Online Appendix

Long-Term Consequences of Election Results

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Does Strategic Sorting Bias our Estimates?

Several recent papers criticize the use of RD designs in electoral settings and raise the possibility that strategic sorting biases RD estimates.[1] The specific concern is that electoral fraud and strategic campaign effort could cause certain high-quality candidates to win extremely close elections more often than expected. We are sensitive to this concern, because this type of strategic sorting could bias our estimates and lead us to overestimate the long term consequences of elections. After conducting a number of robustness tests, we are confident that strategic sorting does not pose a problem for our estimates. Figures A1 presents some of these robustness tests for the U.S. House. The blue line replicates the result previously presented in Figure 8. Each point represents the effect of an election result on the CVP of a district's representative for a particular number of terms downstream.

The red and green lines represent two sets of robustness tests of whether strategic sorting biases our estimates. For the red line, we modify the original analysis by excluding all elections where the Republican vote share was within 0.5 percentage points of the .5 threshold. This "donut" RD estimation[2] excludes those extremely close elections where fraud and strategic sorting are a particular concern. For the green line, we add several covariates—lagged vote share and lagged incumbency—to our RD estimation to control for potential imbalances that may have arisen from strategic sorting. For both sets of robustness tests our results are completely unchanged. Even after accounting for sorting in multiple

[1] Snyder 2005; Caughey and Sekhon 2011; Grimmer et al. 2012.

[2] Almond and Doyle 2011; Barreca et al. 2011; Barreca, Lindo, and Waddell 2011.

ways, we recover almost the exact same point estimates that we obtained in our original analysis. Therefore, strategic sorting does not appear to pose a problem for our estimates.

Further evidence against the sorting hypothesis comes from alternative specifications and implementations of the RD estimation. The researcher must make several choices in estimation including the bandwidth of data included in the analysis and the order of the polynomial. Flexible model specifications (high order polynomials, small bandwidths) will rely very heavily on observations very close to the threshold, while rigid specifications (low order polynomials, large bandwidths) will be less sensitive to strategic sorting around the threshold. If many different specifications of varying flexibility yield the same results, then the researcher can be more confident in the results and the assumption that potential outcomes do not change discontinuously at the threshold. Figure A2 replicates the results from Figure 8 with many different specifications, ranging from very rigid to very flexible. Specifically, we utilize local linear regressions and vary the bandwidth from .03 to .1, third-order polynomials with bandwidths ranging from .05 to .5, and fourth-order polynomials with bandwidths ranging from .05 to .5. Results are nearly identical across all specifications.

