THE VALUE OF POLITICAL GEOGRAPHY: EVIDENCE FROM THE REDISTRICTING OF FIRMS

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Abstract
We demonstrate that political geography has value to firms. We do so by exploiting shocks to political maps that occur around redistricting cycles in the United States. These shocks keep some firms in Congressional districts that are largely unchanged at one extreme and reassign other firms to largely different sets of constituents at the other extreme. Our main finding is that firms suffer from being reassigned into districts that are competitive across parties relative to safer districts. The effects are not trivial in magnitude. Moreover, they do not depend on whether firms retain the same politician or make campaign contributions.
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1 INTRODUCTION

Relationships with politicians are valuable for firms (e.g., Roberts 1990; Fisman 2001). Furthermore, politicians are constrained by the characteristics of their districts (e.g., Friedman and Holden 2009; Stratmann 2000). Yet, no one has linked characteristics of political districts to firm value, despite the role of districts' attributes in determining which politicians get elected and what those politicians can do for firms. We link these empirically, establishing that political geography has value to firms, notwithstanding other factors that shape the political economy of business-government relations.

Our empirical strategy takes advantage of the redistricting process in the United States. Every 10 years, transformed political geographies emerge from a constitutionally mandated reapportionment process, which has been studied intensively (summarized in La Raja 2009 and McGhee 2020), yet not previously linked to firms. Hence, our identification strategy exploits a natural experiment in which some firm headquarters are reassigned to districts with different political characteristics even though the firms themselves do not move. We operationalize this in a financial-market event study around disparate announcements of new electoral maps for the US House of Representatives for the 43 states that experienced changes in district boundaries during the 2010 redistricting cycle.

Consider the case of a pair of identical firms. *Firm X* resides in a district where constituents belong equally to both major parties, but following redistricting is reassigned to one dominated by voters in a single party. *Firm Y* faces the opposite fate, as it is reassigned from a district dominated by voters belonging to a single party to one split equally. How would *Firm X* perform relative to *Firm Y*? We find that redistricting benefits firms reassigned to safer, heavily partisan districts regardless of party (i.e. *Firm X)*; meanwhile firms reassigned to competitive, partisan balanced,

districts (i.e. *Firm Y*) suffer. These results are robust to falsification tests, competing hypotheses, and alternative measures. As we explain below, the value of political geography is distinct both conceptually and empirically from political connections and money-in-politics.[1]

The effects are economically meaningful. Even conservatively estimated, their magnitudes are large: the average shock to firm valuations exceeds 1.5% and reaches up to 2.9% among certain sub-populations. Moreover, they are common, as nearly 25% of publicly traded firms experienced a transition in the partisan nature of their congressional districts in the 2010 cycle. Hence, billions of dollars are capitalized into markets based on political lines drawn around firms.

Why exactly should political geography affect firm value? Shocks to politician's election prospects change the expected duration of both existing and new politician-firm relationships based on geographic ties. For example, increased electoral competition shortens the expected duration of a politician-firm relationship since incumbents are more likely to lose races and retire at higher rates in more competitive districts (Friedman and Holden 2009). Moreover, Snyder (1992) shows that firms make long-term investments in politicians; since these pay off more when relationships are longer-lived, we should expect firms' valuations to fall when the expected duration of such relationships is shorter. Shocks to politicians' incentives in office, via changes in electoral competition in their districts, also change the constraints on the politician side of

[1] There are large literatures on political connections, lobbying dollars, and campaign contribution dollars, respectively. Faccio (2006) demonstrates how pervasive political connections are globally, while the value of political connections is established in event studies of reported deaths of politicians in Roberts (1990), Fisman (2001), Faccio and Parsley (2009), Fisman et al. (2012), among others. De Figueiredo & Richter (2014) provide a recent review on the empirical lobbying literature. Snyder (1990) was one of the first to empirically analyze campaign contributions as investments. Ansolabehere et al. (2003) review the early campaign contributions literature. Interest in the topic has broadened recently, examining the contributions of individual executives rather than political action committees (see Fremeth et. al 2013, Richter & Werner 2017, or Fremeth et. al 2018) or the consequences of the *Citizens United* supreme court decision (e.g., Klumpp et al. 2016 or Werner & Coleman 2014).

politician-firm relationships. Legislators subject to redistricting change their Congressional voting behavior in-line with altered constituency demands (Stratmann 2000). Increased electoral competition also changes politicians' use of scarce time (Hall and Deardorff 2006), incentivizing them to focus on electioneering rather than firms' policy interests (Daley and Snowberg 2011).[2] Finally, differences in district competitiveness imply differences in the uncertainty over the range of politicians a district may elect.[3] In other words, political geography affects the nature and viability of the relational contracts that firms and politicians form.

Naturally, if firms value certain aspects of political geography, we should expect them to act on these preferences and demand them. In California, for example, specialized consultants like Redistricting Partners lobby in favor of specific boundaries through public testimony in front of the Citizens Redistricting Commission.[4] In Florida, several firms rank high among donors to the "Protect Your Vote" PAC formed in opposition to two redistricting-related ballot propositions in 2010 that analysts forecast would have a lead to a major reshuffling of boundaries.[5] One of the involved firms, Honeywell Corporation, acknowledged that its donation stemmed from a preference for "redistricting that is consistent with the historical practices"—a sentiment echoed by another contributor, CSX International, "as a Florida-based corporation."[6]

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[2] Dropp and Peskowitz (2012) document that electoral security increases the number of bills legislators author, while electoral insecurity increases their response rate to requests from voters residing in the district for constituent services. Erikson and Palfrey (2000) show that campaigns spend more in close/competitive races than non-competitive ones—which perhaps suggests a greater need to fundraise limiting a politician's time to serve firms.

[3] Uncertainty over politician type is distinct from policy uncertainty (e.g., Baker et al. 2016, Julio & Yook 2012), stemming from the fact that individual legislators have a limited impact on which laws get passed.

[4] See FiveThirtyEight Politics "The Gerrymandering Project" (http://fivethirtyeight.com/tag/the-gerrymandering-project/) podcast episode on California for more on this example. See also the firm's website at http://redistrictingpartners.com/.

[5] See https://ballotpedia.org/Florida_Legislative_District_Boundaries,_Amendment_5_(2010) .

[6] See https://www.propublica.org/article/hidden-hands-in-redistricting-corporations-special-interests .

Efforts by firms to influence redistricting challenge our ability to reject a null effect (i.e., achieve statistical significance), but do not invalidate our identification of the existence of a value to political geography. Put simply, our identification assumption for the *existence of an effect* of political geography relies on *some element of* new redistricting map announcements being unexpected to market participants. That assumption remains plausible even in the presence of some firms lobbying for preferred outcomes in the redistricting process, particularly if other firms experience as-if random reassignment to new districts by failing to lobby. Moreover, even among firms that do lobby for specific outcomes, there are surprises because some influence campaigns fail, like those in Florida mentioned above. Hence, some element of surprise or uncertainty will remain unresolved until the announcement of final revised maps in practice. That said, to precisely identify the *size of the effect* of political geography on the average firms' returns (rather than merely the existence of an effect as we do), we would need to rely on a stronger and more implausible assumption: that *all elements* of new map announcements are not expected by market participants. If at least some elements of the announcement are foreseen, this would push coefficient estimates of the average effect on all firms towards zero, as market participants have already priced in such effects prior to announcements. We should therefore take our estimates as a lower bound of the causal effect of political geography on the average firm's valuation.

2 INSTITUTIONAL DETAILS ON REDISTRICTING PROCESS

Article 1, Section 2, Clause 3 of the U.S. Constitution requires that electoral districts for the U.S. House of Representatives are reapportioned every 10 years. The Reapportionment Act of 1929 fixed the total number of seats in the House of Representatives at 435 and further defined the rules used today for determining the allocation of seats across states.

