7 to 0.9. Clearly, then, linkage is a concept worth studying in its own right as something distinct from nationalization.

an effort to draw as much support as possible from as many constituencies as possible. The second set of hypotheses might best be characterized as pertaining to *system-level electoral institutions*: parties may enter districts more or less broadly due to there being larger (or smaller) district magnitudes or due to political elites' and voters' level of experience with democratic institutions. Finally, the third set of hypotheses pertains to *party-level characteristics*: larger and smaller parties – in terms of their notoriety and, perhaps, resource endowments – will be differently predisposed to linking their efforts across electoral districts. Furthermore, the effects of the system-level variables might differentially impact large and small parties. I discuss each of these three types of hypotheses in turn.

To begin, Cox (1999) makes the straightforward argument that when political parties need to accomplish tasks that require many legislators (like securing a majority of seats in a unicameral or strongly unitary legislature), they will field candidates across a larger number of electoral districts in the hopes of increasing their vote shares. This logic follows on from an older literature on consociational or consensus democracy, where unitary and unicameral political systems are thought to push political elites into “winner-take-all” strategic mentalities (Lijphart, 1977, 1984; Lijphart and Crepaz, 1991). Completely aside from electoral institutions themselves, the existence of powerful offices with hefty jurisdictional powers over the country at large will orient political elites toward merging their platforms with like-minded parties to benefit from returns to scale that are unavailable to smaller parties opting to retain their own label in just a subset of the country’s electoral districts. Where the value of national office is high – as it is in unitary and unicameral systems – the supply side of the electoral market should be considerably more restricted when compared to countries with significantly devolved decision-making powers or with two houses in the national legislature.

Make no mistake: political parties always prefer more seats in national-level legislatures. The argument for centralization leading to linkage, however, is that centralization creates fewer opportunities for success in a limited set of electoral districts to result in some substantial amount of seats. Under more diffuse power arrangements, by contrast, there are

more opportunities for limited (regional) success to result in actual political representation. In the presence of the devolution of political power in federal systems, for example, smaller parties can gain representation in regional legislatures by only running candidates intensively in a handful of the country's electoral districts. Indeed, Brancati (2008) demonstrates that political decentralization results in the emergence and success of regional political parties. Having gained representation in regional parliamentary bodies, regional parties can use this visibility to contest elections at the national level – but they can only effectively do this in their regions. In unitary systems, by contrast, this cannot happen. Parties must instead cast their policy platforms more broadly for want of plausible region-level venues of political contestation. Additionally, as is the case in many countries – such as Austria, Germany, and India – representatives to the upper chamber are nominated by regional parliaments (Brancati, 2008). For these reasons, as political power becomes increasingly centralized, linkage – measured in the aggregate – should increase as parties contest elections in a more geographically diffuse fashion.

H1: Linkage will be higher in unitary systems than in federal systems.

H2: Linkage will be higher in unicameral than in bicameral systems.

Cox (1999) notes that the choice of executive type could potentially impact linkage as well, specifically that the existence of an independently-elected president offers a substantially valuable national-level office to political parties (Samuels, 2002; Shugart and Carey, 1992; Shugart, 1995). Scholars of the effective number of parties have long explored the link between the size of the party system (again, weighted by demand-side vote distributions) in the presence of presidential elections, especially those that coincide with legislative elections (Clark and Golder, 2006; Golder, 2006; Hicken and Stoll, 2008, 2011). The common empirical finding in this literature is that presidential elections exert a downward pressure on the size of the party system (Cox, 1997; Hicken, 2009; Hicken and Stoll, 2011). While some research in this vein has qualified the relationship somewhat by arguing that this effect is felt more

strongly when presidential and legislative elections are concurrent (Golder, 2006; Samuels, 2002, 2003; Shugart and Carey, 1992) and when the powers of the presidency are substantial (Cox, 1997; Shugart and Carey, 1992), it should at least be the case that – relative to parliamentary systems – presidential systems induce higher levels of linkage.⁷

H3: Linkage will be higher in presidential than in parliamentary systems.

Moving away from the dynamics of centralization, we can consider supply-side hypotheses that have more to do with *system-level electoral institutions*. These include issues of proportionality and of political elites' and voters' level of experience with democratic institutions. As the average magnitude of all districts in a country increases, for example, so too does the proportionality in the translation of votes into seats (Anckar, 1997; Benoit, 2001; Gallagher, 1991; Taagepera and Shugart, 1989). Parties may enter a larger share of districts in a country with higher average district magnitude, not necessarily with an eye toward increasing their national profiles, but as a down-ticket implication of their district-by-district strategic calculations. In a country that has large multimember districts, for example, the higher level of proportionality results in a smaller overall share of votes in each district being converted into seats in the legislature. The marginal utility of fielding an additional candidate or list in another district in the country is high where proportionality is high. Completely aside from any considerations about national policy or the concentration of decision-making

⁷The empirical findings of the literature relating presidentialism to party system size are many and nuanced. For example, the presence of an independently-elected executive has been found to drive down party system size, but only when there are few (as opposed to many) presidential candidates (Golder, 2006). Without data on presidential election outcomes, I cannot in the present study control for these various nuances. However, in regression models not reported in this study, I explored issues of concurrence and presidential power within the subset of presidential countries I have in my data set (10 total). The empirical results relating these variables to linkage were null. Because I am working toward a broader theory of linkage – of which the nature of the executive branch is only one facet – I herein draw only a fairly crude distinction between presidentialism and parliamentarism and leave the exploration of greater nuance in this relationship for future work.

power, then, we might expect more linkage in countries with larger districts.

H4: Linkage will increase as average district magnitude increases.

