

Artés, Joaquín; Richter, Brian Kelleher; Timmons, Jeffrey F.

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The Value of Political Geography: Evidence from the Redistricting of Firms

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Stigler Center

for the Study of the Economy and the State

**The Value of Political Geography: Evidence from
the Redistricting of Firms**

Joaquín Artés

Universidad Complutense de Madrid

Brian Kelleher Richter

University of Texas at Austin McCombs School of Business

Jeffrey F. Timmons

New York University – Abu Dhabi

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University of Chicago Booth School of Business

5807 S Woodlawn Ave

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

Joaquín Artés*
Brian Kelleher Richter†
Jeffrey F. Timmons‡

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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 keep some firms in Congressional districts that are largely unchanged at one extreme and reassign other firms to entirely new 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 firm retain the same politician or actively make campaign contributions.

Keywords: Political Geography, Redistricting, Capture Theory, Representation

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* jartes@ucm.es. Associate Professor of Economics, Department of Economics and Law School. Universidad Complutense de Madrid. <https://sites.google.com/site/jartesenglish/>

† brian.richter@mcombs.utexas.edu. (Corresponding Author) Assistant Professor. Department of Business, Government, and Society. McCombs School of Business. University of Texas at Austin. <http://briankrichter.com>

‡ jft3@nyu.edu. Associate Professor of Political Science. New York University – Abu Dhabi.

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1 INTRODUCTION

Transformed political geographies emerge from a constitutionally mandated reapportionment process every 10 years in the United States. The results typically stir public controversy, inspiring social movements and lawsuits alike.¹ The critical role of redistricting in shaping the political stage has spurred intense academic inquiry, primarily into its *political* consequences.² Yet, no one has studied the *economic* impact on firms. We do so in this research.

We establish that political geography has value to firms, notwithstanding other factors that shape business-government relations. Our identification strategy exploits the redistricting of firms' headquarters. We operationalize it in an event study design around disparate announcements of new electoral maps across 43 states during the 2010 federal redistricting cycle—revealing how focal firms' abnormal returns systematically vary with alternate aspects of their redefined political geographies that only the redistricting event can create.

Consider the case of two identical firms that share a Congressional district prior to redistricting that is then split into two. *Firm A*'s new district largely overlaps with its old district while *Firm B*'s new district hardly overlaps. How would *Firm A* perform relative to *Firm B*? In this scenario, we find that firms subject to greater changes in physical aspects of their districts tend to suffer greater losses.

Consider also the case of another pair of identical firms. *Firm X* resides in a district where constituents belong equally to both major parties, but following redistricting is reassigned

¹ The Supreme Court in 1962, decided in *Baker vs Carr* that courts had the constitutional authority to enter the political fray on redistricting (and other election-related) cases under the equal protection clause enshrining the “one person, one vote” principle as a matter of law (Ansolabehere & Isaccharoff 2003) requiring states to have balanced populations across districts. In June 2019, however, the court opined in *Benisek vs Lamone* and *Common Cause vs Rucho* that “partisan gerrymandering claims present political questions beyond the reach of federal courts” even if “excessive partisan districting leads to outcomes that reasonably seem unjust,” leaving reform to legislatures and voters via referenda. As an example of a social movement response to gerrymandering, the League of Women Voters of Asheville-Buncombe County, North Carolina organized a 5k Walk & Run on November 4, 2017 to bring awareness to odd district boundaries by sending participants down a course that jitters along the borders of NC Congressional Districts 10 and 11, splitting the city of Asheville, NC they say artificially. The event received so much interest nationally, that they've even started sharing resources on how to organize similar events in other locales on their website at <http://www.lwvab.org/gerrymander-5k/make-gerrymander-5k-resources/> [last accessed 4/17/2019].

² The extant academic inquiry tends to focus on three *political* aspects of redistricting: (i) antecedents to mapmaking decisions, (ii) measurement of partisan or other bias in the outcomes, and (iii) consequences of redistricting and gerrymandering on political outcomes. In the stream focused on the antecedents to redistricting schemes several papers study the optimal ways to create a partisan advantage through gerrymandering including work by Grofman (1985), Owen & Grofman (1988), Gilligan & Matsusaka (1999), Coate & Knight (2007), Friedman and Holden (2008), and Gul & Pessendorfer (2010). Shotts (2001, 2002) considers how parties may further partisan ends by exploiting legal restrictions on the form of redistricting plans such as requirements to include majority-minority districts. Notable in the stream focused on measurement of bias in redistricting plans are works by: Gelman & King (1990), Gelman & King (1994), Fryer & Holden (2011), Stephanopoulos & McGhee (2015). Finally, in what's likely the largest tradition in this literature are papers looking at the consequences of redistricting on political outcomes such as: incumbency advantage (Ansolabehere, Snyder, & Stewart 2000), incumbent behavior (Ansolabehere & Snyder 2012), electoral responsiveness (Gelman & King 1994), electoral competition (Gelman & King 1990), electoral outcomes (Cox & Katz 2003), incumbent reelection rate (Friedman and Holden 2009); partisan policy outcomes (Cain 1985; Shotts 2003); and, polarization (McCarthy et al. 2009).

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*? In this scenario, we find that redistricting benefit firms reassigned to safer, heavily partisan, districts; meanwhile firms reassigned to competitive, partisan balanced, districts suffer.

The effects are economically meaningful. Even conservatively-estimated, the magnitudes are large, accounting for average shocks to firm valuations of well over 1.5%—and as large as 2.8% among certain sub-populations of firms starting in the same Congressional district. Moreover, redistricting affects a sizable set of firms in meaningful ways: in the latest cycle, more than one-third of publicly traded firms saw greater than half of the surface area of their congressional districts turn over, and nearly one-quarter experienced a transition in their partisan character. In aggregate, the market has capitalized billions of dollars based on the political lines within which firms sit.

The findings remain robust in batteries of tests aimed at falsifying assumptions related to both statistical identification and measurement choices. The results not only survive, but also are strengthened in tests designed to address what at first may seem like alternative stories about business-government relationships. Moreover, as we detail later, the value of political geography is distinct both conceptually and empirically from typical stories about political-connections and money-in-politics.³

Given how our findings are broad enough to stand side-by-side with the literatures on political connections and money-in-politics, they may even help inform our understanding of how each functions in practice. For example, while maintaining a connection to the same local politician after redistricting is valuable to firms, that value is contingent on shifts in the

³ There are large entire 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 & Parsley (2009), Fisman et al. (2012), among others. deFigueiredo & Richter (2014) provide a recent analytical literature review on the empirical lobbying literature. Some highlights from that recent lobbying literature include work by Richter et al. (2009) showing how firms that spend more on lobbying pay lower taxes by Blanes i Vidal, et al. (2012) and Bertrand et. al (2014) focused on the revolving door and how much lobbyists trade on who they know versus what they know. Snyder (1990) was one of the first to seriously analyze empirically campaign contributions as investments. A series of papers followed including those by Krozner & Stratmann (1998), Stratmann (1998), Stratmann (2001). Ansolabehere et al. (2003) conducted a large-scale analytical literature review into the early campaign contributions literature finding questioning how robust links between PAC dollars and politicians' roll call votes in favor of linked firms were. Interest in the topic has fanned recently taking a turn to more shadowy links towards campaign dollars, examining the contributions of individual executives rather than political action committees (e.g. see Fremeth et. al 2013, Richter & Werner 2017, or Fremeth et. al 2018) or the consequences of the *Citizens United* supreme court decision liberalizing independent expenditure dollars for corporate interests (e.g. see Klumpp et al 2016 or Werner & Coleman 2014). Going beyond the traditional channels for corporate political activity an emerging literature looks at the role of corporate social responsibility and philanthropy as a means of influence (Richter 2016; Bertrand et al 2018; Bertrand et al 2019).

underlying partisan nature of the political geography the firm shares with the politician and her constituency⁴—perhaps due to the relational contract between the firm and the politician changing as the politician’s relational contract with her constituency changes. Hence, politicians representing more competitive districts may be more constrained by constituents in terms of what they can deliver firms, while politicians representing safer districts may be less constrained. Thus while our results are consistent with broader ideas underlying capture theory focused on the demand for and value of favorable policy (Stigler 1971; Peltzman 1976), they can help inform our understanding by bringing into focus potential, unexplored constraints on the supply side of regulation. Specifically, the ability of firms to capture political rents is limited by the politicians’ constituencies and reelection prospects.