In the last two cycles, seven states received only a single member of the House of

Representatives given small populations. Since these states only had one district both before and after reapportionment, their populations were too small to conduct redistricting. The remaining 43 states must redistrict to ensure that each district contains approximately the same number of voters, meaning that district boundaries change even in states that keep the same number of seats, as long as there are population shifts within states[7]

While the federal government retains control over the reapportionment process (allocating the number of seats to each state), each state government retains control over the redistricting process (control over how exactly to draw district lines for congressional districts within its boundaries). Hence, each state has a distinct set of institutional rules for drawing the lines, ranging from independent commissions to state legislatures, sometimes with governor involvement as well (for an overview, see Justin Levitt and Doug Spencer's "All About Redistricting" guide—https://redistricting.lls.edu/). Since at least 1812, when Massachusetts Governor Elbridge Gerry developed what would become known as the gerrymander, that discretion generated creatively shaped districts and large swings in some political maps.

Despite leaving states discretion over the institutional arrangement used to draw lines, the federal government prescribes deadlines for states to complete redistricting. These timeframes are contingent on the state-specific delivery date of detailed federal census data on residents' locations to each state government, which they must use as the basis for creating new districts. Hence, each state produces and announces its new Congressional district map on a different timeline.

In several cases, particularly in states where independent commissions or courts draw the lines, there is only one potential map that enters the public domain, which is almost certain to

[7] The average Congressional district now contains 710,767 constituents, although there is some variance both within and across states in the district sizes given states and precincts cannot be divided.

become the final map adopted at the time it is announced. In other states, where factions of parties want different outcomes or where parties bargain with each other over outcomes, several potential electoral maps tend to enter the public domain with each having different likelihoods of becoming the actual new district boundaries depending upon how legislative bargains are struck and how public different political actors want to make their positions. In these latter cases, various maps are debated and eventually a final map is announced and ratified. Our empirical strategy hinges on when the final maps enter the public domain.

3 EVENT STUDY RESEARCH DESIGN APPLIED TO REDISTRICTING

We employ a financial-market event-study research design (MacKinlay 1997; Kothari and Warner 2007), commonly used in the firms and politics literature to establish causality of headline political events such as politician deaths or party-switches (Roberts 1990; Fisman 2001; de Figueiredo and Richter 2014; Milyo 2014). We first construct a measure of the cumulative abnormal returns (CARs) firms experience following announcements of new U.S. House of Representatives electoral maps throughout the 2010 redistricting cycle. These represent the difference in how the market values each firm given redistricting occurred (along some dimension) against the counterfactual scenario that it did not. The market should only react if participants believe announcements about changes in political geography will impact real-world firm performance and the information embedded in new electoral maps has consequences for it. Otherwise, the CAR will be zero. Given that they reflect expectations of future performance following redistricting, regressing CARs on various measures of changes to firms' political geography should reveal which

aspects of political geography cause some firms to perform better and others worse.[8]

For event study approaches to generate reliable insights we need to make assumptions about the efficiency of markets in processing information (Fama 1970). In the fully efficient case, market participants react instantaneously and simultaneously to the arrival of new information with perfect foresight as to what the implications are for firms. Highly efficient markets allow for short event windows, alleviating concerns about other concomitant events explaining any abnormal returns, particularly in studies with a single firm or a single event date.

Many focal events studied using event study methodology in the literature on firms and politics generate clear, easily digestible implications for firms: e.g. when Senator Henry "Scoop" Jackson unexpectedly dies, it is undeniable that the event instantaneously severs political benefits firms may have received from a man no longer able to dole them out (Roberts 1990). The same logic holds for firms connected to President Suharto in Indonesia had rumors of his death been true (Fisman 2001); or, when Senator James Jeffords' unexpected switch of party affiliation tips the majority in Congress (Jayachandran 2006) and hence firms aligned with the prior majority party enjoy fewer benefits.

Moreover, given the rarity and stakes of the death of a sitting President or a party switch by a sitting Senator, these events made "above the fold" headlines and were widely discussed.

[8] Several papers use similar research designs to study how redistricting impacts voter behavior rather than firm outcomes, including Hayes and McKee (2009), Henderson et. al (2016), and Hunt (2018). Henderson et al. (2016) highlight the importance of going beyond cross-sectional comparisons of post-redistricting outcomes to overcome selection bias. E.g., in the voter behavior context, a difference-in-difference specification is preferable to a cross-sectional approach analyzing only redistricted voters since post-redistricting differences in voter turnout may be driven by the intentional packing of voters with a higher/lower propensity to vote into specific districts rather than characteristics of those districts. Our CAR approach offers similar advantages to a difference-in-differences approach since each firm's CAR represents actual firm financial performance net of counterfactual firm performance without redistricting by construction.

Hence, market participants likely received the relevant information in an expedient manner, and we would expect them to respond quickly. The combination of easily digestible focal events and quick and complete transmission of information allows for CARs to be constructed over short post-event windows in these studies: 1-4 days depending upon the specific health rumor in the Suharto study and 5 days for the "Jeffords effect" study.

In our event study of the 2010 redistricting cycle, by contrast, we have 43 event dates rather than a single one, as every state with more than one congressional district announced new maps.[9] The heterogeneity in announcement dates means that in constructing abnormal returns in event-time we are actually collapsing the data across a period that spans from the announcement of a new map that would credibly be adopted in Florida on February 2, 2011 until the announcement of a new map that would credibly be adopted in New Hampshire on March 22, 2012. These are put on a common scale, where the announcement date for each state is set to 0 in event-time.

The announcement of a new electoral map has less immediate implications for firms than a cut-and-dry story about the death of a politician. Investors that want to benefit from turning information about new maps into profitable trades need to understand the exact course of the new boundaries, their implications for the demographic makeup of districts, and combine this information with knowledge of the geographic locations of firms. To facilitate this process, several major investors have started employing so-called "political intelligence" teams and consultants (Mullins and Pulliam 2011). Nevertheless, there remains substantial heterogeneity in how well different market participants can access and process political information and assess how it is relevant to firms in which they might invest (Gao and Huang 2016; Christensen et al. 2016).

Ultimately, how long it takes for markets to respond to announcements about redistricting

[9] In the online appendix, we perform a placebo test using the seven states that did not redistrict.

is an empirical question. Given the complexity of the information contained in a new electoral map, our baseline model uses a slightly longer event window than the typical event study in our preferred specifications. The event window we choose starts one day before the event date, to account for information leakages (as is standard in the literature) and runs until seven market days after the event date. The main risk in selecting a longer event window is that we may instead be picking up the effects of some concomitant event. This is, however, unlikely to be a major problem in our case given that we have 43 different event dates.[10] To ensure our results do not rest solely on the specific window we choose, we present robustness checks with alternative event windows. The results remain congruent with our main findings in terms of their economic significance. Moreover, they suggest that information about changes in firm-specific political geography may diffuse into the market somewhat slowly as it takes several days for the markets to react fully.[11]

4 DATA AND SUMMARY STATISTICS

For the purposes of this study, the sample of firms we focus on must be limited to publicly traded firms where we can observe market reactions. We also need address data on these firms' headquarters. Hence, our sample is limited to those firms included in both CRSP (for market data) and COMPUSTAT (for address data) that were located in states where redistricting occurred. The timeframe we examine is that around the 2010 redistricting cycle.[12]

[10] Hence, even if there was a concomitant event in one state it would only affect a subset of firms and might be offset by other events having the opposite effect in other states. Moreover, for such an effect to bias our results it would have to be contingent on political geography changing, not merely something that generates higher abnormal returns for all firms in a state (which would be captured by the state fixed-effect in our estimation).

[11] Tests for the speed of information diffusion appear in Figure 3 and Table 6.

[12] Ideally, we would have included other redistricting cycles as well; however, we face measurement

4.1 Calculating Cumulative Abnormal Returns

Our key outcome variable throughout our analysis is CARs representing the difference between actual firm market performance and counterfactual firm market performance in a world without redistricting. We calculated these as is standard in the literature by generating counterfactual firm performance using a market model. We use a Fama-French 3-Factor model with a 250-trading day pre-event estimation window. We aggregate the difference between actual and counterfactual returns using an event window starting 1 trading day before the relevant event date and running 7 trading days after it. It is worth pointing out that, by construction, mean pre-treatment CARs for any treated and untreated groups are zero in a financial-market event study (which need not be the case in a difference-in-difference type event study).