Another system-level hypothesis I offer pertains to *democratic learning* and builds on the well-known relationship between, on the one hand, party and voter coordination and, on the other, the number of times that parties and voters have interacted under a set of democratic electoral institutions.⁸ Substantial empirical evidence supports the claim that the homogeneity of ballot offerings should be higher in older rather than in newer, less established democracies. When voters and parties have little experience with democratic institutions (or immediately after a major reform to electoral institutions) they have a hard time coordinating amongst themselves and assessing which party offerings are truly viable (Tavits, 2007; Tavits and Annus, 2006; Weyden and Meuleman, 2008). The empirical literature has found evidence of less successful coordination in the first, second, third, fourth, and – in some cases – even the fifth election held after a democratic transition (Dawisha and Deets, 2006; Duch and Palmer, 2002; Moser, 1999, 2001; Singer and Stephenson, 2009). When coordination levels are low and voters are not afforded the “heuristic benefit” (Gschwend, 2007) of previous elections, then “every potential entrant is perceived as having as good a chance of winning

⁸A final possible explanation for cross-national variation in linkage that Cox (1999) discusses is social heterogeneity, although he ultimately concludes that its effect on linkage (whether positive or negative) could operate in either direction. The reasoning behind the ambiguity of this relationship is that – for social cleavages to inform linkage – we need some way to measure heterogeneity not only *across* districts, but *within* them as well. The findings of recent studies on the distribution of voter support for political parties across districts reinforce this ambiguity. Morgenstern, Swindle and Castagnola (2009), for example, return null results when they consider social diversity measured only at the national level and Crisp, Olivella and Potter (2013) demonstrate that, without taking into account the ways in which demographic characteristics are spread across districts, we cannot adequately assess the role that diversity plays in determining voter support for party offerings. Due to the theoretical and empirical ambiguity surrounding the relationship between social cleavages and the extent of linkage (as well as severe data limitations for the set of countries considered in this study), I set aside this potentially important explanatory variable for the time being and move onto focus on other determinants of linkage.

as any other” (Tavits, 2007, p. 117). In this environment of confusion, parties in nascent party systems might be overly optimistic about their electoral prospects as they wait for voters to coalesce around the more viable offerings.

This false optimism about their electoral prospects might find smaller (poorly-linked) parties hanging on over the course of the first few elections. Over time, however, such parties will be forced to confront the fact that the electorate has separated out the viable from non-viable parties and has begun to coalesce around more successful ballot offerings. As these learning effects take hold and as these larger (better-linked) parties begin to consolidate their dominance, smaller parties will be forced to drop out of competition. In doing so, the *overall* level of linkage in the party system will increase over the course of the first several elections after democratic transition or major institutional reform. In more established democracies, then, system-level linkage will be higher and this will be due to the fact that the party system is – on the whole – comprised of a larger share of highly linked individual parties.

H5: Linkage will be lower in newer than in more established democracies.

Moving on to consider some party-level dynamics, one could straightforwardly argue that “larger” parties (whether measured by resource endowments, public notoriety, national-level vote share, etc.) will field their candidates or lists in more districts, thereby driving up party-level linkage. This relationship no doubt has both mechanical and strategic components. For instance, it is almost mathematically true that – in party systems around the world – those parties netting the highest national-level vote shares are also those parties whose label appears on almost every ballot in the country. Linkage is, in this sense, a prerequisite for achieving very high levels of success in the national aggregate: no matter how successful a regional party may be, its success at the national level is constrained by its comparatively lower level of linkage. Beyond the mechanical, however, there are also strategic components behind larger parties’ efforts to achieve higher linkage levels. Having one’s candidates in all (or most) districts across the country can, itself, be a signal of electoral viability. Even in those countries with very restrictive electoral rules where strong incumbents sometimes

run unopposed – such as the United States and the United Kingdom – major political parties – such as the Republican Party and the Labour Party – still field candidates in the vast majority of electoral districts (despite the rational expectation of losing many of these contests). Especially when a party has undertaken the task of crafting a policy platform that caters to broader – rather than regionalized – interests, the marginal cost of fielding this platform in an additional district is comparatively low. For both these mechanical and strategic reasons, then, we would expect larger parties to exhibit higher levels of linkage.

H6: At the party level, linkage will increase as party size increases.

Despite my focus on the supply side of the electoral market, I grant the fact that parties are strategic actors and will, to some extent, condition their linkage decisions based on evidence they have garnered from previous iterations of the electoral market. In particular, parties may look to previously high levels of variability in their support among voters as evidence that they are stretching themselves too thinly across too many districts. When their lagged district-level vote shares, for instance, indicate that their support is substantially imbalanced, this is a signal that different constituencies are responding differently to their party label. Accordingly, parties are faced with the prospect of either tailoring their platforms to specific constituencies (a resource-intensive proposition) or simply pulling back on the number of districts they contest. Thus, I hypothesize that previously high levels of variability in a party's district-level voter support will prompt it to drop out of areas where it fared poorly and decrease the overall number of districts in which it previously fielded candidates.

H7: At the party level, linkage will increase as variance in a party's lagged district-level vote shares decreases.

Finally, I argue that differently sized (or endowed or notable) parties will respond to macro-level institutional variables in different capacities. Specifically, I argue that the party-level strategic considerations of larger parties will begin to override these institutional concerns as parties grow in size. Political centralization, proportionality, and democratic experience will all drive up linkage for *most* parties in a country. Larger parties, however –

perhaps making resort to the economies of scale inherent in fielding their candidates across all districts (even those where they expect to lose) – will exhibit high levels of linkage regardless of the institutional circumstance. For this reason, it is possible to find major parties with high linkage levels in both parliamentary and presidential democracies, for instance, as well as in both federal and unitary countries. Smaller parties, by contrast, must be more discerning in choosing where to field their candidates. They possess fewer resources and are less recognizable to the general voting public. They face real tradeoffs in opting to compete in some districts at the expense of others. For smaller parties, then, institutional incentives should bite in the sense that smaller parties, of necessity, will pay much closer attention to these incentives. This leads to my final hypothesis.

H8: The relationships between macro-level institutions and linkage will – at the party level – hold better for smaller parties than for larger parties.

I now turn to an examination of each of these hypotheses in a cross-national, hierarchical regression analysis. Because of the nature of the data set – repeated elections nested within countries with fixed characteristics – I employ a set of hierarchical linear models. I first construct a model at the party-system level taking average linkage across parties as the outcome variable. I next build a model at the individual party level that includes a handful of party-level covariates as well as their interaction with the macro-level institutional variables. This model will seek to explain party-level linkage figures. In both sets of models, I exclude those parties netting less than one-percent of the national-level vote in order to eliminate “nuisance” or very small parties that are so idiosyncratic that their inclusion would only add statistical noise to the estimates. In an appendix, I re-estimate the models by increasing the barrier of exclusion to five-percent or less of the national vote.

Data and Analysis

As described above, my data set is comprised of complete district-level vote totals across more than 500 elections in nearly 50 countries around the world.⁹ For the system-level model, my outcome variable is *average party linkage* across parties gaining more than one-percent of the national vote within any given election.¹⁰ In an effort to leave separate the demand-side vote information, the average is unweighted by party vote shares. This choice of not weighting – while problematic in that it might be potentially biased in favor of smaller parties – is nonetheless a more accurate reflection of “linkage” as defined by *homogeneity in ballot composition* across districts. For the second, party-level model, these weighting concerns are immaterial, as no aggregation of parties must take place. The outcome variable here is simply the party’s level of linkage in a given election.

