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ABSTRACT

This paper develops a new approach to detecting electoral fraud. Our context involves repeaters, individuals voting in multiple states in the U.S. during 19th Century Congressional Elections. Given high travel times, and the associated difficulties of voting in multiple states on the same day, we exploit the staggered introduction of holding federal elections on the first Tuesday after the first Monday in November (1T1M). The key finding is that county-level turnout rates fell when the closest neighboring state coordinated on 1T1M. This result is consistent with 1T1M adoption making repeating more difficult. In terms of mechanisms, the pattern is stronger in states that had not yet adopted the secret ballot, consistent with the secret ballot itself reducing voter fraud. The pattern is also driven by smaller population counties, consistent with repeaters particularly inflating turnout rates in these places.

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1 Introduction

There is substantial concern around the world over the issue of voter fraud and the integrity of elections. Indeed, this issue lies at the core of a well functioning democracy. Electoral fraud allegations and disputed elections can generate political instability. Following the 2020 Presidential Elections, fraud allegations by President Trump have been identified by the January 6 Select Committee as a key contributor to the attack on the Capitol in early 2021. In the historical context, fraud allegations and the associated disputes over who won the 1876 Presidential election had to be resolved by a Congressional committee. Outside of the U.S., disputes over the 2007 Presidential election results in Kenya led to protests and violence, with over 1,000 Kenyans killed.[1] Such disputes can also lead to economic instability. For example, disputes and fraud allegations during the 2017 general elections in Honduras led to a national strike.[2]

In this paper, we focus on a specific form of fraud, repeat voting, under which voters cast multiple ballots. Evidence from U.S. Presidential elections during the modern era suggests that repeat voting is rare (Goel et al. (2020)). Prior to the introduction of modern voting institutions, such as voter registration systems and the secret ballot, by contrast, evidence suggests that the practice was more pervasive. For example, in the 1876 Presidential election, the number of ballots cast in South Carolina exceeded the number of registered voters. Moreover, allegations of repeat voting are common in less developed countries today. During recent elections in Afghanistan, for example, election observers alleged that voters engaged in repeat voting, despite the use of finger ink at polling stations.[3]

To detect repeat voting, we focus on voters casting ballots in multiple states in the U.S. during the 19th Century and exploit the staggered adoption across states of voting on the first Tuesday after the first Monday in November (1T1M). Reducing the practice of repeating was one of the rationales for the movement towards 1T1M, as argued in an editorial by the *New York Times*: "Few things . . . have done more to encourage bribery, fraudulent registry, repeating, and mob violence at the polling-booths than the want of uniform State election days."[4] Given high travel costs at the time, voters often spent considerable time travelling to their polling stations, making it difficult to vote in multiple states on the same day.

We identify repeat voting indirectly, by examining how turnout rates adjust in response to states synchronizing their election dates with their neighbor during the transition to 1T1M.

[1] https://nation.africa/kenya/news/politics/Kenya-since-post-election-violence-/1064-4046876-12j38pyz/index.html (accessed July 2022)

[2] https://en.wikipedia.org/wiki/2017$_Honduran_general_election (accessed July 2022)$

[3] https://www.reuters.com/article/us-afghanistan-election-fraud/afghans-scrub-fingers-clean-to-cast-extra-votes-idUKTRE68H0QF20100918 (accessed July 2022)

[4] New York Times, November 3, 1878

Using county-level data on voting, our key finding is that turnout rates fell 6 percentage points, or roughly 11 percent relative to baseline turnout rates, when counties voted on the same day as their closest neighboring state in midterm elections. After exploring the robustness of our results, we explore the degree of geographic heterogeneity, finding that the reduction in turnout following synchronization of election dates is larger in the South than in the North. This is consistent with existing evidence of more fraudulent voting in the South than in the North during this time period.

We next address four alternative explanations for our results. First, it could be that our results capture the direct effect of the adoption of 1T1M, rather than the indirect effect of synchronizing elections dates with neighboring states. To address this concern, we show that our results with respect to synchronizing election dates with nearby states are robust to direct controls for 1T1M adoption. Second, it could be that states were adopting 1T1M during time periods when turnout was already declining. We address this both via controls for leads and lags of synchronization and via event study figures, which document a lack of pre-trends in county turnout rates and a sharp reduction in turnout following synchronization of election dates with neighbors. Third, it could be that states made other changes to their voting systems in conjunction with 1T1M adoption. We address this, as noted above, by including direct controls for 1T1M adoption. In addition, and given the importance of changes in electoral institutions in the South during this time period, we show that our results are robust to controls for the adoption of new constitutions that were designed in part to disenfranchise black voters in the U.S. South following reconstruction. Fourth, it could be that our results are driven by a reduction in turnout in counties that voted earlier than their neighbors prior to synchronization. That is, if voters are strategic and understand that their vote might have a multiplier effect, in the sense that their vote influences the choices of voters in regions of the neighboring state that vote late, then there could be a reduction in turnout in these early counties when elections are synchronized. To the extent that this reduction in turnout is not offset by an increase in turnout when synchronizing election dates in counties that previously voted late, synchronization could lead to a reduction in turnout on net when elections are synchronized. We address this by examining differences in effects between early and late counties and document that, if anything, our results are driven by synchronization in places that previously voted late.

We then turn to an examination of potential mechanisms driving the reduction in turnout rates. First, we show that our results are driven by states that had not yet adopted the secret ballot. This is consistent with parties providing ballots to and contracting with repeaters prior to the adoption of the secret ballot and also with the secret ballot eliminating the practice of ballot stuffing, the casting of multiple ballots at a single polling station by repeaters. Second, our results are stronger in small population counties, consistent with out-of-state repeaters inflating turnout rates in these counties with a small number of eligible voters. We also investigate the role of rail lines, distance to the border, and water borders, relative to land borders, but do not find any statistically significant roles for these factors in driving our results.

For comparison purposes, we then investigate the effects of synchronization of Presidential election dates across states on turnout in Presidential elections. We have less variation here, given that our county-level voting data start in 1840 and Presidential election dates were fully synchronized much earlier, by 1848. While this makes it difficult to assess pre-trends, our baseline regression results also document that county-level turnout rates fall in response to states synchronizing Presidential election dates with neighboring states. We also use Presidential elections and the electoral college, which might create incentives for repeaters and parties to focus on swing states, to study the role of electoral competition. Finally, we investigate the partisan implications of our results. That is, we investigate whether the Democratic party was advantaged or disadvantaged by the synchronization of elections dates and the associated reduction in repeat voting.

Our paper is most closely related to a literature examining electoral fraud, and efforts to curb such fraud, in the United States during the 19th Century. There is substantial anecdotal around the pervasiveness of electoral fraud during this era, resulting from, among other factors, the provision of ballots by political parties and a lack of voter registration systems (Argersinger (1985)). Reforms designed to reduce the degree of such fraud included the introduction of the secret ballot. Heckelman (1995) studies the staggered introduction of the secret ballot across states in the U.S., documenting that the secret ballot reduced turnout.[5] He interprets this finding as consistent with parties no longer being able to verify voter choices under the secret ballot, making it more difficult for parties to buy votes. Likewise, Kuo and Teorell (2017) document that the secret ballot reduced vote-buying and voter intimidation but also led to the development of new forms of electoral fraud. Another reform during this era, also designed to reduce electoral fraud, involved the development of voter registration systems. Perez (2021) studies the introduction of these laws during the 1880-1916 period, documenting that personal registration laws, which required in-person voter registration, reduced turnout in Presidential elections. In addition to studying a different reform, the synchronization of election dates across states, our paper contributes to this literature by attempting to separate

[5] Fergusson et al. (2020) document a complementary role of the media in improving the degree of democracy associated with the adoption of the secret ballot during this era.

the effects of these reforms on fraud from other effects on turnout. In particular, one limitation of these studies is that it is difficult to disentangle the effects of reducing fraud from other effects of these laws. Voter registration laws, for example, might reduce fraud but also might reduce legal turnout due to the increase in the costs of voting. Our study addresses this issue by examining an indirect effect of states changing their election dates, via the synchronization of such dates with those of neighboring states and the associated higher barriers to the practice of repeating.

More tangentially, our study is related to a broader literature on the detection of electoral fraud and attempts to reduce such fraud in the modern era. Within the U.S., most studies of contemporary fraud conclude that there is little or not fraud at current. Levitt (2007) and Minnite (2011) document that very few individuals in the United States are prosecuted for electoral fraud. Goel et al. (2020) use a national voter registration database in order to detect voters casting ballots in at least two jurisdictions during the 2012 Presidential election. They find evidence that double voting is rare, with at most one in every 4,000 voters casting multiple ballots. Cantoni and Pons (2019) use similar data to study the introduction of voter identification laws, finding that strict voter identification laws do not reduce voter turnout. While these studies are consistent with little or no fraudulent voting in the U.S. at current, our study examines a historical setting, without contemporary voter registration systems and prior to the introduction of the secret ballot. Given this, it is not surprising that we find evidence consistent with fraud in the form of repeat voting, despite the findings of little or no fraud in the U.S. during the contemporary era.

Our paper is also related to modern studies focused on other countries. These studies tend to focus on settings with weaker electoral institutions, making them closer to our setting. A number of studies have examined the role of independent election monitors. Enikolopov et al. (2013) study the role of randomly assigned election monitors in Russia, documenting that the presence of observers at polling stations led to a significant reduction in electoral support for the incumbent party. Hyde (2007), Hyde (2010) and Leeffers and Vicente (2019) document similar findings in developing countries. Researchers have also conducted field experiments in developing countries to gauge the effectiveness of various strategies to reduce voter fraud. Facilitating reporting of fraud by voters increases such reports but discouraged candidates from engaging in such fraud (Garbiras-Dìaz and Montenegro (2021)). Enhanced monitoring of elections led to a reduction in electoral fraud in Afghanistan (Callen and Long (2015)). Likewise, a campaign against vote buying in Uganda changed electoral outcomes, with an increase in support for under-resourced candidates (Blattman et al. (2019)).

The paper proceeds as follows. Section 2 provides background information on our context. Section 3 reviews our empirical strategy and Section 4 describes the data. Section 5 provides our baseline results, Section 6 addresses alternative explanations, Section 7 addresses possible mechanisms, and Section 8 provides additional discussion. Section 9 presents complementary results on Presidential elections, Section 10 investigates the partisan implications of this reform, and Section 11 concludes.

2 Context

In this section, we describe federal elections, voter fraud, and the movement towards 1T1M in the 19th Century. In general, fraud allegations and disputed elections during this era were widespread. Indeed, as documented by Kuo and Teorell (2017), roughly 10 percent of Congressional Elections between 1860 and 1900 were disputed, versus around 1 percent today, and these disputes tended to be concentrated in the South, relative to the North. The most common forms of allegations in election disputes during this era included ballot fraud, registration fraud, violence, and bribery.

Two features of elections during this era likely contributed to fraud. First, the lack of modern voter registration systems made it more difficult for poll workers to identify voters and to confirm their eligibility to vote. For example, a report from Ohio in 1876 on repeat voting noted that "These fellows are also experts in voting under false names, disguising themselves and making round of all the polls they can reach in a day."[6]

A second contributing factor involved the lack of a secret ballot. During this era, parties printed their own ballots and distributed them to voters, and this practice facilitated both parties contracting with repeaters and ballot stuffing. On ballot stuffing, according to an anecdote reported in Evans (1917), multiple small tissue paper ballots printed by parties were "...folded inside a larger one without an outsider being able to tell that there was more than one ticket being deposited. Yet the inside ballot could be so folded that it would fall out if the outer ballot was shaken a little when it was being voted, or if a friendly judge would materially assist by shaking the box".

In terms of the timing of elections, the U.S. Constitution does not designate dates for federal elections. According to Section 4, "The Times, Places and Manner of holding Elections for Senators and Representatives, shall be prescribed in each State by the Legislature thereof...". A federal law passed in 1792 required states to set their presidential election day in a 34-day period before the first Wednesday in December. In 1845, a new federal law mandated a uniform

[6]New York Times, October 8, 1876

date for Presidential Elections: the first Tuesday after the first Monday of November (1T1M). By 1848, all states had converged to 1T1M for the presidential election.

Given the shift in Presidential election dates, states also began converging to 1T1M for Congressional elections, both midterm elections and those held in Presidential years, as documented in Figure 1. In 1872, a federal law mandated that states vote on 1T1M for Congressional elections. Due to exemptions and other issues, full convergence to 1T1M in congressional elections was not reached until after 1900.

