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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 citizenship is a foundational purpose of public education, yet little is known about whether or how K-12 schools impact civic engagement. Using education, birth, and voting records for nine cohorts of students in Indiana, I estimate and assess the validity of high school effects on voting. School effects on voting are significant and practically meaningful: a one standard deviation increase in school civic effects increases the probability of voting by 2.1 percentage points (5.3%) and also predicts increases in voter registration and times voting. School civic effects are positively associated with peer parental civic norms.

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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 47% of voters aged 18 to 29 cast a vote, compared to 64% of eligible voters overall (Medina et al., 2025; Ballotpedia, 2024). Between 2000 and 2024, the self-reported turnout gap for young (18-24) and older (65+) voters averaged 34 percentage points, with larger gaps often observed for local elections (U.S. Census Bureau, 2025; 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, 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 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.

While voting is just one dimension of civic engagement – a term that encompasses a range of political and pro-social behaviors, from political campaigning to volunteering (Putnam, 1996; Adler & Goggin, 2005) – my focus on voting is justified by the fact that voting is the core mechanism for eligible citizens seeking to influence their government and a fundamental right and responsibility of democratic citizenship. 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 for federal elections 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 behaviors (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 present my results in several parts. I start by presenting new descriptive evidence on K-12 schools and adult voting. I show that voter turnout varies widely across schools. The gap in voter turnout between high and low turnout schools in Indiana is about 23 percentage points. I also show that raw differences in civic engagement are associated with school demographic and socioeconomic characteristics, motivating 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 – significantly predict adult voting, as does parental civic engagement. This supports the use of these measures as controls in 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 2.1 percentage point increase in the probability of a student voting in their first four age-eligible election cycles, a 5.3% increase over the sample mean. Relative effects are larger (around 20%) for voting outside of general/primary elections, though actual effects are small in magnitude. I assess the validity of these estimates in several ways. First, I show that my leave-cohort-out school effect estimates are significant predictors of actual outcomes. Second, I estimate forecast bias using four variables I observe but do not use to calculate school effect, finding no significant bias for civic school effects. Third, I use data on siblings and high school movers to show that school effects on civic outcomes remain significant predictors of actual student outcomes within families and for students who moved into the high school. These additional analyses strengthen the argument that my civic school effects can be interpreted as the causal impact of a high school on adult voting behavior, though, absent a source of random variation in school assignment, I cannot prove causality.

Existing studies have highlighted the importance of school effects on both test scores and non-cognitive (or socio-emotional) skills. I find that school civic effects are not strongly related to school effects on these other outcomes, suggesting that civic effects are not primarily driven by improvements in these dimensions. Prior research has shown that effects on one type of student outcome are not necessarily strongly correlated with effects on other types of outcomes, both for teachers (Jackson, 2018; Gershenson, 2016) and schools (Beuermann et al., 2023). I caveat this by noting that I find evidence of bias for my school effect estimates for non-civic outcomes (particularly for student test scores) and that my measure of non-cognitive outcomes may not fully capture non-cognitive skills.

I then present descriptive analyses of the school-level factors associated with school effects on civic outcomes. Schools with high average levels of maternal civic engagement – a proxy for peer group civic norms – have larger effects on voting, pointing to peer effects as a potential mechanism. I also consider the relationship between civic school effects and civics-related coursework and extracurricular programs using data on AP exam participation and performance and original data I collect on high school activities. I find suggestive evidence that performance on the AP U.S. Government and Politics exam is positively related to school civic effects in a sub-sample of schools, though this finding is not robust. I do not find 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 traditional public high schools on adult voting. In doing so, it adds to the slim but growing quasi-experimental literature on the effects of schools on civic outcomes. Using randomized enrollment lotteries, Gill et al. (2020) 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. Other studies have explored links between peer composition within schools and political engagement, attitudes, and partisanship (Kaplan et al., 2025; Campbell, 2006; Billings et al., 2021; Chin, 2025). Outside of the United States, Briole et al. (2025) report results from a randomized evaluation of a civics intervention implemented in 200 schools in France, Greece, and Spain, finding declines in absences and disciplinary events, increases in academic achievement, and increases in the diversity of self-reported friendship networks. Other related research looks at voting and institutions of higher education. The approach of my study is closely related to recent work by Bell et al. (2024), who estimate the effects of colleges on voting behavior. In addition, Firoozi & Geyn (2025) show that access to Cal-Grant, a tuition-free 4-year college program in California, increased turnout in the 2020 presidential election by 4-12 percentage points.

More broadly, this paper contributes to the extensive literature exploring the link between education and civic engagement (Converse, 1972; Dee, 2004; Milligan et al., 2004; Sondheimer & Green, 2010). This paper also relates to the literature on political socialization and the so-called "critical age" hypothesis, which emphasizes the importance of experiences during adolescence and early adulthood in political formation (Mannheim, 1952; Jennings & Niemi, 2015; Krosnick & Alwin, 1989). It also speaks to a large body of work emphasizing the role of contextual factors – such as election processes, local political environments, and social networks – in shaping political participation (Cantoni & Pons, 2022; Gerber et al., 2016; Braconnier et al., 2017; Perez-Truglia & Cruces, 2017; Dyck et al., 2009). Finally, it 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]

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

[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).

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 (2006) 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. About 75% of the population identifies as white, non-Hispanic (U.S. Census Bureau, 2026). Indiana's voter turnout has been low relative to other states, ranking 41 out of 50 states in the 2016 presidential election and 46th in 2020 (Szarleta et al., 2023). In comparative studies of state election laws such as the Cost of Voting Index, Indiana's voting laws are characterized as relatively restrictive (Schraufnagel et al., 2020). Same day voter registration is not allowed in Indiana – voter registration must be submitted at least 29 days before the election in which the individual intends to vote – and the state has not implemented automatic voter registration policies (Ballotpedia, 2025). A government-issued photo

ID is required when voting and absentee voting by mail is allowed only for voters who meet specific eligibility criteria (Movement Advancement Project, 2026).

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 school students. 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 here to students born between 1988 and 2009 [2] I exclude students enrolled in alternative schools, special education schools, juvenile correction schools, virtual schools, adult education schools, very small schools, schools that did not serve students through 10th grade, and schools that closed before 2019 or opened in 2016 or later.[3] 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 (column 1) and estimation (column 2)

[2]This affects < 0.18 % of observations at this stage in the data cleaning process.

[3]There are seven schools in my analytic sample that appear to have served students in my sample in grade 9 for the first time only after SY 2007-08. The latest appeared to start serving grade 9 in 2013. Five 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).

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 based on name and date-of-birth 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).[4] 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 indicators for participating in an election in a year. 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 during the period I observe in my cross-sectional files, as can be the case for women who marry.[5] I use both fuzzy and exact matching to match to records in Indiana

[4]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).

[5]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.

and exact matching only for other states. Matching is based on student names and dates-of-birth. 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). Voting histories ostensibly contain data on voter turnout in elections dating back to the year 2000 (Indiana) for voters who appear in these cross-sectional files; however, voting histories for individuals who had been registered in the state but were later removed would, presumably, be lost. For this reason, it seems, my measures of turnout are generally higher for more recent cohorts of students and their parents. (See Appendix Table A13). Using these approaches, I match 76% of students in my initial sample to voting records (column 1). The vast majority (97%) of individuals in my merging sample who matched to voting records matched to an Indiana voting record.[6] 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 year of student birth. Match rates are lower for earlier birth years.

