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

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Abstract

We estimate effects of early voting on voter turnout using a 2010 homogenization law from Ohio which forced some counties to expand and others to contract early voting. Using voter registration data, we compare 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 0.24 percentage points of additional turnout per additional early voting day. We also find greater impacts on women, Democrats, Independents and those of child-bearing and working age. We simulate impacts of national early day laws on recent election outcomes.

1 Introduction

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.

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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. State-level estrictions 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.

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 the approximately 136.5 million ballots cast used some form of pre-election voting. 23 million of these were cast in-person. 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. Within months of the Supreme Court's

[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".

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

Shelby County vs. Holder (2013) 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 turn out 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 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.

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 run 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 and particularly so during the Obama years.

Card and Moretti (2007) estimated the impact upon turnout of new and improved voting technology using the 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 homogenized the days and even exact hours of early voting across counties in Ohio. This change was passed by Republican Governor John Kasich in time for the general election in 2012.

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). 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, forthcoming). 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 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 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%.

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 election. 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 change 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 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%.[4] The contraction in early voting availability in urban areas happened during a period of increased popularity of early voting.

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

[4]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.

day registration voting[5], 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.

Large pre-2012 discrepancy across counties within Ohio led to large differential changes due to the state policy changes implemented in 2012. 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

[5] 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.

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.

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 by county. We see large reductions in early voting days in 2012 relative to 2008 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. 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, 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

[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.

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 these cases, we compute minimum distance to each census block group in the state using latitude and longitude and assign an individual to the geographic centroid of the 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 and 2012. We use 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.

We also 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

[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.

birth year.

Finally, we use the self-reported ideology question and the party affiliation question from the 2016 Cooperative Congressional Election Study (CCES). The CCES is a stratified sample survey, administered by YouGov, which links questionnaire answers by respondents to actual voting records. We use 40,784 observations from the CCES to show ideological differences by party affiliation.

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. 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:

$$V_{ict} = \alpha_t + \phi_c + \boldsymbol{T}_{\boldsymbol{ct}}'\boldsymbol{\beta} + \epsilon_{ct} + \theta_{ict} \tag{1}$$

where V_{ic} 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. 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 that coincide with Ohio state borders. We refer to such samples as k-mile samples 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. We regress the change in turnout between 2008 and 2012 upon the change in early voting days, using the k-mile sample and controlling for geographical block fixed effects. We thus estimate:

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

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

[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.

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]

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:

$$\triangle V_{ibc} = \rho_b + \beta \Delta T_c + \boldsymbol{D_i'} \triangle \boldsymbol{T_c} \boldsymbol{\gamma} + \epsilon_c + \theta_{ibc} \tag{4}$$

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 equation (3) 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:

$$\triangle V_{ibce} = \rho_{be} + \beta \triangle T_{ce} + \theta \triangle T_{ce}^2 + \epsilon_{ce} + \theta_{ibce} \tag{5}$$

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

[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.

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 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. 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.

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 in 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. 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. Counties with below-mean change were fully 12.5 percentage points less white. 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. Registered voters in above-mean counties are 9.4 percentage points more likely to have most recently participated in a Republican primary and 7.7 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. 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. The estimates are very tight in large part because the sample size is so large. Our estimates range from an increase in turnout of 0.0549 percentage points per additional day of early voting for the county fixed effects model to an increase of 0.2411 percentage points for the baseline geographical fixed effects model. Three of the four 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 we could control for voter demographics in the the county difference-in-differences model, 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.

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.[12] 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.

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

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) for 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. 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[13], 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 full county specification with local comparisons[14]. In fact, sample sizes do not increase much

[13]The number of Democrats, Republicans and Independents are obviously not statistically independent.

[14]Local county comparisons, moreover, are still probably preferable to a county differences-in-differences estimation from an identification standpoint.

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 also consider the possibilty that people might not differ systematically across county borders but that counties which contract are counties which were more generous in early voting access as well as more limited in purging of voters. Counties which purge inactive voters from the voter rolls more liberally might also have lower turnout unrelated to early voting policy. Though we see no correlation between pre-existing generosity of voting and average age of registered voters in our placebos, it is possible that more restrictive counties purge both younger voters who move and older voters who move or pass away. Thus, it is possible that our null-effect on age differences above is consistent with differential purging across counties but on both for both younger and older individuals with no net effect on mean age. We thus consider additional balance placebos using: (1.) Date last voted, (2.) Date last voted before the 2008 general election, (3.) A dummy for never having voted, (4.) A dummy for never having voted prior to the 2008 election, and 7 dummies, one for each decade of age (20, 30, 40, 50, 60, 70, and 80). Of these 11 dummies, 2 are statistically significantly different at the 10% level. If the tests were all independent, two or more would be statistically significant at a 10% level or less 30.2% of the time. The two significant coefficients are on 20 and 30 year olds. A county with a one day larger relative increase in early voting days has a 0.046% lower probability of an registrant being in their twenties. Thus a 22 day differential change would be associated with a 1% differential probability of a registrant being in their twenties. The coefficient on 30s is substantially larger. It is -0.2826 and is by far the largest in magnitude; all other age dummies have coefficients below 0.1. A 1% differential probability of a registrant being in their twenties would be associated with a 3.6 day differential change. Overall, we are not concerned by differences either in voter characteristics or other county level voting policies which might effect turnout trends.

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 4, we show our estimates. The estimates 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.

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 political 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. 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. Our results are reported in Table 5.

We regress change in turnout from 2008 to 2012 on change in days, controlling for geographical 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%. The rank order across partisan leaning of the coefficients is the same in the restricted and full samples. The differences in estimates across samples range are 0.1995 for Democrats, 0.0320 for independents, and -0.1085 for Republicans. In this section, we focus upon the estimates on the restricted sample of those who had turned 18 by the year 2000.