There were two main rationales for convergence. The first rationale was to reduce interstate electoral contagion, which gave excessive importance and power to early-voting states (James (2007)). The second motive, which is the focus of our study, was to eliminate ”the motive and opportunity” for electoral fraud, in the form of repeating, during a period prior to the adoption of modern voter registration systems and prior to the introduction of the secret ballot. Given high travel costs during this time, simultaneous elections made it more difficult to vote in multiple states on the same day. Indeed, Tuesday was chosen as the election day under 1T1M because people in rural areas needed to travel at least one day, or even two, in order to reach the polling station. Since people were in churches on Sundays and often farmer markets were held on Wednesday, Tuesday was chosen.[7]

A natural question, given our use of county-level data, is whether the practice of cross-state repeating is sufficiently common to impact aggregate turnout. As noted above, electoral fraud was widespread during this era due to, among other factors, the lack of both voter registration systems and the secret ballot, and these two factors also likely contributed to the practice of cross-state repeating. On registration systems, for example, according to Holt (2007), ballots in the disputed 1876 Presidential election were ”...cast illegally by white South Carolinians and Georgians who voted repeatedly at different polling stations where the lack of a list of registered voters prevented supervisors from challenging them”. On the secret ballot, the fact that parties provided ballots to voters facilitated repeating. For example, according to news reports on repeaters in Kentucky from Ohio in 1880, ”The news thus far received from the Kentucky elections indicate that the placing in nomination of independent candidates has led to the polling of an unusual large vote in Covington and Newport, which lie opposite Cincinnati (...) repeaters were driven over from Cincinnati together, and were furnished tickets on the suspension bridge”, where the word tickets refers to ballots.[8] Thus, out-of-state voters might have voted repeatedly within the state by either travelling to different polling locations or by engaging in ballot stuffing at a single polling location.

[7] https://www.britannica.com/story/why-are-us-elections-held-on-tuesdays (accessed July 2022)
[8] New York Times, October 30, 1880

3 Empirical Strategy

In order to identify electoral fraud in the form of repeating, or conversely, the impact of synchronization in terms of reducing repeating, we exploit two sources of variation: geographic and temporal. Regarding geographic variation, we match each county to the closest border state, effectively slicing each state into a number of subregions, with one subregion for each state border. Counties in Massachusetts, for example, are grouped into five subregions depending upon distance to the border with Connecticut, Rhode Island, New Hampshire, New York, and Vermont.[9] Using this geographic variation, we then also exploit variation over time, in the sense that we investigate how county-level turnout rates respond to a county becoming synchronized, in terms of the election date in both the home state and in the closest neighboring state. Using these two sources of variation, geographic and temporal, we then measure how turnout changes following states becoming synchronized, in terms of election dates, with their closest neighbor state.

While our voting data, as described below, include both midterm and Presidential elections, we focus on midterm elections as our preferred specification. In Presidential years, turnout in Congressional races will likely increase when states adopt 1T1M since voters can cast ballots in the Presidential election as well. Prior to 1T1M adoption for Congressional elections, by contrast, Congressional and Presidential elections were held on different dates in Presidential years, at least after 1T1M adoption in Presidential elections in 1848. Thus, the adoption of 1T1M for Congressional races also synchronizes Presidential and Congressional elections within the state. In midterm years, by contrast, the only change involves synchronization of election dates with other states. For comparison purposes, however, we also present results for all Congressional elections, pooled across both midterm and Presidential years, and we also have a specification in which we control for the direct effect of 1T1M, capturing the synchronization of Congressional and Presidential elections within a state.

Our estimation procedure includes county and year fixed effects. Thus, the effect is identified by changes in election timing (i.e. synchronization with neighboring states) within counties over time. In addition, given the significant changes in the South during our sample period, which spans the Civil War, Reconstruction, and the Jim Crow era, we also include Confederacy-by-year fixed effects.[10] This set of fixed effects controls for any differen-

[9] Here we implicitly assume that voters can cross water borders (ocean, rivers, etc.) to reach other states in the same fashion as they can cross land borders. We later investigate the role of water boundaries in our section on mechanisms.

[10] Confederacy is here broadly defined as the 11 confederate states as well as three border states that were heavily affected by the war or linked to Confederate states, namely Missouri, Kentucky and West Virginia. In a robustness check, we remove these states from the definition of Confederacy.

tial changes in the South, relative to the North, on a year-by-year basis. We also control for the separation of West Virginia from the state of Virginia and the associated changes in state boundaries during the Civil War era.[11] Finally, we cluster our standard errors to account for the panel nature of the data. In particular, given that our treatment occurs at sub-regions within states, according to distance to the nearest state, we cluster our standard errors at this level.

4 Data

State-level information on the timing of presidential and congressional elections is taken from James (2007).[12] In particular, we have information on the date of Presidential and Congressional Elections in each state and in each election year. Our main variable capturing timing, "Synchronized", indicates that a county voted on the same day as its closest neighboring state. This can happen in two different ways. First, it occurs when both states adopt 1T1M and thus synchronize their election dates. Second, it is possible that some states were already voting on the same date, prior to the adoption of 1T1M. In this case, states can become temporarily unsynchronized and then synchronized again during the transition to 1T1M.

Data on turnout and voting population provided by ICPSR are taken from the work by Clubb et al. (2006). In these data, turnout rates are constructed by simply dividing total votes cast by the estimated number of eligible voters.[13] We exclude those observation with very extreme values of turnout (above 200 percent or below 1 percent) as these extreme cases likely result from data misreporting.

We also use NHGIS IPUMS shapefiles to identify county locations and GIS to then identify the nearest state.[14] In particular, using decennial census historical maps, we associate each county to its closest state (excluding the state in which the county belongs). More specifically, the algorithm computes the closest linear distance between a county's border and any other state border. Among these, we pick the state with a border closest to the relevant county.[15]

There are two complications in this algorithm. The first issue is that some counties located

[11]This control is an indicator equal to one for all Virginia's and West Virginia's counties after 1863.

[12]These data were kindly provided to us by Mr. Philip Lampi and Professor Scott James.

[13]More specifically, in the ICPSR data "Decennial census figures for total population, and population deemed eligible to vote by contemporary age, race, sex and citizenship criteria, were used to compute estimates of the number of persons deemed qualified to vote in each election year. Inter-censal estimates were computed from the decennial figures, using a linear interpolation method."

[14]The shapefiles are downloaded from https://www.nhgis.org/documentation/gis-data.

[15]ICPSR data come with a county code that is consistent over time. While county borders may slightly evolve over time, the ICPSR code allows us to match counties across election years. These counties are associated to a name and a county code. The NHGIS IPUMS data instead come with two names, referred as NHGIS- and ICPSR-name, and two county codes (again, NHGIS and ICPSR). We match counties by State and ICPSR county code. This allows us to match 2961 counties over 3048. Few of the remaining counties were then matched manually by state and ICPSR-county name. As a robustness check, we drop all counties whose area changed in the 80 years we consider and results are qualitatively similar (see Table A.3).

on state borders might have multiple neighbors with the same zero distance. A second potential issue is that, for more precision, we run the same algorithm every decade. Given this, if a county shape or a state shape changes over time, it is possible that the same county ends up having multiple distinct closest neighbor states over time. Given all of this, we adopt a conservative approach and drop all counties with either of these two issues. At the same time, we show that our results are robust to the inclusion of these counties in our "full sample" results.

To summarize, our data build follows four steps. First, we use the ICPSR data to measure turnout rates at the county-year level. Second, using shapefiles, we incorporate information on the geography of each county-year observation. Third, we use GIS to identify the closest neighboring state for each county-year observation. Fourth, we use election dates in the state and in the closest state to create an indicator for the synchronization of these two election dates.

5 Baseline Results

Our baseline results are presented in Table 1. As shown in the first column, turnout rates fall by 6.4 percentage points in midterm elections, our preferred sample for the reasons described above, when counties have election dates that are synchronized with their neighboring state. This is a substantial decrease, representing a decline of 11 percent, relative to the sample average turnout rate of 57 percent in midterm elections. When including Congressional elections held in Presidential years the results are confirmed but the magnitude is slightly lower. As shown in the second column, turnout rates decrease by 5.2 percentage points, a decline of roughly 8 percent relative to the sample average turnout rate of 61 percent when including both midterm elections and Congressional elections held in Presidential years. Taken together, these findings are consistent with our hypothesis that synchronizing election dates reduces the practice of repeating, with fewer voters casting ballots in multiple states.

To investigate more precisely the exact timing of the reduction in turnout, we next present event studies in which we define synchronization as the event and then measure the number of years before and after this event.[16] The results, based upon a 40-year time window before and after the event, are displayed in Figure 2 for midterm elections. As shown, we do not find evidence of systematic pre-trends in turnout rates prior to the synchronization of election

[16]While we include all county-year pairs in our regressions tables, we only consider monotonic counties in the event study. More specifically, we define a "stable synchronization date" for each county as a synchronized election that was preceded by at least two non-synchronized elections and that is followed by at least two synchronized elections (i.e., midterm elections every 4 years or all elections every 2 years). Having defined this, we first drop those counties with more than one "stable synchronization date". Second, we drop those remaining observation whereby a county is synchronized before its "stable synchronization date" or is not synchronized after its "stable synchronization date".

dates. Turnout then drops significantly, by nearly 10 percentage points, in the year of synchronization of election dates, and the effect remains negative thereafter. Results are similar, though a bit more noisy, when including Congressional elections held in Presidential years, as shown in Figure 3, with a sustained reduction in turnout following the synchronization of the timing of elections in the home state and in the neighboring state. As shown in Appendix Figures A1 and A2, the results are also similar when using the Sun and Abraham (2021) approach, which allows for dynamic treatment effects, in the sense that the long-run effects of 1T1M might differ from the short run effects. Taken together, these event studies document sharp drops in turnout in the year of adoption, suggesting that our results are not driven by broader time trends during our sample period.

We next conduct a series of checks designed to investigate the robustness of our results. In Table A.1, we investigate robustness checks around the Confederacy and the Civil War, which occurred during our sample period. While our baseline specification includes a control for the creation of the state of West Virginia, our results, as shown in columns 1 and 2, are very similar when not including this control. As shown in columns 3 and 4, our results are also not driven by the inclusion of Confederacy-by-year fixed effects. Indeed, the results become stronger when dropping these fixed effects, with a 7.4 percentage point decline in turnout rates (column 3) in midterm elections and a 5.8 percentage point decline when including Congressional elections held in Presidential election years (column 4). Likewise, as shown in columns 5 and 6, our results are similar when changing the definition of Confederacy by re-classifying Kentucky and Missouri, two border states and late joiners of the Confederate States of America, along with West Virginia, which separated from Virginia during the Civil War and was also considered a border state, as not part of the Confederacy.

Table A.2 conducts two additional robustness checks involving the definition of our sample. First, given that the eligible voting population, the denominator of our turnout measure is interpolated between Census years, we next only focus on Census years ending in 8, 0, and 2. That is, since the Census is typically conducted in years ending in 0, we exclude from the sample observations based upon years that end in 4 and 6, given that these years are further from the Census year and thus more sensitive to any measurement error associated with this interpolation. As shown in the first two columns, our results are very similar to those in Table 1, our baseline results. We next conduct a robustness check around which counties are included in the sample. In particular, while we do not include counties that border multiple states or those that change neighbors due to the re-drawing of state or county boundaries in our baseline analysis, all counties are included in the robustness check in columns 3 and 4. As shown, our results are again similar to our baseline results when including all counties in our analysis.

In order to provide the reader of a sense of which regions are driving our results, we next allow the effects to vary between Confederate and non-Confederate states, an obviously important divide in the U.S. during our sample period. Kuo and Teorell (2017) document that electoral fraud during this era was concentrated in the U.S. South, meaning that our effects might be stronger in this region. As shown in the first two columns of Table 2, our results are indeed significantly larger in Confederate states, with a differential decline in turnout of almost 13.3 percentage points in midterm elections and 9.7 percentage points when including Presidential election years. Outside of the Confederacy, we continue to find the hypothesized negative effects of synchronization on turnout, though these are now substantially smaller in magnitude and statistically insignificant.[17] We return to an examination of the Confederacy in the next section, where we demonstrate that our measured decline in turnout is not driven by the adoption of laws designed to disenfranchise black voters in the South.

