Restrictive Voting Laws, Voter Turnout, and Partisan Vote Composition: Evidence from Ohio*

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Abstract

We estimate effects of expansion and contraction in early voting availability by using two homogenization laws from the State of Ohio, one in 2012 and the other in 2014, which forced some counties to dramatically expand and others to dramatically contract early voting. Using individual voter registration data, we look at the impact of changes in early voting by comparing individuals who live within the same 1 square mile block but in different counties. We find substantial positive impacts of early voting on turnout equal to 0.19 percentage points of additional turnout per additional early voting day. We find little effect on those below 25 and those over 60 suggesting that work and child-care are important determinants of turnout. Effects are larger on those who have voted in Democratic primaries than those who have voted in Republican primaries. The effect on Independents is small in midterm elections but approximately 0.5 percent per day of early voting in presidential elections. We use our estimates to simulate impacts on national elections and find that a federal mandate on early voting to the level of Minnesota would have altered the outcomes of the 2016 presidential election and majority control of the United States Senate. Our results suggest that early voting increases turnout, tilts the electorate towards the Democratic Party, and reduces the polarization of the electorate.

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1 Introduction

The United States was founded upon the notion that taxpayers should have the right to determine, indirectly through an electoral process, the levels of taxation. However, since its birth, the right to vote has been highly contested. Originally, in most states, the voting franchise was restricted to propertied white adult men. Even before the Civil War, political battles were fought to expand the franchise to include those without property. After the civil war, in 1870, the Fifteenth Amendment granted African-Americans the right to vote. However, through a battery of state laws passed starting in the late 1870s, poll taxes and literacy restrictions were placed upon voting in most Southern states which effectively limited minority participation in voting. Women were enfranchised at the state level starting with Wyoming in 1869. States increasingly passed female suffrage laws until the 19^{th} Amendment to the Constitution was passed in August of 1920. Restrictions upon voting, which dramatically reduced African-American participation in voting, remained in place until the 1965 passage of the Voting Rights Act under the Johnson Administration. Application of literacy requirements were famously selectively applied against African Americans (Keyssar, 2009). Following the Voting Rights Act, minority turnout increased dramatically (Cascio and Washington, 2014). However, minorities continued to turn out at lower rates than white Americans.

In recent years, political parties have been very active in passing legislation at the state level expanding or limiting ease of access to voting. State level legislative activity regulating voting has been primarily concentrated in 4 areas: (1.) Legal changes affecting the ease of voter registration, (2.) Laws expanding or contracting the ability of felons to vote, (3.) Laws which tighten or loosen identification requirements at the ballot box, and (4.) Laws expanding or contracting the prevalence of early voting availability.[1]

Early voting in particular and pre-election voting in general have become common forms of voting. Though pre-election voting began first in California back in 1976 and then in Texas in 1987, most of the rollout of early voting happened in the 1990s and 2000s (Biggers and Hanmer, 2015).[2] As of 1992, 7% of individuals cast their ballots using some form of pre-election voting (McDonald, 2016). By 2008, the beginning of our main sample, pre-election voting had expanded to over 30% of ballots cast nationally; these numbers rose to 34.5% by 2016. Initially, pre-election voting was primarily in the form of mail balloting. However, in recent years, the importance of in-person early voting has risen. In 2016, over 47 million of

[1] Early voting is otherwise known as "no-excuse" absentee voting.

[2] In this paper, we will refer to in-person early voting as early voting. The other form of early voting is absentee balloting. Where we discuss the sum of in-person early voting plus absentee balloting, we will refer to it as "pre-election voting".

the approximately 136.5 million ballots cast used some form of pre-election voting. 23 million of these were cast in-person. Thus, early in-person voting accounted for over 48% of total voting (McDonald, 2008). Early voting is potentially important in the United States because election day is not a national holiday nor a weekend day in the United States as it is in many developed countries.

In the 2016 general election, there were substantial differences across states in early voting availability. On the one hand, ten states had no in-person early voting.[3] At the other extreme, Minnesota provided 46 days of early voting. The changes in early voting which have occurred in the past two decades, have predominantly occurred at the state and county levels. Reductions in early voting have accelerated since the Supreme Court struck down the formulas used in Section 4 of the Voting Rights Act. The Voting Rights Act mandated that specific states and counties within states require federal Department of Justice pre-clearance to make substantive changes to their election law. Since June 2013, when the Supreme Court decided *Shelby County v. Holder* (2013), there has been a flurry of activity altering electoral law. This recent activity has been concentrated in states which were formally subject to the Voting Rights Act. Within months of the Supreme Court's decision, restrictive voting laws had been introduced in state legislatures in Alabama, Arizona, North Carolina, Ohio, South Carolina, Texas, and Wisconsin.

Of course, it is not clear that expanding opportunities to vote will actually increase voting. Some political scientists who have studied early voting have estimated positive effects on turnout (Herron and Smith, 2012; Herron and Smith, 2014), others have found no systematic overall impact upon turnout (Gronke et al., 2007) and others yet have found that early voting expansion has reduced turnout (Burden et al., 2014). The idea that early voting may reduce turnout may sound strange at first. However, there is a well documented effect that people vote in part to tell others (DellaVigna et al., 2017). It is also possible that voters turnout in order to be seen voting and that early voting, by spreading voting across many weeks, reduces the link between being seen and voting. Burden et al. (2014) have a similar explanation for their seemingly perverse findings. They claim that early voting weakens a sense of common solidarity which is important for motivating high turnout.

Unfortunately, given the importance of the subject, there are surprisingly few studies on the impacts of differences in state voting laws in general and early voting in particular. Moreover, what studies exist suffer from plausible endogeneity bias. Gronke et al. (2008) has an early review of the academic literature on the effect of convenience voting (early voting, absentee balloting, electronic voting, and voting by mail). The early literature largely relied

[3]The ten states without in-person early voting were Alabama, Connecticut, Delaware, Michigan, Mississippi, New Hampshire, Oregon, Pennsylvania, Rhodes Island, and Washington

time series variation (Gronke et al., 2007) within a state.

This does a decent job of controlling for endogeneity arising from characteristics of the electorate but does not control well at all for endogeneity due to characteristics of the election. Erikson and Minnite (2009) estimate the impact of photo ID laws rather than early voting laws. They use state-level differences in differences. They have an interesting approach for controlling for endogeneity arising from underlying characteristics of the electorate. They use the Current Population Survey (CPS) voter supplement which allows them to control in a rich way for voter characteristics but leaves them with a small sample size and large standard errors. Also, because they use cross-state variation, they are unable to control for competitiveness of state elections or national elections at the state level. Finally, because they use self-reported turnout data, their data suffers from the well known upward bias in self-reported turnout. They ultimately conclude that the literature is currently unable, given data and statistical tools, to decide whether or not photo ID laws and other similar voting laws have turnout effects.

Burden et al. (2014) and Herron and Smith (2014) are more recent papers which estimate impacts of early voting and use different sources of variation. They also focus upon the impact of early voting laws rather than photo ID laws and are thus more similar to our own paper. Burden et al. (2014) estimates the impact of of early voting laws on individual turnout using cross-state variation in legal changes which expanded or contracted the availability of early voting. They have greater statistical power than Erikson and Minnite (2009) due to the increase in the number of laws passed in recent years. Burden et al. (2014) runs regressions with county level turnout data as well as individual regressions with data from the CPS voter supplement. The identification comes from assuming that state level trends are uncorrelated with other determinants of turnout conditional upon controls.

Herron and Smith (2014) also examine the impact of early voting using variation solely within Florida when Florida reduced early voting from 14 to 8 days. They use voter registration data and thus view not only whether people vote but also when they vote. They also view race at the individual level. They find a decline in voting and particularly in early voting. They also find that the effects are concentrated in minority groups and registered Democrats. However, their identification comes from pure time series variation and thus it is hard to separate the effect of the legal changes to early voting from secular trends in voting patterns or election specific effects which may have differentially impacted African-Americans and Democrats.

Card and Moretti (2007) estimated the impact upon turnout of new and improved voting technology using roll out of the Help America Vote Act. They use cross-county variation controlling for covariates. This variation is cleaner than much of the other literature in that most races that voters pay attention to are at the state level (presidential races at the state level, Senate elections, and Governor races). Using cross-county variation and controlling for state fixed effects, Card and Moretti (2007) substantially reduce electoral endogeneity. However, they do not effectively control for endogeneity due to voter demographics. Voters of different age, race, sex and partisan leanings have different propensities to turn out and these are often election-specific.

In this paper, we estimate the impact of early voting on voter turnout. We do this using voter registration data from Ohio and look at turnout before versus after Ohio homogenized early voting availability across counties. In many states, counties differ in the number of hours and days that they are open. In this paper we make use of a natural experiment in Ohio which was implemented in two phases and which homogenized the days and even exact hours of early voting across counties in Ohio. Both of these changes were passed by Republican Governor John Kasich. The first was passed in time for the general elections in 2012 and the second in time for the general elections in 2014. The first of these changes fully eliminated all differences in early voting days and hours across counties. The second law uniformly reduced the number of days of early voting, eliminated same day registration early voting, and increased the number of weekend days of early voting.[4]

Our paper, rather than trying to add in covariates to control for unobservables, tries to construct treatment and control groups which are similar. We do this using geographical discontinuities in treatment across county borders. We thus follow the literature using spatial neighbors with differential spatial treatment (Dube et al., 2010; Snyder and Strömberg, 2010; Spenkuch and Toniatti, 2016) as well as the related, smaller literature using spatial RD techniques to estimate treatment effects on voter behavior (Ballot Initiatives: Keele et al., 2015; Incumbency Advantage: Erikson and Titiunik, 2015; Political Advertising: Spenkuch and Toniatti, 2016). We also add to a growing literature within economics estimating the impact of electoral interventions using more credible research designs (Braconnier et al., 2016; Naidu, 2012; Pons, 2016). Since we use individual level data from the Ohio voter registration database, we have tight standard errors. However, our spatial discontinuity approach also allows for credible identification.

We also show estimates using cross county variation. However, we only use within-state variation which means that we control for state level candidate quality such as US Senator

[4] All states except North Dakota require registration in order to vote. Ten states and Washington, D.C. allow registration on election day. All other states require pre-registration. In Ohio, registration must occur 30 days or more before the election in order to participate in a national general election. In practice this has meant 28 days before the election because 30 days before the election was a Sunday and the next day has been Columbus day. The State of Ohio always extends the deadline to the next business day if it falls on a weekend or a holiday. In years where early voting extended before the deadline, citizens could register to vote and vote at the same time in an early voting station. This is called same day registration.

and Governor quality as well as competitiveness in Senate, gubernatorial and presidential elections. We also show that cross-county variation in our Ohio sample is not as clean as our geographical discontinuity estimates. As we increase our bandwidth, our estimates fail demographic and political placebo tests for endogeneity.

Our best specification estimates effects within 1×1 mile blocks which straddle county lines where counties differentially changed early voting availability due to the change in state law. Besides looking at aggregate turnout effects, we estimate differential turnout effects for weekend days, same day registration days, and days where polls were open until 7 PM or later. We also estimate models where we allow for non-linearities in treatment. We show estimates broken down by presidential elections and midterm elections. We not only show estimates by different types of treatment but also by different types of voters. We estimate the impacts differentially by sex, party, and age. Overall, we find that an extra day of early voting increases turnout by 0.24% in presidential elections and by 0.13% in midterm elections.

Finally, we use our estimates of partisan effects to linearly simulate the impact of hypothetical national early voting election laws. We find limited impacts on outcomes in the 2012 and 2014 elections. However, for the 2016 election, we find that eliminating early voting would have reduce Democratic House seats by 10 whereas mandating 23 days nationally (the current level in Ohio) would have tilted the presidential election to Hillary Clinton. We find that mandating 46 days nationally (the current level in Minnesota) would also have shifted three Senate seats and the balance of the Senate to the Democrats.

In Section 2, we give an overview of the electoral law changes we use in the state of Ohio. In Section 3, we describe our data. In Section 4, we present our methodology. In Section 5, we discuss our main estimates. In Section 6, we show results of simulations of national electoral law changes on election outcomes. Finally, in Section 7, we conclude.

2 Ohio Election Law Changes

Like many states, Ohio saw large expansions of early voting in the 2000s. In 2002, 6.8% of voters cast pre-election ballots. In 2005, Ohio passed legislation allowing for in-person early voting. By 2008, 29.7% of the electorate voted pre-election (Kaltenthaler, 2010). In 2010 and in 2014, 4.6% of the voting population made use of in-person early voting. In the general election of 2012, the percent making use of in-person voting before the election was 10.6% and by 2016, it had risen to 11.8%.[5] The contraction in early voting availability in urban areas happened during a period of increased popularity of early voting.

[5] These numbers on the prevalence of in-person early voting were obtained from the Ohio Secretary of State website: https://www.sos.state.oh.us/SOS/elections/Research/electResultsMain.aspx.

The expansion in early voting was differential across counties. Urban, Democratic areas expanded early voting at a faster rate than rural, Republican ones. By 2008, rural Pickaway County was open for 109 hours of early voting, spanning a total of 11 days including only 1 weekend day, 2 days of same day registration and no weekend days of same day registration or Sunday voting days. By contrast, urban Franklin County, which contains the city of Columbus, was open for a total of 340 hours spread over 35 days including 7 days of same day registration voting, 10 weekend days of early voting including 2 same day registration weekend voting days, and 5 Sundays.

In November, 2010, Republican John Kasich defeated incumbent Democrat Ted Strickland for the Governorship. In addition, the State Senate remained majority Republican and the State House of Representatives switched majority control to the Republican party. Under unified Republican control, the government passed State Bill 295 which homogenized early voting across counties. Each county early voting station was required to be open the exact same hours on the exact same days as all other counties. This meant that Cuyahoga County with a population of 1.266 million in 2012 ended up with identical hours of early voting as rural Pickaway County with population 56,000. The law eliminated early voting for the three days prior to the election. This meant that early voting in the weekend before the election was eliminated from all counties. The total number of days was changed to 26 with 4 same-day registration days though no weekend days of same day registration and 2 Saturdays though no Sundays.

In 2014, the Ohio legislature passed and the Governor signed Senate Bill 238 which made further changes to early voting. It reduced the number of hours from 246 to 188, reduced the number of days from 26 to 22, eliminated same day registration, but increased the number of weekend days to three including one Sunday. In our paper, we exploit the differential changes across counties for the general elections between the presidential election years, 2008 and 2012, as well as between the non-presidential election years, 2010 and 2014.

Large pre-2012 discrepancy across counties within Ohio led to large differential changes due to the state policy changes implemented in 2012 and 2014. On the one hand, Cuyahoga, Franklin and Summit Counties all saw reductions of 9 days of early voting. This reduction was largely due to reduced weekend voting. In each case, 8 of the 9 days were Saturdays or Sundays. Moreover, Cuyahoga's total hours were reduced by 56.5; Franklin's and Summit's each by 94 hours. By contrast, Wyandot and Pickaway both increased their weekend early voting by one day. Though Wyandot's total number of days of early voting availability did not increase, Pickaway's did by 15 days. Wyandot's total hours of early voting increased by 100 and Pickaway's by 137.

The contracting counties were quite different from the expanding ones in terms of political orientation. Cuyahoga, which contains Cleveland, is a large urban area with a 1.28 million population as of the 2010 census. It is 30.3% African American and had a 68.8% vote share for Obama in 2008. Franklin County, containing Columbus, Ohio, is 21.2% African-American, has a population of 1.16 million and had a 60.1% Obama vote share in 2008. Summit County, containing Akron, Ohio, has a population of 540,000, is 13.2% African-American and had a 56.7% Obama vote share in 2008. By contrast, Pickaway, which is a rural county without a major city, has a population of less than 60,000, is 3.7% African-American and had an Obama vote share of 39.8%; similarly, Wyandot, another rural county, has a population slightly above 20,000, is 0.4% African-American and had a 38.6% Obama vote share.

The changes between 2010 and 2014 are largely similar to those between 2008 and 2012 since much of the homogenization was achieved by the 2012 election. Both Cuyahoga and Summit Counties saw reductions in early voting of 13 days, 7 of which were weekend days. Cuyahoga's reduction totaled 74.5 hours and Summit's 152 hours. Huron County, a County with slightly less than 60,000 population in 2010, an African American population accounting for less than 2% of the overall population, and an Obama vote share of 47.2% in 2008 saw reductions of 4 total days but net expansions of 2 days of weekend voting. In total, Huron County's hours increased by 75. Wyandot County saw a reduction in one day of total voting but an expansion in 3 days in weekend voting. In total, Wyandot expanded by 66.5 hours of early voting.

