Appendix: Supporting Information for *Reevaluating Competition and Turnout in U.S. House Elections*

Contents

A.1 Measuring Competitiveness

A.1.1 Endogeneity Concerns

Geys (2006) classifies measures of competitiveness into ex post and ex ante measures. An ex ante measure captures an expectation of an election outcome, while an ex post measure uses the actual election outcome. Cox (1988) notes the endogeneity concern that ex post competitiveness for an election depends on turnout in the same election. For instance, a political scandal might boost turnout among supporters of both the incumbent candidate and challenger, causing higher turnout and, as a result, a closer election outcome. Voters and elites, however, would have responded to the scandal rather than to the perceived competitiveness of the election, thereby biasing estimates of the effect of competitiveness on turnout. While several studies utilize ex ante measures (e.g., Kunce 2001 uses pre-election polling; De Paola and Scoppa 2014 uses the first-round election in Italian municipal elections as an instrument for the competitiveness of the second-round election; and, Shachar and Nalebuff 1999 uses the predicted vote share based on a model), ex post measures remain the norm in the literature.[1]

As Cox (1988) points out, the fact that the actual margin of victory is sometimes not indicative of how close voters had anticipated the election to be is actually not the primary concern with ex post measures of competitiveness. To the extent that voters' "pre-election beliefs are correct on average," this is not particularly problematic. When measuring the margin of victory (i.e., the typical ex post measure of competitiveness), the numerator is the number of votes cast for the losing candidate subtracted from the number of votes for the winning candidate, and the denominator is the total number of votes cast:

$$M_i = \frac{W_i - L_i}{W_i + L_i} \times 100\%$$

The numerator for the turnout measure is the total number of votes cast, and the denominator is the total number of eligible voters:

$$T_i = \frac{W_i + L_i}{E_i} \times 100\%$$

As is clear, the denominator for the ex post measure of competitiveness is identical to the numerator for the measure of turnout. Cox (1988) notes that there is minimal variation in the number of eligible voters across congressional districts (E_i) post *Baker v. Carr*, so holding constant the numerator of M_i, any variation in the total number of votes cast ($W_i + L_i$) *mechanically* yields a negative correlation between M_i and T_i. In other words, any time that W_i and L_i both increase by a similar amount (for reasons completely independent of the perceived competitiveness of the election), the higher turnout *results* in a lower margin of victory.

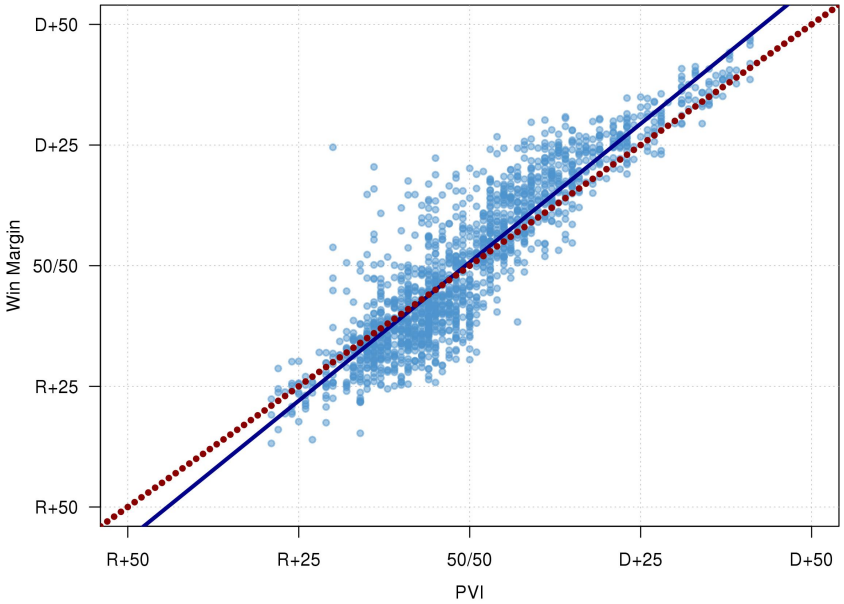
[1] Geys (2006) notes that 259 of the 362 (72%) studies in his review of competitiveness and turnout use ex post measures.

While, for the most part, ex ante measures are preferable to ex post measures, many ex ante measures are still subject to endogeneity concerns. For instance, pre-election polls might suggest a close race precisely because of high anticipated turnout. Even statistical models that predict competitiveness based on, among other things, incumbency status and challenger quality are subject to such concerns given the likelihood of strategic retirement and entry on the part of incumbents and challengers who consider electoral dynamics that affect turnout in their calculus to leave or enter the contest. Expert ratings are similarly subject to such concerns as experts take into account these same dynamics in assigning ratings.

A.1.2 Assessing the Validity of PVI Competitiveness

PVI Competitiveness, however, measures the underlying partisan composition of the district and, thus, is largely immune to such endogeneity concerns. To demonstrate that PVI is a reasonable ex ante measure of electoral competitiveness, we regress the win margin on PVI for all House races contested by a Democratic and Republican candidate between 2008 and 2014.[2] PVI explains 82% of the variation in the electoral outcome (win margin), the intercept is very close to zero (0.7), and the slope is extremely close to one (1.1). Figure A.1 displays a scatterplot of the win margin vs. PVI. The dark blue line is the least-squares line of best fit and the dotted red line is the 45-degree line. The line of best fit provides a relatively close approximation to the 45-degree line. In sum, PVI serves as a reasonable measure for the expected competitiveness of House election outcomes.

Figure A.1 – Win Margin vs. PVI, 2008-2014

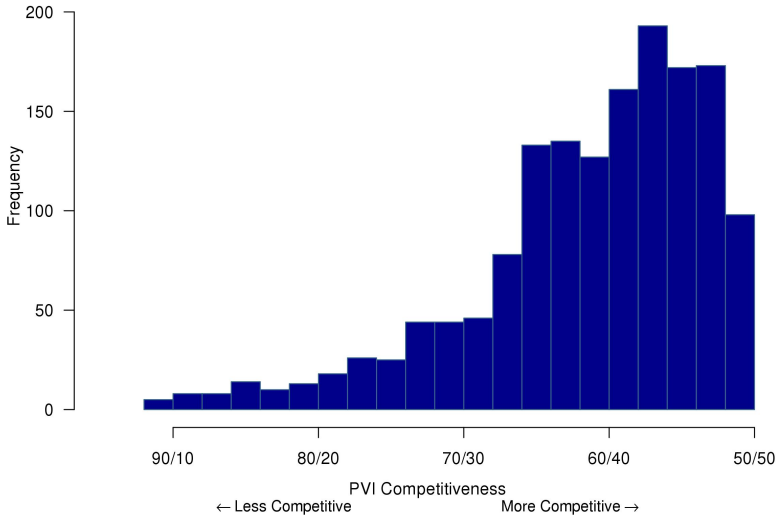


This figure plots win margin (ex post) against PVI (ex ante) and illustrates the strong correlation between the two measures. The least-squares line of best fit (in blue) closely approximates the 45-degree line (red-dotted line), implying that PVI is a valid ex ante measure of electoral competitiveness.

[2]We exclude all House races in Louisiana due to their unusual electoral rules.

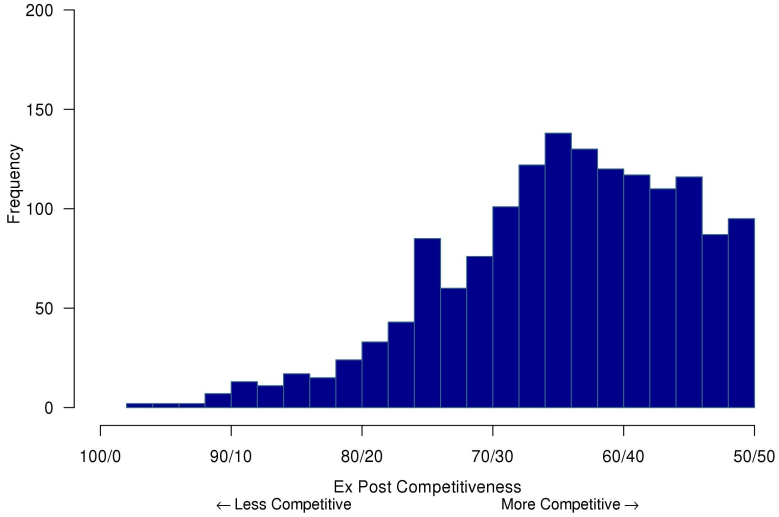
A.1.3 The Distribution of PVI Competitiveness and Ex Post Competitiveness

Figure A.2 – Distribution of PVI Competitiveness, 2008-2014



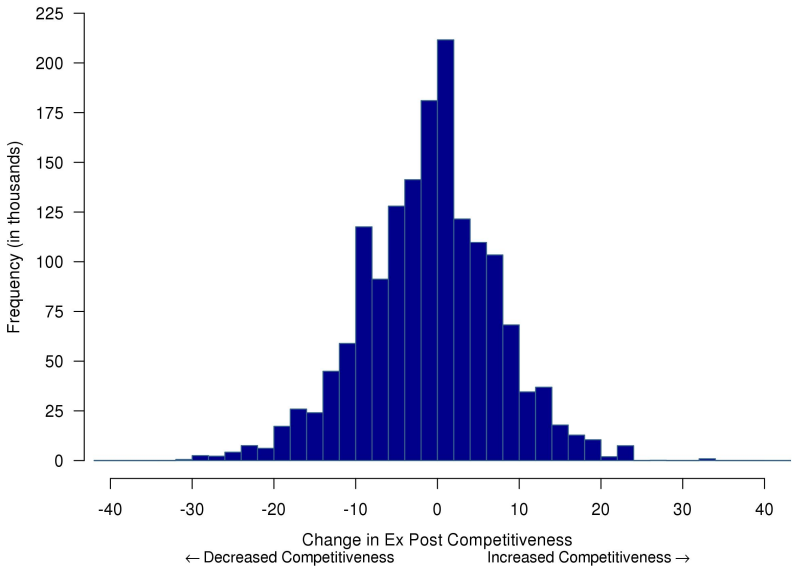
This histogram displays the distribution of districts across different levels of *PVI Competitiveness* between 2008 and 2014.

Figure A.3 – Distribution of Ex Post Competitiveness, 2008-2014



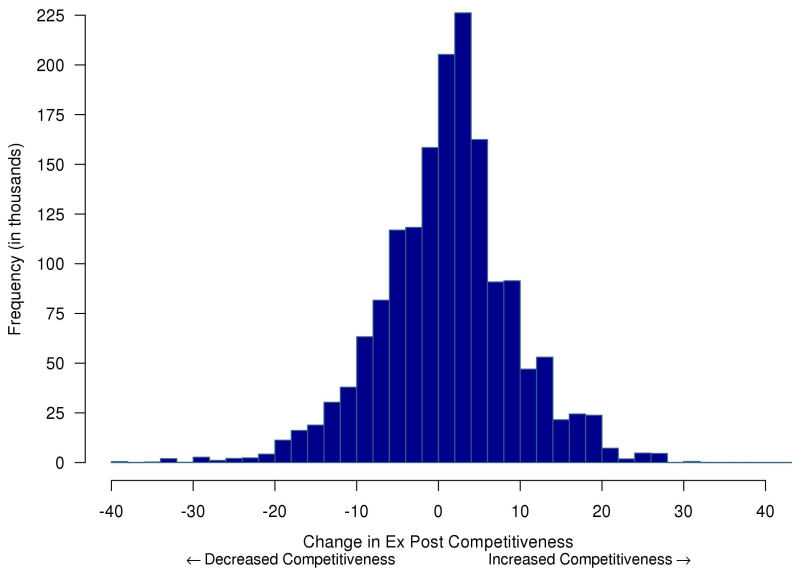
This histogram displays the distribution of districts across different levels of *Ex Post Competitiveness* between 2008 and 2014.