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. (My main results are not sensitive to imputing test scores for students missing scores in both subjects). 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.e., < 20 students in the sample). I also limit my estimation sample to individuals who were at least 18 years old by November 1, 2019 and were not 18 or older when they first enrolled in 9th grade. This ensures I am able to observe the individual's voting behavior for at least four election cycles.[7] This leaves me with a sample of approximately 456,700 unique students in 335 schools. I also identify a

[6] My main results hold when I consider only matches to Indiana state voting records.

[7] 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.

subsample of siblings who attended at least two different schools (column 3) and students who moved from one school in my sample to another (column 4). I use these subsamples in tests to assess the validity of my school effect estimates.

c Measures

I measure civic engagement using an index of voting outcomes measured 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 general elections, (3) indicators for voting in each of the first and second age-eligible primary elections, and (4) the number of years an individual was recorded voting in a non-general, non-primary elections in odd and even years in the first four age-eligible election cycles.[8] I use principal components analysis to create a single civic index using these measures, which I standardize to have a mean of 0 and a standard deviation of 1.

I construct a similar measure of maternal and paternal prior civic engagement, using measures of voting behavior in the eight election cycles (i.e., years) before the child entered 9th grade. The measures I use to construct the maternal/paternal civic engagement measure are as follows: an indicator that is equal to 1 if the individual registered to vote before the year the child entered 9th grade; the number of general elections in the eight pre-9th grade election cycles the parent voted in (maximum possible=4); the number of primary elections in the eight pre-9th grade election cycles the parent voted in; and the number of years the parent was recorded voting outside of a general or primary election in the eight pre-9th grade election cycles. I use principal components analysis to create a single parental civic engagement index and standardize that index measure, as before.[9] Since I only use voting outcomes from the year 2000 onward, parents of children who entered 9th grade in SY

[8]This "number of years" indicator varies between 0 and 4 and is coded based on an indicator for participating in any "other" (non-primary, non-general) election in an even year and an indicator for participating in "any" election in an odd year in the L2 uniform files. These non-primary, non-general elections may include local elections. A value of 4 would indicate that an individual participated at least once in an election in each of the two odd years and at least once in an "other" election in each of the two even years of their first four age-eligible election cycles.

[9]The parental civic engagement indices are defined at the child-level, so siblings who

2007-08 have one fewer (odd) year of voting data available for this measure. Both parental and student civic engagement measures vary by cohort and are lower for earlier cohorts, as shown in Appendix Table A13. This likely reflects the fact that voting data is less complete for earlier years. There is also secular variation in turnout over the election years used to construct these measures.

Summary statistics and additional detail for measures used as outcomes for estimating school effects 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. My main results are not sensitive to including interactions with tests taken in the fall when estimating school effects. 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 assessments 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 they would have been expected to take this assessment for the first time in 8th grade, as was reported for approximately 26% of individuals in my sample. In contrast, about 95% of students who took the English 10 exam were reported as being in 10th grade. For this reason, I show results using the English 10 exam only for assessments reported being taken in grade 9 or later in the student's first grade 9 year 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

share the same parents could have different values of these observations for different children.

within grade/year (ISTEP) or testing year (ECA).

I create an index measure of non-cognitive outcomes 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[10], 9th grade GPA[11], 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 set to missing unexcused absences for individuals with reported enrollment of < 100 days for this and the lagged absence measure used as a control. This affects $< 5\%$ of observations. I replace missing unexcused absences from grade 9 with the mean. GPA is missing for about 34% of students in my sample. To address this issue, I impute missing 9th grade GPA based on a linear regression of GPA on all other components of the non-cognitive outcome measure and 8th grade test scores in math and ELA.[12] I consider results from other specifications of the non-cognitive index, including those that do not impute GPA, as robustness checks. Since the measures used to create this index are not direct measures of non-cognitive skills, such as persistence or self-regulation, but rather measures of outcomes that may be associated with these skills, I exercise caution when interpreting these measures. It is possible, for instance, that this index also reflects aspects of the school environment, such as disciplinary reporting policies.

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.

[10] I define this as the number of passing grades received over all courses taken. I do not count "no grade awarded" classes in these calculations 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 numbers of courses reported.

[11] I calculate this based on letter grades for classes that received letter grades.

[12] For this imputation, I replace missing scores in either subject with non-missing scores from the other subject. I include an indicator for having a missing score in either subject in the measures used for imputation.

Finally, I also estimate school effects on taking the SAT or ACT, which I consider to be a reflection of college-going behavior. (Unfortunately, I do not have access to data on college enrollment or degree attainment). I count only tests taken in the student's first 9th grade year or the four years that follow. 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, about 60% of students are observed voting at least once, compared to only 37% at schools at the 10th percentile, a 23 percentage point gap (Panel A). A similar gap (15.2 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 A3 plots the share of students at a school who ever voted against the share of students who qualify for free or reduced-price lunch, the share of Black students, average 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 7.8 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 θ_e is a vector of dummies for a student's first age-eligible election.[13] By including both cohort and first election fixed effects I am able to control for any common shocks that affect both measured and actual political participation for young people across the state, such as differences in completeness of voting records for earlier cohorts, 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 & Miller, 2014; Mountjoy & Hickman, 2021). The key assumption of

[13] 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.

this model is one of conditional independence: to interpret these estimates as reflections of a school's causal effect on civic outcomes, 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, 2023). 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 2). The individual student-level controls included in X_i include student demographics (race/ethnicity[14]; gender; special education, English learner, and free or reduced-price lunch status; age at the start of 9th grade[15]); 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[16], 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, and an indicator for missing attendance). The school-cohort controls included in X_i are the number of students in the cohort; leave-self out averages of maternal civic score, paternal civic score, and 8th grade test scores in math and ELA; and leave-self-out share missing an 8th grade score in either subject, share matched to mom, and share matched to dad.[17]

By linking children to parents and parental voting records, I am also able to control for parent characteristics. These include an indicator for matching

[14]The race/ethnicity categories I use are white, Black, Hispanic, and Asian/Pacific Islander/Native Hawaiian (abbreviated as "Asian")

[15]Calculated as age on September 1 of the first 9th-grade year. I replace values that are less than 10 or greater than 22 with the mean.

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

[17]Cohort controls are defined at the school/first-time 9th grade year level. Since I residualize on school fixed effects when estimating school effects (see below), controlling for these school-cohort characteristics adjusts for variations in these traits over time, not fixed differences in student characteristics across schools.

to a father on the birth record and maternal/parental indices of voting based on elections before the child's first age-eligible election.[18] 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, 2006). The county-of-birth controls include controls for rurality, poverty rates, educational attainment, and population. I measure rurality using rural-urban continuum codes produced by the United States Department of Agriculture for 1993, 2003, and 2013. I assign students born between 1988 and 1997 the rurality measure from 1993, and students born between 1998 and 2007 the rurality measure from 2003. (Note that because of sample restrictions not all years mentioned here appear in my final sample). County-level poverty rates come from the U.S. Census Bureau (U.S. Census Bureau, 2021b). I use 1980 estimates for birth years 1980-1984, 1990 rates for 1985-1994, and 2000 rates for 1995-2004. I measure educational attainment using data from the USDA on the percent of adults in the county with a B.A. or higher (U.S. Department of Agriculture, Economic Research Service, 2025). I use 1980 rates for birth years 1980-1984, 1990 rates for 1985-1994, and 2000 rates for 1995-2004. I measure population using the natural logarithm of the total population in the county of birth in the year of their birth using data from intercensal estimates (U.S. Census Bureau, 2016). I also control for three measures of county-level politics and political participation using data from David Leip's Atlas of U.S. Presidential Elections (Leip, 2026): (1) the average margin in favor of the Republican candidate in the last three presidential elections prior to or including the year of the student's birth; (2) a Herfindahl-Hirchschmann index (HHI) of political competitiveness based on the vote share of candidates in those three elections[19]; and (3) the average