Figure A1. Long Term Consequences of Elections – 2 Tests of Robustness

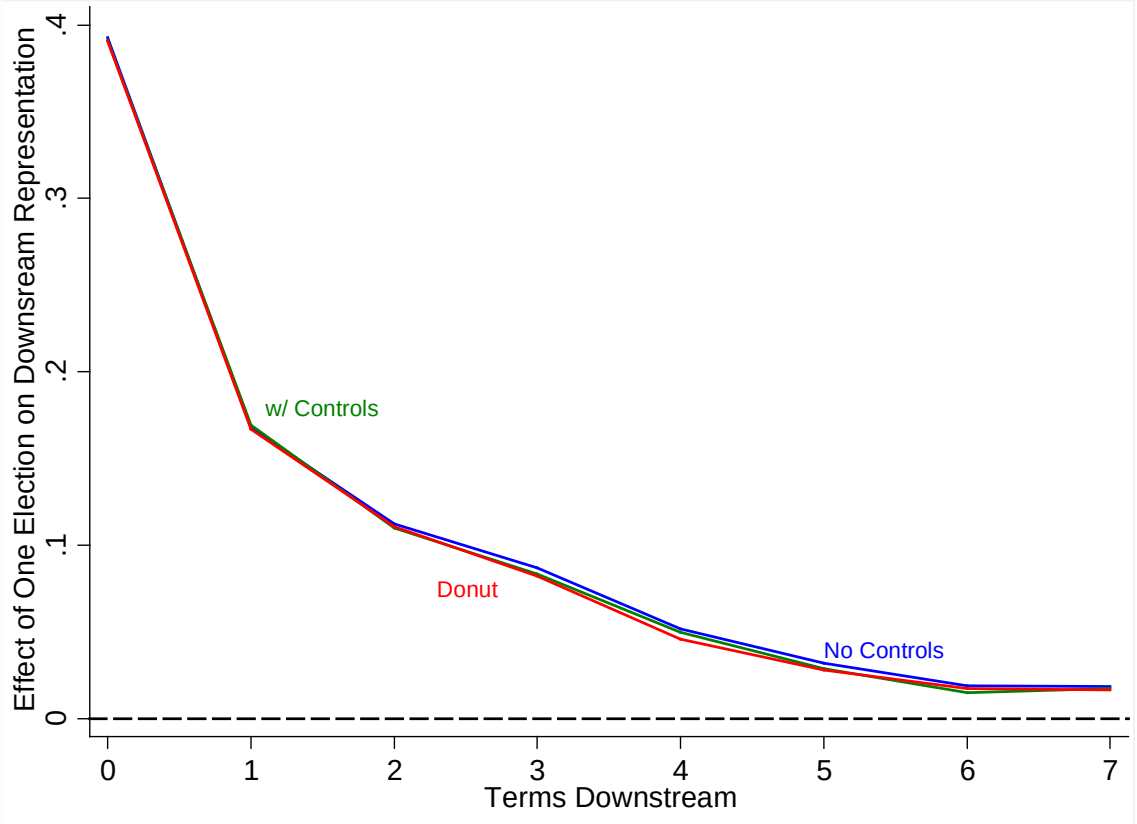
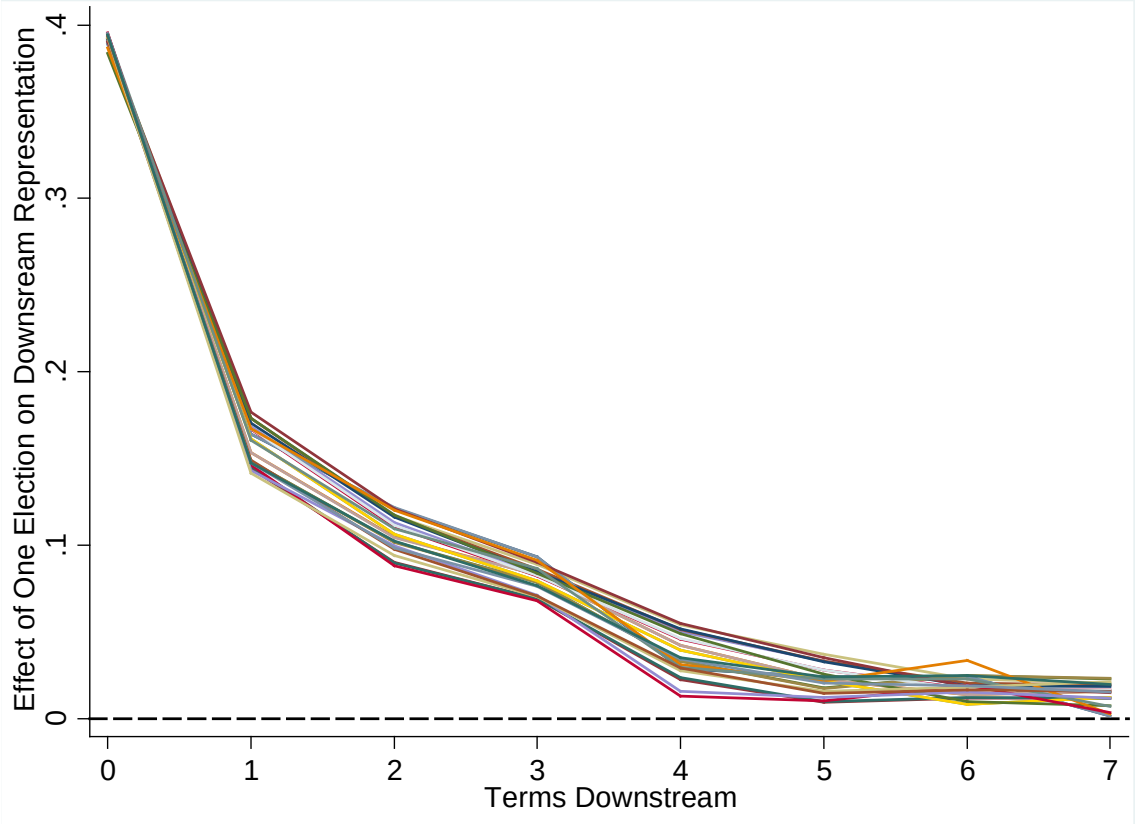


