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High School Effects on Civic Engagement

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High School Effects on Civic Engagement

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Abstract

Preparing young people for the rights and responsibilities of citizenship is cited as a fundamental purpose of public education, yet little is known about whether or how K-12 schools impact civic engagement. Using education records, birth records, and national voting records for nine cohorts of ninth-grade students in Indiana, I estimate and assess the validity of high school effects on adult voting. I find that schools have meaningful and significant effects on voting. School effects on test scores and college-going behavior are positively related to adult voting. Civic school effects are positively related to participation/performance on some civics-related AP exams.

I. Introduction

Preparing young people for the rights and responsibilities of citizenship is cited as a fundamental purpose of public education in the United States, yet low levels of civic engagement for young people suggest that schools are falling short. In the 2024 presidential election, only 42% of voters aged 18 to 29 cast a vote, compared to 63.7% of eligible voters overall (Center for Information & Research on Civic Learning and Engagement, 2024; Ballotpedia, 2024). Between 2000 and 2022, the average turnout gap between young (18-24) and older (65+) voters averaged 34 percentage points and was often even larger in local elections (U.S. Census Bureau, 2021b; Holbein & Hillygus, 2020; Hajnal & Trounstine, 2016). Gaps in voter turnout can be consequential for election outcomes, reflect (and exacerbate) existing social inequalities, and diminish the ability of our democracy to fully represent the interests of its people (Hansford & Gomez,

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2010; Fraga, 2018; Levinson, 2012). Although public schools have been called the "guardians of democracy," it is not well understood whether or how K-12 schools contribute to civic engagement.

In this paper, I present empirical evidence that where you go to high school affects whether you vote as an adult. I do this by estimating the effects of high schools in Indiana on the adult voting behavior of their students using approaches adapted from school and teacher value-added models. I focus on high schools for several reasons. First, it is usually at the end of high school that students turn eighteen, the legal voting age in the United States, and I expect proximate school experiences to be most relevant for these adult outcomes (Naven, 2019). Second, the literature on political socialization points to adolescence as a particularly important time in the development of children's political beliefs and ideals (Niemi & Sobieszek, 1977). It is also in high school that students are often required to participate in civics-related coursework, such as U.S. History or government classes (Erwin et al., 2023). I estimate school effects on voting using data on nine cohorts of 9th graders who started high school in a public school in Indiana between school year (SY) 2007-08 and SY 2015-16. I merge education records to birth records for Indiana and nationwide voting records. This allows me to link students to their adult voting records as well as to their parents and their parents' voting records, making it possible to control for parental civic engagement.

I start by presenting new descriptive evidence on K-12 schools and adult voting. First, I show that voter turnout varies widely across schools. The gap in voter turnout by age 22 between high and low turnout schools in Indiana is about 19 percentage points. Second, I show that raw differences in civic engagement are associated with school demographic and socioeconomic characteristics. This motivates the use of a rich set of controls in my school effect models. Finally, I show that measures derived from students' 8th grade educational records – such as test scores, attendance, and demographics – strongly predict adult voting, as does parental civic engagement. This supports the use of these measures as controls for my civic effect models.

I then estimate high school effects on voting and assess the magnitude, validity, and robustness of these estimates. I find that high schools make modest but meaningful contributions to civic engagement: a one standard deviation increment in school civic effects is associated with a 1.6 percentage point increase in the probability of voting by age 22, a 4% increase over the sample mean. Relative effects are larger (13%) for voting outside of general elections. I assess the validity of these estimates in several ways. First, I show that my leave-cohort-out school effect estimates are reasonably good predictors of actual outcomes. Second, I estimate forecast bias using variables I observe but do not use to calculate school effects. I estimate bias for my civic school effects of only 0.3%. Third, I use data on siblings to show that school effects on civic outcome generally remain significant predictors of actual student outcomes even within families.

Next, I consider how estimates of school effects on voting relate to school effects on measures of cognitive skills (10th grade test scores), non-cognitive skills (an index of disciplinary, attendance, course passing rate, and grade promotion), and college-going behavior (participation in the SAT or ACT). Schools that are good at improving student test scores or increasing college-going behavior also positively impact adult voting behavior, but civic effects remain strong predictors of voting even when controlling for other effects. In contrast, I do not find that school effects are positively related to effects on non-cognitive outcomes. This is surprising given recent work that has emphasized the role of non-cognitive skills for civic engagement (Holbein & Hillygus, 2020; Cohodes & Feigenbaum, 2021).

I then present descriptive analyses of the school-level factors associated with school effects on civic outcomes. The schools that are most effective at promoting civic engagement are not necessarily the schools that serve more advantaged student populations; civic effects are negatively related to test score levels and positively related to the share of low-income students in uncontrolled models. Average maternal civic engagement – a proxy for peer group civic norms – is positively and significantly related to school civic effects, pointing to a role of peer effects and community social norms as mechanisms for these effects. Finally, I consider the relationship between civic school effects and civics-related coursework and extracurricular programs using original data I collect on high school activities and data on AP exam participation and performance. I find that participation and performance on some civics-related AP exams are positively related to school civic effects. I do not find robust evidence that civics-related extracurriculars are related to school effects on voting, though my measures of extracurricular participation are crude.

This study is the first of its kind to estimate the effects of high schools on adult voting. In doing so, it adds to the slim but growing quasi-experimental literature on the effects of K-12 schools on civic outcomes. Using randomized enrollment lotteries, Gill et al. (2018) show that attending a school operated by Democracy Prep, a civics-focused charter school network, increased voter registration by about 16 percentage points and turnout by about 12 percentage points. Also using charter lotteries, Cohodes & Feigenbaum (2021) find positive effects of attending a Boston charter school on voting in a setting where prior work has found large positive effects on test scores and college-going (Abdulkadiroğlu et al., 2011; Angrist et al., 2016). In contrast, Carlson et al. (2017) find no effect of private school vouchers in New York City on voter registration or turnout. This paper is also closely related to recent work by Bell et al. (2024), who estimate the effects of colleges on voting using application portfolio controls. Most broadly, this paper contributes to the extensive literature exploring the well-documented link between education and civic engagement (Converse, 1972; Dee, 2004; Milligan et al., 2004; Sondheimer & Green, 2010). It also adds to the growing number of studies that estimate the effects of schools, teachers, and counselors on non-test and/or longer-term life outcomes (Jackson et al., 2024, 2020; Naven, 2019; Mulhern, 2023).

The remainder of this paper is organized as follows: in the next section, I present a theoretical framework and describe potential mechanisms for high school effects on voting. In section 2, I provide details on the context and discuss my data sources and measures. I present descriptive evidence on variation in voting by high school and the factors that predict voting in section 4. In section 5, I describe my approach to estimating school effects on voting. The results of my school effect analyses are presented in section 6. I conclude in section 7.

II. Theoretical Framework

Why would where you go to high school affect whether you vote as an adult? Riker & Ordeshook (1968)'s canonical model, adapted from (Downs, 1957), describes the decision to vote as follows:

$$R = pB - C + D, \tag{1}$$

where R is the probability an individual will vote, p represents an individual's sense that their vote will matter, B is the perceived benefits of their preferred candidate winning, C is the costs of voting for the individual, and D is the utility (including social utility) derived from voting.

A student's high school could positively affect R in several ways, primarily by decreasing the costs (C) or increasing the utility (D) derived from voting. The literature suggests several important potential mechanisms for these effects:

i **Cognitive Skills:** Forming political opinions, weighing the pros and cons of a candidate or policy, and understanding the procedures for voting are all cognitively demanding tasks (Ottati et al., 2002; Denny & Doyle, 2008). Completing these tasks takes less time and effort for individuals with higher levels of cognitive skills, reducing the costs of voting. Schools, which are largely organized around the development of cognitive skills, may primarily impact voting via their effects on these skills (Hansen et al., 2004).

ii **Non-Cognitive Skills:** Another way for schools to impact adult voting behavior is through their effects on non-cognitive skills (Jackson, 2018). Non-cognitive skills such as self-regulation and grit may reduce the costs of voting by making it easier for an individual to take the steps required to realize their intention of participating in an election. Holbein & Hillygus (2020) argue that non-cognitive skills are more important determinants of voting for young people than cognitive skills.

iii **Civic Knowledge and Skills:** In addition to cultivating generic cognitive and non-cognitive skills, schools can impart skills and knowledge that are specific to political engagement. A large literature has considered the relationship between civics-related coursework and adult political engagement, mostly failing to find robust evidence that civics-related courses predict voting (Jung & Gopalan, 2023; Weinschenk & Dawes, 2022; Manning & Edwards, 2014; Denver & Hands, 1990).[1]

[1] This is generally true of broad-based policies. In contrast, there are a small number of experimental and quasi-experimental studies suggesting positive effects of specific school-based interventions or programs (McDevitt & Kiousis, 2006; Donbavand & Hoskins, 2021).

iv **Peers and Social Norms:** The term "political socialization" describes the process through which a young person learns about politics, develops their political values and beliefs, and sets expectations for their future civic engagement (Niemi & Sobieszek, 1977; Sapiro, 2004). Schools, like family units, have been identified as important forums for political socialization (Campbell, 2013; Bhatti & Hansen, 2012; Andolina et al., 2003). Having peers who value civic engagement can affect the social benefits of voting (Gerber et al., 2016). Peers may also serve as a source of information or practical support – for example, by providing a ride to the polls. Campbell (2010) shows that attending a high school where students hold more "pro-civic" attitudes is associated with higher levels of voting and volunteering even after controlling for a student's own civic interests.

v **Postsecondary Education:** A vast research has demonstrated the positive relationship between educational attainment and civic engagement, with particular returns to earning a college degree (Dee, 2004; Campbell, 2009; Sondheimer & Green, 2010). K-12 schools can affect college enrollment, persistence, quality, and degree completion (Angrist et al., 2016; Mbekeani et al., 2023). Therefore, one way that high schools could affect civic engagement is via their effects on college-going and college choice.

vi **Voter Registration and Polling Sites**: High schools could also affect voting by playing a direct role in facilitating civic engagement. For example, a school could run a voter registration drive for students or could serve as a polling site, making it easy for students to participate (Tomkins et al., 2023).

III. Data, Measures, and Context

a. Civic Engagement in Indiana

Indiana is a politically conservative state in the Midwest and the 17th largest state in the United States by population. Indiana has consistently placed among the worst states in the country for voter turnout, ranking 41 out of 50 states in the 2016 presidential election and 46th in 2020 (Szarleta et al., 2023). Recently, the state has taken several substantial steps to strengthen civic education in its K-12 schools. In 2019, Indiana passed a law requiring students to take the U.S. naturalization exam as a graduation requirement. This is in addition to a requirement that high school students complete a half-year course in civics or government (Craiutu & Ngalande, 2024). In 2021, the state passed a law adding a semester-long civic education class for middle schoolers. The implementation of these requirements postdates the period of my study.

b. Data Sources and Merging

My K-12 education records include all students who enrolled in public schools in the state of Indiana between school year (SY) 2006-07 and SY 2021-2022. These records come from the Indiana Department of Education and include student enrollment, demographics, test scores, Advanced Placement exam participation and scores, attendance, and behavioral outcomes. My data also include student names and birth dates, which I use for merging. My sample is drawn from the population of students who enrolled in 9th grade for the first time between SY 2007-08 and SY 2015-16. I drop the very small number of students without complete information on first name, last name, and date of birth. I limit my sample to students born between 1988-2009 because of birth records availability. I exclude students enrolled in alternative schools, special education schools, juvenile correction schools, adult education schools, very small schools, schools that do not serve students through 12th grade, and schools that closed before 2019 or opened in 2016 or later.[2] I include students in public charter schools, though my sample of charter schools is small (n=10). For simplicity, I will refer to schools in my sample as high schools even though some may serve students in lower grades. Summary statistics for students in my initial and estimation samples are presented in Table 1. Figure 1 plots the schools included in my sample, which are spread across the state.

[Table 1 about here: Sample Summary Statistics]

[Figure 1 about here: Map]

I match students in my sample to birth records from the Indiana Department of Health. Birth records cover babies registered in the state of Indiana and include children's names and dates of birth as well as parents' names, dates of birth, and places of birth. I match birth records from 1988, the first year complete data is available, through 2009 to K-12 records using exact and fuzzy matching approaches using the fastLink package in R (Enamorado et al., 2019). Approximately 70% of students in my initial sample matched to an Indiana birth record (see Table 1), on par with ACS estimates of the share of people born in Indiana who currently reside in the state (U.S. Census Bureau, 2022).[3] Appendix B provides additional detail on the process for matching K-12 and birth records.

I match both students and their parents to national voting records from the commercial vendor L2 using name and date of birth. These records include voter registration and turnout in federal, state, and local elections, coded as turnout

[2] There are four schools in my analytic sample that appear to have opened in SY 2007-08 or later. The latest opened in 2013. Two of these were charter schools. One school appears discontinuously in the data for 9th grade cohorts, likely because of a merger with another school (Proffitt, 2010).

[3] Students who did not match to Indiana birth records may have been born elsewhere in the United States or may have been born outside the U.S. Immigrants make up 6.2% of the population in Indiana, lower than the national share of over 14% (*Immigrants in Indiana*, 2024).

indicators at the year-level. For each state, I pool together multiple years of "snapshot" cross-sectional voting records (generally covering 2017-some of 2023) to create a state-level file that contains all unique iterations of name, date of birth, and voter ID. This allows me to observe individuals who were registered to vote in a state at one point in my files but were later removed because they moved out of state. Pooling together multiple cross-sectional files also allows me to identify individuals who register to vote under one name but change their name later, as can be the case for women who marry.[4] I use both fuzzy and exact matching to match to records in Indiana and exact matching only for other states. For students/parents who matched to voting records in more than one state, I consider whether these voting records could refer to one individual who moved, collapsing or discarding records iteratively and giving preference to states that were more likely destinations for individuals born in Indiana based on 2022 American Community Survey estimates (U.S. Census Bureau, 2022). Using these approaches, I match 76.4% of students in my initial sample to voting records (column 1). The vast majority (96%) of individuals in my merging sample who matched to voting records matched to an Indiana voting record. Appendix C provides additional detail on the process for matching to voting records. Appendix Table A1 breaks down match rates for birth and voting records by years of birth.

My estimation sample (column 2) for my school effect models is limited to students who have at least one non-missing test score from 8th grade and match to maternal birth records. I limit my sample to individuals who match to birth records so that I can control for parental voting and to ensure that students would be eligible to vote based on birthright citizenship. I assess whether my results are sensitive to excluding students without birth records as a robustness check. I exclude students from schools with very small cohorts. I also limit my estimation sample to individuals who were at least 18 years old by November 1, 2019. This ensures I am able to observe the individual's voting behavior for at least four election cycles.[5] I exclude the very small number of individuals who reported being over the age of 18 at the time they were first observed in 9th grade. This leaves me with a sample of approximately 456,700 unique students in 335 schools. I also identify a subsample of siblings who attended at least two different schools (column 3). I use these students to estimate within-family relationships between school civic effects and voting to assess the validity of my school effect estimates.

[4]Per correspondence with L2, the L2 voter ID is designed to remain constant within an individual/state even if the individual changes their name, allowing me to make these links. Anecdotally, I do observe some voter IDs that appear to refer to the same female before/after a name change, though it is difficult to assess whether L2 catches all such instances and the quality of links may vary by state.

[5]I define the first age-eligible election cycle as the first year an individual is at least 18 years old by November 1. This is based on the fact that most high-stakes election are held in early November. I define the first age-eligible general election as the first even numbered year an individual is at least 18 years old by November 1.

c. Measures

I measure civic engagement using an index of voter turnout over the first four cycles an individual is eligible to vote. The measures I use to construct this index are as follows: (1) an indicator for having registered to vote within the first four election cycles (based on registration year), (2) indicators for voting in each of the first and second age-eligible elections, (3) number of times (years) voting in a primary election in the first and second age-eligible even years, (4) number of times voting in any election in the first and second age-eligible odd number election years, and (5) number of times voting outside of primary or general elections in the first two even years.[6] For simplicity, I sum these variables together and then standardize the measure to have a mean of 0 and a standard deviation of 1 in the sample. I refer to this as the civic score or civic index.

I construct a similar measure of maternal and paternal prior civic engagement by summing together an indicator for being registered to vote before the child's first age-eligible election, indicators for voting in the last four general elections before the child's first age-eligible election, and the number of times (years) the parent voted outside of general elections in the last eight election cycles before the child's first age-eligible election.[7] Summary statistics and additional detail for outcome measures are presented in Appendix Table A2. Correlations across the measures used for these civic indices are presented in Appendix Tables A3-A5. Graphs of these measures are presented in Appendix Figure A1.

Test score value-added and control measures are based on state standardized assessments. Lagged (grade 8) scores come from the Indiana Statewide Testing for Educational Progress (ISTEP) assessment, which was administered to students in grades 3-8. I use only grade 8 test scores for tests taken the year before the first 9th grade year. The ISTEP was administered in the fall of each year until 2009, when administration switched to the spring; for this reason, lagged scores for students in my first two 9th grade cohorts come from the fall while all others are from the spring. I assess sensitivity to this as a robustness check. The high school assessments administered in Indiana changed over the time of my study. From SY 2009-10 to 2014-15, Indiana administered end-of-course (ECA) assessments in English 10 and Algebra I. Passing these was a requirement for graduation, though waivers were also available (Wang, 2014). ECA assessments were administered at the time a student took the course; as such, if a student took Algebra I in 8th grade – as was the case for approximately 26% of individuals in my sample – they took this assessment for the first time in 8th grade. In contrast, about 95% of students who took the English 10 exam took it in 10th grade. For this reason, I show results using the English 10 exam only for

[6]The "number of times" indicators vary between 0 and 2, where 2 indicates that individual participate in this type of election in both of the even (odd) years, 1 indicates participating in this type of election in only one year, and 0 indicates the individual did not participate in this type of election in either of the two years. These variables come from the L2 uniform reporting files.

[7]These variables are defined at the child-level, so siblings who share the same parents could have different values of these observations for different children.

assessments taken in grade 9 or later. The assessment changed from SY 2015-16 to 2018-19 and was different in SY 2009-08, so high school test scores are only available for six of the nine cohorts in my sample. I standardize all test scores to have a mean of 0 and a standard deviation of 1 within grade/year.