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

[19]The HHI index of political competitiveness for a specific election is defined as the sum of the squared share of votes for each candidate included in dataset. I take one minus the HHI value such that a larger value indicates a more competitive election. I take the average of these three HHI measures as my county-level control.

turnout as a percent of the population aged 20 older in the county over these three elections.[20] I also include an indicator for individuals who are missing county-of-birth (about 1% of observations that match to birth records); I set to 0 the values of all county-level controls for individuals who are missing county of birth. See Appendix Table A8 for details on the distributions of control variables.

I estimate school effects on civic outcomes in two steps. In my first step, I estimate Equation 2 as written and calculate student-level residuals. I include school fixed effects in Equation 2, 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

[20] I construct these turnout measures as the total number of votes divided by the population aged 20+ based on intercensal estimates from the Census Bureau (U.S. Census Bureau, 2021a, 2016). I use the population aged 20 or older instead of 18 or older because of the age groups reported in these intercensal estimates.

errors.[21] I refer to these estimates interchangeably as "civic school effects" or "school effects on voting."

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

I standardize these civic 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. 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. Table 3 shows the correlations of school effect estimates for cohort t and earlier cohorts. The correlations for civic school effects range from 0.60 for the prior cohort $(t-1)$ to 0.22 for $t-8$.

In Appendix Table A9, I examine correlations across alternative specifications of the civic effects models. My preferred estimates are highly $(\rho > 0.95)$ correlated with estimates from several alternative approaches, including estimates that limit "drift" to three periods and estimates that add controls for fall vs. spring test scores. My preferred estimates are less correlated with models that do not residualize on school effects, though the main results in Table 5 are similar using school effect estimates that do not residualize on school fixed effects.

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

[21] 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.

actual outcomes on average. 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 for all effect estimates except test score effects.

[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 the outcomes of my school effect estimates 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, including squares and cubics of these scores; an indicator for missing a score in either subject; an indicator for being the child of a parent who was born outside the United States;[22] and the share white in the student's county-of-birth in the year of the student's birth.[23] I replace missing test scores in one subject with the score in the non-missing subject. I residualize each of 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 predictors. Finally, I regress the predicted outcome for a school effect 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 no evidence of bias for my civic effect estimates; point estimates are insignificant and close to zero. For the sake of comparison, Chetty et al. (2014) estimate forecast bias of 2.2% for teacher test score value-added measures and Naven (2019) estimates bias of 0.9% (middle) to 3.9% (high school) for school-level test score value-added measures. My estimates of bias for non-cognitive and college entrance exam participation are in line with these prior estimates. My estimates of forecast bias are much larger for test score effects (12.3%), which raises concerns about interpreting results using this measure.

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

[23] Data used to construct county share white comes from the intercensal estimates used for total population (U.S. Census Bureau, 2016).

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 2.1 percentage point increase in the probability of voting by in the first four age-eligible election cycles, a 5.3% increase over the sample mean, and increases the number of times voted by about 9.4%. A standard deviation increment in school effects translates to a 1.7 percentage point (7.4%) increase in voting in the first age-eligible general election (column 3). The magnitude of this point estimate is equivalent to about 22% of the magnitude of the predicted increase in the probability of voting associated with having a mother who is a registered voter or about 36% of the predicted increase in voting for a one standard deviation increase in ELA scores, all else equal, per estimates in Table 2. Relative effects are even greater for voting outside of general/primary elections, where point estimates are small but translate to a 20% increase over the sample mean. The relationship to voter registration is also significant if somewhat smaller– 1.4 percentage points, or 2% over the sample mean – though voter registration rates are already high in my sample (around 71%).

[Table 5 about here: actual effects]

b Assessing Validity Using Siblings

Even with the evidence presented thus far, we might remain unconvinced that my civic school effects can be interpreted as the causal impact of attending a school on adult voting behavior. Positive relationships between civic school effects and actual voting outcomes presented in Panel A of Table 5, for instance, might reflect associations between unobserved student characteristics and the types of schools that students attend. To strengthen my argument for a causal interpretation of these civic effects, I assess the validity of my school civic effects by estimating the relationship between school civic effects and student outcomes *within families*, following (Jackson et al., 2020).

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 over 200,000 individuals with siblings in my sample paired to over 92,000 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 24,950 children with 11,130 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.[24] Individuals in my sibling sample differ from the main sample across several dimensions, as shown in Table 1. Notably, they are less likely to be white, more likely to qualify for free or reduced-price lunch, and have lower test scores, on average. Spillover effects between siblings on civic outcomes would also bias against detecting effects in these within-family models (Bloem et al., 2025). For these two reasons, we may not expect the relationship between school civic effects and actual voting outcomes to be exactly the same for students in this subsample and students in the full sample. Nonetheless, finding significant relationships between school civic school effects and actual voting outcomes from within family estimates would provide evidence in support of a causal interpretation of these civic effects.

To conduct my "within-family" analysis, I replicate the exercise in Panel A – relating actual voting outcomes to school civic effect estimates – using data from my sibling sample and adding family fixed effects and indicators for child birth order to the model. By including a family fixed effect in this model, I am able to adjust for the influence of all shared, time-invariant family factors – such as socioeconomic status, parenting styles, parental political behavior, and genetics – that may influence student outcomes and/or the types of schools these students attend. The results of this exercise are presented in Panel B of Table 5. While point estimates are slightly smaller, five of the six point estimates in Panel B remain positive and statistically significant at the $p < 0.05$ level.

c Assessing Validity Using School Movers

I also assess the validity of my school civic effects using high school movers in an exercise inspired by (Chetty & Hendren, 2018). The intuition behind this

[24] Since school civic effects are defined at the cohort level, there would also still be (some) variation in school civic effects for siblings who attend the same school in different years. However, given the correlations between school civic effect estimates by year (see Table 3), the amount of variation for siblings who attended the same school would, presumably, be much smaller.

test is to assess whether school civic effects impact students who were not originally enrolled in a given school. If civic effects were primarily a product of sorting of families across neighborhoods – most of which, we expect, would have occurred when children were much younger – then the significant relationship between school civic effects and actual outcomes may not exist for "movers," whose observable and unobservable characteristics may differ from students who initially enroll. For my mover sample, I identify students who moved from a school in my sample ("sending school") to another school in my sample ("destination school") one to three years after their first 9th grade enrollment. I limit my sample to students who appeared at the destination school for at least two years to ensure adequate exposure to the new environment. I identify 21,960 movers (see Table 1). Similar to students in the sibling sample, my high school movers have lower average test scores and are more likely to be Black and qualify for free or reduced-price lunch than students in my primary sample. I then repeat the analysis in Table 5, swapping in the civic effect of the student's destination school (measured in the student's first 9th grade year) as the coefficient of interest. I also add to this model indicators for years spent at the sending school prior to entering the receiving school to account for variation in potential causes of mobility at different points in the student's academic career.