For the first model, my institutional explanatory variables are cast at the national level. First, I use a dummy variable taken from Treisman (2007) that measures whether or not a country is a *federal* system.¹¹ To test the effects of bicameralism on linkage, I employ another dummy variable for *bicameralism* that is taken from the World Bank’s (DPI) Database of Political Institutions (Beck et al., 2001). A third dummy variable for *presidentialism* is similarly taken from the DPI, where I coded as “presidential” any country that has a directly-

⁹Although, due to data availability on other covariates, not all of these elections and countries will be included in the regression analyses that follow.

¹⁰Recall that these linkage percentages are weighted by district magnitude. In separate models not reported in this manuscript, robustness checks were undertaken at both the system- and party-level with linkage figures that were *unweighted* by district magnitude. The signs and significance levels of each of the main results were almost always completely unchanged.

¹¹Following, Hicken and Stoll (2008), I substituted in data on the share of a country’s expenditures and revenues that are spent or generated at subnational levels. These data were taken from the World Bank’s Fiscal Decentralization Indicators. The temporal coverage of this data is comparatively sparse compared to the federalism dummy variable, but substituting in either subnational expenditures or revenues failed to return any significant relationship between federalism and linkage.

elected executive. The *average district magnitude* variable was based on original calculations from the electoral returns described above and enters the regressions in logged form. This is to account for two things: first, because studies of proportionality have long noted its diminishing returns as magnitude increases and, second, because the values of this variable are not normally distributed. Logging district magnitude ensures the regression model will meet the basic assumptions of linear regression analysis. Finally, following Crisp, Olivella and Potter (2012), I operationalize *democratic age* as a series of dummy variables for voters' experience with (1) one or fewer elections, (2) two elections, (3) three elections, or (4) four elections. This follows from the logic articulated by Singer and Stephenson (2009) and Tavits and Annus (2006) that democratic learning takes place in electorates up through the first five elections. As with the line of argumentation presented in Crisp, Olivella and Potter (2012), operationalizing democratic experience in this way allows us to measure the specific drop off in learning effects after inaugural elections.

The second, party-level model retains these national-level covariates and adds the addition of two important party-level variables: *party size* and *previous vote variance*.¹² So as to avoid endogeneity concerns, *party size* is measured by the party's national-level vote share from the previous election. Similarly, *previous vote variance* is measured as the square of the standard deviation across all of the party's district-level vote shares (for those districts in which it fielded a candidate or party list). To explore the drop-off in institutional effects as parties increase in size, *party size* is interacted with district magnitude, federalism, bicameralism, and presidentialism.

As I have previously mentioned, the structure of the data set is hierarchical in nature: repeated elections – featuring some of the same parties over time – take place under the same

¹²Democratic age in this formulation is not statistically significant. In order to focus on the other – more robust relationships – in the party-level model, I collapse the four dummy variables into a *new democracy* variable that assumes the value of 1 during the country's first five elections and the value of 0 thereafter. In an appendix to the manuscript, I return to a discussion of age and experience at the party level that aims to shed some light on why the findings are disappointing in this respect.

umbrella of country-level covariates. The outcome variable – whether average system-level linkage or individual party-level linkage – is continuous and can assume any value between 0 and 1. Thus a linear model is an acceptable modeling strategy so long as the outcome variable tends toward values away from the absolutes of 0 and 1. More specifically, the system-level model I will be estimating is the following:

$$\begin{aligned} Linkage_{ce} &\sim N(\gamma_c + \mathbf{X}_{ce}\beta, \sigma_{ce}^2) \\ \gamma_c &\sim N(\mathbf{X}_c\xi, \sigma_c^2) \end{aligned}$$

where $Linkage_{ce}$ is the outcome variable. The model includes a country-election battery of covariates, \mathbf{X}_{ce} , that can assume new values for each election (such as democratic age and the average size of electoral districts, should these change in between elections). The model returns a vector β of coefficients for these variables as well as a measure of variance σ_{ce}^2 . The model also includes country-level intercepts modeled by a separate matrix of country-level covariates, \mathbf{X}_c (such as bicameralism and presidentialism). The vector of country-level coefficient estimates ξ captures the effect of \mathbf{X}_c on linkage when \mathbf{X}_{ce} is 0 and variation among these country-level intercepts is captured by σ_c^2 . The party-level regression model, which will be presented secondly, shifts the unit of analysis to $Linkage_{cep}$. This subscript represents the level of linkage exhibited by a specific party during a specific election in a country. In this second model, a new random intercept γ_{ep} is included and is modeled by a matrix \mathbf{X}_{ep} of party-level covariates (such as size) that are all election-specific and the variation among these party-election intercepts is embodied in σ_{ep}^2 .

The regression results for the system-level model are reported in Table 2 below. The first noteworthy result pertains to democratic learning effects, where we see that initially these effects are quite significant, but tail off rather quickly. Relative to more established democracies (in this case, the point of comparison is a democracy in which the electorate has passed through at least five prior elections), party systems in countries with one or fewer

elections of prior experience have a reduced linkage level of 0.09 (recall the scale ranges from 0 to 1). The effect is still significant – but shrinks in size – when countries have two prior elections of experience and this effect continues to shrink substantively (and lose its statistical significance) after this point. Clearly, very small parties are leaving electoral competition quickly, thereby ceding territory to larger parties. This fact drives up linkage as these larger and more successful parties come to dominate the electoral landscape. To say that party systems in more established democracies have an average linkage level 0.09 units higher than democracies in their first or second election is a substantial finding. Indeed, this figure is nearly 1/10 of all districts in a country. In the United States, this difference would equate to most parties entering all of 435 districts instead of entering roughly 390. In Spain, the substantive difference is between entering all of 52 districts instead of entering roughly 45.

The other macro-institutional variables perform in the hypothesized directions. Increasing district magnitude, for example, drives up the average level of linkage in a party system by about 0.06, but this difference will be felt more strongly moving from a single-member district system to a proportional representation system rather than simply increasing magnitude in preexisting multimember districts. By this, I mean that transforming the relationship out of the logged scale, results in a 0.06 increase in linkage when moving from an average magnitude of 1 to about 2.7 or from 2.7 to about 7.4. Thereafter, average magnitude must increase to roughly 20.1 to realize another 0.06 increase in linkage.