I create an index measure of non-cognitive skills modeled after Jackson (2018). The components of this index are as follows: the natural log (hereafter, "log") of unexcused absences in grade 9 plus one, the log of suspensions in grade 9 plus one, an indicator for being expelled in grade 9, the pass rate of courses taken in grade 9[8], and an indicator for progressing to grade 10 on time. Course pass rates are only available for students who entered 9th grade for the first time between 2012-2016; thus, the non-cognitive index is available for only five of the nine cohorts. I replace missing unexcused absences from grade 9 with the mean. I use principal components analysis to create an index from these measures using the first component and then standardize the measure to have a mean of 0 and a standard deviation of 1. A higher value indicates better non-cognitive outcomes. Correlations for the variables used for this index are presented in Appendix Table A6. The distribution of this index is shown in Appendix Figure A1.

Finally, I also estimate school effects on taking the SAT or ACT, which I consider to be a reflection of college-going behavior. I count only tests taken within four years of enrolling in 9th grade. About 24% of students in my analytic sample took the ACT and 54% took the SAT. The SAT became a requirement for high school students beginning with the class of 2023 but neither the ACT nor SAT was required in Indiana during the period of my study (Appleton, 2021).

IV. Descriptive Evidence

In this section, I present descriptive findings on K-12 schools and voting that motivate the rest of my analysis. My first finding is that adult voting varies substantially across high schools, as shown Figure 2. In schools at the 90th percentile of the distribution, 46% of students vote at least once by the time they are 22, compared to only 27% at schools at the 10th percentile, a 19 percentage point gap (Panel A). A similar gap (15 percentage points) exists for rates of voter registration (Panel B).

[Figure 2 about here: raw voting outcomes by school]

My second finding is that raw differences in adult voting across schools are associated with school-level characteristics. Appendix Figure A2 plots the share of students at a school who voted by age 22 against the share of students who qualify for free or reduced-price lunch, the share of Black students, average

[8] I define this as the number of passing grades received over all courses taken. I do not count "no grade awarded" classes as passing as online guidance suggests this is typically given when a student drops a course or transfers before completing the credit. I exclude from this measure a small number of students with very large or very small numbers of courses recorded.

English 10 test scores, and the average number of suspensions for 10th graders. The directions of these relationships track with measures of student advantage: schools with more low-income or Black students have lower rates of adult voting behavior, while schools with higher test scores and fewer disciplinary incidents have higher rates. This motivates the use of controlled models to identify school effects on civic outcomes.

[Table 2 about here: predict voting]

My third finding is that measures from student educational records and parental voting are strong predictors of a student's adult voting behavior. Table 2 shows the output of a series of regression models predicting whether a student votes in their first age-eligible general election using these measures. A one standard deviation increase in grade 8 ELA test scores increases the probability of voting in the first general election by 4.7 percentage points. Having a mother who is registered to vote is associated with a 7.9 percentage point increase in the probability of voting, a 34% increase over the sample mean (22.9%). This supports the premise that the measures available in my data are reasonable controls for my civic effect models.

V. Methods

a. Estimating School Civic Effects

I am interested in the causal effect of the high school a student attends on their adult voting behavior. This can be modeled as follows:

$$Y_i = \alpha + \beta X_i + \nu_t + \theta_e + \mu_s + \epsilon_i, \tag{2}$$

where Y_i is an index of adult voting behavior for student i who first enrolled in 9th grade in year (cohort) t in school s and was eligible to vote for the first time in election cycle e. X_i is a vector of controls, which includes student, parent, and school-cohort controls. ν_t is a vector of cohort dummies for the year in which a student started 9th grade and μ_e is a vector of dummies for a student's first age-eligible election.[9] By including both cohort and first election fixed effects I am able to control for any common shocks that affect political participation for young people across the state, such as increasing political polarization, changes in voter registration laws, or becoming age-eligible to vote for the first time ahead of a hotly contested election. This is relevant given that I have some imbalance in my panel across schools, as mentioned. In this equation, μ_s represents the effect of the high school on adult voting.

Identification is based on a "selection-on-observables" approach, drawing on the vast value-added literature (Chetty et al., 2014; Mulhern, 2023; Cunha &

[9] There is variation in election eligibility even among students who enroll for the first time in 9th grade as part of their expected age group, since students who were born in September or October would turn 18 by November 1st one calendar year before their peers. There are also students who are older or younger when they appear for the first-time in 9th grade.

Miller, 2014; Mountjoy & Hickman, 2021). The key assumption of this model is one of conditional independence: to interpret these estimates as reflections of a school's causal effect on a civic outcome, it must be the case that assignment to schools is uncorrelated with students' expected civic outcomes, conditional on the controls included in the model. Deming (2014) and Angrist et al. (2017) show that school value-added measures can produce unbiased or minimally biased measures of school causal effects on student test scores.

In a conventional value-added model, X_i would include lagged values of the outcome. I can't control for lagged outcomes because students are typically not eligible to vote until the end of high school. Instead, I follow the growing literature that estimates effects on longer-term outcomes by conditioning on other baseline characteristics that predict the outcome (Petek & Pope, 2023; Mulhern, 2023; Naven, 2019, 2020). Thanks to my links across K-12, birth, and voting records, I am able to assemble a rich set of controls, including those I show are predictive of adult voting (see Table 3). The student-level controls included in X_i include student demographics (race/ethnicity; gender; special education, English learner, and free or reduced-price lunch status; age at the start of 9th grade); lagged test scores and behavioral outcomes (i.e., polynomials up to cubics of lagged 8th grade test scores in math and English and an indicator for missing either math or ELA scores[10], lagged log counts of unexcused absences from the past year plus one set to zero if missing, lagged log counts of suspensions from the past year plus one, an indicator for missing attendance); and cohort size. By linking children to parents and parental voting records, I am also able to control for parent characteristics. These include an indicator for matching to a father on the birth record and maternal/parental indices of voting based on elections before the child's first age-eligible election.[11] Controlling for parental civic engagement is particularly important because a large literature shows that whether a parent votes strongly predicts whether a child grows up to become a regular voter (Gidengil et al., 2016).

Finally, I also control for characteristics of a student's county of birth. I control for these characteristics to disentangle school effects from the effects of growing up in a particular community in a particular time. This is relevant given that where you go to school is largely determined by where you live and there may be place-based effects on civic norms (Campbell, 2010). Specifically, I control for a contemporary measure of county-level urbanicity/rurality and county-level measures of educational attainment (percent B.A. or higher) and poverty rates from around the time of the student's birth.[12] I also include three county-level political measures derived from data from the National Neighbor-

[10]I replace missing math or ELA scores with the score in the opposite subject to minimize missing data.

[11]This index is set to 0 for paternal voting for individuals who do not match to a father's birth record.

[12]Urbanicity/rurality indicator and percent B.A. or higher come from the USDA (U.S. Department of Agriculture, Economic Research Service, 2025). I use years 1980 rates for birth years 1980-1984, 1990 for 1985-1994, and 2000 for 1995-2004. County-level poverty rates come from the U.S. Census Bureau (U.S. Census Bureau, 2021a). I use 1980 estimates for birth years 1980-1984, 1990 rates for 1985-1995, 2000 rates for 1995-2004.

hood Data Archive (NaNDA) (Clary et al., 2024) and the MIT Election Lab (MIT Election Data and Science Lab, 2018): (1) the average turnout in the county as a share of eligible voters in the 2004, 2006, and 2008 general elections; (2) an index measure of Republican partisanship in 2006 calculated by NaNDA based on the percent of votes cast for Republican candidates in the past six years; and (3) a measure of political competitiveness in the county based on MIT Election Lab returns for the 2000, 2004, and 2008 presidential elections.[13] I also include an indicator for individuals who are missing county-of-birth (about 2.5% of observations that match to birth records).

I estimate school effects on civic outcomes in two steps. In my first step, I estimate Equation 1 as written and calculate student-level residuals. I include school fixed effects in Equation 1, following (Chetty et al., 2014), to account for correlations between school effects and the other controls included in the model. I exclude school effects when estimating student-level residuals:

$$\hat{Y}_i = Y_i - (\hat{\alpha} + \hat{\beta}X + \hat{\nu}_t + \hat{\theta}_e) \tag{3}$$

As constructed, the residual $\hat{Y}_i$ includes both the "true" school effect and an error term. Taking the average of these empirical residuals by school and cohort yields $\bar{Y}_s^t$, a vector of average student-level residuals in a school for all cohorts. Under the assumption that $\bar{Y}_s^t$ is not related to any unobserved determinants of student voting, $\bar{Y}_s^t$ is an unbiased estimate of the effect of school s on civic engagement for students in cohort t.

I estimate $\hat{\mu}_{ts}$ using Chetty et al. (2014)'s approach to modeling value-added with "drift," which allows school effects to evolve over time. Allowing for drift in school effects estimates may be appropriate give that changes in a school over time, such as hiring a particularly motivated social studies teacher or getting a new principal, may impact school effectiveness. This approach estimates a school's value-added based on school effect estimates in other cohorts. Let $\bar{Y}_s^{t-1}$ be a vector of $\bar{Y}_s^t$ for all cohorts excluding cohort t. The estimated school effect can then be expressed as:

$$\hat{\mu}_{st} = \phi \bar{Y}_s^{t-1} \tag{4}$$

The weights given to each cohort's estimate are higher for estimates in cohorts that are more closely correlated with the prediction cohort, usually from more proximate years. This approach increases precision of estimates and generates a leave-cohort-out measure that minimizes mean squared forecast errors.[14]

[Table 3 about here: year-over-year correlations]

[13] My measure of political competitiveness is defined using a Herfindahl-Hirschmann index of the concentration of party vote shares in each election, which is defined as the sum of the (squared) share votes for Republican, Democrat, Green Party, and Libertarian candidates in the presidential elections in 2000, 2004, and 2008. I define the measures as one minus the HHI so that a larger value indicates a more politically competitive county. I take the average of this measure over the three general elections and standardize the measure to have a mean of 0 and a standard deviation of 1 in the sample.

[14] I implement this approach using the *vam* function command in Stata (Stepner, 2013). I set drift limits according to the availability of data for each outcome variable. See Table 3.

Table 3 shows correlations of school effect estimates for cohort t and earlier cohorts. The correlations for civic school effects range from 0.68 for the prior cohort $(t-1)$ to 0.40 for $t-8$. I standardize these school effect estimates to have a mean of 0 and a standard deviation of 1 among school-cohort estimates. I use these standardized estimates throughout, except where noted. I use the same approach and controls to estimate school effects on test scores, non-cognitive measures, and participation in college entrance exams. School effects on test scores and non-cognitive measures are less highly correlated over time. The school effects I estimate reflect the combined impacts of all school-based inputs, including school leadership, teacher quality, curricular and extracurricular programs, and peers.

In Appendix Table A8, I examine correlations across alternative specifications of the civic effects model. My preferred estimates are highly ($\rho > 0.95$) correlated with estimates from several alternative approaches, including models estimated using individuals who do not match to birth records (with maternal and paternal civic score imputed), estimates that limit "drift" to three periods, and models that add peer cohort controls. My preferred estimates are less correlated with models that do not residualize on school effect or that drop cohort dummies.

b. Out-of-Sample Predictions and Forecast Bias

I assess the validity of my school civic effect estimates in several ways. First, following Chetty et al. (2014) and others (e.g., Mulhern, 2023; Naven, 2019), I assess whether these school effects, which are constructed as leave-cohort-out estimates, are good predictors of actual student outcomes. To do this, I regress the residualized student outcome $\hat{Y}_i$ on the (unstandardized) school effect estimate for that outcome. I use the residualized outcome instead of the raw outcome because student characteristics that predict actual outcomes can be correlated with school effectiveness, as would be the case if higher-performing students attended more effective schools. A coefficient of 1 on the school effect estimate would suggests the school effect perfectly predicts actual outcomes. I present the results of this analysis for each of my school effect estimates in Panel A of Table 4. Point estimates are indistinguishable from 1.

[Table 4 about here: actual versus predicted]

Second, I estimate to what extent these estimates are biased by omitted variables. I do this by predicting civic outcomes using variables that are available in my data but are not used as controls in my school effect estimates: 7th grade test scores in ELA and math, squares and cubics of these scores, and an indicator for being the child of a parent who was born outside the United States.[15] I replace missing scores in one subject with the mean across other non-missing subjects. I residualize these variables in the same way I residualized the student outcome $\hat{Y}_i$. I then predict the residualized outcome $\hat{Y}_i$ using these residualized

[15] I identify approximately 5% of students in my sample as being the child of an immigrant.

predictors. Finally, I regress the predicted outcome on the (unstandardized) school effect measure for that outcome and report the coefficient on the school effect measure in Panel B of Table 4. Under the assumption that these omitted variables are the only source of bias in my estimates, the point estimates can be interpreted as the proportion of variation in the school effect estimates that is actually attributable to omitted variables. Chetty et al. (2014) refer to this measure as "forecast bias."

I find minimal evidence of forecast bias for my civic effect estimates. Point estimates for the civic school effects indicate 0.3% bias. My estimates of bias for non-cognitive and college entrance exam participation are also very low, though estimates of forecast bias are higher (2.3%) for the test score value-added. As a point of comparison, Chetty et al. (2014) estimate forecast bias of 2.2% for teacher test score value-added measures and Naven (2019) estimates forecast bias of 0.9% (middle) to 3.9% (high school) for school-level test score value-added measures.

VI. Results

a. Civic School Effects on Adult Voting

High school effects on civic engagement relate to meaningful differences in adult voting. Table 5 estimates the relationship between high school effects on the civic index measure and student voting outcomes. A one standard deviation increase in school civic effects is related to an 1.6 percentage point increase in the probability of voting by age 22, a 4% increase over the sample mean. A standard deviation increment in school effects translates to a 1.4 percentage point (6.1%) increase in voter turnout for the first age-eligible general election (column 3). The magnitude of this point estimate is equivalent to about 17% of the magnitude of the predicted increase in the probability of voting associated with having a mother who is a registered voter or about 30% of the predicted increase in voting for a one standard deviation increase in ELA scores, per estimates in Table 2. Relative effects are even greater for voting outside of general elections, where point estimates translate to a 13.2% increase over the sample mean, and voting in odd-year elections (11.4%). The relationship to voter registration is not statistically significant, though voter registration rates are already high in my sample (80%).

[Table 5 about here: actual effects]

b. Assessing Validity Using Siblings

In Panel B of Table 5, I present additional evidence on the validity of my school civic effects by estimating the relationship between school civic effects and student outcomes *within families.* I identify individuals in my sample as siblings in the same family if they report the same mother based on first name, last name, and date of birth. There are 201,275 individuals with siblings in my sample paired to 92,802 unique mothers. Since I only identify two individuals as siblings if they are both part of my analytic sample, this is a lower bound estimate of family size. Of these, there are 24,944 children with 11,128 mothers in families where at least one child attended a different 9th grade school. This is the sample I use for my within-family estimates. Three of the six within-family point estimates remain positive and statistically significant at the $p < 0.05$ level and a fourth is significant at the $p < 0.10$ level. I am unable to explain the marginally significant negative coefficient in column 6 (voting in an odd-year election).

c. Robustness Checks

Table 6 presents results from a series of robustness checks. I show results for three of the outcomes from Table 5 for brevity. Column 1 shows estimates without controls. Column 2 adds controls for the characteristics of the school the student attended in 8th grade as well as school cohort effects as a way to further disambiguate "place" and "school" effects.[16] Results are effectively unchanged. In column 3, I drop individuals with extremely common names, defined as having both a first and last name in the 75th percentile of names in my sample. These individuals might be more likely to match to voting records in multiple states, which could bias estimates. In column 4, I drop individuals who match to voting records in more than 2 states. Again, results are effectively unchanged. In column 5, I restrict my sample to students who switched schools between 8th grade and 9th grade. In column 6, I show results using civic school effects estimated in a sample that does not drop individuals who do not match to birth records but instead imputes maternal and paternal civic engagement using student-level demographics and lagged test scores.[17]

[Table 6 about here: robustness]

VII. Explaining Variation in Civic Effects

a. Effects on Other Outcomes

To assess whether schools that are effective in other ways are also good at increasing civic engagement, I consider whether effects on non-civic outcomes also predict adult voting. Table 7 presents results from a regression analyses that predicts whether an individual votes by age 22 using these other types of school effects. Schools that raise test scores and schools that increase participation in college entrance exams both also increase the probability of adult voting. A

[16] About 12% of students attended the same school in grade 8; for these students, the school-level controls would be cohort-level controls for school characteristics in their 8th grade year.

[17] The variables I use to impute maternal and paternal civic engagement are: race/ethnicity (Black, white, Hispanic, Asian); gender; indicators for free or reduced-price lunch status, English learners, and special education status; age at start of 9th grade; grade 8 math and ELA scores (including squares and cubics); lagged (log) unexcused absences; lagged (log) suspensions; and indicators for missing unexcused absences.

one standard deviation increase in effects on either test scores or college exam participation is associated with a 0.6 percentage point increase in the probability of voting by age 22, about 38% the size of the point estimate for civic school effects from Table 5 (column 2).[18]. When controlling for all three school effect estimates, test score effects remain statistically significant predictors of voting, with point estimates about 63% the size of the estimate for civic effects in the same model (column 7). Controlling for effects on other outcomes does not greatly diminish the relationship between civic school effects and adult voting outcomes.

[Table 7 about here: predicting civic outcomes w/other school effects]

School effects on non-cognitive outcomes are not significantly related to voting. To determine whether this finding is sensitive to the way the non-cognitive outcome measure is constructed, I consider relationships to alternative measures constructed using only grade 9 behavioral measures (log unexcused absences, log suspensions, an indicator for being expelled) and another that drops the indicator for progressing on-time to 10th grade from the primary index measure. Neither is positively related to adult voting in models with student controls.

To examine correlations across school-level estimates, I average the cohort-level estimates of school effects over the students in my sample. This gives more weight to effect estimates based on cohorts with more observations and allows me to summarize a school's effect on an outcome with a single estimate. Appendix Figures A3 and A4 show the correlations between average school effects on civic outcomes and other average school effects in the sample. School civic effects are weakly positively associated with school effects on test scores and more strongly positively associated with school effects on college exam participation but have a weak negative association with effects on non-cognitive outcomes.[19] I use these average school effect estimates for my remaining analyses.