One concern about this approach is that the civic effects of the sending and destination school may be correlated with each other if students tend to move between similar types of schools. (Civic effects for sending/destination schools are correlated 0.21 in my sample). A second concern is that student voting behavior may be influenced – perhaps disproportionately – by time spent at the sending school. This would be particularly concerning if early high school experiences were especially important for political socialization. To address both concerns, I also control for the civic effect of the sending school in these models.

The results of this analysis, presented in Panel C of Table 5, provide additional support that my civic effect estimates reflect causal impacts of schools on student outcomes rather than student sorting. All point estimates on the coefficient for the destination school effect are positive and four of the six estimates are significant at the $p < 0.05$ level. The positive coefficients on the sending school effect are consistent with early (i.e., pre-move) experiences also impacting civic engagement. These findings complement my within-family analysis by showing, together, that civic effects appear to operate via exposure to a specific school.

d 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 from my preferred specification in Table 5. Column 2 shows results estimated without controls. For two of the three outcomes in Table 6, adding controls increases the magnitude of the coefficient on school effects (at least qualitatively), as shown by comparing columns 1 and 2. This could suggest downward bias of uncontrolled estimates, as would be the case if there were a negative relationship between a student's own level of predicted civic engagement (based on his/her own characteristics) and their school's effect on voting. Column 3 adds controls for the characteristics of the school the student attended in 8th grade to further disambiguate "place" and "school" effects.[25] Results are effectively unchanged. In column 4, 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 5, I drop individuals who match to voting records in more than 2 states. Again, results are effectively unchanged. In column 6, I restrict my sample to students who switched schools between 8th grade and 9th grade. In column 7, 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.[26] Results are similar.

[Table 6 about here: robustness]

[25]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.

[26]The variables I use to impute maternal and paternal civic engagement are: race/ethnicity (Black, white, Hispanic, Asian/Pacific Islander/Native Hawaiian); 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 an indicator for missing unexcused absences.

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 regression analyses that predicts whether an individual votes in the first four election cycles using these other school effects (columns 1-4). Schools that raise test scores and schools that increase participation in college entrance exams also increase the probability of adult voting. A one standard deviation increase in effects on English 10 test scores is associated with a 0.6 percentage point increase in the probability of voting, about 29% the size of the point estimate for civic school effects in column 1.[27] Similarly, a one standard deviation increase in the school effect on college exam participation increases the probability of voting by 0.9 percentage points, about 43% of the relationship with a one standard deviation increase in school civic effects. The relationship between voting and school effects on non-cognitive outcomes is not significant (column 3), though it is significant when controlling for all effects simultaneously (column 5). Controlling for school effects on other outcomes does not diminish the relationship between civic effects and voting, as shown in column 5. Table 7 reports results using an unbalanced panel of data. Results are similar using a balanced panel.

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

I also examine correlations across different types of school effects. This allows me to consider whether the same schools that are good at promoting civic engagement are good at improving other kinds of outcomes, though I note concerns about forecast bias for the non-civic school effect estimates, particularly for test scores. I start by calculating an average school-level effect for each outcome type, which I do by averaging the cohort-specific estimates of school effects over the students in my sample. This allows me to summarize a school's effect on an outcome with a single estimate. (Two schools are missing data on non-cognitive effects). I then ask whether these

[27]Since test score effects cannot be calculated for all cohorts due to data availability, these estimates come from a sub-sample of the overall sample. This is also true for effects on the non-cognitive index measures.

average school effects on voting are correlated with school effects on other outcomes. The results of this analysis are presented in Appendix Figures A4, A5, and A6, which plot average civic effects against other school effects on other outcomes. School civic effects are not significantly related to school effects on test scores.[28] There is a positive correlation between school effects on voting and school effects on college exam participation ($\rho = 0.227$). Effects on college exams, test scores, and non-cognitive effects are all moderately correlated with each other (see Appendix Figure A5).

The association between average civic effects and effects on non-cognitive outcomes is negative ($\rho = -0.159$). To explore whether this finding is sensitive to missing GPA data, I consider correlations between school civic effects and two alternative specifications of the non-cognitive index measure, one that drops GPA and one that uses only actual (not imputed) GPA. These results are presented in Appendix Figure A6 (Panels A and B, respectively). The direction and magnitude of the correlations remain about the same. I also consider the relationships between civic effects and two separate sub-indices of non-cognitive outcomes: (1) effects on disciplinary/behavioral outcomes (suspensions, expulsion, and absences) and (2) effects on academic progression outcomes (progression to grade 10, GPA, course pass rate) (Panels C and D of Appendix Figure A6). This distinction could be relevant given that differences in school effects on disciplinary/ behavioral outcomes may reflect differences in school disciplinary policies instead of changes in student behavior (Bacher-Hicks et al., 2024), and Bruch & Soss (2018) finds associations between punitive school environments and reduced civic engagement for middle/high school students. Neither sub-index effect is significantly related to civic effects. To further explore this issue, I also consider whether effects on these alternative non-cognitive indices predict actual voting outcomes, replicating the analyses presented in Table 7. These results can be found in Appendix Table A7. I find some evidence that effects on disciplinary/behavioral outcomes are negatively related to voting while effects on grade progression outcomes are positively related. While this finding appears to run contrary to the expectation that high-discipline environments reduce civic engagement, it is difficult to interpret given the mixed signal of student behavioral outcomes.

Appendix Table A10 summarizes school effects on other outcomes by

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

quartile of civic effects, replicating the analyses in Table 8 (described in the following section).

b School Characteristics

I now describe the characteristics of schools with higher (lower) average civic effects. This correlational analysis is limited both by the number of schools in my sample (n=335) and the fact that the place- and school-level characteristics I consider may be correlated with each other and other unobserved or imperfectly measured factors, such as family socioeconomic status. Table 8 summarizes the characteristics of the school communities (Panel A) and schools (Panel B) by quartile of school effects on voting (columns 1-4), with the top quartile including the schools that impact voting the most. Column 5 presents the coefficient on the community/school characteristic for that row from a simple regression predicting the school civic effect. Column 6 presents the comparable coefficient from a regression that includes controls for urbanicity[29] (indicators for city, suburb, and town; rural omitted); average 8th grade ELA and math scores for students in the sample; average maternal civic engagement scores; the natural log of average school enrollment in grades 9-13; average school-level shares white, Black, Hispanic, Asian/Pacific Islander/Native Hawaiian, free or reduced-price lunch, special education, and English learner students; and county-level political measures (turnout, competitiveness, and Republican partisanship), described below.

[Table 8 about here: quartiles

I start by considering differences across community type (Panel A). Simple comparisons (column 5) show that schools in suburban areas and towns have smaller civic effects, on average, though these relationships are sensitive to the inclusion of controls (column 6). I then consider differences across local political contexts.[30] School effects on voting are not associated with average voter turnout or political competitiveness in simple or controlled models.

[29]For this and the analyses that follow, I define these categories using the school locale indicators from the 2015-16 National Center for Education Statistics EDGE files (National Center for Education Statistics (NCES), 2024).