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.⁵ 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.⁶ Honeywell Corporation acknowledged that its donation stemmed from a preference for “redistricting that is consistent with the historical practices”—a sentiment echoed by CSX International “as a Florida-based corporation.”⁷

In our analysis, we openly acknowledge firms’ potential interest in influencing redistricting. We address how this would affect our estimates, noting that such efforts by firms do not nullify our identification of the existence of a value to political geography, but rather challenge our ability to reject a null effect, achieving statistical significance. 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, particular if other firms experience as-if random

⁴ We note that we are not the first researchers to show value to organizations interested in government outcomes by virtue of geographic ties to local legislators—e.g. in the context of federal appropriations, Roberts (1990) finds evidence of value in ties to relevant Congressional Committee Chairs via their geographic constituencies, deFigueiredo & Silverman (2006) find the same for universities, additionally documenting that they spend less on lobbying to achieve the same outcomes, and Chatterji et al. (2018) show similar accruing to universities that reside in state-legislators’ districts. Faccio & Parsley (2009) document value of geographic ties as well through a large scale event study of deaths of politicians on firms in their hometowns. Where we differ from each of those papers is that we examine what happens when the composition of the local politicians constituency is shocked by redistricting rather than merely showing value in the static geographic tie.

⁵ 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/>.

⁶ See [https://ballotpedia.org/Florida_Legislative_District_Boundaries_Amendment_5_\(2010\)](https://ballotpedia.org/Florida_Legislative_District_Boundaries_Amendment_5_(2010)) [last accessed: 4/17/19]

⁷ See <https://www.propublica.org/article/hidden-hands-in-redistricting-corporations-special-interests> [last accessed: 4/17/19]

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 identify precisely 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 anything, the lack of a surprise to market participants in redistricting announcements would push coefficient estimates of the average effect on all firms towards zero. That dampening occurs because market participants will 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, since we do not believe *all elements* of new redistricting map announcements are unexpected.

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 forty-three states must conduct redistricting so that each district contains approximately the same number of voters.⁸ The need for redistricting is obvious in the states that gained or lost a seat through the reapportionment process. Nevertheless, redistricting remains necessary even in states that keep the same number of seats (greater than one), given shifts in relative populations across localities within states.

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 for new federal House district maps ranging from having independent commissions draw boundaries to having a majority party in the state legislature determine where the lines are

⁸ 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.

drawn. 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 a number of cases, particularly in states where independent commissions or courts draw the lines, there is only one potential map that enters the public domain and that is almost certain to 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, these 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: ADVANTAGES AND LIMITATIONS

Our research design provides a straightforward means to understand whether or not changes in various aspects of political geography embedded in these maps causes market participants to react differently to firms that experience different types of redistricting. Like any other approach, it requires various assumptions that condition our interpretation of the statistical results. We also address measurement challenges with implications for identification and interpretation. Nevertheless, the approach generates a robust causal inference that political geography has a value to firms.

We follow a standard event study design approach.⁹ Hence, 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

⁹ See MacKinlay (1997) and Kothari and Warner (2007) for literature reviews and methodological guides to financial market event studies. In the political economy space, see Roberts (1990), Fisman (2003), Knight (2006), Jayachandran (2006), Ferguson & Voth (2008), and Amore & Bennedsen (2013) among others exploiting the methodology.

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 various measures of changes to firms' political geography against CARs should reveal which aspects of political geography cause some firms to perform better and others worse.¹⁰

For event study approaches to generate reliable insights we need assumptions about market efficiency to hold (Fama 1970). That is, in the ideal scenario, we would want market participants to be able to react instantaneously and simultaneously to the arrival of new information with perfect foresight as to what the implications were for firms. Short event-windows alleviate concerns about other concomitant events explaining any abnormal returns for any given firm, 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 dies unexpectedly, 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 unexpectedly switches his party affiliation from Republican to Democrat that tips the majority in Congress (Jayachandran 2006) and hence firms aligned with the prior majority party no longer enjoy that benefit.

Moreover, given that the death of a sitting Senator/President is rare and that a sitting Senator switching party in the middle of a Congressional term is even rarer, all of these events made "above the fold" headlines and were widely discussed events. Hence, market participants likely received the relevant information in a relatively expedient manner in both of these scenarios and hence, 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 a 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 to the above studies, we have 43 event dates rather than a single one as every state (except for the 7 which retained a single congressional district) announced new maps. The heterogeneity in announcement dates means that in constructing abnormal returns in event-time we are actually collapsing the data

¹⁰ While we can definitively make statements about how the market values these events, we need to be a bit more cautious in stating that these cause changes in real world performance of firms—and even more cautious when it comes to speculating about what mechanisms might explain changes in the real world performance of firms.

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.

That our events occur on different dates for different sets of firms mitigates concerns that singular concomitant event will confound our results. It also helps us isolate the effects of the headquarters' district changing as a given firm's operations located in other states will not be affected on the same date as the headquarters but rather would be affected on the date those other states announced their redistricting instead.

The announcement of a new electoral map, in contrast to the studies above, has less clear immediate implications for firms than a cut-and-dry story about the death of a politician. Moreover, news coverage of new maps is unlikely to make statements about how a firm in question was located in a particular congressional district and reassigned to another, so analysts would have to infer this information on their own, as was the case with firms' closeness to certain politicians/parties in prior studies. This complexity may even require analysts to open up the geographical information systems (GIS) file containing the maps and consider where on a given map a firm is located and how the congressional district changed around it. Additionally, when new map boundaries are released it is not necessarily immediately obvious to individuals without a great deal of familiarity with local demographics to be able to predict the swing in the partisan make-up of a district that the new map portends—although media attention is more likely to focus on this than the location of a firm in a specific district.

Given the need for analysts and media to process the information embedded in new maps to generate insights, we may also be concerned also about how quickly the relevant information content diffuses to the market. While some local, political media outlets focused on state capitols are likely to report on the event, it may take longer for the news to spread that a map has been introduced and is credibly going to be the one that will determine who votes where over the next 10 years. Eventually, larger, national media outlets are likely to carry the story if there are some major changes of broader public interest. This is most likely to happen, and happen quickly, when two major politicians are put into the same district or a senior politician is reassigned towards unfavorable demographics given the media's incentives to run more sensational stories.

Investors that are better able to obtain and process political information benefit from turning that information into profitable trades (Gao & Huang 2016). Hence, a number of major investors have started employing so-called “political intelligence” teams and consultants (Mullins & Pulliam 2011) to process the type of information contained in redistricting maps and

ascertain its relevance to firms. Nevertheless, there remains substantial heterogeneity in how well different market participants are able to access and process political information to assess how it is relevant to firms in which they might invest (Gao & Huang 2016; Christensen et al. 2016). Redistricting maps are likely a particularly difficult type of political information to assess because the information corresponds directly to legislators and their constituencies rather than directly onto industries or firms. In the case of redistricting, analysts would need to download and look at the specific maps and GIS files that states release and then run through information on where firms are located pre- and post-redistricting to be able to understand which firms might be impacted and how. This takes both time and technical expertise to do; however, is something that an enterprising political intelligence team is likely to conduct.

All of this combined suggests that it may take some time to process the maps before analysts and market participants have clear predictions on how they will affect firms.

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.¹¹

First, we address construction of cumulative abnormal returns (CARs) as our key dependent variable. Then, we address the key independent variables that measure meaningful geographic change. Subsequently, we explain the construction of additional independent variables related to firm-politician relationships.

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 implement this using 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

¹¹ Ideally we would have included other redistricting cycles as well; however, we face measurement 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.

day before the relevant event date and running 7 trading days after it. The length of the event window and how we determined the event date both require further discussion.

Determining Event Dates

Determining a specific event date for when each 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 given the messiness of the institutional details of when, how, and who releases new maps in different states.