Panel A of Table 8 summarizes school-level characteristics by quartile of (average) school civic effects (columns 1-4). Column 5 presents the coefficient and standard error on the coefficient for the school-level characteristics in a simple regression predicting civic effects using that characteristic and no other controls. Column 6 does the same including basic controls (see table notes for details). Effects on other outcomes are not significantly related to school civic effects in controlled models, though average effects on taking the SAT/ACT are strongly positively related to school civic effects in the uncontrolled model and average effects on non-cognitive outcomes are negatively related (column 5).

b. School Characteristics

School civic effects are also associated with school and community characteristics, as shown in Panel B of Table 8. If anything, civic school effects appear

[18]Since test score effects cannot be calculated for all cohorts due to data availability, these estimates come from different samples

[19]Jackson et al. (2024) note that since school effects are measured with error, these correlations likely understate true correlations.

to be negatively related to indicators of student advantage, including the share of free or reduced-price lunch eligible students and average test scores. Charter schools have higher average levels of civic effects than non-charters, though this relationship disappears when controlling for other school attributes. School size is negatively related to school civic effects. Maternal civic engagement, which I interpret as a measure of peer civic norms, is strongly positively related to civic outcomes: a one standard deviation increase in average maternal voting is associated with a 1.66 standard deviation increase in school civic effects (column 6). County-level voter turnout does not significantly predict civic school effects, but schools in more politically competitive and less Republican counties tend to have larger civic effects. Serving as a polling site does not significantly predict civic school effects in my models, though point estimates are positive.[20]

[Table 8 about here: quartiles

c. Advanced Placement Exams

Advanced Placement (AP) is a program that offers college-level coursework to high school students and provides opportunities to gain college credit by passing subject-specific exams. About 80% of public high school students in the U.S. attended a school that offered at least five AP courses as of 2023-24 and more than a third of graduating students took at least one AP exam (College Board, n.d., 2025). Of the 335 schools in my sample, 310 recorded at least 10 AP exams on average each year in my study period.[21] I focus my analyses on the 10 most popular exam subjects, which collectively represent over 75% of exams taken in my records. These subjects (in order of popularity) are: English Language, U.S. History, Calculus AB, English Literature, Biology, Psychology, Chemistry, U.S. Government and Politics, World History, and Statistics. Of these, there are two that most directly address civics-related content: U.S. Government and Politics and U.S. History.

I find some evidence that participation and performance on civics-related AP exams are associated with school effects on civic outcomes, as shown in Table 9. Columns 1-4 model the relationship between participation in AP exams and civic school effects in a school-level dataset. Participation on the U.S. History exam is positively related to civic effects, even when controlling for participation on other AP exams, (log) numbers of AP exams taken at the school overall, and school/county controls (column 4). Participation on the U.S. Government exam is not significantly related to school effects on civic outcomes, though point estimates are positive. Participation on the AP Biology exam is also significantly related to school effects on civic outcomes, though this would not be considered a civics-related assessment. Columns 5-8 examine the relationship between average scores on AP exam among test-takers and school civic effects.

[20] I identify polling places using data from 2012-2020 (Public Integrity, 2024), which I match to schools in Indiana by address and name. I flag a school as a polling site if it ever appeared as a site in these data.

[21] Averaged over the students in my sample, as described for Table 8.

Since not all schools participate in all subjects, the number of observations declines as additional subjects are added as controls. Scores on the AP US Government exam are significantly and positively related to civic effects. No other subject is significantly related to civic effects at the $p < 0.05$ level, though scores on AP Statistics are significant at the $p < 0.10$ level.

[Table 9 about here: AP exams

d. Extracurricular Activities

Finally, I look at the relationship between extracurricular activities and civic effects. I collect data on contemporary and/or historic participation in 23 different extracurricular activities and create shool-level indicators for the presence of each activity at the schools in my sample. Drawing on the literature on civic education, I identify seven of these activities as "civics-related" extracurricular programs: debate (Bradley & Roland, 2022), newspaper (Reichert & Print, 2018), National History Day (Quigley, 1998), mock trial (Bengtson & Sifferd, 2010), We the People (Owen & Irion-Groth, 2020), and the Indiana Legislative Youth Advisory Council and U.S. Senate Youth Program.[22] Appendix Table A9 provides detail on each of the civic and non-civic extracurriculars in my data.

[Table 10 about here: extracurriculars

I do not find significant evidence that civics-related extracurriculars are related to school effects on civic engagement. Table 10 presents the results of a series of regressions that relate school civic effects to school-level measures of extracurricular presence by activity type. Having any civics-related extracurricular activities at a school is positively related to school civic effects, even when controlling for the presence of other types of activities (column 2). This finding, however, becomes statistically insignificant when controlling for other school-level characteristics. The number of civics-related extracurricular activities is not significantly related to civic effects (rows 4-6). Appendix Figure A6 shows point estimates relating specific activities to school civic effects.

VIII. Discussion

In this complex political era, policymakers are looking to schools to help students develop the tools needed to improve civic discourse, mitigate the threat of misinformation and polarization, reduce political fatalism, and enhance the quality and durability of our democracy. This paper cannot speak to whether schools are up to this work. Instead, it offers foundational evidence that schools can and do affect whether their students vote, a minimal (but fundamental) expression of civic engagement.

[22] Four of these activities (mock trial, We the People, the Indiana Legislative Youth Advisory Council, and the U.S. Senate Youth Program) are coordinated by the Indiana Bar Foundation, a civic education organization.

Using data from education, birth, and voting records for students in the state of Indiana, I identify significant and robust effects of high schools on voting. Effect estimates are large enough to be practically meaningful, if not large enough to fully address gaps in youth turnout: the predicted impact on voting in the first age eligible general election associated with attending a school with a one standard deviation higher civic effect measure is equivalent to about 6% of the voter turnout gap between voters aged 18-24 and voters aged 25-44 in the 2020 presidential election (23.1 percentage points) (USAFacts, 2024). Effects are even larger for voting outside general elections.

School effects on student test scores and college exam participation also predict adult voting, though these other effects do not fully explain school effects on civic outcomes. In contrast, effects on non-cognitive skills do not relate to civic outcomes. While prior work has shown that school effects on one type of outcome are not necessarily correlated with effects on others (Gershenson, 2016; DeAngelis, 2021), this is somewhat surprising given that other work on schools and voting has found non-cognitive skills are an important potential pathway (Cohodes & Feigenbaum, 2021; Holbein, 2017; Holbein & Hillygus, 2020). One explanation for these divergent findings is that the kinds of non-cognitive skills that facilitate voting are largely developed by the time students enroll in high school. This would explain why my findings diverge from prior work looking at interventions for younger students. Another potential explanation is that measures of behavior in high school reflect on both student and school actions in ways that make school effect estimates difficult to interpret. Cohodes & Feigenbaum (2021) note, for example, that the same Boston charter schools that produce positive effects on test scores have negative effects on an index of non-cognitive outcomes because these charters also have strict disciplinary policies.

I find strong descriptive evidence that parental civic behavior relates to school civic effects, suggesting a role for peers as a mechanism for school effects. Future work should investigate whether civic norms set by teachers, administrators, and other school staff also affect student voting. I also identify significant relationships between participation and scores on the AP U.S. History and AP U.S. Government and Politics exam and school effects on voting. This last finding will be of particular interest to civic educators and policymakers looking to impact civic engagement through civics-related coursework. While I do not find robust evidence of a link between school-level measures of civic extracurricular activities and school civic effects, future work should look at relationships between individual-level measures of participation and adult voting.

In conclusion, this paper provides first-of-its kind evidence that schools can impact democratic participation and sheds light on potential mechanisms for these effects, motivating further research. While schools cannot single-handedly solve lagging youth voter turnout, this study provides reason for optimism about the potential of K-12 schools to positively impact civic outcomes.

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Figure 1: High Schools in Sample

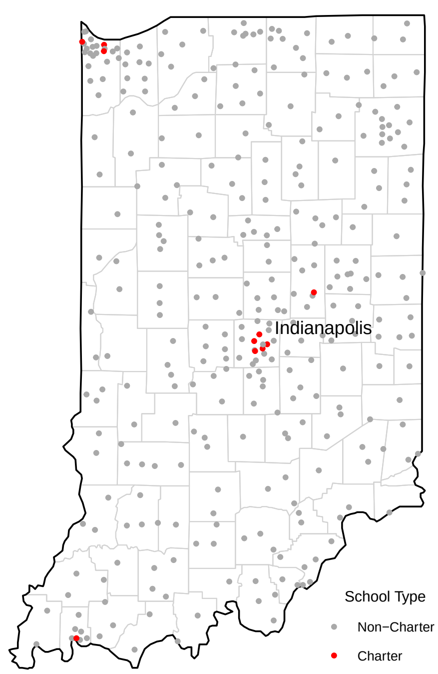
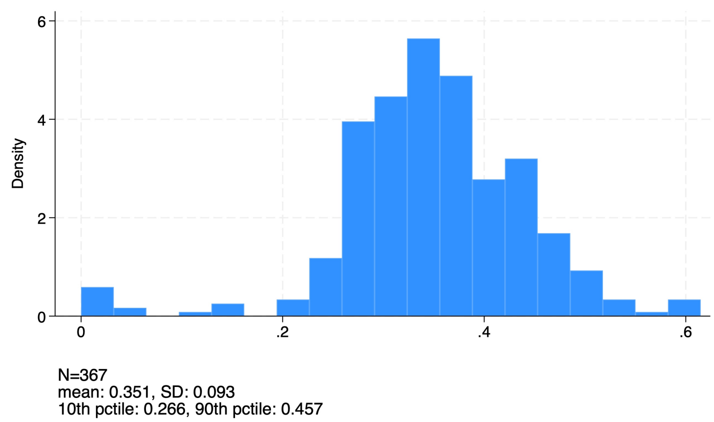
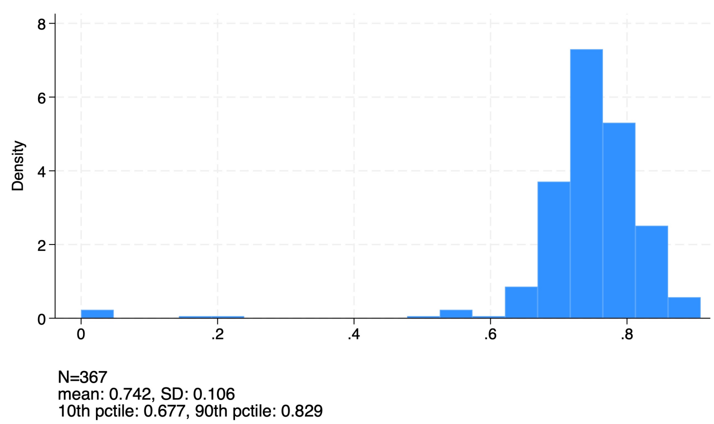


Figure 2: Raw Differences in Voting Outcomes by School

(a) Share: Voted by Age 22



(b) Share: Registered to Vote



Distribution of schools. Shares are calculated including individuals who did not match to birth records/prior test records.

Table 1: Sample Summary Statistics

	All	Sample	Sibling Sample
	(1)	(2)	(3)
N Students	689,188	456,695	24,944
N Schools	367	335	335
N Charter Schools	21	10	10
N Metro/Suburb	164	143	143
N Rural/Town	203	192	192
A. Student Characteristics			
Share Non-Missing 8th Gr Tests	0.932	1.000	1.000
Share White	0.767	0.840	0.760
Share Black	0.098	0.073	0.136
Share Hispanic	0.078	0.046	0.049
Share Free/Reduced-Price Lunch	0.409	0.366	0.581
Share Special Education	0.143	0.119	0.163
Share English Learner	0.029	0.008	0.009
B. Lagged Outcomes			
Gr 8 Math Scores	0.029	0.069	-0.248
	(0.982)	(0.962)	(0.973)
	[640,347]	[455,248]	[24,805]
Gr 8 ELA Scores	0.017	0.053	-0.261
	(0.985)	(0.969)	(0.956)
	[637,701]	[453,906]	[24,701]
Lagged Absences	6.810	6.707	8.108
	(7.334)	(7.017)	(8.150)
	[669,403]	[455,759]	[24,882]
C. Parent Characteristics			
Share Matched to Mom	0.702	1.000	1.000
Share Mom Registered to Vote	0.426	0.611	0.523
Share Matched to Dad	0.645	0.921	0.861
Share Dad Registered to Vote	0.498	0.714	0.614
D. Student Voting			
Share Registered to Vote	0.764	0.797	0.743
Share Voted by Age 22	0.366	0.397	0.274

Sample in column (1) is limited to first-time 9th graders who enrolled in a public high school in Indiana in my sample between SY 2007-08 and SY 2015-16. Column (2) restricts sample to individuals with at least one non-missing grade 8 test score who match to a maternal birth record. Columns (2) also drops individuals with missing civic score measures, individuals who report being over age 18 at the time of their first 9th grade enrollment, and individuals who were not age-eligible to vote by November 1, 2019 or were in very small 9th grade cohorts. Column (3) shows summary statistics for individuals in the sibling sample. Siblings are identified based on mother's name and date of birth. Sibling sample is limited to individuals with non-missing civic score data and to families whose children attended at least two different schools. Test scores are standardized by year and grade to have a mean of 0 and standard deviation of 1 among all test-takers.

Table 2: Student/Parent Characteristics and Voter Turnout

	(1)	(2)	(3)
Male	-0.021***	-0.022***	-0.009***
	(0.00)	(0.00)	(0.00)
White	0.018***	0.011*	0.011**
	(0.00)	(0.00)	(0.00)
Black	0.049***	0.044***	0.070***
	(0.01)	(0.01)	(0.01)
Hispanic	0.022***	0.027***	0.035***
	(0.01)	(0.01)	(0.01)
Free/Reduced-Price Lunch	-0.142***	-0.117***	-0.077***
	(0.00)	(0.00)	(0.00)
Mom Registered to Vote		0.095***	0.079***
		(0.00)	(0.00)
Matched to Dad		0.054***	0.037***
		(0.00)	(0.00)
Gr 8 Math Score			0.022***
			(0.00)
Gr 8 ELA Score			0.047***
			(0.00)
Lag 1 Absences			-0.003***
			(0.00)
Cohort Dummies	X	X	X
First Election Dummies	X	X	X
N	456,695	456,695	451,530
r2	0.11	0.12	0.15

Outcome is a indicator that is equal to one if the individual voted in their first age-eligible general election. Models include dummies for first age-eligible election and 9th grade cohort. Standard errors are clustered by school. *p<0.05, ** p<0.01, ***p<0.001.

Table 3: Correlations across Lags for School Effect Estimates

Lag	Civic Score Effects	English 10 Effects	Non-Test Effects	College Exam Effects
1	0.68	0.54	0.49	0.54
2	0.65	0.51	0.46	0.48
3	0.58	0.44	0.42	0.43
4	0.54	0.34	0.31	0.32
5	0.48	0.37	.	0.26
6	0.47	.	.	0.32
7	0.42	.	.	0.30
8	0.40	.	.	0.28

Correlations across proximate years for school effect estimates. Estimated using the *vam* function in Stata. School effect estimates control for student race/ethnicity (Black, Hispanic, Asian, white); gender; free or reduced-price lunch status; special education status; English learner status; age in grade 9; maternal civic engagement index; paternal civic engagement index (set to 0 if missing father); indicator for matching to father; grade 8 math and ELA scores (including squares and cubics) and an indicator for missing either score; once lagged log unexcused absences plus one; an indicator for missing once lagged absences; once lagged log suspensions plus one; number of students in cohort; birth-county controls (percent in county with BA or higher, poverty rate in county, average county-level turnout, Republican partisan index, political competitiveness measures, rurality/urbanicity measure, and an indicator for missing county-of-birth); and dummies for first age-eligible election and grade cohort.

Table 4: Validity of School Effects

	Civic Score	English 10 Test	Non-Test Index	College Exam
	(1)	(2)	(3)	(4)
A. Actual Outcomes				
Beta on School Effect	0.981	0.979	0.966	0.992
	(0.012)	(0.025)	(0.050)	(0.024)
p-value (Beta=1)	0.104	0.399	0.496	0.732
N	456,695	280,302	238,976	456,695
B. Predicted Outcomes				
Beta on Schoool Effect	0.003	0.023+	0.001	0.001
	(0.002)	(0.013)	(0.001)	(0.005)
N	396,690	274,940	232,827	396,690

Estimates in Panel A and B come from regressions of the student's residualized actual (Panel A) or predicted (Panel B) outcomes on the leave-cohort-out school effect estimates for the outcomes listed in the header. The residual outcome (Panel A) is residualized on all the controls included in Equation 1, as described. The coefficient presented in Panel A is the coefficient on the unstandardized school effect in a regression predicting the residualized outcome. The school effect is a leave-cohort-out school effect by construction. Inference for the p-value in Panel A is conducted under the hypothesis that the coefficient is equal to 1. For Panel B, the outcome is the predicted (residual) outcome for the student based on 7th grade math and ELA scores and an indicator for being the child of an immigrant parent. I replace missing grade 7 ELA/math scores with scores in the opposite subject to reduce missingness. The predicted residual outcome is constructed as described in the text. The coefficient in Panel B is the coefficient on the (unstandardized) school effect in a regression predicting the predicted residual outcome. Inference in Panel B is conducted under the hypothesis that the coefficient is equal to 0. Standard errors are clustered by school throughout. + p<0.10, *p<0.05, ** p<0.01, ***p<0.001

Table 5: Civic School Effects and Voting Behavior

	Registered to Vote	Ever Voted by 22	Voted 1st General Election	Voted 2nd General Election	# Times Voted: Non-General	# Times Voted: Odd Year
	(1)	(2)	(3)	(4)	(5)	(6)
A. Full Sample						
Beta on School Effect	0.003+	0.016***	0.014***	0.011***	0.025***	0.004*
	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)	(0.002)
	0.078	0.000	0.000	0.000	0.000	0.010
N	456,695	456,695	456,695	456,695	456,695	456,695
Mean	0.80	0.40	0.23	0.25	0.19	0.04
SD					0.49	0.19
B. Sibling Sample						
Beta on School Effect	0.008*	0.012**	0.009*	0.006+	0.003	-0.003+
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.001)
N	24,944	24,944	24,944	24,944	24,944	24,944
Mean	0.74	0.27	0.15	0.16	0.11	0.02
SD					0.37	0.14

Panel A reports results from a regression of the student-level voting outcome listed in the column headers on the (standardized) school civic effect in the full sample. Regressions control for student race/ethnicity (Black, white, Hispanic, or Asian); special education, English learner, and free or reduced-price lunch status; gender; age in grade 9; lagged test scores in ELA and math (including squares and cubics and a missing indicator); log absences from prior year plus one; an indicator for missing lagged absences; log suspensions from prior year plus one; parental controls (indicator for matching to dad, paternal civic score, maternal civic score); and dummy variables for first age-eligible election and 9th grade cohort. Panel B present results from similar regression limited to the sibling sample. The sibling sample includes students who paired to at least one other individual in the sample with non-missing outcome data is limited to families (mom groups) where students attended two or more different schools. Panel B includes all the same controls as Panel A except parental controls. Standard errors are clustered by school. + p<0.10, *p<0.05, ** p<0.01, ***p<0.001.