Figure A2. Long Term Consequences of Elections – Many Alternative Specifications



Does Redistricting Influence the Long-Term Consequences of Election Results?

For our main analysis of the U.S. House and state legislatures, we track the voting behavior and roll-call representation of the same district over time, despite the fact that the composition of the district may change as a result of redistricting. This is obviously not an issue for our analysis of the U.S. Senate as state boundaries are constant. Because the time frame of our analysis could potentially span one or more redistricting cycles, we might wonder whether changes in district boundaries influence the long-term consequences of elections.

The effects of redistricting on these long term effects are ambiguous. On one hand, incumbents could use redistricting to keep themselves in power by redrawing their district to improve their electoral chances, although Ansolabehere and Snyder find that redistricting does not systematically benefit incumbents.[3] On the other hand, incumbents could suffer from redistricting as they lose more of the "personal vote"[4] and as the preferences of their electorate change. Relatedly, redistricting could introduce attenuation bias in our analysis as the location and representative for a particular district can completely change as a result of redistricting. Here, we test for these possibilities and find that redistricting appears to have no detectable effect on our estimates in either direction.

To address these questions, we replicate our U.S. House analyses from Figures 3 and 8 using only elections that took place in redistricting years (1952, 1962, 1972, 1982, 1992, and 2002). For this subset of elections, we test for the effects of one election on subsequent elections and downstream representation through 4 terms downstream, while the composition of districts remains constant. For example, consider the election of 2002. We

[3] Ansolabehere and Snyder 2008.

[4] Ansolabehere, Snyder, and Stewart 2000.

can estimate the effect of 2002 election results on subsequent elections from 2004 to 2010 and on roll-call voting from 2003-2012, a time period during which the composition of districts did not change. Figure A3 replicates the analysis from Figure 3 for all years (purple) and only redistricting years (green). If incumbents use redistricting to benefit their electoral chances, the estimates for all years should be significantly greater than those for only redistricting years. Alternatively, if redistricting hurts incumbents or induces attenuation bias, the estimates for redistricting years should be significantly great. However, the estimates are virtually identical for both samples suggesting redistricting does not exhibit a meaningful effect in either direction.

Figure A4 presents the same analysis for downstream representation (a replication of Figure 8), and again we find virtually identical estimates from the entire sample and from only redistricting years. Redistricting exhibits no discernable effect in either direction on the effect of one election on downstream representation.

Unfortunately, this kind of analysis is not possible for state legislatures, because we only have state legislative roll-call data available for 1999-2000 and 2011-2012. Since state legislatures are responsible for redistricting, it could play a more important role in this setting. However, we do not detect a kink in our state legislative results from Figures 3 and 8 that we might expect if redistricting exhibits large effects in these settings. Moreover, since both of these periods of available roll-call data take place toward the end of their respective redistricting cycles, the first 8 to 10 years of our state legislative analyses are not influenced by redistricting.

