Appendix
The Elusive Quest for Convergence
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External Validation of Census Measures of District Interest

Here, we show that our demographic measures of district interest from the census correspond with other, independent measures of district interest. Many surveys measure the preferences of voters, but few measure their intensity of preferences, which is what we aim to measure in our study, so there are few survey questions that can be directly compared with our measures. However, the 2006 Cooperative Congressional Elections Study (CCES), fielded in the middle of our period of study, asked one multiple-choice question which provides a proxy for the intensity with which voters care about different policy domains. Specifically, the question asked "What is the most important problem facing the country?" Respondents could choose from 18 different choices or offer their own open-ended response. Two of the choices (war in Iraq and terrorism) relate to defense, one (education) relates to education, one (energy supply/gas and oil prices) relates to energy, one (social security and pensions) relates to senior issues, and two (poverty and housing) relate to welfare. Therefore, using these responses, we code binary variables indicating whether each respondent appears to care strongly about each policy domain. Of course, these measures of interest are noisy and imperfect, most choices were only selected by a few percent of all respondents. For example, someone might care much more about education than the average citizen but nonetheless say that the war in Iraq was the most important problem at that particular time.

In Table A1, we regress each of these measures of interest on the district-level demographic measure of interest used in this study. For example, we ask how survey interest in defense corresponds with the number of military personnel in the district, how survey interest in welfare corresponds with the percent of a district below the poverty line, etc. In each case, the demographic measure is rescaled as a percentile and then recoded to range from 0 to 1. In other words, each coefficient can be interpreted as the change in the probability a survey respondent selected that particular answer as the district-level demographic goes from the lowest to the highest value. The constant term provides an estimate of the proportion of respondents in the lowest interest district who selected this response. In 4 of the 5 cases, our district-level demographic measure of interest is positively related to the individual-level survey measure, and this relationship is statistically significant in 2 of those 4 cases. As we move from districts with the least energy workers to those with the most energy workers, the percentage of respondents identifying oil supply and gas prices as the most important issue increases from 2.2 to 3.1, a 40 percent increase. As we move from districts with the least residents below the poverty line to the most, the percentage of respondents listing poverty or housing as the most important issue increases from 1.3 to 2.6 percent, a 100 percent increase. Our results for defense, education, and senior issues are statistically null, although this could be attributable to the fact that these survey measures are only weak proxies for the extent to which residents care about that particular policy domain.

The 2006 CCES did ask another set of questions which partly measures the extent to which residents care about defense: "If the Congress were to balance the budget it would have to consider cutting defense spending, cutting domestic spending, raising taxes, or borrowing money to cover the deficit . . . What do you least want Congress to do?" If a respondent stated that their last choice is cutting defense spending, we interpret that to mean that they care about defense as a policy domain. Therefore, in the last column of Table A1, we test whether responses to this question correspond to the military personnel in the district, and indeed, the relationship is quite strong. As we move from the district with the least military personnel to that with the most, the percentage of respondents who least want to cut defense spending increases from 20.5 to 26.4, a 30 percent increase that is strongly statistically significant. In short, the results in Table A1 lend additional, independent support of our census measures of district interest. On the whole, our district-level demographics used to measure the intensity of preferences correspond strongly to individual-level survey reports of policy priorities.

Table A1. Survey and Census Measures of Interest, 2006 CCES

	Most Important Problem					Defense Spending
	Iraq/Terror	Education	Oil/Gas	Soc.Sec.	Poverty/Housing	
Military	−.010					.059
	(.012)					(.011)
Education		.001				
		(.004)				
Energy			.009			
			(.004)			
Senior				.001		
				(.003)		
Poverty					.013	
					(.004)	
Constant	.412	.023	.022	.013	.013	.205
	(.007)	(.002)	(.002)	(.002)	(.002)	(.006)
Observations	21,667	21,667	21,667	21,667	21,667	17,331