Figure A.4 – Distribution of Changes in Ex Post Competitiveness from 2010 to 2014



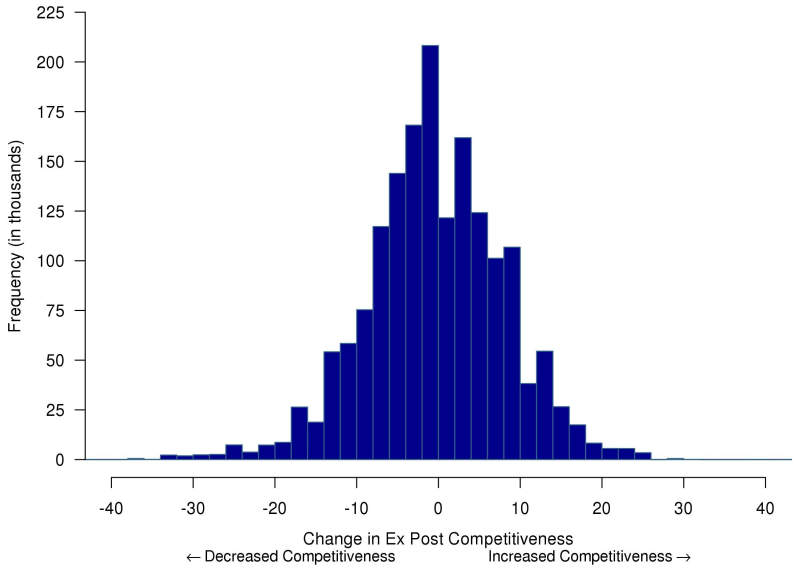
This histogram displays the distribution of changes in *Ex Post Competitiveness* for individuals from 2010 to 2014.

Figure A.5 – Distribution of Changes in Ex Post Competitiveness from 2008 to 2012



This histogram displays the distribution of changes in *Ex Post Competitiveness* for individuals from 2008 to 2012.

Figure A.6 – Distribution of Changes in Ex Post Competitiveness from 2010 to 2012



This histogram displays the distribution of changes in *Ex Post Competitiveness* for individuals from 2010 to 2012.

A.2 Aggregate-Level Analyses

A.2.1 About the District-Level Data

Data tracking congressional district characteristics from 2008–2014 are primarily based on the one-year estimates from the Census Bureau's American Community Survey (ACS) congressional district-level summary file.[3] These characteristics include the composition of the district by age, race/ethnicity, education, employment, and income as well as the population density of the district and residential mobility. Definitions for these measures and the ACS summary file table number that contains each of these measures are displayed in Table A.1.

Table A.1 – Measures for Congressional District Characteristics

Measure	Definition	ACS Table
Age 65+ (%)	Percentage of residents age 65 and over	S0101
Hispanic (%)	Percentage of Hispanic residents	B03002
Non-Hispanic Black (%)	Percentage of non-Hispanic Black residents	B03002
Non-Hispanic Asian (%)	Percentage of non-Hispanic Asian residents	B03002
HS or higher (%)	Percentage of residents (age 25+) with ≥HS/equivalent	S1501
BA or higher (%)	Percentage of residents (age 25+) with ≥BA	S1501
Employment-pop. ratio (%)	Percentage of employed residents (ages 25-64)	S2301
Median household income	Median household income (constant 2013 dollars)	S1901
Population density	Residents per square mile of land area	S0101
Residential mobility (%)	Percentage of residents who moved in the past year	S0701

Data on land area (population density) are from:
`https://www.census.gov/geo/maps-data/data/cd_national.html`
Median household income is adjusted for inflation using the CPI-U-RS.

The district-level turnout rate corresponds to the total votes cast in the House election divided by the district's citizen voting-age population (CVAP).[4] The CVAP measure is from the ACS, and the total votes cast tally is from the CQ Voting and Elections Collection.[5] Because some states do not report vote tallies for uncontested races, we exclude those races from our sample. We also exclude contests between two Democrats or two Republicans and all Louisiana races due to their unusual rules.[6] We use data from Jacobson and Carson (2016) to determine which races include Democratic and Republican candidates and the two-party margin of victory in each race.

[3]Data were retrieved from American Fact Finder: http://factfinder.census.gov/.

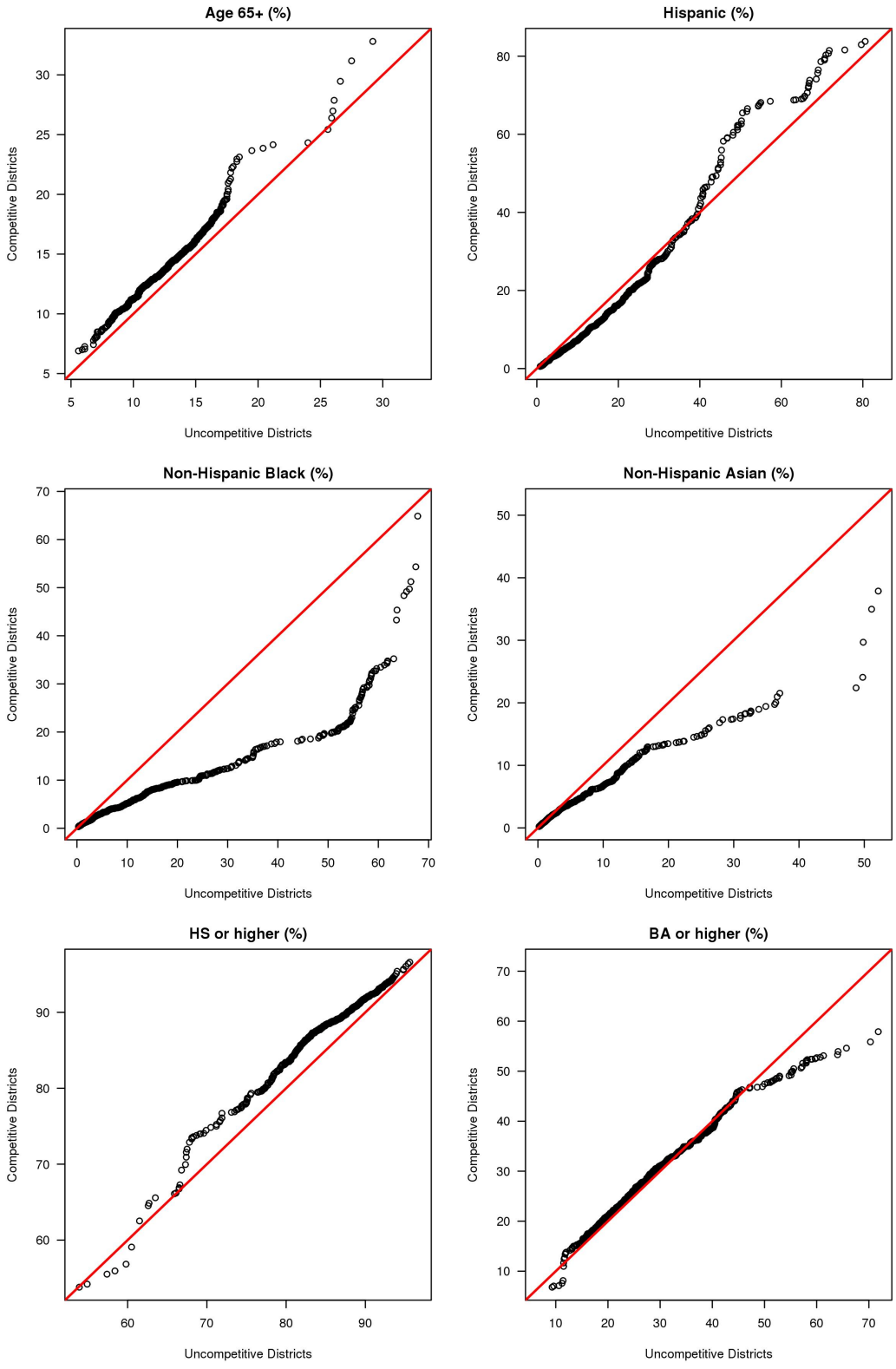
[4]Data on the voting-eligible population are not available at the congressional district level, so the best available measure is the citizen voting-age population (McDonald 2016).

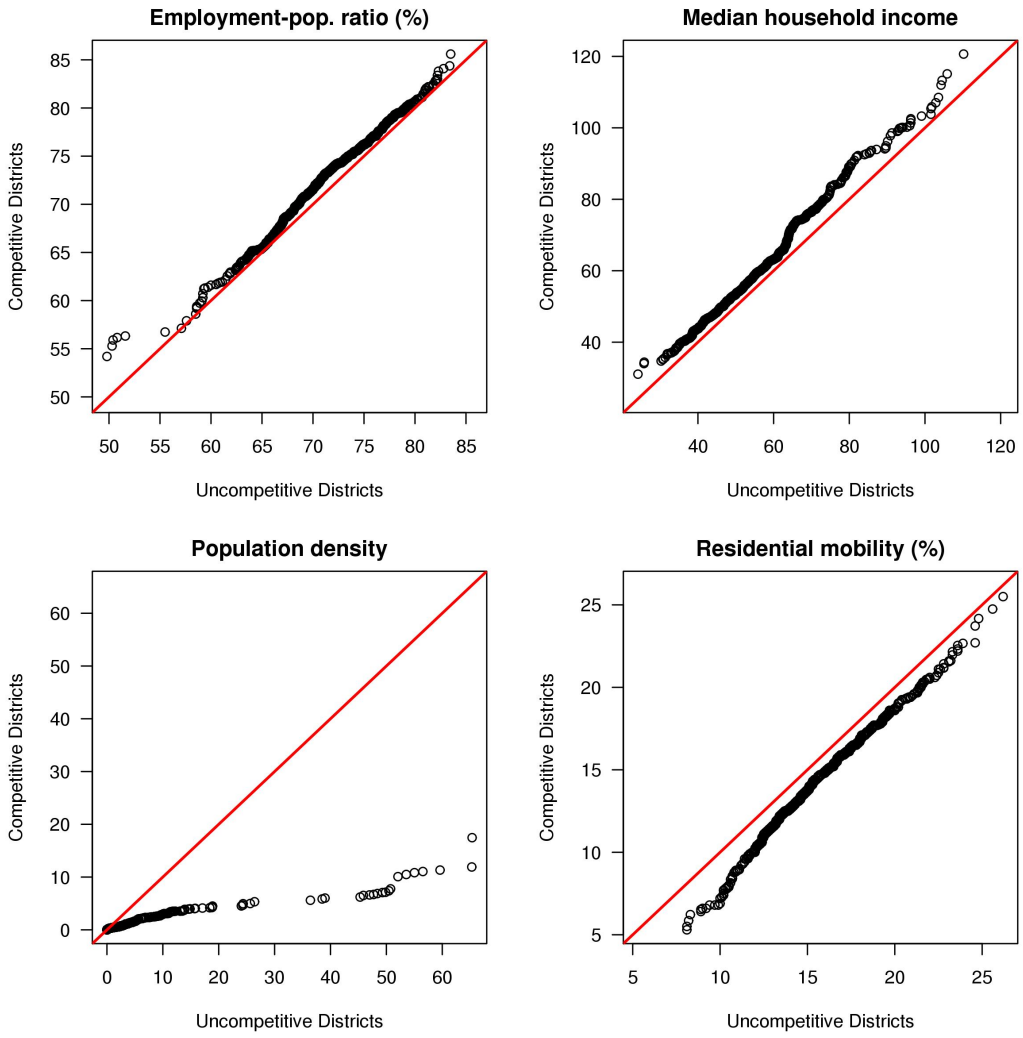
[5]CVAP data are accessible from ACS summary file table B05003.