[30]I measure turnout using the average county-level turnout among eligible voters in the 2008, 2012, and 2016 presidential elections as reported in data from the National Neighborhood Data Archive (NaNDA) (Clary et al., 2024). I measure political competitiveness using a Herfindahl-Hirchschmann index (HHI), which is calculated by taking the sum of

The lack of an association between civic effect estimates and local political contexts here does not necessarily contradict earlier research that finds local political measures predict voting behavior, including for young people (Gimpel et al., 2003; Pacheco, 2008). (Correlations between *levels* of civic engagement (index) for students at a high school and local political factors show the theoretically expected relationship – that is, positive associations with political competitiveness and turnout). In the uncontrolled model there is a negative association with county-level Republican partisanship.

I then consider relationships to school-level attributes (Panel B). Two primary findings are worth highlighting. First, school effects on civic engagement are positively related to levels of voting for students at a school; that is, there is a positive relationship between being a school that promotes voting and being a school with high levels of voter turnout. This is relevant given the related literature that suggests that school effects on test scores may be only weakly correlated with average test performance (MacLeod & Urquiola, 2015; Meyer, 1997). Second, there are strong associations between average maternal civic engagement – which I interpret as a proxy for peer civic norms – and school effects on voting. Controlled estimates imply that a one standard deviation increase in maternal civic engagement is associated with about a 1.57 standard deviation increment in school civic effects. This evidence suggests peer effects may be a channel for school effects on voting.

Several additional points also bear mentioning. Civic effects are not associated with student race/ethnicity or the share of students who qualify for free or reduced-price lunch. This means that it is not necessarily the case that students from traditionally disadvantaged (and sometimes low turnout) demographic groups are concentrated in schools that are less effective at promoting civic engagement. This is encouraging given theories that schools can play a compensatory role in political socialization (Neundorf et al., 2016; Campbell, 2019). There is a marginally positive ($p < 0.10$) relationship between serving as a polling site and school civic effects. There is a notable negative correlation between school size and civic effects. I find a negative

squared vote shares for Republican, Democrat, Green Party, and Libertarian candidates in the 2008, 2012, and 2016 presidential elections. I subtract this value from 1 so that higher values indicate more competitive environments and then average over the three elections and standardize over counties (MIT Election Data and Science Lab, 2018). I measure Republican partisanship using the average from 2008 to 2016 (even years) of a Republican partisanship measure produced by NaNDA, which is based on voting in current and three prior elections.

relationship between average 8th grade math scores (while also controlling for 8th grade ELA scores, which are not significantly related to civic school effects). There is a marginally significant negative relationship between English learners and school civic effects. Lastly, I note that charter schools, on average, have larger positive effects on civic engagement than non-charters, though this only true of the uncontrolled relationship and my sample of charters is small (n=10).

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, 2025a,b). Of the 335 schools in my sample, 310 recorded at least 10 AP exams on average each year in my study period. 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: U.S. History, English Language, Calculus AB, English Literature, Biology, Chemistry, Psychology, 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 define average school participation rates by taking the number of exams recorded at a school in a year divided by average enrollment in grades 9-13, which I then average over individuals in my sample (using the school measure from their grade 9 year). I define school average scores in a subject as the average score among test-takers in this subject by school and year, averaged over individuals in my sample as before. Appendix Table A11 provides details on the distribution of AP exam measures.

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 2-4 model the relationship between participation in AP exams and civic school effects in a school-level dataset, controlling for the same school and county-level controls included in Table 8. Participation on the U.S. History is not significantly related to school civic effects. The coefficients on participation in the U.S. Government and Politics exams are positive and significant or marginally significant in columns 2 and 3 but become insignificant when controlling for participation in all eight other subjects. Columns 6-8 examine the relationship between average scores on AP exams among test-takers and school civic effects. Since not all schools have students testing in all subjects, the number of observations declines as additional subjects are added as controls. Scores on the AP US Government and Politics exam are significantly and positively related to civic effects. No other subject is significantly related to civic effects at the $p < 0.05$ level. While these results could reflect a link between schools that excel at civics-related teaching and school impacts on voting, an alternative explanation is that schools that increase voting for other reasons (e.g., peer effects) also serve students who are motivated and interested in civics-related content. I also note this finding comes from a small sample of schools and is sensitive (e.g., to the exclusion of school years with small numbers of test-takers in a subject).

[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.[31] Appendix Table A12 provides detail on each of the civic and non-civic extracurriculars in my data.

[Table 10 about here: extracurriculars

I do not find robust evidence of relationships between the presence or number civics-related extracurriculars school effects on voting. Table 10

[31] 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.

presents the results of a series of regressions that relate school civic effects to school-level measures of extracurricular presence by activity type. Relationships between having any civics-related extracurricular activities and civic effects are insignificant (columns 1-3). There is a positive relationship between the number of civics-related extracurriculars at a school and civic school effects, but this finding becomes statistically insignificant with controls. Appendix Figure A7 plots point estimates relating specific activities to school civic effects.

VIII Discussion

This paper presents first-order evidence that where you go to high school affects whether you vote as an adult. School impacts are significant and sizable, if not large enough to single-handedly eliminate gaps in voter turnout: attending a school with a one standard deviation higher effect on civic outcomes increases the probability a student votes in their first general election by 1.7 percentage points, or about 6% percent of the gap in voter turnout for younger (age 18-24) and older (65+) voters in the 2024 election (U.S. Census Bureau, 2025). Impacts are even larger for voting outside of high-turnout primary and general elections. I show evidence that my civic effect estimates do not generate significant estimates of forecast bias, are robust to alternative specifications, and behave in ways consistent with a causal interpretation, including through analyses using siblings and school movers.

I also explore how school effects on voting relate to effects on other outcomes and school and community characteristics, providing insight into potential mechanisms. School effects on college exam participation are positively associated with school effects on voting, pointing to post-secondary enrollment as a possible pathway, and non-cognitive effects are negatively related to school effects on voting, though I note significant bias in these other school effect estimates. Taken at face value, the negative association with effects on non-cognitive outcomes is somewhat surprising given other work that identifies non-cognitive skills as a mediator of educational effects on civic outcomes (Cohodes & Feigenbaum, 2021; Holbein, 2017; Holbein & Hillygus, 2020). One possible explanation is that the non-cognitive outcome measures used to construct my index may reflect other characteristics of schools, such as school disciplinary policies or expectations for student behavior, that also impact voting. 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. Another possibility is that the kinds of non-cognitive skills that facilitate voting are largely developed by the time students enroll in high school, which would explain why my results diverge from work focused on younger students.

Other school-level correlates of civic effects point to other mechanisms. Civic effects are strongly associated with maternal civic engagement, suggesting a role of peer effects within schools. Smaller schools have larger impacts on voting, an intriguing finding that – if confirmed in other settings – warrants further exploration. Most relevant for policymakers are my findings on civics-related coursework and activities. While I do not find associations between school-level indicators of civics extracurriculars and effects on voting, I do find some evidence of positive associations between average scores on the AP U.S. Government and Politics exam and civic effects, though this finding comes from a sub-sample of schools and is sensitive to specification.

This study points to several directions for future research. One priority is better understanding the channels of school effects on voting with an eye to identifying factors that are malleable for policymakers. For instance, my finding that parental civic norms relate to school effects suggests student sorting may play a role in determining which schools positively impact voting. While student sorting is difficult to manipulate, future work could consider whether civic behaviors modeled by teachers and other school staff also impact voting. The positive association between school effects on voting and effects on college-going behavior highlights another important avenue for future work on how and under what circumstances post-secondary experiences impact political participation (Firoozi & Geyn, 2025; Bell et al., 2024). More immediately, researchers should seek to expand the experimental and quasi-experimental research on how and in which settings teachers and schools contribute to the development of civics-related knowledge and skills and how these types of knowledge and skills relate to adult civic outcomes. A significant challenge to this type of research has been the lack of comprehensive data on student skills in these areas. Making progress in this area may require generating new data on these competencies or leveraging state standardized assessments in social studies, a largely overlooked subject in existing research using student test scores.