Of course, comparing the counties which contracted versus expanded early voting risks strong endogeneity bias due to correlation of differences in demographics and thus voting trends with the magnitudes and signs of early voting changes. Our main strategies thus rely on finding locations with differential contractions and expansions but similar demographics and thus voting trends.

In Figure 1, we show the changes in early voting days between 2008 and 2012 and between 2010 and 2014 by county in two maps of Ohio. We see large reductions in early voting days both in 2012 relative to 2008 and in 2014 relative to 2010 for the large urban counties of Cuyahoga which contains Cleveland, Summit which contains Akron and Franklin which contains Columbus. We also show changes of early voting by six other different measures across counties between 2008 and 2012 in Figure A.1 and between 2010 and 2014 in Figure A.2. These measures of early voting access are the numbers of weekend days, days allowing same day registration, days open late, weekdays, Saturdays, and Sundays. For most measures, urban counties experienced large reductions of early voting in 2012 and 2014, while rural counties saw increases, no changes, or relatively small decreases.

3 Data

Our main data source is the voter registration database from the State of Ohio. The database contains full name, exact date of birth, date of registration, individual voting history dating back to the year 2000, address of residence including county, precinct, and party for those who have participated in primaries.[6]

Ohio is an open primary state. Therefore, the data does not contain party registration but instead records the party of the primaries the voter participated in. We record an individual as a Republican if the most recent primary they participated in was a Republican party primary, a Democrat if the most recent primary they participated in was a Democratic party primary, and an Independent for those who have never participated in a primary. 43.1% of registered individuals are listed as Independent in our sample, 30.4% are listed as Democrats and 26.5% as Republicans.[7]

Using ArcGIS and Google Maps, we geocoded each individual registration address into longitude and latitude. We then divide the State of Ohio into a mutually exclusive and exhaustive set of equal-sized square geographical blocks.

We additionally use the geocoded locations to assign each individual to a census block group and we then merge demographic information on race, education, and income at the census block level to each individual. Thus each individual within a census block-group has a set of demographic variables which do not vary across individuals within the same census block-group. These set of variables include % white, % black, % Hispanic, median household income, % high school dropouts, and % college graduates. In approximately 10 percent of cases, ArcGIS does not match to a census block group. In this case, we could compute minimum distance to each census block group in the state using latitude and longitude and assign an individual to the geographically closest block group.[8] For consistency, we match all individuals to census block groups using the minimum distance to block group centroids.[9]

Next, for each of Ohio's 88 counties, we obtain from each individual county secretary of state the exact hours of early voting availability for each day of early voting. We do this for the years 2008, 2010, 2012, and 2014. The data was not available before 2008. We use

[6]The registration data for Morgan County is missing from the files that we obtained from the Secretary of State of Ohio. Morgan County is one of the smallest population counties in Ohio. It has a total of 14,904 residents out of state with 7.6 million registered voters. Thus, less than 0.1% of Ohio voters who reside in Morgan County are not included in our sample.

[7]In our data, which goes back to the year 2000 and covers eight national primaries, only 7.2% of registered voters voted in a Republican primary for one election and a Democratic primary for another election.

[8]We have run our results dropping the individuals with imputed census block group and the results are near identical.

[9]Estimates change little when we use a sample by matching through either ArcGIS or minimum distance, or by dropping the individuals unmatched to a census block group by ArcGIS.

this data to compute our main treatment variable: number of days of early voting by county for each election. We also compute other treatment variables which we use for estimating heterogeneity in the treatment effect by type of treatment. These additional variables are number of hours, number of weekend days, number of Saturdays, number of Sundays, number of week days, number of days of same day registration, and number of days where polls were open until 7 PM or later.

Finally, we compute for each individual, the probability that their sex is female. Ohio voter registration data does not record sex. However, the social security administration keeps a registry of all baby first names by sex. These lists are maintained by year. For confidentiality reasons, the data are truncated. Names with fewer than 5 occurrences in a given year for a given sex are not reported. As an example, in the year 1980, 94.8% of births in the United States are in our national baby name list. We obtained both the national lists as well as the lists for the State of Ohio. For each year and for each of the two lists (national and Ohio), we compute the probability that a name is female as the proportion of babies with that name who are female. If a name is not listed for a particular gender, we assume that zero babies were born with that name for that gender. We use the probability that a baby is female as our sex variable. We drop unmatched observations. 95.6% of individuals in our voter registration file match to one of the first names in the national baby name file in their birth year; 89.9% match to one of the first names in the Ohio state baby name file in their birth year.

4 Methodology

We employ four empirical strategies to estimate the impact of restrictive voting laws upon voter turnout. The last of these is our preferred strategy. The first is the standard county-level difference in differences estimator where we regress voter turnout on treatment controlling for a county fixed effect and a time fixed effect. We show pooled estimates for all general elections from 2008 to 2014 as well as estimates separately for presidential and midterm election years respectively. Our main treatment variable is the number of days of early voting. However, we also estimate models where we are interested in the heterogeneity of the treatment effect across different types of treatment (i.e. weekdays versus weekends, same day registration days versus normal days, days when early voting extends to 7 PM or later versus regular hour days). In these cases, we simultaneously regress upon multiple regressors. In order to compare our difference-in-differences estimates to estimates of the impacts of voting interventions at the county level, ours differ only in that we do not use cross state variation. Our estimation equation is given by:

$$V_{ict} = \alpha_t + \phi_c + \boldsymbol{T_{ct}'}\boldsymbol{\beta} + \epsilon_{ct} + \theta_{ict} \tag{1}$$

where V_{ict} is a binary variable equal to 100 if voter i turns out in county c for the general election in time period t and is zero otherwise, α_t is an election-year fixed effect, ϕ_c a county level fixed effects, T_{ct} is a vector of treatment variables, ϵ_{ct} is a mean zero serially correlated county-specific random term which is independent across counties, and θ_{ict} is an idiosyncratic individual level random term. We choose our dependent variable to take on the values of 100 or zero so that our estimates are expressed in units of percentage point effects per unit of treatment. We cluster standard errors for equation (1) at the county level. This specification assumes that aggregate voting trends by county are uncorrelated with treatment. In particular, it assumes that trends in voter turnout in urban counties which saw large reductions in early voting would have been the same as in rural counties whose early voting access stayed constant or increased absent the early voting changes.

Our second main specification replaces the county-level fixed effects ϕ_c from equation (1) with individual fixed effects γ_i. Since there are no covariates in these regressions, the switch to individual fixed effects operates by dropping those who were not registered continuously over the time period. In the pooled estimates, this eliminates all individuals who registered for the first time in Ohio after the 2012 presidential election. In the estimates reported separately by election-type, it also eliminates individuals who were not registered for both elections within an election-type (i.e. both midterm elections for the midterm election results and both presidential elections for the presidential election results are required). First time registrants include those who were previously too young to register, those who were not too young but had never registered or voted, and those who moved to Ohio from out of state.[10] The individual fixed effects identification strategy relies upon weaker assumptions than the identification strategy assumed by the best related papers in the observational methods literature such as Card and Moretti (2007) which use county instead of individual fixed effects because the county fixed effects results are not robust to demographic shifts in the registered electorate. Our individual fixed effects model, by contrast, correctly estimates treatment effects for those whose registration did not change across elections. However, this is still under the maintained assumption that voting trends for registered individuals across counties was uncorrelated with treatment. Our model of turnout, in this case, is given by:

$$V_{ict} = \alpha_t + \gamma_i + \boldsymbol{T'_{ct}}\boldsymbol{\beta} + \epsilon_{ct} + \theta_{ict} \tag{2}$$

We next restrict our sample to individuals living within k miles of county borders, excluding borders coincide with Ohio state borders. We refer to such sample as k-mile sample

[10]The data is already purged of those who have passed away. If there is measurement error in reporting of death, it does not impact our estimation as long as it is not differential across county lines and in a way that is systematically correlated with treatment.

and re-estimate equation (2) using the k-mile sample with standard errors still clustered at the county level. We restrict the sample because our fourth and baseline estimation strategy requires restriction to individuals near county borders and we separately estimate on that sample using equation (2) in order to isolate the impact of the geographical discontinuity design method. Our benchmark block size is 1 square mile, though we also show estimation with block sizes ranging from 0.1×0.1 miles to 20×20 miles. Individuals living within one mile of counties borders inside Ohio are marked by violet dots in Figure 2.

Our final and preferred specification is a geographic discontinuity design. We divide up the State of Ohio into a mutually exclusive and exhaustive set of $k \times k$-mile square blocks (i.e. k^2 square mile blocks). Each individual then belongs to a unique block. For the presidential elections and the midterm elections respectively, we regress the change in turnout between year t and year $t-4$ upon the change in early voting days, using the k-mile sample and controlling for geographical block fixed effects. We separately estimate for each of the two types of elections: presidential elections and midterm elections. We thus estimate:

$$\triangle V_{ibc} = \boldsymbol{\triangle T_c'\beta} + \rho_b + \epsilon_c + \theta_{ic} \tag{3}$$

where ρ_b is a geographical block fixed effect. Notice that the first differencing eliminates any individual fixed effect and the geographical block fixed effect accounts for any year-specific local geographical/demographic effects which are constant within small areas across county lines. This specification is our most taxing and is thus the specification which requires the weakest identification assumption. Our maintained assumption under this identification strategy is that turnout trends for individuals are not correlated with change in treatment ∇T_c, within small geographical blocks.[11]

We additionally estimate the geographical discontinuity model pooling the presidential and midterm elections. Since nationally, turnout is usually around 15 percentage points lower in a midterm election than in a presidential election, it is reasonable to believe that voting trends may differ by type of election. We thus allow for voting trends to differ by block separately in midterm elections and in presidential elections respectively. With this

[11] This estimation strategy derives from the geographical discontinuity design literature which initially arose in the context of the empirical literature on the minimum wage (Card and Krueger, 1994; Dube et al., 2010). Here, instead of comparing counties within pairs of counties which straddle state lines and have different minimum wage levels over time, we are comparing individuals within small geographical blocks who live in different counties with differential changes in the availability of early voting over time. Our estimation strategy would be analogous to the minimum wage literature if we put in block×county fixed effects instead of first differencing by individual. However, since we only have two data points per individual, first differencing our data by individual is identical to putting in individual fixed effects and putting in individual fixed effects is a more stringent specification than putting in block×county fixed effects. The first differencing is computationally preferable to the fixed effects approach due to the large sample of individuals.

specification, we estimate:

$$\triangle V_{ibce} = \rho_{be} + \boldsymbol{\triangle T'_{ce}\beta} + \epsilon_{ce} + \theta_{ibce} \tag{4}$$

where ρ_{be} is an election-type-specific geographical block fixed effect. Both equations (3) and (4) cluster standard errors two-way at the county and county-pair levels to account for county specific correlation as well as correlation in the nearby area.[12]

In addition to running regressions with voter turnout as our dependent variable, we also put placebo variables on the left hand side. Placebo variables measured at the individual level include age and party affiliation (Democrat, Republican, and Independent). However, we also put in census aggregate variables which come from matching individuals to census block-groups. For variables measured at the individual level, we also estimate our geographical fixed effects model interacted with variables for subgroups of the population. We do this for Democrats, Republicans, and Independents as well as for the estimated probability of being female. In this case, we estimate interactive models given by:

$$\triangle V_{ibce} = \rho_{be} + \beta \Delta T_{ce} + \boldsymbol{D'_i \triangle T_{ce} \gamma} + \epsilon_{ce} + \theta_{ibce} \tag{5}$$

where $\mathbf{D_i}$ is a vector of demographic variables measured either at the individual level or the block group level.

We also separately estimate equations (3) and (4) by five year age groups where we break up our sample into mutually exclusive sets of people born within the same set of 5 contiguous years.

Finally, we additionally estimate models where we allow for non-linearities in treatment in which case we estimate:

$$\triangle V_{ibce} = \rho_{be} + \beta \triangle T_{ce} + \theta \triangle T^2_{ce} + \epsilon_{ce} + \theta_{ibce} \tag{6}$$

where ΔT^2_{ce} is treatment squared (i.e. squared changes in number of days).

[12] We follow the geographical discontinuity literature here in clustering two way by the treated unit (county in our case) and the common local geographical unit (geographical block in our case). In the minimum wage literature, clustering is done two-way at the state-pair and state levels in order to remove correlation in local areas as well as mechanical correlation due to the fact that main counties appear in multiple county pairs. Since each geographical block is uniquely matched across counties to itself in our case, there is less need to cluster on county than there is to cluster on state in the minimum wage literature. However, clustering at the county level does account for correlation within counties such as county-level policy or, in some cases, school districts. In general, clustering two-way is more conservative as standard errors tend to be approximately 10% larger on average than clustering only on county.

5 Results

In this section, we discuss our main results. We first present covariate balance by size of geographic blocks after which we present our main aggregate turnout effects. We show our main turnout effect pooled across election type as well as individually for presidential and midterm elections. We additionally show robustness of our main turnout effects by bandwidth. We then break down our results by age, sex, and party. We end the section by showing evidence on whether turnout effects are non-linear in the number of days of early voting available.

In Table 1, we show the potential endogeneity issues of cross-county comparisons. We do this by comparing demographic and voting history characteristics of counties with above-mean versus below-mean change in number of early voting days. We do this separately for the changes between presidential elections (i.e. between 2008 and 2012) as well as the changes between non-presidential general elections (i.e. between 2010 and 2014). In appendix Table A.1, we also break down counties by above versus below mean change between presidential election years in hours, days open late (7 PM or later), weekend days, Sundays, and days with same day registration respectively. In appendix Table A.2, we show the same table for changes between the midterm elections.

We discuss the results for our main treatment variable, changes in number of days, as reported in Table 1; however, the results are broadly similar to those for the other treatment variables reported in the first two appendix tables. We then show average demographic characteristics from the Census as well as average individual characteristics from the voter registration data in 30 rows (15 characteristics for each of expanding and contracting counties). At the bottom of the table, we show the numbers living in counties with expanding versus contracting early voting according to the measure in that column. Most individuals saw expansions in hours, declines in days, expansions in weekend days, and declines days with same day registration.

Important for our identification strategy, there are substantial political and demographic differences which correlate strongly with the size and magnitude of the changes in early voting days. The distribution of changes in days is left-skewed both for presidential and for midterm elections. As shown in Figure 1, between 2008 and 2012, only 2 counties increased the number of days of early voting, one by 4 days and the other by 15. In contrast, 20 counties decreased their early voting, 4 by between 5 days and 9 days. Between 2010 and 2014, three counties increased early voting and by at most three days. On the other hand, 17 counties decreased their number of days of early voting by between 5 and 13 days. Across counties, individuals in counties with larger reductions in days of early voting were substantially less likely to be white. Counties with below-mean change in days for midterm elections were fully 18.5 percentage points less white than above-mean counties and 12.5 percentage points less white for presidential elections. Counties with larger reductions were unsurprisingly also more African-American. Though median household income varies by less than 10% across above-mean and below-mean counties, the college graduation rate in below-mean counties is more than 25% higher than in above-mean counties in presidential elections and almost twice as high for midterm elections. Uniformly across measures, registered voters in above-mean counties are around 10 percentage points more likely to have most recently participated in a Republican primary and 5-8 percentage points less likely to have participated in a Democratic primary. We geocoded polling stations and computed distance to polling station for each individual based upon their registration address. Average distance is approximately 10 miles and does not differ substantially across above-mean and below-mean counties. We also show turnout for 2008, 2010, 2012 and 2014 respectively. There are larger drops in turnout in counties with larger drops in number of days of early voting both for presidential and for midterm elections. However, demographic and political differences across expanding and contracting counties should give us pause in interpreting those differential changes in turnout as causally attributable to changes in early voting policy.

5.1 Aggregate Turnout Effects

We present our main effects in Table 2. We show our results in three panels: pooled results, presidential election results and midterm election results. We show results in each panel from specifications (1), (2), and (4) for the pooled results and (1), (2), and (3) for the presidential and midterm results. The estimates are remarkably stable across specifications for the pooled panel. Estimates range from 0.1488 per day for the individual fixed effects model in the 1-mile border sample to 0.1938 with our geographical discontinuity model in the 1-mile sample. The magnitude of the results suggests that an additional day of early voting leads to additional turnout equal to roughly 0.2 percentage points. Thus, 5 days of additional early voting increases turnout by around 1 percentage point. The conventional county difference-in-differences estimate is 0.1618. The range of the estimates is within 24% of the benchmark geographical fixed effects model. The standard errors are also relatively tight. All four specifications are statistically significant with a 95% level of confidence or higher and three of the four at above a 99% level of confidence. The standard errors for all the models except for the geographical fixed effects model are quite similar. They are around 0.05 percentage points. The estimates are very tight in large part because the sample size is so large. The standard errors for the individual fixed effects model restricted to the 1-mile border sample are roughly the same magnitude as the full sample county difference-in-differences despite the fact that the sample size drops by slightly more than 95%. This is likely, at least in part, because the border samples are a more homogeneous sample so that the reduction in sample size does not come at the expense of higher standard errors. The standard errors rise with the final geographical fixed effects model because they are clustered two-way on county and county-pair rather than just on county.[13] This tells us again that the comparison across county borders is apt because the increase in standard errors comes from accounting for positive correlation within a county-pair in addition to controlling for within-county correlation.