Determining a specific event date for when a credibly enacted new electoral map enters the public domain is not as straightforward as demarking the timing of politician deaths or press conference announcing a politicians' decision to leave a party. We used Justin Levitt's "All About Redistricting" guide as the starting point for determining event dates.[13] The focal dates in Levitt's work are the formal dates that maps are officially adopted based on legislative histories; key for our analysis, however, are the dates on which credible maps first enter the public domain. For example, in a state where an independent commission draws the lines, analysts can be nearly certain upon introduction of new maps that they will be adopted. Alternatively, things are less clear cut in a state where the legislature debates several maps. However, even then there typically

challenges if we go back further. In particular, we would have a difficult time selecting appropriate event dates for earlier redistricting cycles. The public records for even the 2000 cycle are less widely published on state house or redistricting commission websites as sharing government information online was less common at that point in time.

[13] See these files here: http://redistricting.lls.edu/2010districts.php

emerges some breaking point in the process where a concession is made between parties and adoption of the map in question becomes a near certainty. Hence, getting focal dates for the announcement of credible new electoral maps in each state requires looking back at the weeks before the formal adoption of a map to determine when the first public news stories and copies of maps detailed enough for the public to locate specific addresses on them emerged. We hand-coded these dates using legislative archives, redistricting commission releases, and media databases. Appendix A of the online material shows the exact event dates we use.

We believe we coded these dates accurately, but mistakes cannot be ruled out entirely as our choices sometimes rest on informed judgements, rather than hard facts. The effect of event miscoding is a bias towards zero in the estimated CARs. The logic is simple: if nothing happened on our selected date, then the sources of heterogeneity in political geography have no reason to be correlated with abnormal firm performance. Alternatively, if a potential map was announced but not judged to be final, then the market response would be accordingly dampened. In other words, this type of mismeasurement would make our estimates conservative.[14]

4.2 Measuring Redistricting

All firms located in states where redistricting occurred are subject to some level of change in the political geographies in which they are situated. These changes can be small—such as moving a boundary one block to incorporate a few additional voters—at one extreme. Or these changes can be large—such as moving boundaries in a way that produces a wild swing in the geographic and/or partisan make-up of the constituency of a firm—at the other extreme. A challenge then arises to

[14] As part of the sensitivity analysis, we also tested for systematic effects following the official release of the final maps (using Levitt and Spencer's dates). We did not find any effects, consistent with the idea that markets had already reacted to the information contained in the maps.

construct measures of changes in political geography that are meaningful. First, we focus on physical aspects of the political geography and turn to partisan aspects next.

As a baseline for constructing both measures, we employ geographic information systems (GIS) software to locate firms in districts before and after the 2010 redistricting cycle. We pull shapefiles defining the official borders of Congressional Districts from the Census Bureau's Master Address File/Topologically Integrated Geographic Encoding and Referencing (MAF/TIGER) database.[15] We take firms' headquarters addresses from COMPUSTAT.[16] This allows us to assign firms to political districts before and after redistricting based on their headquarters locations.[17] From this basis, we go on to construct measures of discrete changes in attributes of the firms' political geography post-redistricting relative to pre-redistricting.

Variation in Surface Changes to Firms' Districts

One way to quantify changes in political geography around redistricting is to examine changes in the surface area of a district. Doing so yields a measure of physical change for the district in which a firms' headquarters resides pre- and post- redistricting, where 0% would indicate that the district

[15] For more information, see: https://www.census.gov/geo/maps-data/data/tiger-cart-boundary.html

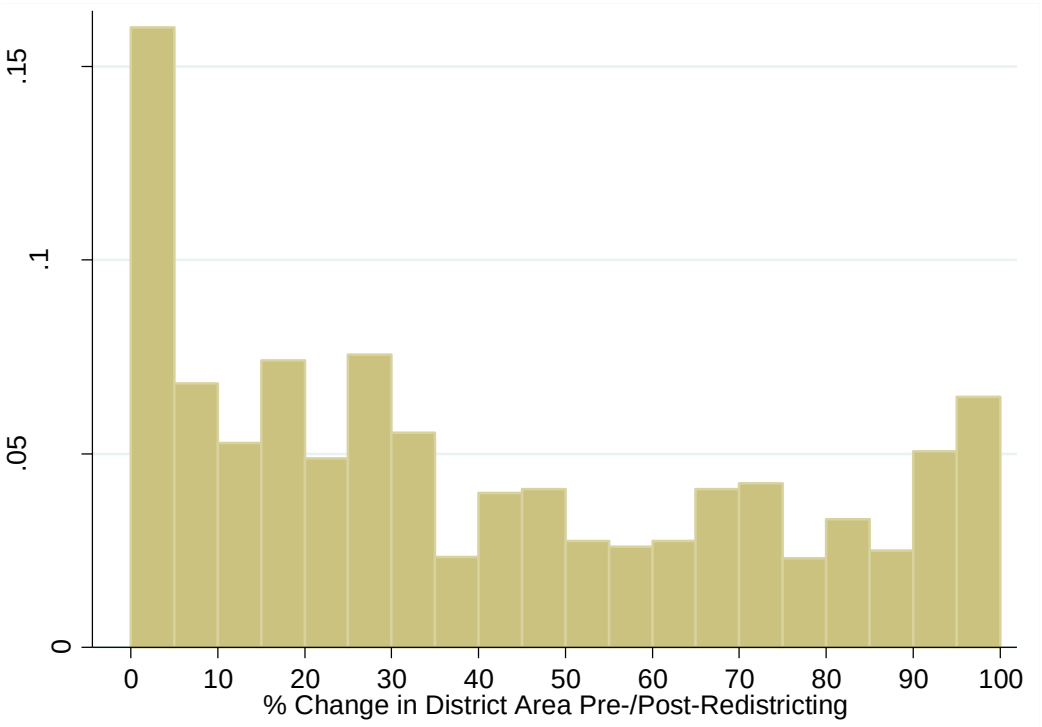
[16] COMPUSTAT lists current headquarter addresses, not historical addresses. We used the COMPUSTAT 2014 version of the database. Because corporate headquarters move infrequently, most headquarters will be properly located; furthermore, the slippage between historical and measured addresses should mostly induce attenuation bias.

[17] One potential problem with defining firms' political districts as being located where firms' headquarters are is that large firms, like many of the ones in our sample, typically have operations in multiple locations and across multiple states. This dispersion would be a problem for trying to assign the entire effect of redistricting solely to the headquarters district when different parts of firms' operations are redistricted simultaneously in different ways. Fortunately, in our research design, different states announce redistricting maps at different times, meaning the redistricting that happens in different states would not contaminate the effects of the headquarters district changing, unless the event windows around redistricting in other states overlapped. To address this concern, Table 7 presents robustness tests related to the geographic dispersion of firms; we do not find that dispersed activities threaten our main finding.

in which a firm resides covers the exact same area before and after redistricting. Conversely, values approaching 100% would indicate that a firm is located in a district with almost no surface overlap with the prior district.

Figure 1 presents a histogram of this measure of the change in surface area of firms' headquarters' congressional districts around the 2010 redistricting cycle. Roughly 40% of firms experience a relatively small change in the area of the district in which their headquarters reside—such that at most 25% of the district covers new physical space. A reasonable number of firms find themselves at the other extreme, however, with nearly 20% of firms seeing a change in the surface area of their districts greater than 75%. If retaining the same physical boundaries of political geography matters, these are the firms where we should expect to see the biggest shock. Overall, Figure 1 indicates that, at least in terms of surface area, the amount of re-shuffling of political geography is both varied and substantial.

Figure 1 – Histogram of fraction of change in geographic overlap between area in new congressional districts relative to old ones, by headquarters location of firms



Variation in Substantive Changes to Firms' Districts

Of course, the change in the physical area of a given political geography or district does not represent the only possible measure of change. Plausibly more important is the partisan composition of constituents within a district (Hill 2004). There are at least two dimensions to consider: (i) the party that most voters belong to and (ii) the degree of balance between the numbers of voters attached to each party.