Figure A3. Replication of Figure 3 for Windows with Constant Districts

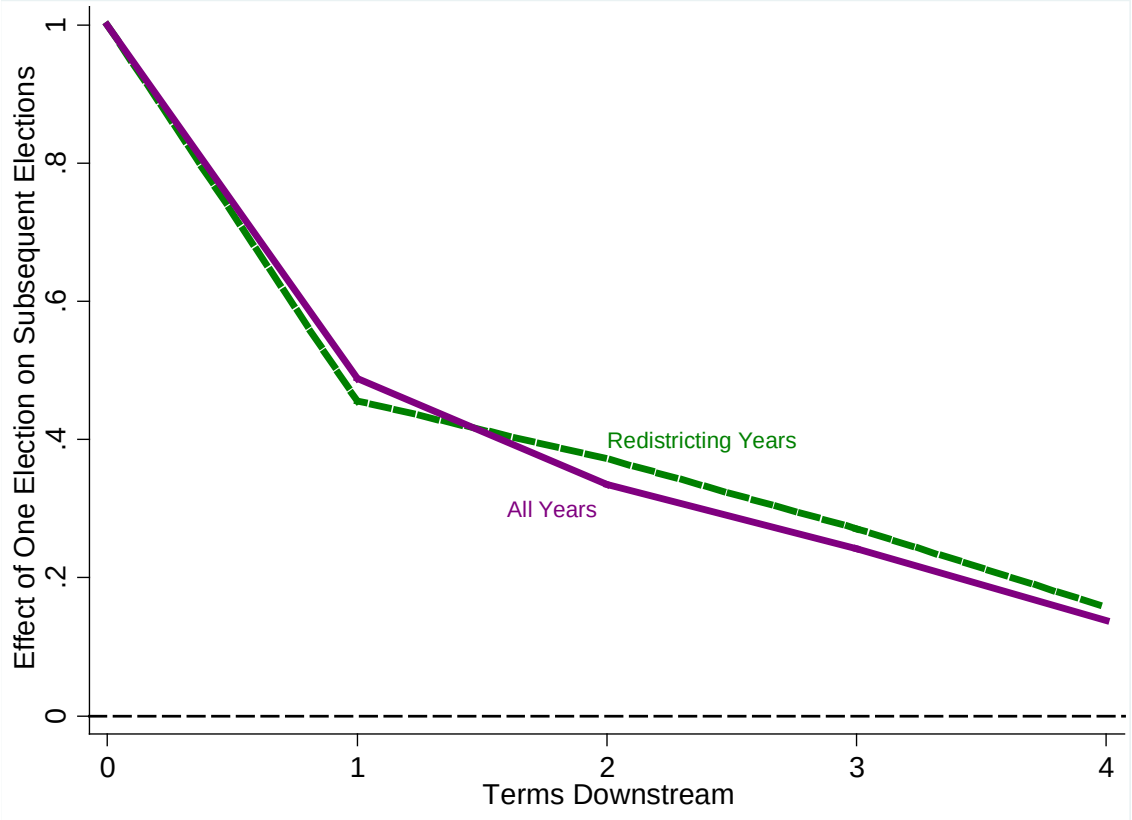
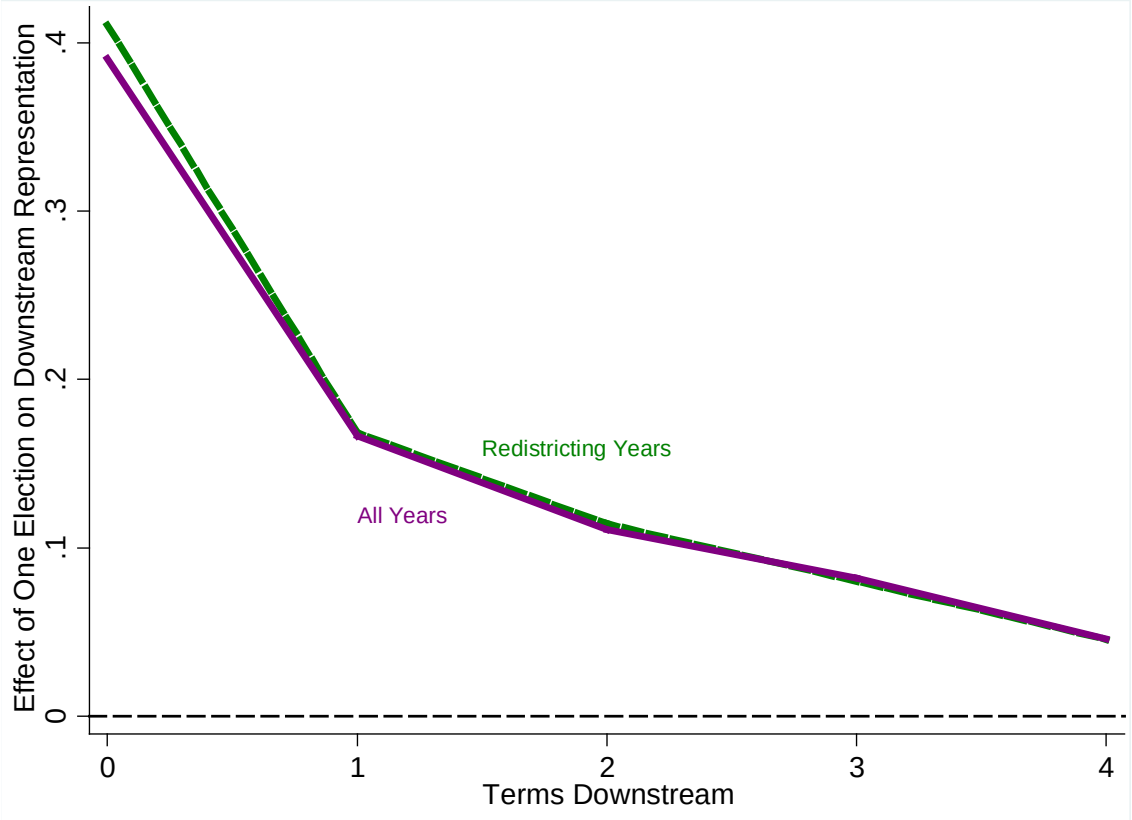