The homogeneity across specifications, however, masks heterogeneity in the effect as well as in estimation of the effect across models between effects upon presidential and midterm election years. The presidential year estimates range from 0.0549 for the county fixed effects model to 0.2411 for the baseline geographical fixed effects model. Three of the models are statistically significant at a 95% level of confidence or higher. However, the county difference-in-differences model is not. As shown in Table 1, the places which expanded early voting hours were Republican counties and the contracting areas were white. The lower numbers in the county fixed effects model reflects declining support and thus lower turnout for President Obama in the more rural, Republican areas of Ohio. The Obama vote share remained largely stable in urban areas but declined by a couple of percentage points in rural areas where pro-Obama voters were less energized to turn out in 2012. Though the county difference-in-differences model could control for voter demographics, bias is a problem if the statistical model does not include all relevant variables correlated with treatment and also if the functional form of the relationship between turnout and controls is not correctly specified. The geographic discontinuity model does not, by contrast, rely upon correctly specifying covariates or upon finding the correct functional form of the relationship between turnout and covariates.

Finally, we also show estimates from midterm elections. Again, the county difference-in-differences model is an outlier. This time it is twice the size of the other three models. Also for midterm elections results, the estimates from the other three models are similar (different from the presidential elections estimates). This is not the case for the presidential year results. The substantially larger estimates in the county difference-in-differences model is due to the fact that 2010 was a year of very low Democratic turnout and a Republican wave. 2014, by contrast was a normal year. Much of the increase in Democratic turnout happened precisely in the more rural areas where there was less Democratic enthusiasm in 2010. Thus, for the midterm elections, the increase in voting by Democrats in Republican areas bolstered the increase from relative early voting increases. Thus, the county difference-in-differences

[13] If we estimate the pooled geographical discontinuity model and cluster only on county, then the standard errors are smaller.

model, where as shown in Table 1, Democrats are more prevalent in areas where early voting was cut, is plagued by this endogeneity. The one mile border sample with geographical fixed effects where, as shown in Table 3 and Table 4, there is covariate balance, the estimates are largely purged of this endogeneity. Overall, our estimates suggest that endogeneity bias in the two-way fixed effects model is a concern. In estimates of effects during presidential years, the bias is negative because many turned out to vote for Obama, particularly in less Democratic areas where early voting expanded, initially in 2008 but not later in 2012. The midterm two-way fixed effects estimates, by contrast, suffers from the opposite bias, since Democratic unpopularity boosted Republican turnout in 2010 relative to 2014 and particularly in areas where expansions of early voting occurred. Our baseline estimates suggest a statistically insignificant turnout impact of 0.1348 percentage points per additional day of early voting for midterm elections and a statistically significant 0.2411 percentage point impact per additional day of early voting for presidential elections. Pooled, this is equivalent to an additional 0.1938 percentage points per day.

5.2 Covariate Balance

In the prior section, we presented geographical discontinuity estimates with a bandwidth of one mile. In this section, we motivate our bandwidth choice by running placebo estimates for a range of different bandwidths. We estimate equation (3) separately for midterm and presidential election. Our bandwidths of square blocks range from 0.1 miles $\times$ 0.1 miles to 20 miles $\times$ 20 miles. Overall, we include 8 different block sizes including our benchmark block size of one mile. These results are shown in Table 3 for presidential years and Table 4 for midterm years. We regress placebos on our main treatment variable: the change in the number of days of early voting. Our individually measured placebo variables are dummy variables for Independents, Democrats, and Republicans, age in 2008, sex and distance to early voting station. We also put in census variables, measured at the individual's census block-group, as placebos. These include % college graduates, % high school dropouts, median household income, % Hispanic, % black and % white. Out of our ten placebos, none are statistically significant with presidential year treatments at a 5 percent level of confidence for bandwidths of 0.1, 0.5, 1, 1.5, 2, or 3. At a bandwidth of 0.5 miles, the number of Independents, the number of Republicans, and the number of Democrats are all statistically different at a 10 percent level for counties which experienced increases in early voting. The point estimate implies that for 5 extra days of early voting, there are 1.8 percentage points more Independents. If all the placebo tests were independent, the chances that out of 10 covariates and 6 bandwidths totaling 60 tests, three or more would be statistically significant at a 10 percent level or greater by randomness alone is 95%. Starting with a bandwidth of 5 miles, the share

Democrat becomes larger and statistically significant. The statistical significance is due to the rise in the coefficient since the standard errors actually uniformly increase across placebo variables. Interestingly, the standard errors increase due to the increased heterogeneity which also validates our use of a smaller bandwidth for our benchmark estimates. At a bandwidth of twenty miles, we find that 4 of the 15 covariates are statistically significant at below a 1 percentage point level: Democrat, Republican, share black and share white. Moreover, the magnitudes are quite large. An addition of 5 days increases the share Republican by approximately 5 percentage points. By contrast to the first 5 bandwidths, at a bandwidth of 20, 4 of 10 placebos are statistically significant at the 1% level. The chances of finding 4 or more covariates out of 10 which are statistically significant at a 1% level by random chance is 2×10^{-6}. When we expand bandwidths, we do not include observations from counties outside of the county-pair. Therefore, the 20 mile bandwidth estimates are close to a fully county specification with local comparisons. In fact, sample sizes do not increase much from the 10 mile to the 20 miles bandwidth. This failure of placebos at large bandwidths suggest problems with cross-county comparisons even when those comparisons are local.

We now turn to the placebos for the midterm election estimates. Again, our treatment variable is the number of early voting days. The midterm placebos are not quite as clean as the presidential year placebos. Figure 1 shows that more counties experienced cuts in the number of days between the two midterm elections than between the presidential elections. Moreover, there is greater concentration of large reductions in early voting days in the Northeast of Ohio near Cleveland between the midterm years relative to the changes between the presidential years.

We see greater evidence of sorting at the border for the changes in the midterm years. Four of our ten placebos (share Independent, share Republican, share college or greater, and household median income) are statistically significant at a 10% level or less with a 0.1 bandwidth. Three of the four are statistically significant at a 5% level. Sorting at geographical boundaries has been noticed before. Bayer et al. (2007) note that there is often sorting at school district boundaries and school district boundaries change at county borders in Ohio. We also find this to be true though not for the border differences arising from the presidential year changes. This sorting largely disappears by a one-mile bandwidth but then increases again as block sizes increase above a one-mile bandwidth. In particular, the share of the population with a college degree tends to be lower and the share of Independents higher on the side of the border with more positive changes in numbers of early voting days reflecting that more rural areas expanded early voting while more urban areas contracted early voting. At a one mile bandwidth, none of the covariates are statistically different across the county borders. This is largely due to smaller mean differences rather than higher standard errors.

As a result, we use as a benchmark a 1-mile bandwidth for our estimation.

5.3 Bandwidth Robustness

We augment our discussion of bandwidth selection by placebo from the prior section by showing how our estimates change as we change the bandwidth. In Table 5, we show these estimates using samples pooling across elections as well as separately for presidential and midterm elections. The estimates for presidential years are remarkably stable across bandwidths. This reflects an absence of endogeneity bias as seen in the stability of covariate balance across bandwidths for the changes in days of early voting. However, it also reflects stability of the treatment effects across bandwidths. Across 7 different bandwidths ranging from 0.1 miles to 10 miles, the estimates range from 0.1891 per day (bandwidth = 0.5) to 0.2411 (bandwidth = 1). Thus the range across these 7 bandwidths is less than 27.5% of the baseline estimate. The 20 mile bandwidth is an outlier at 0.3264. This is over 35% above our benchmark estimate and over 72.5% above our minimal estimate. The lack of alignment of the 20 square mile bandwidth estimate with the other bandwidth estimates is likely driven by endogeneity at larger bandwidths which is reflected in the failure of a much larger set of placebos at this highest bandwidth. However, we cannot rule out the covariate imbalance does not lead to endogeneity bias and the change in the coefficients thus reflects heterogeneity in the impact for large bandwidths.

The midterm election estimates are, by contrast, less stable and none are statistically significant across bandwidths. The 0.1 mile and 3 mile bandwidths both yield negative estimates though with large standard errors. The other estimates are positive, ranging from 0.0337 to 0.1665 with a benchmark estimate of 0.1348 at a 1-mile bandwidth. Near uniformly, midterm election standard errors are larger than presidential election standard errors. Nonetheless, five of the bandwidths yield estimates that are within 0.07 of the benchmark estimates.

5.4 Party Effects

Typically the Democratic party has fought to expand early voting and the Republican party has fought to reduce it (Biggers and Hanmer, 2015). We now ask whether the parties are acting in a way which is consistent with their own interest. Of course, parties acting in their own interest may also be ideologically motivated. In this section, we will estimate the partisan impacts of early voting expansion and contraction for Democrats, Republicans, and Independents. To be clear, we are not estimating the causal impact of party on the treatment effect of early voting expansion. Party preferences are correlated with gender, race, education and many other determinants of political preferences. We do not try to isolate the pure impact of party. However, the differential impact by party (and age and gender) is of great importance both politically and legally. We thus focus on estimation of differential impacts by party (and in other sections, by age and gender).

In order to estimate early voting impacts by partisan affiliation, we first measure partisanship at the individual level. For those who have participated in a primary, we record their partisanship as the party whose primary they most recently voted in. For those who have never voted in a primary or for the very small number of individuals who have most recently voted in a third party primary, we record them as Independents. This means that we record anyone who turned 18 after 2008 as an Independent. This is a small fraction of the total electorate since, by first differencing within election-type, we exclude anyone from the sample who was too young to vote for the first time in the 2010 general election. We also consider estimates on a sample of those who turned 18 by the year 2000 and thus had greater chance to declare partisan leanings through primary participation by the year 2008. We consider this second sample our preferred one due to better measurement of partisanship. We then separately estimate the impact of an additional day of early voting upon voter turnout for Democrats, Republicans and Independents. We do this for midterm elections, for presidential elections and for the two election types combined. Our results are reported in Table 6.

We regress change in turnout within election-type on change in days, controlling for election-type$\times$block fixed effects. We also regress on change in early voting interacted with a dummy for Democrat as well as a dummy for Independent. The baseline change in days coefficient can, therefore, be interpreted as the effect for Republicans whereas the other two coefficients reflect the additional effects upon Democrats and Independents.

The restricted sample of those who were 18 by 2000 is approximately 10.6% smaller for the presidential elections and 13.2% smaller for midterm elections. The rank order across partisan leaning of the coefficients for an election type is the same in the restricted and full samples. The differences in estimates across the sample range from quite to small (0.0083 for Democrats in the pooled sample) to somewhat sizable (-0.1085 for Republicans in presidential elections). In this section, we focus upon the estimates on the restricted sample of those who had turned 18 by the year 2000.

Pooled, there are moderate-sized effects for Republicans, larger for Independents, and substantially larger for Democrats: approximately +0.0716 percentage points per day for Republicans, +0.2000 for Independents and +0.2918 for Democrats. The effect for Republicans is not statistically distinguishable from zero at conventional levels. However, the estimate for Independents is statistically separable from the estimate for Republicans with a p-value less than 7%. The effect for Democrats is statistically different from the Republican effect with above a 99% level of confidence. These estimates show that increases in early voting turns out more Democrats, more Republicans, and more Independents. However, it differentially benefits Democrats at a 4:1 ratio. These results show that the positions generally advocated by the parties are consistent with their electoral advantage.

The pooled effects, while interesting, cloak important heterogeneity by election type and that heterogeneity is very informative about who the marginal voters are in the midterm elections versus the presidential elections. An additional day of early voting is estimated to have virtually no effect on Republican turnout in presidential elections. The coefficient is 0.0002. The coefficient for Democrats slightly more than 0.1 higher and is statistically different with greater than a 99% level of confidence. The effect for Independents is extremely large at 0.5777. The large size of the impact upon Independents underscores that Independents are more weakly attached to politics, and in presidential elections, increasing the availability of voting has a large impact. The way we measure Independents is by their participation in primaries. This is the only measure available to us because Ohio is an open primary state. Having said that, our measures of Democrats, Republicans, and Independents roughly correspond to what is found in closed primary states such as Florida, North Carolina and California. If we view Independents as more politically neutral, then early voting has a depolarizing impact upon the vote in presidential elections.

The impact of early voting on partisan turnout has very different consequences in midterm elections than it does in presidential elections, underscoring the different composition of the electorate and the difference in the voters who are on the margins of participation in the two elections. Turnout in presidential elections is often around 50% higher than in midterm elections. This substantial difference is consequential for our estimates. Those who vote in midterm elections tend to be more attached to politics and more partisan.[14] In the third set of columns of Table 6, we show impacts upon the three groups during midterm elections. The impact upon Republicans is a statistically insignificant +0.1133. The effect on Independents is statistically different with a p-value of 1% and is lower by 0.1870. In fact, the overall estimated effect for Independents is negative. It is -0.0737 though it is not statistically distinguishable from zero. The impact on Democrats, by contrast, is 0.2629 larger than that for Republicans and is statistically different from the effect upon Republicans (and thus also from zero) with a 99% level of confidence.

Republicans seem to be more reliable voters. Republican vote shares are higher in midterm elections which are less salient for most Americans. Most Democrats and Republicans turn out for presidential elections. The marginal voters are thus Independents who are more politically indifferent in presidential elections. Easing access to voting thus largely impacts

[14] We report turnout shares by partisan leaning for the 2008, 2010, 2012, and 2014 elections in appendix Table A.6.

Independents during presidential election years. By contrast, during midterms, Independents participate at substantially lower levels and are largely not on the margins of participation. Early voting in midterm elections largely benefits Democrats.

There are three caveats which limit the interpretation of our estimates on differential effects by partisan leaning as effects upon the partisan vote share. First, we do not know that those who have voted in a party's primary will vote for that party in the general election. Second, we do not know how Independents vote. However, as shown in Table A.5, the correlation between our measure of Democratic vote share and the precinct-level vote share is 0.571 for 2008 and 0.658 for 2010. The correlation coefficients don't vary much by party or year conditional upon election type. The correlations are surprisingly high given that different people turn out from election to election. Finally, in order to compute partisan vote share impacts, we need to weight Republicans and Democrats by their voter registration shares. We do this in Section 6. Overall, given the very high correlation between partisanship and voting at the precinct level, we do think we can use our causal estimates by partisan affiliation to compute the partisan vote share impacts of early voting expansion.

5.5 Effects by Age

The heterogeneity in the effect of early voting expansion by partisan affiliation is interesting in large part because it is informative about the impact on the partisan vote share. We next turn towards estimation of differential effects by age. These estimates are interesting not only inherently but also because they are informative about who the marginal voter is and what that tells us about the costs and benefits of voter turnout. We next estimate the heterogeneity in the effect of early voting expansion by age. Age heterogeneity tells us about the age profile of the marginal voter and thus about the life cycle determinants of turnout.

We use our main identification border discontinuity design strategy to estimate the effect of an additional day of early voting by age. Since there are not many registered voters of a given age within a one square mile block, we group individuals into bins by five year age groups starting with the group 18-22. Each group is centered around a multiple of 5: 20, 25, 30, etc, The final group we use is the one centered around 75 years of age. After the 75 year old group, the numbers become too thin to estimate effects upon. We estimate the coefficients jointly in one regression where we regress changes in turnout within election type on changes in early voting days, controlling for geographical block $\times$ age group $\times$ election type fixed effects.

We show pooled estimates in Figure 3. We list the estimated treatment effect for a group on the y-axis and the median age of the age group on the x-axis. We do not show effects broken down by election type. However, the presidential effects, while similar to the overall effects as well as the midterm effects, are slightly flatter. Again, this suggests that voters in midterm elections are more committed voters who are less impacted by early voting availability and this is also true differentially across age groups.

The first thing which we note is that the effects are positive for 11 of the 12 age groups. This would happen by random chance if the estimates were independent across age group with below a 0.03 percent probability (i.e. less than 0.0003). We see this as additional proof of the positivity of the average effect of additional early voting days.