Table 6: Robustness: Civic School Effects and Voting Behavior

	No Controls	Add Gr 8 School Characteristics	Drop Common Names	Drop Multi-State	Drop Non-Switchers	Sample Includes Students w/out Birth Records
	(1)	(2)	(3)	(4)	(5)	(6)
A. Ever Voted by Age 22						
Beta on School Effect	-0.0017	0.0212***	0.0160***	0.0162***	0.0152***	0.0147***
	(0.0065)	(0.0015)	(0.0020)	(0.0020)	(0.0022)	(0.0024)
Obs	456,695	456,695	429,396	455,097	402,172	636,871
B. Voted in First General Election						
Beta on School Effect	0.0025	0.0177***	0.0137***	0.0139***	0.0130***	0.0122***
	(0.0040)	(0.0015)	(0.0015)	(0.0015)	(0.0017)	(0.0017)
Obs	456,695	456,695	429,396	455,097	402,172	636871
C. Voted Outside General Elections						
Beta on School Effect	0.014*	0.028***	0.025***	0.025***	0.024***	0.021***
	(0.0059)	(0.0020)	(0.0026)	(0.0027)	(0.0029)	(0.0026)
Obs	456,695	456,695	429,396	455,097	402,172	636,871

Presents point estimate on school effect from a regression of the outcome listed in the right-hand column on the civic school effect estimate. Column 1 presents results from uncontrolled model. Column 2 controls for all variables included in preferred estimates in Table 5 and adds controls for school-level characteristics for the school the student attended in grade 8 (share free or reduced-price lunch; share English learner; share Black, white, Hispanic, and Asian) with an indicator for missing grade 8 school, in which case all other grade 8 school controls are set to 0. Column 3 drops students which common names, which I define as having a first and last name that both appeared in the upper 75th percentile of names in my sample. Column 4 drops students who match to voting records in more than 2 states. Column 5 drops students who did not switch schools between 8th grade and 9th grade. Column 6 uses school effect estimates that are estimated using a sample that includes students without birth records and imputes maternal and paternal civic score. Standard errors are clustered by school. + p<0.10, *p<0.05, ** p<0.01, ***p<0.001.

Table 7: Adult Voting and Civic/Non-Civic School Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
English 10 Test Effect	0.030***	0.006**					0.012***
	(0.004)	(0.002)					(0.002)
Non-Cognitive Effect			0.018*	-0.001			0.001
			(0.008)	(0.002)			(0.003)
College Exam Effect					0.008*	0.006***	0.003
					(0.004)	(0.002)	(0.002)
Civic Exam Effect							0.019***
							(0.002)
First Election/Grade Cohort Dummies	X	X	X	X	X	X	X
Other Controls		X		X		X	X
Obervations	297,663	297,663	250,965	250,965	456,695	456,695	148,479

Outcome is an indicator for voting by age 22. The number of observations does not match Table 5 and may differ across columns because it is limited to observations with non-missing values of each of the school effect estimates and some cohorts do not have data on measures used to construct other school effect estimates. Other controls include student demographics, lagged outcomes, and parental controls (as described for Table 5). All models include dummies for first age-eligible election and grade cohort. Standard errors are clustered by school. + p<0.10, *p<0.05, ** p<0.01, ***p<0.001.

Table 8: School Characteristics by Quartile of School Civic Effects

	Q1 (smallest)	Q2	Q3	Q4 (largest)	Beta (uncontrolled)	Beta (controlled)
	(1)	(2)	(3)	(4)	(5)	(6)
N Schools	84	84	84	83		
Effect on English 10 Exams	0.174	-0.203	-0.125	0.159	0.009	-0.005
	(1.025)	(0.781)	(0.755)	(1.132)	(0.078)	(0.058)
Effect on Non-Cognitive Outcomes	0.205	0.003	-0.086	-0.144	-0.182**	-0.016
	(1.068)	(0.825)	(1.156)	(0.746)	(0.059)	(0.044)
Effect on Taking the SAT/ACT	-0.242	-0.169	0.058	0.393	0.265***	0.034
	(0.805)	(0.805)	(0.843)	(1.091)	(0.054)	(0.052)
(Log) Enrollment	7.077	6.601	6.303	5.949	-0.693***	-0.603***
	(0.710)	(0.683)	(0.685)	(0.635)	(0.064)	(0.082)
Charter	0.000	0.012	0.024	0.084	1.320***	-0.563
					(0.292)	(0.515)
City	0.226	0.155	0.179	0.253	0.094	0.034
					(0.157)	(0.143)
Suburb	0.417	0.214	0.131	0.133	-0.560***	-0.073
					(0.118)	(0.111)
Town	0.167	0.060	0.119	0.048	-0.307*	-0.091
					(0.132)	(0.131)
Cohort Share Free/Reduced-Price Lunch	0.365	0.374	0.368	0.417	1.000**	0.608
	(0.158)	(0.139)	(0.140)	(0.181)	(0.359)	(0.640)
Cohort Share White	0.800	0.872	0.869	0.787	-0.230	0.557
	(0.190)	(0.156)	(0.145)	(0.302)	(0.303)	(2.425)
Cohort Share Matched to Mother	0.726	0.755	0.745	0.719	-0.266	-1.118*
	(0.078)	(0.099)	(0.094)	(0.135)	(0.534)	(0.465)
Cohort Avg Grade 8 Math Scores	0.093	0.024	0.029	-0.090	-0.884***	-1.462***
	(0.293)	(0.234)	(0.224)	(0.342)	(0.226)	(0.337)
Cohort Avg Grade 8 ELA Scores	0.048	-0.005	0.016	-0.056	-0.774*	-0.881
	(0.256)	(0.190)	(0.192)	(0.289)	(0.306)	(0.541)
Cohort Maternal Civic Engagement	-0.009	-0.033	-0.007	0.036	0.365	1.656***
	(0.201)	(0.157)	(0.160)	(0.188)	(0.395)	(0.444)
School is Polling Site	0.226	0.214	0.345	0.301	0.048	0.104
					(0.121)	(0.097)
Voter Turnout (County)	0.507	0.516	0.513	0.516	0.627	0.339
	(0.047)	(0.045)	(0.038)	(0.045)	(1.264)	(1.012)
Leans Republican (County)	0.630	0.640	0.602	0.573	-2.369***	-1.885**
	(0.100)	(0.078)	(0.080)	(0.113)	(0.635)	(0.634)
Political Competitiveness (County)	-0.283	-0.135	0.351	0.155	0.186***	0.134**
	(1.102)	(1.111)	(0.802)	(0.887)	(0.053)	(0.046)

Quartiles refer to quartile of (standardized) civic school effects. Column 5 reports the coefficient on the school characteristics from a regression predicting civic school effects in a dataset of school-level observations (N=335 for outcomes with maximum coverage). Column 6 does the same for the coefficient on the school characteristic from a regression that includes the following (leave-self-out): controls: log school enrollment; indicators suburban/town/city locations (rural is the omitted category); shares white, Black, Hispanic, Asian, and free-or-reduced-price eligible students; average maternal civic engagement; and average test 8th grade ELA and math scores. (I leave out the other subject's test score in regressions for grade 8 math and ELA scores). County-level political measures (leans Republican, political competitiveness, and voter turnout) are defined as described in the text. Robust standard errors in parentheses. + p<0.10, *p<0.05, ** p<0.01, ***p<0.001

Table 9: AP Exam Participation Rates and Civic Effects

	AP Exam Participation Rate				AP Exam Scores			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
U.S. History	-1.830	5.251*	4.856*	5.145*	-0.221+	-0.072	-0.164	-0.181
	(2.925)	(2.269)	(2.362)	(2.397)	(0.127)	(0.091)	(0.133)	(0.135)
U.S. Government	0.826	5.004	3.998	3.931	-0.042	0.143*	0.190*	0.188*
	(6.699)	(3.152)	(3.272)	(3.243)	(0.076)	(0.070)	(0.091)	(0.091)
Calculus AB		5.491	4.938	5.465		-0.074	-0.079	-0.089
		(4.225)	(4.192)	(4.089)		(0.078)	(0.096)	(0.086)
English		2.768	3.232	2.207		-0.033	-0.169	-0.103
		(2.228)	(2.456)	(2.319)		(0.086)	(0.138)	(0.139)
Chemistry		7.438+	5.914	5.782		0.059	0.141	0.055
		(3.822)	(3.866)	(3.668)		(0.085)	(0.128)	(0.125)
Biology			6.632+	7.001*			-0.053	-0.064
			(3.441)	(3.337)			(0.106)	(0.105)
English Literature			-1.095	-0.170			0.062	0.093
			(2.636)	(2.583)			(0.146)	(0.153)
Statistics			-0.195	1.377			0.157+	0.168+
			(4.189)	(4.169)			(0.087)	(0.087)
School Controls			X	X			X	X
County Political Controls				X				X
Observations	335	335	335	335	167	146	100	100

Present coefficients on measures of AP exam participation/performance in a regression predicting school-level state civic school effects (averaged over cohorts, as described). Observations are school-level. In columns 1-4, the AP predictors are subject-specific AP participation rates, which I defined as the total number of students who took the AP exam in that subject (dropping duplicates) in a school in a year divided by the number of students in the school in grades 9-13. I assign each student the AP participation rate in their 9th grade year and take the mean of these for a school, collapsing over students in my sample. In columns 5-8, the AP predictors are the average scores among test-takers. I assigned students the average AP scores for each subject in their school in their 9th grade year and collapse over students in my sample. The number of observations reflects the number of schools with non-missing scores in each of these subjects. The school-level controls are (log) total average number of AP tests; (log) average enrollment in grades 9-13; indicators for city, suburban, or town location (rural omitted); average percent white, Black, Hispanic, and Asian students; average percent free or reduced-price lunch eligible students; average maternal civic scores; average grade 8 test scores in ELA and Math. The county-level controls are the measures of voter turnout, Republican leanings, and political competitiveness, as described in the text. Robust standard errors in parentheses. + $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table 10: Extracurricular Activities and Civic Effects

	Any Extracurricular			Number of Extracurriculars		
	(1)	(2)	(3)	(4)	(5)	(6)
Civic	0.218+	0.239*	0.175	0.093	0.001	-0.084
	(0.120)	(0.121)	(0.115)	(0.076)	(0.063)	(0.061)
Music		-0.033	0.073		-0.144***	-0.137***
		(0.102)	(0.105)		(0.037)	(0.037)
Science		0.173	0.097		0.030	-0.081
		(0.106)	(0.096)		(0.100)	(0.089)
Math		0.154	0.201		-0.036	-0.066
		(0.133)	(0.132)		(0.087)	(0.079)
Quiz		-0.017	0.138		-0.021	0.053
		(0.101)	(0.103)		(0.059)	(0.062)
Best Buddies					-0.218	-0.337*
					(0.133)	(0.143)
Speech					-0.125	-0.163+
					(0.101)	(0.093)
N Extracurriculars (all)	-0.080***	-0.092***	-0.125***	-0.085***		
	(0.019)	(0.024)	(0.028)	(0.022)		
(Log) HS Enrollment	-0.520***	-0.527***	-0.418***	-0.498***	-0.485***	-0.374***
	(0.085)	(0.086)	(0.085)	(0.083)	(0.083)	(0.081)
School Controls			X			X
Observations	335	335	335	335	335	335

Presents coefficients on measures of school extracurricular participation in a regression predicting school-level state civic school effects (averaged over cohorts, as described). Observations are school-level. In columns 1-4, the extracurricular predictors are indicator variables that are equal to 1 if any activity I identify in that activity group was present at the school. In columns 5-8, the extracurricular predictors are the number of activities of each type found at the school. See Appendix Table A9 for details on the activity types and activity measures. The school-level controls are (log) average enrollment in grades 9-13; indicators for city, suburban, or town location (rural omitted); average percent white, Black, Hispanic, and Asian students; average percent free or reduced-price lunch eligible students; average maternal civic scores; average grade 8 test scores in ELA and Math. Robust standard errors in parentheses. +p<0.10, *p<0.05, ** p<0.01, ***p<0.001

Last updated: September 9, 2025

Appendix Figures

Figure A1: Distribution of Index Outcome Measures

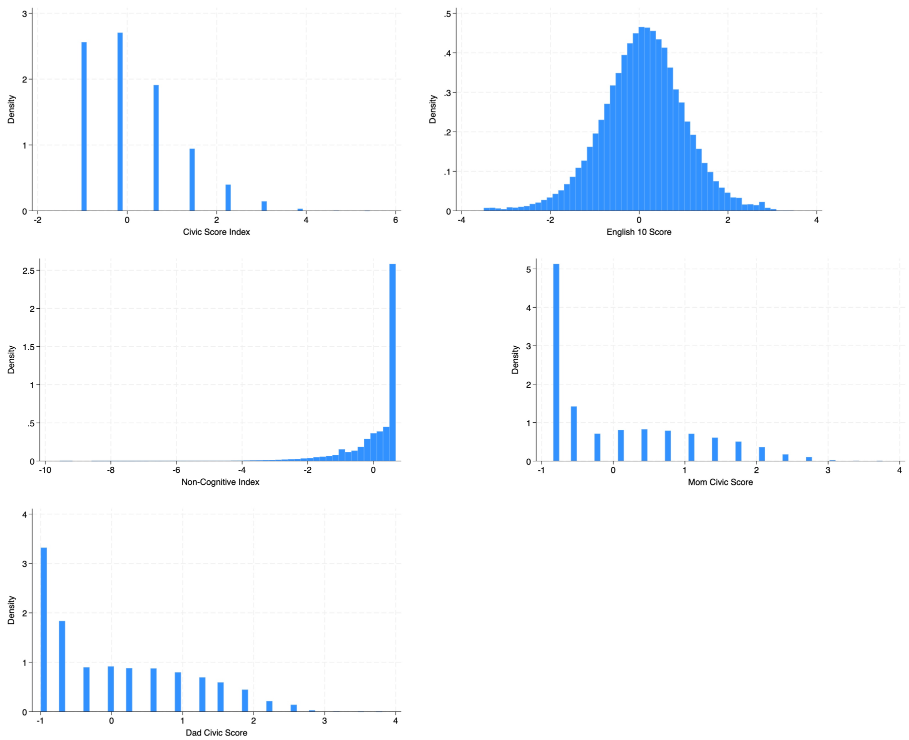


Figure A2: Distribution of School Effect Estimates

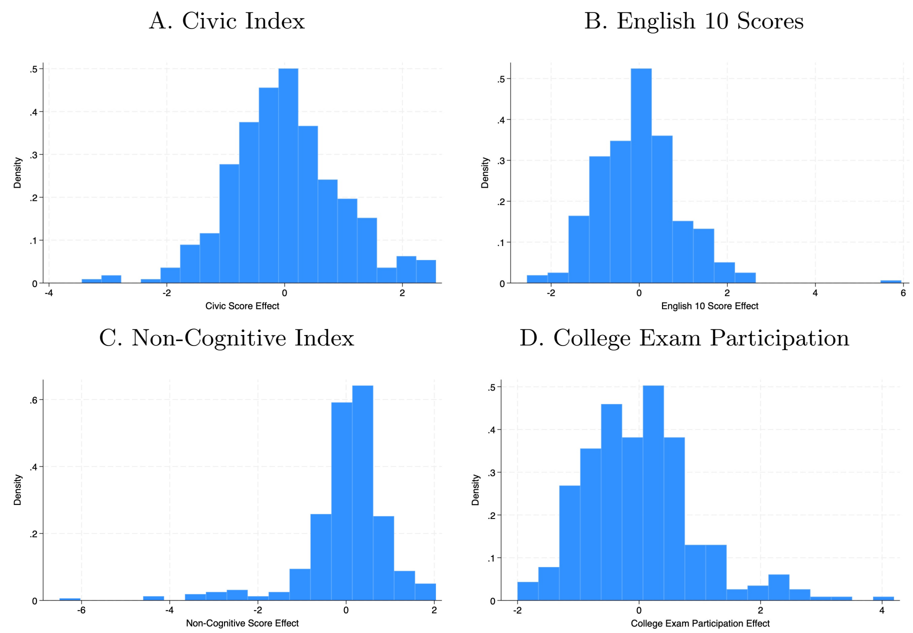
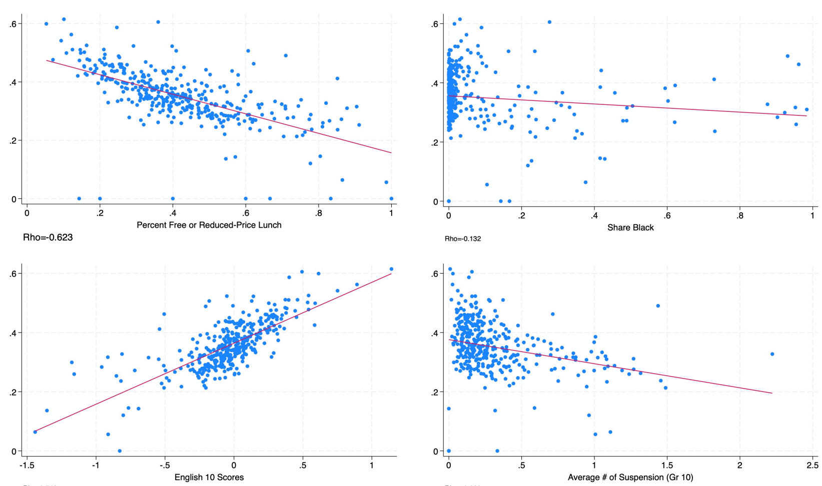


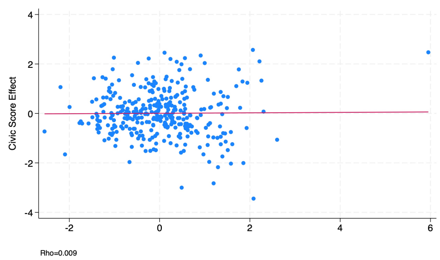
Figure A3: Share of Students who Voted by 22 and School Characteristics



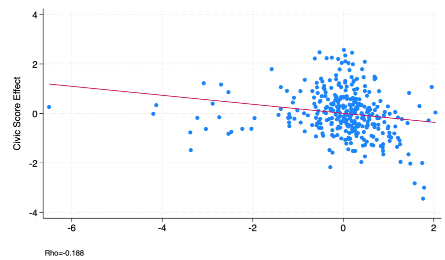
Includes all students, whether or not they matched to birth records. Y-axis is share of students who ever voted by age 22.