Finally, the power centralization variables influence system-level linkage in intuitive ways, although bicameralism falls just short of statistical significance at conventional levels. Relative to unitary systems, federal systems experience 0.14 less linkage. Similarly, relative to unicameral systems, bicameral systems experience 0.07 less linkage. Presidential systems, when compared to parliamentary systems, seem to increase average linkage by a somewhat substantial amount: 0.11. On the whole, then, we can leverage the results of this model to explain rather marked variation in linkage levels across countries. A nascent presidential system, for example, that utilizes single-member districts and has devolved some political

Table 2. Multilevel Linear Regression Model of System-Level Linkage.

| D.V. | Estimate | 95% C.I. |
|-------------------------|-----------------|------------------|
| Experience ≤ 1 | -0.09 | [-0.13, -0.05] |
| Experience = 2 | -0.07 | [-0.12, -0.02] |
| Experience = 3 | -0.04 | [-0.09, 0.01] |
| Experience = 4 | -0.02 | [-0.08, 0.04] |
| Log(District Magnitude) | 0.06 | [0.03, 0.09] |
| Federal System | -0.14 | [-0.24, -0.04] |
| Bicameral System | -0.07 | [-0.17, 0.03] |
| Presidential System | 0.11 | [0.01, 0.21] |
| Intercept | 0.85 | [0.77, 0.93] |
| $\hat{\sigma}_{ce}^2$ | 0.01 | |
| $\hat{\sigma}_c^2$ | 0.02 | |
| N countries | 47 | |
| N observations | 472 | |

power to its regional governments can be expected to have a party-system level of linkage of 0.73 (the positive influences of presidentialism and low district magnitude offsetting the abbreviated levels of democratic experience and diffuse political power). By contrast, an established parliamentary democracy with a bicameral legislature and relatively sizable average district magnitudes has a predicted level of linkage of 0.92.

To the extent that we can equate party-level “linkage” levels with party “size”, these predicted values might seem counterintuitive. After all, we tend to think that presidential systems with restrictive electoral institutions (like the United States) will harbor very few large parties (which are presumably highly linked across districts), while parliamentary countries with more permissive electoral institutions (such as many proportional representation countries in Western Europe) will be characterized by a more fractionalized party system (and again, presumably lower levels of linkage). This somewhat counterintuitive set of results can be explained by shifting the level of analysis to the party level, where we can roughly distinguish “types” of parties based on their size and previous variability in voter support across electoral districts. The results of a model including this additional level of analysis are presented in Table 3 below.

The macro-institutional variables in the party-level model all retain their signs, but many of them fail to clear the conventional threshold of statistical significance.¹³ As *H7* argues, however, this is actually what we should expect to see when we take into account the fact that parties have varying sizes and patterns of electoral support. Indeed, *party size* – or the lagged share of the national vote – is among the strongest performing explanatory variables in this model and its interactions with each of the macro-institutional variables indicate that its qualifying effect on the previous hypotheses is somewhat substantial. On its own, the coefficient on party size indicates that increasing a party’s previous vote share from 5% to 20% should increase its level of linkage by nearly two-thirds of its level in the previous election. In interaction with the macro-institutional variables, we see that as parties increase in size, the

¹³This is also true of a model, not reported here, that omits the interaction terms in Table 3.

Table 3. Multilevel Linear Regression Model of Party-Level Linkage.

| D.V. | Estimate | 95% C.I. |
|-------------------------|-----------------|------------------|
| New Democracy | -0.01 | [-0.03, 0.02] |
| Log(District Magnitude) | 0.07 | [0.04 0.10] |
| Federal System | -0.12 | [-0.26, 0.02] |
| Bicameral System | -0.20 | [-0.32, -0.08] |
| Presidential System | 0.09 | [-0.04, 0.23] |
| Party Size | 0.25 | [0.09, 0.41] |
| Lagged Vote Variance | -0.57 | [-1.41, 0.27] |
| Party Size × Federal | 0.14 | [-0.09, 0.37] |
| Party Size × Bicameral | 0.60 | [0.38, 0.82] |
| Party Size × President | -0.20 | [-0.47, 0.07] |
| Intercept | 0.74 | [0.64, 0.84] |
| $\hat{\sigma}_{cep}^2$ | 0.02 | |
| $\hat{\sigma}_{ep}^2$ | 0.03 | |
| $\hat{\sigma}_c^2$ | 0.02 | |
| N countries | 47 | |
| N parties | 492 | |
| N observations | 1,861 | |

hypothesized effects of these institutions lessen and, eventually, lose their predictive power altogether. While *lagged vote variance* operates in the expected direction (more variance results in a party reducing the number of districts it contests), it falls shy of statistical significance.¹⁴

In an effort to gain some clearer traction on these interactions – and also to directly assess the value of the macro-institutional variables one at a time, rather than side-by-side – Table 4 reports the abbreviated results of four separate repetitions of the party-level hierarchical model. In each case, district magnitude is included as a statistically significant control variable, while democratic age is dropped due to its apparent lack of predictive power alongside these other variables. The table does not report data on each model’s random intercepts and measures of variance in order to highlight the theoretical relationships depicted in each model. The first pane examines federalism and party size; the second, bicameralism; third, presidentialism; and, finally, I model the interaction of party size with district magnitude.

Given the relative dearth of control variables in these models, it is not terribly surprising that virtually all of the coefficient estimates are statistically significant (with the single exception being presidentialism, which is correctly-signed, but insignificant). In general, we see that the interaction term between party size and any of the institutional variables is *working against* the term assigned to the institutional variable itself. For example, federalism and bicameralism both drive down linkage (note their negative coefficients), but these effects are made *less negative* by increasing party size. Similarly, both presidentialism and district magnitude drive up linkage levels (both coefficients being positive), but increasing party size makes these effects *less positive*.

The four panels in Figure 1 draw on the four models’ results to construct the marginal effects and attendant confidence intervals on each of these four macro-institutional predictors.

¹⁴Although in an appendix to this manuscript that repeats this analysis on a subset of more sizable parties, this variable is considerably more statistically robust.