[6]In Louisiana, the November general election includes all candidates (a primary does not restrict ballot access). If no candidate receives a majority of the vote, a top-two runoff election is held (typically in December). California and Washington both have top-two primary elections in which the two candidates with most votes qualify for the ballot in the general election. The top-two primary occasionally yields two candidates from the same party for the general election.

A.2.2 Covariate Balance

Figure A.7 – Quantile-Quantile Plots: Competitive vs. Uncompetitive Districts, 2008-2014





This figure displays quantile-quantile plots by district competitiveness for each of the 10 measured covariates. Districts between 50/50 and 60/40 (based on *Ex Post Competitiveness*) are considered competitive.

A.2.3 Covariate Adjustment

We use a regression framework for covariate adjustment to account for observed confounders in Tables A.2-A.5 . We report results for six specifications for each of the four election years in our sample. Model (1) is the simple bivariate case. In model (2), we include state fixed effects, which account for statewide electoral dynamics (e.g., statewide gubernatorial and senatorial elections, ease of voting based on election laws and administration, etc.) that affect statewide turnout in a given election. Model (3) includes six of the district-level covariates (without any fixed effects) that account for the age composition, racial/ethnic composition, and educational attainment of the district, while model (4) includes those same six covariates as well as state fixed effects. Model (5) includes all 10 district-level covariates from the balance plot, and model (6) includes the 10 district-level covariates along with state fixed effects. Depending on the specification and the year, the expected difference in the turnout rate between a district with a competitiveness of i and $i+10$ (e.g., a 60/40 vs. 50/50 district) ranges from about 0 to 2.7 percentage points.[7]

Our conclusion from this aggregate analysis is not that any one specification is the "correct" model yielding an unbiased estimate of the effect of competitiveness on turnout. Instead, we conclude that estimating the effect of competitiveness on turnout using aggregate, cross-sectional data is an extremely precarious exercise. Any covariate adjustment strategy requires a selection on observables (conditional independence) assumption to identify an effect. In other words, conditional on the observable covariates included in the analysis, the level of competitiveness is independent of the potential outcomes: $Y_{ci} \perp\!\!\!\perp c_i | X_i$, for all c.[8] With a regression framework, we must also correctly specify the functional form of all covariates and assume linearly separable confounding.

While the estimated effect of competitiveness on turnout decreases with the inclusion of this particular set of covariates, our estimate remains biased in either direction if any covariate — measured or unmeasured — that is correlated with both competitiveness and turnout is omitted from the model. Given the multitude of possible (un)measured covariates not included in the model and our largely arbitrary decision as to which measured covariates to include, it is nearly impossible to recover an unbiased estimate of the effect of competitiveness on turnout.

[7] While Tables A.2-A.5 display results based on the PVI competitiveness measure, results are of a similar magnitude and are similarly sensitive to the specification based on the ex post measure of competitiveness.

[8] Because electoral competitiveness is measured as a continuous variable, following Angrist and Pischke (2009), we express the potential outcomes using district-specific functional notation: $Y_{ci} \equiv f_i(c)$, which denotes the potential turnout rate in district i for electoral competitiveness c.

Table A.2 – Results Based on 2008 House Elections

	(1)	(2)	(3)	(4)	(5)	(6)
PVI Competitiveness	0.219***	0.065	0.102*	0.029	0.018	-0.048
	(0.046)	(0.036)	(0.040)	(0.027)	(0.042)	(0.027)
State FEs	No	Yes	No	Yes	No	Yes
Parial Controls	No	No	Yes	Yes	No	No
Full Controls	No	No	No	No	Yes	Yes
Observations	373	373	373	373	373	373
Adjusted R^2	.0554	.509	.485	.821	.543	.854

Standard errors in parentheses. * p < 0.05, ** p < 0.01, *** p < 0.001
The sample is comprised of contested general elections by a D and R candidate. All elections in Louisiana are excluded from the sample due to their unusual rules. Partial control variables include share age 65+, share Hispanic, share non-Hispanic Black, share non-Hispanic Asian, share HS or higher, share BA or higher, and employment-population ratio. Full control variables include all partial controls as well as median household income, population density, and residential mobility. Due to the scaling of the outcome and explanatory variables, an estimated coefficient of 0.1 implies a 1 percentage point increase in turnout for a 10-point increase in competitiveness.

Table A.3 – Results Based on 2010 House Elections

	(1)	(2)	(3)	(4)	(5)	(6)
PVI Competitiveness	0.267***	0.165***	0.078*	0.098***	0.013	0.030
	(0.041)	(0.030)	(0.039)	(0.024)	(0.042)	(0.024)
State FEs	No	Yes	No	Yes	No	Yes
Parial Controls	No	No	Yes	Yes	No	No
Full Controls	No	No	No	No	Yes	Yes
Observations	401	401	401	401	401	401
Adjusted R^2	.093	.575	.446	.845	.48	.872

Standard errors in parentheses. * p < 0.05, ** p < 0.01, *** p < 0.001
The sample is comprised of contested general elections by a D and R candidate. All elections in Louisiana are excluded from the sample due to their unusual rules. Partial control variables include share age 65+, share Hispanic, share non-Hispanic Black, share non-Hispanic Asian, share HS or higher, share BA or higher, and employment-population ratio. Full control variables include all partial controls as well as median household income, population density, and residential mobility. Due to the scaling of the outcome and explanatory variables, an estimated coefficient of 0.1 implies a 1 percentage point increase in turnout for a 10-point increase in competitiveness.

Table A.4 – Results Based on 2012 House Elections

	(1)	(2)	(3)	(4)	(5)	(6)
PVI Competitiveness	0.193***	0.052	0.160***	0.077**	0.091*	0.005
	(0.051)	(0.037)	(0.043)	(0.026)	(0.044)	(0.025)
State FEs	No	Yes	No	Yes	No	Yes
Parial Controls	No	No	Yes	Yes	No	No
Full Controls	No	No	No	No	Yes	Yes
Observations	385	385	385	385	385	385
Adjusted R^2	.0329	.584	.524	.864	.568	.897

Standard errors in parentheses. * p < 0.05, ** p < 0.01, *** p < 0.001
The sample is comprised of contested general elections by a D and R candidate. All elections in Louisiana are excluded from the sample due to their unusual rules. Partial control variables include share age 65+, share Hispanic, share non-Hispanic Black, share non-Hispanic Asian, share HS or higher, share BA or higher, and employment-population ratio. Full control variables include all partial controls as well as median household income, population density, and residential mobility. Due to the scaling of the outcome and explanatory variables, an estimated coefficient of 0.1 implies a 1 percentage point increase in turnout for a 10-point increase in competitiveness.

Table A.5 – Results Based on 2014 House Elections

	(1)	(2)	(3)	(4)	(5)	(6)
PVI Competitiveness	0.247***	0.115***	0.119*	0.090***	0.100	0.004
	(0.057)	(0.034)	(0.052)	(0.025)	(0.054)	(0.025)
State FEs	No	Yes	No	Yes	No	Yes
Parial Controls	No	No	Yes	Yes	No	No
Full Controls	No	No	No	No	Yes	Yes
Observations	354	354	354	354	354	354
Adjusted R^2	.0484	.717	.429	.893	.468	.915

Standard errors in parentheses. * p < 0.05, ** p < 0.01, *** p < 0.001
The sample is comprised of contested general elections by a D and R candidate. All elections in Louisiana are excluded from the sample due to their unusual rules. Partial control variables include share age 65+, share Hispanic, share non-Hispanic Black, share non-Hispanic Asian, share HS or higher, share BA or higher, and employment-population ratio. Full control variables include all partial controls as well as median household income, population density, and residential mobility. Due to the scaling of the outcome and explanatory variables, an estimated coefficient of 0.1 implies a 1 percentage point increase in turnout for a 10-point increase in competitiveness.

A.3 Individual-Level Analyses

A.3.1 Catalist Data: Geocoding and Sample Restrictions

Catalist provides the post-redistricting (2012 and 2014) congressional district for nearly all individuals based on their registration address. They also provide the pre-redistricting (2008 and 2010) congressional district for a large subset of the sample. We use the congressional districts provided by Catalist when available. When the pre-redistricting congressional district is not available from Catalist, we geocode these individuals into a pre-redistricting congressional district based on the block group of their registration address. Individuals who move during this period of time might be geocoded into the wrong pre-redistricting congressional district, so we exclude all individuals from our analysis sample who Catalist identifies as moving between 2008 and 2014. We further restrict our analysis sample to individuals who were age 18 or older on election day in 2008 and are not deceased. In addition to the residential non-mobility, age, and non-deceased sample restrictions, we also exclude voters who reside in districts in Louisiana, districts with an uncontested election, or districts with an election between two Democrats or two Republicans. As a robustness check, we also restrict the sample to only those individuals who are continuously registered since 2008 (in addition to the residential non-mobility, age, and non-deceased restrictions). Results based on this more restrictive sample are extremely similar to results based on the primary analysis sample. In Section A.4.2 below, we also display results based on a sample that includes uncontested races. Again, our estimates are insensitive to the inclusion (or exclusion) of uncontested races in the sample.

A.3.2 Cross-Sectional Regression Results

In this section, we explore the cross-sectional relationship between competitiveness and turnout using individual-level data. As discussed in greater detail below, the competitiveness-turnout link is present even in the individual-level, cross-sectional data, which allows us to rule out aggregation bias.

For each election year from 2008–2014, we regress turnout on our measure of electoral competition, *PVI Competitiveness*, as well as a host of individual-level voter characteristics including an indicator variable capturing citizens who are female, over age 65, Hispanic, non-Hispanic Black, and non-Hispanic Asian. We also include a variable measuring individual educational attainment — capturing whether someone holds a college degree or higher.[9] Then, we repeat this exercise using *Ex Post Competitiveness* as an outcome variable instead.

We present point estimates for each year across three specifications: the simple correlation between *PVI Competitiveness* and *Turnout*, the estimate when including state fixed effects, and, finally, the estimate when including the control variables along with state fixed effects.

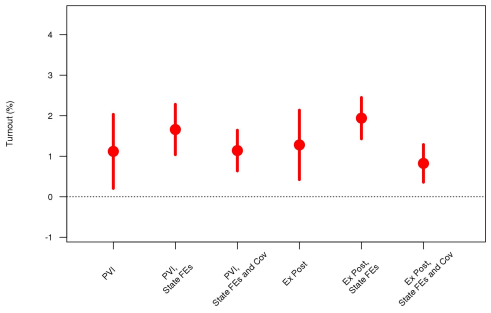
With few exceptions, we find that a 10-point increase in competitiveness (i.e., going from a 60/40 to a 50/50 district) is associated with a 1-2 percentage point increase in the probability of turning out. For example, when conditioning on state of residence, a 10-point swing in competitiveness is associated with an increase in turnout of 1.66 percentage points in 2008, 1.69 percentage points in 2010, 1.75 percentage points in 2012, and 1.8 percentage points in 2014. Similarly, estimates of the bivariate relationship range between 2 and 2.5 percentage points.

Finding a relationship between competitiveness and turnout in the cross section for both our aggregate-level and individual-level analyses implies that aggregation bias does not account for our null findings in the individual-level panel analyses (the main results of the paper). The presence of a competitiveness-turnout link in individual-level, cross-sectional data contrasts with some results from previous studies. Matsusaka and Palda (1993), examining individual-level self-reported turnout for the 1979 and 1980 Canadian national elections, finds no relationship between competitiveness and turnout, and concludes that aggregation bias must be a primary factor for the established result in the literature of a positive relationship between competitiveness and turnout at the aggregate level. Our evidence suggests that U.S. congressional elections do not exhibit the same properties.