Figure A4: Civic School Effect and Effects on Other Outcomes

A. Test Scores



B. Non-Cognitive Outcomes



C. Taking the ACT/SAT

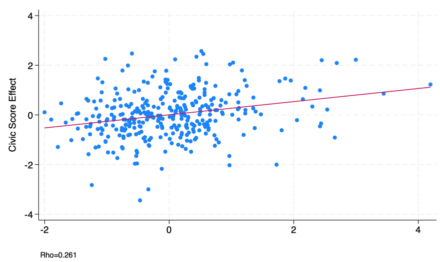
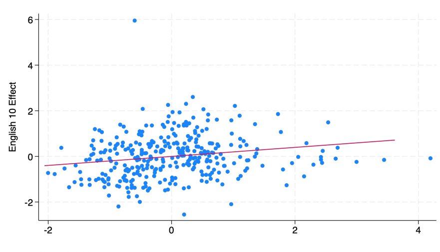
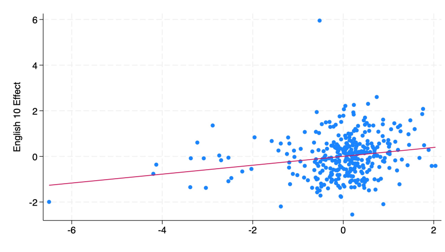


Figure A5: Correlations: Non-Civic Effects

A. College Exam vs. Test Scores



B. Test Scores vs. Non-Cognitive



C. College Exam vs. Non-Cognitive

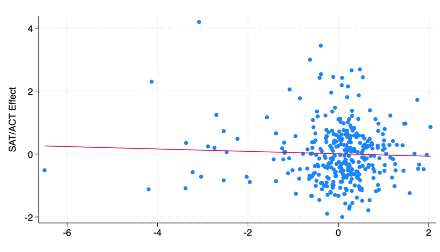
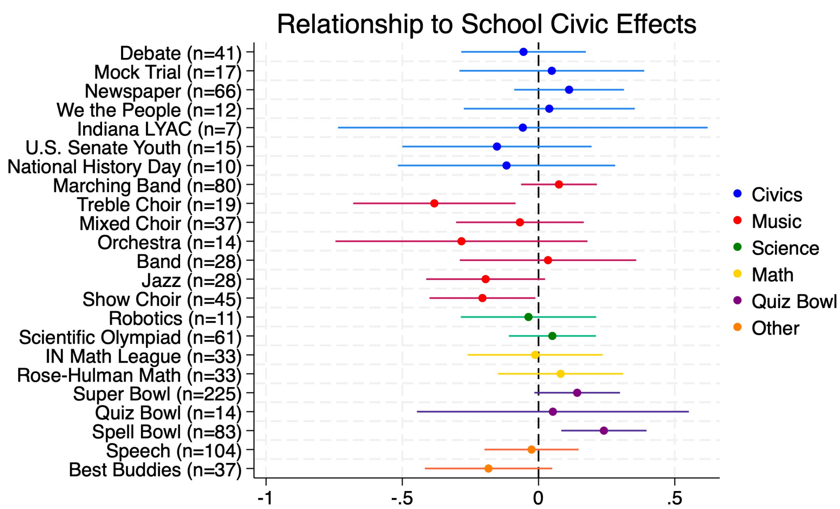


Figure A6: Extracurricular Activities and School Civic Effects



Plots point estimates for indicators for each activity from separate regressions predicting civic school effects on an indicator for the presence of each activity with the following controls: number of all activities found at the school; log high school grade enrollment; indicators for city, suburban, and town location (rural omitted); shares white, Black, Hispanic, Asian, and free or reduced-price lunch eligible students; average grade 8 test scores in math and ELA; and average maternal civic engagement).

Appendix Tables

Table A1: Matching Rates by Birth Year

Birth Year	N	Share Matched to Birth	Share Male (if matched)	Share Matched to Voting	Share Male (if matched to voting)
1988	32	0.28	0.22	0.63	0.30
1989	76	0.32	0.38	0.43	0.48
1990	380	0.48	0.64	0.54	0.61
1991	5502	0.61	0.65	0.60	0.68
1992	49401	0.69	0.54	0.71	0.57
1993	76284	0.66	0.51	0.73	0.54
1994	75280	0.69	0.51	0.76	0.53
1995	74982	0.70	0.51	0.76	0.52
1996	74532	0.71	0.51	0.78	0.51
1997	73661	0.72	0.50	0.79	0.51
1998	75242	0.71	0.51	0.80	0.51
1999	75572	0.75	0.51	0.77	0.50
2000	73065	0.76	0.51	0.78	0.50

Merging sample. Only includes birth years included in estimation sample.

Table A2: Outcome Measures

	Source	Min	Max	Mean	SD	N non-missing	Min Grade Cohort	Max Grade Cohort	Share missing (included cohorts)
A. Civic Score (Student)									
Registered to vote by 4th age-eligible election cycle	L2	0.00	1.00	0.7	0.457	456,695	2008	2016	0.000
Voted in first age-eligible election	L2	0.00	1.00	0.23	0.42	456,695	2008	2016	0.000
Voted in second age-eligible general election	L2	0.00	1.00	0.25	0.43	456,695	2008	2016	0.000
Times voted in non-primary/non-general elections (even year) in first 4 cycles	L2	0.00	2.00	0	0.018	456,695	2008	2016	0.000
Times voted in odd-year elections in first 4 cycles	L2	0.00	2.00	0.04	0.187	456,695	2008	2016	0.000
Times voted in primaries (even year) in first 4 cycles	L2	0.00	2.00	0.15	0.408	456,695	2008	2016	0.000
B. Test Score		1.00	1.00	1	0	456,695	2008	2016	0.000
English 10 Score	IN DOE	-3.50	3.48	0.09	0.935	280,302	2009	2014	0.058
C. Non-Cognitive Measures		1.00	1.00	1	0	456,695	2008	2016	0.000
Log: suspensions in grade 9 (+1)	IN DOE	0.00	4.19	0.16	0.447	456,695	2008	2016	0.000
Log: days unexcused absences in grade 9 (+1)	IN DOE	0.00	5.07	0.56	0.84	456,695	2008	2016	0.000
Indicator: ever expelled in grade 9	IN DOE	0.00	1.00	0.01	0.087	456,695	2008	2016	0.000
Share of credits passed in grade 9	IN DOE	0.00	1.00	0.89	0.228	238,976	2012	2016	0.077
Indicator: progressed to grade 10 on time (observed in following year in grade 10 in data)	IN DOE	0.00	1.00	0.97	0.182	456,695	2008	2016	0.000
D. College-Going Measure		1.00	1.00	1	0	456,695	2008	2016	0.000
Indicator: Took ACT or SA	IN DOE	0.00	1.00	0.59	0.491	456,695	2008	2016	0.000
E. Mom Civic Score		1.00	1.00	1	0	456,695	2008	2016	0.000
Mom registered to vote (prior to child's first age-eligible)	IN DOE	0.00	1.00	0.58	0.494	456,695	2008	2016	0.000
Mom voted: 1 general election prior		0.00	1.00	0.32	0.468	456,695	2008	2016	0.000
Mom voted: 2 general elections prior	L2	0.00	1.00	0.31	0.464	456,695	2008	2016	0.000
Mom voted: 3 general elections prior	L2	0.00	1.00	0.32	0.465	456,695	2008	2016	0.000
Mom voted: 4 general elections prior	L2	0.00	1.00	0.31	0.462	456,695	2008	2016	0.000
Mom voted: years voted in non-general/non-primary, pirmary, and odd year elections in 8 years before child's first age-eligible election (summed)	L2	0.00	9.00	0.75	1.361	456,695	2008	2016	0.000
F. Dad Civic Score	L2	1.00	1.00	1	0	456,695	2008	2016	0.000
Dad registered to vote (prior to child's first age-eligible)	L2	0.00	1.00	0.71	0.451	420,481	2008	2016	0.079
Dad voted: 1 general election prior		0.00	1.00	0.4	0.49	420,481	2008	2016	0.079
Dad voted: 2 general elections prior	L2	0.00	1.00	0.38	0.487	420,481	2008	2016	0.079
Dad voted: 3 general elections prior	L2	0.00	1.00	0.38	0.486	420,481	2008	2016	0.079
Dad voted: 4 general elections prior	L2	0.00	1.00	0.37	0.483	420,481	2008	2016	0.079
Dad voted: years voted in non-general/non-primary, pirmary, and odd year elections in 8 years before child's first age-eligible election (summed)	L2	0.00	10.00	0.9	1.474	420,481	2008	2016	0.079

Table A3: Correlations: Civic Index Measures (Student)

	Civic Index	Registered to Vote	Voted: 1st General Election	Voted: 2nd General Election	Years Voted: Non-Primary, Non-General (Even Year)	Years Voted: Odd Year Election	Years Voted: Primary Elections
Civic Index	1.00	0.70	0.68	0.70	0.04	0.39	0.69
	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]
Registered to Vote	0.70	1.00	0.35	0.36	0.01	0.12	0.24
	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]
Voted: 1st General Election	0.68	0.35	1.00	0.26	0.01	0.18	0.34
	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]
Voted: 2nd General Election	0.70	0.36	0.26	1.00	0.02	0.17	0.38
	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]
Years Voted: Non-Primary, Non-General (Even Year)	0.04	0.01	0.01	0.02	1.00	0.02	0.02
	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]
Years Voted: Odd Year Election	0.39	0.12	0.18	0.17	0.02	1.00	0.23
	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]
Years Voted: Primary Elections	0.69	0.24	0.34	0.38	0.02	0.23	1.00
	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]

All variables over the first four age-eligible vote cycles (two even years and two odd years). "Years Voted" variables are coded as 0, 1, or 2 depending on the number of years the individual voted at all in the indicated election. Observations for pairwise correlations in brackets. Registered to vote is based on registering to vote within the first four age-eligible elections (based on year of voter registration from L2 files).

Table A4: Correlations: Maternal Civic Index Measures

	Mom Civic Index	Mom Registered to Vote	Mom Voted: 1 General Election Prior	Mom Voted: 2 General Elections Prior	Mom Voted: 3 General Elections Prior	Mom Voted: 4 General Elections Prior	Years Voted: Primaries, Other Non-General, Odd Year Elections
Mom Civic Index	1.00	0.72	0.79	0.79	0.81	0.80	0.90
	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]
Registered to Vote	0.72	1.00	0.59	0.58	0.58	0.57	0.47
	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]
Voted: 1st General Election	0.79	0.59	1.00	0.55	0.72	0.53	0.60
	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]
Voted: 2nd General Election	0.79	0.58	0.55	1.00	0.55	0.72	0.62
	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]
Years Voted: Non-Primary, Non-General (Even Year)	0.81	0.58	0.72	0.55	1.00	0.58	0.63
	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]
Years Voted: Odd Year Election	0.80	0.57	0.53	0.72	0.58	1.00	0.62
	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]
Years Voted: Primary Elections	0.90	0.47	0.60	0.62	0.63	0.62	1.00
	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]	[456695]

All variables are defined over the 8 election cycles before the child's first age-eligible election cycle. "Years Voted" variable is the number of years the parent was observed voting in a primary, other non-primary/non-general (even year), or odd year election added together over the 8 years before the child's first age-eligible election. Observations for pairwise correlations in brackets. Registered to vote is based on registering to vote before the child's first age-eligible election (based on year of voter registration from L2 files).

Table A5: Correlations: Paternal Civic Index Measures

	Dad Civic Index	Dad Registered to Vote	Dad Voted: 1 General Election Prior	Dad Voted: 2 General Elections Prior	Dad Voted: 3 General Elections Prior	Dad Voted: 4 General Elections Prior	Years Voted: Primaries, Other Non-General, Odd Year Elections
Dad Civic Index	1.00	0.64	0.77	0.78	0.79	0.78	0.89
	[420481]	[420481]	[420481]	[420481]	[420481]	[420481]	[420481]
Registered to Vote	0.64	1.00	0.52	0.50	0.50	0.49	0.39
	[420481]	[420481]	[420481]	[420481]	[420481]	[420481]	[420481]
Voted: 1st General Election	0.77	0.52	1.00	0.55	0.67	0.52	0.57
	[420481]	[420481]	[420481]	[420481]	[420481]	[420481]	[420481]
Voted: 2nd General Election	0.78	0.50	0.55	1.00	0.56	0.68	0.59
	[420481]	[420481]	[420481]	[420481]	[420481]	[420481]	[420481]
Years Voted: Non-Primary, Non-General (Even Year)	0.79	0.50	0.67	0.56	1.00	0.58	0.60
	[420481]	[420481]	[420481]	[420481]	[420481]	[420481]	[420481]
Years Voted: Odd Year Election	0.78	0.49	0.52	0.68	0.58	1.00	0.59
	[420481]	[420481]	[420481]	[420481]	[420481]	[420481]	[420481]
Years Voted: Primary Elections	0.89	0.39	0.57	0.59	0.60	0.59	1.00
	[420481]	[420481]	[420481]	[420481]	[420481]	[420481]	[420481]

All variables are defined over the 8 election cycles before the child's first age-eligible election cycle. "Years Voted" variable is the number of years the parent was observed voting in a primary, other non-primary/non-general (even year), or odd year election added together over the 8 years before the child's first age-eligible election. Observations for pairwise correlations in brackets. Registered to vote is based on registering to vote before the child's first age-eligible election (based on year of voter registration from L2 files).

Table A6: Correlations: Non-Cognitive Index Measures

	Noncognitive Index	Log Gr9 Suspensions	Log Gr9 Unexcused Absences	Indicator: Expelled (Gr 9)	Gr9 Pass Rate	Entered Grade 10 On Time
Noncognitive Index	1.00	-0.71	-0.67	-0.33	0.72	0.40
	[238976]	[238976]	[238976]	[238976]	[238976]	[238976]
Log Gr9 Suspensions	-0.71	1.00	0.31	0.18	-0.33	-0.17
	[238976]	[456695]	[456695]	[456695]	[238976]	[456695]
Log Gr9 Unexcused Absences	-0.67	0.31	1.00	0.07	-0.28	-0.16
	[238976]	[456695]	[456695]	[456695]	[238976]	[456695]
Indicator: Expelled (Gr 9)	-0.33	0.18	0.07	1.00	-0.11	-0.13
	[238976]	[456695]	[456695]	[456695]	[238976]	[456695]
Gr 9 Passing Rate	0.72	-0.33	-0.28	-0.11	1.00	0.16
	[238976]	[238976]	[238976]	[238976]	[238976]	[238976]
Entered Grade 10 On Time	0.40	-0.17	-0.16	-0.13	0.16	1.00
	[238976]	[456695]	[456695]	[456695]	[238976]	[456695]

Table A7: Summary of Control Variables

Covariate	Min	Max	Mean	SD	N non-missing	Min Grade Cohort	Max Grade Cohort
Black	0.00	1.00	0.07	0.261	456695	2008	2016
White	0.00	1.00	0.84	0.367	456695	2008	2016
Hispanic	0.00	1.00	0.05	0.209	456695	2008	2016
Asian	0.00	1.00	0.01	0.075	456695	2008	2016
FRPL	0.00	1.00	0.37	0.482	456695	2008	2016
Male	0.00	1.00	0.50	0.500	456695	2008	2016
English Leaner	0.00	1.00	0.01	0.090	456695	2008	2016
Sepcial Education	0.00	1.00	0.12	0.324	456695	2008	2016
Age in Gr 9	11.95	17.87	14.81	0.440	456695	2008	2016
Gr 8 Math (z-score)	-6.63	4.75	0.07	0.964	456695	2008	2016
Gr 8 ELA (z-score)	-6.96	5.66	0.05	0.972	456695	2008	2016
Misisng Either Gr 8 Test	0.00	1.00	0.01	0.096	456695	2008	2016
(Ln) Lag1 Unexcused Absences	0.00	4.84	0.60	0.829	456695	2008	2016
(Ln) Lag1 Unexcused Suspensions	0.00	4.45	0.16	0.443	456695	2008	2016
Missing Lag 1 Unescued Absences	0.00	1.00	0.00	0.045	456695	2008	2016
Mom Civic Score	-0.84	3.76	0.01	1.004	456695	2008	2016
Dad Civic Score	-1.00	3.81	0.01	0.961	456695	2008	2016
Matched to Dad	0.00	1.00	0.92	0.270	456695	2008	2016
Cohort Size	26.00	1288.00	367.88	239.352	456695	2008	2016
Missing Birth County	0.00	1.00	0.01	0.101	456695	2008	2016
County % BA or Higher	0.00	48.90	19.32	7.111	456695	2008	2016
County Poverty Rate	0.00	19.40	10.40	3.249	456695	2008	2016
County Average Turnout (2004-2008)	0.00	0.63	0.50	0.061	456695	2008	2016
County Republican Partisan Inex (2006)	0.00	0.78	0.56	0.114	456695	2008	2016
County Political Competitiveness (2000-2008)	-3.33	1.47	0.34	0.907	456695	2008	2016
County Rural/Urbanicity	0.00	9.00	2.39	1.727	456695	2008	2016
Cohort: Share Matched to Mom	0.19	1.00	0.72	0.103	456695	2008	2016
Cohort: Share Old for Grade 9	0.00	0.23	0.02	0.020	456695	2008	2016
Cohort: Share FRPL	0.00	1.00	0.40	0.177	456695	2008	2016
Cohort: Share Special Education	0.00	0.32	0.14	0.044	456695	2008	2016
Cohort: Share English Learner	0.00	0.29	0.03	0.040	456695	2008	2016
Cohort: Share Black	0.00	1.00	0.09	0.151	456695	2008	2016
Cohort: Share Hispanic	0.00	0.74	0.07	0.086	456695	2008	2016
Cohort: Share Asian	0.00	0.23	0.02	0.024	456695	2008	2016
Cohort: Mom Civic Score (avg)	-0.64	0.96	0.01	0.213	456695	2008	2016
Cohort: Share Matched to Dad	0.18	0.95	0.67	0.112	456695	2008	2016
Cohort: Share Missign Gr8 Tests	0.00	0.41	0.06	0.031	456695	2008	2016
Cohort: Average Grade 8 Tests	-1.54	1.20	0.03	0.291	456695	2008	2016
Cohort : Lag1 Days Unexcused Absences (avg)	0.00	21.44	2.00	1.702	456695	2008	2016
Cohort: Lag1 Suspensions (avg0	0.00	9.78	0.41	0.448	456695	2008	2016
Cohort: Share Missing Absences	0.00	0.90	0.03	0.030	456695	2008	2016
Cohort: Size	26.00	1288.00	367.88	239.352	456695	2008	2016
Gr 8 School: Share FRPL Eligible	0.00	1.00	0.42	0.194	456695	2008	2016
Gr 8 School: Share English Learner	0.00	0.74	0.03	0.049	456695	2008	2016
Gr 8 School: Black	0.00	1.00	0.08	0.156	456695	2008	2016
Gr 8 School: Share White	0.00	1.00	0.79	0.217	456695	2008	2016
Gr8 School: Share Hispanic	0.00	0.99	0.07	0.090	456695	2008	2016
Gr 8 School: Share Asian	0.00	0.39	0.02	0.022	456695	2008	2016
Gr8 School: Indicator for Missing School	0.00	1.00	0.00	0.003	456695	2008	2016

Table A8: Correlations with Alternative Civic School Effect Estimates

Model	Description	Preferred Specification
1	Preferred (esimated with vam)	1.0000
2	Drift Limit=3	0.9765
3	Sample Includes Students w/out Birth Records	0.9730
4	Lagged Fall Test Interactions	0.9837
5	Add Peer Controls	0.9796
6	No Residualization on School Effects	0.6876
7	No Cohort FE	0.9978
8	No First Election FE, No Cohort FE	0.3518
9	No First Election FE	0.9996
10	SchoolxCohort Residuals	0.7702
	Observations	456,695

This table shows the correlations between my preferred civic school effect estimates and estimates from alternative models in the student sample. Model 1 confirms that estimates I calculate via my two-step process (residualizing values first and then running the vam function) are equivalent to those produced when residualization is done with the vam function. Model 2 shows that results are similar when I limit the drift period for civic effect estimates to 3 periods. Model 3 shows results estimated in a sample that includes children who do not match to birth records, with maternal and paternal civic engagement imputed based on child race/ethnicity, gender, free or reduced-price lunch status, special education status, English learner status, age at 9th grade, and lags of test scores and behavioral indicators. Model 4 includes indicators for taking (lagged) scores in fall, interacted with test sores. Model 5 was estimated with cohort peer controls. Model 6 was estimated without residualizing on school fixed effects in step one. Model 7 was estimates without grade cohort dummies but with first election dummies. Model 8 was estimated without grade cohort dummies and without first election dummies. Model 9 was estimate with grade cohort dummies but without first election dummies. Model 10 shows average school-by-cohort residuals from equation (3).