Figure A4. Replication of Figure 8 for Windows with Constant Districts



Replications using DW-NOMINATE Instead of CVP

Here, we replicate our analyses from Figures 2 and 8 using DW-NOMINATE instead of CVP. We prefer CVP for our main analysis because of its simplicity and substantive interpretability, although we obtain the same results using DW-NOMINATE. Similarly, we would obtain nearly identical results using Heckman-Snyder scores and Bayesian IRT scores which are both highly correlated with DW-NOMINATE.[5]

One complication is that we conduct our main analyses at the district or seat level rather than at the level of individual politicians. For examples, in analyzing the U.S. House, we construct CVP estimates for every district-congress, pooling roll-call votes or multiple individuals in cases where multiple legislators represented the same district in the same term due. However, DW-NOMINATE estimates apply to individual legislators. Therefore, in the cases where multiple legislators served in the same seat and the same congressional session, we average the DW-NOMINATE scores for all legislators that served, weighting each one by the proportion of roll-call votes cast by each person.

Figures A5 and A6 replicate our analyses of the House and Senate from Figures 2 and 8, respectively, using DW-NOMINATE instead of CVP as the dependent variable. In each case, the figures look nearly identical to those using CVP. Of course, the scales and point estimates are not comparable so we cannot directly compare the point estimates, but the shapes of the figures are nearly identical. One advantage of CVP is that the point estimates can be directly interpreted in terms of probabilities of voting conservatively while the point while the point estimates using DW-NOMINATE are arbitrary and can only be interpreted relative to one another. Nonetheless, the figures demonstrate that our results are not sensitive to the specific roll-call measure that we use.

[5] See Clinton, Jackman, and Rivers 2004 for comparisons of these three measures.