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 percentage points 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 moderate, then early voting has a depolarizing impact upon the vote in presidential elections. In appendix Figure A.2, we show that independents are much more likely to identify themselves as ideologically moderate as opposed to conservative or liberal than either registered Republicans or registered Democrats.

Republicans seem to be more reliable voters. Republican vote shares are also higher in midterm elections which are less salient for most Americans. A higher fraction of Democrats and Republicans turn out for presidential elections than do Independents. The marginal voters are thus Independents who are more politically indifferent in presidential elections. Easing access to voting differentially impacts Democrats but impacts Independents to a an even greater degree during presidential election years.

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.2, the correlation between our measure of Democratic vote share and the precinct-level vote share is 0.571 for 2008. The correlation coefficients don't vary much by party. 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 show our estimates graphically 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. The first thing which we note is that the effects are positive for 9 of the 12 age groups. This would happen by random chance if the estimates were independent across age group with below a 7.3 percent probability. In addition, the three negative estimates are the three smallest in magnitudes (-0.0323, -0.0713, and -0.1219). The other nine groups range from +0.1737 to +0.5036.

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 three age groups with the smallest estimated effects are the lowest age group (18-22) and the two highest age groups (68-77). These are, in the first case, the group predominantly without children and, in the second case, the retired. For all three of these age groups, estimates are very small, statistically insignificant and below zero. In contrast, all other estimates are larger in magnitude and above zero.

The largest effects are approximately 0.5 percentage points per additional early voting days for 28-32 year olds. This age group is largely comprised of working parents with infants and young children. The median age of first birth for women in Ohio was 25 in 2006 (Mathews et al., 2009) and nationally, the first age of first birth for men is two years more than that for women. We test formally for a difference between the population-weighted average effects for the 25-65 age groups and the 20, 70 and 75 age groups. The average difference in estimated effects is 0.22 percentage points of turnout per additional early voting day and the difference is statistically significant at below a 5% level. Our age results suggest that the costs of voting are born particularly by those who have limited time: those who work 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 (4). 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 6, 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 0.2013 percentage points. This coefficient is statistically significant with above a 90% level of confidence. There is an additional 0.0428 impact for women which is statistically significant at more than a 99% level. The tightness of the standard errors on the gender coefficient reflects the uniformity of systematic differences in voting behavior across the sexes. The effect of early voting laws on female turnout is roughly 21% higher than that for men.

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.

In general, we do not see differential patterns by age across males and females.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 and that they are present both for men and women.

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), 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. Most counties saw expansions in number of weekend days as well as number of Sundays. 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. All other counties saw reductions in early voting. The larger declines occurred in the more urban areas. 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.

We estimate heterogeneous effects by type of treatment in Table 7. 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 effects of early voting expansion. Moreover, the estimate magnitudes are relatively stable. The effects for days or weekdays all range from between +0.1357 percentage points per day to +0.3027 percentage points per day.

In column 4 of Table 7, we see that the coefficients on Sundays and same day registration days are sizeable. The coefficients suggest that a Sunday early voting day adds additionally 75% to the main effect and a same day registration day 45%. Even the Saturday point estimates are fully 25% larger than the baseline weekday effects. Unfortunately, most of the variation in numbers of weekend days and same day registration days come from a small number of highly urban counties. Thus, after clustering properly, there is insufficient variation to determine with statistical precision the effect sizes. Out of five specifications, the baseline coefficient for days is not statistically significant at conventional levels in two specifications: columns 2 and 5. However, the effect for same day registration days (the sum of the coefficients in Column 2) has a p-value below 0.01 and in Column 5, the p-value for the sum of the three coefficients (weekend days with same day registration) also has a p-value below 0.01. In contrast to our other estimates, we find very small estimated effect sizes for days open late.

5.9 Non-Linear Treatment Effects

The average impact of an additional day of early voting is 0.2411 percentage points of additional turnout. 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 between 2008 and 2012 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 (5). We show these results in column 1 of Table 8. The quadratic term is small and statistically insignificant. Our estimates show 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.

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 have a negligible impact upon both the linear and quadratic term coefficients for the impact of number of early voting days.

We also consider the possibility that changing the number of early voting days may have an impact beyond the raw number of days made available. In particular, expansions may have a smaller impact in the short run than contractions. People may not realize that early voting changes have occurred. Thus, people may plan to vote early only to realize that they are too late because the timing of early voting availability has changed. On the other hand, people who would otherwise not have voted may not be aware of expansions and may, thus, underutilize them. To deal with these concerns, we estimate the effects of expansions and contractions separately. We then re-estimate restricting to cases where one of the counties had no change in early voting days and the other side had either an expansion or a contraction. Column 2 of Table 8 shows the full sample estimates. We do find that the impact of expansions is larger than the impact of contractions though the difference is far from statistically significant. Expansions raise turnout by 0.2584 percentage points per day. The coefficient is statistically significant at only a 90% level because the standard errors are high in light of the small number of expansion counties. Contractions reduce turnout by 0.1661 percentage points per day; the estimate is statistically distinguishable from zero at a 99% level.

In cases where we have two neighboring expanding counties, two neighboring contracting counties, or one of each, it may be difficult to cleanly identify expansion or contraction effects. We thus also show estimates which restrict to comparisons where one side of the border had no change in aggregate days. This shrinks our sample size by 36%. Nonetheless, the greater comparability of the treatments reduces the standard errors for the expansion coefficient. The two coefficients, in this sample, are near identical. Both are equal to 0.19 rounding to two digits. Thus, our evidence suggests that only the aggregate number of days matters and that changes in the timing of early voting provision does not have an impact in addition to the change in the number of days of early voting availalble.