Second, we note that all age group pairs have overlapping 95% confidence intervals. We do not have the statistical power to differentiate the heterogeneity of effect by age group. We also estimate effects solely using cross-county variation. We present this graph in Figure A.3. The effects show a similar age pattern but are more pronounced. In Figure 3, the four age groups with the smallest estimated effects are the two lowest age groups and the two highest age groups. The highest effects are for individuals with intermediate ages who often have children and also often work. Effects increase between the 25 age group (23-27 year olds) and the 30 age group (28-32 year olds) and decrease from the 60 age group (57-62 year olds) to the 65 age group (63-67 year olds). The first of these is the time of the beginning of child-bearing. The median age for first child is 25 for women and 27 for men in the State of Ohio. Thus most people in the 25 age group do not yet have children and if they do, they have babies or young toddlers who are easier to take to a voting booth. In the 30 year old group, by contrast, the effects are larger potentially reflecting the role of greater child care constraints. This is then followed by a sizable drop from the 60 age group to the 65 age group which surrounds the median as well as modal age of retirement.

Our age results suggest that the costs of voting are born particularly by those who have limited time: the working and those with kids.

5.6 Effects by Gender

We also estimate the impact of early voting expansion by gender. In contrast to partisanship and age, which Ohio voting records measure directly, Ohio does not record gender or sex on voter registration forms. Therefore, we only indirectly measure gender. We impute gender probabilities for each individual in our data set by matching first names by year of birth to lists of first names by gender and year of birth from the Social Security Administration as described in Section (3.). For uncommon first names (those with less than five individuals of a given sex born in a given year for both genders), we cannot match them to the social security files and we drop them. For the remaining sample, we estimate equation (5). We do this in two ways. First we interact our treatment variable with the estimated probability that an individual is female. Second, we create a binary variable taking on the value of 1 if the probability of being female is at least 95% and 0 if the probability of being female is less than or equal to 5%. For this second specification, we drop all observations with a probability of being female in between 5% and 95%. As shown in Table 7, using the binary variable drops the sample size by only 4.5%, reflecting that most names are either definitively male or definitively female.

In addition to estimating models with continuous and binary gender measures, we also estimate the impacts using gender imputed by national Social Security lists as well as State of Ohio Social Security lists. The State of Ohio lists are smaller and thus fewer names can be matched. However, if gender specificity in naming varies by state, the Ohio data is probably more accurate for the Ohio voting population. Using the state lists lowers the sample size by 4.2% for the continuous measure of gender and 2.8% for the binary measure of gender. In the text, we report estimates using the continuous measure of gender and from the national sample. However, all estimates of differential effects by gender are very similar. In all specifications, switching from national to state or from continuous to binary impacts the estimated coefficient by less than 5%.

We find robust evidence that there is a differential effect across the genders. For men, an additional day of early voting increases turnout by a statistically insignificant 0.1097 percentage points. There is an additional 0.1261 impact for women. The differential is statistically significant with more than a 99% level of confidence. This means that overall, an additional day of early voting increases turnout by women by 0.2358 percentage points. The standard error for the differential effect on women is uniformly 75%-80% smaller than for the effect on men. This we attribute to differences between the genders in voting patterns being small relative to differences across individuals even in neighboring counties.

Much of the differential effect across genders is for midterm elections. However, the effect of additional early voting is larger for women than for men in both midterm and presidential elections and is statistically significantly different in both as well. For men, the effect in a presidential election of an additional day of early voting is 0.2013 percentage points and it is statistically significant with more than a 90% level of confidence. The differential effect for women is 0.0428 higher (slightly more than 20% higher) and is statistically significant at a 99% level. The effect of an additional early voting day for men in presidential elections is very small and statistically insignificant at conventional levels. It is only 0.0314. However, the additional impact upon women, equal to 0.1619, is more than five times the size.

Overall, early voting laws have a robust larger effect on women than men. This is more true for midterm than presidential elections but is present in both election types.

5.7 Effects by Age and Gender

We also show estimates by age group broken down by gender. Since we have small numbers of men and women respectively in many of our geographical cells, we estimate treatment effects using a two way county-time fixed effects model with days interacted with age group. We estimate for men and women separately. Our results are in appendix Figure A.4. We estimate pooled across election in our first row of graphs and then separately for presidential and midterm elections respectively.

The pooled figures look like a muted version of the presidential election figures. In general, we do not see differential patterns by age across males and females. For midterm elections, we see no patterns at all. This is consistent with the participants in midterm elections being more attached to politics and thus less influenced by the life cycle. For presidential elections, we see low estimates for the youngest age group followed by large and declining estimates. Both for men and women, estimates are highest for those in their late 20s and 30s. These estimates are not as well identified as the prior estimates by gender alone and by age alone. However, they are suggestive that life cycle effects are strong, that they are present both for men and women, and that they are largely present in general not midterm elections.

5.8 Effects by Type of Early Voting Day

Having shown heterogeneity of effects across different types of voters (by partisanship, by gender and by age) as well as by type of election (presidential and midterm), we now look at the differential impact by type of early voting day. Appendix Figure A.1 shows the changes in total hours of early voting, number of weekend days, number of Sundays, number of days of same day registration, number of weekend days with same day registration, and number of days for presidential elections. Appendix Figure A.2 is similar to Figure A.1 but for midterm elections. Most counties saw expansions in number of weekend days as well as number of Sundays both for presidential and midterm elections. The counties which saw declines in weekend or Sunday early voting were the large urban counties. Same day registration days are early voting days more than 28 days before the election when people could still register to vote and then actually vote at the same early voting polling station. Only two counties saw increases in same day registration between 2008 and 2012 and only one county saw no decline between 2010 and 2014. All other counties saw reductions in early voting both for presidential and midterm elections. For presidential elections, the larger declines occurred in the more urban areas. However, for the midterm elections, most counties saw between 5 and 8 days fewer same day registration days as same day registration was eliminated. Since we have the exact hours that polling stations were open on each day, we also computed the number of days that polling stations were open until 7 PM or later (which we term days open late). Most counties saw an increase in days open late. The only exceptions are 4 counties with large, urban populations. For the midterm elections, most counties saw no changes in days open late again with larger, more urban counties seeing reductions.

We estimate heterogeneous effects by type of treatment in Table 8. We look at multiple types of days: week days versus weekend days, same day registration days versus non-registration days, and days open until 7 p.m. or later versus days closing before 7 p.m. We even show differential impacts of Saturdays and Sundays. In all cases, we continue to find positive and statistically significant (at a 90% level of confidence or higher) effects of early voting expansion. Moreover, the estimate magnitudes are relatively stable. The effects for days or weekdays all range from between +0.2 percentage points per day and +0.3 percentage points per day.

We do not, however, find a differential impact of weekend days, same day registration days, or days open late. The coefficients on the differential effects are all small to moderate and very far from statistical significance. In column 4 of Table 8, we do find a large differential effect of Sunday expansion: +0.3231 and a null effect of Saturday expansion: -0.0201. Neither are statistically significantly different from zero. In the case of Sundays, this is due to the large standard errors which is unsurprising given the limited variation that the identification of these estimates comes from.

In appendix Table A.3 and appendix Table A.4, we present our heterogeneity of effect by type of day broken down by type of election. The effects in presidential elections are similar to the overall effects. In general, the effects are slightly larger. Also, both Saturday and Sunday effects are larger than weekday expansions. Also the effect for same day registration is almost 50% higher than that for non-registration days. This may reflect that for marginal voters in presidential elections, who are more weakly attached to voting, the convenience of weekend voting and of same day registration matters more than for the more attached voters who are on the margins of participation during midterm elections.

The main differences between the heterogeneity in the effects during midterm elections, which are presented in Table A.4, and the heterogeneity in the effects during presidential elections are a large negative though statistically insignificant effect of Saturday voting. The magnitude is sizable: -0.2148. However, the standard errors are very large: 0.4258. The Sunday impact, however, remains larger than the weekday effect though the standard errors are also higher and not statistically distinguishable from zero much less from the weekday effect. The only other interesting result is that most of the day effect seems to come from early registration days in the midterm elections. Neither same day registration days nor non-registration days have an effect that is statistically separable from zero.

5.9 Non-Linear Treatment Effects

The average impact of an additional day of early voting is 0.1938 percentage points of additional turnout. That number is 0.2411 for presidential elections and 0.1348 for midterm elections. However, some counties saw large contractions in early voting, other large expansions and yet others very modest changes or even no change at all. Moreover, some counties increased from low levels of early voting availability while others reduced from very high levels. In this section, we test whether turnout is linear in the number of early voting days. We do this by adding a quadratic term to our baseline linear specification estimating the impact of an additional day of early voting. We note that we first difference the data at the individual level within election type after computing the quadratic term so that our model is quadratic in the number of early voting days rather than in the change in early voting days. The estimation is given by equation (6). We show these results in columns 1, 3, and 6 of Table 9. In all three cases, the quadratic terms are small and statistically insignificant. Our estimates that even at 35 days of early voting (the largest observed in our sample), the linear component of the effect of early voting expansion is more than twice the size of the quadratic component. Moreover, for the pooled and presidential estimates, the coefficients on the linear term are similar to the baseline linear specification estimates. However, the coefficient on midterm days does near double from 0.1348 per day to 0.2473 additional percentage points of turnout per additional day of early voting, though it is not statistically distinguishable from the old one with a 90% level of confidence.

The few urban counties that saw large reductions in the number of days of early voting also saw reductions in same day registration. It is possible that the effect of reducing large numbers of days captures the effect of reducing or eliminating same day registration. Non-linear impacts of days and impacts of same day registration may confound each other. As a result, we regress turnout changes on a linear change in days term, a quadratic change in days term and a change in days of same day registration term. The coefficients on same day registration are very small and statistically insignificant. Moreover, they do have a negligible impact upon both the linear and quadratic term coefficients for the impact of number of early voting days.

It is difficult to separate out non-linear effects from additional days versus effects of same-day registration. The places that experienced very large reductions in days were also places that eliminated same-day registration and so it is difficult to differentiate between the two. In this section, we attempt to do exactly that by regressing simultaneously on both and by also including higher order terms. We also look to see whether large increases or decreases in numbers of days that are not accompanied by a change in the number of days with same-day registration.

6 Aggregate Effects

In this section, we use our geographic fixed effect estimates on turnout in presidential and midterm elections respectively to simulate the impact of the Kasich reform as well as three different benchmark scenarios for national early voting legislation. In the case of our national simulation, this is made under the maintained assumption that our estimates from Ohio are externally valid.

6.1 Ohio Impacts

We use the estimates by election type and party to estimate the impact on voter turnout and on the Democratic vote share of the Kasich reform for 2012 and 2014.[15] For turnout effects, we multiply the estimated effect by the number of registered voters in each county and then multiply by the change in the number of days. We then add up across counties to get the total turnout effect. We express this in the equation below:

$$\mathrm{T}_e = \sum_c \beta_e \mu_{ce} R_c \tag{7}$$

where β_e is our estimated election-type-specific effect per day of early voting on turnout, μ_{ce} is the change in the number of days of early voting available in a county by election-type (i.e. in 2012 or 2014 respectively), and R_c is the number of registered voters in a county. We find that though many counties increased early voting days between 2008 and 2012, large reductions in dense urban counties like Cuyahoga, Franklin, and Summit more than outweighed the early voting expansions. The net effect was to reduce total voting by 45,225 votes in the 2012 election. For the midterm elections, the changes in early voting days were more uniformly negative. However, the effect size was smaller. Nonetheless, the greater uniformity in early voting contraction explains the larger overall effect; our computations suggest that electoral law changes from 2010 to 2014 were responsible for a turnout contraction of 61,642 votes. Since the amount of the contraction in early voting was larger and the turnout was substantially lower in the midterm elections of 2014 compared to the presidential election of 2012, the percent of the contraction relative to total turnout was about twice as high as for the presidential election. The total contraction was 1.96% in 2014 and 0.82% in 2012.

We now look at the impact on the democratic vote share. In order to do this, though we have estimated the impact of early voting expansion by party, we face two main problems. First, we do not know that all Democrats vote Democratic and all Republicans vote

[15] We consider the impact of changes between 2008 and 2012 on the 2012 general election and changes between 2010 and 2014 for the 2014 general election.

Republican. Second, we don't know who Independents vote for. We proxy the probability of voting for the Democrats using the precinct-level correlation between a partisan group's registration share and the aggregate Democratic vote share. We show these correlations by year and party in appendix Table A.5. The correlation coefficients are decently stable across elections. The Republican registration share correlation with the Democratic vote share ranges between -0.769 and -0.872. The Democratic registration share is significantly lower mainly because Independents lean heavily Democrat. The correlation ranges from 0.548 to 0.658. The Independent share is positively correlated with the Democratic vote share. It is also more unstable. The correlation for the Independent share ranges from 0.297 to 0.480.

We then compute the net vote change for Democrats by Democrats, Republicans, and Independents. We start by computing the expected increase in votes for Democrats per registered Democratic primary voter. This is obtained by multiplying the effect of an additional early voting day on a registrant of party p during election type e by the probability that a registrant of party p votes for the Democrats. We denote by β_{pe} the turnout effect for registrants with party p registrants in election type e and by ρ_{pe} the correlation between registration shares in a precinct and the Democratic vote share in the precinct. We then multiply this by the number of registered party p voters in county c: ω_{pc} . Altogether, this gives us the expected net change in Democratic votes from a one day increase in early voting in county c and election type e. Finally, we multiply this by the net change in days of early voting in the county which we denote by μ_{ce}. The expected increase in votes for Democrats in county c during election type e is thus $\beta_{pe}\rho_{pe}\mu_{ce}\omega_{pce}$. Our equation for the net change in Democratic votes, T_{pe}, is given by summing over all counties:

$$T_{pe} = \sum_{c} \beta_{pe}\rho_{pe}\mu_{ce}\omega_{pce}.$$

We then compute the total effect on Democratic votes by adding the effect on Democrats to that on Independents as well as the effect on Republicans. We then divide by total votes in the election to get the impact of the Democratic vote share:

$$\Delta V_{pe} = \frac{T_{De} + T_{Re} + T_{Ie}}{Turn_e}$$

where **D** denotes Democrat, **I** denotes Independent, **R** denotes Republican, and $Turn_e$ is the actual total election turnout.

On net, our estimates imply an increase in the Republican vote share of 0.36 percentage points in the 2012 presidential election and an increase in the Republican vote share of 0.51 percentage points in the 2014 midterm election. This may seem small given the magnitude of the contractions in Democratic counties combined with the fact that some Republican counties actually saw increases in days. However, a few things must be kept in mind. First, the change in the number of days matters more than the distribution of changes over counties. The reason for this is twofold. First, the effects on Republicans are small. Therefore, the effects upon the Democratic vote share largely rely upon the magnitude of changes to Democrats and Independents. This is particularly true for presidential elections. In addition, the differences across counties in partisanship are modest. Going from the 25^{th} to 75^{th} percentile in Democratic share of registrants only increases the Democratic registrant share by 10 percentage points. Moreover, much of the overall effect is concentrated in the very large, urban counties which lean Democrat less heavily than the very rural areas lean Republican.

It is also somewhat surprising to note that the effect for the midterm election is larger than for the presidential election. This is mainly because the change in the number of days facing the average voter was much more negative in 2014 than it was in 2012. Taking a population-weighted average of the change in early voting days, the average registered citizen saw a cut of 2.88 days between 2008 and 2012. However, between 2010 and 2014, the average registered citizen saw a cut of 6.03 days.

Overall, the changes to early voting in Ohio had a positive though modest impact on the Republican vote share. The size of the cuts are consistent with the fact that, overall, the changes led to modest sized cuts.

6.2 Impacts on Federal Election Outcomes

We now simulate the effect of three potential national early voting laws. The first scenario is a national ban. The second scenario is a national mandate at 23 days of early voting. This is what the State of Ohio currently provides. Finally, we consider a third scenario with double Ohio's provision of early voting: 46 days. Minnesota has the most generous early voting in the country and it has 46 days of in-person early voting.