Some limitations of this work also bear discussion. One relevant question is whether my findings from 335 schools in a single Midwestern state would generalize to other settings. Place-based heterogeneity may be particularly relevant for outcomes like political participation, which can be highly contextual. Another key limitation is my use of voting as the only available measure of adult civic engagement. Future work should broaden this lens to assess outcomes such as political knowledge, reasoning, attitudes, and engagement.

In our complex political era, politicians and policymakers have looked to our schools to improve civic discourse, mitigate the threats of misinformation, and reduce the ills of affective polarization and political fatalism. Whether or not schools are up to this work is far beyond the scope of this paper. What this paper shows, however, is that public schools are relevant for democratic participation.

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Figure 1: High Schools in Sample

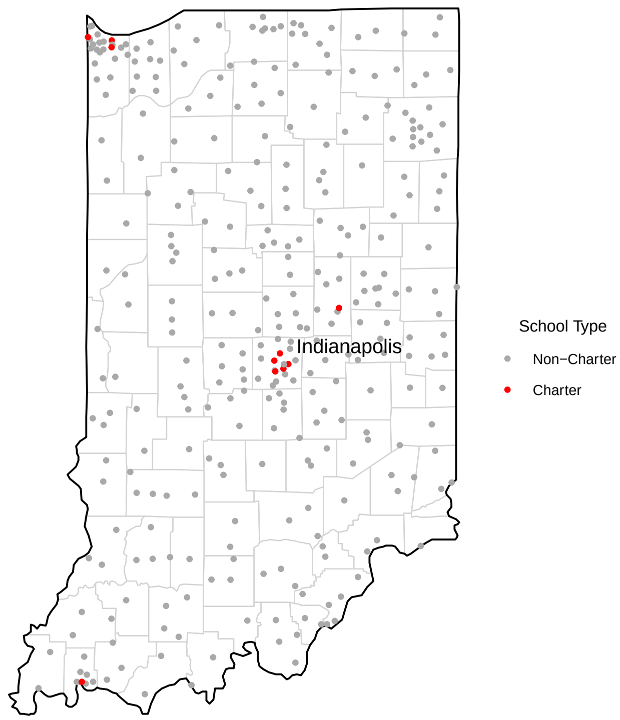
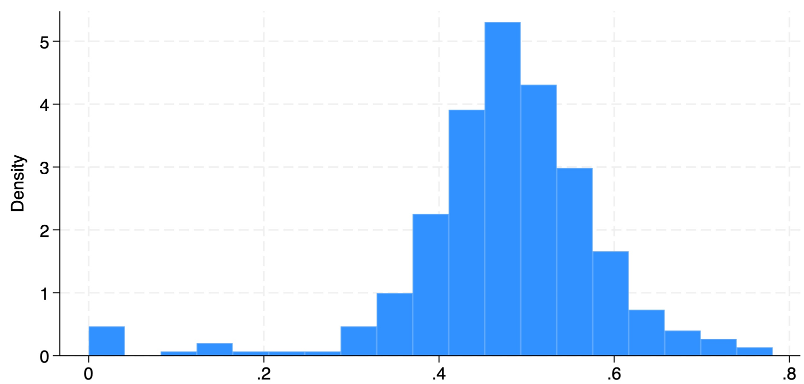


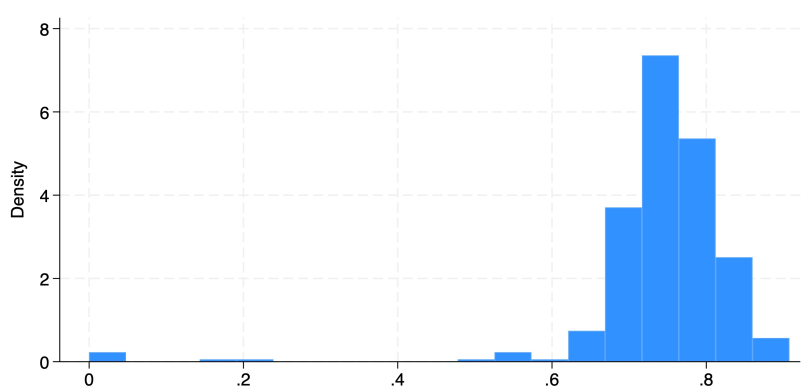
Figure 2: Raw Differences in Voting Outcomes by School

(a) Share: Ever Voted



N=367
mean: 0.475, SD: 0.113
10th pctile: 0.370, 90th pctile: 0.595

(b) Share: Registered to Vote



N=367
mean: 0.742, SD: 0.106
10th pctile: 0.677, 90th pctile: 0.829

Distribution of schools. Shares are calculated including individuals who did not match to birth records/prior test scores. Registered to vote means ever observed registered to vote. Ever voted means ever observed voting.

Table 1: Sample Summary Statistics

	All	Sample	Siblings	Movers
	(1)	(2)	(3)	(4)
N Students	689,188	456,699	24,950	21,960
N Schools	367	335	335	335
N Charter Schools	21	10	10	10
N Metro/Suburb	164	143	143	143
N Rural/Town	203	192	192	192
A. Student Characteristics				
Share Non-Missing 8th Gr Tests	0.93	1.00	1.00	1.00
Share White	0.77	0.84	0.76	0.76
Share Black	0.10	0.07	0.14	0.14
Share Hispanic	0.08	0.05	0.05	0.05
Share Free/Reduced-Price Lunch	0.41	0.37	0.58	0.56
Share Special Education	0.14	0.12	0.16	0.15
Share English Learner	0.03	0.01	0.01	0.01
B. Lagged Outcomes				
Gr 8 Math Scores	0.03	0.07	-0.25	-0.26
	(0.98)	(0.96)	(0.97)	(0.91)
	[640,347]	[455,252]	[24,811]	[21,845]
Gr 8 ELA Scores	0.02	0.05	-0.26	-0.27
	(0.98)	(0.97)	(0.96)	(0.90)
	[637,701]	[453,910]	[24,707]	[21,773]
Lagged Absences	6.81	6.71	8.11	8.10
	(7.33)	(7.02)	(8.15)	(7.39)
	[669,403]	[455,763]	[24,888]	[21,916]
C. Parent Characteristics				
Share Matched to Mom	0.70	1.00	1.00	1.00
Share Mom Registered to Vote	0.43	0.61	0.52	0.47
Share Matched to Dad	0.65	0.92	0.86	0.87
Share Dad Registered to Vote	0.50	0.71	0.61	0.61
D. Student Voting				
Share Registered to Vote	0.76	0.80	0.74	0.75
Ever Voted	0.50	0.53	0.39	0.39

Sample in column (1) is limited to first-time 9th graders who enrolled in a public high school in Indiana between SY 2007-08 and SY 2015-16 in the initial sample. Column (2) restricts sample to individuals with at least one non-missing grade 8 test score who match to a maternal birth record, among other restrictions (e.g., dropping 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, 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. Column (4) shows summary statistics for high school movers, as described in text. Test scores are standardized by year and grade to have a mean of 0 and standard deviation of 1 among test-takers. The number of observations varies in Panel B because of missing data; unless otherwise noted, the number of observations is the same as the number of students in the first row of that column. Registered to vote is ever matched to voting records.