6 Aggregate Effects

In this section, we use our geographic fixed effect estimates on turnout in presidential elections 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 party to estimate the impact on voter turnout and on the Democratic vote share of the Kasich reform for 2012. 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:

$$T = \sum_{c} \beta \mu_c R_c \tag{6}$$

where β is our estimated effect per day of early voting on turnout, μ_c is the change in the number of days of early voting available, 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.

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 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.2. The correlation coefficients are decently stable across elections. The Republican registration share correlation with the Democratic vote share ranges is -0.769 in 2012 and -0.822 in 2008. The Democratic registration share is significantly lower mainly because Independents lean heavily Democrat. The correlation is 0.548 is 2012 and 0.571 in 2008. The Independent share is positively correlated with the Democratic vote share. It is also unsurprisingly more unstable. The correlation for the Independent share is 0.380 in 2012 and 0.297 in 2008.

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 by the probability that a registrant of party p votes for the Democrats. We denote by β_p the turnout effect for registrants with party p and by ρ_p, 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 in election e: ω_{pc} . Altogether, this gives us the expected net change in Democratic votes from a one day increase in early voting in county c. Finally, we multiply this by the net change in days of early voting in the county which we denote by μ_c. The expected increase in votes for Democrats in county c is thus $\beta_p \rho_p \mu_c \omega_{pc}$. Our equation for the net change in Democratic votes, T_p, is given by summing over all counties:

$$T_p = \sum_c \beta_p \rho_p \mu_c \omega_{pc}.$$

We 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:

$$\Delta V_p = \frac{T_D + T_R + T_I}{Turn}$$

where **D** denotes Democrat, **I** denotes Independent, **R** denotes Republican, and $Turn$ 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. 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. 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. Overall, the changes to early voting in Ohio had a positive though modest impact on the Republican vote share.

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. One caveat to our results is that the ban and the 46 day expansion both extrapolate linearly out of sample. Though our estimates show evidence of linearity, it is possible that increasing the amount of early voting to 46 days may reduce the marginal effects of early voting expansions and even more so, it is possible that a shift on the extensive as opposed to intensive margin may have an additional impact.

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, 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.[15] We then sum across parties to get an effect for presidential elections. We denote by Θ the total effect of an extra day of early voting in Ohio:

$$\Theta = \sum_p \beta_p \rho_p s_p$$

where β_p is the effect of an extra day for members of party p, ρ_p is the correlation between registration and voting for members of party p, and s_p is the share of registrants from party p.[16] If our estimates are externally valid across states, our calculations suggest that though Minnesota may have added 0.0992 percentage points per day[17]. This means that 10 days of additional early voting would add 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

[15] We use the average across the 2008 and 2012 elections to compute the correlation coefficients and the group shares that we use in this equation.

[16] We take ρ_p and s_p from our Ohio voter registration data.

[17] Ohio, as a swing state, is probably decently representative, particularly in its partisanship composition, of the distribution of swing state and swing district voters.

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 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 9. Impacts upon Independents are large in presidential elections where they are the marginal voters and they swing Democrat in their voting patterns. In 2012, we predict that in the Senate, 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 2016, a 46-day law would only induce a movement towards the Democrats of 3 seats and an elimination of early voting in 2016 would shift 1 seat towards the Republicans.

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 of early voting as Minnesota would have switched to Republican. A national 23-day early

[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 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.

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 0.24 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 20% more strongly than men to additional early voting. We do not find strong responses to days where polls are open late. However, we do find large (though statistically insignificant) differential turnout response to Saturday and Sunday voting as well as a sizeable but more modest statistically insignificant impact of same day registration. 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.

We further find that effects are larger on Democrats than on Republicans and that effects on Independents are very large. 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 that 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.

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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		
	All	+ / -	Mean
Black (%)	13.2	+	9
		-	19.6
Hispanic (%)	2.9	+	2.3
		-	3.9
White (%)	83	+	87.9
		-	75.4
Democrat (%)	30.4	+	27.4
		-	35.1
Independent (%)	43.1	+	42.4
		-	44.1
Republican (%)	26.5	+	30.2
		-	20.8
College Graduate (%)	25.2	+	22.5
		-	29.3
High School Dropout (%)	12	+	12.4
		-	11.5
Mediam Household Income ($1,000)	53.7	+	52.8
		-	55.1
Age in 2008	44.6	+	45.1
		-	43.8
Distance to Early Voting Site (miles)	10.9	+	10.7
		-	11.1
Voted in 2008 (%)	86.2	+	86.4
		-	85.9
Voted in 2010 (%)	59.9	+	60.3
		-	59.2
Voted in 2012 (%)	76.3	+	76.7
		-	75.6
Voted in 2014 (%)	43.7	+	45
		-	41.9
Observations	6559589	+	3998136
		-	2561453

Notes: Each row reports means of one variable indicated by the first column. Column "+ / -" indicates a sub-sample of counties with above (+) or below (-) mean changes of early voting days between 2008 and 2012. 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 on Turnout

	Full Sample		1-Mile Border Sample	
	(1)	(2)	(3)	(4)
Number of Days	0.0549	0.2011**	0.1338***	0.2411**
	(0.0366)	(0.0769)	(0.0348)	(0.1142)
Year Fixed Effects	Y	Y	Y	
County Fixed Effects	Y			
Individual Fixed Effects		Y	Y^-	Y^-
Year-specific Geo Fixed Effects				Y
Observations	11,532,916	11,532,916	562,616	562,616