Figure A5. Replication of Figure 2 with DW-NOMINATE

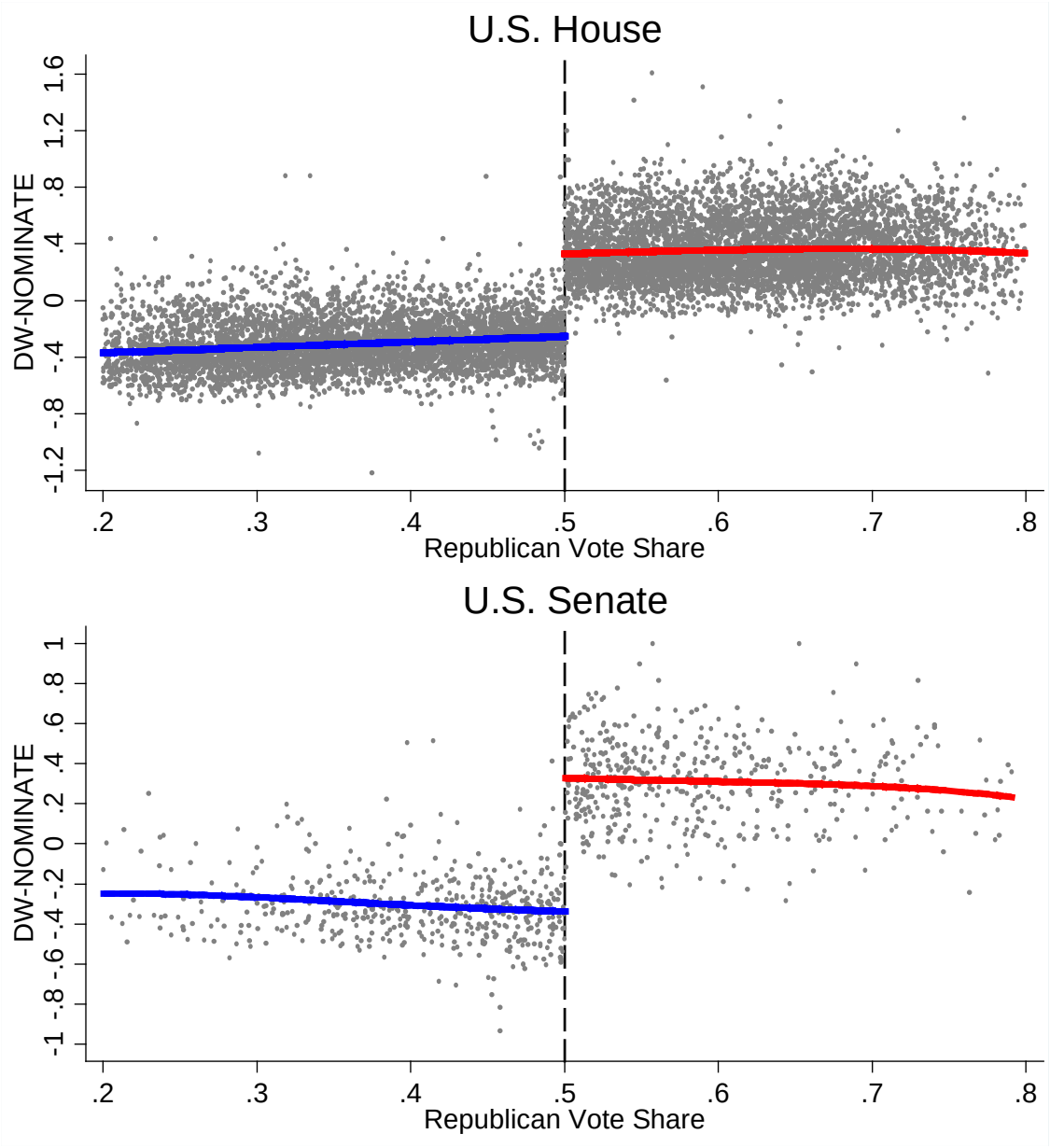
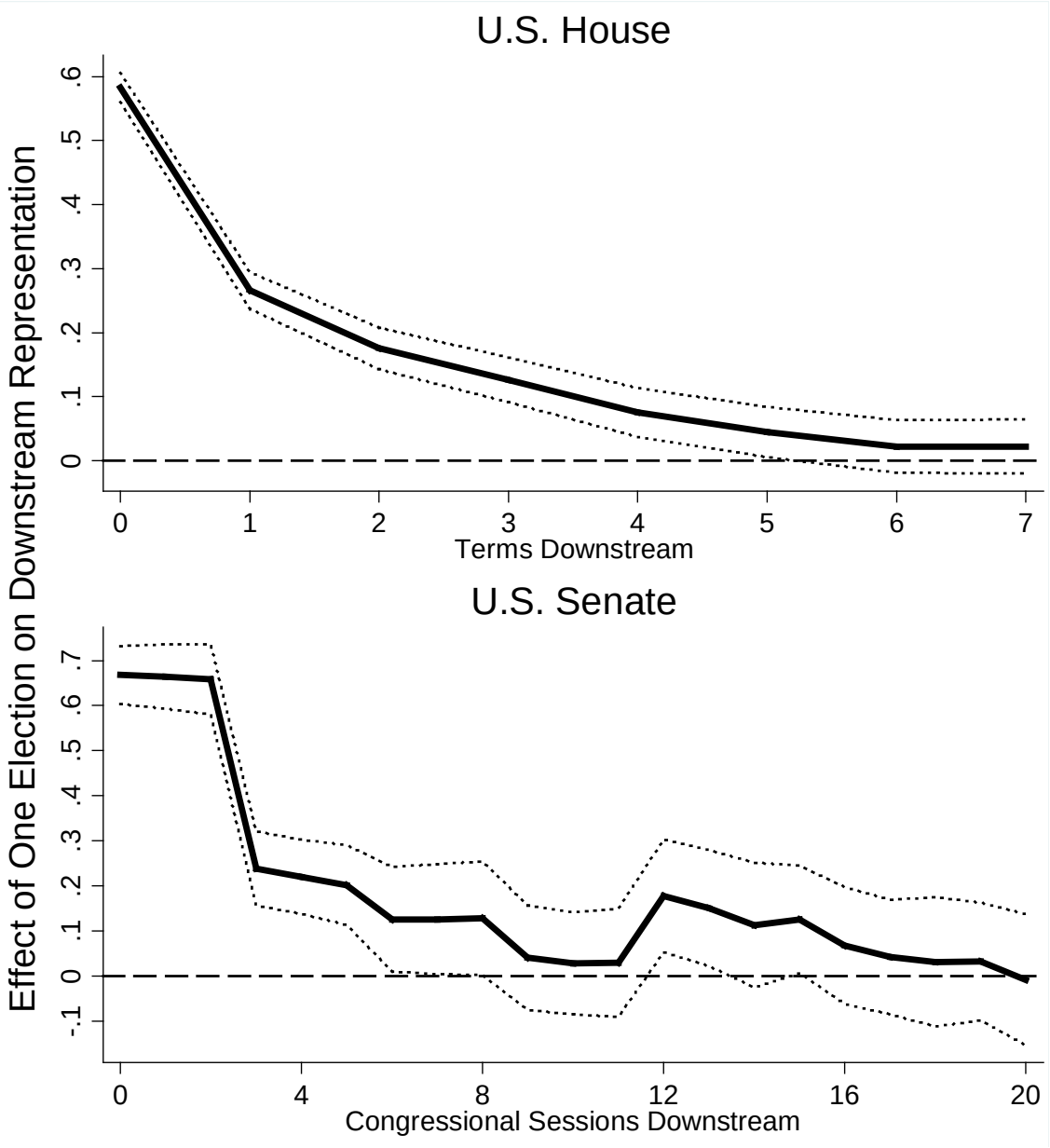