Our basis for determining event dates is the work shared in Justin Levitt's "All About Redistricting" Project which has a focus on the institutional and legal details of the process for each state.¹² The focal dates in Levitt's work are the formal dates that maps are officially adopted based on legislative histories; however, these dates can occur far after credible maps enter the public domain when it could be clear they will be adopted earlier than that given institutional dynamics. For example, in a state where an independent commission draws the lines, analysts can be nearly certain that upon introduction of those maps that they will be adopted. Alternatively, it is less clear cut in a state where the legislatures debates several maps; however, even then, there typically emerges some breaking point in the process where a concession is made between parties and even though a vote has yet to be formally recorded to adopt a map, analysts can be nearly certain the map will be adopted—and the media may even report on this event. Hence, to get focal dates for the announcement of credible new electoral maps in each state requires looking back in the weeks between formal adoption of a map at the details of the process of when that map entered the public domain and 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 pouring into the state-specific histories by going through data available in state legislative archives, redistricting commissions, and media databases. The dates we use as the event dates are included in a Table in Appendix A.

While we believe we coded these dates accurately, given the process of determining them required researcher judgement calls, it is certainly possible that we miscoded some event dates. The effect of miscoding event dates is a bias towards zero in the CARs we calculate if market participants did not believe the announced maps on the days we selected were credibly the final maps. The logic is simple: if nothing happened on the dates we use for announcement of final maps then the sources of heterogeneity in political geography we explore should have no reason to be correlated with abnormal firm performance around a completely non-relevant date. Alternatively, if a potential map was announced and the market believed there was a high

¹² See these files here: <http://redistricting.lls.edu/2010districts.php> [Last accessed: 4/17/19]

probability it would be the final map the state adopted, but that probability was not 100%, CARs would be biased towards zero because the market may react on that date to the probabilistic expectation that relevant attributes of the revised political geography would be adopted and incorporate those into their expectations of firm performance. Hence, if we use announcements of maps not being seen by the market as dispositive then the market would have smaller reactions to the information contained in them and potentially no reactions whatsoever. The primary implication of this potential concern is that it makes our attempts to demonstrate the existence of a market value of political geography conservative.

Selecting Appropriate Event Windows

In a world where the strong form of the efficient markets hypothesis holds, event windows would be short. An event window of one-day before an event and several days after the event occurs is typical in the literature based on this assumption holding. This is reasonable for information that makes national headlines or that is firm specific.

Given that in our case market participants may take some time to process the implications of new boundaries on firms, we use 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.¹³

To ensure our results do not rest solely on the specific window we choose, we run a robustness check using CARs with different event windows, both shorter and longer. 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.¹⁴ Graphical analysis of how the cumulative abnormal returns evolve for different groups also confirm this.

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—

¹³ 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 in a firm fixed-effect in our estimation).

¹⁴ Tests for the speed of information diffusion will appear in Figure 3 in the body of the paper and Table B1 in the appendix.

such as moving a boundary one block to incorporate an additional few voters whose partisan composition remains identical to that in their prior district—at one extreme. Or these changes can be large—such as moving boundaries in a way that the firm is located in the only block that was in its’ previous district and seeing a wild swing in the partisan make-up of the constituency. A challenge then arises to construct measures of changes in political geography that are meaningful. First, we focus on physical aspects of the political geography and next we focus on partisan aspects of the political geography.

As a baseline for constructing both measures, we employ geographic information systems (GIS) software to locate firms in districts prior to the 2010 redistricting cycle and after the 2010 redistricting cycle. We pull Congressional district boundaries from the Census Bureau’s Master Address File/Topologically Integrated Geographic Encoding and Referencing (MAF/TIGER) database’s shapefiles defining the exact, official borders of Congressional Districts.¹⁵ We pull 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 of the primary ways we can quantify changes in political geography around redistricting is to look at how much change there is in the surface area of a pre-redistricting district corresponding to the surface area of the post-redistricting district. Doing so yields a measure of physical change in overlapping surfaces area for the district in which a firms’ headquarters resides pre- and post- redistricting where 0% would indicate that the district 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 an entirely different district than before where there is no overlap with the prior district.

Figure 1 presents a histogram of this measure of the change in surface area of political geography of firms’ headquarters’ congressional districts around the 2010 redistricting cycle.

¹⁵ For more information on these, see: <https://www.census.gov/geo/maps-data/data/tiger-cart-boundary.html>

¹⁶ We acknowledge that while useful, this is imperfect as COMPUSTAT headquarters addresses are only available for the most recent firm headquarters and not for historical addresses. It is, however, a good proxy given headquarters move infrequently.

¹⁷ One problem with defining firms’ political districts as being located where firms’ headquarters are is that large firms like the ones in our sample typical have operations in multiples locations spanning states and therefore are physically located in multiple congressional districts. This 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 and the effects of those parts of the firm being redistricted would confound any results if those redistrictings occurred simultaneously. Fortunately in our research design, different states announce redistricting maps at different times, which means that redistrictings that happen in different states don’t contaminate the effects of the headquarters district changing unless the event windows around redistricting in other states overlap. Moreover, in Appendix B and Table B2 we run some robustness tests around geographic dispersion of firms.

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 of greater than 75% in the surface area that overlaps between old and new districts. These are the firms where if retaining the same physical boundaries of political geography matters, we should expect to see the biggest shock. Overall, Figure 1 indicates that at least for the districts in which firms are located, the amount of re-shuffling of political geography is both varied and substantial.

<Insert Figure 1 Here>

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. By construction, it only captures the physical surface change rather than anything deeper underlying it. Despite its simplicity and tractability as a measure, it may not capture the most important aspects of a political district. More substantively, if we believe that political geography is destiny for a given politician, then we should probably be more concerned about the partisan composition of constituents within a district (Hill 2003). There are at least two dimensions on which this breaks: (i) the party that voters belong to and (ii) whether or not there is balance or imbalance between the two parties voters belong.

We collect data on the partisan make-up of political districts from *Daily Kos*;¹⁸ we use this data rather than the Cook Partisan Voting Index (PVI) data because it is a better measure of what we are trying to capture. Cook PVI data, while commonly used in the political science literature, is *ex-post* data based on vote shares of elections that have not occurred at the time of the redistricting so that information would not be available to market participants upon redistricting.¹⁹ *Daily Kos*' solution aggregates 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*; they use results from the prior presidential election in the projections. All of this information would in theory be available to market participants when redistricting occurs and 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 that change from one type of partisan district to another. Districts are divided up into four types: Safe Democrat, Leans

¹⁸ See <https://www.dailykos.com/stories/2012/11/19/1163009/-Daily-Kos-Elections-presidential-results-by-congressional-district-for-the-2012-2008-elections> [Last accessed: 4/17/19]

¹⁹Our results, nevertheless, are robust to using this *ex-post* measure.

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 aligned 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.²⁰

<Insert Table 1 here>

Table 1 shows that despite how common re-shuffling of physical aspects of political geography is, that approximately three-quarters of firms remain in a district where both aspects of the partisan make-up of the district—its party-alignment and projected competitiveness—go unchanged. This implies that in our attempts to determine whether or not there is a 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. 5.1% 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. 10.6% of firms' districts transition via reassignment to a safe district after redistricting (with 2.4% switching dominant party as well), while 7.5% of firms' district transition from safe to competitive. Hence, we observe a lot of different types of variation in the partisan nature of firms' districts.

4.3 Other Variables

We will 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 or not firms had a political action committee (PAC).

We construct a dummy variable for whether or not firms retain the same politician before/after redistricting by looking at election return data. While this is a useful proxy for a firm maintaining a geographically based political connection, it is something that market participants have not observed yet as it does not get determined until a future election, but may be a useful proxy for market perceptions of firms that have a high probability of having the same

²⁰ In robustness checks, discussed in Appendix B and shown in Table B3, we consider alternative bounds on the cut-points for intervals used to define safe and competitive districts and substantively the results carry through. We also note that other things determine whether or not a given election is competitive beyond the partisan balance of constituents, but this is a reasonable proxy from a 5,000 foot view not knowing who the specific candidates in a race will be.

politician win their redrawn districts. Market participants will have a reasonable sense of whether or not firms will retain the same politician based on new districts in which incumbents will likely run.

We also construct a measure of whether or not a given firm is politically active or not. We use data from the Federal Elections Commission (FEC) cleaned by the Center for Responsive Politics to create a dummy variable for whether or not a firm had a PAC in the election cycle prior to redistricting. 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 a 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. Hence, we begin our analysis with tests of how changes in physical aspects of political geography explain abnormal returns, shown in Table 2.