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. 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 general elections in 2008 and 2012 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 Fixed Effects

	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 the same for each row and are indicated by 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 by 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: The Turnout Effects of Early Voting Laws: by Area of Geo Fixed Effects

	0.1	0.5	1	1.5	2	3	5	10	20
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

Notes: Each cell reports the estimated coefficient of the number of early voting days from one OLS regression with geo fixed effects. 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. Individual fixed effects have been differenced out. The sample is restricted 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 5: The Turnout Effects of Early Voting Laws: by Party

	(1)	(2)
Number of Days	-0.1083	0.0002
	(0.1088)	(0.0855)
Days × Independent	0.7773***	0.5777***
	(0.0969)	(0.0801)
Days × Democrat	0.1327***	0.1007***
	(0.0253)	(0.0190)
Individual Fixed Effects	Y	Y
Year-specific Geo F.E.	Y	Y
Sub-sample (18 years old by 2000)		Y
Observations	562616	502740

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. Columns (2) restrictd 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. All regressions include a set of 1×1 mile geo fixed effects. Individual fixed effects for turnout propensity are differenced out. 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 Gender

Inferring Gender Using National First Names

	(1)	(2)
Number of Days	0.2013*	0.1993*
	(0.1185)	(0.1171)
Days × Pr(Female)	0.0428***	
	(0.0132)	
Days × Female		0.0400***
		(0.0113)
Observations	544407	520126

Inferring Gender Using Ohio First Names

	(1)	(2)
Number of Days	0.2109*	0.1992*
	(0.1188)	(0.1157)
Days × Pr(Female)	0.0497***	
	(0.0128)	
Days × Female		0.0488***
		(0.0115)
Observations	521814	505948

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. $Pr(Female)$ 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 1×1 mile geo fixed effects. Individual fixed effects for turnout propensity are differenced out. 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 Type of Day

	(1)	(2)	(3)	(4)	(5)
Days	0.1661***	0.2191	0.3027***	0.1668***	0.1357
	(0.0603)	(0.1584)	(0.1077)	(0.0591)	(0.2359)
Weekend Days	0.0923				0.0952
	(0.1861)				(0.1976)
Days with Same Day Regis.		0.0989			0.1261
		(0.9085)			(0.9167)
Days Open Late			-0.0269		
			(0.0626)		
Saturdays				0.0428	
				(0.1851)	
Sundays				0.1253	
				(0.3130)	
Observations	562616	562616	562616	562616	562616

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. All regressions include a set of 1×1 mile geographic fixed effects. Individual fixed effects for turnout propensity are differenced out. 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: Nonlinearity

	(1)	(2)	(3)
ΔDays	0.2249***		
	(0.0717)		
ΔDays^2	-0.0034		
	(0.0095)		
min{ΔDays, 0}		0.2584*	0.1928*
		(0.1464)	(0.1004)
max{ΔDays, 0}		0.1661***	0.1881**
		(0.0603)	(0.0741)
Individual Fixed Effects	Y	Y	Y
Geo Fixed Effects	Y	Y	Y
Sub-sample			Y
Observations	562616	562616	362487

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 last column, 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 1×1 mile geo fixed effects. Individual fixed effects for turnout propensity are differenced out. Standard errors are clustered two-way by county and by county-border.
$*\ p < 0.10$; $**\ p < 0.05$; $***\ p < 0.01$.

Table 9: 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	2016	52/100	0	-1	-3*
House	2012	234/435	5	-4	-10
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