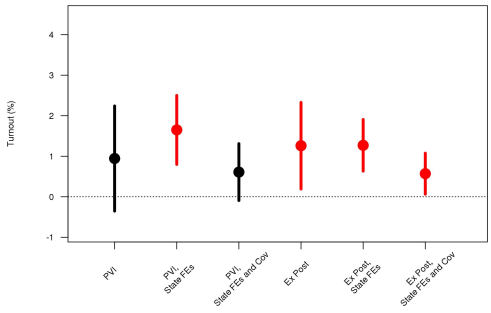
[9]This variable takes the form of a percentage giving the probability that an individual holds a college degree or higher. To estimate this probability, the educational attainment model is based on survey data from 25,000 respondents who answered questions about their educational attainment. Using these results, a logistic model of educational attainment infers education levels for the whole population based on other observable characteristics.

Figure A.8 – Cross Sectional Results based on House Elections, Individual Level

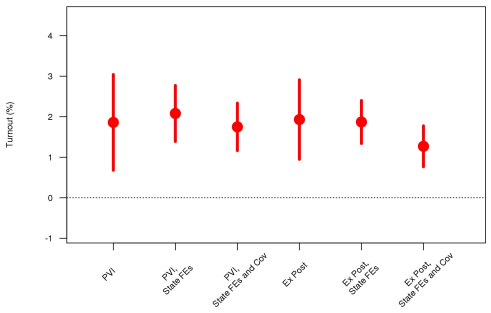
2008



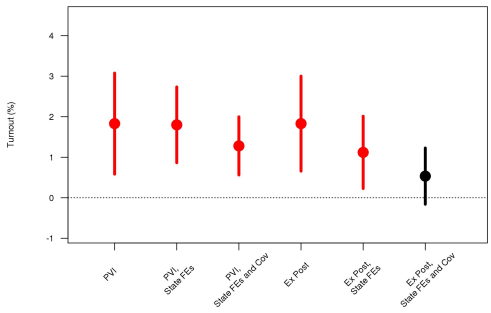
2010



2012



2014



A.3.3 Strategic Redistricting: Alternative Matching Method

One concern is that strategic boundary drawers intentionally and systematically place a certain type of voter into a more or less competitive district. For instance, suppose a Democratic mapmaker places high propensity Republican voters into an uncompetitive, highly Democratic district (to "waste" these votes). While the differences-in-differences approach protects against time-invariant confounding, it does not ensure that the sets of voters compared share the same characteristics (common support) and have the same distributions of these characteristics (common distribution). By combining a difference-in-differences approach with an entropy balancing matching procedure, we can protect against time-invariant confounding and ensure common support and common distributions with respect to observable covariates between "treated" voters (those placed into a competitive district post-redistricting) and "control" voters (those placed into an uncompetitive district post-redistricting). Because we are matching within the pre-redistricting congressional district, the effect is estimated entirely from the comparison of individuals who started in the same district (Sekhon and Titiunik 2012).

To implement this estimation procedure, we first classify individuals as residing in either competitive or uncompetitive districts, based on whether the measure of competitiveness has an absolute value less than or equal to 10 points (i.e., a 55/45 election is considered competitive but a 61/39 election is not). In this framework, "treated" units take a value of one and "control" units take a value of zero. We further restrict the sample to include only individuals who reside in an uncompetitive district pre-redistricting.

We then match all units that ever receive treatment to a weighted set of control units. We employ an entropy balancing matching procedure (Hainmueller 2012), which re-weights control units to achieve covariate balance for their first and second moments. After blocking on state and pre-redistricting congressional district, we match along an individual's turnout decision in 2006 as well as covariates including Black, Hispanic, Asian, female, age, and education. We additionally ensure that age and education are balanced both across treatment and control in the full sample and within racial groups.[10]

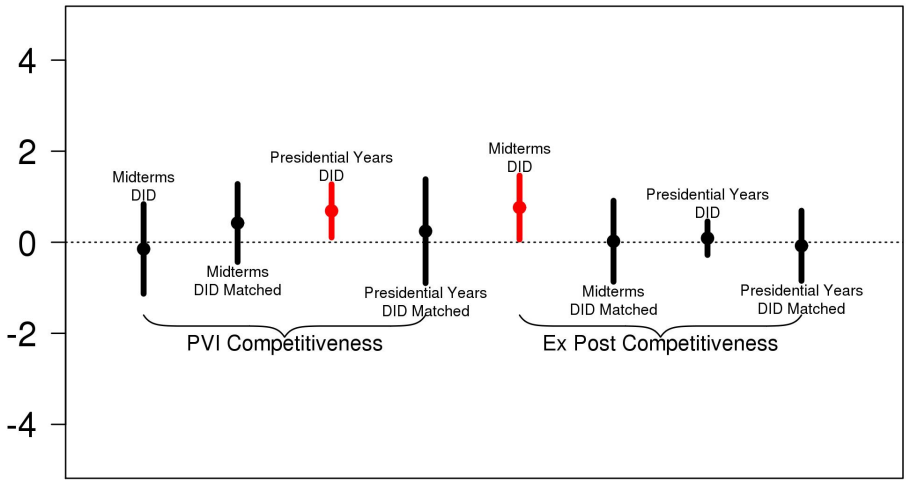
Figure A.10 illustrates covariate balance for individuals in competitive (treated) vs. uncompetitive (control) districts in midterm elections. The plot shows that covariate imbalances occur primarily in terms of racial characteristics. Hispanic voters have a higher propensity to reside in competitive districts; conversely, black citizens tend to reside in less competitive districts on average. These imbalances present problems for inference if there are individuals in competitive districts for whom a reasonable comparison in uncompetitive districts does not exist. For example, consider a Hispanic citizen redistricted into a more competitive district in 2012; the ideal comparison group would consist of Hispanic citizens from the same district who remain in an uncompetitive district.

[10]Similar to Henderson, Sekhon, and Titiunik (2016), we exclude individuals from pre-redistricting congressional districts where there are fewer than twice as many control units as treated units.

If this comparison group does not exist, then our matching procedure prunes these observations rather than attempting to interpolate (or extrapolate, depending on the case).

Balance improves markedly after implementing the matching procedure, and no meaningful covariate imbalances remain. That said, matching only ensures balance along observable covariates. We thus employ difference-in-differences estimation to account for time-invariant confounders.

Figure A.9 – Panel Data, Binary Indicator for Competitiveness (DID and Matched DID): Marginal Effect of Competitiveness on Turnout (%)

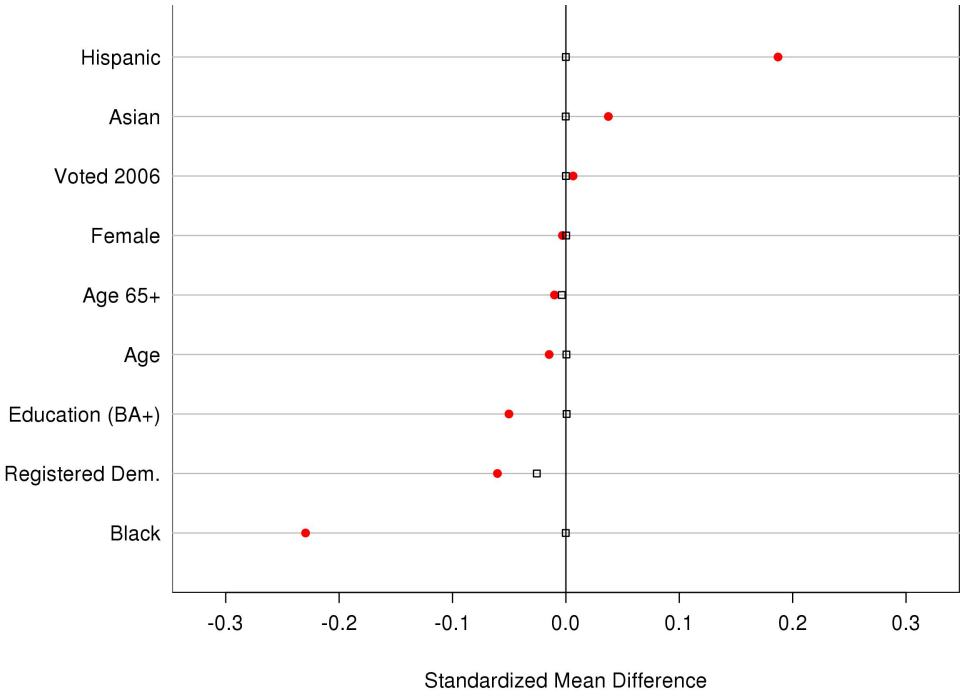


This figure illustrates the effects when employing a classic difference-in-differences approach. Competitive districts are those that had a competitiveness measure greater than or equal to -10 (i.e., a 60/40 race or better). The sample is restricted only to those individuals who resided in an uncompetitive district pre-redistricting. Second, we also provide estimates after having further pre-processed the data by performing an entropy balancing matching procedure.

Figure A.9 reports the results of the estimator using a binary treatment for the matched and unmatched samples for midterm election years and, separately, for presidential election years (the full results are reported in Table A.6). When employing matching, the effect size is never more than three-tenths of one percentage point. Even without matching, in no case do we estimate an effect greater than three-quarters of a percentage point, despite the fact that the binary treatment going from uncompetitive to competitive marks a large swing in district competitiveness.

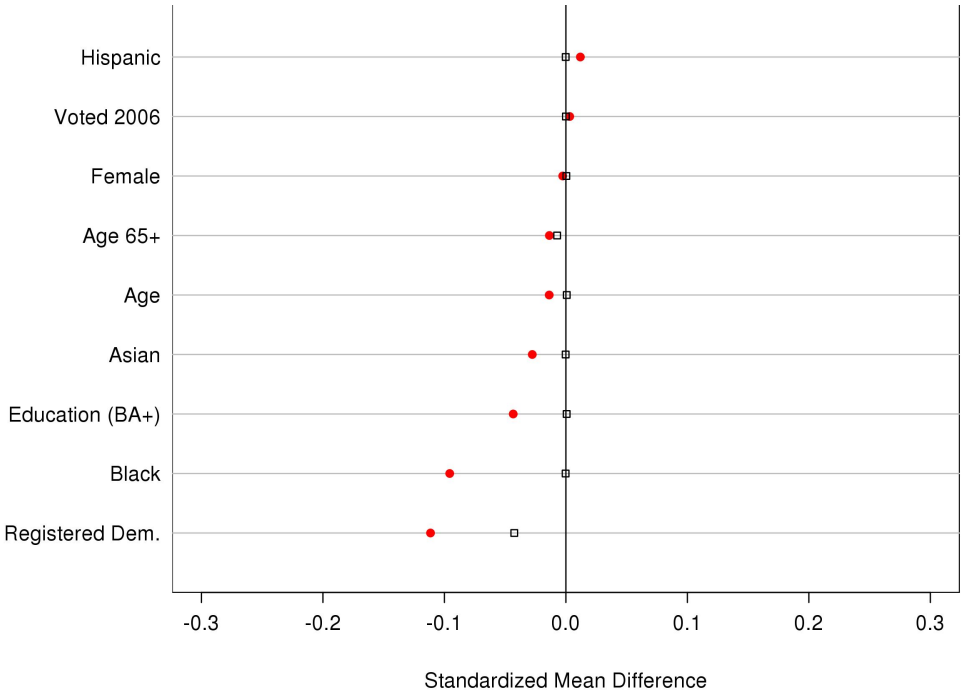
Finally, to deal even more explicitly with the prospect of strategic redistricting based on race, we restrict the sample to include states that do not have any majority-minority districts. This sample restriction means that a redistricter is never confronted with the question of whether to move a minority voter into or out of a majority-minority district. Table A.10 displays the results of this robustness check for the years 2010 and 2014. Implementing the difference-in-differences approach yields an estimate with a magnitude that is less than one-tenth of a percentage point. Again, this provides evidence of an effect of competitiveness on turnout that approximates a precise zero, even when strategic redistricting is likely not a factor.