We use *Daily Kos* data on the partisan make-up of political districts.[18] These data aggregate precinct-level partisan voting records of pre-redistricting constituents placed into post-redistricting districts as a measure of the partisan composition of the new districts *ex-ante,* using results from the prior presidential election in the projections. This is the exact type of information those conducting redistricting use.

Table 1 summarizes changes in the partisan composition of firms' headquarters' districts by presenting a transition matrix showing what fraction of firms change from one type of partisan district to another. Districts are divided into four types: Safe Democrat, Leans Democrat, Leans Republican, and Safe Republican. Firms are defined as being located in a district belonging to a given party if more than 50% of voters align with that party. Conservatively, we define a district as safe if one party has greater than 55% of the expected vote share; we define a district as competitive (labeled "leans" in Table 1) if projected vote shares for both major parties fall within the 45% to 55% interval.[19]

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[18] See https://www.dailykos.com/stories/2012/11/19/1163009/-Daily-Kos-Elections-presidential-results-by-congressional-district-for-the-2012-2008-elections .

[19] In robustness checks in Online Appendix B, we present alternative cut-points for the intervals used to define safe and competitive districts; the substantive results carry through.

Table 1 – Transition Matrix for Partisan Composition of District before/after Redistricting

Before Redistricting		*After Redistricting*				
		Safe Democrat	Leans Democrat	Leans Republican	Safe Republican	
	Safe Democrat	49.8%	3.5%	1.2%	1.6%	***56.0%***
	Leans Democrat	2.0%	5.2%	3.0%	0.4%	***10.6%***
	Leans Republican	3.5%	2.7%	4.7%	2.1%	***13.0%***
	Safe Republican	0.9%	0.1%	2.5%	16.8%	***20.3%***
		56.2%	***11.6%***	***11.4%***	***20.9%***	***100.0%***

Notes:
"Safe" is defined as having greater than 55% of the expected vote share for a given party.
"Leans" is defined as having greater than 50% of the expected vote share for a given party.

Despite how common re-shuffling of physical aspects of political geography is, Table 1 shows that approximately three-quarters of firms remain in a district where both aspects of the partisan make-up—its party-alignment and projected competitiveness—go unchanged. This implies that in our attempts to determine whether there is value to partisan aspects of political geography we will exploit the variation in the one-quarter of firms that do experience swings.

In particular, we will focus on changes in (i) party—via reassignment to Republican or Democratic districts—and (ii) partisan balance—via reassignment to safe or competitive districts. We create distinct dummy variables for each of these four attributes. 6.2% of firms are reassigned to Republican districts from Democratic districts, while 7.2% of firms are reassigned to Democratic districts from Republican districts in our sample focused on the 2010 redistricting cycle. 10.5% of firms' districts transition via reassignment to a safe district after redistricting (including 2.5% switching from one party being dominant to a different party being dominant), while 7.3% of firms' district transition from safe to competitive. Hence, we observe a variety of

changes in the partisan nature of firms' districts.

4.3 Other Variables

We round out our analysis by considering other aspects of firm-politician relationships. To do so, we consider whether firms retain the same politician before/after redistricting and whether firms had a political action committee (PAC).

We construct a dummy variable for whether firms retain the same politician at the next election relative to before redistricting using election returns data. This variable captures instances of firms being shifted to a district with a different incumbent, as well as cases where the incumbent initially stays the same but is not re-elected. While the latter cases may be driven by the partisan makeup of the district changing to the disadvantage of the incumbent and thus also a consequence of redistricting, it is true that market participants have not yet observed this outcome at the time new maps are published. Nevertheless, the variable is a useful proxy for market perceptions of firms that have a high probability of having the same politician win their redrawn districts.

We also construct a measure of political activity, creating a dummy variable for whether a firm had a PAC in the election cycle prior to redistricting, using Federal Elections Commission (FEC) data cleaned by the Center for Responsive Politics. This variable enables us to address potential concerns that more politically active firms may either influence how lines are drawn around them or were selected into certain districts given potential dollars at stake.

5 RESULTS

Our research design to identify the value of political geography regresses our measures of meaningful redistricting against CARs. We find that the measures of redistricting we construct systematically explain the returns, showing that political geography has market value. Further tests affirm that the effects of political geography are distinct from the value of political connections

and not affected by the inclusion of money-in-politics variables.

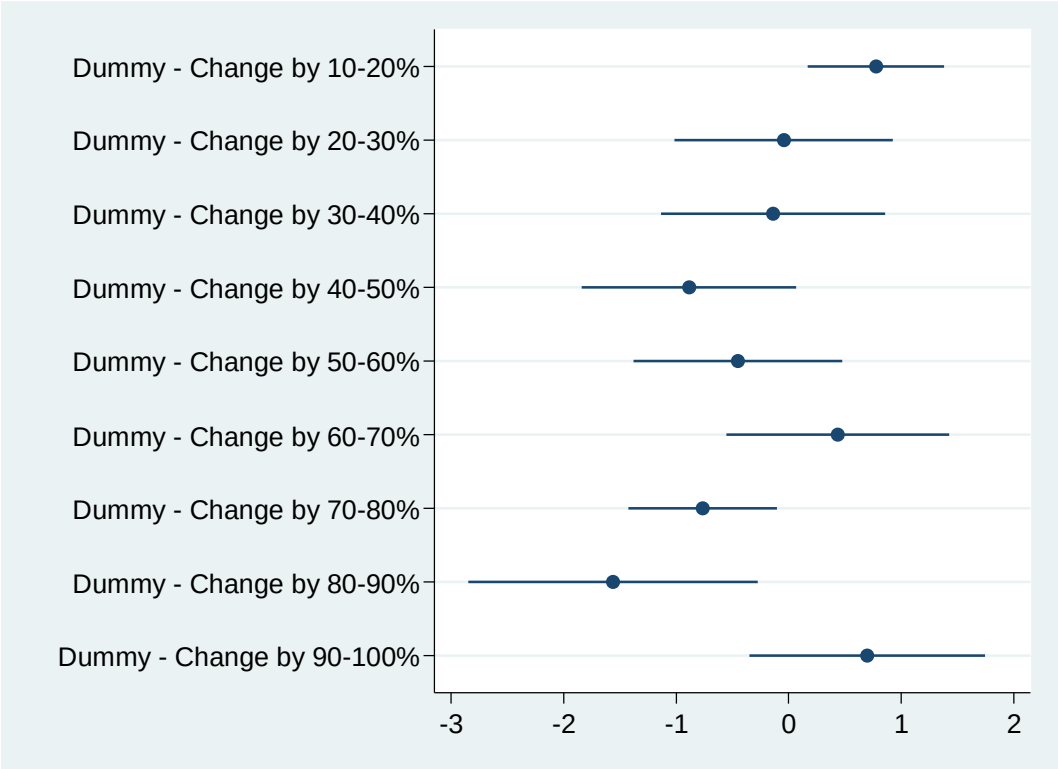
5.1 Estimating the Effect of Physical Components of Political Geography

Changes in the physical boundaries of districts represent a straightforward way of capturing changes in political geography via redistricting. Figure 2 presents a graphical representation of the results of a regression of CARs on a series of dummy variables capturing the fraction of spatial overlap between old and new districts. The dummy variables cover 10% intervals of overlap, with the reference category being firms who experience less than 10% change. Overall, we find a weak tendency towards changes in physical aspects of political geography having a negative effect on firms´ value. The results are not dispositive: the only statistically significant negative effects are shown for firms whose new districts are 70% to 90% different than their previous ones. Regression models with alternative measures of physical change (linear and squared terms and the aforementioned dummy variables) are shown in Table A2 in the online appendix; they are substantively similar.

5.2 Estimating the Effect of Partisan Components of Political Geography

Table 2 examines changes in the partisan aspects of political geography. It contains our main results and provides a baseline for further testing of alternative/related hypotheses about how other aspects of firm-politician relationships matter. We focus on decomposing the effects of district level competitiveness and partisan identity on firm valuations. First, in Column 1, we look at partisan competitiveness. Then, in Column 2, we look at partisan identity. Columns 3 and 4 look at both channels simultaneously.