6 Alternative Explanations

In this section, we address four potential alternative explanation for our results. First, the decline in turnout could be driven directly by the switch to 1T1M, rather than by having the election synchronized with the neighboring state. Second, states might have adopted 1T1M during periods in which turnout was already declining. Third, states might have adopted 1T1M in conjunction with, or during a similar time frame to, other laws that reduced turnout. Fourth, it could be that our results are driven by a strategic reduction in turnout in counties that previously voted earlier than counties on the other side of the state border.

The first potential alternative explanation involves the decline in turnout being driven directly by the switch to 1T1M, rather than having the election synchronized with the neighboring state. That is, it could be that, prior to 1T1M adoption, states chose an election date that was convenient for their constituents. Given that the synchronous indicator turns on when states (and their neighbor) adopt 1T1M, our documented reduction in turnout could be driven by the direct effect of 1T1M. To address this potential alternative explanation, we control directly for the adoption of 1T1M within the state as well as the adoption of 1T1M in the neighboring state. In this case, identification is driven by comparing, for example, two counties that both vote on 1T1M but with neighbors that vote on different dates, one on 1T1M and one on a different date.

[17]We also provide event studies separately for the states of the Confederacy and states outside of the Confederacy in Appendix Figures A7- A10. These also document that the reductions in turnout following the synchronization of election dates was driven by states in the Confederacy

As shown the first column of Table 3, adoption of 1T1M in both the neighboring state and in the own-state reduces turnout, with only neighbor adoption statistically significant. After controlling for 1T1M status, however, our baseline effect of synchronization remains, with a 3.8 percentage point reduction in midterm elections, an effect that retains statistical significance at the 90 percent level. When including Congressional elections held in Presidential years, we document a similar reduction of 3.8 percentage points, after controlling for 1T1M status in the own-state and in the neighboring state, and this reduction is also statistically significant at the 95 percent level. Taken together, these findings suggest that our baseline result, synchronization of the timing of elections reducing turnout, is not driven by the direct effects of 1T1M adoption.

The second potential alternative explanation is that states might have adopted 1T1M during periods in which turnout was already declining. That is, any pre-trends in turnout might violate our identifying assumption of parallel trends between treated counties and control counties. We address this alternative explanation via an investigation of the dynamics of synchronization. As shown in Table 4, our results are robust to the inclusion of leads and lags of synchronization. In particular, after controlling for two leads and two lags of synchronization, the immediate effect of synchronization involves a 7.1 percentage point reduction in turnout rates. Moreover, the coefficients on the leads and lags are small in magnitude and, for the most part, statistically insignificant. Results are similar when including turnout in Congressional election held during Presidential years, as shown in the second column, with a reduction in turnout of 3.2 percentage points. These results are consistent with the event study results described previously. In particular, Figures 2 and 3 document a sharp drop in turnout just after synchronization of election dates. At the same time, these figures show no evidence of pre-trends in voter turnout. Since time and county fixed effects are included in the regressions generating these estimates, these event study results are only driven by the specific timing and subregion of synchronization. Taken together, these results are inconsistent with a story of adoption of 1T1M during time periods and in places associated with general declines in turnout.

The third potential alternative explanation involves other changes in voting rules during our sample period. In particular, one concern might involve states adopting 1T1M as part of larger package of election reforms. As discussed above, our results are not driven by the direct effect of 1T1M but rather by the synchronization of election dates with neighboring states. Thus, any other laws adopted in the same year as 1T1M adoption are not driving our results with respect to synchronization. A second related concern, given that our results are stronger for the U.S. South, involves the adoption of laws designed to disenfranchise black voters in the U.S. South following Reconstruction and during our sample period. To address this concern, we directly control for the adoption of new state constitutions in the U.S. South that, among other Jim Crow laws, led to the imposition of measures such as poll taxes and literacy tests designed to disenfranchise black voters.[18] As shown in columns 3 and 4 of Table 2, we find very large reductions in turnout, 28 percentage points in midterms elections and 29 percentage points when including Congressional elections held in Presidential election years, associated with the adoption of new state constitutions in the South. These effects are consistent with the large reductions in turnout in the South documented by James (2007). In particular, turnout rates in the South were roughly 50 to 60 percent during the Reconstruction era but fell to 10 to 20 percent by the early 1900s. While there were also declines in turnout in the North during this era, these changes were smaller in magnitude. After controlling for these changes in state constitutions, however, our results are very similar to our baseline findings, with a drop in turnout rates following synchronization of 5.3 percentage points in midterm elections and 4 percentage points when including Congressional elections held during Presidential years. Taken together, our results do not seem to be driven by other changes in voting rules during our sample period.

Fourth, it could be that our results are driven by a reduction in turnout in counties that voted earlier than their neighbors prior to synchronization. That is, if voters are strategic and understand that their early vote might have a multiplier effect, in the sense that their vote influences the choices of voters in regions of the neighboring state that vote late, then there could be a reduction in turnout in these early counties when elections are synchronized. For example, voter turnout in Presidential Primaries in New Hampshire, which has historically hosted the first primary, tends to be higher than turnout in other states. In the 2020 primary, for instance, New Hampshire recorded the third-highest turnout across all states in the U.S.[19] To the extent that any reduction in turnout for early voters, when synchronizing election dates, is not offset by an increase in turnout in counties that previously voted late, synchronization could lead to a reduction in turnout on net for reasons unrelated to voter fraud and repeating. To address this issue, we distinguish between counties that vote earlier than their neighbors prior to synchronization and counties that vote later. We then compare turnout in these counties to turnout in synchronized counties. As shown in Table 5, turnout is higher in both early and late counties, when compared to counties with synchronized elections. If anything, however,

[18] We take the year of adoption for these new constitutions in the South from Monnet (1912). This information is also provided by Wikipedia at https://en.wikipedia.org/wiki/Disfranchisement_after_the_Reconstruction_era.

[19] https://www.statista.com/statistics/1102189/voter-turnout-us-presidential-primaries-state/

our result of lower turnout under synchronized elections is driven by the comparison to late counties. This suggests that our results are not driven by a strategic reduction in turnout when early counties become synchronized.

7 Mechanisms

We next provide evidence on the mechanisms behind our results by investigating the heterogeneity of our documented reduction in turnout. First, we investigate whether the reduction in turnout is larger prior to the adoption of the secret ballot in the U.S., which occurred during the 19th century. The second involves the role of population, and we hypothesize that there might be larger reductions in small population counties, given that an increase in the number of repeaters from neighboring states will lead to a larger increase in turnout when measured in percentage points. Likewise, we expect larger shifts in turnout rates in areas with large population centers on the other side of the border. The third involves whether the effects are larger in counties with access to railroad lines, a common form of travel during the 19th century. Fourth, we investigate whether our documented reductions are larger in counties situated close to the state border. Finally, we investigate whether our effects differ between land borders and water borders, which might be more difficult for repeaters to cross.

First, we investigate the role of the secret ballot.[20] Prior to the adoption of the secret ballot, parties printed their own ballots, and, as noted above, anecdotal evidence suggests that parties provided repeaters with ballots. Moreover, there is also evidence from this era of ballot stuffing by repeaters, a practice facilitated by party ballots. With the adoption of the secret ballot, government officials were in charge of ballots, meaning that parties could no longer provide repeaters with ballots, and ballot stuffing became more difficult. Given all of this, we hypothesize that our results are driven by states without the secret ballot. As shown in Table 6, this is indeed the case. Consistent with prior evidence, adoption of the secret ballot led to large reductions in turnout rates, 13.2 percentage points in midterm elections and 12.4 percentage points when including Congressional elections held in Presidential years. Moreover, the effects of synchronization are larger in states without the secret ballot, with reductions in turnout of 6.5 percentage points in midterm elections and 5.5 percentage points in all years. Comparing places with and without secret ballots, there is a difference in the effect of 5.7 percentage points in midterm elections and 6.7 percentage points in all years, and these differences are statistically significant at conventional levels. That is, the effects of synchronization are close to zero

[20]The information on when each state adopted the secret (or Australian) ballot is taken by Walker (2005). Using this information, we construct an indicator variable that turns on for the years in which the secret ballot was in place.

in states with the secret ballot. Taken together, the evidence suggests that lack of a secret ballot facilitated the practice of repeating and the associated shift in turnout rates.[21]

Regarding the role of population, we hypothesize that the effects should be driven by small population counties since a given reduction in the number of voters from neighboring states will lead to sharper reductions in turnout rates in these small population places. For example, if 100 repeaters from nearby states do not materialize following synchronization, this will lead to a 10 percentage point reduction in turnout rates in a county with 1,000 eligible voters but only a 1 percentage point reduction in a county with 10,000 eligible voters. As shown in the first column of Table 7, this is indeed the case, with a reduction in the turnout rate of 7.5 percentage points as county population approaches zero and a corresponding increase of 0.2 percentage points in the effect of synchronization for every 1,000 additional voters. Thus, the effect of synchronization approaches zero as the number of eligible voters in the county approaches 36,000. Results are broadly similar when including Congressional elections held in Presidential years (column 2). Taken together, this analysis documents that our baseline effects are driven by less populated counties.

Likewise, we hypothesize that the effects should be driven by areas with large population centers on the other side of the border. For example, if 100 repeaters from nearby states do not materialize following synchronization, this will lead to a 10 percentage point reduction in a county with 1,000 residents but only a 1 percentage point reduction if 10 repeaters do not materialize following synchronization. To investigate this issue, we create a measure of the population in the largest county in the subregion on the other side of the border.[22] As shown in Table 8, we find some evidence that our results are stronger in cases with large populations on the other side of the border. In particular, turnout rates fall by more when elections are synchronized in states with large populations on the other side of the border, as shown in columns 1 and 3. These interaction effects, however, are not statistically significant at conventional levels. Given that areas with large population centers on the other side of the border are also likely to be large population areas themselves, given spatial correlations in population, we

[21]A possible further analysis could investigate the role of voter personal registration, and indeed we would expect a similar pattern to secret ballot. However, this analysis is complicated by two main issue. First, to the best of our knowledge the most comprehensive source of information for early adoption of personal registration laws is Perez (2021), who only focuses on non-confederate states and mainly on years after 1880. The reason for this choice, and here the second issue arises, is that, in northern states, registration laws were not introduced homogeneously: first registration was required in the larger cities, then they were typically extended to mid-sized towns and eventually they were introduced in rural areas, but even in a very heterogeneous way. In addition, personal registration in the South was highly connected to the movement towards disenfranchising black voters. Given these imprecise measures and issues around omitted variable bias, it is difficult to properly capture how these laws interacted with synchronization.

[22]Here we use Census population counts rather than the number of eligible voters since the latter is not available in some cases, such as when the other side of the border is a territory and has not yet gained statehood.

next control for own-side population. As shown in columns 2 and 4, results are similar, with statistically significant effects for own-side population and statistically insignificant effects for the population on the other side of the border.

Third, we investigate the role of railroads, a common form of travel during the 19th century.[23] Some anecdotal evidence suggests that repeaters used trains to travel from state to state. For example, a newspaper report noted that men from Philadelphia, Pennsylvania were arrested as suspected repeaters for the Republican Party when they arrived at the train station in Baltimore, Maryland.[24] Thus, the presence of a rail line might facilitate out-of-state repeaters entering the state. As shown in Table 9, we do find some evidence that the presence of rail lines leads to a bigger reduction in turnout following synchronization, with a 4.4 percentage point reduction during midterm elections in places without rail and a 6.1 percentage point reduction in places with rail. Differences between these coefficients are not statistically significant at conventional levels, however, so we cannot reject the null hypothesis that the effects of synchronization of election dates on turnout are similar in places with and without access to rail. Patterns are similar when including Congressional elections held in Presidential years, as shown in column 2. Given all of this, we conclude that there is not strong evidence of heterogeneity according to the presence of railroad lines.

Fourth, we investigate the role of distance to the border. In terms of our hypothesis, the role of distance is unclear. On the one hand, repeating should be more common close to the border, under the assumption that repeaters attempt to minimize travel costs. Thus, to the extent that synchronization reduces repeating, the effects should be concentrated close to the border. On the other hand, repeating close to the border might continue under synchronization, given that voters can feasibly vote in both states on the same day in this case. Thus, the role of distance to the border in repeat voting is ultimately an empirical question. To investigate a potential role for distance, we measure the distance from every county to the border associated with the neighboring state. As shown in Table 10, the effects are similar between counties close to the border and further from the border, with the coefficient on the interaction between distance and synchronization small and statistically insignificant. This is consistent with the potentially offsetting effects described above. In Figures 4 and 5, we allow for a more flexible specification, in which we allow the effects of synchronization to vary according to the following distance

[23]Information on the presence of railroads in a given year is taken from Atack (2016), which provides the location of all the railways between 1826 and 1911 as well as the precise date in which the railway first started operating. While multiple tracks were built in the same county over time, the indicator variable "rail" turns on starting from the first year in which a railway was in operation in a given county. While we do not have data on the presence of train stations, historical evidence suggests they were locate extremely close to one another, suggesting that, when a county was crossed by a railroad line, it most likely also had a station.