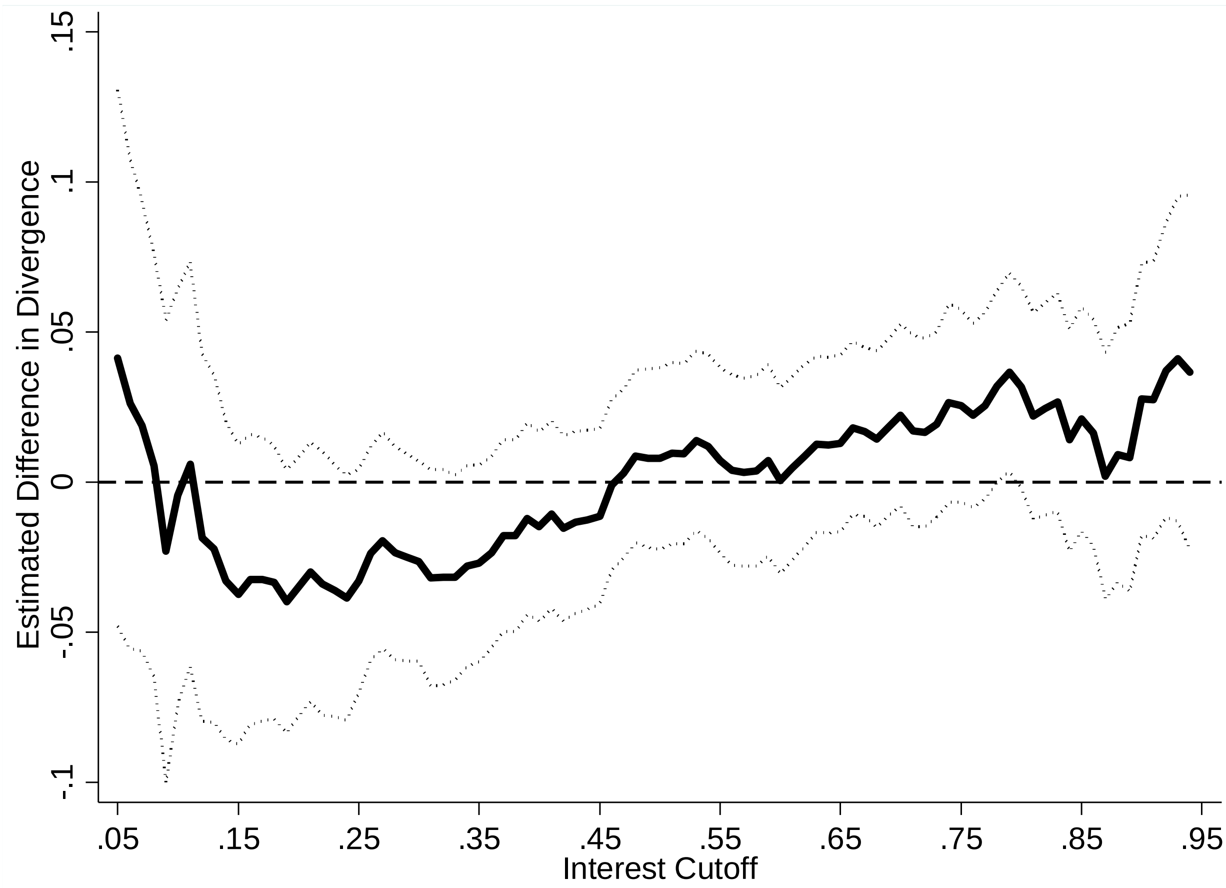
Robust standard errors in parentheses

Robustness across Different Interest Cutoffs

Our categorizations of low- and high-interest districts are somewhat arbitrary. For our main results, we categorize the 10 percent of districts exhibiting the highest level of the relevant demographic characteristic as *high interest*. For example, when analyzing agricultural bills, we classify the 10 percent of districts with the highest proportion of the labor force employed in farming as the high-interest districts. As discussed previously, coding interest as binary highly simplifies our analysis and prevents our results from dependence upon functional form assumptions. Of course, we have little *a priori* justification for a cutoff of .1 as opposed to .05, .15, .2, etc. If the cutoff is too close to 0 or 1, we will have limited statistical power, because our estimates of divergence for a group of few districts will be highly imprecise. However, outside of this constraint, we can re-estimate all of the previous analyses using any other cutoff. In the Appendix, we show results for all possible cutoffs between .05 and .95, and we see virtually the same results for all possible cutoffs. Also in the Appendix, we show that our results across many different RD specifications.