Figure 2 – Plot of Coefficient for Change in Geographic Overlap Dummies



Column 1 shows that being reassigned to a competitive district decreases the average firm's valuation by approximately 1.6%, while being reassigned into a safe district increases a firm's valuation by approximately 1%. We note that the latter coefficient is not statistically significant when compared to zero; however, for competing firms, the net distributional effect is both sizable and statistically significant. If one firm was reassigned to a safe district from a competitive district, while the other was reassigned to a competitive district from a safe one, the difference in valuations would be 2.6% percent.[20] These are our baseline results.

[20]A Wald test of the difference between reassignment to a safe district and reassignment to a competitive district taking on a value different than zero has a p-value of 0.01.

Table 2 – Effect of Changes in Partisan Composition of Political Geographies on CARs

Dependent Variable	Cumulative Abnormal Return (%)				
	(1)	(2)	(3)	(4)	(5)
Dummy - Reassigned to Safe District	1.046		0.702	0.789	1.5742*
	(0.748)		(0.496)	(0.628)	(0.893)
Dummy - Reassigned to Competitive District	-1.592***		-1.589***	-1.525**	-1.751***
	(0.588)		(0.591)	(0.615)	
Dummy - Reassigned to Safe District of Other Party			1.518	0.869	
			(2.237)	(2.177)	
Dummy - Reassigned to Democratic District		0.353		-0.236	
		(0.487)		(0.697)	
Dummy - Reassigned to Republican District		1.649*		1.084*	
		(0.907)		(0.588)	
State-Fixed Effects	Yes	Yes	Yes	Yes	Yes
N	2,657	2,657	2,657	2,657	1,738
Sample:	Core	Core	Core	Core	Split Districts

Notes:
Robust standard errors clustered by state are reported. (The results remain robust at other levels of clustering, e.g. by districts.)
* indicates significance at the .1 level, ** at the .05 level, and *** at the .01 level.
CARs are estimated for a (-1, +7) event window using a Fama-French 3 Factor model and a 250 day estimation window. Table 6 shows the results with alternative event windows.
District Safety is defined as a 10% total margin so party balance outside a 45/55 or 55/45 split defines a "safe district" while party balance falling within those ranges define "competitive districts". Results with alternative definitions of safe/competitive can be found in the online appendix. Districts defined as belonging to a party (Democratic or Republican) if more than 50% of voters lean towards it.
The 'Core' Sample includes all firms from all Congressional Districts, while the 'Split Districts' only includes firms from pre-redistricting districts where firms were dispersed into more-than-one district by redistricting.

Column 2 tests whether the value of firms depends on the balance of party preferences in a given district. The results indicate that being reassigned to a district with a majority of Republican voters adds 1.6% to the value of firms relative to staying in a district that did not change its partisanship. The effect of being reassigned to a district with a majority of Democratic voters, by contrast, is positive, but far from significant. At face value, these results may suggest that markets perceive districts with more Republican voters more favorably for firms, perhaps because

Republicans are perceived as being more pro-business.[21] However, later results (e.g., Table 3) will show that this finding is not robust. Furthermore, a Wald test of whether the partisan coefficients on reassigned to Democrat versus reassigned to Republican are distinguishable from each other is not significant at conventional levels (as p=0.17).[22] Column 3 looks at whether the effects of being reassigned to a safe district vary based on whether a firm is reassigned to a safe district with an extreme difference in partisan identity. There might be a substantive difference between firms being reassigned from safe Republican districts to safe Democratic districts, and vice-versa, relative to firms being reassigned from competitive districts to safe districts dominated by either party. However, the corresponding coefficient is statistically indistinguishable from zero.

Column 4 puts all the partisan variables in a single regression, allowing us to compare directly partisan competition with partisan identity. None of the coefficients change appreciably from those in previous columns. Hence, the partisan aspects of political geography that have the largest and most systematic effect on firm value relate to the partisan safety and competitiveness of districts rather than the partisan identity of the districts. Being reassigned to a competitive district decreases firm valuations, while being reassigned to safe districts increases them. The net effect has a similar magnitude at 2.3%.

Finally, Column 5 presents results using the specification in Column 1 adding the sample

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[21] Another competing expectation from Kim et al. (2012) might be that markets give a premium to firms located in states (or districts in our case) that are aligned with the President's party (which would have been the Democratic Party in 2012). Our results for firms in Democratic districts are consistent with that expectation, while those in Republican districts are not. However, the results are also not directly comparable as we are measuring the partisan identity of the district, not the partisan identity of the elected politician from that district.

[22] We drilled down on industry-level differences in partisan effects using industries that clearly seem to align with one party (Energy, Materials, and Financials with Republicans—see Gimpel 2014), but did not obtain clear-cut results, possibly because the sample size of firms that are re-allocated within any given industry is small.

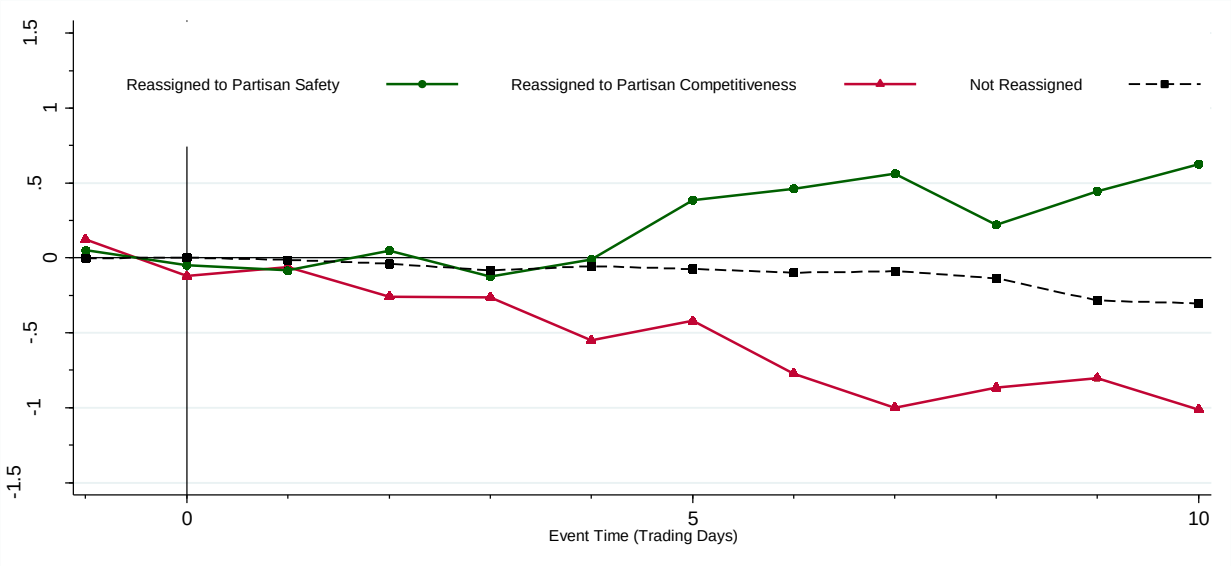
constraint that firms that start in a common Congressional district end up being split into multiple districts after redistricting. In other words, we drop a firm if redistricting did not remove any firms from the district of the firm in question. This restriction limits the sample to firms who see a stronger change in the nature of their district. The net effect of being reassigned to a safe district relative to a competitive one in this sample remains robust and now is 3.3% suggesting that our results are driven by the splitting of firms into multiple districts.

Figure 3 graphs the main finding over event-time, averaging cumulative abnormal returns for firms that are reassigned to safe districts and those that are reassigned to competitive ones; it also displays results for firms in the omitted category in our regressions (i.e. those not reassigned to a different type of district).[23] The graph shows a clear divergence between the two focal groups after the event, which increases over time and stabilizes after day 5. These dynamics are consistent with information about the effects of redistricting on firms diffusing gradually through markets as analysts process new political maps. Firms not reassigned to a different type of district (our omitted category) experience relatively stable, near zero, CARs around redistricting.