Figure A.10 – Pre- and Post-Matching Covariate Balance Plot: Competitive vs. Uncompetitive Districts, 2010 & 2014



This figure is a balance plot for a set of observed individual covariates (for midterm election years) before and after employing an entropy balancing matching procedure. The red circles indicate pre-matching balance for competitive vs. uncompetitive districts; the black squares indicate post-matching balance. Districts between 50/50 and 60/40 (based on *PVI Competitiveness*) are considered competitive. We do not match on the Registered Dem. variable (due to the high incidence of missing data), but include to show balance improves even for variables we did not match on.

Figure A.11 – Pre- and Post-Matching Covariate Balance Plot: Competitive vs. Uncompetitive Districts, 2008 & 2012



This figure is a balance plot for a set of observed individual covariates (for presidential election years) before and after employing an entropy balancing matching procedure. The red circles indicate pre-matching balance for competitive vs. uncompetitive districts; the black squares indicate post-matching balance. Districts between 50/50 and 60/40 (based on *PVI Competitiveness*) are considered competitive. We do not match on the Registered Dem. variable (due to the high incidence of missing data), but include to show balance improves even for variables we did not match on.

Table A.6 – Canonical Diff-in-Diff Individual Regressions of Turnout on Competitiveness: Binary Indicators for Competitiveness

	2010 & 2014 PVI		2008 & 2012 PVI		2010 & 2014 Vote Marg.		2008 & 2012 Vote Marg.	
	DID	DID Matched	DID	DID Matched	DID	DID Matched	DID	DID Matched
PVI Comp. Election (Binary)	-0.00147	0.00423	0.00689	0.00243				
	(0.00502)	(0.00437)	(0.00298)	(0.00582)				
Ex Post Comp. Election (Binary)					0.00763	0.000226	0.000891	-0.000774
					(0.00358)	(0.00454)	(0.00188)	(0.00393)
Observations	1,195,758	410,166	1,298,752	440,964	1,855,950	519,792	2,193,994	647,292
State-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting CD level, are in parentheses.
The sample is comprised of contested general elections with a D and R candidate.
All elections in Louisiana are excluded from the sample due to their unusual rules.

A.4 Additional Robustness Checks

A.4.1 Highly Responsive Voters

To test the limits of the null result further, we restrict the sample to subsets of voters most likely to respond to changes in competitiveness. This represents a very stringent test; throughout our analysis, we have followed the norm in the existing literature on competitiveness and turnout of examining the full eligible voting population. We now restrict our study to sub-groups theoretically more likely to respond to the treatment. The first test consists of identifying citizens likely to respond to changes in competitiveness based on their past voting behavior. For example, if voters fit into categories such as never-voters, always-voters, and sometimes-voters, then the first two groups would not respond to changes in competitiveness. The third group of voters, who sometimes turn out and sometimes do not, would potentially respond to changes in competitiveness on the margin. We identify these voters by isolating individuals eligible for the 2006 and 2008 elections who only voted in one of those elections. We also restrict the sample to groups traditionally thought of as having more information, political knowledge, or the resources to participate. Specifically, we identify individuals in the top quartile of our measure of education and those who live in census blocks with median household incomes in the top quartile of the full distribution. Finally, we identify partisans by identifying voters registered with one of the two major parties.[11]

Figure 6 (in the paper) displays the results for those voters most likely to respond to changes in competitiveness, and Table A.7 includes the full table of results. In our view, these estimates represent the ceiling for the plausible magnitudes of the effect of competitiveness on turnout in congressional elections. The effects still appear minimal. When using our preferred measure of competitiveness, *PVI Competitiveness*, the effect of a 10-point swing in competitiveness on turnout remains below one half of one percentage point for "sometimes-voters" (0.485 percentage points), for educated voters (0.232), for high income voters (0.131), and for partisans (0.333). When employing the ex post measure of competitiveness, we find slightly larger magnitudes, though three of four estimates remain at half a percentage point or below. When using ex post competitiveness, sometimes-voters (0.710) have the largest effect. To put this in perspective, this is the largest effect size we find — and it still comes in substantially below most past estimates in the literature that rely on the full set of voters rather than those likely to have the largest effects. The subset of voters likely to have the largest effects still exhibit minimal effects when compared to past, cross-sectional results; this provides additional support pointing towards a null effect for the full population of eligible voters.

[11] This last measure is imperfect since some states do not make party registration available. Those states are omitted from the analysis.

Table A.7 – Individual Regressions of Turnout on Competitiveness: Voters on the Margin, Midterm Elections (2010 & 2014)

	Marginal Voters		Top Quartile Educ.		Top Quartile Med. HH Inc.		Partisans (Reg. D/R)		1st-Time Voters	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PVI Comp.	0.000485		0.000232		0.000131		0.000333		0.00000894	
	(0.000218)		(0.000232)		(0.000217)		(0.000197)		(0.000381)	
Ex Post Comp.		0.000710		0.000504		0.000504		0.000211		0.0000120
		(0.000164)		(0.000178)		(0.000166)		(0.000149)		(0.000278)
Observations	696482	692688	892462	891250	870610	869876	1315588	1306226	104170	103620

Standard errors, clustered at the pre/post redistricting CD level, are in parentheses.
The sample is comprised of contested general elections with a D and R candidate.
All elections in Louisiana are excluded from the sample due to their unusual rules.
Due to the scaling of the outcome and explanatory variables, an estimated coefficient of 0.001 implies a 1 percentage point increase in turnout for a 10-point increase in competitiveness.

A.4.2 Inclusion of Uncontested Races

One interesting possibility is that competitiveness increases turnout by decreasing the odds of an uncontested election. Specifically, low odds of winning might deter candidates of one party from entering a race. Because we have restricted the analysis to races contested by a Democrat and a Republican, we could as a result miss this aspect of the relationship between competitiveness and turnout. While we have shown no relationship exists in elections with two major-party candidates, meaningful variation in competitiveness could still arise out of a switch from an uncontested to a contested election. To check for this possibility, we expand our sample to include uncontested races. Table A.8 in the Appendix demonstrates that the results remain unchanged upon inclusion of uncontested races for the House. For midterm years, presidential years, and all years between 2008 and 2014, in no case does the point estimate surpass more than one tenth of one percentage point for a 10-point swing in competitiveness.

Table A.8 – Panel Data Individual Regressions of Turnout on Competitiveness (Including Uncontested Races)

	2008 & 2012	2010 & 2014	All Years
PVI Competitiveness	0.0000877	0.0000315	0.000104
	(0.000109)	(0.000140)	(0.0000886)
Observations	4031352	3909494	7622868

Standard errors, clustered at the pre/post redistricting CD level, are in parentheses.
The sample is comprised of contested general elections with a D and R candidate.
All elections in Louisiana are excluded from the sample due to their unusual rules.
Due to the scaling of the outcome and explanatory variables, an estimated coefficient of 0.001 implies a 1 percentage point increase in turnout for a 10-point increase in competitiveness.

A.4.3 Top of the Ticket Races

Using House races to test the relationship between competitiveness and turnout underpins our empirical approach since redistricting occurs at the congressional district level. Furthermore, variation at this level yields more observations (and more precise estimates) than when examining statewide or nationwide races that do not occur every two years. However, one can make the argument that, because presidential, gubernatorial, and senatorial elections are top of the ticket races, they may drive people to the polls when House races do not. Put differently, the competitiveness of the most salient races (those at the top of the ballot) may play a role in driving turnout. We explicitly test the effect of being on the top of the ticket by identifying instances when a House race actually was at the top of the ticket. Such a phenomenon can only occur in midterm years, in those states not holding senatorial or gubernatorial elections. To test the importance of being on the top of the ticket, we include a dummy variable identifying whether the House race is a top-of-the-ticket race, and we also interact this dummy variable with our measure of competitiveness. We provide estimates for just midterm years and enlarging the sample to include all years (in this case, presidential years never include House races at the top of the ticket, but they provide additional information about patterns of turnout when the House race is not at the top of the ticket).

This exercise leads to mixed results, which we present in Table 4 (in the paper). As before, we find no evidence that competitiveness on its own leads to higher turnout. However, the results do suggest that competitiveness can make slightly more of a difference, conditional on being a top-of-the-ticket race. In three of four instances, we find that, conditional on being on the top of the ticket, a 10-point increase in competitiveness leads to a one percentage point increase in turnout.

A.4.4 Roll Off

Another political participation outcome of interest is whether or not a voter casts a vote specifically in the House election. The turnout outcome that we rely on from voter file data only tells us whether or not an individual turns out to the polls, but it cannot tell us whether or not she cast a vote in a specific race. Even if it is the case, as we find, that the competitiveness of House elections has a near-zero effect on turnout, it is still possible that individuals at the polls may be less likely to cast a vote in less competitive House races. To examine whether "roll off" is higher in less competitive districts, we employ data from the 2010-2014 Cooperative Congressional Election Study (CCES) Panel. See Section A.5.4 for more information on this data set and our sample restrictions.

For the roll-off analysis, we further restrict our sample to individuals who turned out to vote both in 2010 and 2014 based on validated turnout.[12] Roll off occurs when a voter reports casting a vote in a senatorial and/or gubernatorial race but *not* the House race. As with our other voter-level panel research designs, we include individual fixed effects to analyze within person variation in competitiveness. The state-year fixed effects account for statewide idiosyncrasies in a given election year (e.g., a senatorial election in 2010 but not 2014). Standard errors are clustered by the pairwise combination of pre- and post-redistricting congressional districts. The point estimate for the effect of competitiveness on roll off for all of our specifications is essentially zero. Despite a smaller sample size, our estimates are quite precise and near zero.

Table A.9 – Roll Off

	(1)	(2)	(3)	(4)
PVI Competitiveness	-0.0002	-0.0002		
	(0.0006)	(0.0006)		
Ex Post Competitiveness			0.0000	-0.0001
			(0.0004)	(0.0004)
Observations	6760	6760	6760	6760
R^2	0.000	0.024	0.000	0.024
Individual FEs	Yes	Yes	Yes	Yes
State-Year FEs	No	Yes	No	Yes

Standard errors, clustered at the pre/post redistricting CD level, are in parentheses.
* p < 0.05, ** p < 0.01, *** p < 0.001
The dependent variable is coded =1 if the respondent reported voting in a Senate or governor race but not a House race. The sample is based on respondents to the CCES 2010-2014 panel and is restricted to validated voters in contested general elections by a D and R candidate. All Louisiana residents are excluded from the sample due to their unusual rules.

[12]The CCES matches respondents from the survey to the voter file to validate participation in elections.

A.4.5 District Level Time-Varying Covariates

Redistricting alters more than just competitiveness. In fact, one might view redistricting as applying a bundle of treatments to an individual who moves to a new district. District competitiveness changes, but so too do a number of other features related to the political environment. As a result, redistricted citizens may find other changes in their district salient beyond just the level of electoral competition. The new racial and economic context for their district could also influence their decisions to vote. For example, voters of a certain race may grow more likely to turn out as they make up a greater overall share of the population in their district (Fraga 2016). The fact that redistricting changes more than just a district's level of competitiveness poses a problem for inference if these changes are not orthogonal to changes in competitiveness — this would violate a fundamental assumption of difference in differences, that no time-varying, unobserved confounders exist.