Figure A1 presents the results of our pooled analysis for all possible cutoffs between .05 and .95. For each cutoff, we re-estimate the average level of divergence in high- and low-interest districts and plot the difference, along with the corresponding 95% confidence interval. This difference is substantively small and statistically indistinguishable from zero for virtually all possible cutoffs, and the relationship between our cutoff and the estimated difference is virtually flat. This figure shows, in a non-parametric way, that there appears to be little relationship between constituent interest and divergence at any level of interest. Our main results would have been the same had we used any other cutoff in classifying high- and low-interest districts in each issue area.

Figure A1. Robustness of Results across Different Interest Cutoffs

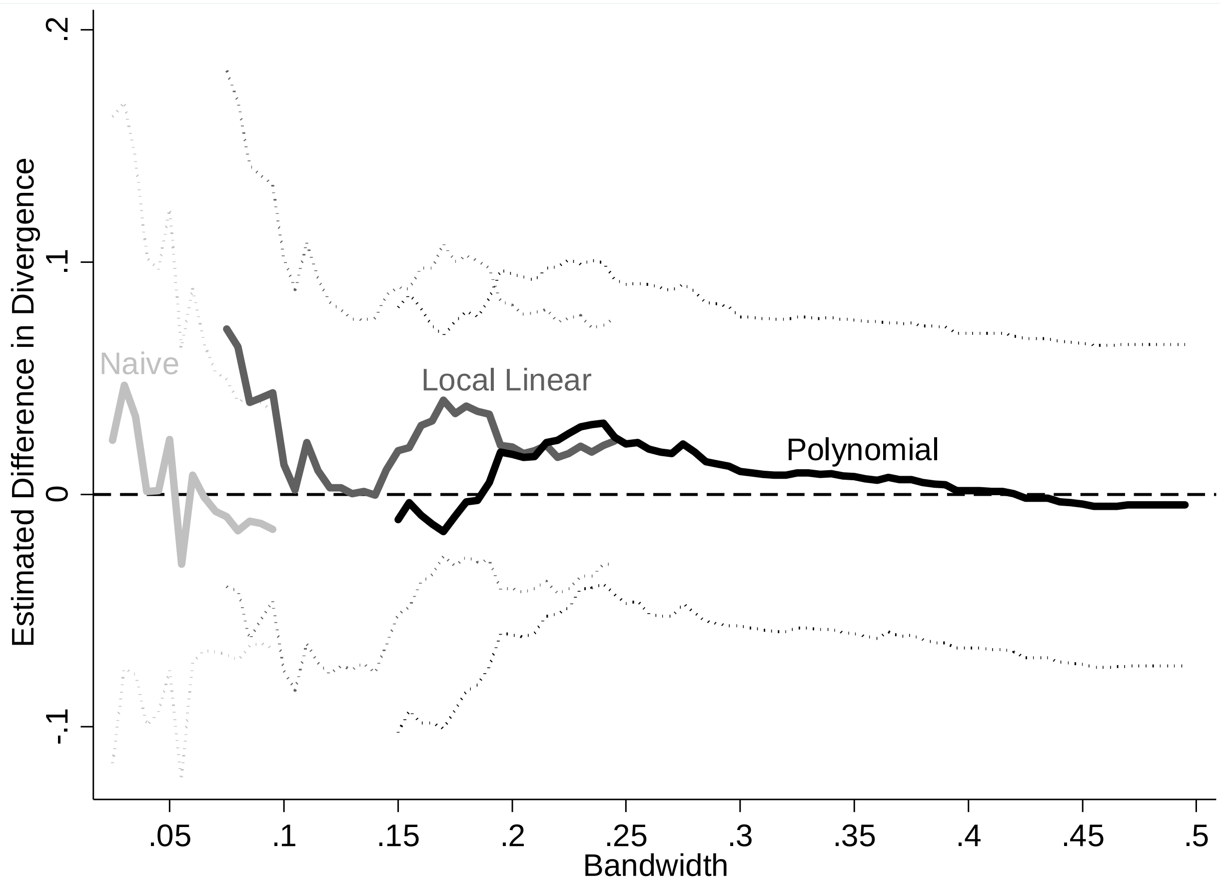


The figure presents our estimate of the difference in divergence between high- and low-interest districts when pooling across all policy domains as in Row 9 and Column 3 of Table 2. Specifically, the figure presents this estimated quantity for all possible cutoffs of district interest between .05 and .95. The solid curve provides the point estimates, and the dotted curves represent 95% confidence intervals computed from district-clustered standard errors. Our estimate from Table 2 is shown in the figure where the interest cutoff is set at .1—i.e., the top 10% of districts are classified as high-interest. We see that our estimated difference in divergence is substantively small and statistically indistinguishable from zero for virtually all possible cutoffs. This non-parametric analysis suggests that high-interest districts indeed see no greater divergence than low-interest districts for any possible delineation of interest. It also suggests that our main results are not sensitive to any particular cutoff.

Robustness across Different RD Specifications

For our main results, we have implemented our RD design by including all contested elections and modeling the running variable as a fourth-order polynomial, following the convention of Lee (2008). However, Figure A2 shows that our results are highly robust to alternative specifications and bandwidths. Specifically, Figure A2 presents our pooled estimate of the difference in divergence between high- and low-interest districts using three different RD estimation strategies and many different bandwidths. The naive specification estimates divergence by simply computing a difference in means between close Republican and Democratic victories, the local linear specification estimates divergence through separate linear regressions for close elections on each side of the electoral threshold, and the polynomial approach includes a fourth-order polynomial. The figure shows that our results are highly robust across different specifications and bandwidths. This is reassuring for several reasons. First and foremost, our results are not sensitive to arbitrary choices of specification. Second, our results are not sensitive to the use of high-order polynomials, which can produce misleading results if not used carefully (Gelman and Imbens 2014). Third, although some imbalances appear for the small sample of very close U.S. House races in recent years (Caughey and Sekhon 2011; Snyder 2005), the robustness of our results across specifications which implicitly put more or less weight on this small sample of very close races suggests that these observed imbalances hold little implication for our quantities of interest.