A concern might remain that the results establishing the value of partisan aspects of political geography could be largely a re-statement of factors the literature on business and politics has already found to be important, such as political connections and money-in-politics. For example, reassignment-to-competitive could simply capture the effect that some firms lose an important connection to the politician who represents its headquarters' district. Similarly, with respect to money, reassignment-to-safety may simply be a function of efforts to obtain that outcome. We explore these possibilities in the next section.

[23] Table 6 presents regressions with CARs constructed around alternative event windows.

Figure 3 – Plot of Mean CARs in Event Time for all Firms Experiencing Similar Shifts in Nature of Partisan Competition in their Headquarters' Congressional District



Note:
Since this is a financial market-based event study, the mean cumulative abnormal return (CAR) for all groups should be zero, by construction, in the pre-treatment period. Hence, we do not need to concern ourselves with examination of divergent pre-treatment trends as we would if this were a difference-in-difference type design. Table 6 provides similar analysis in a regression-based format (including state fixed-effects).

Partisan Components of Political Geography in the context of Political Connections

Table 3 shows how our baseline results from Table 2 hold up in the context of firms' headquarters' districts either retaining or failing to retain the same politician with whom they shared geographically based connections before redistricting. As explained above, the variable "same politician" can be thought of as a proxy for the market's belief that a firm's headquarters' district will see a geographically connected politician run again in the same district as the firm and win.

Column 1 looks for a direct and independent effect of retaining the same politician on explaining the distribution of CARs. We find that after redistricting maps are announced firms who will eventually have the same politician fare marginally better than firms who will not. The positive, although non-significant, coefficient on the "same politician" variable is consistent with

the literature demonstrating a value of political connections (e.g. Roberts 1990, Fisman 2001).[24]

Column 2 shows our baseline regression with the same politician variable as a control. The main results from Table 2—that being reassigned to a competitive district decreases valuations, while being reassigned to a safe district increases valuations—remains essentially identical to that reported previously.

Table 3 – Baseline Results adding Same Politician

Dependent Variable	Cumulative Abnormal Return (%)			
	(1)	(2)	(3)	(4)
Dummy - Reassigned to Safe District		1.062	1.617	0.807
		(0.726)	(1.041)	(0.641)
Dummy - Reassigned to Competitive District		-1.576***	-1.795**	-1.506**
		(0.578)	(0.763)	(0.601)
Dummy - Reassigned to Safe District of Other Party				0.881
				(2.163)
Dummy - Reassigned to Democratic District				-0.238
				(0.700)
Dummy - Reassigned to Republican District				1.081
				(0.594)
Dummy - Same Politician	0.037	0.057	0.193	0.068
	(0.327)	(0.275)	(0.210)	(0.258)
Interaction Dummy - Same x Reassigned to Safe			-1.541	
			(1.058)	
Interaction Dummy - Same x Reassigned to Competitive			0.760	
			(1.123)	
State-Fixed Effects	Yes	Yes	Yes	Yes
N	2,657	2,657	2,657	2,657

Notes:
Same politician is an indicator taking a value of 1 when, after the next election, the firm's district is represented by the same politician as before redistricting.
Interaction Dummies are dummy variables representing interactions of the above dummy variables.
All other variables, test statistics, etc. are the same as those described in Table 2.

Column 3 adds interaction variables to what is in Column 2. This model helps us better understand subsets of the population: those firms who will eventually retain the same politician

[24] It is not surprising, however, that our coefficient is smaller than the ones implied by previous literature given that we look at all politicians and not only at those that hold the most important positions.

(67% of our sample) and those firms who will not.[25] The firms that end up with new politicians appear to be driving our main results: the net effect of being assigned to a safe district versus a competitive one is 3.4% for them vs the 2.6% baseline effect for the total population.[26] Nevertheless, we still find an effect among the set of firms who retain geographic ties to the same politician, consistent with a mechanism where politicians' constraints in office change. The importance of political geography thus goes beyond political connections.

Column 4 adds variables for the district's partisan identity. The results remain unchanged.

Partisan Components of Political Geography in the context of Monetary Quid pro Quo

Table 4 takes on the role of money-in-politics vis-à-vis political geography. It demonstrates that our results are not threatened by, and are conceptually distinct from, campaign contributions as *quid pro quo*. We might be concerned that political geography works differently for politically active firms: they may be allocated to more favorable political geographies or the market may be better able to anticipate the consequences of shifts in political geography on their future performance. We also need to address concerns that reassignment-to-safe and reassignment-to-competitive may actually be capturing the effect of successfully using money to influence political geography rather than the effect of political geography itself.

If politically active firms come out of the redistricting process better situated than politically inactive firms do, then we should expect that there is a direct effect of firms having a PAC in explaining the heterogeneity in CARs. [27] We test for this in Column 1. We find a coefficient

[25] Note that 67% is lower than the roughly 95% reelection rates of incumbents (Friedman & Holden 2009).
[26] We obtain this 3.4% net effect by taking the difference between the coefficient estimates for the effect of reassignment-to-safe (1.6%) and the coefficient for reassignment-to-competitive (-1.8%).
[27] Note that we also conducted the exact same tests but constructed a variable for whether a firms' PAC had

that is not distinguishable from zero, suggesting either that (i) politically active firms cannot always select themselves into more favorable districts, or (ii) the market anticipates their on-average better outcomes and has priced them in prior to our event window.

Table 4 – Baseline Results adding Measure for Politically Active Firms

Dependent Variable	Cumulative Abnormal Return (%)		
	(1)	(2)	(3)
Dummy - Reassigned to Safe District		1.047	1.038
		(0.750)	(0.798)
Dummy - Reassigned to Competitive District		-1.604***	-1.737**
		(0.584)	(0.685)
Dummy - Has PAC	-0.291	-0.313	-0.362
	(0.339)	(0.341)	(0.408)
Interaction Dummy - Reassigned to Safe x Has PAC			0.042
			(1.011)
Interaction Dummy - Reassigned to Competitive x Has PAC			0.697
			(0.881)
State-Fixed Effects	Yes	Yes	Yes
N	2,657	2,657	2,657

Notes:
Has PAC is a dummy taking on a value of 1 if a firm has an active Political Action Committee prior to redistricting.
Interaction Dummies are dummy variables representing interactions of the above dummy variables.
All other variables, test statistics, etc. are the same as those described in Table 2.

Column 2 shows that our main results on being reassigned to a safe district and being reassigned to a competitive district remain unaltered when we include the dummy for whether a firm had a PAC as a control. This regression model tests for whether some fraction of the positive coefficient on reassignment-to-safe is driven by a firm being politically active. We find no evidence of this being the case. Column 3 tests whether the market reacts differently to various

contributed to the candidate representing the pre-redistricting district in which the firm was headquartered and got substantively similar results as we do for whether a firm has a PAC at all. Ideally, we would observe and be able to construct a firm-level measure of who tried to—and who was successful at—influencing political maps; however, that is infeasible given disclosure laws in various states.

shifts in political geography depending upon a firm's political activity. Again, we find no evidence it does. The takeaway from Table 4 is that political geography, specifically, the level of partisan competition, stands independently of the role of money-in-politics.

5.3 Validity, Robustness, and Heterogeneity Analysis

In this section we discuss the validity of our results and present robustness checks. We also investigate how our results differ across subsamples based on redistricting institutions and politician attributes. Online Appendix B presents additional robustness checks (e.g., safe/competitive districts defined using alternative thresholds, controls for population density, and states without redistricting as placebos).