[24]Philadelphia Inquirer, October 26, 1887

categories in kilometers: on the border (zero km), 0 to 50, 50-100, 100-150, 150-200, and 200+. As shown, there is some evidence of a U-shaped pattern, with smaller effects close to the border, consistent with repeating continuing after synchronization, and far from the border, consistent with little or no repeating prior to synchronization, and larger effects at moderate distances from the border.

Finally, we investigate the role of the border itself, distinguishing between land borders and water borders, which might be difficult to cross, especially during the 19th Century.[25] Given this, we hypothesize that any reductions in turnout associated with synchronization should be larger with land borders and smaller with water borders. As shown in Table 11, the effects are indeed stronger with land borders. In particular, turnout falls by 7.2 percentage points during midterm elections in places with land borders but by only 4.3 percentage points in places with water borders, as shown in column 1. We also find stronger effects with land borders when including Congressional elections held during Presidential years (column 2). These differential effects are not statistically significant at conventional levels, however, so we cannot reject the null hypothesis that the effects of synchronization of election dates on turnout are similar in places with land borders and in places with water borders. Thus, we conclude that there is not strong evidence of heterogeneity according to the type of border.

Given that the measures used to investigate these mechanisms might be correlated with each other, we next present results with all of these measures interacted with synchronization. As shown in Table 12, our results are again driven by places that have not yet adopted the secret ballot and in counties with small populations. Other interactions retain the expected sign (larger drops in turnout in areas with a large population on the other side of the border, larger drops in turnout along land borders, relative to water borders, and larger drops in turnout in places with rail lines) but are not statistically significant at conventional levels.

8 Discussion

In terms of the magnitude of the effects, our analysis suggests a large reduction in turnout. That is, we find a 11 percent reduction in turnout. For three reasons, however, this does not imply that 11 percent of voters were repeaters prior to the adoption of 1T1M. First, as noted above, anecdotal evidence suggests that repeaters both voted at multiple polling locations and

[25] Our variable indicating a water border is an indicator equal to one for all the sub-regions that are entirely divided from their closest neighboring state by water (this includes a lake or a river 30 meter or wider, from Popelka and Smith (2020)). If the border is not entirely divided by water, only the counties that touch the water-border take value one. The only exception to this rule is Michigan, where only a part of the state (Lower Michigan) is entirely separated from Wisconsin by water: in this case thus all the Wisconsin-Michigan sub-region takes value one.

stuffed as many ballots as possible in a given polling station, and this practice was facilitated by the printing of ballots by political parties. That is, instead of 11 percent of voters being repeaters, our results imply that 11 percent of ballots were cast by repeaters. Second, when directly controlling for the direct effect of the introduction of 1T1M or for the disenfranchising constitutions in the South (Tables 3 and 2, respectively), the point estimates are smaller, implying a reduction in turnout that ranges between 7 percent and 9 percent. Third, the reduction in turnout was not equally distributed across space but was instead concentrated in less populated counties, as reported in Table 7. This suggests that, on a national basis, fewer than 11 percent of ballots were cast by repeaters.

9 Presidential Elections

Finally, for comparison purposes, we also investigate turnout in Presidential elections. In this case, we have significantly less variation over time since, as noted above, all states were required to adopt 1T1M in Presidential elections by 1848, convergence was much faster, and our county-level turnout data do not start until 1840.[26] Despite this lack of variation, we do find evidence that the synchronization of election timing in 1848 reduced turnout. In particular, comparing counties with election timing that was synchronized between the home state and the neighboring state, we document that synchronization reduced turnout by 4.4 percentage points, as shown in column 1 of Table 13, and the effect is similar when not including Confederacy-by-year fixed effects, as shown in column 2. Thus, the evidence suggests that the synchronization of election timing in Presidential elections in 1848 also reduced turnout in Presidential elections.

To further investigate the timing of these effects in Presidential elections, we also conduct an event study in Figure 6. As shown, there is a reduction in turnout in the year following synchronization of election timing, and this effect persists for several years after synchronization. On the other hand, there is also evidence of pre-trends in this case. Given that every state adopted 1T1M in 1848 and that our data do not begin until 1840, we have a very limited pre-period in this case. Given the pre-trends and our limited variation over time, we interpret these results with respect to turnout in Presidential elections with more caution.

Finally, in the context of Presidential elections, we can examine whether repeating is more common in competitive elections. That is, given the electoral college for Presidential elections, we might expect more repeating, and thus a larger reduction in turnout following synchro-

[26] The considered time-frame is 1840-1888. We restrict our sample to the states that were already part of the US in 1844.

nization, in states with competitive Presidential elections. In order to measure the degree of competition, we use the margin of victory by the winning party in the previous Presidential election within the state. As shown in Table 14, we do not find any evidence along these lines. That is, while the coefficient on the interaction is negative in column 1, which includes Confederacy-by-year fixed effects, and positive in column 2, which excludes Confederacy-by-year fixed effects, neither coefficient is statistically significant at conventional levels. Thus, overall, there is not statistically significant evidence of a larger reduction in turnout rates in states with more competitive Presidential elections, as proxied by the margin of victory in the previous Presidential election in that state.

10 Partisan implications

A natural follow-up question involves the partisan implications of the reduction in repeating. That is, did one party benefit from the reduction in repeating associated with synchronization of election dates? Importantly, we did not find any evidence that the adoption of 1T1M was partisan in nature. Despite this, we can investigate this issue empirically by measuring directly whether one party benefited from this reform in terms of electoral outcomes. To do so, we measure the vote share for the Democratic party, the most stable party during this time period, relative to all other parties, which tended to be less stable. As shown in Table 15, we do find some evidence of an increase in the Democratic vote share following synchronization, in terms of midterm elections (column 1), all Congressional elections (column 2) and Presidential elections (column 3). None of these effects, however, are statistically significant at conventional levels. Thus, there is not compelling evidence of partisan effects from the synchronization of elections and the associated reduction in the practice of repeating.

11 Conclusion

We provide evidence that the synchronization of election dates in U.S. during the 19th Century reduced turnout rates in Congressional elections. We interpret this result as consistent with synchronization increasing the cost of repeating, the practice of voters casting ballots in multiple states in a single election. Our results are driven by the U.S. South, consistent with existing evidence of higher voter fraud in this region. Our results are also driven by states that had not yet adopted the secret ballot, consistent with existing evidence that the secret ballot was successful in reducing the degree of electoral fraud. That is, synchronizing election dates was most effective in reducing repeating for cases in which parties retained control over the printing and distribution of ballots. Finally, our results are driven by small population counties, whose turnout rate would be particularly inflated by out-of-state repeaters.

It is important to note that these results are specific to our setting and that a movement away from 1T1M today in the U.S. would not necessarily lead to an increase in repeating, due to the current use of the secret ballot and contemporary voter registration systems. Nonetheless, our study does have contemporary implications for less developed countries, which tend to have less robust electoral institutions and more evidence of fraud. Our study also has implications for the timing of elections, and staggered elections remain in place today. In India, which has national elections spread out over several weeks in different states, for example, a politician in Mumbai recently encouraged migrant workers in his party to vote in their home states on May 17 and then to return to vote in Mumbai on May 24.[27] Finally, our study also highlights more generally the promise of electoral reforms in countering voter fraud.

[27] https://www.firstpost.com/politics/sharad-pawars-call-to-vote-twice-is-fraud-not-humour-1447137.html

Tables and Figures

Table 1: Baseline identification

	(1) Turnout, mid	(2) Turnout, all
Synchronized	-6.3968**	-5.1762***
	(2.5055)	(1.8166)
County FE	Yes	Yes
Year FE	Yes	Yes
Confederacy-by-year FE	Yes	Yes
Subregion Clustering	Yes	Yes
Observations	31,331	64,328
R-squared	0.569	0.557

The dependent variable is turnout in congressional elections in midterms years (col. 1) or all years (col. 2). *Synchronized* is a dummy that takes value 1 if the county votes on the same day as its closest neighboring state. In all specifications we control for the separation of the Virginias. Standard errors, clustered at the subregion level, are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table 2: Confederacy and disenfranchising laws

	(1) Turnout, mid	(2) Turnout, all	(3) Turnout, mid	(4) Turnout, all
Synchronized	-0.6026	-0.7294	-5.2793***	-4.0390***
	(0.8481)	(0.8271)	(1.9526)	(1.1825)
Confederacy	-	-		
Synchronized*Confederacy	-13.286***	-9.7129***		
	(4.8128)	(3.2629)		
Disenfranchising Constitut.			-28.331***	-29.448***
			(3.0154)	(3.1698)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Confederacy-by-year FE	Yes	Yes	Yes	Yes
Subregion Clustering	Yes	Yes	Yes	Yes
Observations	31,331	64,328	31,331	64,328
R-squared	0.575	0.560	0.609	0.599

The dependent variable is turnout in congressional elections in midterms years (col. 1, 3) or all years (col. 2, 4). Synchronized is a dummy that takes value 1 if the county votes on the same day as its closest neighboring state. Confederacy is an indicator for states who were directly or indirectly part of the Confederacy (i.e. the 11 core states plus Missouri, Kentucky, West Virginia). *Disenfranchising Constitut.* indicates years after which state constitutions were approved (in the South) in order to disenfranchise black voters (poll taxes and literacy tests included). In all specifications we control for the separation of the Virginias. Standard errors, clustered at the subregion level, are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 3: Including direct controls for IT1M.

	(1) Turnout, mid	(2) Turnout, all
1T1M	-2.5282	-1.9347
	(2.1846)	(1.7457)
1T1M neighbor	-5.2807*	-2.3702
	(2.7245)	(2.0757)
Synchronized	-3.8391*	-3.7938**
	(2.1949)	(1.5650)
County FE	Yes	Yes
Year FE	Yes	Yes
Confederacy-by-year FE	Yes	Yes
Subregion Clustering	Yes	Yes
Observations	31,223	64,058
R-squared	0.571	0.557

The dependent variable is turnout in congressional elections in midterms years (col. 1) or all years (col. 2). *Synchronized* is a dummy that takes value 1 if the county votes on the same day as its closest neighboring state (be it on 1T1M or on another day). *1T1M* and *1T1M neighbor* are dummies respectively indicating whether the county's own state or its closest-neighbor state have adopted 1T1M. In all specifications we control for the separation of the Virginias. Standard errors, clustered at the subregion level, are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 4: Including control for leads and lags.