Table 4. Multilevel Linear Regression Models of Separate Interaction Results.

| D.V. | Estimate | 95% C.I. |
|------------------------------------|-----------------|------------------|
| Log(District Magnitude) | 0.07 | [0.04, 0.10] |
| Federal System | -0.22 | [-0.35, -0.09] |
| Party Size | 0.48 | [0.36, 0.60] |
| Party Size \times Federal | 0.45 | [0.25, 0.65] |
| Log(District Magnitude) | 0.07 | [0.06, 0.08] |
| Bicameral System | -0.25 | [-0.36, -0.14] |
| Party Size | 0.20 | [0.05, 0.35] |
| Party Size \times Bicameral | 0.69 | [0.50, 0.88] |
| Log(District Magnitude) | 0.07 | [0.05, 0.09] |
| Presidential System | 0.08 | [-0.06, 0.22] |
| Party Size | 0.66 | [0.56, 0.76] |
| Party Size \times President | -0.32 | [-0.59, -0.05] |
| Log(District Magnitude) | 0.13 | [0.10, 0.16] |
| Party Size | 0.92 | [0.80, 1.04] |
| Party Size \times Log(Magnitude) | -0.28 | [-0.35, -0.21] |
| <i>N</i> countries | 47 | |
| <i>N</i> parties | 492 | |
| <i>N</i> observations | 1,861 | |

Notes: Insignificant control variables have been deleted from the models. Coefficient estimates and confidence intervals reported here; intercepts and measures of variance at each level of the model are omitted to highlight the theoretical relationships of interest.

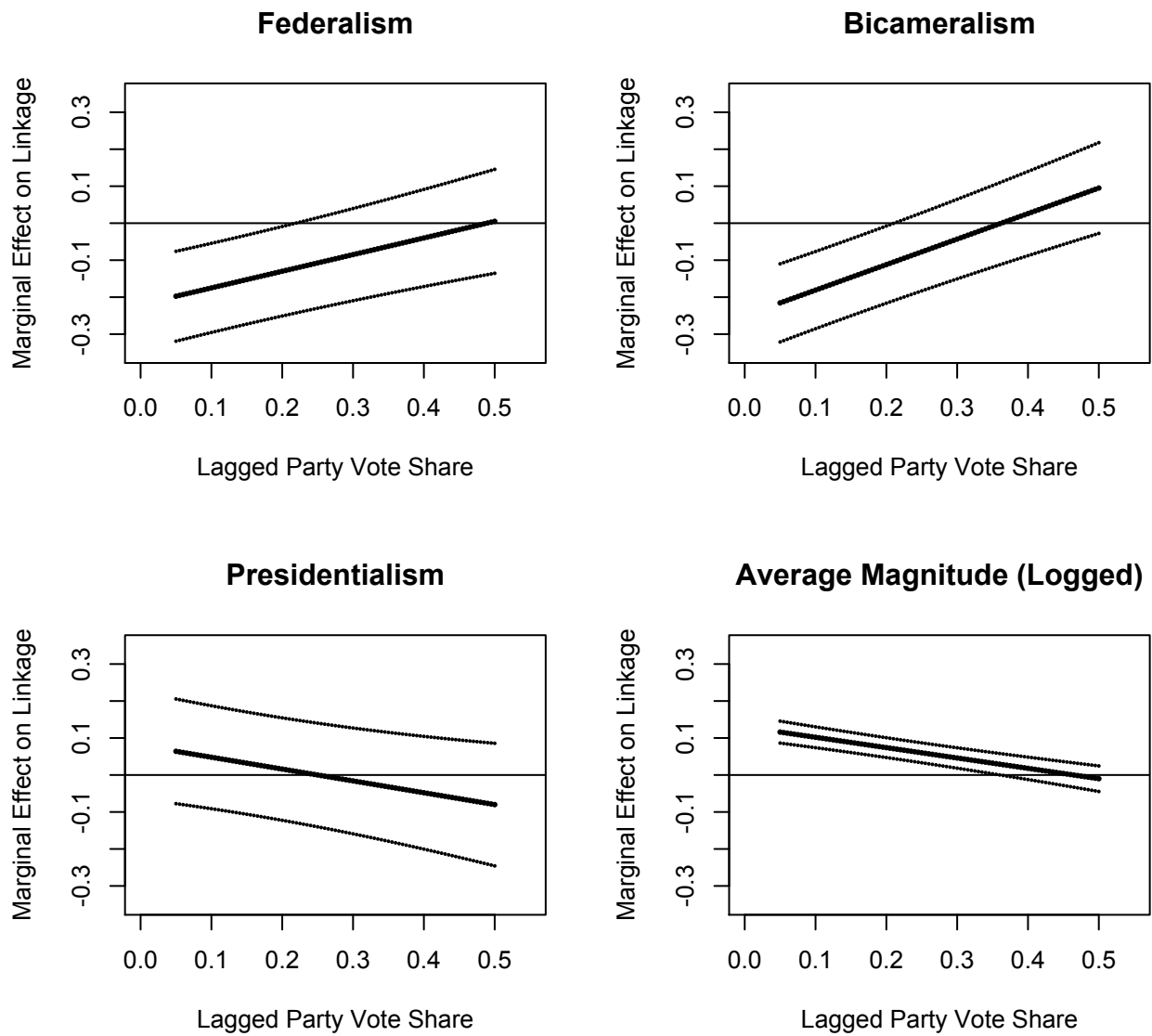


Figure 1. Marginal Effect Plots for Four Institutional Interactions at the Party Level. Effects and confidence intervals are calculated from the separate interaction regression models.

Lagged party vote share (or party size) ranges from about 5% to about 50% along the x -axis in each figure, while the marginal effect of the institution in question – whether positive (above the $y=0$ horizontal line) or negative (below this line) – is represented by y -values. The confidence intervals comprised 95% bands that were drawn from each model’s variance-covariance matrix. Thus, we see that the impact of federalism on party-level linkage is negative and discernible from zero *until such point* that the party netted around 20% of the overall national vote. This 20% cutoff point is virtually the same as that for the bicameralism dummy variable as well. A similar trend is evident in the panel depicting the marginal effect of presidentialism (i.e., presidentialism exerts a positive marginal effect, up to a certain party size), but this effect is not discernible from zero. In the final panel, district magnitude’s positive marginal effect has more staying power: indeed, it is not until a party becomes quite large – obtaining around 35% of the national vote – that its linkage decisions are no longer driven by the overall level of proportionality in the translation of votes into seats. The fact that party size clearly plays an important qualifying role in institutions’ abilities to shape the incentives of parties is an interesting finding. I will return to this point on greater length by way of conclusion.