Revisiting the Key Identifying Assumption

The primary identifying assumption underlying our empirical work is: market participants respond to the *new information* contained in the release of new political maps emerging from 43 distinct state-level redistricting efforts *in the days following* the release of credible final maps into the public domain for each state. As we argued above, if we either failed to identify the correct dates when maps were released, or market participants were able to correctly anticipate the shapes of new maps, this would bias our results towards zero. The fact that we do find systematic effects indicates that we managed to closely approximate the dates when relevant information entered the public domain. To underscore this point, we carry out a placebo test on alternative dates where we would expect to find null effects on our main treatment groups. We move all event dates for the release of credible final maps backward by 12, 9, 6, and 3 months and re-run our baseline

regression found in Column 1 of Table 2. The results appear in Table 5.[28] As we should expect given no actual event on these alternative dates, we find no meaningful results across these four placebo regressions: (i) neither the coefficient on reassignment to safe districts nor the coefficient on reassignment to competitive districts is statistically indistinguishable from zero, (ii) nor does the p-value from a Wald test looking at whether the net effect is different from zero come close to conventional levels of statistical significance. Moreover, in some instances the signs of the coefficients in question run in the opposite direction of those we find around the actual event dates.

Hence, we take our placebo test and the logic for a bias towards zero stemming from a critical assessment of our key identifying assumption as suggesting that our results are valid and, if anything, might represent a lower bound for the true effect.

Table 5 – Placebo Tests varying Event Date in Baseline Regression

Dependent Variable		**Placebo Cumulative Abnormal Return (%) offset by…**			
Placebo relative to Event Date in HQ State		***-12 Months***	***-9 Months***	***-6 Months***	***-3 Months***
		(1)	(2)	(3)	(4)
Dummy - Reassigned to Safe District		0.304	0.318	0.472	-0.188
		(0.612)	(0.607)	(0.774)	(0.679)
Dummy - Reassigned to Competitive Distric		0.628	-0.828	0.892	-0.820
		(0.521)	(0.535)	(0.798)	(0.644)
State-Fixed Effects		Yes	Yes	Yes	Yes
N		2,346	2,345	2,399	2,461
Wald Test Results					
Is the benefit of moving to a safe district distinguishable from cost of moving to a competitive district? B1=B2?	***F-Stat***	0.13	2.02	0.23	0.35
	p-value	0.72	0.16	0.64	0.56

Notes:
Variables, test statistics, etc. are the same as those described in Table 2.

Robustness to Altering Abnormal Return Windows

In our regressions above we chose a window of -1 trading days to +7 trading days after the release

[28] We only examine placebos for the pre-event period because the estimation window used to calculate CARs in the post event placebos would be contaminated by data covering the actual event date.

of credible final political maps when calculating CARs. We test for the robustness of our core results in Column 1 of Table 2 to using event windows that are both shorter and longer or do not include a pre-treatment day. The results in Table 6 suggest that our primary findings are robust to different cumulative abnormal returns windows. Also, the results show it takes several days after the event date until we achieve statistically significant results on the differences between firms reassigned to safe districts relative to those reassigned to competitive districts. This finding is not surprising given both the time required to process redistricting maps into implications for firms and the evolution over time as CARs get longer; moreover, we saw a similar pattern in Figure 3. Finally, it is worth noting that the magnitudes of the results remain relatively stable over event windows close to the baseline window we use.

Robustness to Geographic Concentration of Firms in Headquarters Location

A concern that readers may have is that some firms are geographically dispersed or have a headquarters in name only. For example, Boeing does most of its work in Washington state but is legally headquartered in Chicago. We would expect that firms that do not do the majority of their work at the site of their headquarters are less likely to see a (large) effect of changing political geography around that site. This is something we can test for directly. We use Garcia and Norli's (2012) data on the geographic dispersion of firms to construct a measure of whether firms conduct the majority of their business near their headquarters. The dummy variable takes a value of 1 if no more than two states are mentioned in the firm's annual report.

Table 6 – Tests of Alternative Event Windows

Dependent Variable		Cumulative Abnormal Return (%) with Event Window as…									
Event Window for CAR		*(0,7)*	*(-1,1)*	*(-1,2)*	*(-1,3)*	*(-1,5)*	*(-1,6)*	*(-1,7)*	*(-1,8)*	*(-1,9)*	*(-1,10)*
Note on Event Window		*No Pre-Day*	*Shorter than Baseline*					*Baseline*	*Longer than Baseline*		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dummy - Reassigned to Safe District		0.910	0.103	0.340	0.179	0.794	0.915	1.046	0.585	0.891	1.097
		(0.652)	(0.232)	(0.266)	(0.263)	(0.773)	(0.731)	(0.748)	(0.670)	(0.677)	(0.770)
Dummy - Reassigned to Competitive District		-1.666***	-0.457	-0.645	-0.633	-0.800	-1.201**	-1.592***	-1.397**	-1.158*	-1.258**
		(0.488)	(0.478)	(0.472)	(0.583)	(0.657)	(0.586)	(0.588)	(0.687)	(0.623)	(0.613)
State-Fixed Effects		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N		2,657	2,657	2,657	2,657	2,657	2,657	2,657	2,657	2,657	2,657
Wald Test Results											
	F-Stat	9.28***	1.20	3.07*	1.41	2.38	4.77**	7.16**	3.43*	4.51**	4.81**
Is the benefit of moving to a safe district distinguishable from cost of moving to a competitive district? B1=B2?	*p-value*	0.00	0.28	0.09	0.24	0.13	0.03	0.01	0.07	0.04	0.034

Notes:

Variables, test statistics, etc. are the same as those described in Table 2.

The results of tests incorporating this variable appear in Table 7. Column 1 shows that the importance of the headquarters location has no statistically significant connection with the size of access returns. Furthermore, according to Columns 2 and 3, the magnitudes of our results are larger when we control for the geographic concentration around the headquarters than when we do not. This suggests not only that our core results in the paper are robust to measurement leakages around results based on firms' headquarters districts (regardless of their importance to the firm), but also that our core results probably understate the magnitude of our main result that firms benefit from being reassigned to safe districts relative to competitive ones.

Table 7 – Testing for Role of Geographic Concentration of Firms around Headquarters

Dependent Variable	**Cumulative Abnormal Return (%)**		
	(1)	(2)	(3)
Dummy - Reassigned to Safe District		1.246	0.351
		(0.840)	(1.035)
Dummy - Reassigned to Competitive District		-1.679***	-2.484***
		(0.625)	(1.098)
Dummy - HQ Main Location	-0.336	-0.393	-0.590*
	(0.324)	(0.321)	(0.344)
Interaction Dummy - Reassigned to Safe x HQ Main Location			1.167
			(1.265)
Interaction Dummy - Reassigned to Competitive x HQ Main Location			1.304
			(1.558)
State-Fixed Effects	Yes	Yes	Yes
N	2,478	2,478	2,478

Notes:
HQ Main Location is a dummy variable taking on a value of 1 if a firm's financial statement text indicates that most of the firm's business occurs in physical proximity to the firm's headquarter location based on data scraped in Garcia & Norli (2012).
The remaining variables, test statistics, etc. are the same as those described in Table 2.

Limiting Sample to Sets of Redistricting Institutions

Some readers may be curious if our results depend on who draws district lines, recalling that redistricting institutions differ across states. These can be either (i) courts [9 states], (ii) independent commissions [4 states], (iii) strong partisan legislatures dominated by a single party

[21 states], or (iv) split, bi-partisan legislatures [9 states]. While an interesting point of inquiry, it is difficult to make strong theoretical predictions about how results may differ depending on who draws district boundaries. We might expect the strongest results in the case of courts or commissions, given that maps released by these institutions are most binding and therefore represent 'sharper events.[29] On the contrary, the choices made by these institutions may be more predictable a priori and hence *less* consequential for abnormal market returns. Finally, reassignment to competitive or safe districts may mean different things depending upon the institution drawing the lines. E.g., strong partisan legislatures might strategically move out-party members to more competitive districts, which could introduce a further bias against firms reassigned to competitive districts in such states.

Results of our core regression run on different sub-samples by redistricting institution appear in Table 8. They are more consistent with the no-strong-relationship hypothesis. We caution against over-interpretation given relatively small sub-samples, but to the extent that we can glean anything from these results we might say that it seems worst for firms to be redistricted to a competitive district when the maps are drawn by one dominant party and best for firms to be reassigned to a safe district by an independent commission.