Table A9: Details: Extracurricular Activities

Type	Activity	Time Coverage	Description of Activity	Indicator	Source	URL
Civic	Debate	2011-2020	Students argue for or against a resolution using evidence, logic, and structured formats to sharpen critical thinking and public speaking.	Schools that participated in that year's Indiana Schools Speech and Debate Association debate contests.	Speechwire	https://www.speechwire.com
Civic	Mock Trial	2016-2020	Students simulate courtroom trials by taking on roles such as attorneys and witnesses to practice legal reasoning, public speaking, and teamwork.	Participated in Mock Trial.	Indiana Bar Foundation	Shared with me.
Civic	We The People	2020	Students participate in simulated congressional hearings to demonstrate their understanding of the U.S. Constitution, government, and civic responsibility.	Participated in We the People.	Indiana Bar Foundation	Shared with me.
Civic	U.S. Senate Youth Program	2000-2020	Two Indiana students are selected annually to spend a week in DC and observe the political process	Student from school participated.	U.S. Senate Youth Progr	https://ussenateyouth.org/about_alumni_rosters/
Civic	Indiana Legislative Youth Advisory Council	2018-2020	High school and univerisity students in Indiana (age 16-22) are appointed to advise state asembly on youth issues.	Student from school participated.	Indiana General Assembly	https://ussenateyouth.org/about_alumni_rosters/
Civic	National History Day	2020	Students conduct original historical research and present their findings through exhibits, documentaries, papers, performances, or websites for regional, state, and national competititons.	Student placed in state competition.	Indiana Historical Society	https://indianahistory.org/wp-content/uploads/NHDI-State-Winners.pdf
Civic	Newspaper	2025 (present)	The Indiana High School Press Association is a statewide organization for school newspapers.	School appeared listed as a member in the directory of IHSPA organizations (has student newspaper).	Indiana High School Press Association	https://ihspa.net/
Other	Speech	2011-2020	Students prepare and perform original or interpretive speeches to develop communication, persuasion, and performance skills.	Schools that participated in that year's Indiana Schools Speech and Debate Association speech contests.	Speechwire	https://www.speechwire.com
Other	Best Buddies	2025 (present)	Volunteer group to support individuals with development diasbilities.	School has a chpater of the best buddies program.	Best Buddies	https://www.bestbuddies.org/indiana/friendship
Music	Band	2008-2019	Music	School participated in state band finals (finalist or placed).	Indiana State School Music Association	https://www.issma.net/orghistory.php
Music	Orchestra	2008-2019	Music	School participated in state orchestra finals (finalist or placed).	Indiana State School Music Association	https://www.issma.net/orghistory.php
Music	Mixed Choirs	2008-2019	Music	School participated in state mixed choir finals (finalist or placed).	Indiana State School Music Association	https://www.issma.net/orghistory.php
Music	Treble/Men's Choir	2008-2019	Music	School participated in state treble men's choir finals (finalist or placed).	Indiana State School Music Association	https://www.issma.net/orghistory.php
Music	Jazz	2016-2019	Music	School participated in state insturmental or vocal jazz choir finals (finalist or placed).	Indiana State School Music Association	https://www.issma.net/orghistory.php
Music	Show Choir	2008-2019	Music	School participated in jazz show choir finals (finalist or placed).	Indiana State School Music Association	https://www.issma.net/orghistory.php
Music	Marching Band	2008-2019	Music	School participated in state marching band finals (finalist or placed).	Indiana State School Music Association	https://www.issma.net/orghistory.php
Quiz	Quiz Bowl	2020 only	Team buzzer competition to answer questions on academic questions across a wide variety of areas.	Participated in state competition.	Indiana Association of School Principals	https://iasp.org/students/quiz-bowl/
Quiz	Spell Bowl	2008-2020	Team-based oral spelling contests.	Participated in state competition.	Indiana Association of School Principals	https://iasp.org/students/academic-spell-bowl/
Quiz	Super Bowl	2008-2020	Tean buzzer competiton that focuses on specific academic topics with a theme.	Participated in state competition.	Indiana Association of School Principals	https://iasp.org/students/academic-super-bowl/
Science	Science Olympiad	2019-2020	Team-based competition with evens across scientific disciplines.	Registered team.	Indiana Science Olympiad	http://indianascienceolympiad.org/
Science	Robotics	2018-2019	Team-based robotics competition.	Team registered to participate in Vex robotics competiton.	Robot Events.Com (TechPoint Foundation)	https://web.archive.org/web/20221116195540/https://www.robotevents.com/robot-competitions/vex-robotics-competition/RE-VRC-17-4490.html
Math	Rose Hulman Math Competition	2008-2019	Team-based math competiton.	Team participated.	Rose Hulman Math Competition	https://www.rose-hulman.edu/~rickert/NovContest/novcont2019.html
Math	Indiana Mathematics League	2015-2020	Team-based math competiton.	Team registered to participate in one or more contests.	Indiana Mathematics League	https://web.archive.org/web/20190318073759/https://old.mathleague.com/reglist/REGIN.HTM

Table A10: AP/Extracurricular Measure Summary Statistics

Measure	Min	Max	Mean	SD	N non-missing
(Log) Average # AP Tests	0.00	7.70	4.27	1.331	335
Participation rate (exams/HS student)					
US History	0.00	0.14	0.02	0.021	335
US Government	0.00	0.20	0.01	0.015	335
Calculus AB	0.00	0.07	0.02	0.012	335
English	0.00	0.25	0.02	0.022	335
Chemistry	0.00	0.07	0.01	0.011	335
Biology	0.00	0.11	0.01	0.013	335
English Literature	0.00	0.20	0.02	0.020	335
Statistics	0.00	0.09	0.00	0.010	335
Average Score (among test-takers)					
US History	1.00	5.00	2.08	0.736	264
US Government	1.00	5.00	2.29	0.885	184
Calculus AB	1.00	4.56	2.01	0.843	325
English	1.00	5.00	2.55	0.752	270
Chemistry	1.00	5.00	1.93	0.853	259
Biology	1.00	4.54	2.16	0.747	259
English Literature	1.00	4.00	2.44	0.647	258
Statistics	1.00	5.00	2.35	1.082	177
Any Extracurricular					
Civic	0.00	1.00	0.31	0.462	335
Music	0.00	1.00	0.37	0.483	335
Science	0.00	1.00	0.21	0.407	335
Math	0.00	1.00	0.15	0.360	335
Quiz	0.00	1.00	0.70	0.458	335
National Honor Society	0.00	1.00	0.31	0.463	335
Speech	0.00	1.00	0.11	0.314	335
Count of Extracurriculars					
All Extracurriculars	0.00	21.00	3.47	3.524	335
Civic	0.00	6.00	0.50	0.935	335
Music	0.00	6.00	0.75	1.296	335
Science	0.00	2.00	0.21	0.426	335
Math	0.00	2.00	0.20	0.498	335
Quiz	0.00	3.00	0.96	0.775	335

B. Matching K-12 and Birth Records

B1 Preparing K-12 Records

My initial matching sample was drawn from the records of students who were first observed in grade 9 in an Indiana school in my data[23] between SY 2006-07 and 2021-22. I limited my sample to individuals between 1988 (the first available year of complete birth record data) and 2010. I cleaned names and dropped the small number of observations with incomplete first or last names (including one-letter first or last names) or missing date of birth (DOB). There was also a small number of records that were duplicates by first name, middle initial, last name, and DOB. I randomly selected one observation per name/DOB combination.

My initial sample consisted of approximately 1.38 million student-level observations that were unique by first name, middle initial (including missing), last name, and DOB.[24] This sample was used for merging to both birth and voting records.

B2 Preparing Birth Records

I compiled and cleaned birth records for babies born in the state of Indiana between 1988 and 2009. I dropped records with missing or single letter first or last names as well as records without complete DOBs. I also dropped the small number of observations for children born in Canada or in a "not classifiable" location, since it was unclear if they would be eligible for birthright citizenship. A small share (2.5%) of births in my records reported that the child was born in a state that was not Indiana. Of these, Kentucky had the largest share (48%) followed by Ohio (24%) and Illinois (22%) and Michigan (2%); no other state exceeded 1% of out-of-state births. I further refined my sample so that it was unique by first name, middle initial (including missing), last name, and DOB, collapsing duplicates. For parent entries, I set to missing entries with single-letter last names or incomplete date of birth information. I also set to missing the small number of birth dates reported for parents that indicated the mother or father was younger than 13 or older than 60 at the time of the baby's birth.

This resulted in a sample of about 1.9 million babies (children) paired to about 1.31 million unique mothers (based on name and date of birth) and about 1.15 million unique fathers. Middle initial for children was missing for 2.7% of observations. Coverage for fathers was bit worse in early years such as 1988 and 1989 when about 75% and 80% of children matched to fathers with complete information, as compared to about 85.6% overall.

[23]Note that the statewide enrollment files include students from public and some private schools. I did not apply sample restrictions based on school type at this stage.

[24]Middle initial was missing for about 10.3% of observations.

B3 Matching Process: Birth/K-12

I matched K-12 and birth records using a combination of exact and fuzzy matching. Exact matching was based on a combination of (1) first name, last name, and date of birth or (2) full name (first name, middle name, and last name combined into a single string) and date of birth. I used full name to increase the probability of matching individuals who reported first/middle/last name in different fields across data sources. I discarded matches based on first and last name that did not match on non-missing middle initials. A small number of children matched to more than one student and vice versa at this stage. I disambiguated these based on matching middle or full name and used randomization to break ties for a small number of observations as necessary.

I supplemented exact matching by using the fastLink function in R to conduct fuzzy matching (Enamorado et al., 2019). I blocked data by year of birth to speed up matching. Matching was based on first and last name (strings) and day and month of birth (numeric). Matching for strings was conducted using Jaro-Winkler string distance and included partial matching. I specified a minimum posterior match probability of 0.90. I discarded fastLink matches involving student or birth records that had already been matched using exact matching. I required that matches identified by fastLink exactly match on last name, first name, birth date, and/or full name. I refined matches to ensure a 1:1 student to birth record match was achieved, using randomization to break ties as necessary.

Using fuzzy and exact matching, I paired roughly 996,000 of the approximately 1.38 million students in my sample to an Indiana birth record, or approximately 72% of observations. Of these, the vast majority (98.9%) matched to a mother with complete information and 89.4% matched to a father with complete information.

Appendix Table B1 reports summary statistics on match rates and gender breakdown of matched observations by year of birth.

Appendix Table B2 examines how match rates vary for different groups of students using my initial matching sample. The columns of Appendix Table B1 report the coefficient, standard error, and p-value for the coefficient on an indicator variable for that characteristic estimated by regressing the matching outcome listed in the column header on that indicator variable. Intuitively, it answers the question if having the listed characteristic predicts matching. The outcome of column 1 is matching to birth records at all and is estimated using the full initial merging sample; columns 2-5 are estimated using the sample of individuals who matched to a birth record to identify if there are group-based differences in how students matched/whether they matched to both parents. The outcome of column 2 is matching to a mother with non-missing information and the outcome of column 3 is matching to a father with non-missing information. The outcome of column 4 is exactly matching to a birth records. Differential matching is evident. Note that it is not clear that we should expect uniform match rates across demographic groups, since declaring an individual on a birth record and cross-state mobility may both be related to socioeconomic status and other factors. Match rates were very slightly higher for female students than for male students in my sample and were higher for white students than non-white students. Match rates were substantially lower for English language learners (ELLs) than for non-ELLs – which makes sense given that many ELLs may be children in immigrant families, who tend to be mobile, and may be immigrants themselves. Matching was also lower for children who qualified for free or reduced-price lunch than for those who did not.

Appendix Table B3 shows match rates by student birth year across voting and birth records.

Table B1: Match Rates: K-12 to Birth Records, by Birth Year

	Student obs	Birth record obs	Share students matched to birth records
All	1,383,526	996,028	0.72
Matched to Birth	996,027	996,027	1.00
Born 1988	431	163	0.38
Born 1989	1,539	782	0.51
Born 1990	8,724	5,290	0.61
Born 1991	58,153	40,198	0.69
Born 1992	86,105	57,634	0.67
Born 1993	85,799	56,371	0.66
Born 1994	84,610	58,189	0.69
Born 1995	82,065	56,925	0.69
Born 1996	82,394	57,860	0.70
Born 1997	83,818	59,355	0.71
Born 1998	85,604	60,453	0.71
Born 1999	85,778	64,248	0.75
Born 2000	86,590	65,367	0.76
Born 2001	85,072	63,937	0.75
Born 2002	83,792	62,748	0.75
Born 2003	85,116	64,011	0.75
Born 2004	85,504	63,943	0.75
Born 2005	85,140	63,920	0.75
Born 2006	84,000	63,087	0.75
Born 2007	43,142	31,504	0.73
Born 2008	142	62	0.43

* Birth above 2008 suppressed for small sample size

Table B2: Match Rates by Student Characteristic

		Matched to Birth Records		
	Matched to Birth Records	Matched to Mom	Matched to Dad	Exact Match
Male				
Coefficient	-0.005***	0.0000	0.004***	-0.027***
Std. error	(0.001)	(0.000)	(0.001)	(0.000)
p-value	0.00	0.87	0.00	0.00
Number of observations	1,383,526	996,027	996,027	996,027
White				
Coefficient	0.248***	-0.008***	0.173***	0.031***
Std. error	(0.001)	(0.000)	(0.001)	(0.000)
p-value	0.00	0.00	0.00	0.00
Number of observations	1,383,526	996,027	996,027	996,027
Black				
Coefficient	-0.156***	0.007***	-0.259***	-0.050***
Std. error	(0.001)	(0.000)	(0.002)	(0.001)
p-value	0.00	0.00	0.00	0.00
Number of observations	1,383,526	996,027	996,027	996,027
Hispanic				
Coefficient	-0.254***	0.008***	-0.023***	-0.007***
Std. error	(0.001)	(0.000)	(0.001)	(0.001)
p-value	0.00	0.00	0.00	0.00
Number of observations	1,383,526	996,027	996,027	996,027
Asian American/Pacific Islander (Hawa				
Coefficient	-0.488***	0.007***	0.070***	0.006***
Std. error	(0.003)	(0.001)	(0.002)	(0.002)
p-value	0.00	0.00	0.00	0.00
Number of observations	1,383,526	996,027	996,027	996,027
EL				
Coefficient	-0.468***	0.009***	-0.017***	-0.009***
Std. error	(0.002)	(0.000)	(0.003)	(0.002)
p-value	0.00	0.00	0.00	0.00
Number of observations	1,383,526	996,027	996,027	996,027
Special Education Status				
Coefficient	-0.018***	0.001**	-0.061***	-0.014***
Std. error	(0.001)	(0.000)	(0.001)	(0.001)
p-value	0.00	0.00	0.00	0.00
				0
Number of observations	1,383,526	996,027	996,027	996,027
Free or Reduced-Price Lunch				
Coefficient	-0.085***	0.006***	-0.132***	-0.017***
Std. error	(0.001)	(0.000)	(0.001)	(0.000)
p-value	0.00	0.00	0.00	0.00
Number of observations	1,383,352	995,968	995,968	995,968

Reflects coefficient on the variable listed in the right-side column in a regression predicting the outcome listed in the column. Robust standard errors in parentheses. Estimated using initial matching sample. *p<0.05, ** p<0.01, *** p<0.001

C. Matching Children/Parents to Voting Records

C1 Preparing State Voting Records

Voting records were purchased from the commercial vendor L2. L2 collects and consolidates voting records from various sources in a uniform format for political campaigns and research purposes. L2 provided me with multiple files for each state from different points in time, generally covering the years 2017-2023.[25] Each file represents a snapshot view of voters registered in that state at that moment, with voter turnout data generally going back to the year 2000. To create my state-level merging files, I took one cross-sectional state file per year from among the available L2 files and assembled a state-level dataset of observations that were unique by voter ID, first name, last name, middle initial (including missing) and DOB. Combining voting files from multiple years allowed me to capture voters who entered/exited the sample over time. It also allowed me to capture different iterations of names/DOB reported for the same voter, as identified by their L2-assigned voter ID, over time.[26] The source files used to create the merging file for each state are listed in Appendix Table C1.