Conclusion

This study contributes to our understanding of electoral competition and the nature of party systems in several ways. It is the first to take seriously the concept of “linkage” as a purely supply-side, elite-level consideration that temporally precedes the electoral stage at which voters’ evaluate a range of ballot offerings. Thinking about the composition of ballots across districts can lead to theoretically interesting qualifications about party system nationalization, the size of party systems, and the interpretability of electoral outcomes as being fully informative representations of collective decision-making. Indeed, the fact that voters in different districts within the same country can potentially be faced with radically different menus of party offerings on election day presents a difficult challenge to interpreting

electoral outcomes as fair-minded expressions of collective preferences (Austen-Smith and Banks, 1999). When different constituencies are offered incomplete subsets of the broader national set of parties, then vote choices – and their aggregation – may simply not be fully informative measures of a society’s deeper preferences. Whenever a party enters one district, but not another, it inserts a complication for voters when it comes time for them to choose some parties over others or support some parties more broadly than others. Broad-based party support and collective preferences, as concepts, are most informative either when most parties universally enter all districts or – as this is rarely the case – when we can *accurately measure and control for* variation in linkage levels.

The first step in deepening our understanding of electoral outcomes as indications of nationalization and collective preferences is to move away from statistics along the lines of the effective number of parties. Weighting party offerings by vote shares has, in a way, become the *sine qua non* of electoral politics, standing in variously as an indicator of (1) voter coordination, (2) the size of the party system, (3) the stability of the party system, when compared across elections, and (4) linkage in party offerings, when compared across different level of aggregation. In this paper, however, I have made the case that we stand to benefit by partitioning the supply and demand sides of the electoral market. When we begin to think about linkage as the level of homogeneity in ballot offerings (rather than “inflation” between district- and national-level ENP figures and rather than vote-weighted estimates of nationalization in voter support), we can isolate the specific strategic decisions undertaken by political elites and study these in an environment that is not biased by subsequent vote outcomes. Indeed, I have argued that conflating raw party entry figures with vote distributions not only allows for conceptual drift, but also obscures important variation in supply-side dynamics that are deserving of separate investigation.

The value added of this approach is readily apparent from the regression results, where I have offered the first cross-national, multilevel statistical analysis of the determinants of party label linkage. While Cox (1999) offered anecdotal evidence from a subset of countries in

support of his *centralization of political power* arguments, the size and scope of the present analysis is far more comprehensive and offers some insight into why linkage levels at the party level and the broader, system level might differ from one another. In general, I find that voters living in well-established democracies with proportional electoral systems and political institutions that centralize power are bound to be presented with a more-or-less homogeneous ballot on election day, regardless of in which area of the country they reside. The same is not true of newer democracies and of democracies with more diffuse power structures, such as regional parliaments and bicameral legislatures. In these settings, knowing something about where a voter resides within the country should tell you something about the composition of her ballot. More than likely, she must select her legislative representatives from a circumscribed roster of the full field of party offerings.

In some sense, these results are counterintuitive if we suspect that linkage and party size go hand-in-hand (which, empirically speaking, this study has demonstrated that they do). Scholars of electoral politics often look to countries with restrictive electoral institutions (like the United States) as denizens of large-party, high-linkage cases. Conversely, we tend to think of the electorally permissive, parliamentary democracy as the canonical case of deep fractionalization and large (presumably poorer-linked) party systems. This surprising finding can be adjudicated by moving away from my paper's system-level results – where smaller parties are perhaps given an unfair share of attention in the aggregation of linkage – and drilling down to the level of the individual party, where size clearly plays an important qualifying role on these dynamics of centralization, democratic learning, and electoral permissiveness.

Specifically, it seems that larger parties – those netting above, say, 20% of the national vote – have the resources, notoriety, and wherewithal they need to simply field candidates or lists across the vast majority of districts, regardless of the broader institutional incentives. That is to say, those parties *most likely* to gain substantial legislative representation – thereby experiencing firsthand the results of more or less centralized power-making structures – are

the parties that are the most cavalier in the extent of their entry decisions. Rather than worry about the translation of votes into seats or the spoils of office, larger parties seem to prioritize geographic coverage. Indeed, the homogeneity of their ballot offering may, in itself, be a salient electoral strategy: on some level, being able to field your candidates in almost every district around the country is a signal of electoral viability. This is a luxury that is not afforded to smaller parties, who must pay much closer attention to the political realities of their specific institutional environment. The strategic linkage calculations of the Libertarian Party in the United States must look quite different than those of the Green Party in Germany or the Basque National Party in Spain. By contrast, the Democrats in the United States, the Christian Democrats in Germany and the People's Party in Spain are all, no doubt, similarly unconcerned about the distinction between fielding candidates in 90% or 95% of electoral districts throughout their respective countries.

Around the world, the distribution of party-level linkage figures seems to reflect this “grim trigger” breaking point: once a party hits a certain size, it tends to enter uniformly regardless of other considerations. Figure 2 presents three distributions of linkage figures for parties of various sizes. In the first pane are very small parties that netted less than 5% of the national vote; in the middle pane are parties of middling size that netted somewhere between 5% and 20% of the vote; and the right pane shows the distribution of linkage figures for parties netting in excess of 20% of the vote. It is immediately clear that there is not much in the way of middle ground when it comes to linkage. Very small parties tend to enter only a subset of districts. Middling parties begin to make the transition to extensive linkage levels, with only a comparative few trying to hold onto more moderate levels of entry across districts. For large parties, this sparsely-populated area in the middle of the distribution virtually disappears altogether. Ultimately, there exists a systematic and gaping hole in linkage levels across parties of different sizes (even across those parties operating within the confines of the same country).

The story that emerges for voters living in different areas of the same country, then, is one