Figure A6. Replication of Figure 8 with DW-NOMINATE



Variation in the Effects over Time

Our main analyses present average effects across a wide range of years. Even if the effects of elections have varied significantly across the period of study, our estimates still provide unbiased averages. Nonetheless, this variation is interesting in and of itself, and we explore it here. Due to sample size limitations in the Senate and data limitations for state legislatures (remember that the roll-call data is only available for 1999-2000 and 2011-2012), we focus our analysis here on the U.S. House.

Figure A7 presents variation over time on the effect of one election on subsequent elections. In principle, we could replicate our analyses for each year in our study, but these estimates would be highly imprecise due to sampling error and the small number of close elections in each year. However, we can obtain more precise estimates by examining several elections at once. Here, we analyze eight-year windows, encompassing 5 different election years. Figure A7 replicates the results from Figure 3 for all possible eight-year windows in our sample. For example, the left most points on the figure—1950—refer to the 1946, 1948, 1950, 1952, and 1954 elections, those closest in proximity to 1952. Then, for example, at 1952, we present results from 1948-1956, etc. At any given point along the horizontal axis, the graph presents RD estimates of the effect of one election result on subsequent election results 1 to 8 terms downstream. As k increases, the maximum year on the graph decreases, because we have not yet observed the relevant elections.