	(1) Turnout, mid	(2) Turnout, all
Synchronized	-7.0620***	-3.2228***
	(2.4550)	(1.0513)
Synchronized (t+1)	-2.7797	-1.6162
	(1.7149)	(1.1700)
Synchronized (t+2)	2.0079	-0.8372
	(1.9010)	(1.3421)
Synchronized (t-1)	2.6098*	-1.7380*
	(1.5714)	(0.9019)
Synchronized (t-2)	-0.2198	0.2170
	(1.3478)	(1.1057)
County FE	Yes	Yes
Year FE	Yes	Yes
Confederacy-by-year FE	Yes	Yes
Subregion Clustering	Yes	Yes
Observations	25,627	58,625
R-squared	0.545	0.547

The dependent variable is turnout in congressional elections in midterms years (col. 1) or all years (col. 2). Synchronized is a dummy that takes value 1 if the county votes on the same day as its closest neighboring state. Leads or lags of such variables are included as controls. In all specifications we control for the separation of the Virginias. Standard errors, clustered at the subregion level, are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 5: Timing of election among non-synchronized counties

	(1) Turnout, mid	(2) Turnout, all
Voting first	1.7427	2.9988**
	(1.7851)	(1.3441)
Voting later	5.8484**	4.3606**
	(2.5378)	(2.1410)
County FE	Yes	Yes
Year FE	Yes	Yes
Confederacy-by-year FE	Yes	Yes
Subregion Clustering	Yes	Yes
Observations	27,512	54,620
R-squared	0.566	0.550

The dependent variable is turnout in congressional elections. *Voting first* and *Voting later* are dummy variables indicating whether the county's state votes before or after the closest other state (these variables can only take value 1 in non synchronized counties). The dummy for synchronized counties is the omitted category. Other notes as in previous tables. In all specifications we control for the separation of the Virginias. Standard errors, clustered at the subregion level, are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 6: The role of secret ballot

	(1) Turnout, mid	(2) Turnout, all
Synchronized	-6.5093***	-5.4913***
	(2.3328)	(1.7601)
Secret Ballot	-13.244***	-12.391***
	(4.6489)	(3.9516)
Secret*Synchronization	5.6860**	6.6866***
	(2.5531)	(2.1661)
County FE	Yes	Yes
Year FE	Yes	Yes
Confederacy-by-year FE	Yes	Yes
Subregion Clustering	Yes	Yes
Observations	31,331	64,328
R-squared	0.574	0.560

The dependent variable is turnout in congressional elections in midterms years (col. 1) or all years (col. 2). Synchronized is a dummy that takes value 1 if the county votes on the same day as its closest neighboring state. Secret Ballot indicates whether that county was voting using secret ballot, according to the information provided by Heckelman (1995) and Walker (2005). In all specifications we control for the separation of the Virginias. Standard errors, clustered at the subregion level, are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 7: The role of population size

	(1) Turnout, mid	(2) Turnout, all
Synchronized	-7.5021***	-5.9273***
	(2.7473)	(1.9570)
Legal voters (1,000)	-0.2550**	-0.1934**
	(0.1236)	(0.0936)
Legal voters*Synchronized	0.2103*	0.1424*
	(0.1091)	(0.0794)
County FE	Yes	Yes
Year FE	Yes	Yes
Confederacy-by-year FE	Yes	Yes
Subregion Clustering	Yes	Yes
Observations	31,330	64,326
R-squared	0.570	0.558

The dependent variable is turnout in congressional elections in midterms years (col. 1) or all years (col. 2). Synchronized is a dummy that takes value 1 if the county votes on the same day as its closest neighboring state. Legal voters is the census-estimated number of legal voters in the county; density normalizes this on the area of the county. In all specifications we control for the separation of the Virginias. Standard errors, clustered at the sub-region level, are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 8: The role of population size and large neighbouring cities

	(1) Turnout, mid	(2) Turnout, mid	(3) Turnout, all	(4) Turnout, all
Synchronized	-6.5297**	-5.8150**	-5.1322**	-4.5871**
	(2.7387)	(2.4643)	(2.0468)	(1.8919)
Population other side (1000)	0.0028	0.0138	0.0077	0.0164*
	(0.0130)	(0.0115)	(0.0109)	(0.0095)
Population other side*Synchronized	-0.0023	-0.0121	-0.0064	-0.0141
	(0.0120)	(0.0108)	(0.0097)	(0.0087)
Nb. counties, other side		-0.6011***		-0.4642**
		(0.2085)		(0.1990)
Legal voters (1,000)		-0.2659**		-0.2286**
		(0.1260)		(0.0993)
Legal voters*Synchronized		0.2193*		0.1744**
		(0.1114)		(0.0846)
Border distance (km)		-0.0178		-0.0268
		(0.0288)		(0.0281)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Confederacy-by-year FE	Yes	Yes	Yes	Yes
Subregion Clustering	Yes	Yes	Yes	Yes
Observations	30,419	30,364	62,338	62,227
R-squared	0.578	0.585	0.566	0.571

The dependent variable is turnout in congressional elections. The independent variables include: the population of the largest county on the other side of the border as measured by a linear interpolation of census data (Population other side (1000)), the number of counties (Nb. counties, other side), the distance of the most populated counties from the border (Border distance, other side) within the subregion on the other side of the border. In all specifications we control for the separation of the Virginias. Standard errors, clustered at the subregion level, are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 9: The role of railways

	(1) Turnout, mid	(2) Turnout, all
Synchronized	-4.3851*	-3.9061**
	(2.4731)	(1.8405)
Railroad access	4.1212***	4.6679***
	(1.1687)	(1.2647)
Synchronized*Railroad	-1.6873	-0.8887
	(1.6636)	(1.5661)
County FE	Yes	Yes
Year FE	Yes	Yes
Confederacy-by-year FE	Yes	Yes
Subregion Clustering	Yes	Yes
Observations	27,489	54,754
R-squared	0.519	0.504

The dependent variable is turnout in congressional elections in midterms years (col. 1) or all years (col. 2). Synchronized is a dummy that takes value 1 if the county votes on the same day as its closest neighboring state. Rail is a time varying indicator for whether the county was connected to the railway in that specific year. This variable is missing after 1910 due to data limitations. In all specifications we control for the separation of the Virginias. Standard errors, clustered at the subregion level, are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 10: Impact by distance

	(1) Turnout, mid	(2) Turnout, all
Synchronized	-6.3142**	-5.2546***
	(2.5859)	(1.9045)
Dist (km)	0.0184	0.0327
	(0.0487)	(0.0562)
Synchronized*Dist	-0.0027	0.0007
	(0.0193)	(0.0143)
County FE	Yes	Yes
Year FE	Yes	Yes
Confederacy-by-year FE	Yes	Yes
Subregion Clustering	Yes	Yes
Observations	31,288	64,238
R-squared	0.570	0.557

The dependent variable is turnout in congressional elections in midterms years (col. 1) or all years (col. 2). Synchronized is a dummy that takes value 1 if the county votes on the same day as its closest neighboring state. Dist indicates how close the county is to the closest neighboring state. In all specifications we control for the separation of the Virginias. Standard errors, clustered at the subregion level, are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 11: The role of water borders

	(1) Turnout, mid	(2) Turnout, all
Synchronized	-7.2351**	-5.7430***
	(2.8133)	(2.1517)
Water	1.5531	1.4662
	(5.0835)	(5.3888)
Water*Synch	2.9216	1.9143
	(3.1395)	(2.9461)
County FE	Yes	Yes
Year FE	Yes	Yes
Confederacy-by-year FE	Yes	Yes
Subregion Clustering	Yes	Yes
Observations	31,331	64,328
R-squared	0.570	0.557

The dependent variable is turnout in congressional elections. Water is a dummy equal to 1 if the subregion is completely divided from the other side of the border by a river of 30 meters or larger, or by a lake; if the river does not entirely separates the states, this variable is 1 only for counties effectively on the river. The only exception is the Wisconsin-Michigan border in which the southern part of Michigan is entirely separated from Wisconsin and thus the indicator is 1; while on the Wisconsin side every county can reach Michigan via-land and thus only counties on the lake have a value of 1. In all specifications we control for the separation of the Virginias. Standard errors, clustered at the subregion level, are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 12: Horse-race of interactions

	(1) Turnout, mid	(2) Turnout, all
Synchronized	-6.8567**	-5.6838**
	(3.4069)	(2.8408)
Secret Ballot	-12.579***	-12.664***
	(4.7991)	(4.2136)
Secret*Synchronized	5.9575*	7.6547***
	(3.0816)	(2.7479)
Legal voters (1,000)	-0.3285**	-0.3062**
	(0.1601)	(0.1265)
Legal voters*Synchronized	0.2714**	0.2163**
	(0.1339)	(0.0958)
Population other side (1000)	0.0077	0.0124
	(0.0113)	(0.0087)
Population other side * Synchronized	-0.0066	-0.0097
	(0.0105)	(0.0079)
Railroad	4.1233***	4.2180***
	(1.3321)	(1.2754)
Synchronized*Railroad	-1.9548	-0.6703
	(1.8157)	(1.4962)
Dist (km)	0.0128	0.0340
	(0.0505)	(0.0629)
Synchronized*Dist	0.0092	0.0021
	(0.0230)	(0.0210)
Water	-5.0425	-1.5705
	(6.7105)	(7.7045)
Water*Synch	3.3118	2.0749
	(3.4258)	(3.1460)
County FE	Yes	Yes
Year FE	Yes	Yes
Confederacy-by-year FE	Yes	Yes
Subregion Clustering	Yes	Yes
Observations	26,553	52,720
R-squared	0.532	0.518

The dependent variable is turnout in congressional elections. Other notes as in previous tables. In all specifications we control for the separation of the Virginias. Standard errors, clustered at the subregion level, are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 13: Impact on presidential vote

	(1) Turnout pres.	(2) Turnout pres.
Synchronized	-4.4478**	-6.6344**
	(1.8878)	(2.7371)
County FE	Yes	Yes
Year FE	Yes	Yes
Confederacy-by-year FE	Yes	No
Subregion Clustering	Yes	Yes
Observations	15,450	15,450
R-squared	0.502	0.452

The dependent variable is turnout in presidential elections. Synchronized is a dummy that takes value 1 if the county votes on the same day as its closest neighboring state. The considered time-frame is 1840-1888. We restrict our sample to the states that were already part of the US in 1844. In all specifications we control for the separation of the Virginias. Standard errors, clustered at the subregion level, are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table 14: Presidential vote: state margin of victory

	(1) Turnout pres.	(2) Turnout pres.
Synchronized	-5.096*	-5.861*
	(2.784)	(3.253)
Margin (1,000 votes), lagged	0.0947	0.316
	(0.269)	(0.264)
Margin*Synch	-0.176	-0.419
	(0.268)	(0.271)
County FE	Yes	Yes
Year FE	Yes	Yes
Confederacy-by-year FE	Yes	No
Subregion Clustering	Yes	Yes
Observations	14,439	14,439
R-squared	0.510	0.466

The dependent variable is turnout in presidential elections. Synchronized is a dummy that takes value 1 if the county votes on the same day as its closest neighboring state. The considered time-frame is 1840-1888. We restrict our sample to the states that were already part of the US in 1844. Margin is the state-level margin of victory in past election (1836 results from Wikipedia). In all specifications we control for the separation of the Virginias. Standard errors, clustered at the subregion level, are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 15: Party-specific effect

	(1) Dem. share %, congr.	(2) Dem. share %, congr.	(3) Dem. share %, pres.
Synchronized	1.7892	1.6127	1.5147
	(1.1761)	(1.0562)	(1.0693)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Confederacy-by-year FE	Yes	Yes	Yes
Subregion Clustering	Yes	Yes	Yes
Observations	30,065	62,718	32,112
R-squared	0.543	0.537	0.621

The dependent variable is the vote share of the democratic party (in percentage terms) in congressional (col 1,2) or presidential elections. We exclude counties where vote share was 100% or above. In all specifications we control for the separation of the Virginias. Standard errors, clustered at the subregion level, are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Figure 1: Share of the existing states that converged to 1T1M. Only considers the 35 states included in our analysis.

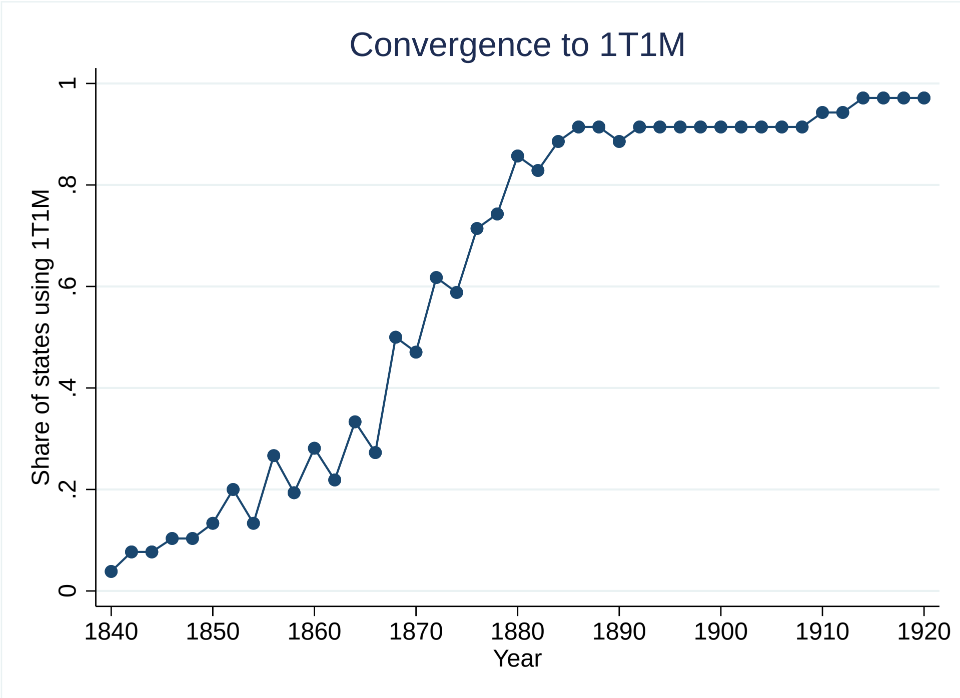


Figure 2: Midterm elections (40 years window). Year, county and Confederacy-by-year FE are included. We also control for the separation of Virginias.