Table 2: Student/Parent Characteristics and Voting in First Eligible General Election

	(1)	(2)	(3)
Male	-0.021***	-0.022***	-0.008***
	(0.00)	(0.00)	(0.00)
White	0.018***	0.011*	0.008*
	(0.00)	(0.00)	(0.00)
Black	0.049***	0.044***	0.079***
	(0.01)	(0.01)	(0.01)
Hispanic	0.022***	0.027***	0.037***
	(0.01)	(0.01)	(0.01)
Free/Reduced-Price Lunch	-0.142***	-0.117***	-0.074***
	(0.00)	(0.00)	(0.00)
Mom Registered to Vote		0.095***	0.078***
		(0.00)	(0.00)
Matched to Dad		0.054***	0.035***
		(0.00)	(0.00)
Gr 8 Math Score			0.022***
			(0.00)
Gr 8 ELA Score			0.047***
			(0.00)
Log Lagged Unexcused Absences			-0.026***
			(0.00)
Cohort Dummies	X	X	X
First Election Dummies	X	X	X
N	456,699	456,699	445,223
r2	0.11	0.12	0.15

Outcome is an 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. Log lagged unexcused absences are constructed as the natural log of unexcused absences +1 and set to missing if fewer than 100 days of enrollment were reported. The number of observations drops in column 3 because these estimates drop observations that are missing grade 8 math or ELA scores or lagged unexcused absences. 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-Cognitive Effects	College Exam Effects
1	0.60	0.72	0.66	0.59
2	0.57	0.71	0.64	0.55
3	0.48	0.67	0.62	0.51
4	0.44	0.61	0.57	0.41
5	0.36	0.61	.	0.36
6	0.36	.	.	0.41
7	0.29	.	.	0.39
8	0.22	.	.	0.34

Correlations across proximate years for school effect estimates. Produced 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); an indicator for matching to father; grade 8 math and ELA scores, including squares and cubics (missing scores in one subject are replaced with non-missing scores in the other); an indicator for missing a test score in either subject; once lagged log unexcused absences plus one (set to 0 if missing or if reported enrollment is less than 100 days); 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, log county population, average county-level turnout, average margin for Republican candidate, political competitiveness measure, rurality/urbanicity measure, and an indicator for missing county-of-birth); cohort controls (number of students in cohort, leave-self out average 8th grade test scores in math and ELA; share of students missing 8th grade test score in either subject; shares matched to dad and mom; and average maternal and paternal civic engagement): and dummies for first age-eligible election and grade cohort.

Table 4: Validity of School Effects

	Civic Score	English 10 Test	Non-Cognitive Index	College Exam
	(1)	(2)	(3)	(4)
A. Actual Outcomes				
Beta on School Effect	0.991	0.945**	0.991	0.986
	(0.013)	(0.019)	(0.026)	(0.017)
p-value (Beta=1)	0.508	0.004	0.727	0.434
N	456,699	280,311	246,955	456,699
B. Predicted Outcomes				
Beta on Schoool Effect	-0.001	0.123***	0.020***	0.027***
	(0.005)	(0.010)	(0.005)	(0.007)
p-value (Beta=0)	0.829	0.000	0.000	0.000
N	392,150	271,650	237,255	392,150

Estimates in Panel A and B come from regressions of the student's residualized actual (Panel A) or predicted (Panel B) outcomes on the school effect estimate for the outcomes listed in the header. The residual outcome (Panel A) is residualized on all the controls included in Equation 2 (see Equations 2 and 3 for detail). The coefficient presented in Panel A is the coefficient on the unstandardized school effect in a regression where the outcome is the residualized actual outcome. 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 value of the outcome listed in the header, where prediction is based on grade 7 math and ELA scores and squares and cubics of these values (I replace missing scores in one subject with non-missing scores in the other subject), an indicator for missing 7th grade test scores in either subject, an indicator for being the child of an immigrant parent (based on birth records), and the share white in the child's county of birth. Predicted values are generated in two steps. First, the outcome and each predictor are residualized, as before. Second, the residualized outcome is predicted using the residualized versions of the predictor variables. The coefficient in Panel B is the coefficient on the (unstandardized) school effect in a regression where the outcome is this predicted outcome. Inference in Panel B is conducted under the hypothesis that the coefficient is equal to 0. Standard errors are clustered by school throughout. + $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$. The number of observations is smaller in Panel B than Panel A because not all students have non-missing data for variables used to estimate predicted outcomes in Panel B. The number of observations differs across columns because the outcomes used for school effect estimates are not available for all cohorts/individuals.

Table 5: Civic School Effects and Voting Behavior

	Registered to Vote	Ever Voted	Times Voted	Voted in 1st General Election	Voted in 2nd General Election	Ever Voted: Non-General, Non-Primary
	(1)	(2)	(3)	(4)	(5)	(6)
A. Full Sample						
Beta on School Effect	0.014***	0.021***	0.062***	0.017***	0.014***	0.006***
	(0.002)	(0.002)	(0.004)	(0.002)	(0.001)	(0.001)
Student Controls	X	X	X	X	X	X
Parental Controls	X	X	X	X	X	X
N	456,699	456,699	456,699	456,699	456,699	456,699
Mean	0.71	0.40	0.66	0.23	0.25	0.03
SD			1.00			
B. Sibling Sample						
Beta on School Effect	0.011*	0.012**	0.022**	0.008*	0.008*	-0.002
	(0.004)	(0.004)	(0.007)	(0.004)	(0.003)	(0.001)
Student Controls	X	X	X	X	X	X
Family Fixed Effects	X	X	X	X	X	X
N	24,950	24,950	24,950	24,950	24,950	24,950
Mean	0.65	0.27	0.42	0.15	0.16	0.02
SD			0.81			
C. Movers Sample						
Beta on Destination School Effect	0.014***	0.011**	0.026***	0.008**	0.005	0.002
	(0.003)	(0.004)	(0.008)	(0.003)	(0.003)	(0.001)
Beta on Sending School Effects	0.006+	0.010**	0.018**	0.006*	0.006*	0.002
	(0.003)	(0.003)	(0.006)	(0.002)	(0.003)	(0.001)
Student, Parental Controls	X	X	X	X	X	X
Switch Year Fixed Effect	X	X	X	X	X	X
N	21,959	21,959	21,959	21,959	21,959	21,959
Mean	0.66	0.27	0.40	0.14	0.15	0.02
SD			0.47			

Panel A presents results from a regression of the student-level voting outcome listed in the column on the (standardized) school civic effect in the full sample. "Student controls'' include the following: race/ethnicity (Black, white, Hispanic, and Asian); special education, English learner, and free or reduced-price lunch status; male gender; age in grade 9; lagged test scores in ELA and math (including squares and cubics, with missing scores in one subject replaced with non-missing scores from the other); an indicator for individuals missing one score; log unexcused absences from prior year plus one (set to 0 if missing); an indicator for missing lagged absences; log suspensions from prior year plus one; and fixed effects for first age-eligible election year and 9th grade cohort. "Parental controls'' include an indicator for matching to dad, paternal civic score (set to 0 if missing), and maternal civic score. Panel B presents results from a similar regression in the sibling sample (see text for description). Panel B omits ``parental controls" and adds a family fixed effect and a fixed effect for sibling birth order (defined within sibling sample). Panel C presents estimates in the sample of school switchers, as described. Panel C controls include all controls in Panel A plus a fixed effect for years spent at the sending school before the switch. Standard errors are clustered by school (in Panel C, destination school). The number of observations differs depending on the estimation sample used. + p<0.10, *p<0.05, ** p<0.01, ***p<0.001.