[29] For example, court drawn maps are the result of a legislature's failure to make timely decisions, cannot be contested, and are thus binding. On the other end of the spectrum, a map coming out of a bi-partisan legislature may be less credible because it could fail a vote in the legislature more easily even if reported on as that being a likely event.

Table 8 – Baseline Regression, Limiting Sample to Sets of Redistricting Institutions

Dependent Variable	Cumulative Abnormal Return (%)			
Sample [Limited to Maps Drawn by…]	*Courts*	*Independent Commission*	*Strong Partisan Legislature*	*Bi-Partisan Legislature*
	(1)	(2)	(3)	(4)
Dummy - Reassigned to Safe District	1.627	1.258***	0.310	1.432
	(2.483)	(0.037)	(0.767)	(1.973)
Dummy - Reassigned to Competitive District	0.117	0.373	-2.319***	-1.697503
	(0.889)	(1.748)	(0.599)	(1.459)
State-Fixed Effects	Yes	Yes	Yes	Yes
N	751	502	1,112	292

Notes:
The variables, test statistics, etc. are the same as those described in Table 2.

Robustness to Politician Attributes

We examined the consequences of firms retaining the same politician earlier, but without considering the attributes of that politician. Home district politicians' tenure and leadership roles may affect the type of district they are reassigned to. These may covary with firms' reassignment to partisan safe districts or partisan competitive districts, which is why we test for robustness to these factors in Table 9. Columns 1 and 4 confirm that politicians with above median tenure or who hold committee chair positions have no independent effect on CARs around redistricting announcements. Columns 2 and 5 show that our results survive when treating politician tenure and leadership roles as potentially omitted variables. Column 3 adds an interaction term for tenure with district type and Column 6 adds an interaction term for committee leadership with district type. Column 3 shows that firms reassigned to safe districts benefit when they have geographic ties to longer tenured politicians; the effect is substantively larger and statistically stronger than in the baseline regression, consistent with the relational contracting mechanism we posit. Column 6, by contrast, indicates that ties to politicians in leadership positions dampen both the positive effect of being assigned to a safe district and the negative effect of being assigned to a competitive district.

A possible interpretation is that committee chairs have attributes such as competence that make district composition less consequential for their re-election prospects. Note that these results are based on a small sample, as few firms with an incumbent in a leadership position see a change in the partisan nature of their district.

Table 9 – Robustness to Politician Characteristics

Dependent Variable	Cumulative Abnormal Return (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy - Move to Safe District		1.039	0.330748		1.040587	1.257407
		(0.777)	(0.784)		(0.755)	(0.792)
Dummy - Move to Competitive District		-1.601***	-1.407*		-1.582**	-1.720***
		(0.585)	(0.747)		(0.594)	(0.588)
Dummy - Long Tenure	-0.070	-0.073	-0.215			
	(0.360)	(0.359)	(0.391)			
Interaction Dummy - Long Tenure * Move to Safe			1.693*			
			(0.973)			
Interaction Dummy - Long Tenure * Move to Competitive			-0.920			
			(1.250)			
Dummy - Leader				-0.544	-0.513	-0.412
				(0.472)	(0.481)	(0.537)
Interaction Dummy - Leader * Move to Safe						-1.463***
						(0.524)
Interaction Dummy - Leader * Move to Competitive						3.200**
						(1.293)
State-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	2,657	2,657	2,657	2,657	2,657	2,657

Notes:
Leader is a dummy taking on a value of 1 if a firms' headquarters district was represented by a candidate serving as chairman of any congressional committee.
Long Tenure is a dummy variable taking on a value of 1 if the firms' headquarters' district was represented by a politician with tenure greater than the median number of terms.
The remaining variables, test statistics, etc. are the same as those described in Table 2.

6 DISCUSSION

In this section, we discuss the magnitude of the effects we find and the broader relevance of our results.

6.1 Magnitude of Effects

The primary goal of this research was to establish whether firms accrue value by virtue of their political geography. We showed that they do. The secondary aim of this research was to identify

what aspects of political geography matter for firms—and we showed that it is fundamentally the level of partisan competitiveness of districts that matters. Having established that there is value in political geography to firms, how meaningful is that value in both an absolute sense and relative to other factors that determine business-government relationships?

In absolute terms, a net impact of 2.6% on firms' values is large. For an S&P500 firm, which must have a minimum market capitalization over $6B, a difference of 2.6% equates to a bonus of $156M for firms located in safe districts compared to those located in competitive districts after redistricting. Moreover, according to Table 1, over 75% of publicly traded firms appear to be located in safe districts, so the cumulative amount of value embedded in the political geography of firms is quite large, particularly considering social movements whose aim is to create more competitive districts. Of course, our results do not directly address the general equilibrium or social welfare implications if we were to see substantial reforms in the redistricting process such as moves to independent commissions in more states.[30]

The value of political geography we establish is sizable relative to the value of political connections in a US context. The largest estimate of the value of political connection in the US context appear to be approximately 2.5% for firms tightly connected to senior members of the Senate in leadership positions (Roberts 1990) but could also be next to nothing even if connected to the Vice President (Fisman et. al 2012). Of course, these numbers pale in comparison to the largest estimate of value of political connections in a developing country context with rampant

[30] Given the magnitude of the results, one may wonder whether firms move headquarters to capture this value. We think this is unlikely given how costly it may be to move headquarters and employees and how difficult it is to predict future electoral maps. Moreover, firms would have to be prepared to move every 10 years with redistricting cycles. Without *ex ante* knowledge of future political maps, the lead time necessary to move would require several of those 10 years. We note also if anticipatory moves of firm headquarters happened, this would attenuate our results, pushing them towards zero.

corruption and cronyism as Fisman (2001) estimated that close connections to Suharto were worth approximately 23% of firms' market capitalizations. Nevertheless, when we look at the subset of firms reassigned to a safe congressional district and not retaining a political connection, the net bump they see in value at 3.4% is approximately as large as it is for any political connection estimate in the U.S. context—suggesting that political geography can be at least as valuable for firms as even the most valuable political connections.

6.2 Revisiting Capture Theory

Our results inform a broader academic conversation on business-government relationships and the balance of power between firms and the state (Zingales 2017). Taken together, we believe that our results suggest at a very fundamental level that political geography is a key determinant of business-politician relationships.

Capture theory (Stigler 1971) emerged in response to the then predominant public-interest view of how governments function. Put simply, capture theory posits that firms demand policies and receive them in return for a quid pro quo with politicians, a view later formally modeled by Grossman and Helpmann (2001) in various configurations. Our results suggest that the susceptibility of a politician to capture depends on the characteristics of their district, notably the level of competition that a given politician faces in his electoral district. More competition may force politicians to cater more intensely to voters, leaving less scope/time for firms, meaning firms may partly be held hostage by the voters in the political districts in which they operate. Put slightly differently: political geography affects the relational contract between firms and politicians (e.g., Baker et al. 2002) in ways that increase/decrease the possibility of politician capture by firms.

7 CONCLUSION

This paper establishes that firms see an increase in market value when reassigned to districts with

larger partisan divides and suffer when reassigned to more competitive districts: the net differential between safe and competitive U.S. House districts equates to 2.6% of firm value. Given that the main threat to inference is attenuation bias, this number likely represents a lower bound. The effects are plausible compared to previous studies of politically connected firms and sizable in absolute terms: for an S&P500 firm, which must have a market capitalization over $6B, the difference equates to $156M.[31] With ~75% of publicly traded firms residing in safe districts (see Table 1), the market value embedded in political geography is enormous. Nothing in our work undermines existing findings on political connections and money-in-politics; nonetheless, we showe that our channel—the constraint imposed by voter characteristics/the level of competition—is distinct from existing stories about political connections and money-in-politics. In short, we provide compelling evidence that political geography has a tangible and independent effect on the nature of firm-politician relationships that has heretofore been overlooked in the business-and-politics literature.

[31] Given the size of our results, we may wonder if firms move districts. We think such moves are unlikely given they are time-consuming, costly, and require exceptional foresight about future maps. Empirically, anticipatory moves would attenuate our results.

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