Figure A2. Robustness of Results across RD Specifications



The figure presents the robustness of our results across alternative RD specifications. Specifically, the figure shows the estimated difference in divergence across all issue areas (Row 9 and Column 3 of Table 2) for three different specifications and for many possible bandwidths within those specifications. The naive specification (light gray) estimates divergence by comparing means around the electoral threshold. The local linear specification (dark gray) estimates divergence using separate linear regressions on each side of the threshold. The polynomial specification (black) includes the running variable as a fourth-order polynomial. The solid curves provide the point estimates, and the dotted curves represent 95% confidence intervals computed from district-clustered standard errors. The alternative specifications and bandwidths all yield nearly identical results. The difference in divergence between high- and low-interest districts is substantively small and statistically indistinguishable from zero.

Alternative Designs and Specifications

To address concerns about the generalizability of our estimates across different districts, and to address concerns about the validity of the RD assumptions in this setting, the following tables and corresponding discussion provide additional estimates which utilize alternative research designs and specifications. In Table A2, we replicate the results from Table 2 using a selection-on-observables design. Specifically, in this analysis we estimate divergence through a cross-sectional comparison of Democratic and Republican members of Congress, while flexibly controlling for the normal presidential vote share in the district—arguably the best available proxy for the general ideological orientation of the district. In each issue area, we regress CVP on a party dummy, congress fixed effects, and a fourth-order polynomial of the average presidential two-party vote share in the district across the 2004 and 2008 elections. When implementing the pooled analysis, we include congress-issue fixed effects, and we interact the presidential vote variables with dummies for each issue area, allowing presidential vote to predict roll-call voting in each issue area differently. A major shortcoming of this design is that it requires the strong identifying assumption that conditional upon normal presidential vote share, the party of a district's representative is as-if randomly assigned. However, this approach yields more statistically precise estimates and it allows us to estimate divergence for a much more generalizable set of districts. Reassuringly, the results in Table A2 are nearly identical to those in Table 2, suggesting that our main results are not a result of noise, not specific to the identifying assumptions of the RD design, and not specific to the particular set of districts that have close elections.