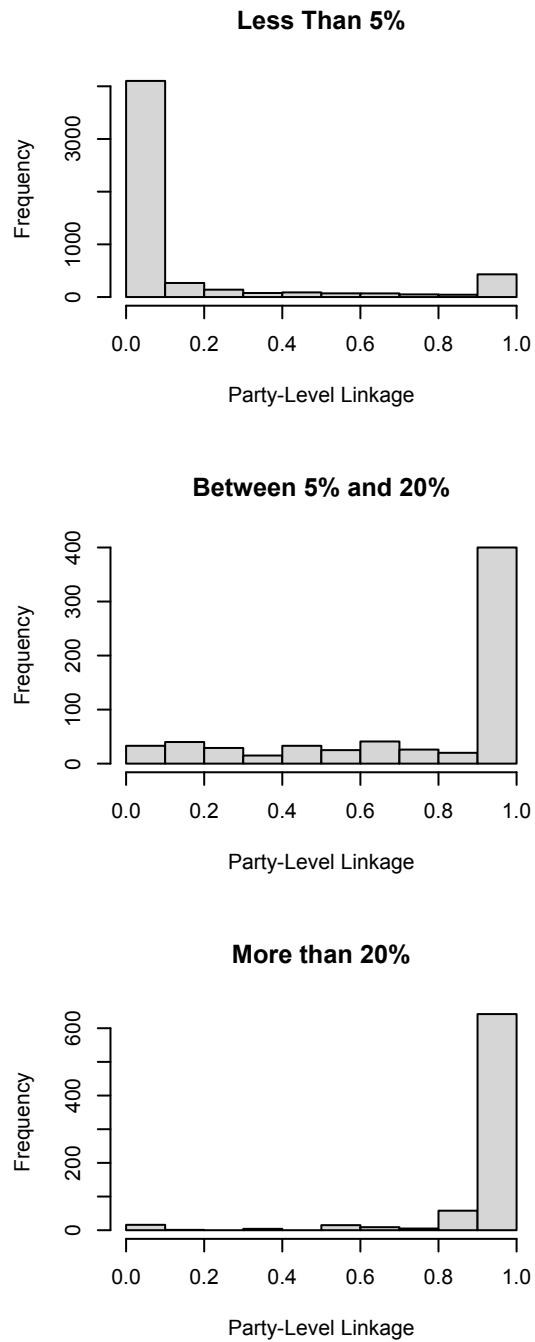


Figure 2. Distribution of Party-Level Linkage Figures for Different Subsets of Party Size (percentage figures refer to lagged national-level vote share).

of ballot variability at the margins. On most ballots, we can expect a core of major, uniform entrants accompanied by a (perhaps larger) roster of fringe offerings. On a strategic level, this outcome is not terribly accommodating of voters; indeed, it demands a lot from them. If the primary tasks for a voter are, first, to decide which party she most closely identifies with and, second, to determine whether or not that party is a viable contender, it seems that there are two ways to facilitate her decision. In the first scenario, ballots are uniform and populated only by truly viable party offerings. Political elites have learned their lessons over the course of multiple elections and either drop out of the race (if they are not viable) or expand their patterns of contestation. All voters easily recognize these offerings and are able to cast ballots for their most-preferred option without running the risk of wasting their votes. In the second scenario, political parties – regardless of their size – recognize that they are stronger in some areas of the country than in others. They cede territory when they know the situation is hopeless. Ballots in this case are no longer uniform, but voters at least know that the offerings on *their* ballot are all plausible options.

But neither scenario is realized in empirical reality. The parties that appear homogeneously across all ballots are at least sometimes bluffing (in the sense that they are ignoring some important incentives crafted by macro-level institutions) and the parties that appear only on a very small subset are unfamiliar to voters (and, by virtue of *not* appearing on other ballots, voters cannot look to election outcomes in other areas of the country to make conjectures about the outcomes of their own race). The tactic taken by political elites seems to be to shift the burden to the voters and let the demand side of the market sort out the supply side. In a sense, this finding presents very few challenges for studies of nationalization in voter support, which have generally relied on the assumption that major parties enter everywhere (the “linkage prerequisite” to which I earlier referred), thereby allowing demand-side vote distributions to be actually reflective of voters’ responses to more-or-less uniform ballots (rather than arising incidentally from dramatic within-country variation in ballot offerings). More problematic, however, are the implications of this finding for treating

the outcomes of elections as fair-minded representations of some underlying collectively-held preference ordering of a country's political parties. With so much demanded of the voter in sorting out the wheat from the chaff, we should hesitate to characterize electoral results as mistake-free embodiments of "what the people wanted." Exploring this nexus between linkage and collective choice in greater depth is a potentially valuable subject for future work.

Appendix A: Party Age and Linkage

In addition to the size of a party (or, perhaps, its resource level), the age of a party ought also to have some effect on the extent of its linkage across electoral districts. This is due to the simple fact that individual parties face different electoral realities and, over time, will necessarily adapt in different ways. A party that enters electoral competition and meets with some initial success, for example, might branch out into additional districts over time. By contrast, a newer party that fails to net much notice upon entering a handful of districts may, over time, winnow down its efforts to a subset of districts before, ultimately, dropping out of electoral competition altogether. Compiling data on party age is difficult in a large, cross-national study such as this one (especially one that is concerned with very small parties in addition to larger, more established parties) and attempts at integrating party age as a variable in the party-level regression analysis repeatedly failed to return significant results. However, in this appendix, I will briefly take up the issue in an effort to explain how individual party-level ages can be considered in the aggregate to explain why we see increasing linkage at the system-level over time.

For those countries in the data set for which I have a temporal series of election results that begin with the inaugural democratic election (as determined by the Database of Political Institutions), I can gain some traction on party age without having to delve deep into the contextual specifics of individual countries. Specifically, parties that appeared with some district-level vote total (no matter how small) in this inaugural election begin with an *age* = 1 counter and those that did not are simply coded with missing values. Whenever a party received a vote total in subsequent elections *without* having received a vote total in the prior election, that party was at that point similarly coded with an *age* = 1 counter. For subsequent elections in which these parties garnered some vote, the counter was increased by one unit. Once (if) the party failed to subsequently receive any votes in any districts (this serving as a proxy for its departure from electoral competition), it again assumed missing values. For those countries in the data set for which I did *not* possess vote totals

for the inaugural democratic election, I was unable to code for party age; indeed, without the inaugural benchmark, I cannot be sure what any given party's age might be unless I examined other sources of (context-rich and country-specific) data.

The temporal data coverage was such that I had uninterrupted data for the first (and subsequent) elections for 26 (or little more than half) of the countries. These include a diverse set of both established and newer democracies, single-member district and multimember district electoral systems, as well as presidential and parliamentary systems. In the figure below, I plot party age (for the first 10 elections) along the x -axis and party-level linkage levels along the y -axis. As can be seen fairly readily from the graphic, the first election in which parties compete (regardless of the age of the party system more generally) shows substantial variation in linkage. Over the course of the second, third, and fourth elections, however, a dramatic sorting effect emerges where parties either enter systematically more or fewer districts (with the middle ground clearing out substantially). As we move into significantly older parties, it is apparent that entering few districts is an unsustainable strategy; indeed, the clump of low-linkage observations that pop up in the second, third, and fourth elections have almost dissipated entirely at older ages.

Despite the visual trends, when it comes to the party-level hierarchical regression models in this manuscript, including party age additively in the model fails to return any significant findings (as does interacting party age with lagged national vote share to proxy for “previous success” or “resource endowments”). This is probably due to the fact that there is less statistical leverage to begin with (recall that we drop many parties from countries where I am unable to code for party age). The effect of party age may also simply get overwhelmed by the other variables that clearly play an important role in determining linkage levels.