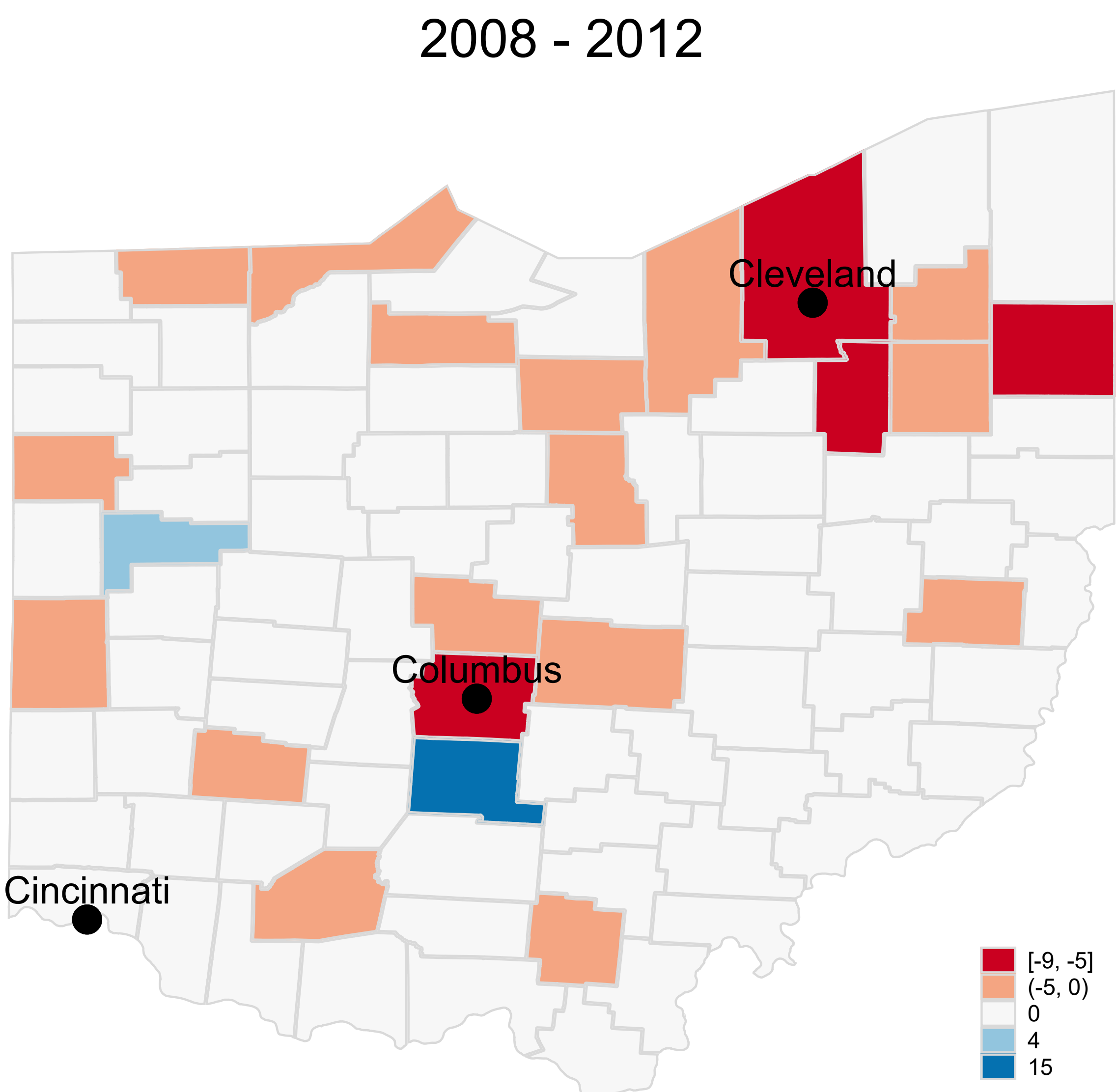


Figure 2: Full Sample of Ohio Registered Voters and One-Mile