In Table A3, we replicate the results of Table 2 using a differences-in-differences design. Here, we estimate divergence by leveraging the set of districts that changed the party of their representative at some point in our period of study. Specifically, we estimate divergence by regressing CVP on a party dummy, congress fixed effects, and district fixed effects. When implementing the pooled analysis, we include congress-issue and district-issue fixed effects. The identifying assumption here is that districts that switched the party of their representative would have followed the same trends, on average, as other districts had they not switched the party of their representative. This assumption is largely independent of the identifying assumption required for the RD design, and it allows us to estimate the local average extent of divergence for all districts that switched parties, a different and potentially more generalizable set of districts than those leveraged in the RD design. Again, the results of Table A3 are nearly identical to those of Table 2, lending additional credibility to the internal validity and generalizability of our results.

Table A4 presents additional RD specifications designed to address concerns about the validity of our identifying assumptions. Snyder (2005) and Caughey and Sekhon (2011) show that in recent U.S. House elections, the incumbent party is more likely to have won very close elections—i.e., those few elections decided by less than one quarter of one percentage point. Additional variables that are strongly correlated with incumbent party are also imbalances, but Eggers et al. (2015) show these imbalances disappear when controlling for incumbent party, suggesting that there is essentially only one imbalance rather than many. Eggers et al. (2015) also argue and present evidence that this imbalance is most likely attributable to chance, but nonetheless, a chance imbalance could still influence our estimates. Table A4 presents our pooled results using our original specification along with three additional specifications designed to account for this potential imbalance. In the second row, we control for lagged incumbent party—the variable for which imbalance has been identified. In the third row, we control for lagged CVP. In the last row, we implement a donut RD design, where we exclude the sample of potentially problematic elections decided by less than one percentage point. The results are nearly identical across all 4 specifications, suggesting that observed imbalances hold little implication for our results.

Table A2. Selection-on-Observables Estimates

Issue Area	Low-Interest Districts	High-Interest Districts	Difference
Agriculture	.463 (.012)	.477 (.024)	.014 (.026)
Civil Rights	.377 (.009)	.312 (.088)	−.065 (.086)
Defense	.337 (.004)	.330 (.011)	−.007 (.012)
Education	.388 (.006)	.365 (.015)	−.023 (.016)
Energy	.438 (.009)	.401 (.021)	−.037 (.023)
Public Transportation	.477 (.016)	.131 (.039)	−.346 (.041)
Senior Issues	.688 (.008)	.671 (.023)	−.017 (.023)
Welfare	.588 (.007)	.612 (.009)	.023 (.011)
All Issues Pooled	**.468 (.007)**	**.466 (.018)**	**−.002 (.018)**
Placebo	.414 (.005)	.409 (.008)	−.005 (.007)

District-clustered standard errors in parentheses.

Table A3. Differences-in-Differences Estimates

Issue Area	Low-Interest Districts	High-Interest Districts	Difference
Agriculture	.447 (.021)	.532 (.044)	.085 (.048)
Civil Rights	.419 (.022)	.337 (.166)	−.082 (.164)
Defense	.328 (.009)	.315 (.031)	−.012 (.032)
Education	.388 (.012)	.349 (.022)	−.039 (.025)
Energy	.435 (.017)	.319 (.058)	−.116 (.059)
Public Transportation	.461 (.032)	.117 (.008)	−.345 (.033)
Senior Issues	.681 (.017)	.637 (.040)	−.044 (.042)
Welfare	.583 (.012)	.608 (.005)	.025 (.013)
All Issues Pooled	**.468 (.012)**	**.436 (.031)**	**−.032 (.034)**
Placebo	.403 (.009)	.396 (.014)	−.008 (.014)

District-clustered standard errors in parentheses.