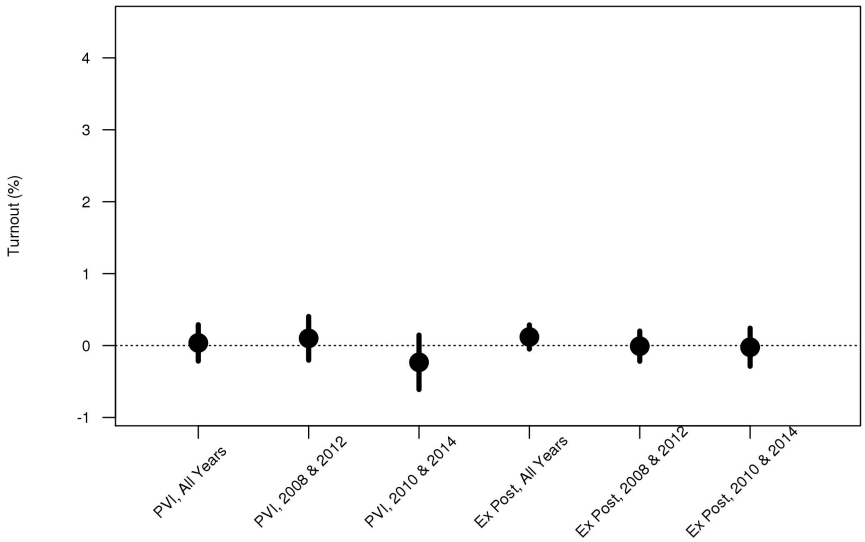
Keele and Titiunik (2015, 2016) discuss the assumptions required to identify an effect based on geographic boundaries. One especially important assumption is what both papers term, "Compound Treatment Irrelevance." Administrative boundaries (e.g., municipal, school district, county) often lie directly on top of one another. Thus, it can be difficult to isolate the effect of one administrative geographic unit from another without assuming the irrelevance of the other geographic units. When comparing units across static boundaries, this is a strong assumption that can be difficult to justify. However, in our case, the congressional district boundaries are dynamic, and we observe the same individuals in different competitive contexts at different points in time. In our empirical setting, the concern of compound treatments remains, but the generalized difference-in-differences framework allows for a weaker assumption. The presence of a compound treatment is only a threat to inference if *changes* in the treatment of interest are correlated with *changes* in a compound treatment. Most administrative boundaries are static, which precludes any correlation. One set of boundaries that does change at the same time as congressional boundaries is state legislative district boundaries. However, it is exceedingly difficult to concoct a scenario in which the changes to state legislative district boundaries present a threat to inference. For one, it is difficult to imagine that state legislative elections drive voters' turnout decisions. Moreover, in order for state legislative boundaries to bias our estimates toward zero, voters would need to be placed in more (less) competitive state legislative districts *and* less (more) competitive congressional districts. Such a strategic redistricting scenario seems entirely implausible.

A more worrisome concern is that congressional redistricting is itself a compound treatment: redrawing district boundaries changes the composition of a district in other ways besides competitiveness. For this class of compound treatments to present a threat to inference, changes in the compound treatments still must be correlated with changes in competitiveness. We demonstrate the plausibility of the Compound Treatment Irrelevance Assumption by conditioning on various possible compound treatments (in this class of compound treatments) and showing that changes in other compound treatment variables are not correlated with changes in competitiveness.

We present results in which we condition on a set of district-level time-varying covariates that could plausibly influence individual vote choice. Specifically, we include the district's median household income, racial composition (i.e., percent Black, Hispanic, Asian), and whether the district had a Republican or Democratic incumbent candidate. Figure A.12 includes the full results.[13] In no case does the point estimate appear higher than one-fifth of one percentage point — the null results remain robust and entirely in line with the evidence presented thus far. Furthermore, to the degree that selection on observed time-varying characteristics provides an informative signal about the degree of selection on unobserved time-varying characteristics, the fact that the point estimates remain stable provides reassurance that selection on unobserved time-varying characteristics does not present an important threat to making valid causal inferences.

To deal specifically with the possibility that the racial composition of a district is correlated with the competitiveness of a district, we also estimated results in which we restrict our analysis to states without any majority-minority districts. The difference-in-differences estimate of the effect of competitiveness on turnout is approximately zero restricting to voters residing in states without majority-minority districts as well.

Figure A.12 – Panel Data Individual Regressions of Turnout on Competitiveness (Including Time Varying District Characteristics



This figure reports results from a set of specifications for which we have added a set of time varying district covariates to our generalized differences in differences specification.

[13]We also demonstrate in Section A.4.6 of the Appendix that changes in the competitiveness of a district from pre- to post-redistricting is not correlated with proportion of residents remaining in the district from pre- to post-redistricting.

Table A.10 – Panel Data Individual Regressions of Turnout on Competitiveness (Only States with No Majority Minority Districts): 2010 & 2014

	(1)	(2)	(3)
PVI Competitiveness	0.00164	0.00164	-0.0000884
	(0.000564)	(0.000564)	(0.000415)
Observations	706,440	706,440	706,440
State-Year FEs	Yes	Yes	Yes
Individual FEs	No	No	Yes
Controls	No	Yes	No

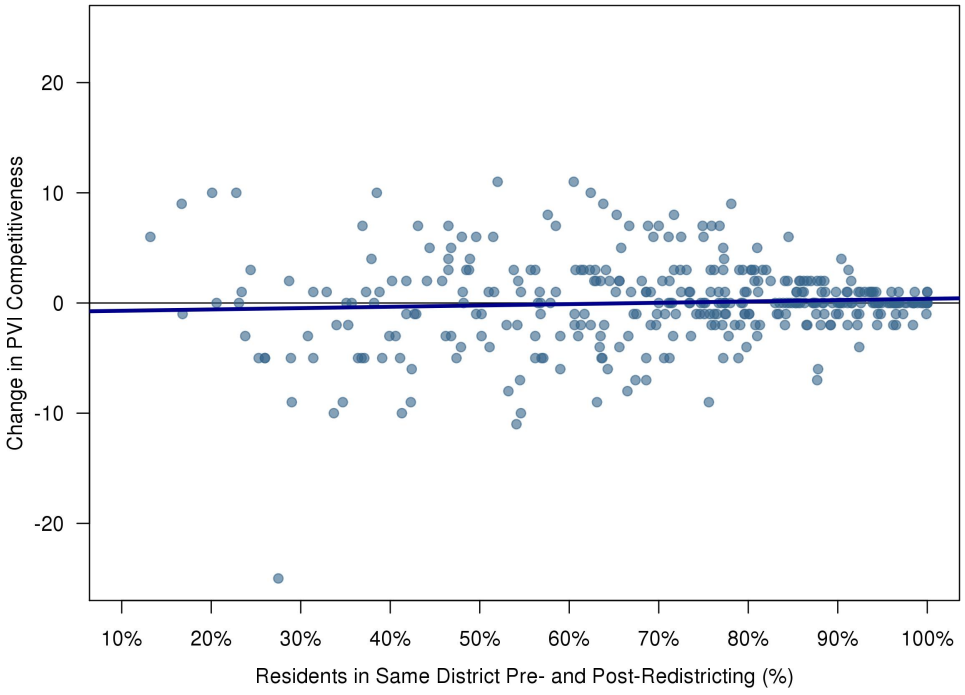
Standard errors, clustered at the pre/post redistricting CD level, are in parentheses.
The sample is comprised of contested general elections with a D and R candidate.
All elections in Louisiana are excluded from the sample due to their unusual rules.
Due to the scaling of the outcome and explanatory variables, an estimated coefficient of 0.001 implies a 1 percentage point increase in turnout for a 10-point increase in competitiveness.

A.4.6 Incumbents Focus on Voters from Their Old Districts

One potential concern is that incumbents might focus their mobilization efforts on the voters they are most familiar with: voters who resided in their pre-redistricting district. Some incumbents experience dramatic changes to their districts (in terms of the share of residents who remain from the pre-redistricting period), while other incumbents experience minimal changes to their districts. If the degree to which incumbents' districts change (in terms of the share of residents from their old districts in their new districts) is correlated with changes in competitiveness, this would present a serious threat to inference.

In this section, we investigate the extent to which the share of residents remaining with their incumbent from the pre-redistricting period is related to changes in competitiveness. For Figure A.13, we link districts across the redistricting period through incumbents. In other words, an observation is an incumbent seeking reelection in 2012. The horizontal axis indicates the proportion of residents in the 2012 district who resided in the incumbent's 2010 district, and the vertical axis indicates the difference in competitiveness between 2012 and 2010 districts for each incumbent. As is clear from the figure, incumbents who experience only modest changes in their districts also tend to experience only modest changes in competitiveness, while incumbents who experience more substantial changes in their district often experience more substantial changes in competitiveness. However, these substantial changes in competitiveness go in both directions: increases and decreases in competition. Overall, the slope is extremely flat, and the R^2 is a mere 0.004.

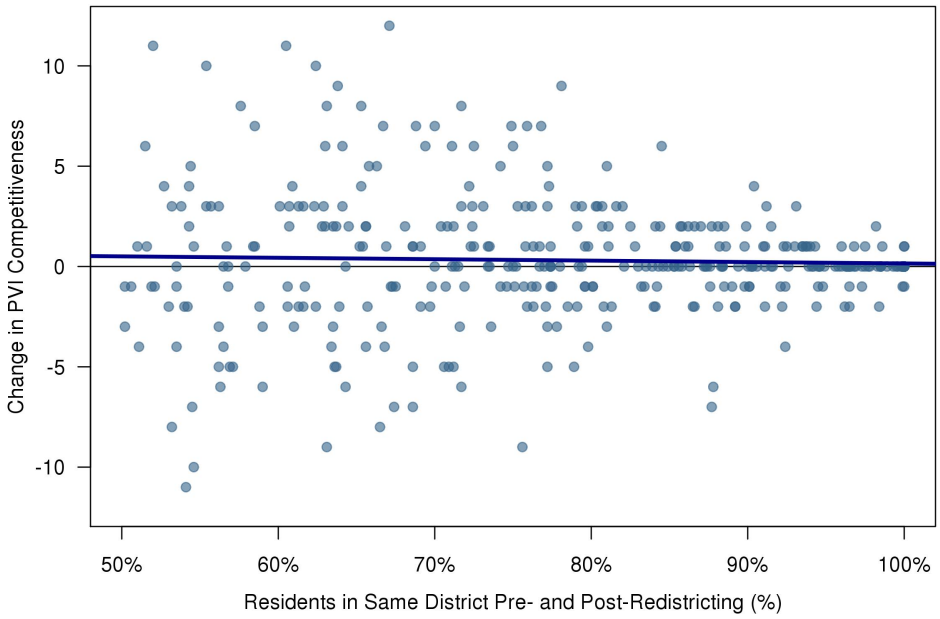
Figure A.13 – Changes in Incumbents' Districts vs. Changes in Competitiveness



This figure illustrates the relationship between the degree to which incumbents' new districts are comprised of voters from their old districts and changes in competitiveness.

In Figure A.14, we link districts from the pre- to post-redistricting period based on the share of voters. More specifically, a 2012 district i is linked to 2010 district j if at least 50% of the 2012 residents in i resided in j. If we link districts using this method, the results are nearly identical. Again, the slope is extremely flat, and the R^2 is 0.001. Based on these results, incumbent mobilization of voters from their old districts is *not* a threat to inference.