County Border Sample

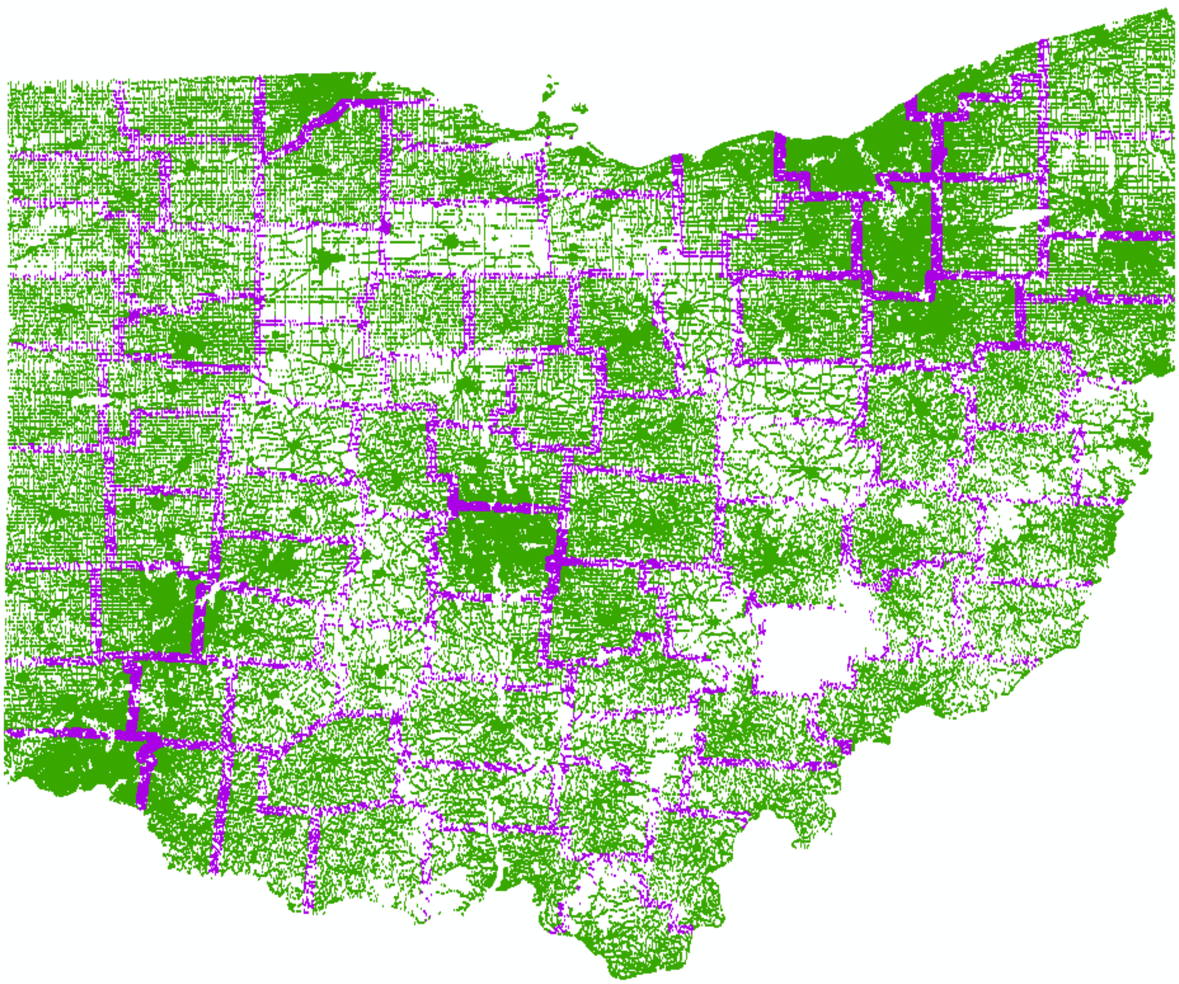
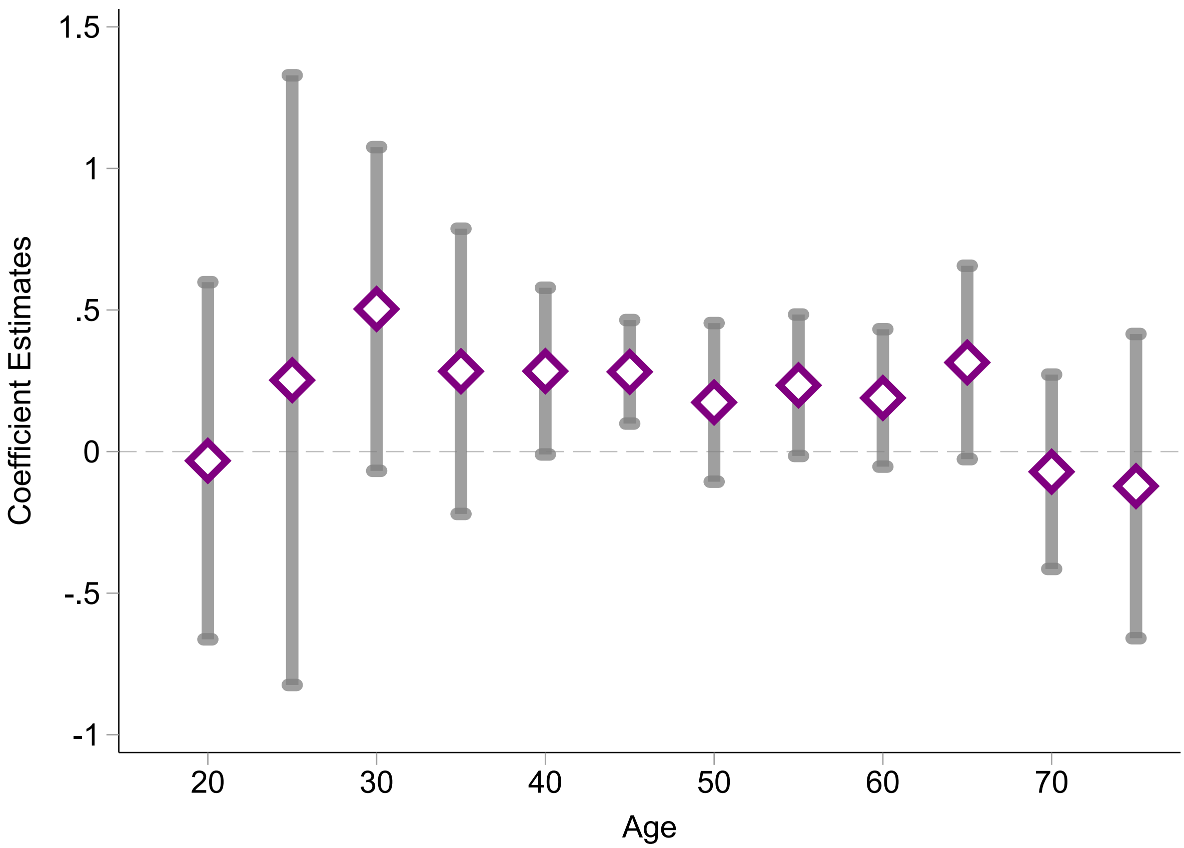