I cleaned names and filled in missing information on voter gender and DOBs with information from non-missing entries for the same voter ID as needed. Observations missing year of birth were dropped, as were observations that were missing first or last name, had single-letter first/last names, or had incomplete DOB information. In some states (not including Indiana), I observed an unusually high incidence of birthdays reported as 01/01/YEAR. This suggests incomplete data on month and day of birth. I report the share of observations with 1/1/YEAR dates of birth in Appendix Table C2. For the purpose of matching, I treated these as true dates of birth since I was unable to disambiguate otherwise. This could lead to under-matching in these states.

A small number of observations reported conflicting non-missing gender information for the same voter ID over time. I was unable to determine whether this reflected a shift in gender identity or a data recording error. For each voter ID, I identified the gender that was most frequently reported for that ID and used this as the gender of record. Where multiple genders were reported with equal frequency, I broke ties using the earliest reported gender.

Since merging was based on first name, last name, middle initial, an DOB, I wanted each voting record to reflect a unique combination of these fields within a state. At this stage, state voting records were unique by voter ID/first name/last name/middle initial and DOB but some combinations of names/DOB were associated with more than one voter ID. The vast majority of these duplicate name/DOB combinations were paired to only two voter IDs. A small number were paired to more than one voter ID. These duplicate observations could reflect duplicated entries for the same individual registered under different voter IDs or they could reflect distinct individuals in the state who shared the same name/DOB. To distinguish between these two possibilities, I collapsed voter histories by first name, middle initial, last name, and DOB using data from all voter IDs associated with the name/DOB

[25] Data on voter turnout were incomplete for the 2023 year.

[26] This could be helpful for catching individuals who change their name over time, as is often the case for women to marry. In such a case, however, this would only be helpful if the individual registered to vote in a state under their maiden name during a time covered by my cross-sectional files and later changed it. I do not have access to prior names in these records.

combination. If there were no overlapping turnouts across collapsed records – that is, if turnout was never recorded twice in the same election across the collapsed records– I determined that these were duplicate records and I used this collapsed record as the new voting record for that name/DOB combination, assigning a new voter ID and taking the first reported registration date and the maximum of an indicator for turnout in each election in the records. I refer to these as "collapsed voters" in Appendix Table C1. In a few cases, collapsed voters were formed using the same voter IDs for different names/DOB combinations. I allowed this to occur since I was unable to determine which match was preferable. I used randomization to select one observation per name/DOB for the handful of observations that could not be collapsed to ensure my data were unique within state by first name, last name, middle initial, and date of birth.

I limited my sample for parents to individuals who were born between 1928-1995. I chose this range because it covered the years for which the oldest parent could have been approximately 15-60 years old at the time of the child's birth. I limited my sample of children to those who were born between 1988 and 2010.

Appendix Table C1 presents basic statistics about each state-level sample.

Appendix Table C2 presents the share of observations in each state with 1/1/YEAR birthdays and information on coverage of general election turnout in the earliest years.

C2 Matching Process: Students/Voting Records

a. Matching Students to Indiana Voting Records

I matched students to voting records for Indiana using a multi-step process that combined exact and fuzzy matching. This design was informed by the following expectations: (1) that student records were unlikely to include duplicate entries, (2) that most state-level records referred to unique individuals (after the pre-processing described above), (3) that most individuals would register to vote in only one state and appear in only one state (though some would appear in more), and (4) that matches found in Indiana and common destination states for people from Indiana were more likely to be "true matches" than matches found in other states.

I started by exactly matching students to voting records for Indiana. Exact matching was based on a combination of (1) first name, last name, and date of birth or (2) full name (first name, middle name, and last name combined into a single string) and date of birth. I discarded matches based on first and last name that did not match on non-missing middle initials. A small share of students matched to multiple voter IDs or vice versa. To disambiguate these matches, I applied the following hierarchy to pick the best student per voter (and vice versa): (1) I preferred matches that matched on gender over those that did not, (2) I preferred matches that matched on middle initial (including missing) over those that did not, and (3) I preferred full name matches over first/last matches. I used random selection to break ties where needed and then treated these exact matches as true matches.

Second, I matched students to voting records for Indiana using the fastLink package in R. I blocked data by birth year and conducted matching within birth year blocks to speed up the matching process. I matched students to Indiana voter records based on first name and last name (strings) and day of birth, month of birth, and gender (numeric). Matching for strings was conducted using Jaro-Winkler string distance and specified a minimum posterior match probability of 0.90. I included partial matching for string variables. I dropped observations for students or voters that had exactly matched from fastLink output since these had already been accounted for. I refined matches to ensure a 1:1 student to child match was achieved, using randomization to break ties as needed. Fuzzy matching added about 5,900 additional matches to my sample. Fuzzy matches represented $< 1\%$ of matches to Indiana voting records at this point in my sample.

1. Matching Students to Out-of-State Voting Records

I conducted exact matching for out-of-state voting records using the same process I used to exactly match to Indiana voting records. These out-of-state voting files were prepared the same way that the file for Indiana was, pooling different iterations of name/DOB over time for the same voter ID. I did not use fuzzy matching for out-of-state voting records both to save time – fuzzy matching had only minimally increased match rates for Indiana, accounting for $< 1\%$ of matches – and because including fuzzy matches increased the probability of false matches. Informal analyses indicated that pooling records across multiple voter files increased match rates more substantially than fuzzy matching did. This may be in part because the data on names and dates of birth available to me from K-12 and birth records were of relatively high quality.

After exact matching files for all states (including D.C., throughout), I pooled together voting records matches from across all states. Some individuals matched to voting records in multiple states. I did the following to reconcile matches across states:

1. I combined data across non-Indiana states to create a dataset that was unique at the student-by-state level. Each observation in this dataset represented a potential match that could be accepted, incorporated into an existing record (multi-state matches), or discarded.

2. If a student matched to voting records for just one state, I accepted this as a match.

3. If a student matched to voting records for more than one state, I sorted state-level potential matches based on the share of Indiana-born

individuals who reported living in that state, based on the ACS 2022 data (U.S. Census Bureau, 2022).

4. I then iterated through potential matches, starting with the "most likely" and "next most likely" match states and assessing whether these records could be collapsed (i.e., contained no conflicting turnout records) per the process previously described. If the observations could be collapsed, I collapsed these records and created a new record for the individual that incorporated data from both state records. If not, I discarded the "less likely" observation. I repeated this process as needed until I had one match per student.

Appendix Table C3 shows match rates by state for students in the sample.

b. Matching Process: Parents/Voting Records

I matched parents to voting records using a similar process as was described for matching students to voting records, starting with exact matching (all states) and then using fuzzy-matching (Indiana only). Fuzzy matches made up a large share of matches for parents than for children (around 4%). The following notes are relevant re: adapting this process for parents:

- Since gender was not observed for parents, I assigned female gender to individuals listed as mothers and male gender to individuals listed as fathers. I did this because it was necessary to block by both birth year and gender to increase the computational speed of fuzzy matching processes. Unfortunately, using this approach could lead to under-matching for children of same-sex parents. According to estimates from the Williams Institute, there were 4.4 same-sex households per 1,000 households in Indiana and 18.9% of same-sex couples were raising children (The Williams Institute, 2019).
- I started by exactly matching parents to voting records across all states and the District of Columbia, as described.
- I then used fastLink to fuzzy match parents to voting records for Indiana, using the same specification as was used for children.
- Finally, I pooled together matches for parents across states. If a parent matched to one state voting record, I treated this as a match. I determined whether to collapse, keep, or discard other matches using the process described above for student voting records, preferring states that were more likely destinations for individuals from Indiana.

Appendix Table C3 shows match rates to voting records by state for parents in the sample.

Table C1: State Voting Files

State	IN	IL	KY	OH	FL	TX	MI	CA	GA	NC	VA	TN	AZ	MO	MN	SC	KS
A. Basic Information																	
Source Files	20230327, 20220302, 20210115, 20200227, 20190213, 20180901, 20170418	20230318, 20220418, 20210305, 20200303, 20190514, 20180728, 20170418	20230906, 20221011, 20210704, 20200413, 20190502, 20180502, 20170418	20230627, 20220302, 20210716, 20200503, 20191126, 20180628, 20170418	20230913, 20220310, 20210314, 20200422, 20190508, 20180802, 20170418	20230312, 20220916, 20210612, 20200203, 20190224, 20180629, 20170418	20230421, 20220901, 20211103, 20200814, 20190513, 20180717, 20170418	20230905, 20220920, 20210502, 20200510, 20190517, 20180817	20230627, 20220302, 20210416, 20200407, 20190611, 20180705, 20170418	20230922, 20220510, 20210128, 20200408, 20191120, 20180628, 20170418	20230909, 20220827, 20210923, 20200301, 20190312, 20180830, 20170418	20230909, 20220827, 20210923, 20200301, 20190312, 20180830, 20170418	20230318, 20221008, 20210520, 20201001, 20190510, 20180814, 20170418	20231024, 20220822, 20210211, 20200305, 20190510, 20180628, 20170418	20230912, 20220513, 20210708, 20200510, 20191003, 20180731, 20170418	20230511, 20221111, 20210706, 20200510, 20190511, 20180404, 20170418	20231024, 20220714, 20210712, 20200318, 20190503, 20180709, 20170418
N obs (most recent file)	4356818	8221447	3,196,880	7,518,644	14,557,650	16,589,087	7,751,737	23,699,847	7,221,668	6,721,134	5,687,648	4,144,698	4,206,321	4,008,503	3,495,736	3,331,400	1,803,010
B. Full Multiyear Sample																	
N records	5705453	12442218	4648679	9430520	18486097	20218652	12901125	28367236	11435533	11776105	7186070	5290813	7975410	5172402	5284371	4265411	2273565
Unqiue Voter IDs	5469205	10184953	4455856	9147781	17753278	19363210	9788532	27866357	8784322	8760651	6847437	5121014	5758244	4946229	4227071	4109230	2192687
Unique First/Last/Middle Initial/DOB	5695694	12336750	4371751	9373305	18449318	20151584	12135125	28227975	11335171	11643363	7173918	5255638	7774126	5166930	5266540	4262176	2271532
Share duplicated at all	0.003	0.017	0.104	0.012	0.004	0.007	0.113	0.01	0.017	0.022	0.003	0.013	0.049	0.002	0.006	0.001	0.002
Share duplicated 1 time	0.003	0.016	0.064	0.012	0.004	0.006	0.098	0.009	0.015	0.02	0.003	0.011	0.046	0.002	0.006	0.001	0.002
Share duplicated >1 time	0	0.001	0.039	0	0	0	0.015	0	0.002	0.002	0	0.002	0.003	0	0.001	0	0
C. Student Sample																	
N records	1437986	3157957	1149696	2403884	4371612	5767862	3066966	8264329	3346795	3634161	1952263	1336278	1853125	1378366	1293052	968134	611335
Unique First/Last/Middle Initial/DOB	1437986	3157957	1149696	2403884	4371612	5767862	3066966	8264329	3346795	3634161	1952263	1336278	1853125	1378366	1293052	968134	611335
Unqiue Fullname/DOB	1437773	3155626	1149221	2403490	4370761	5765775	3066633	8238050	3345863	3632185	1950974	1335515	1850035	1377956	1292347	968134	611233
Unique Voter IDs	1347226	2639011	1071156	2298952	4162397	5546892	2427302	8139041	2553522	2485557	1848108	1280028	1509759	1291902	1077646	917864	576691
Share collapsed voters	0.001	0.007	0.035	0.004	0.001	0.003	0.06	0.002	0.008	0.018	0.002	0.006	0.025	0.001	0.002	0.001	0.001
Share duplciate names/DOB (randomly selected)	0.001	0.006	0.044	0.005	0.001	0.001	0.029	0.004	0.004	0.005	0.001	0.003	0.012	0.001	0.004	0.001	0.001
Share missing gender	0.001	0	0	0.021	0.002	0.001	0	0.032	0	0.002	0	0	0.009	0.012	0.013	0	0
Share male	0.479	0.466	0.462	0.486	0.469	0.47	0.479	0.48	0.468	0.467	0.469	0.444	0.491	0.478	0.477	0.446	0.465
Share missing middle initial	0.096	0.184	0.044	0.049	0.129	0.139	0.046	0.266	0.091	0.063	0.052	0.107	0.128	0.063	0.067	0.069	0.088
D. Parent Sample																	
N records	4981655	10826999	3832568	8244465	16232400	17145059	10671666	24165166	9688078	9778532	6234302	4642110	6869588	4483119	4638058	3819708	1959852
Unique First/Last/Middle Initial/DOB	4981655	10826999	3832568	8244465	16232400	17145059	10671666	24165166	9688078	9778532	6234302	4642110	6869588	4483119	4638058	3819708	1959852
Unqiue Fullname/DOB	4980683	10818365	3831119	8243128	16228790	17139162	10670235	24039966	9684629	9773925	6229685	4639290	6861485	4482286	4636125	3819699	1959596
Unique Voter IDs	4778105	8873598	3720408	7997387	15578906	16381925	8076747	23738270	7457227	7455609	5928725	4493551	4885093	4285832	3660270	3678737	1890448
Share collapsed voters	0.001	0.003	0.017	0.002	0.001	0.002	0.039	0.001	0.005	0.005	0.001	0.004	0.014	0	0.001	0	0
Share duplciate names/DOB (randomly selected)	0.001	0.004	0.026	0.004	0.001	0.001	0.02	0.004	0.003	0.003	0	0.002	0.01	0.001	0.002	0	0.001
Share missing gender	0.001	0	0	0.011	0.001	0.001	0	0.019	0	0.001	0	0	0.004	0.008	0.009	0	0
Share male	0.469	0.468	0.459	0.467	0.458	0.458	0.471	0.468	0.453	0.455	0.455	0.445	0.476	0.461	0.466	0.45	0.466
Share missing middle initial	0.094	0.194	0.063	0.072	0.159	0.138	0.071	0.234	0.097	0.07	0.067	0.103	0.112	0.071	0.053	0.09	0.066
	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34

Table C1 State Voting Files (cont)

State	NJ	CO	NY	WI	AL	MA	PA	NV	AR	WV	WA	MS	MD	MT	CT	OR	OK
A. Basic Information																	
Source Files	20231024, 20220720, 20210711, 20200510, 20190403, 20180306, 20170418	20230711, 20220426, 20210703, 20200123, 20190508, 20180808, 20170418	20231024, 20220330, 20210722, 20200305, 20190502, 20180814, 20170418	20230428, 20220831, 20210716, 20200510, 20190623, 20180602, 20170418	20230614, 20221231, 20211114, 20201009, 20190827, 20180707, 20170418	20221231, 20211011, 20200510, 20190510, 20180511, 20170418	20231024, 20220830, 20210520, 20200320, 20190509, 20180822, 20170418	20230720, 20220825, 20210613, 20200111, 20190604, 20180810, 20170418	20231024, 20220326, 20210721, 20200207, 20190513, 20180901, 20170418	20231024, 20220302, 20210311, 20200227, 20190322, 20180814, 20170418	20230628, 20220324, 20211016, 20200303, 20190512, 20180715, 20170418	20231024, 20221027, 20210709, 20200320, 20190510, 20180323, 20170418	20221231, 20210915, 20200507, 20190620, 20180222, 20170418	20230518, 20220823, 20211122, 20200510, 20190923, 20180803, 20170418	20230524, 20220817, 20210713, 20201001, 20190603, 20180718, 20170418	20230516, 20221201, 20210205, 20200225, 20190508, 20180726, 20170418	20231024, 20220609, 20210208, 20200225, 20190503, 20180806, 20170418
N obs (most recent file)	6127664	3824119	14416297	4785050	3438589	4528434	8170707	2024695	1608203	1073592	4941569	1932177	4170282	692447	2329404	3268212	2085813
B. Full Multiyear Sample	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
N records	7526668	6165519	16656726	5475510	4321346	5665793	10305077	2653750	2130431	1470782	6257229	2217633	4474663	896633	3094009	4171226	2743766
Unqiue Voter IDs	7214891	4815307	16135640	3950204	4068510	5515137	9965863	2541791	2054696	1420480	6056341	1790807	4302532	867284	2997971	3845660	2657868
Unique First/Last/Middle Initial/DOB	7502199	6152683	16518668	5372533	4311115	5648730	10284883	2642904	2127835	1469240	6247200	2199101	4468859	896254	3051966	4166289	2729206
Share duplicated at all	0.006	0.004	0.016	0.037	0.005	0.006	0.004	0.008	0.002	0.002	0.003	0.017	0.003	0.001	0.025	0.002	0.01
Share duplicated 1 time	0.006	0.004	0.015	0.036	0.004	0.006	0.004	0.008	0.002	0.002	0.003	0.016	0.002	0.001	0.017	0.002	0.009
Share duplicated >1 time	0	0	0.001	0.002	0	0	0	0	0	0	0	0.001	0	0	0.008	0	0.001
C. Student Sample																	
N records	1942939	1910424	3771318	513538	1093508	1469044	2526556	761559	563787	354122	1694708	232942	924306	217965	746934	1169055	683709
Unique First/Last/Middle Initial/DOB	1942939	1910424	3771318	513538	1093508	1469044	2526556	761559	563787	354122	1694708	232942	924306	217965	746934	1169055	683709
Unqiue Fullname/DOB	1942907	1909701	3770546	513512	1092770	1468504	2526150	760986	563679	354100	1694206	232938	923940	217961	746861	1168606	683525
Unique Voter IDs	1867607	1473329	3622651	449030	1024984	1425856	2419954	724205	533181	334302	1627154	203697	875216	207124	721160	1049705	655907
Share collapsed voters	0.002	0.002	0.005	0.007	0.002	0.002	0.001	0.005	0.001	0.001	0.002	0.001	0	0	0.006	0.001	0.005
Share duplciate names/DOB (randomly selected)	0.001	0.001	0.003	0.02	0.001	0.001	0.001	0.001	0	0	0	0.002	0.001	0	0.011	0.001	0.001
Share missing gender	0.021	0	0.001	0.017	0	0.01	0.006	0.022	0.016	0.002	0.001	0.031	0	0.03	0	0.012	0.01
Share male	0.481	0.485	0.462	0.456	0.456	0.478	0.468	0.499	0.485	0.477	0.479	0.396	0.463	0.504	0.468	0.5	0.474
Share missing middle initial	0.166	0.073	0.182	0.049	0.046	0.181	0.166	0.182	0.057	0.054	0.108	0.06	0.067	0.197	0.151	0.118	0.044
D. Parent Sample																	
N records	6499618	5273021	14684844	5179350	3773994	4964360	9080118	2278915	1835687	1291345	5464414	2114266	4135014	794910	2692424	3592916	2393097
Unique First/Last/Middle Initial/DOB	6499618	5273021	14684844	5179350	3773994	4964360	9080118	2278915	1835687	1291345	5464414	2114266	4135014	794910	2692424	3592916	2393097
Unqiue Fullname/DOB	6498685	5269987	14680461	5178942	3771184	4960203	9078638	2277784	1835366	1291174	5462862	2114049	4132092	794871	2691994	3591977	2392552
Unique Voter IDs	6225353	4102368	14260414	3704176	3548204	4830074	8776220	2181943	1772329	1248880	5284703	1707103	3977880	768931	2607104	3328351	2319901
Share collapsed voters	0.002	0.001	0.006	0.006	0.001	0.002	0.001	0.003	0.001	0.001	0.001	0.002	0	0	0.003	0.001	0.004
Share duplciate names/DOB (randomly selected)	0.002	0.001	0.003	0.012	0.001	0.001	0.001	0.001	0	0	0	0.006	0.001	0	0.008	0	0.001
Share missing gender	0.012	0	0	0.009	0	0.004	0.004	0.015	0.009	0.001	0	0.015	0	0.013	0.001	0.01	0.006
Share male	0.46	0.48	0.453	0.474	0.44	0.464	0.469	0.489	0.451	0.464	0.476	0.436	0.454	0.486	0.457	0.487	0.456
Share missing middle initial	0.252	0.065	0.257	0.068	0.059	0.182	0.167	0.166	0.065	0.058	0.117	0.128	0.088	0.207	0.187	0.097	0.05