<Insert Table 2 Here>

In column 1, we regress our continuous measure of physical overlap—the fraction of the pre-redistricting area of a district that remains in the firms’ district post-redistricting—on CARs. In column 2, we break the continuous variable into dummy variables capturing those firms that experienced changes in the area overlapping old-and-new districts relative to the pre-redistricting district in 10% intervals of overlap with the reference category being firms who experience less than 10% change. A graphical depiction of this regression result appears in Figure 2. In all of this we find a weak tendency towards changes in physical aspects of political geography having a negative effect on firms’ value. It is not dispositive: the only statistically significant negative effects are shown for firms whose new districts are 70% to 90% different than their previous ones.

<Insert Figure 2 Here>

Our results suggest that political geography has market value for firms under certain circumstances. Next we characterize exactly how further—moving beyond measures of physical aspects of political geography towards more nuanced ones that capture partisan realities.

5.2 Estimating the Effect of Partisan Components of Political Geography

Table 3 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.

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 0.6%. 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 were reassigned to a safe district from a competitive district, while the other were reassigned to a competitive district from a safe one, the difference in valuations would be 2.2% percent.²¹ These are our baseline results.

Column 2 tests whether or not 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% to the value of firms relative to staying in a district that did not change its partisanship. 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.²² We implore caution in that interpretation on various grounds: (i) later results will show that this finding is not robust; and (ii) in this regression a Wald test of whether the partisan coefficients on reassigned to Democrat versus reassigned to Republican are distinguishable from each is not significant at conventional levels (as $p=0.15$).

²¹ 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.

²² Another competing expectation from Kim, Pantzalis, and Park (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 on Democrat are consistent with that expectation but on Republican are inconsistent; however, they 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.

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. That is, we might be concerned that there is 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. While the sign on being reassigned from a safe district of one party to a safe district of the other party is negative—which we might expect if extreme reassignments proves particularly not helpful—it is statistically indistinguishable from zero.²³

Finally, 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 remains 2.2%.

<Insert Table 3 Here>

<Insert Figure 3 Here>

Figure 3 shows the main effect graphically over event-time, averaging cumulative abnormal returns for firms that are reassigned to safe districts and those that are reassigned to competitive ones. The graph shows a clear divergence between the two groups after the event, which increases over time and stabilizes after day 5. These dynamics are consistent with the idea that it takes some time for information to diffuse to the markets and for analysts to process the locations of firms in the new political maps. We are cautious to not over-interpret this figure.²⁴

A concern might remain that the results establishing the value of partisan aspects of political geography could be confounded by other things the literature on business and politics

²³ We caution against over-interpretation that there is no substantive effect here as well as the effect may be insignificant for both statistical and substantive reasons. Statistically, we may have limited power given small samples. Substantively, we may have confounding effects between the positive effect of reassignment to a safe district but negative effects from reassignment to the wrong party—which we attempt to disaggregate further in Column 4 by including additional measures of party control.

²⁴ Note that since this is a financial market-based event study, by construction the mean cumulative abnormal return (CAR) for all groups should be zero in the pre-treatment period if the efficient markets hypothesis hold (which is a core assumption for financial market based event studies efficacy). Hence, we do not need to concern ourselves with examination of divergent pre-treatment trends in CARs as we would if we were conducting a policy outcome based event-study where we were looking at a time-series of some aggregate state-level policy variable for treated and untreated states. Moreover, a pre-treatment time-series of cumulative abnormal returns from day -1 to the day in event-time would have no meaningful interpretation if the event-time went negative from when the series starts since by construction CARs go from a start date to some positive date in the future. Note also, that this figure does not include state fixed-effects as it is simply displaying the average of CARs for all firms; however, Appendix Table B1 includes the fixed effects in regressions against CARs over different event windows starting at -1 and going up to 10.

finds important, such as political-connections and money-in-politics. For example, reassignment-to-safety could simply reflect a higher probability that a firm retains an important connection to the politician who represents its' headquarters' district. With respect to money, reassignment-to-safety may simply be a function of efforts to obtain that outcome. Next we provide evidence that our main results about changes in the political geography of a district are not explained away by these potentially confounding factors.

Partisan Components of Political Geography in the context of Political Connections

Table 4 shows how our baseline results from Table 3 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.

We can think of the same-politician variable in our regressions almost as-if it measures this type of political connection. The reality is more complex, as whether a firm will retain the same politician is determined at a future date, the next election. Moreover, this variable is not directly observable to market participants at the time new district maps are announced. Analysts and media could nevertheless, infer it probabilistically. In fact, many news articles speculate on this very thing in their coverage of redistricting. In this sense, our variable might best be thought of as a rough 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 (which isn't far-fetched given high incumbent reelection rates). These measurement nuances necessarily temper our interpretation of results.

<Insert Table 4 Here>

In column 1 of Table 4, we simply examine whether or not there is 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).²⁵

Column 2 of Table 4 shows our baseline regression with the same politician variable as a control. The main results from Table 3—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.

²⁵ 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 posts where the effect may be strongest as that work does.

In column 3, we add 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 (67% of our sample) and those firms who will not.²⁶ The firms that end up with new politicians appears to be driving our main results, the net effect is 2.9% for them vs the 2.2% baseline effect for the total population. We obtain this 2.9% net effect by taking the difference between the coefficient estimates for the effect of reassignment-to-safe (1.1%) and the coefficient for reassignment-to-competitive (-1.8%).

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

Table 5 takes on the role of money-in-politics vis-à-vis political geography. It demonstrates that are 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—and in particular that 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 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 outcomes and has priced that in prior to our event window. We note that if the latter were true, this would bias the average effects in our main results towards zero given our identifying assumption relies on the announcement of new maps being a surprise to market participants.

In Column 2, we show that our main results on being reassigned to a safe district and being reassigned to a competitive district remains unaltered when we include the dummy for whether or not a firm had a PAC as a control. This regression model tests for whether or not some fraction of the positive coefficient on reassignment-to-safe is driven by a firm being politically active. We find no evidence that it contaminates our results. In Column 3, we test for

²⁶ Note that 67% is lower than the approximately 95% reelection rates of incumbents (Friedman & Holden 2009).

²⁷ Note that we also conducted the exact same tests but constructed a variable for whether or not a firm's PAC had 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 or not 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.

whether the market reacts differently to various shifts in political geography depending upon a firm's political activity. We find no evidence it does. The takeaway from Table 5 is that political geography, specifically, the level of partisan competition, stands independently of the role of money-in-politics.

<Insert Table 5 Here>

5.3 Validity and Robustness

In this section, we discuss how we know that our main finding—that firms benefit from being reassigned to safe districts and suffer from being reassigned to competitive districts—is valid and robust. We discuss the validity of our results vis-à-vis our key identification assumptions and a placebo test aimed at demonstrating they hold to the extent we can make such an assessment. Following that discussion of validity we discuss robustness of our results to other potential concerns not addressed earlier, mostly related to measurement of key variables, but also looking into sub-samples of the data.

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 when* credible final maps enter the public domain for each state. We would ideally like to know how plausible both the *new information* and *timing* components of this identifying assumption are—and what the implications are if we got them wrong. In both cases, if they do not hold (or only partially hold) then we would expect a null result (or a bias towards zero). This is precisely why we stated in our introduction that our goal was to establish the *existence of an effect* of political geography rather than the precise *magnitude of the effect* of political geography.

We cannot test directly whether there is *new information* content that market participants obtain but we can trace through the logic of what would happen if there was not. First, if there were no new information content, then political intelligence analysts among the market participants would not pay attention to the release of new maps at all as there would be nothing new to infer. We find this extremely implausible as redistricting is the fundamental institution determining what politicians get elected where, but if it were true, then we would expect a null result in all of our regressions. Another risk to our identification is that despite new maps being released there is little *new information* content in them. Little new information content is possible if a majority of market participants follow the redistricting process closely enough to try to forecast which firms will be re-districted and how prior to the release of new maps and invested probabilistically based on those forecasts linking firms to politics. This is somewhat

plausible if there is value from an investment strategy (our primary results would support) suggesting there are systematic costs and benefits to firms depending upon outcomes, but unlikely given findings about differential quality political intelligence groups (Gao & Hung 2016). Nevertheless, some of these forecasts could also be quite accurate at least vis-à-vis some districts as it is not only possible, but somewhat likely that as multiple potential maps are considered and debated that compromises might be made in certain districts rather than others. Some investors might trade on this before the actual event date around the release of the new map in the headquarters state. If this were true, then we would expect a smaller reaction than we would otherwise find given the lack of *new information* available on the dates that credible final maps enter the public domain vis-à-vis some firms in some districts. Hence, if this component of the identifying assumption only held weakly, as just described, it would make it more difficult for us to find *the existence of an effect of political geography* at all given the misidentification of timing of the market absorbing new information. This reasoning is similar to Fisman's (2001) for why his event study results are biased towards zero.