Table A4. Additional RD Specifications (All Issues Pooled)

Specification	Low-Interest Districts	High-Interest Districts	Difference
Baseline	.468 (.015)	.464 (.035)	−.005 (.035)
Control for Lagged Incumbency	.472 (.016)	.487 (.032)	.014 (.033)
Control for Lagged CVP	.457 (.015)	.485 (.032)	.027 (.033)
Donut	.476 (.015)	.461 (.033)	−.016 (.034)

District-clustered standard errors in parentheses.

Has Congressional Representation Changed?

Our primary analyses focus on a recent period (2003-2010) where we have the richest available data on congressional districts and when members of Congress cast many roll-call votes in the relevant issue areas. Was representation importantly different in previous eras believed to have lower levels of political polarization (e.g., McCarty, Poole, and Rosenthal 2006) and party pressure (e.g., Snyder and Groseclose 2000)? Perhaps members of Congress were more likely to converge to their district's preferences on important issues in the past, and the recent era of strong parties and political polarization has produced the stark divergence that we detect for recent years.

To answer this question, we assemble the same data and follow the same procedures described above going back to 1953. Data on agricultural employment, military population, black population, and elder population are available from E. Scott Adler's Congressional District Data Set and have been analyzed in previous work including Adler (2002). Unfortunately, data on employment in the energy or education sectors, public transportation use, and poverty are not available for this earlier period. Also unfortunately, there were very few roll-call votes cast per congress in this earlier period on civil rights or senior issues. As a result, when analyzing this earlier period, we must confine our analyses to the issue areas of agriculture and defense.

In Table A5, we conduct the same analyses presented in Table 2 for agriculture and defense, but focusing on earlier periods. Specifically, the table presents separate results for each redistricting cycle starting in 1953, and the table re-prints the same results from Table 2 which focused on the most recent period (2003-2010). For both issues and for all time periods, divergence in low-interest districts is substantively large and statistically significant. However, unlike our previous results, we see modest evidence that divergence was smaller in high-interest districts. Our estimated difference in divergence between low- and high-interest districts is negative for 8 of the 10 new tests, and this difference is statistically significant in 3 cases. We estimate that divergence was 7 to 10 percentage points lower in high-interest districts for agriculture in the 1980s and for defense in the 1980s and 1990s. We also obtain negative point estimates for the 1950s and 1960s, although the standard errors are too large to reject the null of no difference.

While this evidence is only suggestive, our results in Table A5 indicate that representation may have been importantly different in the past, particularly in the 1980s. Anecdotal evidence suggests that Congress has become increasingly partisan, and that the extent to which members deviate from their party to cater to constituent interests has decreased. Our findings partly corroborate this account. They also suggest that previous scholarly accounts of congressional representation revolving around constituent interests may have been accurate in a previous era, but that those accounts are now outdated.

Table A5. Historical Results for Agriculture and Defense

	Agriculture			Defense		
Years	Low-Interest	High-Interest	Difference	Low-Interest	High-Interest	Difference
1953-1962	.463 (.020)	.374 (.086)	−.089 (.085)	.215 (.012)	.195 (.040)	−.020 (.040)
1962-1972	.369 (.023)	.344 (.051)	−.025 (.054)	.145 (.015)	.105 (.040)	−.039 (.041)
1973-1982	.256 (.017)	.247 (.040)	−.009 (.043)	.222 (.018)	.223 (.041)	.001 (.044)
1983-1992	.259 (.023)	.152 (.038)	−.108 (.044)	.390 (.020)	.294 (.041)	−.096 (.044)
1993-2002	.330 (.016)	.356 (.033)	.025 (.036)	.384 (.016)	.314 (.032)	−.071 (.035)
2003-2010	.488 (.027)	.506 (.067)	.018 (.070)	.332 (.011)	.304 (.032)	−.028 (.033)

District-clustered standard errors in parentheses.

Appendix References

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