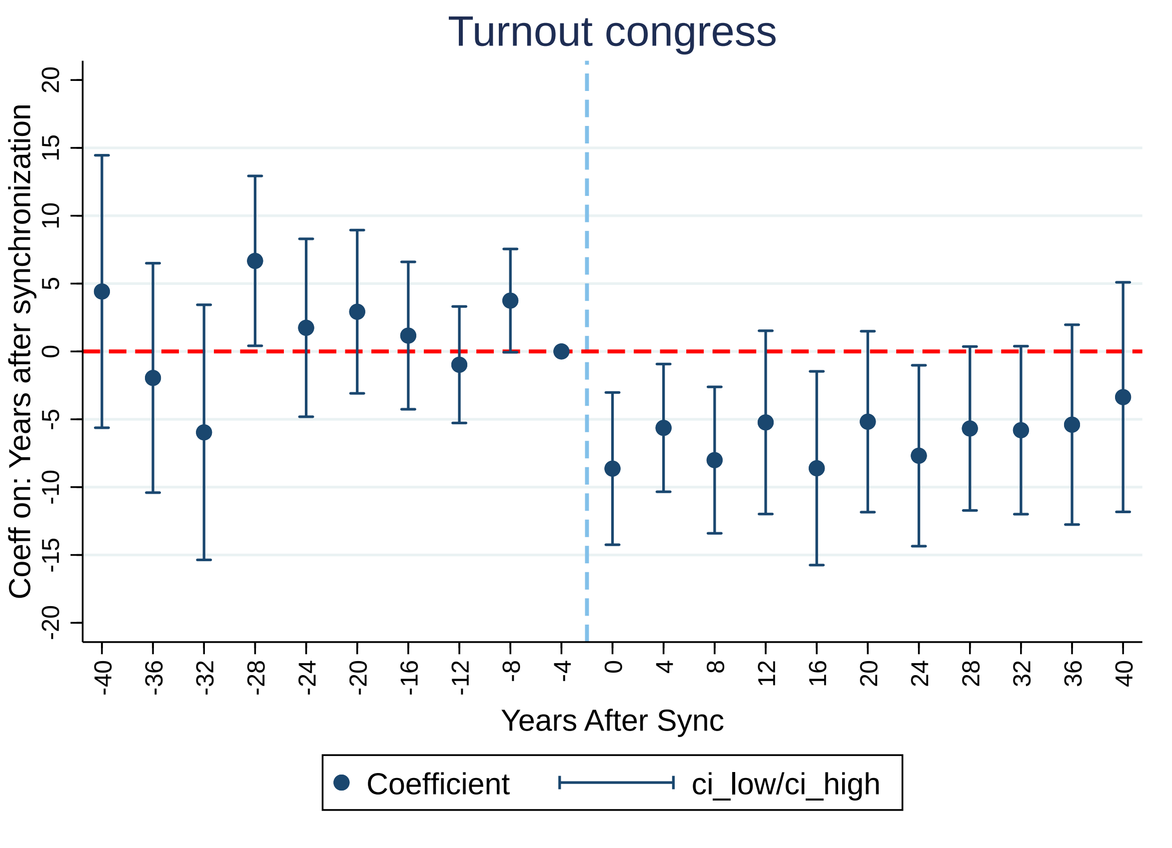


Figure 3: All elections (40 years window). Year, county and Confederacy-by-year FE are included. We also control for the separation of Virginias.

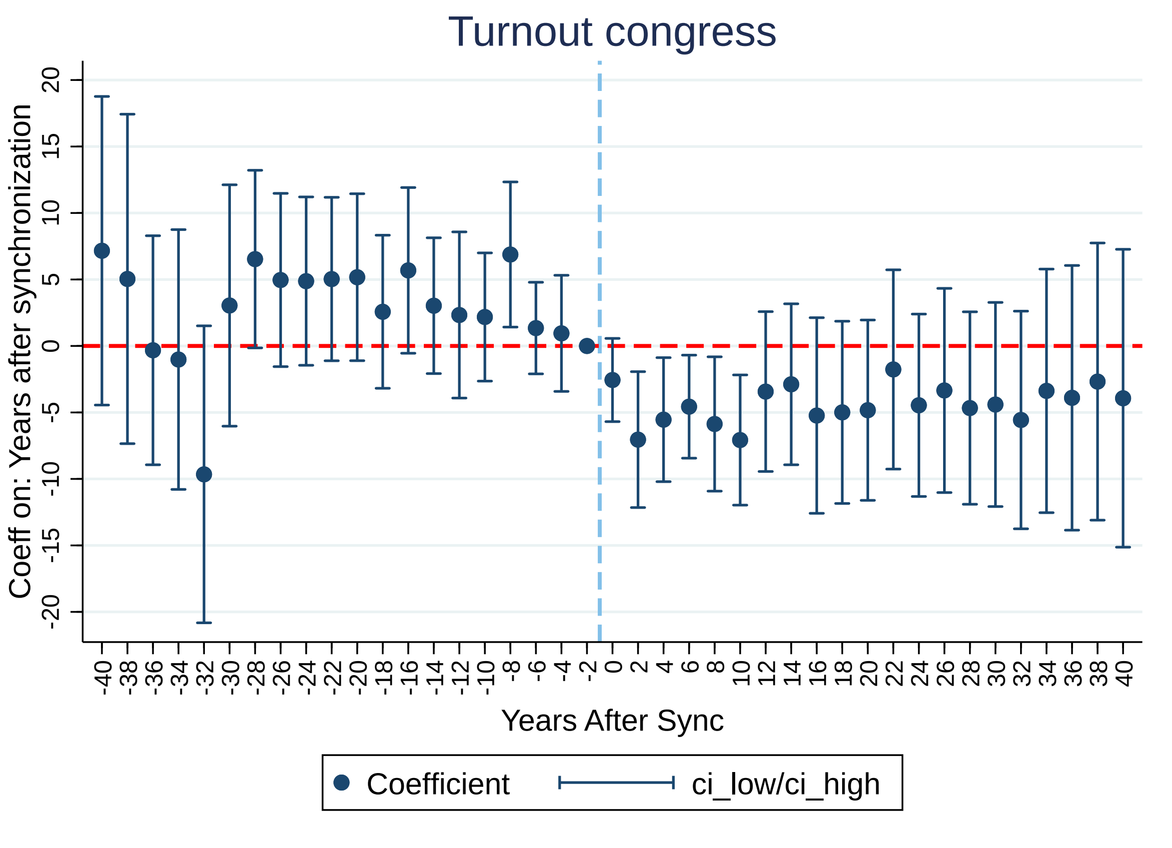


Figure 4: Effect of synchronization by border-distance (no main effect included). Year, county and Confederacy-by-year FE are included. Midterm elections.

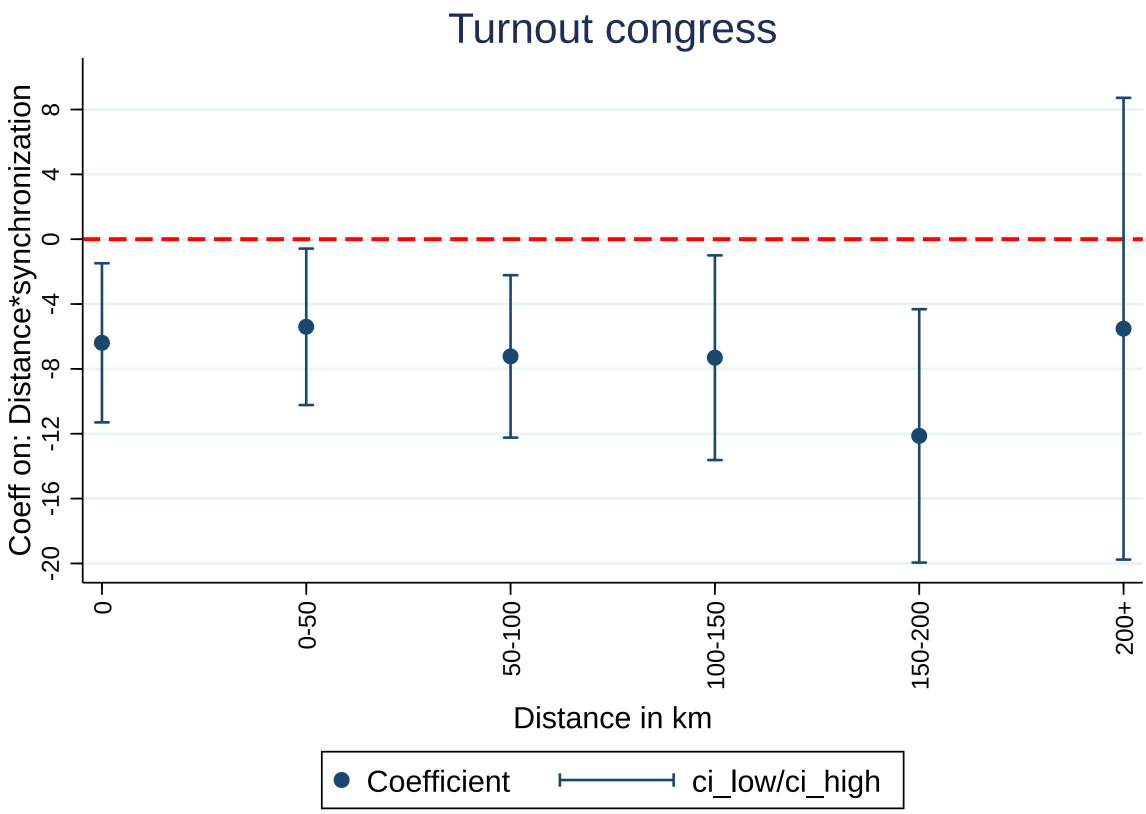


Figure 5: Effect of synchronization by border-distance (no main effect included). Year, county and Confederacy-by-year FE are included. All elections.

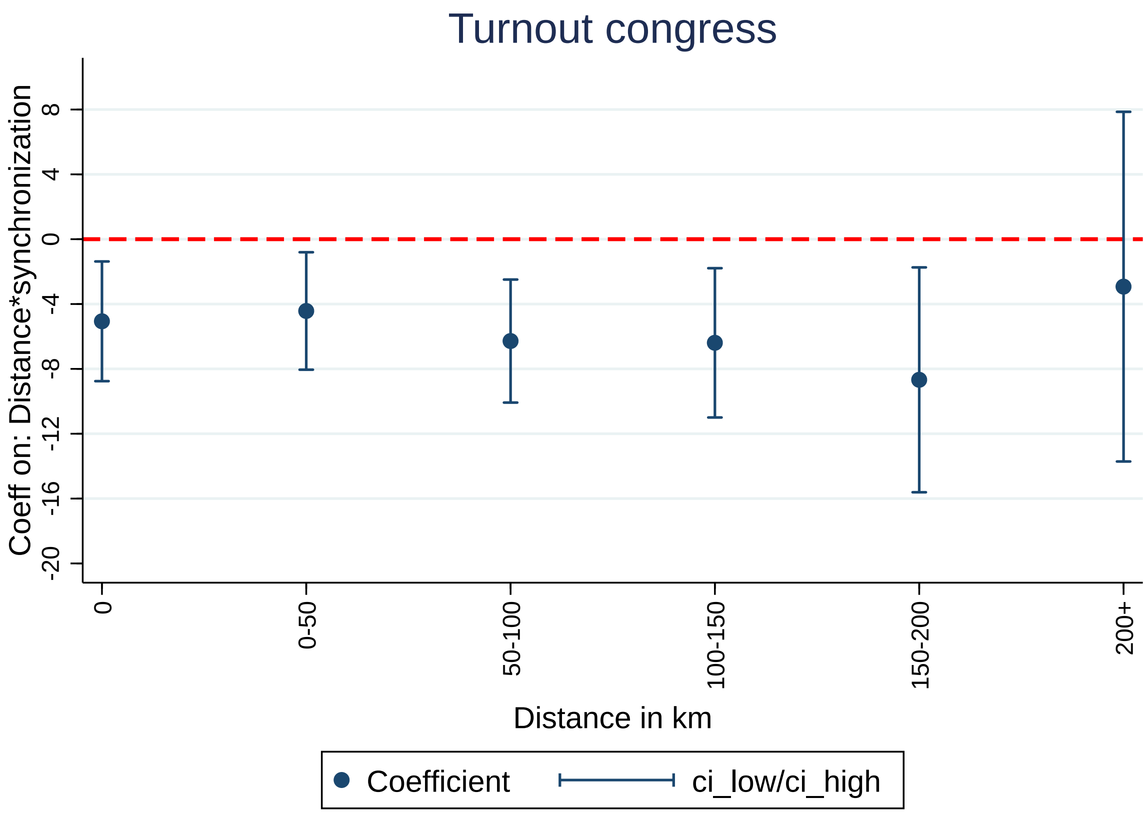
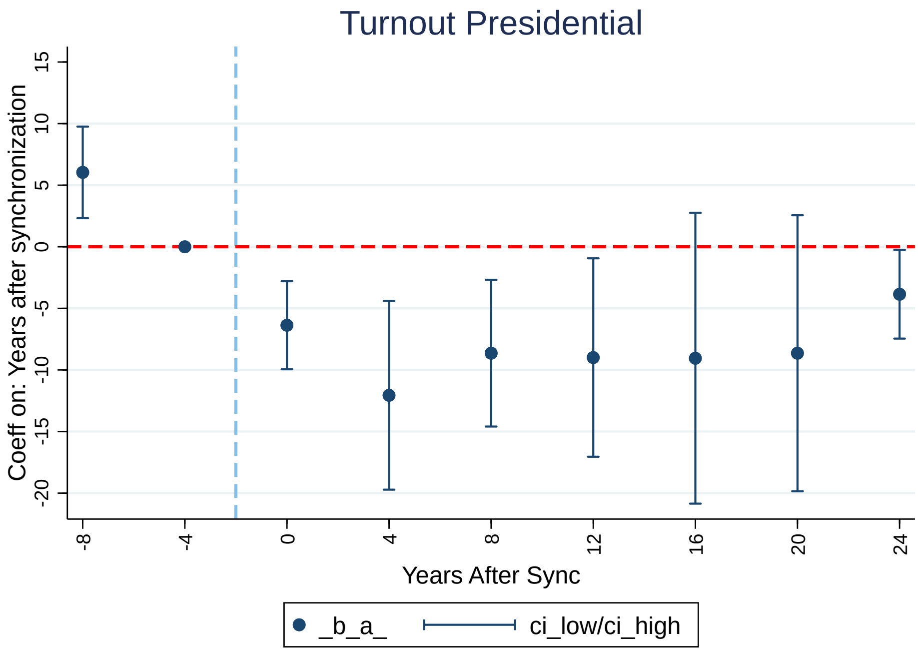