To simulate the impact of these three scenarios, we first compute the impact per additional day of early voting on the Democratic vote share. For each party and election type, we multiply the effect of an extra day of early voting on each group (Democrats, Independents, and Republicans) by the probability for each group of voting Democrat; we then multiply this product by the share of each group in the registered population.[16] We then sum across parties to get an effect for an election-type. We denote by Θ_e the total effect of an extra day

[16] We use the average across elections within an election type to compute the correlation coefficients and the group shares that we use in this equation.

of early voting in Ohio by election type:

$$\Theta_e = \sum_p \beta_{pe}\rho_{pe}s_{pe}$$

where β_{pe} is the effect of an extra day for members of party p in election type e, ρ_{pe} is the correlation between registration and voting for members of party p in election type e, and s_{pe} is the share of registrants from party p in election type e.[17] The effect of extended early voting on Democrats is higher during midterm elections than during presidential elections. However, since Independents are not marginal voters during midterm elections and since the impact of early voting on Independents during general elections is substantially larger than that on Democrats during midterm elections, the overall effect is larger for presidential than for midterm elections. This is true despite the fact that Independents are less likely to vote Democrat than registered Democrats. Θ_e is 0.0278 for midterm elections. This means that every additional day of early voting yields an additional 0.0278 percentage points to the two-party Democratic vote share. In other words, 36 additional days of early voting yields one percentage point for the Democrats. If these estimates are externally valid across states, this suggests that early voting in Minnesota has added more than 1 percentage point to the midterm election Democratic vote share from its extended early voting. Our calculations suggest that though Minnesota may have added over a percentage point to the Democratic vote share during midterm elections, few other states have. The overall effect for presidential elections is 3.56 times the size of the midterm effect. It is 0.0992 percentage points per day. This means that 10 days of additional early voting adds roughly a percentage point to the Democratic vote share.

We now move from computing the impact upon the two-party Democratic votes share of an additional day of early voting to the impact on the outcome of national elections of our three different national early voting law scenarios. We can compute the change in election outcome for chamber c, under scenario r and during year y. We express the outcome change as ΔO_{cry}. An outcome is the number of seats for House and Senate elections and number of electoral votes for presidential elections. We also denote by α_{csy} the change in early voting days for scenario c, state s, and year y.[18] $F(\alpha_{csy})$ is a function which takes on +1 if plurality in a state changes towards the Republicans, -1 if plurality in a state changes towards the Democrats, and 0 otherwise. Finally, we denote by E_{cs} the electoral votes in state s and chamber c. For House and Senate elections, the value of E_{cs} is 1. For presidential elections, the value of E_{cs} is equal to the electoral votes in the state.[19] The formula we use for computing

[17] We take ρ_{pe} and s_{pe} from our Ohio voter registration data.

[18] s denotes House district in the case that the chamber, c, is the House of Representatives.

[19] In the case of Maine and Nebraska, each electoral district decides its own elector and the remaining two

outcome changes for national elections is thus given by:

$$\Delta O_{cry} = \sum_{s} \Theta_e F(\alpha_{csy}) E_{cs}$$

We present the results of our predictions in Table 10. Overall, we see much larger effects on presidential elections than midterm elections. Impacts upon Independents are large in presidential elections where they are the marginal voters and they swing Democrat in their voting patterns. We find no impact in the Senate of moving to zero, 23 or 46 days of early voting in the 2014 midterm elections. This is because the two closest elections were North Carolina and Virginia. North Carolina, which the Republicans won with a 1.5 percentage point margin, was not close enough given the more modest impact of early voting in midterm elections. Virginia, where a Democrat won by 0.8 percentage point, had no early voting to contract in favor of the Republicans. In contrast, in 2012, we predict that one state, Nevada, would have swung towards the Democrats with 46 days and one state, North Dakota, towards the Republicans with the elimination of early voting. In 2016, we find no impact of getting rid of early voting in the Senate, one additional Senate seat for the Democrats (Pennsylvania) from a move to 23 days of early voting and three additional Senate seats (Missouri, Pennsylvania, and Wisconsin) with a change to 46 days of early voting. Although the New Hampshire Senate race in 2016 was the closest with a 0.14 percentage point margin of victory for the Democrat, New Hampshire has no early voting and thus there would have been no impact in New Hampshire of moving to a national early voting ban. A switch to a 46-day early voting law, however, would have led to a switch from a Republican to a Democratic majority in the Senate.

For House elections, the 2012 election was the one where early voting mattered the most. This is because of the very large number of close elections. Though we predict no impact of a national 23-day early voting law, we do predict an increase of 5 Republican seats from a national early voting ban and a swing of 10 seats to the Democrats from a national 46-day early voting law. Since there were few close House races in 2014 and 2016, a 46-day law would only induce a movement towards the Democrats of 2 seats in 2014 and 3 seats in 2016; an elimination of early voting in 2016 would shift 1 seat towards the Republicans but would have had no impact in 2014.

Finally, we consider the impact upon the 2012 and 2016 presidential elections. Whereas we find little impact on the 2012 election, we do find substantial impacts upon the 2016 election. We find a swing towards the Republicans of 10 electoral votes from the elimination

electors are decided by the plurality outcome in the state. For these two states, s indexes each of the electoral districts as well as the state.

of early voting as Minnesota would have switched to Republican. A national 23-day early voting law would also have turned Minnesota Republican in the 2016 presidential election but it also would have caused Florida, Michigan, Pennsylvania, and Wisconsin to switch to the Democrats. This is due to the small margins in these states and their relatively low levels of early voting. The net change would have been 65 electoral votes and would have swung the close 2016 presidential election to Hillary Clinton. A national 46-day early voting law would have eliminated the switch of Minnesota and thus would have resulted in a 75 electoral vote shift in favor of the Democrats.

7 Conclusion

In this paper, we estimate the impact of early voting upon voter turnout. We compare people within the same square mile block on opposite sides of county borders when Ohio Governor John Kasich passed laws homogenizing early voting across counties. We find that a day extra of early voting increases turnout by 0.19 percentage points. We additionally show evidence that those in child-rearing years and prime working years are particularly impacted by early voting availability. We further find that women react almost twice as strongly as men to additional early voting. We do not find strong differential responses to same day registration or to days where polls are open late. However, we do find a strong (though statistically insignificant) differential turnout response to Sunday voting. The methods we use for this paper are also well suited for looking at heterogeneity by race which is crucial for electoral law in the United States.[20]

We further find that effects are larger on Democrats than on Republicans and that effects on Independents are very large in presidential elections though not in midterm elections. We use our estimates on partisan impacts of early voting to simulate the impact of national early voting legislation and find that requiring all states to implement 46 days of early voting, as was the case in Minnesota during the 2016 presidential election, would have swung the outcome of both the Presidency and majority control of the Senate in the close 2016 elections.

We find that early voting expansion likely has a depolarizing effect on the electorate in presidential years when Independents are most impacted. Overall, our evidence demonstrates substantive electoral impacts of early voting on turnout, on partisan outcomes, and on the polarization of the electorate.

[20] We are currently working on matching Ohio birth records, which records race at the individual level, to our voter registration records.

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Table 1: Sample Means of Registered Ohio Voters by Change in Early Voting Days

	2008 - 2012 Changes in Number of Days			2010 - 2014 Changes in Number of Days		
	All	+ / -	Mean	All	+ / -	Mean
Black (%)	13.2	+	9.0	13.5	+	6.4
		-	19.6		-	22.5
Hispanic (%)	2.9	+	2.3	3.0	+	2.2
		-	3.9		-	4.0
White (%)	83.0	+	87.9	82.7	+	90.8
		-	75.4		-	72.3
Democrat (%)	30.4	+	27.4	28.6	+	26.4
		-	35.1		-	31.3
Independent (%)	43.1	+	42.4	46.6	+	44.7
		-	44.1		-	49.0
Republican (%)	26.5	+	30.2	24.8	+	28.9
		-	20.8		-	19.7
College Grad. (%)	25.2	+	22.5	25.0	+	20.9
		-	29.3		-	30.3
HS Dropout (%)	12.0	+	12.4	12.2	+	12.5
		-	11.5		-	11.8
Med. Household Income	53.7	+	52.8	53.4	+	51.5
		-	55.1		-	55.8
Age in 2008	44.6	+	45.1	42.9	+	43.7
		-	43.8		-	41.8
Distance to Early Voting Site	10.9	+	10.7	10.8	+	11.2
		-	11.1		-	10.2
Voted in 2008 (%)	86.2	+	86.4	86.2	+	85.9
		-	85.9		-	86.7
Voted in 2010 (%)	59.9	+	60.3	59.9	+	59.9
		-	59.2		-	59.8
Voted in 2012 (%)	76.3	+	76.7	76.3	+	76.1
		-	75.6		-	76.6
Voted in 2014 (%)	43.7	+	45.0	40.9	+	41.9
		-	41.9		-	39.7
Observations	6559589	+	3998136	7597048	+	4257198
		-	2561453		-	3339850

Notes: Each row reports means of one variable indicated by the first column. Column "All" reports the sample means of Ohio residents who registered and were eligible to vote in 2008 (for 2008-2012 changes) or 2010 (for 2010-2014 changes). Column "+ / -" indicates a sub-sample of counties with above (+) or below (-) mean changes of early voting days between 2008 and 2012 or between 2010 and 2014. Variable "Med. Household Income" is the median household income of a registered voter's Census block group in thousands of dollars. "Distance to Early Vote Site" is measured in miles. "Age in 2008" is measured in years as of the general election day in 2008. All other variables are in percentage points.

Table 2: Early Voting Effects: Main Table

	Full Sample		1-Mile Border Sample	
	(1)	(2)	(3)	(4)
	Both Presidential and Midterm Elections			
Number of Days	0.1618***	0.1685***	0.1488***	0.1938**
	(0.0514)	(0.0567)	(0.0431)	(0.0809)
Observations	24,629,989	24,629,989	1,188,288	1,188,288
	Presidential Elections			
Number of Days	0.0549	0.2011**	0.1338***	0.2411**
	(0.0366)	(0.0769)	(0.0348)	(0.1142)
Observations	11,532,916	11,532,916	562,616	562,616
	Midterm Elections			
Number of Days	0.2899***	0.1476	0.1632	0.1348
	(0.0946)	(0.1235)	(0.0987)	(0.1058)
Observations	13,097,073	13,097,073	625,672	625,672
Year Fixed Effects	Y	Y	Y	
County Fixed Effects	Y			
Individual Fixed Effects		Y	Y^-	Y^-
Year-specific Geo Fixed Effects				Y

Notes: Each cell reports one coefficient estimate of an OLS regression. In all regressions, the dependent variable is a binary variable equal to 100 if a registered voter turns out to vote in a general election; and zero otherwise. The rows indicate the key explanatory variable in each regression. In Column (1) and Column (2), the samples include the full sample of registered Ohio voters and the specifications include county fixed effects and individual fixed effects respectively. In Column (3) and Column (4), the samples include individuals living within one mile of a county border. Both specifications include individual fixed effects estimated taking 4-year differences of the regression equations. The differencing is carried out within the same type of elections, i.e. presidential or midterm and is motivated by easing computational burden. Y^- indicates the allowance of individual fixed effects through the 4-year differencing. Column (4) additionally includes 1×1 mile geographic fixed effects. All samples include four general elections from 2008 to 2014 and all specifications include year (election) fixed effects. Standard errors in Column (1) to (3) regressions are clustered by county. Standard errors in Column (4) are clustered two-way by county and by county-border segment.
$* \, p < 0.10$; $** \, p < 0.05$; $* * * \, p < 0.01$.

Table 3: Tests of Covariates Balance by Area of Geo-Year Fixed Effects (Presidential Elections; Number of Days)

	0.1	0.5	1	1.5	2	3	5	10	20
Independent	0.1951	0.3617*	0.1001	0.0741	0.1919	-0.1278	0.0644	-0.0160	-0.1980
	(0.2575)	(0.2063)	(0.2114)	(0.2189)	(0.3610)	(0.1761)	(0.3458)	(0.2774)	(0.3386)
Republican	-0.1161	-0.2756*	0.0057	0.0301	-0.0801	0.2405	0.2076	0.4693	0.9588***
	(0.2230)	(0.1565)	(0.1584)	(0.1909)	(0.3053)	(0.1913)	(0.3425)	(0.2894)	(0.3304)
Democrat	-0.1072	-0.2653*	-0.2164	-0.1950	-0.2563	-0.1463	-0.3889**	-0.5273**	-0.7830***
	(0.2171)	(0.1528)	(0.1531)	(0.1402)	(0.2167)	(0.1505)	(0.1896)	(0.2413)	(0.2718)
Age in 2008	-0.0522	-0.0067	0.0432	0.0419	0.0381	0.0715	0.0578	0.0674	0.1312*
	(0.1142)	(0.0633)	(0.0616)	(0.0859)	(0.0734)	(0.0710)	(0.0891)	(0.0655)	(0.0712)
Female	0.2655*	-0.0359	-0.0197	-0.0001	-0.0469	-0.0700	-0.0718	-0.0886	-0.0833
	(0.1463)	(0.0514)	(0.0453)	(0.0515)	(0.0419)	(0.0648)	(0.0757)	(0.0620)	(0.0634)
Distance to	0.0009	0.0043	0.0047	0.0119	0.0059	0.0201	0.0133	0.0448	0.2183
Early Voting Site	(0.0006)	(0.0045)	(0.0103)	(0.0189)	(0.0212)	(0.0336)	(0.0619)	(0.0800)	(0.1552)
College Grad. (%)	-0.4123	-0.3692	-0.1024	-0.1919	-0.5132	-0.2238	-0.2867	-0.1767	-0.4537
	(0.2816)	(0.2370)	(0.2412)	(0.2356)	(0.3732)	(0.2899)	(0.3734)	(0.3171)	(0.3261)
HS Dropout (%)	0.1548	0.0971	0.0571	0.1029	0.1561	0.1189	0.1642	0.0486	-0.0497
	(0.1330)	(0.1198)	(0.1340)	(0.1209)	(0.1353)	(0.1654)	(0.1553)	(0.1455)	(0.1134)
Med. HH. Income	-0.3108	-0.3850	0.1453	0.2091	-0.5721	0.3032	0.1885	0.3870	0.8964
	(0.4060)	(0.4307)	(0.5735)	(0.6041)	(0.6183)	(0.5804)	(0.8160)	(0.6195)	(0.6434)
Hispanic (%)	-0.0071	0.0087	-0.0414	-0.0273	-0.0151	-0.0280	-0.0300	-0.0476	-0.1091*
	(0.0174)	(0.0241)	(0.0347)	(0.0323)	(0.0411)	(0.0321)	(0.0546)	(0.0488)	(0.0588)
Black (%)	-0.0781	0.0705	-0.0400	-0.1850	0.1673	-0.3697	-0.3329	-0.6001	-1.4486***
	(0.2155)	(0.1696)	(0.1203)	(0.3942)	(0.2302)	(0.3820)	(0.4316)	(0.4009)	(0.4785)
White (%)	0.0882	-0.0827	0.0921	0.2189	-0.0227	0.4051	0.4201	0.7211*	1.6438***
	(0.2536)	(0.2020)	(0.1513)	(0.4097)	(0.2629)	(0.3952)	(0.4541)	(0.4136)	(0.4845)
Observations	55586	274718	562616	855835	1141271	1703645	2669117	4260076	4322569

Notes: Each cell reports the estimated coefficient of the number of early voting days in an OLS regression. In all regressions, the dependent variables are indicated by the first column. Each row has the same dependent variable as indicated in the first column. “Med. Household Income” is the median household income of a registered voter’s Census block group in thousands of dollars. “Distance to Early Vote Site” is measured in miles. “Age in 2008” in measured in years as of the general election day in 2008. “Democrat” is a binary variable equal to 100 if the most recent primary the registered participated in since 2000 is a democratic primary and is equal to zero otherwise. “Republican” is similarly defined for Republican primary participation. “Independent” is defined as registered voters who have not voted in the primary of either party between 2000 and 2008. “Female” is the percentages of females who, according to the Social Security Administration, were born in the year of birth of the registrant and share the registrant’s first name. Other demographic variables are measured in percentages at the block group level. The sample is restricted to presidential elections in 2008 and 2012, and individuals living within k miles of a county border, where k is indicated by the column headings. Standard errors are clustered two-way by county and by county-border.