At the system-level, however, the scatterplot manages to empirically underscore a point that was argued theoretically: namely, that linkage should increase over time as non-viable parties become better acquainted with their fates and drop out of electoral competition. Obviously, there is more nuance at play (for these observations, party age and system age

are only correlated with one another at $r = 0.6$), but the general trend makes conceptual sense. Linkage is positively correlated with past performance (which I've taken as a proxy for resource endowments), current performance (which I measure as national-level vote share), and longevity. Other things being equal and if one has the resources, offering one's platform across as many districts as possible is clearly a strategic best response.

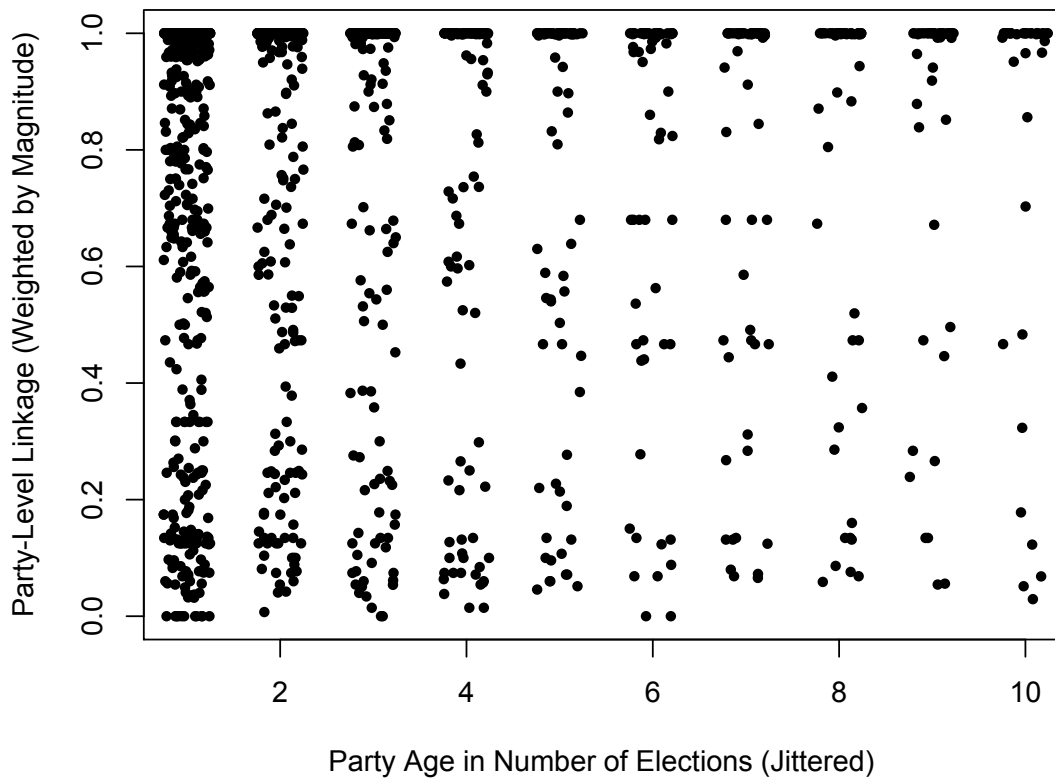


Figure A1: Scatterplot of Party-Level Linkage (Weighted by District Magnitude) Plotted Against Party Age (in Number of Elections). Observations have been jittered slightly along the x -axis to better illustrate areas of density.

Appendix B: System-Level Model Robustness Check

In this appendix, I present the results of a model that is identical to that used to model system-level average linkage, but – instead of dropping those parties netting 1% or less of the national vote before averaging linkage figures – I here drop those parties netting 5% or less of the national vote before averaging linkage across parties. The results hold up well. In particular, we can see that nascent party systems are still subject to systematically lower levels of linkage. Increasing district magnitude drives up linkage and so does presidentialism. Bicameralism fails again to achieve statistical significance.

Table B1: Multilevel Linear Regression Model of System-Level Linkage.

| D.V. | Estimate | 95% C.I. |
|-------------------------|-----------------|------------------|
| Experience ≤ 1 | -0.07 | [-0.10, -0.04] |
| Experience = 2 | -0.03 | [-0.07, 0.00] |
| Experience = 3 | -0.03 | [-0.06, 0.01] |
| Experience = 4 | -0.04 | [-0.07, -0.01] |
| Log(District Magnitude) | 0.03 | [0.01, 0.05] |
| Federal System | -0.13 | [-0.20, -0.06] |
| Bicameral System | -0.02 | [-0.08, 0.04] |
| Presidential System | 0.08 | [0.02, 0.14] |
| Intercept | 0.94 | [0.89, 0.99] |
| $\hat{\sigma}_{ce}^2$ | 0.01 | |
| $\hat{\sigma}_c^2$ | 0.02 | |
| N countries | 47 | |
| N observations | 472 | |

Notes: The outcome variable was constructed as a system-level average across all parties *after having dropped* those parties that failed to gain at least 5% of the national vote.

Appendix C: Party-Level Model Robustness Check

In this appendix, I present the results of a model that is identical to that used to model party-level linkage, but – instead of dropping those parties netting 1% or less of the national vote – I drop those parties netting 5% or less of the vote. The results generally hold up well, with variance in previous district-level vote shares emerging as a very strong predictor for this subset of larger parties.

Table C1: Multilevel Linear Regression Model of Party-Level Linkage.

| D.V. | Estimate | 95% C.I. |
|-------------------------|----------|------------------|
| New Democracy | -0.01 | [-0.03, 0.02] |
| Log(District Magnitude) | 0.05 | [0.02, 0.07] |
| Federal System | -0.13 | [-0.26, 0.00] |
| Bicameral System | -0.07 | [-0.18, 0.04] |
| Presidential System | 0.07 | [-0.06, 0.20] |
| Party Size | 0.18 | [0.03, 0.34] |
| Lagged Vote Variance | -1.74 | [-2.61, -0.88] |
| Party Size × Federal | 0.16 | [-0.10, 0.41] |
| Party Size × Bicameral | 0.30 | [0.08, 0.53] |
| Party Size × President | -0.17 | [-0.46, 0.12] |
| Intercept | 0.82 | [0.73, 0.91] |
| $\hat{\sigma}_{cep}^2$ | 0.02 | |
| $\hat{\sigma}_{ep}^2$ | 0.01 | |
| $\hat{\sigma}_c^2$ | 0.02 | |
| N countries | 47 | |
| N parties | 492 | |
| N observations | 1,861 | |

Notes: The outcome variable is linkage at the individual party level, but those parties that failed to net at least 5% of the national vote are excluded from the data set.

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