Figure 3: Heterogeneous Treatment Effects of Early Voting on Age Groups



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 the 2008 and 2012 general elections are included. Each age group spans 5 years. Individual fixed effects are differenced out and age-group-specific 1×1 mile geographic fixed effects are included. 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: Correlation between Individual Partisanship and Precinct-level Democratic Vote Share

	2008	2012
Democrat	0.571	0.548
Independent	0.297	0.38
Republican	-0.822	-0.769

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. The column header indicates the year of the election. The row headers indicate individual partisanship by past primary turnouts.

Table A.3: Partisanship by Election

Year	Democrats	Independents	Republicans
2008	38.3	38.3	23.4
2012	30.4	43.1	26.5

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. 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)

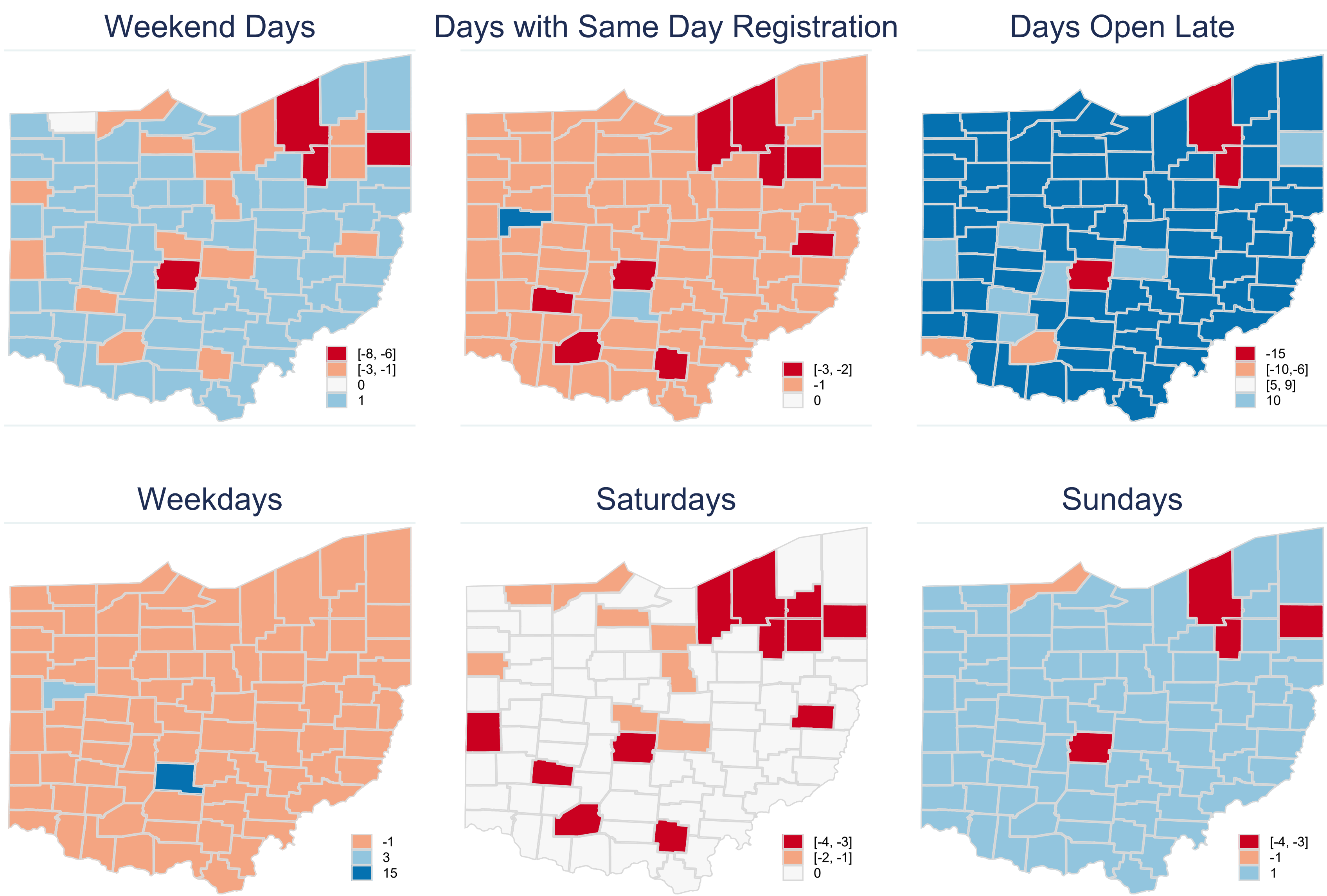
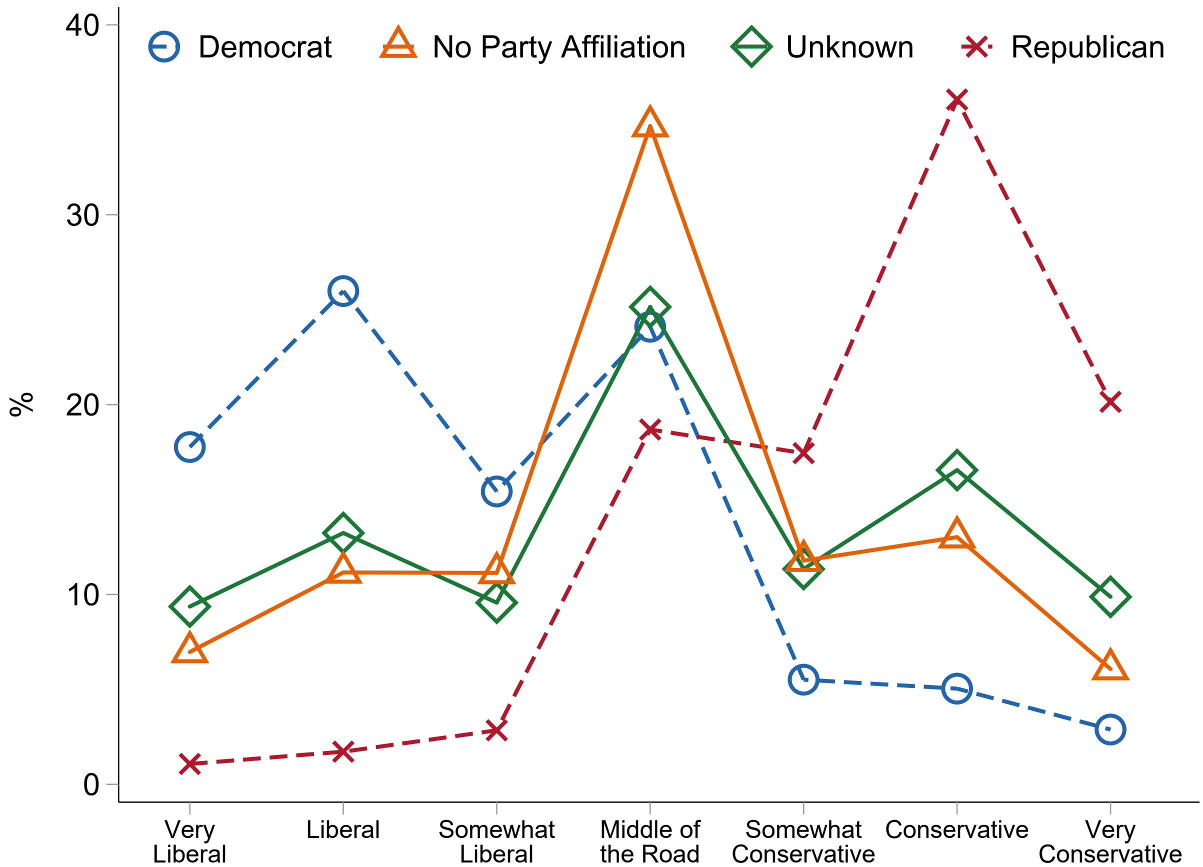
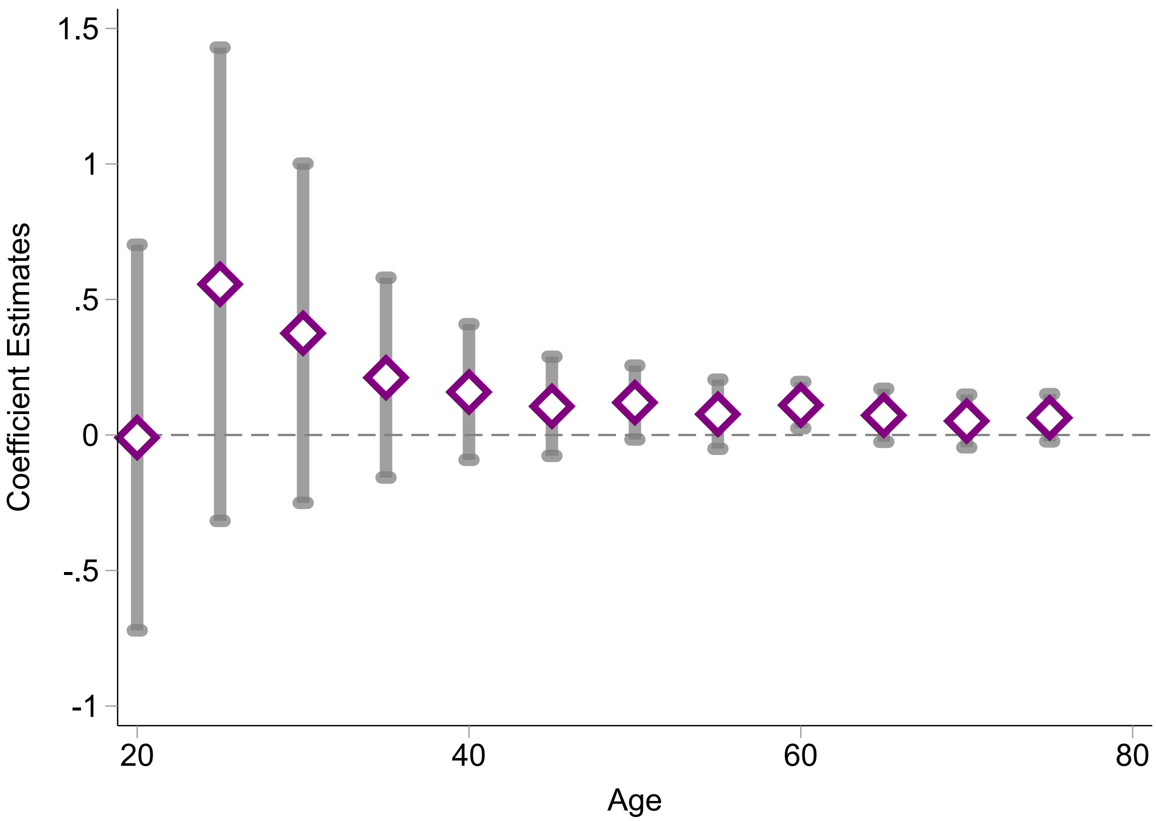