Table C1 State Voting Files (cont)

State	IA	ND	AK	LA	DC	NH	HI	VT	DE	NE	UT	WY	SD	ME	ID	RI	NM
A. Basic Information																	
Source Files	20231024, 20220823, 20210304, 20200303, 20190510, 20180825, 20170418	20230919, 20221014, 20211019, 20200501, 20190513, 20180321, 20170418	20230906, 20221231, 20211124, 20201009, 20190702, 20180815, 20170418	20231024, 20220412, 20210122, 20201001, 20190515, 20180625, 20170418	20230918, 20220402, 20210130, 20200302, 20190503, 20180301, 20170418	20230616, 20220822, 20210325, 20200303, 20191022, 20180815, 20170418	20230912, 20220823, 20210703, 20201001, 20190513, 20180730, 20170418	20230606, 20220302, 20210702, 20200212, 20190512, 20180611, 20170418	20230520, 20220824, 20210703, 20200330, 20190510, 20181012, 20171226	20231024, 20221004, 20210713, 20200510, 20191126, 20180711, 20170418	20230624, 20220330, 20210708, 20200407, 20190503, 20180822, 20170418	20230919, 20221021, 20210113, 20200302, 20190402, 20180726, 20170418	20230920, 20220824, 20210122, 20200218, 20190511, 20180608, 20170418	20230607, 20220302, 20210702, 20200510, 20190717, 20180428, 20170418	20221231, 20210316, 20200429, 20190503, 20180821, 20170418	20230920, 20220825, 20210316, 20200422, 20190510, 20180717, 20170418	20230904, 20221128, 20210709, 20200224, 20190503, 20180821, 20170418
N obs (most recent file)	2074150	422604	530462	2854325	534312	1002660	793102	465359	722662	1174456	1511677	284937	576989	1043961	941511	723223	1269553
B. Full Multiyear Sample	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
N records	2626605	467429	573139	6345180	583460	1239075	813370	777785	1203683	1492126	2522630	332270	838057	1542682	2093193	988627	2056977
Unqiue Voter IDs	2524616	382552	461136	3673147	466332	950420	682982	590845	843122	1433984	1996854	262568	630442	1230195	1164533	918223	1563649
Unique First/Last/Middle Initial/DOB	2625307	466729	570569	6108844	579455	1233452	808705	774037	1201947	1491173	2518351	326310	836192	1535257	2076772	986207	2050535
Share duplicated at all	0.001	0.003	0.009	0.07	0.013	0.009	0.011	0.01	0.003	0.001	0.003	0.036	0.004	0.009	0.016	0.005	0.006
Share duplicated 1 time	0.001	0.003	0.008	0.057	0.013	0.008	0.011	0.009	0.003	0.001	0.003	0.035	0.004	0.009	0.015	0.005	0.006
Share duplicated >1 time	0	0	0	0.013	0.001	0	0	0	0	0	0	0	0	0.001	0	0	0
C. Student Sample																	
N records	737357	47339	73273	1439071	95841	130632	65366	165939	308410	396416	811692	24161	145140	295802	477454	267112	459056
Unique First/Last/Middle Initial/DOB	737357	47339	73273	1439071	95841	130632	65366	165939	308410	396416	811692	24161	145140	295802	477454	267112	459056
Unqiue Fullname/DOB	737231	47328	73174	1438844	95840	130628	62603	165925	308319	396407	810105	24160	145128	295782	477409	267095	458904
Unique Voter IDs	690220	41450	61736	904944	87602	110542	60524	142321	213351	369809	628462	22124	116856	247409	279513	236649	371619
Share collapsed voters	0	0	0	0.02	0.001	0.001	0.003	0.004	0.001	0	0.001	0.002	0.001	0.005	0.004	0.002	0.003
Share duplciate names/DOB (randomly selected)	0	0	0	0.039	0.002	0.001	0.002	0.001	0.001	0	0.001	0.054	0.001	0.002	0.009	0	0.001
Share missing gender	0	0.007	0	0	0.02	0.005	0.024	0.034	0.015	0.015	0.011	0.02	0.014	0.016	0	0.009	0
Share male	0.474	0.466	0.432	0.441	0.432	0.432	0.406	0.489	0.489	0.48	0.497	0.467	0.477	0.482	0.466	0.475	0.476
Share missing middle initial	0.045	0.087	0.044	0.106	0.104	0.051	0.095	0.144	0.066	0.045	0.085	0.028	0.02	0.149	0.115	0.117	0.106
D. Parent Sample																	
N records	2238683	447267	548207	5440847	557106	1186838	774775	695867	1055328	1289982	2110564	316287	778330	1400007	1849834	855874	1825648
Unique First/Last/Middle Initial/DOB	2238683	447267	548207	5440847	557106	1186838	774775	695867	1055328	1289982	2110564	316287	778330	1400007	1849834	855874	1825648
Unqiue Fullname/DOB	2238295	447164	547715	5439465	557078	1186795	751010	695660	1055146	1289889	2107926	316266	778289	1399881	1849705	855704	1825358
Unique Voter IDs	2152225	365130	440977	3130170	443805	908842	650852	520493	731651	1240326	1693605	248391	583775	1105773	1028464	804840	1374124
Share collapsed voters	0	0	0.002	0.008	0.002	0.001	0.003	0.003	0	0	0.001	0.001	0.001	0.003	0.002	0.002	0.002
Share duplciate names/DOB (randomly selected)	0	0.001	0.003	0.024	0.005	0.003	0.003	0.002	0.001	0	0.001	0.016	0.001	0.002	0.006	0.001	0.001
Share missing gender	0	0.003	0	0	0.016	0.005	0.004	0.013	0.01	0.005	0.009	0.008	0.008	0.007	0	0.005	0
Share male	0.468	0.486	0.493	0.445	0.457	0.467	0.476	0.473	0.465	0.47	0.484	0.477	0.475	0.465	0.468	0.461	0.463
Share missing middle initial	0.038	0.071	0.049	0.084	0.121	0.077	0.08	0.163	0.099	0.049	0.097	0.08	0.038	0.109	0.091	0.146	0.128

Table C2 State Voting File Details

State	Share IN born	Rank	N unique name/DOB	N 1/1/ Birthdays	Share 1/1 Birthdays	Voted: General 2000	Voted: General 2002	Voted: General 2004
IN	0.687	1	5,695,701	24,348	0.00	0.25	0.19	0.32
FL	0.036	2	18,449,324	61,356	0.00	0.18	0.17	0.28
IL	0.028	3	12,336,749	3,210,250	0.26	0.30	0.24	0.38
TX	0.022	4	20,151,573	81,394	0.00	0.14	0.12	0.25
OH	0.021	5	9,373,307	42,863	0.00	0.32	0.24	0.45
KY	0.021	6	4,371,750	991,854	0.23	0.00	0.16	0.26
MI	0.021	7	12,135,128	4,934,927	0.41	0.29	0.23	0.37
CA	0.017	8	28,227,922	143,078	0.01	0.24	0.18	0.34
TN	0.014	9	5,255,653	17,832	0.00	0.23	0.20	0.31
GA	0.012	10	11,335,214	5,356,079	0.47	0.21	0.17	0.29
AZ	0.011	11	7,774,106	3,835,371	0.49	0.04	0.14	0.26
NC	0.010	12	11,643,395	4,864,693	0.42	0.16	0.15	0.26
CO	0.008	13	6,152,672	2,651,443	0.43	0.19	0.18	0.30
VA	0.007	14	7,173,919	29,265	0.00	0.00	0.15	0.29
MO	0.007	15	5,166,951	172,414	0.03	0.27	0.25	0.40
WI	0.006	16	5,372,539	1,701,903	0.32	0.00	0.32	0.49
SC	0.006	17	4,262,176	14,946	0.00	0.19	0.17	0.28
WA	0.006	18	6,247,207	26,728	0.00	0.22	0.19	0.31
AL	0.005	19	4,311,098	15,189	0.00	0.22	0.21	0.33
PA	0.005	20	10,284,915	44,758	0.00	0.29	0.23	0.40
NY	0.004	21	16,518,657	134,572	0.01	0.10	0.20	0.35
MN	0.004	22	5,266,538	2,110,096	0.40	0.40	0.39	0.51
OR	0.003	23	4,166,295	826,386	0.20	0.17	0.20	0.32
MD	0.003	24	4,468,863	54,077	0.01	0.28	0.26	0.39
AR	0.003	25	2,127,837	6,453	0.00	0.24	0.23	0.33
OK	0.003	26	2,729,206	9,225	0.00	0.24	0.23	0.34
NV	0.003	27	2,642,902	7,781	0.00	0.11	0.11	0.19
KS	0.003	28	2,271,535	8,062	0.00	0.18	0.17	0.35
MA	0.002	29	5,648,752	19,835	0.00	0.30	0.27	0.37
IA	0.002	30	2,625,307	11,801	0.00	0.13	0.27	0.42
NJ	0.002	31	7,502,192	58,113	0.01	0.26	0.19	0.36
MS	0.002	32	2,199,105	184,700	0.08	0.15	0.12	0.24
UT	0.002	33	2,518,351	704,256	0.28	0.22	0.18	0.32
NM	0.002	34	2,050,535	1,059,597	0.52	0.24	0.24	0.38
LA	0.002	35	6,108,844	4,107,221	0.67	0.37	0.28	0.46
ID	0.001	36	2,076,772	1,420,533	0.68	0.00	0.20	0.30
MT	0.001	37	896,257	2,461	0.00	0.21	0.20	0.28
WV	0.001	38	1,469,238	4,823	0.00	0.26	0.19	0.36
NE	0.001	39	1,491,173	43,403	0.03	0.24	0.20	0.37
CT	0.001	40	3,051,956	23,539	0.01	0.11	0.10	0.26
HI	0.001	41	808,705	97,447	0.12	0.23	0.27	0.32
AK	0.001	42	570,569	49,090	0.09	0.00	0.00	0.43
ME	0.001	43	1,535,257	617,048	0.40	0.00	0.11	0.10
ND	0.001	44	466,729	27,906	0.06	0.02	0.26	0.49
DC	0.000	45	579,455	76,409	0.13	0.22	0.18	0.29
WY	0.000	46	326,310	35,185	0.11	0.00	0.00	0.38
NH	0.000	47	1,233,452	76,720	0.06	0.21	0.22	0.38
SD	0.000	48	836,192	449,669	0.54	0.30	0.35	0.42
RI	0.000	49	986,207	146,038	0.15	0.00	0.20	0.31
DE	0.000	50	1,201,947	378,385	0.31	0.11	0.17	0.30
VT	0.000	51	774,037	374,132	0.48	0.19	0.18	0.33

Share IN-born is based on U.S. Census Bureau ACS estimates from 2022. General election indicators are mean of indicators for having voted in the indicated election and are presented as a way to assess coverage of elections in early years.

Table C3 Match Rates by State

State	N Matched	Share Matched	Share Matched (if any)	Share Males Matched (if any vote match)	N has Mom	N Mom Matched	Share Mom Matched	Share Mom Matched (if any vote match)	N Has Dad	N Dad Matched	Share Dad Matched	Share Dad Matched (any vote match)
IN	503018	0.730	0.955	0.517	483798	287520	0.594	0.979	444526	331042	0.745	0.965
FL	9968	0.014	0.019	0.517	483798	4277	0.009	0.015	444526	7355	0.017	0.021
IL	5123	0.007	0.010	0.465	483798	1107	0.002	0.004	444526	2507	0.006	0.007
TX	6440	0.009	0.012	0.527	483798	1300	0.003	0.004	444526	2315	0.005	0.007
OH	7896	0.011	0.015	0.495	483798	1179	0.002	0.004	444526	2296	0.005	0.007
KY	3508	0.005	0.007	0.479	483798	1540	0.003	0.005	444526	2886	0.006	0.008
MI	1430	0.002	0.003	0.504	483798	859	0.002	0.003	444526	1358	0.003	0.004
CA	5721	0.008	0.011	0.522	483798	616	0.001	0.002	444526	1119	0.003	0.003
TN	3722	0.005	0.007	0.496	483798	1044	0.002	0.004	444526	1884	0.004	0.005
GA	759	0.001	0.001	0.447	483798	390	0.001	0.001	444526	590	0.001	0.002
AZ	704	0.001	0.001	0.521	483798	415	0.001	0.001	444526	566	0.001	0.002
NC	763	0.001	0.001	0.494	483798	465	0.001	0.002	444526	649	0.001	0.002
CO	1086	0.002	0.002	0.501	483798	286	0.001	0.001	444526	360	0.001	0.001
VA	1943	0.003	0.004	0.529	483798	275	0.001	0.001	444526	498	0.001	0.001
MO	1839	0.003	0.003	0.499	483798	319	0.001	0.001	444526	610	0.001	0.002
WI	287	0.000	0.001	0.460	483798	158	0.000	0.001	444526	221	0.000	0.001
SC	1412	0.002	0.003	0.496	483798	573	0.001	0.002	444526	967	0.002	0.003
WA	2173	0.003	0.004	0.575	483798	181	0.000	0.001	444526	334	0.001	0.001
AL	1341	0.002	0.003	0.478	483798	452	0.001	0.002	444526	777	0.002	0.002
PA	1518	0.002	0.003	0.514	483798	240	0.000	0.001	444526	422	0.001	0.001
NY	1777	0.003	0.003	0.464	483798	271	0.001	0.001	444526	468	0.001	0.001
MN	185	0.000	0.000	0.454	483798	60	0.000	0.000	444526	99	0.000	0.000
OR	434	0.001	0.001	0.495	483798	86	0.000	0.000	444526	173	0.000	0.001
MD	291	0.000	0.001	0.436	483798	76	0.000	0.000	444526	148	0.000	0.000
AR	539	0.001	0.001	0.542	483798	128	0.000	0.000	444526	279	0.001	0.001
OK	592	0.001	0.001	0.522	483798	138	0.000	0.000	444526	245	0.001	0.001
NV	1187	0.002	0.002	0.526	483798	227	0.000	0.001	444526	469	0.001	0.001
KS	562	0.001	0.001	0.577	483798	93	0.000	0.000	444526	210	0.000	0.001
MA	748	0.001	0.001	0.477	483798	53	0.000	0.000	444526	108	0.000	0.000
IA	1095	0.002	0.002	0.505	483798	140	0.000	0.000	444526	275	0.001	0.001
NJ	457	0.001	0.001	0.466	483798	67	0.000	0.000	444526	116	0.000	0.000
MS	80	0.000	0.000	0.438	483798	77	0.000	0.000	444526	106	0.000	0.000
UT	262	0.000	0.000	0.542	483798	71	0.000	0.000	444526	92	0.000	0.000
NM	75	0.000	0.000	0.520	483798	39	0.000	0.000	444526	54	0.000	0.000
LA	78	0.000	0.000	0.641	483798	50	0.000	0.000	444526	110	0.000	0.000
ID	<=25	[]	[]	[]	483798	<=25	[]	[]	444526	28	0.000	0.000
MT	290	0.000	0.001	0.566	483798	60	0.000	0.000	444526	127	0.000	0.000
WV	292	0.000	0.001	0.589	483798	63	0.000	0.000	444526	162	0.000	0.000
NE	297	0.000	0.001	0.566	483798	32	0.000	0.000	444526	103	0.000	0.000
CT	216	0.000	0.000	0.495	483798	41	0.000	0.000	444526	56	0.000	0.000
HI	<=25	[]	[]	0.389	483798	<=25	[]	[]	444526	<=25	[]	[]
AK	33	0.000	0.000	0.636	483798	<=25	[]	[]	444526	34	[]	[]
ME	<=25	[]	[]	0.391	483798	<=25	[]	[]	444526	<=25	[]	[]
ND	<=25	[]	[]	0.444	483798	<=25	[]	[]	444526	<=25	[]	[]
DC	79	0.000	0.000	0.418	483798	<=25	[]	[]	444526	<=25	[]	[]
WY	<=25	[]	[]	0.500	483798	<=25	[]	[]	444526	<=25	[]	[]
NH	<=25	[]	[]	0.563	483798	<=25	[]	[]	444526	<=25	[]	[]
SD	<=25	[]	[]	0.462	483798	<=25	[]	[]	444526	<=25	[]	[]
RI	36	0.000	0.000	0.472	483798	<=25	[]	[]	444526	<=25	[]	[]
DE	<=25	[]	[]	0.467	483798	<=25	[]	[]	444526	<=25	[]	[]
VT	<=25	[]	[]	0.476	483798	<=25	[]	[]	444526	<=25	[]	[]

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Note: I list all dates I have access to, whether or not all years were used in the final analysis.