Figure 6: Presidential vote. Year, county and Confederacy-by-year FE are included. We also control for the separation of Virginias.



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A Appendix

Table A.1: Robustness checks: the Confederacy

	(1) Turnout, mid	(2) Turnout, all	(3) Turnout, mid	(4) Turnout, all	(5) Turnout, mid	(6) Turnout, all
Synchronized	-6.1014**	-4.9858***	-7.4295***	-5.8398***	-5.2659**	-3.9492**
	(2.5568)	(1.8491)	(2.7671)	(2.0312)	(2.5253)	(1.6802)
Separation of Virginias			9.8065	11.868*	20.080***	22.442***
			(6.7447)	(6.1738)	(3.2260)	(2.7745)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Confederacy-by-year FE	Yes	Yes	No	No	Yes, narrow	Yes, narrow
Subregion Clustering	Yes	Yes	Yes	Yes	Yes	Yes
Observations	31,331	64,328	31,331	64,328	31,331	64,328
R-squared	0.563	0.551	0.487	0.491	0.593	0.592

The dependent variable is turnout in congressional elections in midterms years (col. 1, 3) or all years (col. 2, 4). Synchronized is a dummy that takes value 1 if the county votes on the same day as its closest neighboring state. Columns 1 and 2 do not include the control for the separation of the Virginias; Columns 3 and 4 include no Confederacy-by-year FE, while columns 5 and 6 include them with a narrower definition of confederacy (11 Confederate states). Standard errors, clustered at the subregion level, are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A.2: Robustness checks: census years and full-sample

	(1) Census years	(2) Census years	(3) Full sample	(4) Full sample
Synchronized	-5.5591**	-4.7672**	-5.6735**	-4.4648**
	(2.7032)	(2.0828)	(2.4035)	(1.7734)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Confederacy-by-year FE	Yes	Yes	Yes	Yes
Subregion Clustering	Yes	Yes	Yes	Yes
Observations	18,899	39,680	34,501	70,733
R-squared	0.579	0.542	0.565	0.553

The dependent variable is turnout in congressional elections in midterms years (col. 1, 3) or all years (col. 2, 4). Synchronized is a dummy that takes value 1 if the county votes on the same day as its closest neighboring state. In column 1 and 2 we only include those years close to the census years (...8, ...0, ...2). In column 3 and 4 every county is included (even the ones that border with multiple states or those that change neighbor due to redistricting, that are not included in the baseline analysis). In all specifications we control for the separation of the Virginias. Standard errors, clustered at the subregion level, are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A.3: Stable counties with no change in area

	(1) Turnout cong.	(2) Turnout cong.
Synchronized	-5.7534**	-4.3222**
	(2.4214)	(1.7253)
County FE	Yes	Yes
Year FE	Yes	Yes
Confederacy-by-year FE	Yes	Yes
Subregion Clustering	Yes	Yes
Observations	15,034	30,785
R-squared	0.613	0.605

The dependent variable is turnout in congressional elections. Synchronized is a dummy that takes value 1 if the county votes on the same day as its closest neighboring state. We restrict our sample to the subset of counties whose area never changed between 1840 and 1920. In all specifications we control for the separation of the Virginias. Standard errors, clustered at the subregion level, are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A.4: Stable states: statehood before 1840

VARIABLES	(1) Turnout cong.	(2) Turnout cong.
Synchronized	-3.3241*	-3.3736*
	(1.9712)	(1.7138)
Observations	22,961	47,280
R-squared	0.595	0.575

The dependent variable is turnout in congressional elections. Synchronized is a dummy that takes value 1 if the county votes on the same day as its closest neighboring state. We restrict our sample to the subset of states that could vote since 1840 (early states). In all specifications we control for the separation of the Virginias. Standard errors, clustered at the subregion level, are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A.5: Population Ratio

	(1) Turnout, mid	(2) Turnout, mid	(3) Turnout, all	(4) Turnout, all
Synchronized	-4.7065*	-3.4953*	-4.1870**	-3.2466**
	(2.4129)	(2.0079)	(1.8127)	(1.5756)
Population Ratio	0.1243***	0.1236***	0.1027***	0.1024***
	(0.0390)	(0.0371)	(0.0299)	(0.0291)
Population Ratio*Synchronized	-0.0826***	-0.0815***	-0.0589**	-0.0582**
	(0.0315)	(0.0307)	(0.0247)	(0.0246)
Nb. counties, other side		-0.5952***		-0.4550**
		(0.1994)		(0.1921)
Border distance (km)		-0.0161		-0.0243
		(0.0287)		(0.0281)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Confederacy-by-year FE	Yes	Yes	Yes	Yes
Subregion Clustering	Yes	Yes	Yes	Yes
Observations	30,418	30,364	62,336	62,227
R-squared	0.584	0.590	0.571	0.575

The dependent variable is turnout in congressional elections. The independent variables include: the ratio between the population of the largest county on the other side of the border as measured by a linear interpolation of census data (Population other side (1000)) and the county voting population; the number of counties (Nb. counties, other side); the distance of the most populated counties from the border (Border distance, other side) within the subregion on the other side of the border. In all specifications we control for the separation of the Virginias. Standard errors, clustered at the subregion level, are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Figure A1: Midterm elections. Year, county and Confederacy-by-year FE are included. Sun and Abrahams (2020)

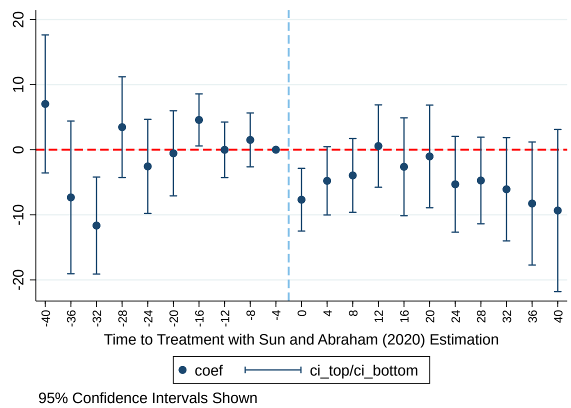

Figure A2: All elections. Year, county and Confederacy-by-year FE are included. Sun and Abrahams (2020)

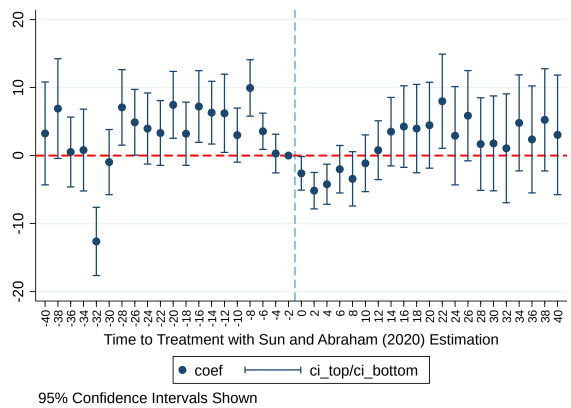

Figure A3: Midterm elections (25 years window). Year, county and Confederacy-by-year FE are included. Control for the split of Virginias

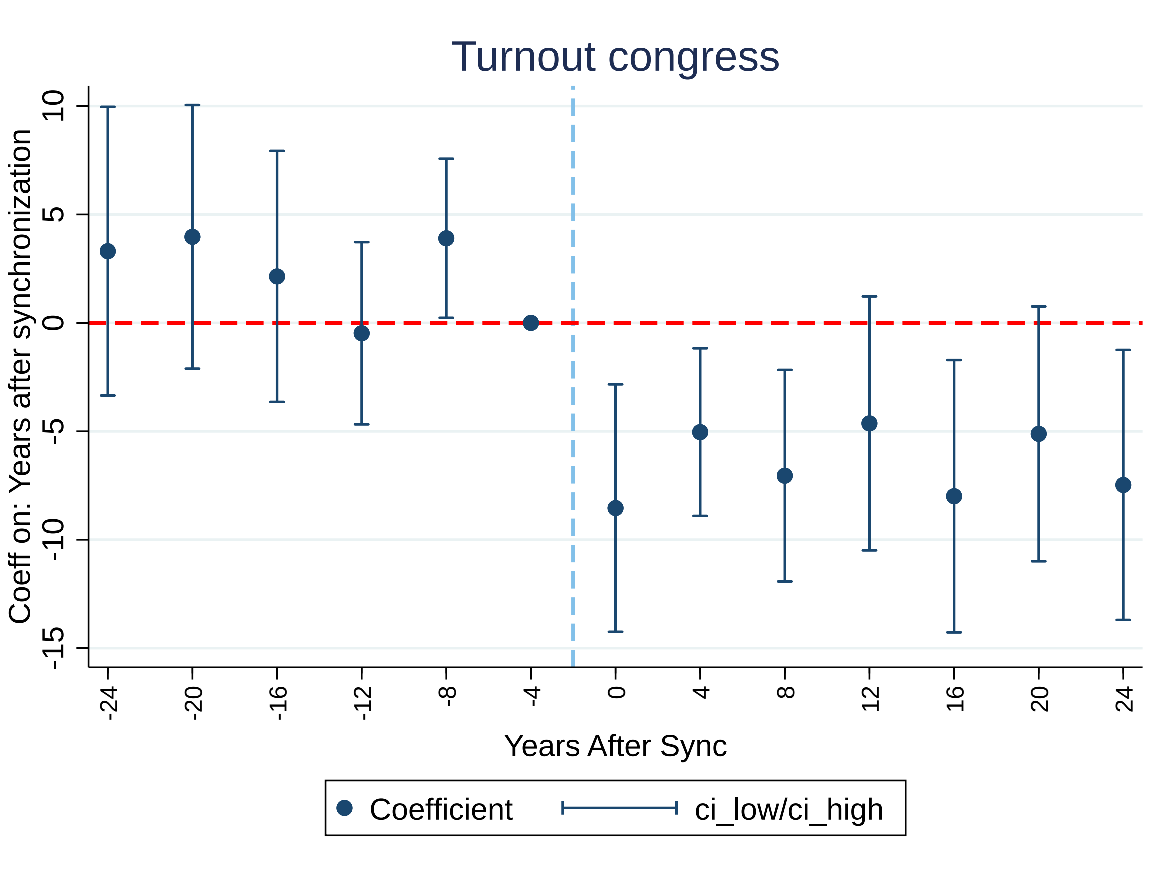

Figure A4: All elections (25 years window). Year, county and Confederacy-by-year FE are included