Partially consistent with the incumbency advantage literature, we see that the effect of one election result on subsequent elections increased in the last half of the 20th century and then declined in recent years. However, the changes have been neither drastic nor monotonic. Throughout the entire period of study, a single election result had had substantively large and statistically significant effects on subsequent elections at least 4 or 5

terms downstream, and the consistency and magnitude of these results stand out more than the variation.

Figure A8 presents the same extension for Figure 8, analyzing variation over time in the effect of one election result on downstream representation. Figure A8 presents one version of this analysis using CVP, to mirror that in Figure 8, and another using DW-NOMINATE, mirroring the analysis in Figure A6. We show results for both measures to assure readers that our results are not sensitive to the specific measure and because DW-NOMINATE scores are intended to be comparable over time while CVP scores are not (their scale can vary depending on the congressional agenda in each session). Again, we find that the effects of one election on downstream representation have increased slightly over time. However, these effects are substantively large and quite consistent across the entire period of study.

Figure A7. Variation in Figure 3 Estimates over Time

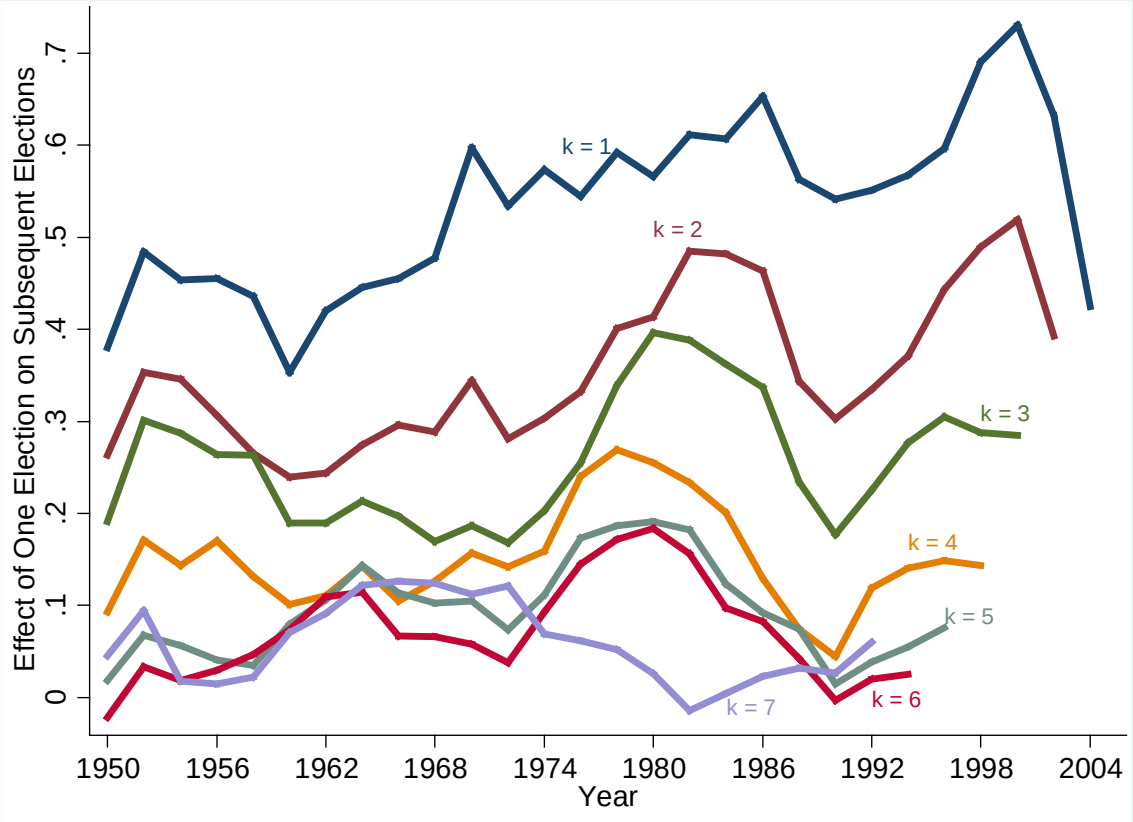


Figure A8. Variation in Figure 4 Estimates over Time