This leads us naturally into a discussion of the second major component of our key identification assumption—which is essentially that we got the *timing* of the release of credible new political maps in each of the 43 states right. Relying on the discussion of *new information* content above, it should be clear that misidentifying the date of the release of new maps would yield a null result as we could not plausibly expect there to be *new information* affecting only firms redistricted in specific ways on alternative dates. One way to validate that we did get the event dates right then is to consider a placebo test on alternative dates where we would expect to find null effects on our main treatment groups as the alternative timing should not be associated with *new information* being released on those dates. We conduct such placebo tests, shifting all event dates for hypothetical releases of credible final maps by 12, 9, 6, and 3 months prior to the actual event dates, for our baseline regression found in Table 3, Column 1. The results appear in Table 6.²⁸ 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 or 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 the coefficients in question run in the opposite direction of those we find around the actual event dates.

<Insert Table 6 – Placebo Test results>

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.

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 our results have strong validity and if anything might be conservative and represent a lower bound for the true effect.

Additional Robustness Checks

Appendix B (for online publication) shows our how our results remain intact, and are in some cases stronger, when considering several different robustness checks aimed at understanding if our assumptions are valid and/or if measurement concerns might be driving our results. In particular, our results are robust when we perform the following checks: (i) re-running our regressions using different event windows in calculating CARs; (ii) incorporating into our analysis a measure of geographic concentrations of firms around their headquarters from Garcia & Norli (2012) to assess if the effect is stronger for firms that are more rooted in their headquarters district; (iii) using an alternatively defined measure of safe versus competitive districts with a different threshold for vote margins; and, (iv) re-running our regressions on sub-samples of the data depending on who draws the final map in a given state as different sets of institutions could lead to different outcomes and information environments.

6 DISCUSSION

The primary goal of this research was to establish whether or not 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. We have yet to put the magnitude of the effects we uncover into context nor have we begun to explain why firms might benefit from safer districts and suffer from more competitive districts. We do that and put our findings into a broader context here.

6.1 Magnitude of Results

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 things that determine business-government relationships. In particular, this leads to questions about how big is the value of political geography relative to the value of political connections found in other contexts—and how can we put the magnitude of the effects of reassigning political geography into context.

In absolute terms, a net impact of 2.2% on firms' values is large. For an S&P500 firm, which must have a minimum market capitalization over \$6B, a difference of 2.2% equates to \$130M 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 address directly 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.²⁹

The value of political geography we establish is also quite large relative to the value of political connections at least 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 very little and next to nothing even if connected to the Vice President (Fisman et. al 2012). Of course, these pale in comparison to the largest estimate of value of political connections in a developing country context with rampant corruption and cronyism as Fisman (2001) estimated the firms best connected 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 2.9% 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 Explaining Why Partisan Aspects Matter

While pinpointing exactly how and why firms benefit from safe districts relative to competitive districts in a causally well-identified way remains beyond the scope of this paper, potential mechanisms warrant preliminary consideration because (i) without plausible mechanisms we might question the validity of our results and because (ii) it may help guide future research. Importantly, in doing so, we should consider explanations that hold both when firms experience turnover in politicians as well as when firms keep the same politician given the competitiveness of districts may affect firms in both scenarios. One reason that competitive districts might be more harmful for firms is that these firms may be less likely to retain the same politicians and as Snyder (1992) shows long-term relationships with politicians are valuable. Nevertheless, this explanation alone is probably insufficient, as the effect of political competition we find, although dampened, may still matter for firms that retain the same politician post-redistricting. A second explanation for why safer districts might matter that could apply when

²⁹ 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 with changing underlying demographics. Moreover, firms would have to be prepared to move every 10 years with redistricting cycles—and the lead time necessary to move would require several of those 10 years without *ex ante* knowledge of future political maps. We note also if anticipatory moves of firm headquarters happened, this would attenuate our results, pushing it towards zero.

firms retain their politicians is that politicians in districts that are safer have greater slack vis-à-vis their constituents consistent with Mayhew (1974) who first pointed out how electoral incentives play a major role in structuring the behavior of legislators. Following from this logic, we might expect politicians in safer districts to be able to get away with more than politicians who likely will have a tougher time getting re-elected.³⁰ Finally, an additional potential mechanism consistent with our results is that differences in district competitiveness imply differences in the uncertainty over the range of politicians a district may elect.³¹ Hence there are numerous potential mechanisms for how political geography fundamentally determines how politicians represented or selected by a given district interface with firms.

6.3 Revisiting Capture Theory

Our results, regardless of the exact mechanism underlying them, 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 a then predominant public interest view of how governments function, positing that politicians focus on policies that benefit incumbent firms, rather than on what matters to their constituents. Put simply: firms demand policies and receive them in return for a quid pro quo with the politicians, a later view formally modeled by Grossman and Helpmann (2001) in various configurations. Our results suggest that the susceptibility of a politician to capture and the degree to which they can be captured depends on the characteristics of their district. Whether or not the capture model of thinking or the public interest model of thinking about how politicians interface with firms predominates is partly determined by political geography—and, in particular, the level of competition that a given politician faces in his electoral district. Politicians that face a larger degree of electoral competition may fit better with a public interest model of regulation, while politicians less constrained by electoral competition may fit better with a capture model of regulation. In essence, firms are partially held hostage by the voters in the political districts in which they operate—as voters provide constraints on politicians keeping them from being infinitely vulnerable to corporate influence. There has been some theoretical consideration of supply side

³⁰ There are a number of reasons why we might expect this. For example, 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. Alternatively, politicians in tighter districts might simply have less time: 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 (e.g. see Daley and Snowberg 2009).

³¹ This is distinct from the large literature on policy uncertainty, e.g. see Baker et al. (2016), Julio & Yook (2012).

constraints in models of capture by Peltzman (1976) and Denzau and Munger (1986). Our empirical work in this paper suggests these probably deserve greater consideration moving forward. Political geography bounds firms' ability to influence politicians. Likewise, political geography bounds politicians' capacity to help firms.

All of this suggests that perhaps we should be thinking about business-government relationships more through a relational contracting lens (e.g. Baker, Gibbons, and Murphy 2002). That is, firms make informal agreements with politicians to look out for each others' interests, in which both are susceptible to potential shocks in the relationships due to changes in external constraints. Hence, political representation of firms is not just about money, power, or ideological alignment, but also about political geography, which is an underlying determinant of the degree to which each of those factors matters. Just because a firm stays connected to the same individual politician does not mean the nature of that relationship cannot change: as political geography changes, so does the relationship.

7 CONCLUSION

In this paper, we bridge the work of political scientists focused on representation with the work of economists focused on capture to show that political geography has a value to firms.

We have established that there is a market value to the political geography in which firms reside. In particular, we have shown that firms see an increase in market value when reassigned to districts with larger partisan divides and suffer when reassigned to more competitive districts. We believe these results stem from how political geographies shape or change the nature of relational contracts between firms and politicians, who also have relational contracts with their constituencies that constrain what they can do.

Our results have large real world implications. The shape of political districts changes regularly due to population shifts, census reapportionments, and gerrymandering efforts, among other factors. Our paper shows that all these changes have effects on firms' value. For this reason, the recent Supreme Court decisions in *Benisek vs Lamone* and *Common Cause vs Rucho*—which declared that political gerrymandering is constitutional and beyond the reach of the courts (but not legislatures, referenda, or Constitutional amendments)—will determine what type of relationships firms will have with politicians in more fundamental ways than the *Citizens United* decision (unbounding independent expenditures on elections) did.