Figure A.14 – Changes in District Population vs. Changes in PVI Competitiveness



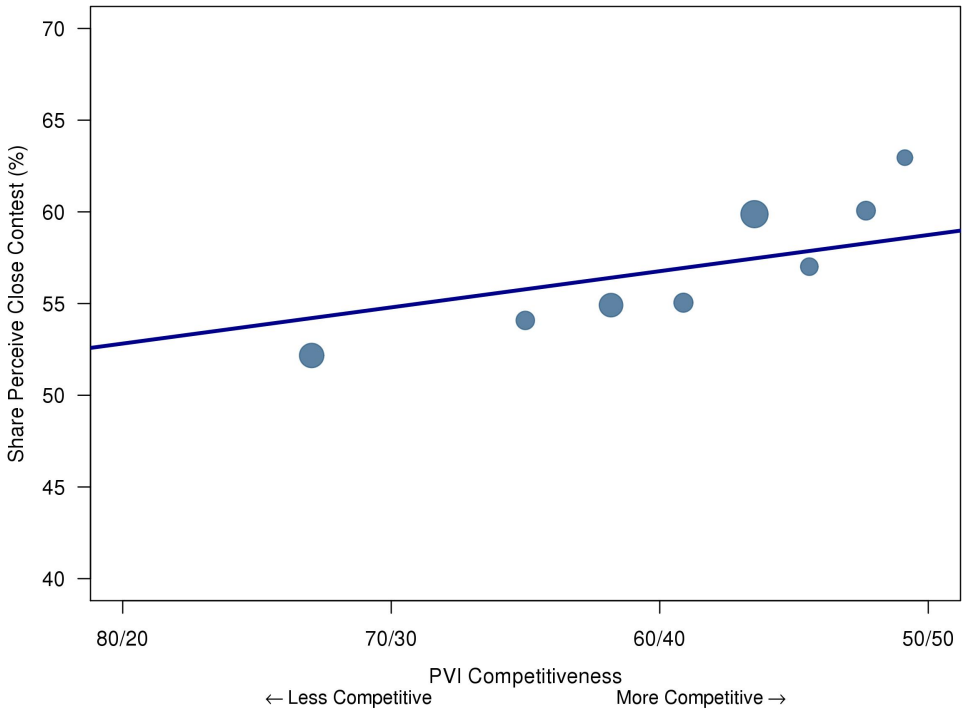
This figure illustrates the relationship the extent to which new districts are comprised of voters from the same old districts and changes in competitiveness.

A.5 Proposed Mechanisms

A.5.1 Instrumental Voting

From October 17-22 in 2006, the Pew Research Center for the People & the Press fielded a survey to gauge voters' opinions on electoral competition.[14] While the American National Election Studies has asked respondents about the expected competitiveness of presidential elections since 1952, to our knowledge, this Pew survey is one of the few surveys that asks respondents about the expected competitiveness of their U.S. House elections. The full sample is comprised of 2,006 respondents, which includes an oversample of 515 respondents residing in competitive House districts. Only registered voters are asked about their perceptions of the competitiveness of their House race. Respondents are asked: "What's your impression – in the race for the U.S. House in your district, is one candidate heavily favored to win or do you think this will be a close contest?" Respondents then choose: "One candidate heavily favored" or "Will be a close contest." All of our analyses of the Pew data utilize sampling weights.

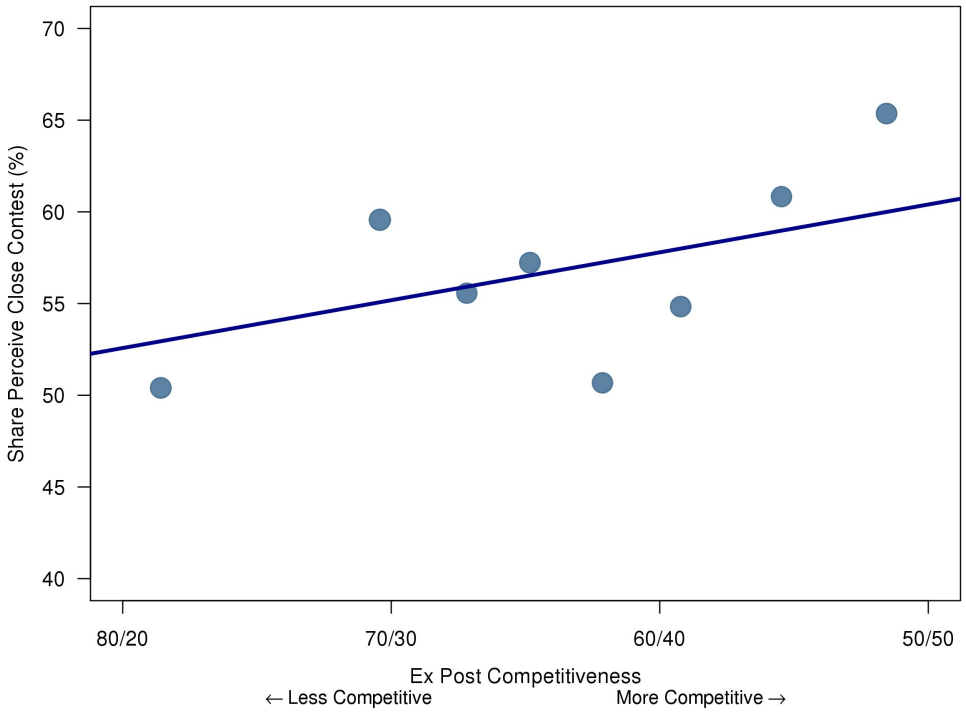
Figure A.15 – Perceived Competitiveness vs. PVI Competitiveness



This figure shows the relationship between voters' perceptions of competitiveness and *PVI Competitiveness*; the correlation is very weak, suggesting voters' have minimal knowledge of competitiveness.

[14] For more information on the survey and to download the micro data, see here: http://www.people-press.org/2006/10/27/october-2006-survey-on-electoral-competition/.

Figure A.16 – Perceived Competitiveness vs. Ex Post Competitiveness



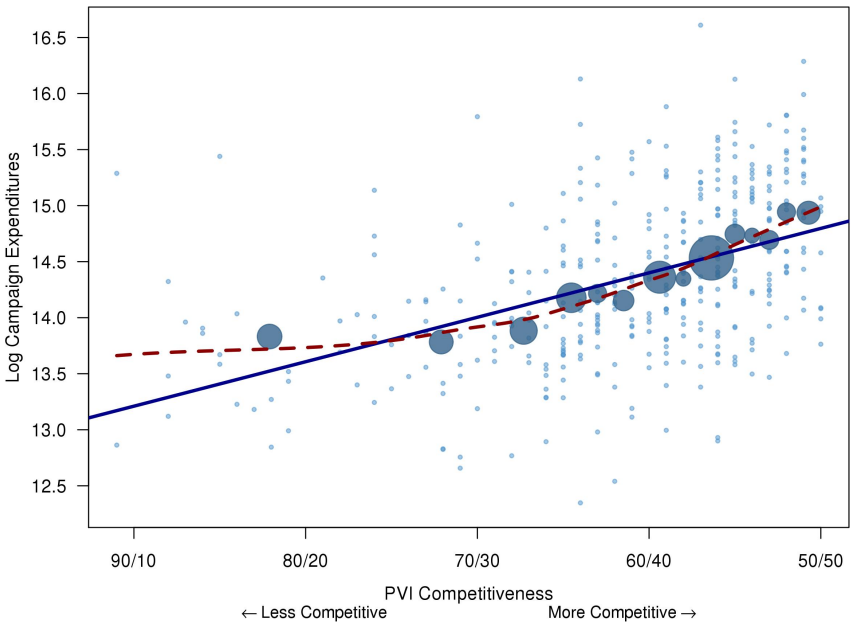
This figure shows the relationship between voters' perceptions of competitiveness and *Ex Post Competitiveness*.

A.5.2 Elite Mobilization: Campaign Expenditures

We use the Federal Election Commission Candidate Summary data to calculate the total campaign expenditures for the two major-party candidates throughout the election cycle. Given the highly skewed distribution of expenditures across congressional districts, we log-transform the expenditures variable. We display results below for 2010 and 2012 (the pre- and post-redistricting cycles).

As noted in the paper, one concern is that a cross-sectional analysis of the relationship between campaign expenditures and competitiveness could be subject to the same confounding that we document for the turnout-competitiveness cross-sectional relationship. In columns (5) and (6) of Table A.11, we report results from a fixed effects specification by examining the subset of candidates who won election in 2010 and sought reelection in 2012. We analyze whether candidates and their opponents ramp up (or slash) expenditures in response to an increase (or decrease) in competitiveness. We also include year fixed effects to account for changes in the overall level of campaign expenditures between 2010 and 2012; thus, the results in columns (5) and (6) are based on a generalized difference-in-differences approach. The incumbency status variable accounts for variation in whether the candidate was an incumbent in 2010 (excluding this variable hardly changes the estimated coefficients and standard errors). The estimates from the fixed effects specifications are remarkably similar to the cross-sectional estimates. In the paper, we speculate that confounding is less of an issue for campaign spending because contributions are less constrained by geography (about 60-65 percent of contributions flow from outside of candidates' districts) than turnout.

Figure A.17 – Log Campaign Spending vs. PVI Competitiveness



This figure displays the relationship between campaign spending and competitiveness for the 2010 election; see column (1) from Table A.11). The solid blue line represents the least-squares regression line, while the red dashed smoothed curve is from a locally weighted regression (LOESS). Each of the larger circles in the figure corresponds to a local mean and is proportional in size to the number of observations within the locale. The small dots that are uniform in size represent each contested House race between a Democrat and Republican in 2010 (excluding races in Louisiana).

Table A.11 – Log Campaign Spending in 2010 and 2012 House Elections

	2010		2012		Fixed Effects	
	(1)	(2)	(3)	(4)	(5)	(6)
PVI Competitiveness	0.040***		0.048***		0.045***	
	(0.004)		(0.004)		(0.010)	
Ex Post Competitiveness		0.052***		0.049***		0.046***
		(0.003)		(0.003)		(0.004)
Incumbency Status					-0.427***	-0.397***
					(0.076)	(0.066)
Constant	14.797***	15.085***	14.821***	15.004***	15.178***	15.305***
	(0.054)	(0.050)	(0.054)	(0.054)	(0.115)	(0.074)
Observations	401	401	385	385	612	612
Adjusted R^2	0.207	0.435	0.266	0.381	0.191	0.375
Candidate FEs	No	No	No	No	Yes	Yes
Year FEs	No	No	No	No	Yes	Yes

Standard errors in parentheses (clustered by candidate for the fixed effects specifications).
* p < 0.05, ** p < 0.01, *** p < 0.001
The dependent variable is the log of campaign spending by the two major-party candidates. For the 2010 and 2012 specifications, the sample is comprised of contested general elections by a D and R candidate. For the fixed effects specification, the sample is comprised of candidates elected in 2010 who sought re-election in 2012. For all specifications, all elections in Louisiana are excluded.

A.5.3 Elite Mobilization: Television Advertising

While campaigns must report many of their itemized disbursements to the Federal Election Commission (FEC), it is difficult to determine which of these disbursements are directed to television advertising or other mobilization strategies. In recent years, many congressional campaigns hire political consultants and other firms to help allocate their resources to various campaign strategies. The FEC data show disbursements to these firms, but we do not know where these intermediaries direct campaign resources. As a result, it is essentially impossible to know how much modern congressional campaigns spend on mobilization strategies based on the FEC disbursements data.

Instead of relying on the FEC data, we use data from the Wesleyan Media Project (WMP) for 2012 House elections (Fowler, Franz, and Ridout 2015). WMP relies on the firm Kantar Media/CMAG (which "specializes in providing detailed, real-time tracking information to corporate and political clients") to supply information on the airing of political ads throughout the election cycle. Included in the data is each airing of a political advertisement along with the station, media market, date, time, duration of the ad, and television program during which the ad aired.[15] Based on this information, Kantar/CMAG estimates the cost of each ad airing. The Wesleyan Media Project then codes information about the sponsor of the ad and the content of the ad.

Using the WMP data, we calculate three measures of the usage and cost of television advertising across each congressional district in the 2012 election. For all of our measures, we restrict to advertisements sponsored by the two major party general election candidates throughout the election cycle.[16] One measure is the share of total campaign expenditures directed to television advertising ("TV as Share of Exp."). We take the sum of the total estimated cost of all airings sponsored by the two major party candidates in a given congressional district and divide this by the sum of campaign expenditures by the two major party candidates as reported to the FEC. A second measure is the count of airings sponsored by the two candidates ("Number of Ads"). And, a final measure is the number of unique ads aired ("Unique Ads"), which represents the number of unique ads produced that made it on air at least once. The count of unique ads gives a rough indication of production costs.