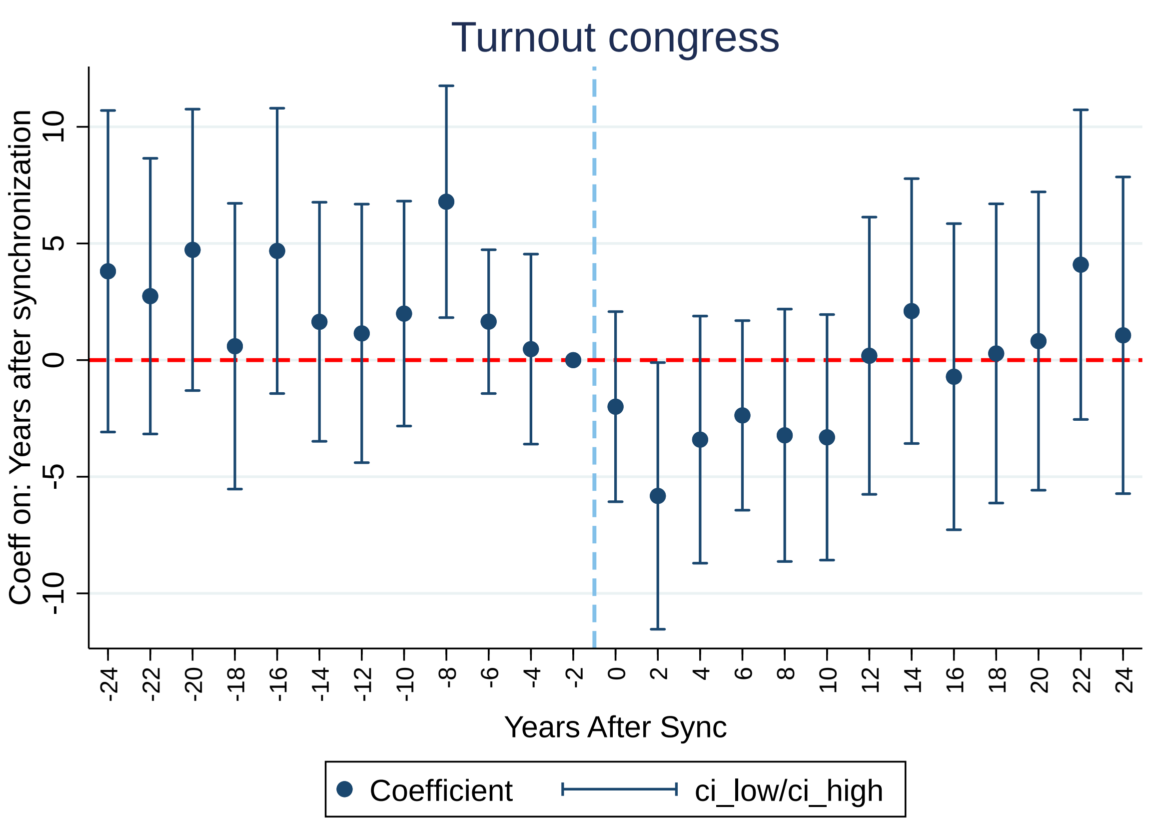

Figure A5: Stable states from 1840 (midterm). Year, county and Confederacy-by-year FE are included

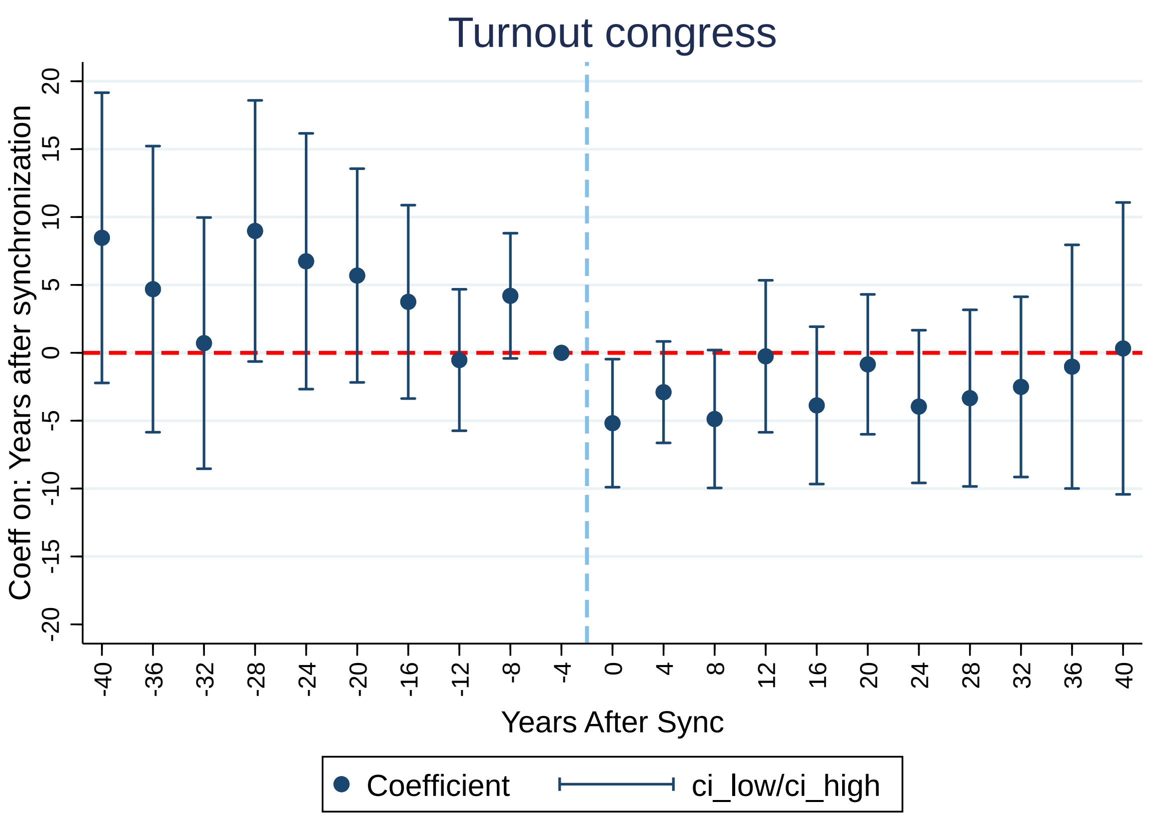

Figure A6: Stable states from 1840 (all). Year, county and Confederacy-by-year FE are included

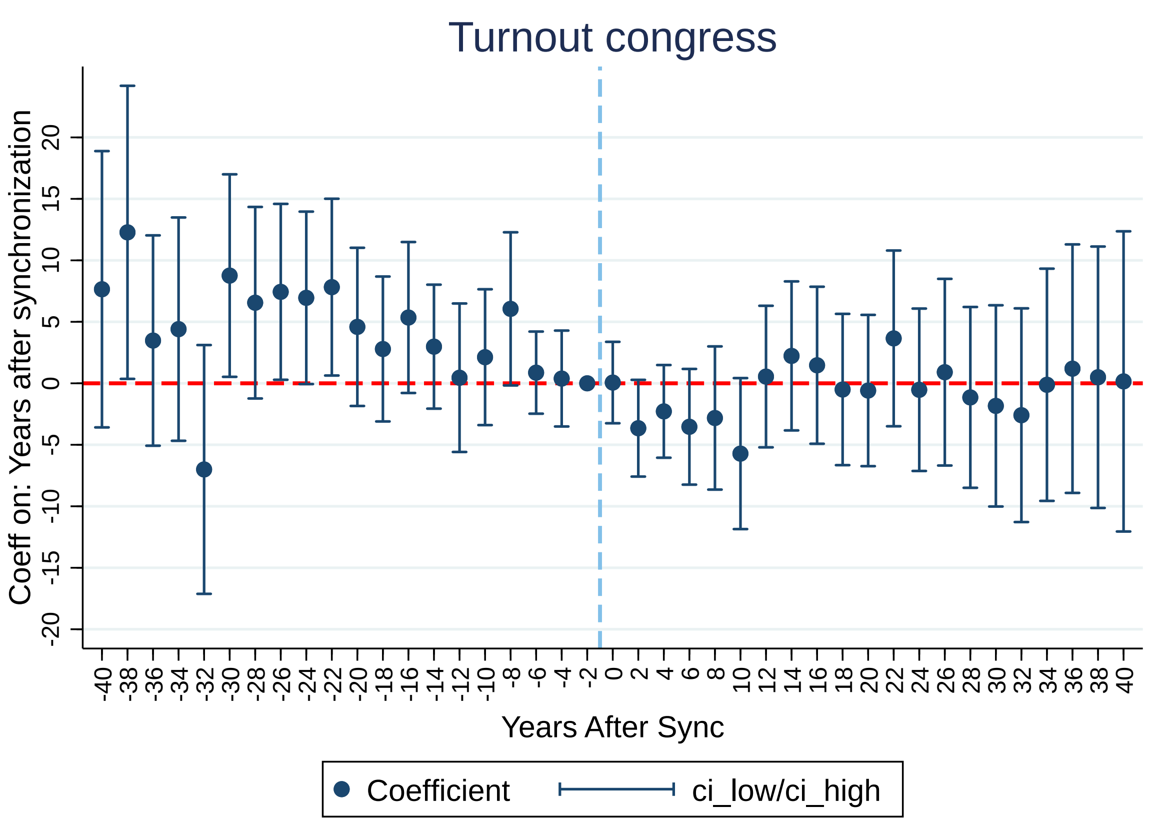

Figure A7: Midterm elections (confederacy). Year, county and Confederacy-by-year FE are included

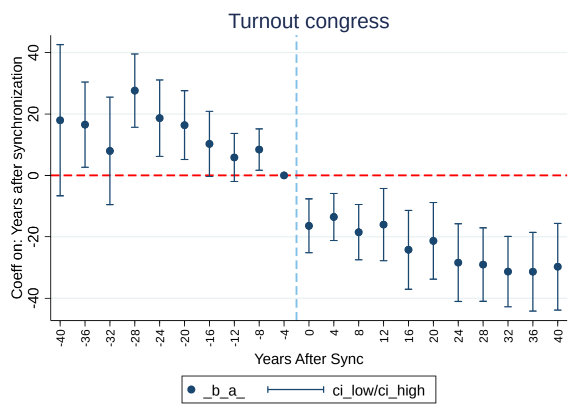

Figure A8: All elections (confederacy). Year, county and Confederacy-by-year FE are included

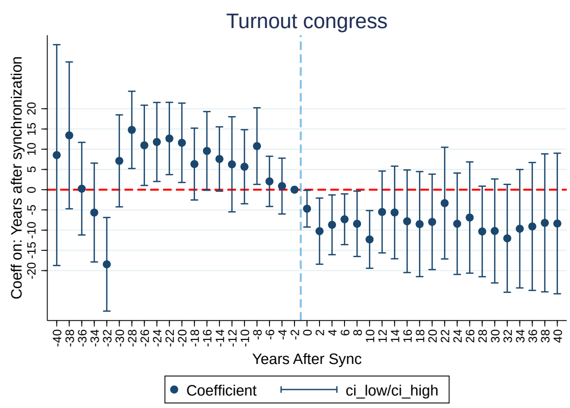

Figure A9: Midterm elections (excluding confederacy). Year, county and Confederacy-by-year FE are included

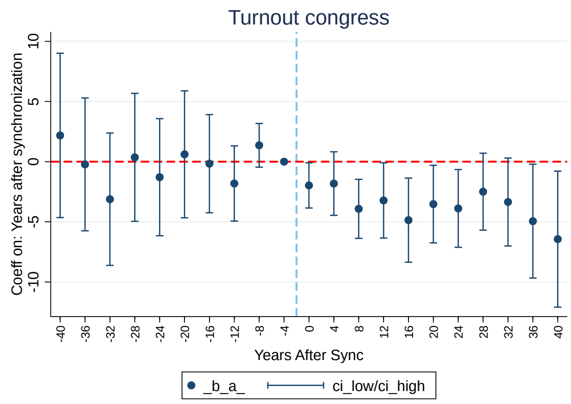

Figure A10: All elections (excluding confederacy). Year, county and Confederacy-by-year FE are included