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8 FIGURES AND TABLES

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

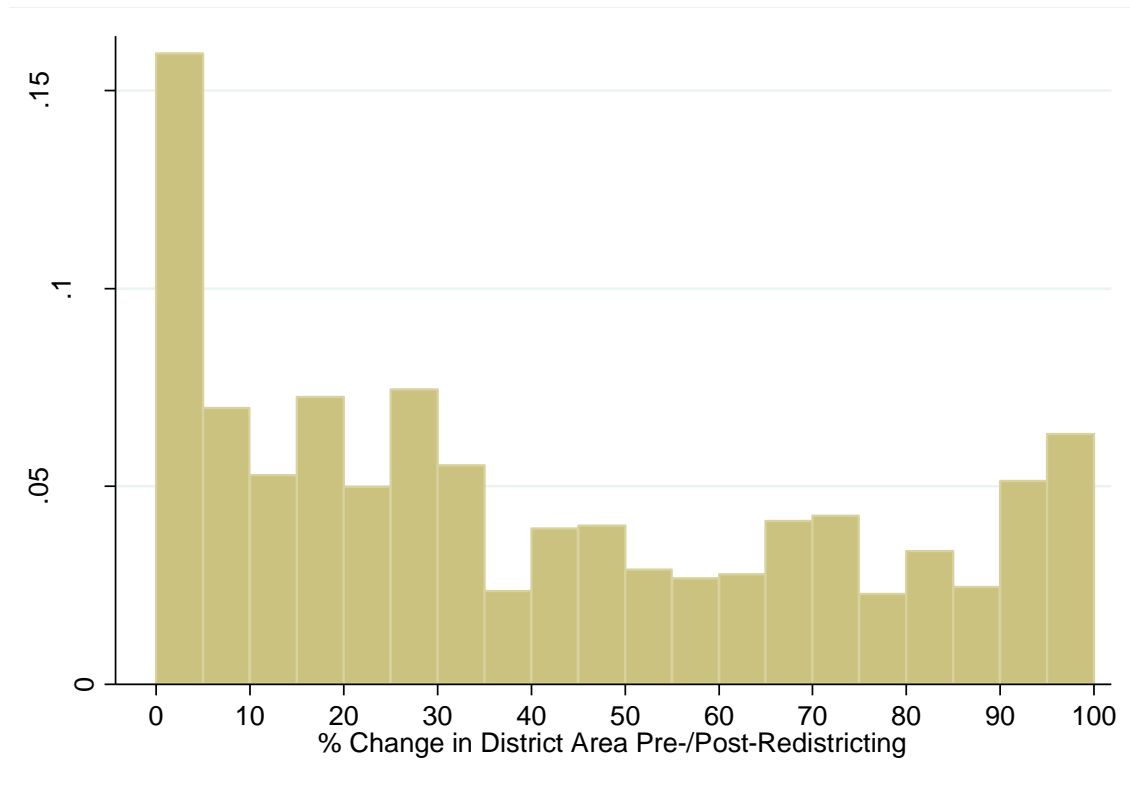


Figure 2 – Plot of Coefficient for Change in Geographic Overlap Dummies

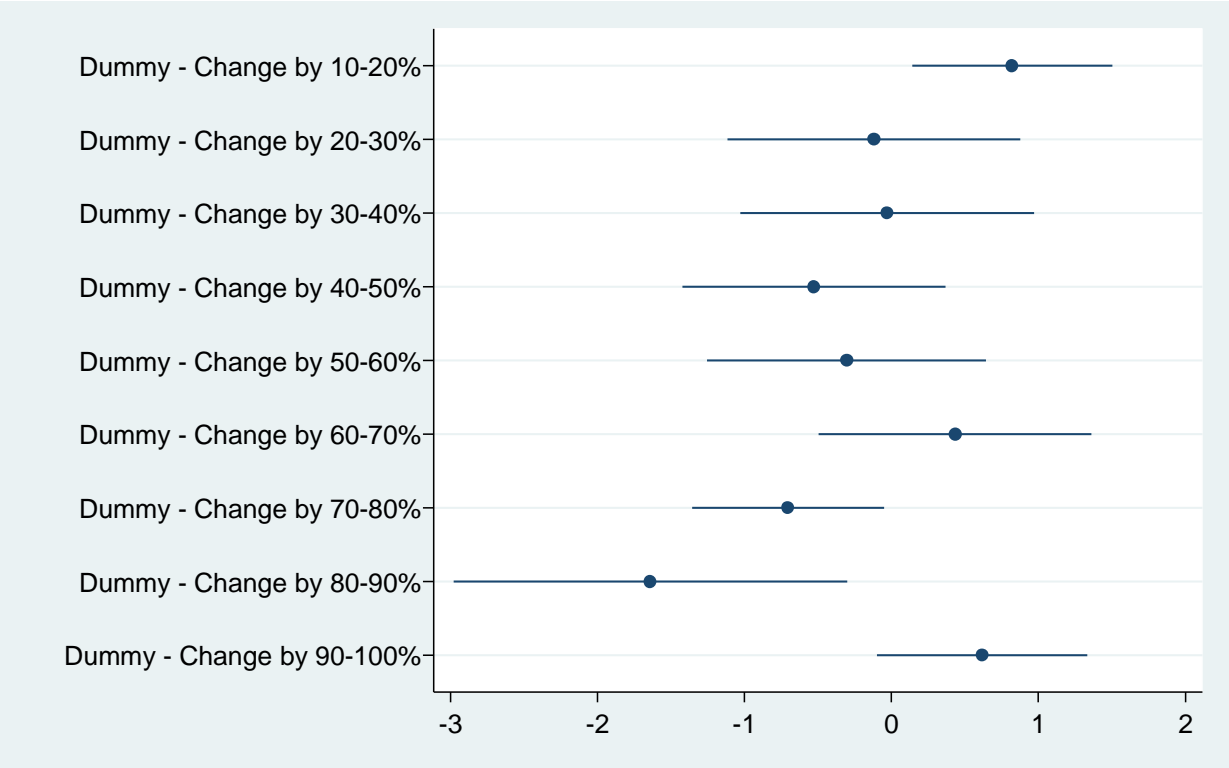


Figure 3 – Plot of Mean CARs in Event Time for all Firms Experiencing Similar Shifts in Nature of Partisan Competition in their Headquarters’ Congressional District



Table 1 – Transition Matrix for Partisan Make-up of District before/after Redistricting
After Redistricting

		Safe Democrat	Leans Democrat	Leans Republican	Safe Republican	
<i>Before Redistricting</i>	Safe Democrat	49.5%	3.5%	1.1%	1.6%	55.7%
	Leans Democrat	2.1%	5.2%	3.0%	0.4%	10.7%
	Leans Republican	3.5%	2.7%	4.6%	2.1%	12.9%
	Safe Republican	0.9%	0.1%	2.8%	16.9%	20.7%
		56.0%	11.6%	11.4%	21.0%	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.

Table 2 – Effects of Change in Geographic Overlap on CARs

Dependent Variable	Cumulative Abnormal Return (%)		Fraction of observations in Data
	(1)	(2)	
Percentage Change in Geographic Overlap	-0.004 (0.004)		
Dummy - Change by 10-20%		0.775* (0.412)	12.5%
Dummy - Change by 20-30%		-0.075 (0.626)	12.4%
Dummy - Change by 30-40%		0.200 (0.609)	7.9%
Dummy - Change by 40-50%		-0.389 (0.512)	8.0%
Dummy - Change by 50-60%		-0.420 (0.550)	5.6%
Dummy - Change by 60-70%		0.359 (0.526)	6.9%
Dummy - Change by 70-80%		-0.882** (0.388)	6.5%
Dummy - Change by 80-90%		-1.468* (0.802)	5.8%
Dummy - Change by 90-100%		0.659 (0.459)	11.5%
State-Fixed Effects	Yes	Yes	
N	2,541	2,541	

Notes:

Robust standard errors clustered by state are reported.

* indicates significance at the .1 level, ** at the .05 level, and *** at the .01 level.

Cumulative Abnormal Returns are estimated for an event window of (-1, +7) using a Fama-French 3 Factor model and a 250 day estimation window.

Dummy variables are calculated based on percentage changes in how much of the geography between a firms' pre-redistricting and post-redistricting district overlap with higher numbers indicating greater change.

Table 3 – Effect of Changes in Constituent Make-up within Political Geographies on CARs

Dependent Variable	Cumulative Abnormal Return (%)			
	(1)	(2)	(3)	(4)
Dummy - Reassigned to Safe District	0.602 (0.592)		0.651 (0.595)	0.677 (0.703)
Dummy - Reassigned to Competitive District	-1.587*** (0.569)		-1.587*** (0.568)	-1.534*** (0.587)
Dummy - Reassigned to Safe District of Other Party			-0.224 (0.959)	-0.776 (1.101)
Dummy - Reassigned to Democratic District		0.287 (0.400)		-0.073 (0.542)
Dummy - Reassigned to Republican District		1.010** (0.407)		0.917* (0.545)
State-Fixed Effects	Yes	Yes	Yes	Yes
N	2,541	2,541	2,541	2,541

Notes:

Robust standard errors clustered by state are reported

* indicates significance at the .1 level, ** at the .05 level, and *** at the .01 level

Cumulative Abnormal Returns are estimated for an event window of (-1, +7) using a Fama-French 3 Factor model and a 250 day estimation window.