Prior to reporting the results of our analyses, it is worth considering some of the limitations of the WMP data. First, the cost of each airing is only an estimate (although given the expertise of Kantar/CMAG, these estimates seem reasonably accurate), so there is some degree of (likely random) measurement error. Second, the WMP tracks ads aired on local broadcast television,

[15] As the documentation from Fowler, Franz, and Ridout (2015) notes, "CMAG gathers such data by using a market-based tracking system, deploying 'Ad Detectors' in each media market in the U.S. In addition to all local advertising activity, these detectors track advertisements on the major national networks, as well as national cable networks."

[16] In other words, we exclude ads sponsored by parties, coordinated ads, or those sponsored by outside groups. We also exclude ads sponsored by primary election candidates that do not win their party's nomination.

national broadcast networks, and national cable networks, but it does not track ads aired on local cable stations. Fowler and Ridout (2014) notes that, while campaigns are utilizing local cable more frequently in recent election cycles, it still represents a "relatively small proportion of their campaign budgets on local cable."[17] Third, the estimated cost is only based on the cost of airing the advertisement; it does not account for production costs and consultant/agency fees, which can be considerable.

The likely consequence of these limitations (in particular, the latter two) is that the campaign expenditures directed to television are systematically understated across all levels of competitiveness. However, unless campaigns in more competitive districts spend more (or less) on production costs and local cable advertising relative to their television advertising expenditures as measured in the WMP data, it should not present a problem for inference.[18] Despite the limitations, the WMP offers the highest-quality data availability to assess the relationship between campaign expenditures on television advertising and competitiveness.

Table A.12 contains the OLS results for the three television advertisement outcomes. Looking first at columns (3) and (4), a 10-point increase in competitiveness is associated with an increase of about 700-900 airings of ads (depending on the measure of the competitiveness). As a reference point, on average, a 2012 congressional district had 901 airings sponsored by the two candidates. Moving to columns (5) and (6), a 10-point swing in competitiveness is associated with 2.3-2.8 additional unique ads aired. For the 2012 election, on average, a congressional district had 3.35 unique airings, so 2-3 additional unique advertising spots is a sizable increase.

The primary question of interest from our analysis of television advertising is whether campaigns allocate a greater share of their budgets to television advertising as competitiveness increases. Based on columns (1) and (2), a 10-point increase in competitiveness is associated with an 8-10 percentage point increase in the share of campaign budgets devoted to television advertising (depending on the measure of competitiveness). However, examining Figures A.18-A.19, it is apparent that the relationship between the share of expenditures on television and competitiveness is non-linear. Eyeballing these two figures and the LOESS curve, the relationship is approximately flat between 100/0 and 70/30 districts, the slope of the curve is somewhat steep between 70/30 and 60/40 districts, and very steep between 60/40 districts and 50/50 districts. Based on the fitted values of the locally weighted regression, moving from a 60/40 to a 50/50 district is associated with a 12 percentage point increase in the share of campaign budgets spent on television for *PVI Competitiveness* and a massive 24 percentage point increase for *Ex Post Competitiveness*.

[17] One recent projection ahead of the 2014 election estimated that spending on local cable political ads would comprise 19-27 percent of all spending on political television ads (Wilner 2014). This estimate is based on all political ad spending not just spending for House races.

[18] Said differently, to the extent to that campaigns in more competitive districts spend a similar amount on production costs and local cable ads *relative to their WMP advertising expenditures*, the data limitations result in a shift in the intercept (lower expenditures across all districts) rather than a change in the slope).

Table A.12 – Television Advertising in 2012 House Elections

	TV as Share of Exp.		Number of Ads		Unique Ads	
	(1)	(2)	(3)	(4)	(5)	(6)
PVI Competitiveness	0.008***		71.143***		0.231***	
	(0.001)		(10.929)		(0.031)	
Ex Post Competitiveness		0.010***		90.488***		0.283***
		(0.001)		(8.629)		(0.024)
Constant	0.219***	0.273***	1667.559***	2200.041***	5.841***	7.410***
	(0.015)	(0.015)	(144.840)	(146.592)	(0.413)	(0.413)
Observations	385	385	385	385	385	385
Adjusted R^2	0.121	0.248	0.097	0.221	0.124	0.259

Standard errors in parentheses. * p < 0.05, ** p < 0.01, *** p < 0.001
The three dependent variables are: expenditures on television advertisements as a share of total campaign expenditures, the number of television ads aired throughout the campaign, and the number of unique ads aired. The sample is comprised of contested general elections by a D and R candidate. All elections in Louisiana are excluded from the sample due to their unusual rules.

Figure A.18 – TV Ads as Share of Campaign Expenditures vs. PVI Competitiveness

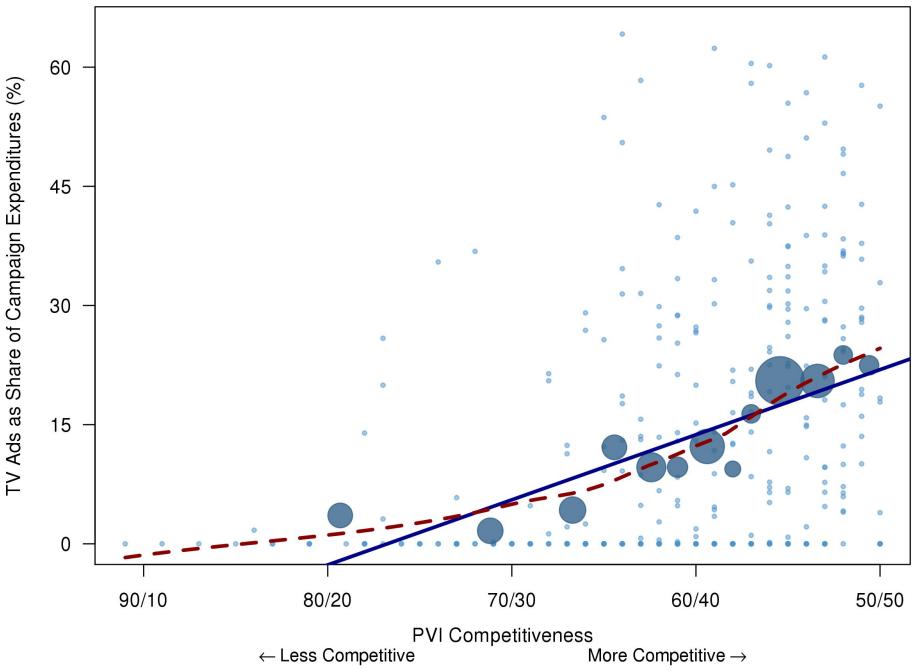
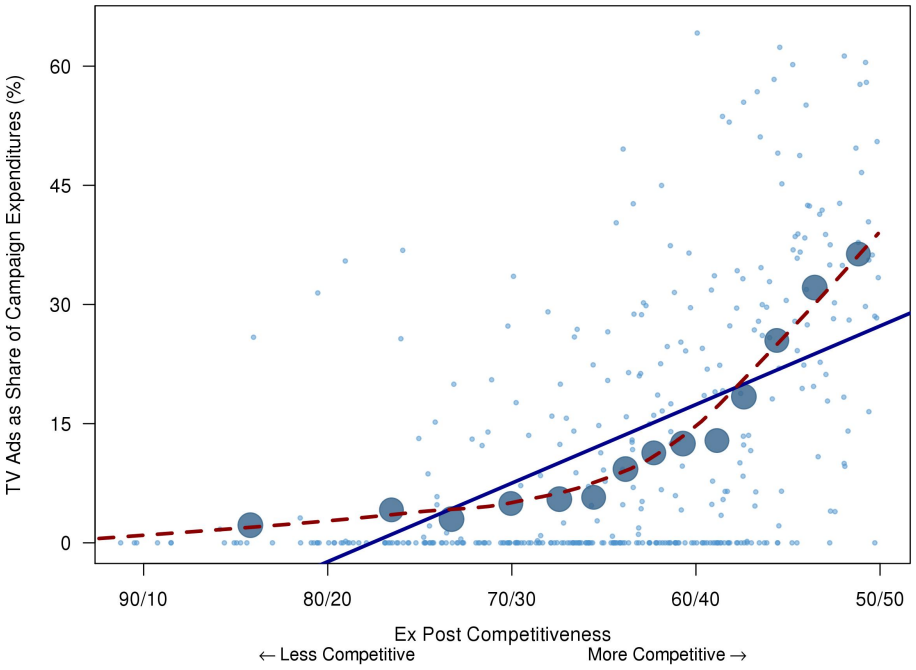


Figure A.19 – TV Ads as Share of Campaign Expenditures vs. Ex Post Competitiveness



These figures shows the strong relationship between the share of campaign expenditures directed to television advertising and electoral competitiveness. *PVI Competitiveness* is displayed in the top panel and *Ex Post Competitiveness* in the bottom panel. The solid blue line represents the least-squares regression line, while the red dashed smoothed curve is based on a locally weighted regression (LOESS). Each of the larger circles in the figure corresponds to a local mean and is proportional in size to the number of observations within the locale. The small dots represent each contested House race between a Democrat and Republican in 2012 (excluding races in Louisiana). Five observations are not displayed on the plot due to their unusually high values (possibly measurement error), but they were used to fit the least-squares line and LOESS curve in the plots.

A.5.4 Elite Mobilization: Direct Contact with Voters

We use data from the 2010-2014 Cooperative Congressional Election Study (CCES) Panel Study to measure direct campaign contact with voters. Because the 2010-2014 CCES Panel spans the redistricting cycle, we can analyze the extent to which individuals report more or less campaign contact under differing levels of electoral competitiveness. The CCES asks respondents' if they experienced any campaign contact and which forms of contact in 2010, 2012, and 2014. We focus our analyses on the midterm election years (2010 and 2014), as House elections likely play a more prominent role in non-presidential years. While there are 9,500 respondents in this panel, as in our other voter-level panel analyses, we restrict to individuals who do not move residences (which reduces the sample by 1,712 respondents). Consistent with our other analyses, we also remove individuals if they reside in DC (which lacks representation in Congress) or Louisiana (which has strange electoral rules), or if they experience an uncontested election or an election between two Democrats or two Republicans in either 2010 or 2014; these restrictions reduce the sample by 1,547 respondents. Finally, an additional 19 respondents are excluded from the sample because they did not respond to questions about campaign contact in either 2010 or 2014.

These sample restrictions result in a balanced panel of 6,222 respondents. As with our other voter-level panel research designs, we include individual fixed effects to analyze *within person* variation in competitiveness. The state-year fixed effects account for statewide idiosyncrasies in a given election year (e.g., a senatorial election in 2010 but not 2014). Standard errors are clustered by the pairwise combination of pre- and post-redistricting congressional districts.

Table A.13 – Reported Campaign Contact

	Any Contact		In-Person		Phone		Text/Email		Mail/Postcard	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PVI Competitiveness	0.0024		-0.0022		0.0043		0.0014		0.0013	
	(0.0021)		(0.0025)		(0.0031)		(0.0026)		(0.0032)	
Ex Post Competitiveness		0.0041**		-0.0008		0.0056**		-0.0008		0.0040*
		(0.0015)		(0.0015)		(0.0018)		(0.0015)		(0.0020)
Observations	12444	12444	12444	12444	12444	12444	12444	12444	12444	12444
R^2	0.060	0.063	0.035	0.034	0.072	0.077	0.044	0.044	0.036	0.039
Individual FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting CD level, are in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
The dependent variable is coded =1 if the respondent reported campaign contact. The sample is based on respondents to the CCES 2010-2014 panel and is restricted to individuals in contested general elections by a D and R candidate. All Louisiana residents are excluded from the sample due to their unusual rules.

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