Table 4: Tests of Covariates Balance by Area of Geo-Year Fixed Effects (Midterm Elections; Number of Days)

	0.1	0.5	1	1.5	2	3	5	10	20
Independent	0.6233**	0.7340***	0.2988	0.4042**	0.4645	0.2164	0.4674**	0.4447**	0.2832
	(0.3049)	(0.2241)	(0.2285)	(0.1964)	(0.3418)	(0.1683)	(0.2120)	(0.2071)	(0.2749)
Republican	-0.5410**	-0.5855***	-0.2193	-0.2346	-0.3577	-0.0782	-0.3028	-0.0373	0.3166
	(0.2255)	(0.1481)	(0.1700)	(0.2131)	(0.3131)	(0.2972)	(0.3343)	(0.3828)	(0.4159)
Democrat	-0.0774	-0.3453	-0.2129	-0.2862	-0.2962	-0.2187	-0.3589	-0.5893*	-0.7027*
	(0.1962)	(0.2196)	(0.2335)	(0.2266)	(0.2470)	(0.2455)	(0.2704)	(0.3065)	(0.3637)
Age in 2008	-0.1989	-0.0305	0.0398	0.0102	0.0310	0.0483	0.0104	0.0186	0.0233
	(0.1904)	(0.0800)	(0.0777)	(0.0781)	(0.0654)	(0.0590)	(0.0518)	(0.0451)	(0.0683)
Female	0.0797	-0.0242	-0.0479	-0.0184	-0.0179	-0.0398	0.0054	-0.0311	-0.0059
	(0.1772)	(0.0527)	(0.0301)	(0.0544)	(0.0405)	(0.1018)	(0.0924)	(0.0783)	(0.0693)
Distance to	0.0015*	0.0057	0.0169	0.0345*	0.0231	0.0489	0.0905	0.2149**	0.3302**
Early Voting Site	(0.0008)	(0.0065)	(0.0137)	(0.0208)	(0.0297)	(0.0424)	(0.0715)	(0.1045)	(0.1681)
College Grad. (%)	-0.7053**	-0.7622***	-0.3200	-0.5722**	-0.7422*	-0.7311**	-0.8694***	-0.6312**	-0.6613*
	(0.3212)	(0.2651)	(0.3344)	(0.2558)	(0.4232)	(0.3685)	(0.3090)	(0.2772)	(0.3748)
HS Dropout (%)	0.1359	0.1705	0.0992	0.1574	0.1579	0.1510	0.2586*	0.0858	-0.0644
	(0.1559)	(0.1288)	(0.1140)	(0.1247)	(0.1551)	(0.1590)	(0.1404)	(0.1388)	(0.1406)
Med. HH. Income	-0.8792**	-1.2678**	-0.6455	-0.7015	-1.2525**	-0.6005	-1.0085*	-0.4508	0.0433
	(0.4215)	(0.5435)	(0.5678)	(0.4689)	(0.6131)	(0.4636)	(0.5250)	(0.4731)	(0.4888)
Hispanic (%)	-0.0447	-0.0095	-0.0560	-0.0377	-0.0371	-0.0464	-0.0170	-0.0443	-0.0724
	(0.0464)	(0.0356)	(0.0597)	(0.0292)	(0.0454)	(0.0325)	(0.0280)	(0.0299)	(0.0661)
Black (%)	-0.1639	0.0076	-0.1232	-0.4538	0.0957	-0.5730	-0.3711	-0.5223	-1.1362
	(0.3164)	(0.2019)	(0.1265)	(0.4299)	(0.1747)	(0.4385)	(0.3358)	(0.5386)	(0.7072)
White (%)	0.2531	0.0201	0.1592	0.5166	0.0263	0.6755	0.5054	0.6425	1.2909*
	(0.3474)	(0.2378)	(0.1715)	(0.4356)	(0.2104)	(0.4271)	(0.3452)	(0.5330)	(0.7277)
Observations	62112	306255	625672	950826	1267210	1888314	2953737	4716425	4787724

Notes: Each cell reports the estimated coefficient of the number of early voting days in an OLS regression. In all regressions, the dependent variables are indicated by the first column. Each row has the same dependent variable as indicated in the first column. "Med. Household Income" is the median household income of a registered voters' Census block group in thousands of dollars. "Distance to Early Vote Site" is measured in miles. "Age in 2008" in measured in years as of the general election day in 2008. "Democrat" is a binary variable equal to 100 if the registered voter voted in a democratic primary previously and zero otherwise. "Republican" is similarly defined for Republican primary votings. "Independent" are registered voters who had never voted in the primary of either party from 2000 to 2010. "Female" is the percentages of females who were born in the same year and shared the same first name as the individual. Other demographic variables are measured in percentages at the block group level. The sample is restricted to midterm elections in 2010 and 2014, and individuals living within k miles of county border, where k is indicated by the column headings. Standard errors are clustered two-way by county and by county-border.

Table 5: The Turnout Effects of Early Voting Laws: by Area of Geo-Year Fixed Effects

	0.1	0.5	1	1.5	2	3	5	10	20
	Both Presidential and Midterm Elections								
Number of Days	0.0182	0.1791**	0.1938**	0.1320	0.1848**	0.0569	0.1532	0.1657**	0.2282***
	(0.1099)	(0.0821)	(0.0809)	(0.0904)	(0.0906)	(0.1009)	(0.0988)	(0.0764)	(0.0820)
Observations	117698	580973	1188288	1806661	2408481	3591959	5622854	8976501	9110293
	Presidential Elections								
Number of Days	0.2044*	0.1891*	0.2411**	0.2142*	0.2176***	0.1967**	0.2243**	0.2351**	0.3264***
	(0.1191)	(0.1082)	(0.1142)	(0.1204)	(0.0771)	(0.0940)	(0.1119)	(0.0942)	(0.0975)
Observations	55586	274718	562616	855835	1141271	1703645	2669117	4260076	4322569
	Midterm Elections								
Number of Days	-0.2453	0.1665	0.1348	0.0337	0.1461	-0.1051	0.0747	0.0719	0.1098
	(0.2530)	(0.1203)	(0.1058)	(0.1296)	(0.1859)	(0.1678)	(0.1856)	(0.1339)	(0.1413)
Observations	62112	306255	625672	950826	1267210	1888314	2953737	4716425	4787724

Notes: Each cell reports one coefficient estimate of an OLS regression with individual and geo-year fixed effects. In all regressions, the dependent variable is a binary variable equal to 100 if a registered voter turns out to vote in a general election; and zero otherwise. The rows indicate the key explanatory variable in each regression. The sample is restricted to all elections from 2008 to 2014 and individuals living within m miles of a county border, where m is indicated by the column headings. Standard errors are clustered two-way by county and by county-border.
$* \; p < 0.10$; $** \; p < 0.05$; $*** \; p < 0.01$.

Table 6: The Turnout Effects of Early Voting Laws: by Party

	Both		Presidential		Midterm	
	(1)	(2)	(3)	(4)	(5)	(6)
Number of Days	0.1010	0.0716	-0.1083	0.0002	0.0565	0.1133
	(0.0653)	(0.0666)	(0.1088)	(0.0855)	(0.1212)	(0.1114)
Days × Independent	0.0593	0.1284*	0.7773***	0.5777***	-0.0691	-0.1870***
	(0.0739)	(0.0697)	(0.0969)	(0.0801)	(0.0780)	(0.0714)
Days × Democrat	0.1991***	0.2202***	0.1327***	0.1007***	0.2877***	0.2629***
	(0.0547)	(0.0559)	(0.0253)	(0.0190)	(0.0791)	(0.0789)
Individual Fixed Effects	Y	Y	Y	Y	Y	Y
Year-specific Geo F.E.	Y	Y	Y	Y	Y	Y
Sub-sample (18 years old by 2000)		Y		Y		Y
Observations	1188288	1045573	562616	502740	625672	542833

Notes: Each column reports the coefficient estimates of an OLS regression. The dependent variable is the 4-year difference of a binary variable, which equals to 100 if a registered voter turns out to vote in a general election; and zero otherwise. The rows indicate the explanatory variables in each regression. In all regressions, the samples limited to individuals living within one mile of a county border. The samples under headings "Both" include both presidential and midterm election from 2008 to 2014. The samples under heading "Presidential" only include presidential elections in 2008 and 2012. The samples under heading "Midterm" only include midterm elections in 2010 and 2014. Columns (2), (4), and (6) restrict to individuals who had turned 18 by the year 2000. Party affiliation is identified by the most recent primary vote before the 2008 general election for presidential years and before the 2010 general election for midterm years. All regressions include a set of election-type-specific 1×1 mile geo fixed effects. Individual fixed effects for turnout propensity are differenced out. Year-specific geo fixed effects become election-type-specific geo fixed effects in the regression equations after the 4-year differencing. Standard errors are clustered two-way by county and by county-border.

$* \ p < 0.10$; $** \ p < 0.05$; $*** \ p < 0.01$.

Table 7: The Turnout Effects of Early Voting Laws: by Gender

	Both		Presidential		Midterm	
	Inferring Gender Using National First Names					
Number of Days	0.1097	0.1144	0.2013*	0.1993*	0.0314	0.0431
	(0.0834)	(0.0850)	(0.1185)	(0.1171)	(0.1090)	(0.1111)
Days × Pr(Female)	0.1261***		0.0428***		0.1619***	
	(0.0191)		(0.0132)		(0.0298)	
Days × Female		0.1209***		0.0400***		0.1556***
		(0.0184)		(0.0113)		(0.0288)
Observations	1148907	1096971	544407	520126	604500	576845
	Inferring Gender Using Ohio First Names					
Number of Days	0.1176	0.1172	0.2109*	0.1992*	0.0360	0.0477
	(0.0851)	(0.0840)	(0.1188)	(0.1157)	(0.1174)	(0.1173)
Days × Pr(Female)	0.1308***		0.0497***		0.1656***	
	(0.0199)		(0.0128)		(0.0309)	
Days × Female		0.1264***		0.0488***		0.1597***
		(0.0192)		(0.0115)		(0.0303)
Observations	1100490	1066515	521814	505948	578676	560567

Notes: Each column reports the coefficient estimates of an OLS regression. The dependent variable is the 4-year difference of a binary variable, which equals to 100 if a registered voter turns out to vote in a general election; and zero otherwise. The rows indicate the explanatory variables in each regression. In all regressions, the samples limited to individuals living within one mile of a county border. The samples under headings "Both" include both presidential and midterm election from 2008 to 2014. The samples under heading "Presidential" only include presidential elections in 2008 and 2012. $Pr(Female)$ is the probability of an individual being female based on the gender frequency in Social Security administrative data of his/her first name. $Female$ is an indicator variable equal to one if $Pr(Female) \geq 0.95$, zero if $Pr(Female) \leq 0.05$, and missing otherwise. In the upper panel, $Pr(Female)$ is inferred from the national frequency of females based on national birth record in the same birth year of the individual; in the lower panel, $Pr(Female)$ is inferred from the national frequency of females based on Ohio birth record in the same birth year of the individual. All regressions include a set of election-type-specific 1×1 mile geo fixed effects. Individual fixed effects for turnout propensity are differenced out. Year-specific geo fixed effects become election-type-specific geo fixed effects in the regression equations after the 4-year differencing. Standard errors are clustered two-way by county and by county-border.

$* \, p < 0.10$; $** \, p < 0.05$; $*** \, p < 0.01$.

Table 8: The Turnout Effects of Early Voting Laws: by Type of Day

	(1)	(2)	(3)	(4)	(5)
Days	0.2378***	0.1867*	0.2737***		
	(0.0319)	(0.0993)	(0.0570)		
Weekend Days	-0.0622				0.2013**
	(0.1055)				(0.0812)
Days with Same Day Regis.		0.0237			-0.1113
		(0.2014)			(0.3571)
Days Open Late			-0.0480		
			(0.0517)		
Weekdays				0.2367***	0.2890*
				(0.0305)	(0.1565)
Saturdays				-0.0201	
				(0.2650)	
Sundays				0.3231	
				(0.2239)	
Observations	1188288	1188288	1188288	1188288	1188288

Notes: Each column reports the coefficient estimates of an OLS regression. The dependent variable is the 4-year difference of a binary variable, which equals to 100 if a registered voter turns out to vote in a general election; and zero otherwise. The rows indicate the explanatory variables in each regression. In all regressions, the samples limited to individuals living within one mile of a county border in both presidential and midterm elections.All regressions include a set of election-type-specific 1×1 mile geo fixed effects. Individual fixed effects for turnout propensity are differenced out. Year-specific geo fixed effects become election-type-specific geo fixed effects in the regression equations after the 4-year differencing. Standard errors are clustered two-way by county and by county-border.

$* \ p < 0.10$; $** \ p < 0.05$; $*** \ p < 0.01$.

Table 9: The Turnout Effects of Early Voting Laws: Nonlinearity

	Both		Presidential			Midterm	
ΔDays	0.2197***		0.2249***			0.2473***	
	(0.0565)		(0.0717)			(0.0930)	
ΔDays^2	0.0034		-0.0034			0.0100	
	(0.0052)		(0.0095)			(0.0093)	
min{ΔDays, 0}		0.1851*		0.2584*	0.1928*		0.0564
		(0.1051)		(0.1464)	(0.1004)		(0.1566)
max{ΔDays, 0}		0.2403***		0.1661***	0.1881**		0.7224***
		(0.0758)		(0.0603)	(0.0741)		(0.2651)
Individual Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Year-specific Geo Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Sub-sample					Y		
Observations	1188288	1188288	562616	562616	362487	625672	625672

Notes: Each column reports the coefficient estimates of an OLS regression. The dependent variable is the 4-year difference of a binary variable, which equals to 100 if a registered voter turns out to vote in a general election; and zero otherwise. The rows indicate the explanatory variables in each regression. In all regressions, the samples limited to individuals living within one mile of a county border. The samples under headings "Both" include both presidential and midterm election from 2008 to 2014. The samples under heading "Presidential" only include presidential elections in 2008 and 2012. The last column under the presidential heading, i.e. the column with "Sub-sample" marked as Y, restricts the sample to those counties with at least one county experiencing no change in the number of early voting days between 2008 and 2012. All regressions include a set of election-type-specific 1×1 mile geo fixed effects. Individual fixed effects for turnout propensity are differenced out. Year-specific geo fixed effects become election-type-specific geo fixed effects in the regression equations after the 4-year differencing. Standard errors are clustered two-way by county and by county-border.

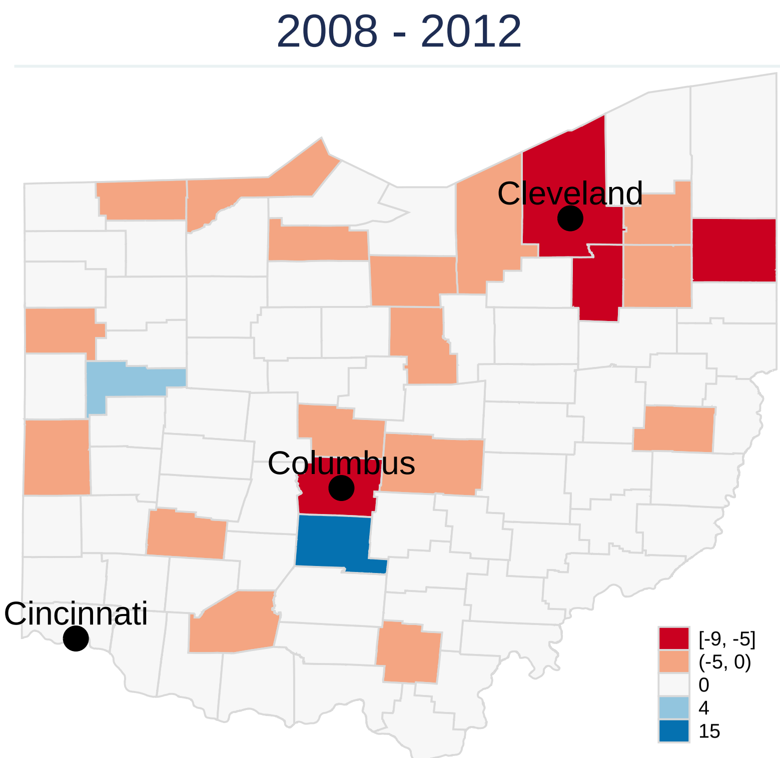
$* \ p < 0.10$; $** \ p < 0.05$; $*** \ p < 0.01$.

Table 10: Changes of Republican Seats and Electoral College Votes under Hypothetical Standardized Early Voting

		Observed Republican	Standardized Early Voting		
Election Type	Year	Seats / Electoral Votes	0 Days	23 Days	46 Days
President	2012	332/538	0	0	-15
President	2016	304/538	10	-65*	-75*
Senate	2012	51/100	1	-1*	-1*
Senate	2014	54/100	0	0	0
Senate	2016	52/100	0	-1	-3*
House	2012	234/435	5	-4	-10
House	2014	247/435	0	-1	-2
House	2016	241/435	1	0	-3

Notes: Each element of the columns under the heading "Standardized Early Voting" reports the simulated impact of national legislation requiring 0, 23 and 46 days of early voting respectively. Impacts are computed using estimates by party and election type from Ohio but are applied nationally. For Senate and House rows, numbers reflect the change in the number of seats to the Republican party. For the President row, numbers reflect the change in the number of electoral votes to the Republican party. Positive numbers indicate a net shift in favor of the Republican party and negative numbers indicate a net shift in favor of the Democratic party. $*$ indicates a change of majority control in the congress or majority of electoral votes.