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". Districts defined as belonging to a party (Democratic or Republican) if more than 50% of voters lean towards it.

Table 4 – Baseline Results adding Same Politician

Dependent Variable	Cumulative Abnormal Return (%)			
	(1)	(2)	(3)	(4)
Dummy - Reassigned to Safe District		0.656 (0.598)	1.074 (0.726)	0.723 (0.716)
Dummy - Reassigned to Competitive District		-1.529*** (0.559)	-1.797*** (0.772)	-1.481** (0.571)
Dummy - Reassigned to Safe District of Other Party				-0.743 (1.093)
Dummy - Reassigned to Democratic District				-0.077 (0.551)
Dummy - Reassigned to Republican District				0.906 (0.544)
Dummy - Same Politician	0.220 (0.260)	0.194 (0.239)	0.277 (0.216)	0.180 (0.244)
Interaction Dummy - Same x Reassigned to Safe			-1.152 (0.742)	
Interaction Dummy - Same x Reassigned to Competitive			0.882 (1.143)	
State-Fixed Effects	Yes	Yes	Yes	Yes
N	2,541	2,541	2,541	2,541

Notes:

Robust standard errors clustered by state are reported

* indicates significance at the .1 level, ** at the .05 level, and *** at the .01 level

Cumulative Abnormal Returns are estimated for an event window of (-1, +7) using a Fama-French 3 Factor model and a 250 day estimation window.

District Safety is defined as a 10% total margin so partisan balance exceeding either 45/55 or 55/45 for safe and competitive falling within those bands.

Districts defined as belonging to a party (Democratic or Republican) if more than 50% of voters lean towards it.

Same politician is an indicator taking a value of 1 when the firm's district is represented by the same politician

Interaction Dummies are dummy variables representing interactions of the above dummy variables.

Table 5 – Baseline Results adding Measure for Politically Active Firms

Dependent Variable	Cumulative Abnormal Return (%)		
	(1)	(2)	(3)
Dummy - Reassigned to Safe District		0.602 (0.593)	0.542 (0.659)
Dummy - Reassigned to Competitive District		-1.599*** (0.564)	-1.743*** (0.686)
Dummy - Has PAC	-0.307 (0.363)	-0.326 (0.366)	-0.407 (0.429)
Interaction Dummy - Reassigned to Safe x Has PAC			0.348 (1.115)
Interaction Dummy - Reassigned to Competitive x Has PAC			0.723 (0.938)
State-Fixed Effects	Yes	Yes	Yes
N	2,541	2,541	2,541

Notes:

Robust standard errors clustered by state are reported

* indicates significance at the .1 level, ** at the .05 level, and *** at the .01 level

Cumulative Abnormal Returns are estimated for an event window of (-1, +7) using a Fama-French 3 Factor model and a 250 day estimation window.

Has PAC is a dummy taking on a value of 1 if a firm has an active Political Action Committee prior to redistricting.

District Safety is defined as a 10% total margin so party balance exceeding either 45/55 or 55/45 for safe and competitive falling within those bands.

Interaction Dummies are dummy variables representing interactions of the above dummy variables.

Table 6 – Placebo Tests varying Event Date in Baseline Regression

Dependent Variable <i>Placebo relative to Event Date in HQ State</i>		Placebo Cumulative Abnormal Return (%) offset by...			
		<i>-12 Months</i>	<i>-9 Months</i>	<i>-6 Months</i>	<i>-3 Months</i>
		(1)	(2)	(3)	(4)
Dummy - Reassigned to Safe District		0.233 (0.615)	0.153 (0.543)	0.476 (0.846)	-0.391 (0.624)
Dummy - Reassigned to Competitive District		0.475 (0.573)	-0.799 (0.522)	0.881 (0.804)	-0.894 (0.673)
State-Fixed Effects		Yes	Yes	Yes	Yes
N		2,309	2,487	2,379	2,383
Wald Test Results					
<i>Is the benefit of moving to a safe district distinguishable from cost of moving to a competitive district? B1=β2?</i>	<i>F-Stat</i>	0.06	1.58	0.18	0.22
	<i>p-value</i>	0.80	0.22	0.67	0.64

Notes:

Robust standard errors clustered by state are reported

* indicates significance at the .1 level, ** at the .05 level, and *** at the .01 level

Placebo Cumulative Abnormal Returns are estimated for an event window of (-1, +7) using a Fama-French 3 Factor model and a 250 day estimation window.

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".

APPENDIX A (FOR INCLUSION WITH PUBLICATION) – STATE SPECIFIC DATES

Table A1 – Dates Credible Maps are Released

State	Date Credible Map Released
Alabama	5/19/2011
Alaska	-
Arizona	10/3/2011
Arkansas	4/13/2011
California	6/10/2011
Colorado	11/10/2011
Connecticut	1/13/2012
Delaware	-
Florida	2/2/2011
Georgia	8/22/2011
Hawaii	-
Idaho	10/17/2011
Illinois	5/27/2011
Indiana	4/11/2011
Iowa	3/29/2011
Kansas	6/7/2011
Kentucky	2/6/2011
Louisiana	3/20/2011
Maine	9/27/2011
Maryland	10/4/2011
Massachusetts	11/7/2011
Michigan	6/17/2011
Minnesota	2/21/2012
Mississippi	12/19/2011
Missouri	4/27/2011
Montana	-
Nebraska	5/5/2011
Nevada	10/14/2011
New Hampshire	3/22/2012
New Jersey	12/23/2011
New Mexico	12/29/2011
New York	3/6/2012
North Carolina	7/25/2011
North Dakota	-
Ohio	12/14/2011
Oklahoma	4/14/2011
Oregon	6/29/2011
Pennsylvania	12/13/2011
Rhode Island	12/19/2011
South Carolina	7/26/2011
South Dakota	-
Tennessee	1/6/2012
Texas	2/28/2012
Utah	10/17/2011
Vermont	-
Virginia	1/10/2012
Washington	1/1/2012
West Virginia	8/5/2011
Wisconsin	7/8/2011

APPENDIX B – FOR ONLINE PUBLICATION – ROBUSTNESS

This Appendix contains additional analysis aimed at two types of robustness. Some of these are aimed at robustness towards identification assumptions in our statistical model (like how long it takes until the market absorbs information in the new maps) and others are aimed at robustness towards potential measurement concerns in our data.

Robustness to Altering Abnormal Return Windows

In our regressions in the body of the paper, we chose a window of -1 trading days to +7 trading days after the release of credible final political maps in the 2010 redistricting cycle. That (-1, +7) window seemed appropriate since we expected it to take a bit longer for implications of these on firms to proliferate via trades than if we were looking for response to headline news.

Some readers, however, may be interested in how our results vary if we change the event window across which we accumulate the cumulative abnormal returns that compose our dependent variable throughout. They may be interested in how robust they are to shorter and longer windows. Moreover, they may also be interested in such results to understand how long it might take until an effect appears given that may reflect the time it takes to process the information content of new political maps to glean implications for firms embedded in them.

We test directly for the robustness of our core results in Table 3, Column 1 to modifying the event window over which cumulative abnormal returns are aggregated from (-1, +7), used in our baseline estimation, to others that are both shorter and longer—and one that does not include a pre-treatment day (in case news of the maps leaked). The results appear in Table B1 and suggest that our primary findings are robust to different cumulative abnormal returns windows. Also as expected given the time required to process redistricting maps into implications for firms, 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. These results are not surprising given both the theoretical rationale for this and the evolution over time as CARs get longer; moreover, we saw a similar pattern in Figure 3 of the paper. Finally, it is worth noting that the magnitudes of the results remain relatively stable over event windows close to the baseline window we use.