Figure A.2: Ideological Leaning by Partisan Affiliation



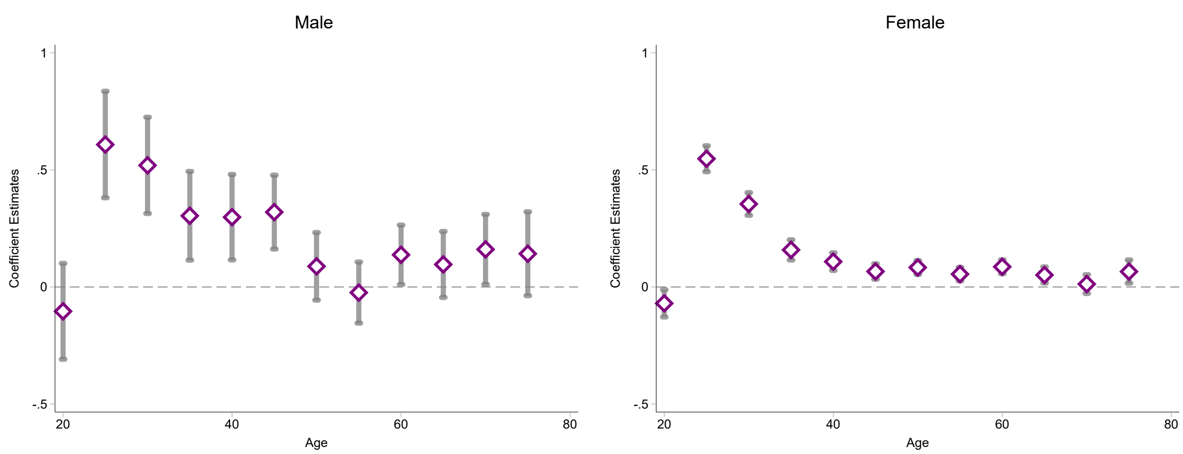
Notes: The graph above plots the shares of self-reported ideological leaning by partisan affiliation (or lack thereof) using data from the 2016 CCES.

Figure A.3: Heterogeneous Treatment Effects of Early Voting by Age Group from the Full Geographic 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 regression using the full geographic sample and a 5-year age group subsample as indicated by the horizontal axes. The specification includes 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 left subplot uses the male subsample, and the right subplot uses the female subsample. All specifications include individual fixed effects and election fixed effects. Standard errors are clustered by county.