Figure 1: Changes in Early Voting Days



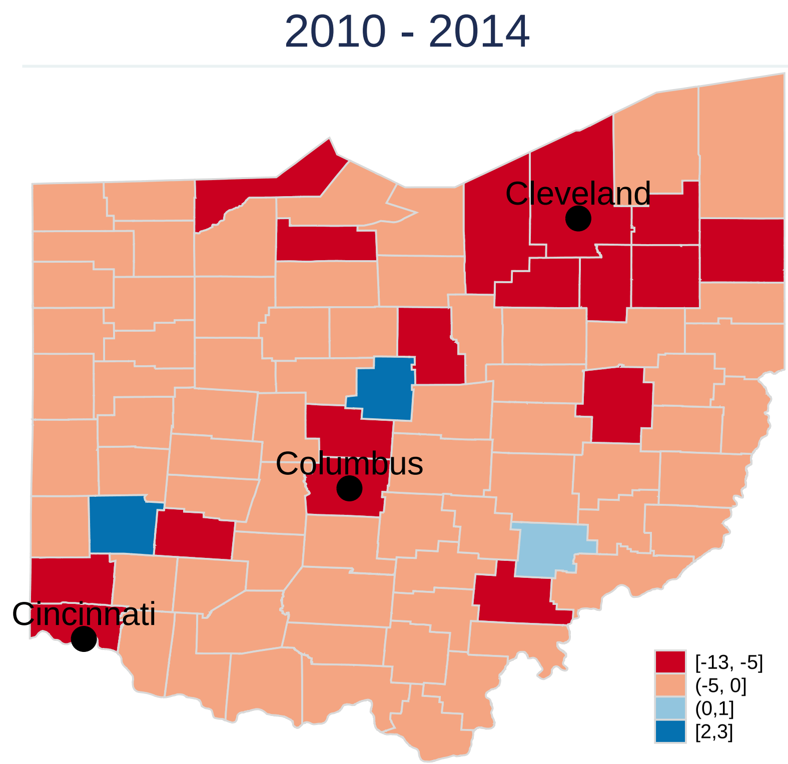


Figure 2: Full Sample of Ohio Registered Voters and One-Mile County Border Sample

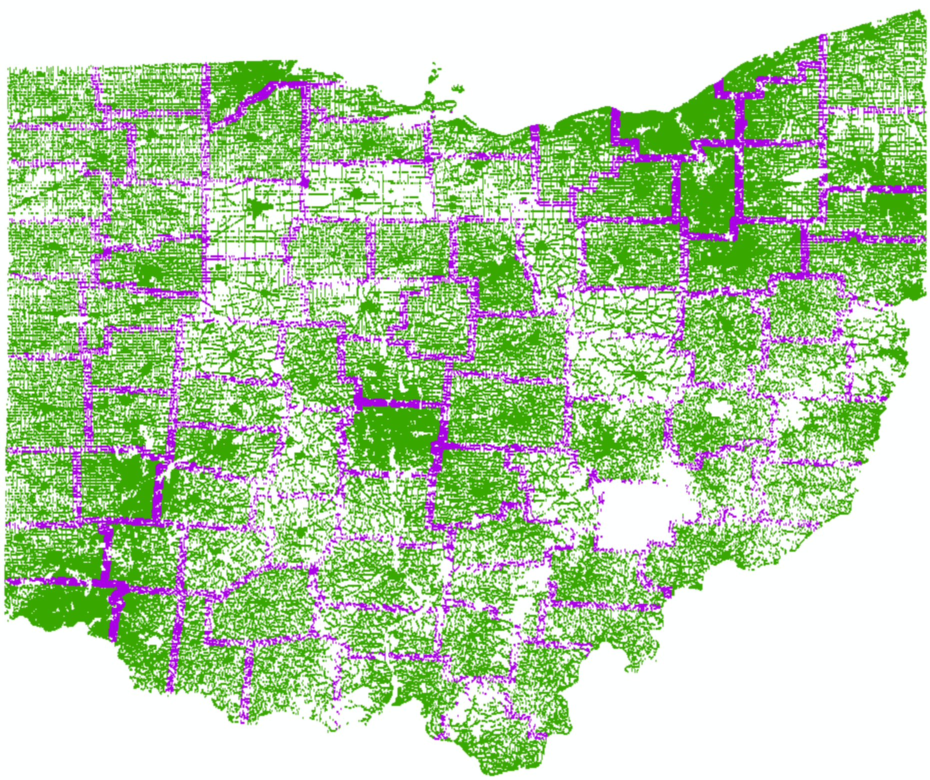
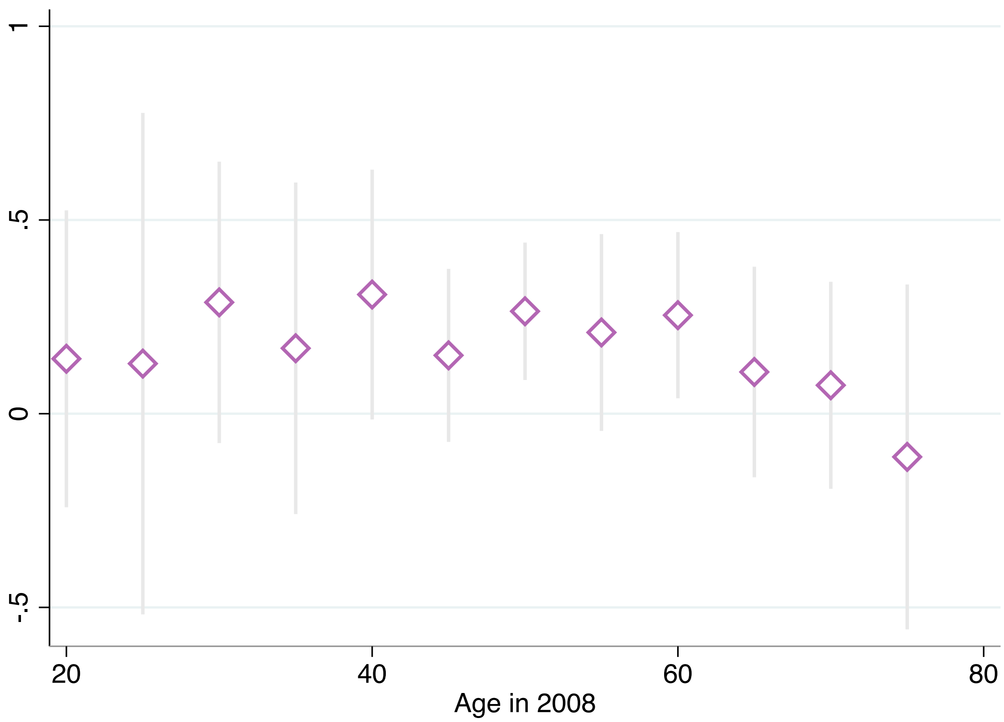


Figure 3: Heterogeneous Treatment Effects of Early Voting on Age Groups



Notes: The graph above plots age-group specific coefficients with 95% confidence intervals from a model with turnout regressed upon early voting days. Data from all general elections between 2008 and 2014 are included. Each age group is 5 years. The regression includes individual fixed effects first differenced within election type and age-group-year-specific 1×1 mile geo fixed effects. The dependent variable is a binary variable equal to 100 if an individual turns out to vote and zero otherwise.

Appendix

Table A.1: Sample Means of Ohio Registered Voters in Counties by Change in Early Voting Duration: 2012

	All	Change Related to Mean	# Hours	# Days in Weekend	# Days with Same Day Registration	# Days Open Late	# Weekdays	# Saturdays	# Sundays
Black (%)	13.2	+	7.2	9.0	9.4	7.2	1.4	10.1	8.7
		-	23.8	19.6	20.9	24.8	13.3	17.6	23.0
Hispanic (%)	2.9	+	2.7	2.3	2.4	2.6	1.1	2.5	2.5
		-	3.3	3.9	3.9	3.4	2.9	3.5	3.9
White (%)	83.0	+	89.6	87.9	87.5	89.7	97.0	86.8	88.2
		-	71.2	75.4	73.7	70.0	82.9	77.5	71.6
Democrat (%)	30.4	+	28.1	27.4	28.1	28.5	22.7	28.2	27.7
		-	34.5	35.1	35.0	34.0	30.4	33.6	36.2
Independent (%)	43.1	+	42.1	42.4	42.6	42.0	40.4	42.9	42.2
		-	44.8	44.1	44.2	45.3	43.1	43.4	45.1
Republican (%)	26.5	+	29.8	30.2	29.3	29.5	37.0	28.9	30.1
		-	20.7	20.8	20.7	20.7	26.4	23.1	18.7
College Grad. (%)	25.2	+	21.8	22.5	22.5	21.6	13.6	22.1	22.8
		-	31.2	29.3	30.7	32.0	25.3	29.6	30.4
HS Dropout (%)	12.0	+	12.2	12.4	12.4	12.2	13.4	12.6	12.2
		-	11.8	11.5	11.4	11.7	12.0	11.2	11.6
Med. Household Income	53.7	+	52.9	52.8	52.7	52.6	52.0	51.6	53.2
		-	55.2	55.1	55.8	55.8	53.7	56.7	54.7
Age in 2008	44.6	+	45.3	45.1	45.1	45.3	45.4	45.0	45.0
		-	43.4	43.8	43.4	43.1	44.6	44.0	43.6
Distance to Early Voting Site	10.9	+	11.2	10.7	10.5	11.2	13.4	10.4	11.1
		-	10.3	11.1	11.6	10.2	10.9	11.6	10.3
Voted in 2008 (%)	86.2	+	85.9	86.4	86.3	85.9	87.3	86.1	86.4
		-	86.9	85.9	86.1	87.0	86.2	86.4	85.9
Voted in 2010 (%)	59.9	+	60.1	60.3	60.5	60.2	61.2	60.0	60.1
		-	59.4	59.2	58.6	59.3	59.9	59.7	59.2
Voted in 2012 (%)	76.3	+	76.0	76.7	76.5	76.0	79.0	76.2	76.7
		-	76.9	75.6	75.8	77.0	76.3	76.4	75.5
Voted in 2014 (%)	43.7	+	44.3	45.0	44.8	44.4	45.7	44.5	44.9
		-	42.7	41.9	41.5	42.5	43.7	42.6	41.3
Observations	6559589	+	4206014	3998136	4428625	4337070	56223	3879580	4512346
		-	2353575	2561453	2130964	2222519	6503366	2680009	2047243

Notes: Each row reports the means of one variable indicated by the first column. Column "All" reports the sample means of Ohio residents who registered and were eligible to vote in 2008. Column "Change Related to Mean" indicates a sub-sample of counties with above (+) or below (-) mean changes of early voting duration from 2008 to 2012 as measured by the subsequent column headings. Variable "Med. Household Income" is the median household income of a registered voter's Census block group in thousands of dollars. "Distance to Early Vote Site" is measured in miles. "Age in 2008" is measured in years as of the general election day in 2008. All other variables are in percentage points.

Table A.2: Sample Means of Ohio Registered Voters in Counties by Change in Early Voting Duration: 2014

	All	Change Related to Mean	# Hours	# Days in Weekend	# Days with Same Day Registration	# Days Open Late	# Weekdays	# Saturdays	# Sundays
Black (%)	13.5	+	6.4	4.8	7.3	9.7	14.3	8.5	8.6
		-	22.5	22.6	22.7	20.7	13.4	21.7	24.6
Hispanic (%)	3.0	+	2.2	2.2	2.4	2.8	2.4	2.8	2.4
		-	4.0	3.8	3.8	3.3	3.0	3.4	4.2
White (%)	82.7	+	90.8	92.5	89.7	87.1	82.3	87.8	88.3
		-	72.3	72.4	72.1	74.2	82.7	74.0	69.7
Democrat (%)	28.6	+	26.4	26.2	26.7	28.9	25.5	26.9	26.4
		-	31.3	31.0	31.4	27.9	28.8	31.3	33.6
Independent (%)	46.6	+	44.7	44.5	45.4	44.6	47.3	46.4	45.5
		-	49.0	48.8	48.3	50.4	46.5	47.0	49.2
Republican (%)	24.8	+	28.9	29.3	27.9	26.5	27.2	26.7	28.2
		-	19.7	20.2	20.3	21.7	24.6	21.8	17.3
College Grad. (%)	25.0	+	20.9	20.6	21.1	22.2	23.1	23.2	22.6
		-	30.3	29.7	31.0	30.5	25.2	28.0	30.6
HS Dropout (%)	12.2	+	12.5	12.5	12.5	12.5	11.8	12.2	12.3
		-	11.8	11.8	11.7	11.5	12.2	12.1	11.8
Med. Household Income	53.4	+	51.5	51.6	51.3	52.2	50.0	52.3	53.0
		-	55.8	55.2	56.4	55.6	53.7	55.1	54.3
Age in 2008	42.9	+	43.7	43.8	43.6	43.7	43.0	42.9	43.4
		-	41.8	41.9	41.8	41.4	42.9	42.8	41.6
Distance to Early Voting Site	10.8	+	11.2	10.9	10.9	10.9	14.5	10.7	11.2
		-	10.2	10.7	10.6	10.5	10.4	11.0	9.9
Voted in 2008 (%)	86.2	+	85.9	86.1	85.7	86.1	84.3	85.7	86.4
		-	86.7	86.4	87.1	86.4	86.4	87.1	85.9
Voted in 2010 (%)	59.9	+	59.9	60.3	59.9	60.4	57.5	59.8	60.2
		-	59.8	59.3	59.8	58.9	60.1	60.0	58.9
Voted in 2012 (%)	76.3	+	76.1	76.3	75.8	76.3	74.2	75.9	76.6
		-	76.6	76.3	77.0	76.3	76.5	77.0	75.5
Voted in 2014 (%)	40.9	+	41.9	42.1	41.5	41.7	40.3	40.9	42.1
		-	39.7	39.7	40.1	39.5	41.0	41.0	38.3
Observations	7597048	+	4257198	3887251	4559020	4979027	632830	4750376	5291847
		-	3339850	3709797	3038028	2618021	6964218	2846672	2305201

Notes: Each row reports the means of one variable indicated by the first column. Column "All" reports the sample means of Ohio residents who registered and were eligible to vote in 2010. Column "Change Related to Mean" indicates a sub-sample of counties with above (+) or below (-) mean changes of early voting duration from 2010 to 2014 as measured by the subsequent column headings. Variable "Med. Household Income" is the median household income of a registered voter's Census block group in thousands of dollars. "Distance to Early Vote Site" is measured in miles. "Age in 2008" is measured in years as of the general election day in 2008. All other variables are in percentage points.

Table A.3: The Turnout Effects of Early Voting Laws: by Type of Days in Presidential Elections

	(1)	(2)	(3)	(4)	(5)
Days	0.1661***	0.2191	0.3027***		
	(0.0603)	(0.1584)	(0.1077)		
Weekend Days	0.0923				0.2309
	(0.1861)				(0.1563)
Days with Same Day Regis.		0.0989			0.1261
		(0.9085)			(0.9167)
Days Open Late			-0.0269		
			(0.0626)		
Weekdays				0.1668***	0.1357
				(0.0591)	(0.2359)
Saturdays				0.2095	
				(0.1740)	
Sundays				0.2921	
				(0.2827)	
Observations	562,616	562,616	562,616	562,616	562,616

Notes: Each column reports the coefficient estimates of an OLS regression. The dependent variable is the 4-year difference of a binary variable, which equals to 100 if a registered voter turns out to vote in a general election; and zero otherwise. The rows indicate the explanatory variables in each regression. In all regressions, the samples limited to individuals living within one mile of a county border and observations in presidential elections. All regressions include a set of election-type-specific 1×1 mile geo fixed effects. Individual fixed effects for turnout propensity are differenced out. Year-specific geo fixed effects become election-type-specific geo fixed effects in the regression equations after the 4-year differencing. Standard errors are clustered two-way by county and by county-border.
$* \; p < 0.10$; $** \; p < 0.05$; $*** \; p < 0.01$.

Table A.4: The Turnout Effects of Early Voting Laws: by Type of Days in Midterm Elections

	(1)	(2)	(3)	(4)	(5)
Days	0.2419***	0.0459	0.2369***		
	(0.0396)	(0.2363)	(0.0721)		
Weekend Days	-0.1843				0.1338
	(0.1323)				(0.2399)
Days with Same Day Regis.		0.2277			-0.2795
		(0.3981)			(0.5364)
Days Open Late			-0.1153		
			(0.0880)		
Weekdays				0.2412***	0.3966
				(0.0379)	(0.2981)
Saturdays				-0.2148	
				(0.4258)	
Sundays				0.2904	
				(0.2370)	
Observations	625,672	625,672	625,672	625,672	625,672

Notes: Each column reports the coefficient estimates of an OLS regression. The dependent variable is the 4-year difference of a binary variable, which equals to 100 if a registered voter turns out to vote in a general election; and zero otherwise. The rows indicate the explanatory variables in each regression. In all regressions, the samples limited to individuals living within one mile of a county border and observations in midterm elections. All regressions include a set of election-type-specific 1×1 mile geo fixed effects. Individual fixed effects for turnout propensity are differenced out. Year-specific geo fixed effects become election-type-specific geo fixed effects in the regression equations after the 4-year differencing. Standard errors are clustered two-way by county and by county-border.
$* \ p < 0.10$; $** \ p < 0.05$; $* * * \ p < 0.01$.