<Insert Table B1>

Robustness to Geographic Concentration of Firms in Headquarters Location

Another concern that readers may have had is that we are only testing in our regressions throughout the paper for effects of districts changing around the headquarters locations of the

firms when some firms may be more geographically dispersed or other firms may have a headquarters in name only, e.g. Boeing does most of its work in Washington state but is legally headquartered in Chicago. To that end we'd expect that firms that don't 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. To that end we use the data constructed by Garcia and Noorli (2012) on the geographic dispersion of firms—and from it construct a measure of whether or not the firm does the majority of its business near its headquarters. This is a dummy variable that takes on a value of 1 if the answer is yes—and no otherwise.

The results of tests incorporating this variable on the headquarters being the main location for the firm appear in Table B2. They show, in Columns 2 and 3, that to the extent that we can measure that a firm's headquarters is an important location and control for it that the magnitude of our results are larger 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.

<Insert Table B2>

Robustness to Different Cut-Points for Safe Districts

Another measurement question, we could have asked throughout and might be concerned about robustness to is how we define districts as being safe or competitive. Throughout the paper, we use a 55/45 vote margin to define this—where firms inside that range are considered to be in competitive districts and those outside that range are considered to be in safe districts. Alternatively, we could have defined that range as 52.5/47.5—or any number of other bands. We may be concerned about whether or not our results remain robust to this narrower margin defining which districts are safe versus which are competitive. The results in Table B3— which replicate Table 3 exactly but switch the definition of safe/competitive districts to the narrower range—show that our results remain robust to the alternatively defined measure.

<Insert Table B3>

Limiting Sample to Sets of Redistricting Institutions

Finally, some readers may be curious about how or if who draws the lines influences our results—recalling that in different states different institutional rules and arrangements mean that the final credible maps are drawn by different redistricting institutions. 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 results probing at this question may prove an interesting point of inquiry it is difficult to make strong theoretical predictions about what they might yield *a priori*. We might roughly expect stronger results in order from (i) to (iv) descending, since maps released are most binding in that order and therefore represent ‘sharper events’.³² Alternatively, we might also expect no relationship as perhaps maps drawn by some institution, are more predictable *a priori* and hence less consequential on abnormal market returns as it is possible that it is more predictable what courts and independent commissions might do. Results of our core regression run on different sub-samples by redistricting institution appear in Table B4. They appear to be more consistent with the no strong relationship hypothesis. We caution against over-interpretation of them given relatively small sub-samples, but to the extent that we can glean anything from these results we might say that it is worst for firms to be redistricted to a competitive district when the maps are drawn by one dominant party and that firms appear to benefit most from being reassigned to a safe district by an independent commission.

<Insert Table B4>

³² For example court drawn maps a the result of a legislature’s failure to make a timely decisions cannot be contested and in practice are not appealed so these are 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 B1 –Tests of Alternative Event Windows

Dependent Variable	Cumulative Abnormal Return (%) with Event Window as...										
	<i>Event Window for CAR</i> <i>Note on Event Window</i>	(0,7)	(-1,1)	(-1,2)	(-1,3)	(-1,5)	(-1,6)	(-1,7)	(-1,8)	(-1,9)	(-1,10)
	No Pre-Day (1)	Shorter than Baseline					Baseline (7)	Longer than Baseline			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	
Dummy - Reassigned to Safe District	0.501 (0.543)	-0.018 (0.239)	0.218 (0.272)	0.060 (0.288)	0.257 (0.345)	0.500 (0.488)	0.602 (0.592)	0.169 (0.524)	0.513 (0.491)	0.648 (0.594)	
Dummy - Reassigned to Competitive District	-1.651*** (0.496)	-0.451 (0.462)	-0.645 (0.456)	-0.595 (0.541)	-0.767 (0.505)	-1.147** (0.542)	-1.587*** (0.569)	-1.472** (0.654)	-1.193* (0.594)	-1.294** (0.591)	
State-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
N	2,541	2,541	2,541	2,541	2,541	2,541	2,541	2,541	2,541	2,541	
Wald Test Results											
	<i>F-Stat</i>	0.77	2.52	1.02	2.65	4.74**	6.70**	2.96*	4.25**	4.06	
<i>Is the benefit of moving to a safe district distinguishable from cost of moving to a competitive district? B1=82?</i>	<i>p-value</i>	0.38	0.12	0.32	0.11	0.04	0.01	0.09	0.05	0.050	

Notes:

Robust standard errors clustered by state are reported

* indicates significance at the .1 level, ** at the .05 level, and *** at the .01 level

Cumulative Abnormal Returns are estimated for an event window that varies as above using a Fama-French 3 Factor model and a 250 day estimation window.

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".

Table B2 –Testing for Role of Geographic Concentration of Firms around Headquarters

Dependent Variable	Cumulative Abnormal Return (%)		
	(1)	(2)	(3)
Dummy - Reassigned to Safe District		0.736 (0.632)	0.192 (1.115)
Dummy - Reassigned to Competitive District		-1.779*** (0.635)	-2.552** (1.138)
Dummy - HQ Main Location	-0.384 (0.356)	-0.430 (0.352)	-0.584 (0.368)
Interaction Dummy - Reassigned to Safe x HQ Main Location			0.714 (1.056)
Interaction Dummy - Reassigned to Competitive x HQ Main Location			1.258 (1.584)
State-Fixed Effects	Yes	Yes	Yes
N	2,424	2,424	2,424

Notes:

Robust standard errors clustered by state are reported

* indicates significance at the .1 level, ** at the .05 level, and *** at the .01 level

Cumulative Abnormal Returns are estimated for an event window of (-1, +7) using a Fama-French 3 Factor model and a 250 day estimation window.

HQ Main Location' is a dummy variable taking on a value of 1 if a firm's financial statements' text indicates that most of the firms business occurs in physical proximity to the firm's headquarters location based on data scraped in Garcia & Øyvind (2012).

District Safety is defined as a 10% total margin so party balance exceeding either 45/55 or 55/45 for safe and competitive falling within those bands.

Interaction Dummies are dummy variables representing interactions of the above dummy variables.

Table B3 –Table 3 Regressions with Alternative Margin on Competitive/Safe Districts

Dependent Variable	Cumulative Abnormal Return (%)			
	(1)	(2)	(3)	(4)
Dummy - Move to Safe District	0.491 (0.441)		0.442 (0.618)	0.416 (0.622)
Dummy - Move to Competitive District	-1.124* (0.603)		-1.125* (0.603)	-1.085* (0.623)
Dummy - Move to Safe District of Other Party			0.074 (0.410)	-0.434 (0.514)
Dummy - Move to Democratic District		0.287 (0.400)		0.225 (0.615)
Dummy - Move to Republican District		1.010** (0.407)		0.996* (0.577)
State-Fixed Effects	Yes	Yes	Yes	Yes
N	2,541	2,541	2,541	2,541

Notes:

Robust standard errors clustered by state are reported

* indicates significance at the .1 level, ** at the .05 level, and *** at the .01 level

Cumulative Abnormal Returns are estimated for an event window of (-1, +7) using a Fama-French 3 Factor model and a 250 day estimation window.

District Safety is defined as a 10% total margin so party balance outside a 47.5/52.5 or 52.5/47.5 split defines a "safe district" while party balance falling within those ranges define "competitive districts". Districts defined as belonging to a party (Democratic or Republican) if more than 50% of voters lean towards it.

Table B4 –Baseline Regression, Limiting Sample to Sets of Redistricting Institution

Dependent Variable <i>Sample</i> <i>[Limited to Maps Drawn by...]</i>	Cumulative Abnormal Return (%)			
	<i>Courts</i>	<i>Independent Commission</i>	<i>Strong Partisan Legislature</i>	<i>Bi-Partisan Legislature</i>
	(1)	(2)	(3)	(4)
Dummy - Reassigned to Safe District	-0.056 (1.335)	1.464*** (0.053)	0.220 (0.884)	1.423 (1.974)
Dummy - Reassigned to Competitive District	-0.209 (0.837)	0.254 (1.776)	-2.149*** (0.619)	-1.704345 (1.467)
State-Fixed Effects	Yes	Yes	Yes	Yes
N	697	483	1077	284

Notes:

Robust standard errors clustered by state are reported

* indicates significance at the .1 level, ** at the .05 level, and *** at the .01 level

Cumulative Abnormal Returns are estimated for an event window of (-1, +7) using a Fama-French 3 Factor model and a 250 day estimation window.

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".

Districts defined as belonging to a party (Democratic or Republican) if more than 50% of voters lean towards it.