Table A.5: Correlation between Individual Partisanship and Precinct-level Democratic Vote Share

	2008	2010	2012	2014
Democrat	0.571	0.658	0.548	0.553
Independent	0.297	0.283	0.38	0.48
Republican	-0.822	-0.853	-0.769	-0.872

Notes: Each cell reports a correlation coefficient between the precinct-level democratic vote share and the precinct-level average individual partisanship. Party affiliation is identified by the most recent primary vote before the 2008 general election for presidential years and before the 2010 general election for midterm years. The column header indicates the year of the election. The row headers indicate individual partisanship by past primary turnouts.

Table A.6: Partisanship by Election

Year	Democrats	Independents	Republicans
2008	38.3	38.3	23.4
2010	35.2	39.9	24.9
2012	30.4	43.1	26.5
2014	28.6	46.6	24.8

Notes: Each cell above reports the share of a partisan group voting in an election. Party affiliation is identified by the most recent primary vote before the 2008 general election for presidential years and before the 2010 general election for midterm years. The row header indicates the year of election. The column header indicates individual partisanship by past primary turnouts. All values are in percentage points.

Figure A.1: Changes in Early Voting Duration (2008 - 2012)

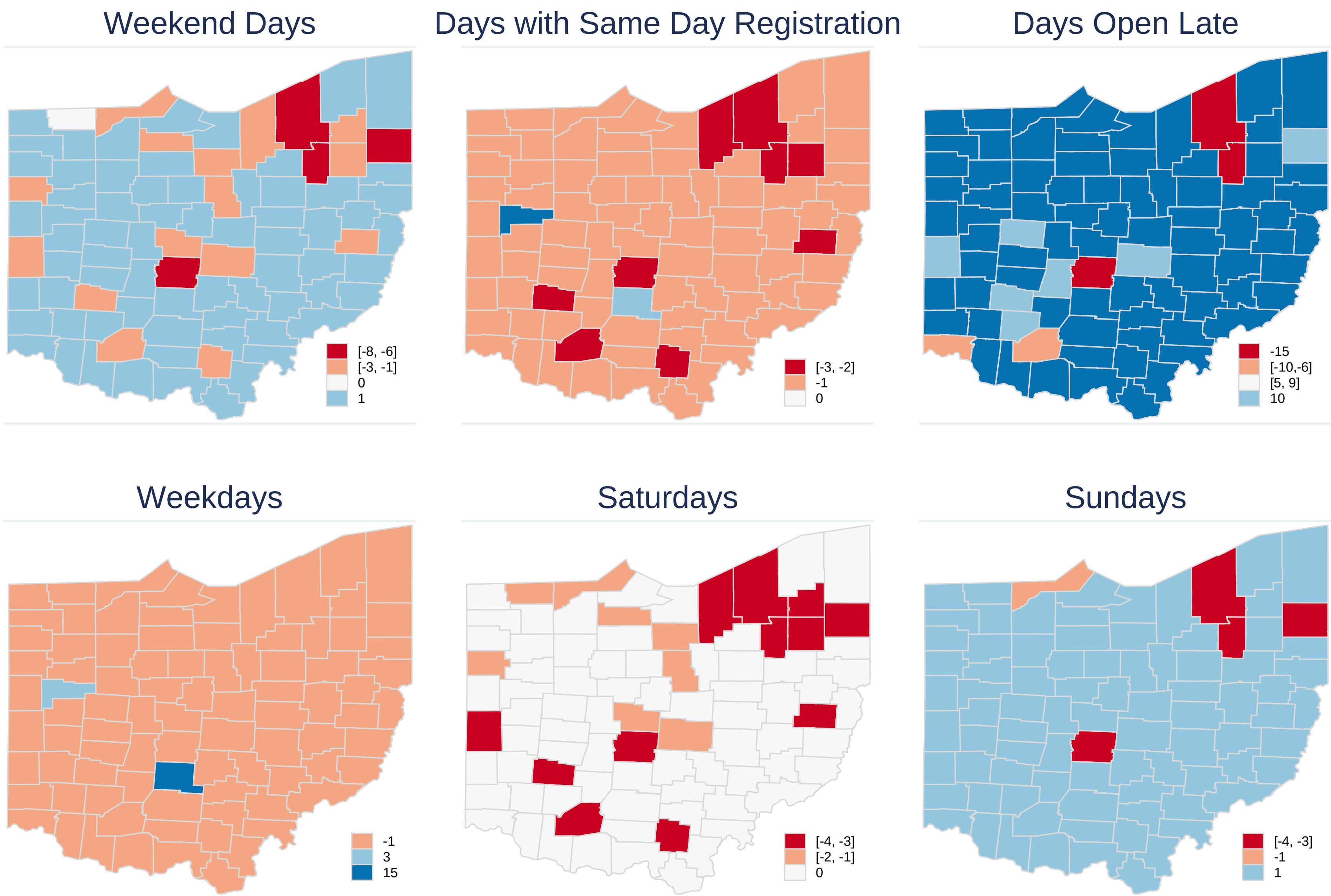


Figure A.2: Changes in Early Voting Duration (2010 - 2014)

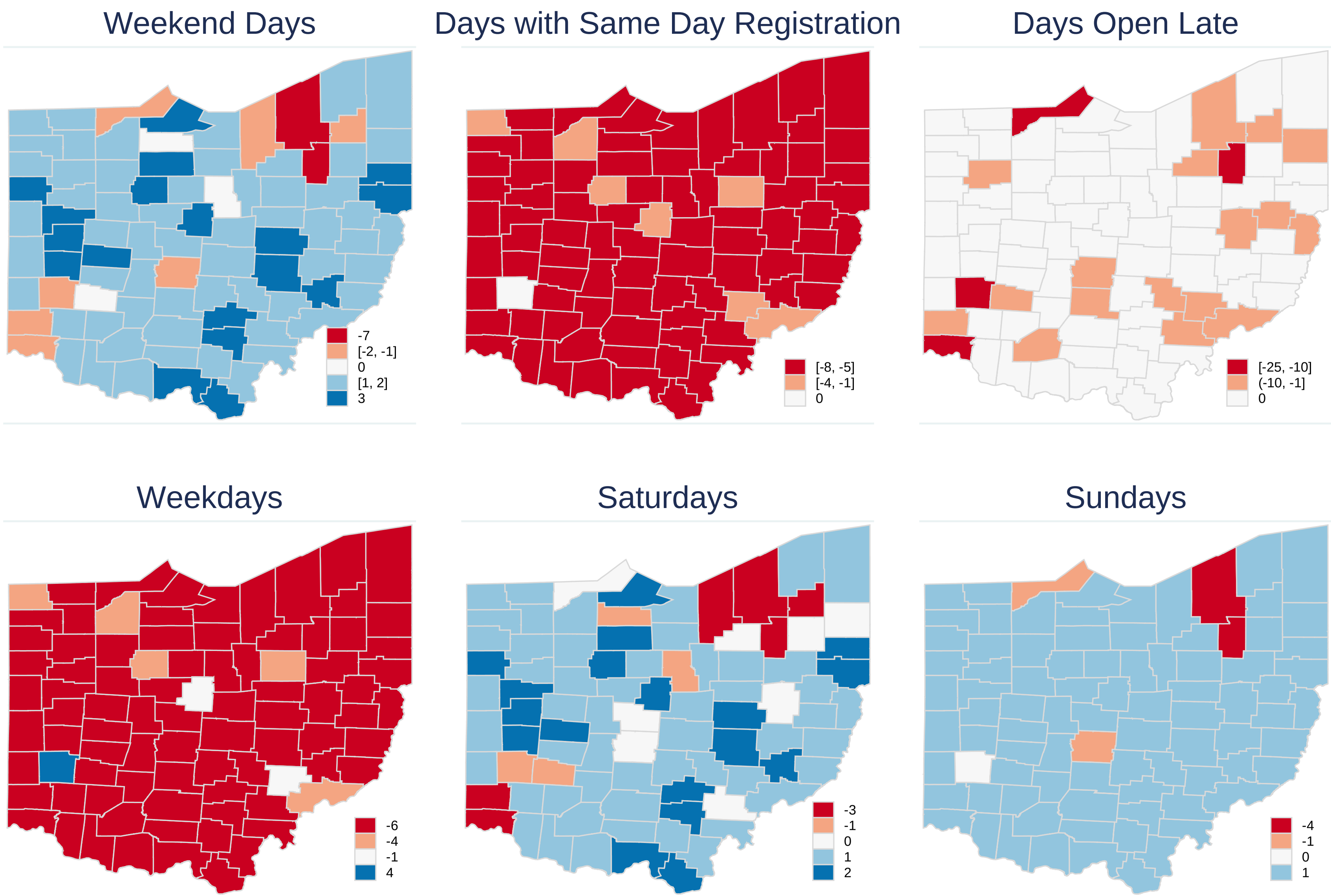
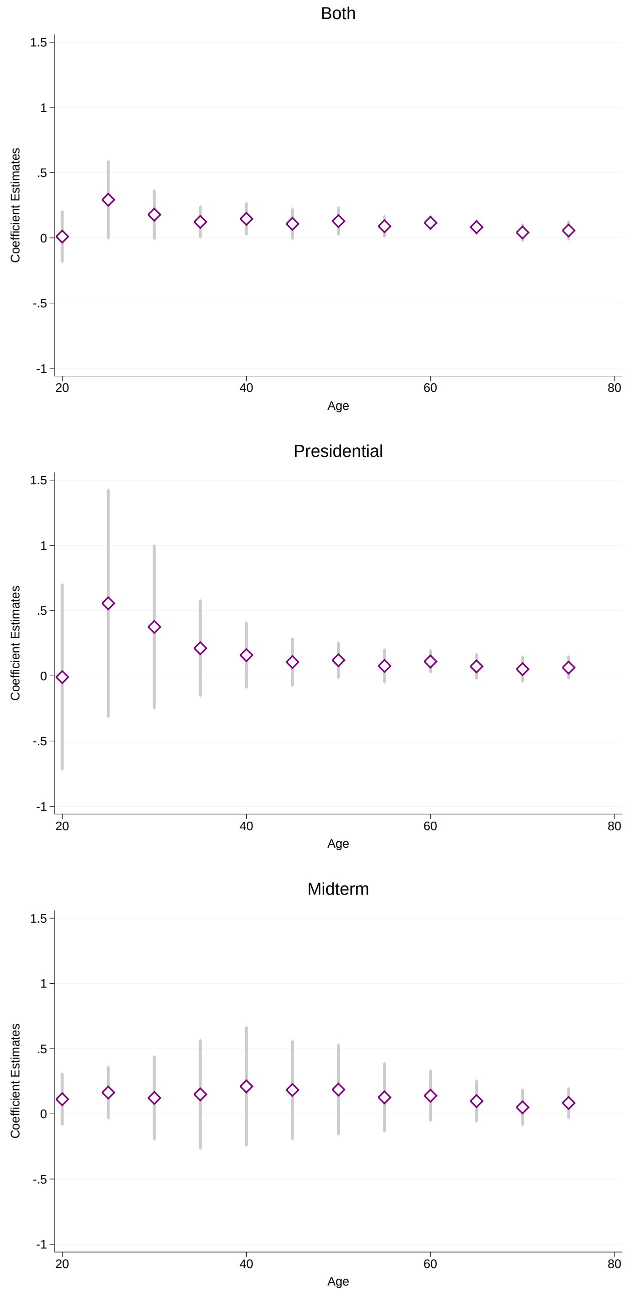
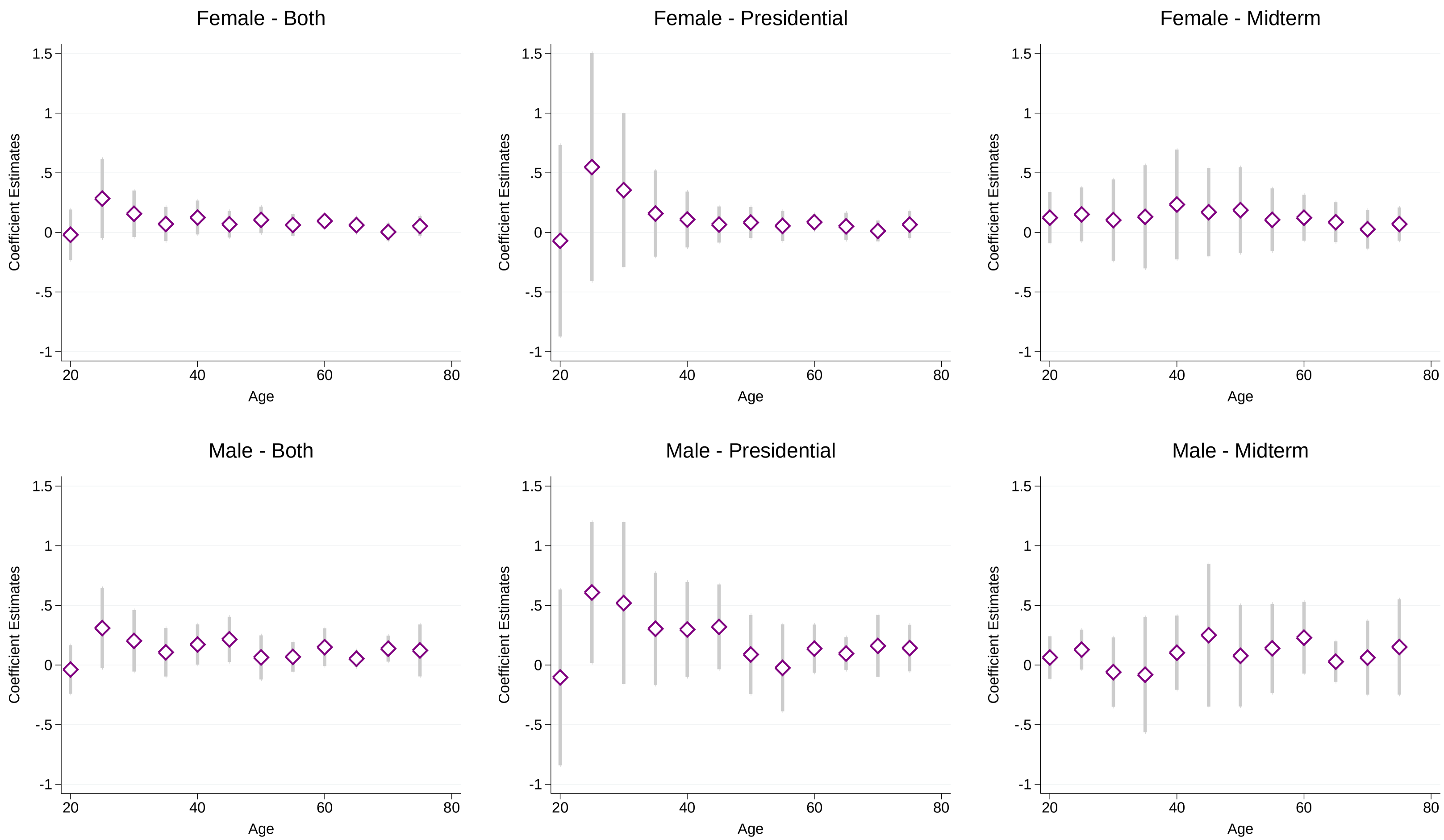


Figure A.3: Heterogeneous Treatment Effects of Early Voting by Age Group from Full Sample



Notes: The graph above plots the estimated impacts of early voting on turnout rates for each 5-year age group. Each plotted coefficient and its 95% confidence interval is from one equation using the full geographic sample and a 5-year age group subsample as indicated by the horizontal axes. The top subplot uses sample from both presidential elections and midterm elections. The second and third subplots use presidential and midterm elections respectively. All specifications include individual fixed effects and election fixed effects. Standard errors are clustered by county.

Figure A.4: Heterogeneous Treatment Effects of Early Voting by Age Group and Gender



Notes: The graph above plots the estimated impacts of early voting on turnout rates for each 5-year age group by gender. Each plotted coefficient and its 95% confidence interval is from one equation using the full geographic sample and a 5-year age group subsample as indicated by the horizontal axes. The top subplots are for females, and the bottom subplots are for males. The first column uses samples pooling presidential elections and midterm elections from 2008 to 2014. The second and third columns use only presidential elections and midterm elections respectively. All specifications include individual fixed effects and election fixed effects. Standard errors are clustered by county.