Table 6: Robustness: Civic School Effects and Voting Behavior

	Preferred	No Controls	Add Gr 8 School Characteristics	Drop Common Names	Drop Multi-State Vote Matches	Drop Non-Switchers	Include Students without Birth Records
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Ever Voted							
Beta on School Effect	0.0209***	0.0141*	0.0236***	0.0208***	0.0210***	0.0203***	0.0175***
	(0.0018)	(0.0063)	(0.0014)	(0.0018)	(0.0018)	(0.0020)	(0.0023)
Obs	456,699	456,699	456,699	429,418	455,082	402,177	636,871
B. Voted in First General Election							
Beta on School Effect	0.0170***	0.0130**	0.0192***	0.0168***	0.0170***	0.0162***	0.0142***
	(0.0015)	(0.0039)	(0.0014)	(0.0016)	(0.0015)	(0.0017)	(0.0017)
Obs	456,699	456,699	456,699	429,418	455,082	402,177	636,871
C. Ever Voted Non-General, Non-Primary							
Beta on School Effect	0.006***	0.006**	0.007***	0.006***	0.006***	0.007***	0.005**
	(0.0015)	(0.0019)	(0.0012)	(0.0015)	(0.0015)	(0.0016)	(0.0014)
Obs	456,699	456,699	456,699	429,418	455,082	402,177	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 replictates the preferred estimates (see also Table 5). Column 2 presents results from uncontrolled model. Column 3 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 based on IN DOE data which may include some non-public school enrollment) and an indicator for missing grade 8 school, in which case all other grade 8 school controls are set to 0. Column 4 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 5 drops students who match to voting records in more than 2 states. Column 6 drops students who did not switch schools between 8th grade and 9th grade. Column 7 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. Number of observations differs because of differences in estimation samples, as described.

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

	(1)	(2)	(3)	(4)	(5)
Civic Effect	0.021***				0.025***
	(0.002)				(0.002)
English 10 Test Effect		0.006**			0.009***
		(0.002)			(0.002)
Non-Cognitive Effect			0.002		0.004*
			(0.002)		(0.002)
College Exam Effect				0.009***	0.002
				(0.002)	(0.002)
Student, Cohort, First Election Controls	X	X	X	X	X
Observations	456,699	297,672	253,171	456,699	146,014

Outcome is an indicator for ever voting in the first four election cycles. The number of observations differs across columns because it is limited to observations with non-missing values of each of the school effect estimates in the model and school effect outcome data is only available for certain cohorts and may be missing for some students. Controls are the same as in 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		
A. Community Characteristics						
City	0.250	0.143	0.143	0.277	0.125	-0.177
					(0.157)	(0.193)
Suburb	0.369	0.167	0.226	0.133	-0.355**	-0.018
					(0.116)	(0.129)
Town/Rural	0.155	0.107	0.083	0.048	-0.338*	-0.124
					(0.136)	(0.144)
Voter Turnout (County)	0.578	0.576	0.571	0.578	-0.353	-0.321
	(0.063)	(0.055)	(0.045)	(0.047)	(0.983)	(0.982)
Leans Republican (County)	0.576	0.602	0.570	0.540	-1.399*	-1.266
	(0.110)	(0.081)	(0.088)	(0.118)	(0.599)	(0.922)
Political Competitiveness (County)	0.223	0.123	0.405	0.136	-0.009	-0.054
	(0.945)	(1.017)	(0.959)	(1.044)	(0.057)	(0.072)
B. School Characteristics						
Share Voted (1st General Election)	0.200	0.212	0.228	0.261	8.478***	19.171***
	(0.053)	(0.035)	(0.039)	(0.058)	(0.978)	(1.459)
Avg Maternal Civic Engagement	-0.030	-0.034	-0.014	0.060	1.206**	1.565**
	(0.188)	(0.123)	(0.144)	(0.209)	(0.390)	(0.450)
Avg Gr 8 ELA Scores	0.011	-0.008	0.014	-0.012	-0.054	0.826
	(0.254)	(0.185)	(0.217)	(0.286)	(0.303)	(0.530)
Avg Gr 8 Math Scores	0.048	0.021	0.015	-0.027	-0.314	-0.897*
	(0.302)	(0.220)	(0.263)	(0.336)	(0.231)	(0.364)
School Was Polling Site	0.226	0.190	0.333	0.337	0.142	0.191+
	(0.421)	(0.395)	(0.474)	(0.476)	(0.113)	(0.105)
Share Free/Reduced-Price Lunch	0.385	0.370	0.366	0.403	0.301	0.830
	(0.158)	(0.131)	(0.151)	(0.182)	(0.360)	(0.657)
Share White	0.795	0.878	0.863	0.793	-0.217	-0.293
	(0.197)	(0.158)	(0.170)	(0.283)	(0.285)	(2.652)
Share Black	0.090	0.042	0.041	0.093	0.167	-1.177
	(0.144)	(0.103)	(0.119)	(0.220)	(0.397)	(2.808)
Share Hispanic	0.061	0.046	0.060	0.074	1.055+	2.500
	(0.053)	(0.065)	(0.099)	(0.126)	(0.611)	(2.853)
Share Special Education	0.141	0.144	0.143	0.131	-2.645	1.255
	(0.037)	(0.032)	(0.034)	(0.037)	(1.623)	(1.492)
Share English Learners	0.027	0.014	0.021	0.018	-1.400	-4.811+
	(0.030)	(0.027)	(0.044)	(0.031)	(1.446)	(2.732)
Log High School Enrollment	6.985	6.523	6.377	6.046	-0.560***	-0.530***
	(0.746)	(0.715)	(0.706)	(0.713)	(0.063)	(0.082)
Charter School	0.000	0.012	0.024	0.084	1.374***	-0.334
	(0.000)	(0.109)	(0.153)	(0.280)	(0.360)	(0.522)

Columns 1-4 present the mean of the characteristics indicated in the row by quartile of (average) school civic effects, where the 4th quartile represents the schools with the largest impacts on civic outcomes. Column 5 reports the coefficient on the characteristics from a regression predicting civic school effects in a dataset of school-level observations (n=335). Column 6 does the same for the coefficient for a regression that includes controls for urbanicity (indicators for city, suburb, and town; rural omitted); average 8th grade ELA and math scores for students in the sample; average maternal civic engagement scores for students in the sample; the natural log of average school enrollment in grades 9-12; average school-level shares white, Black, Hispanic, Asian, free or reduced-price lunch, special education, and English learner students; and county-level political measures (leans Republican, political competitiveness, voter turnout). Averages are defined over students in the sample. County-level political measures 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)
	Mean				Mean			
U.S. History	0.019	2.514	3.056	2.877	2.153	0.062	0.083	-0.069
		(2.598)	(2.592)	(2.617)		(0.102)	(0.139)	(0.234)
U.S. Government and Politics	0.007	7.845*	6.636+	3.305	2.303	0.174*	0.164+	0.665*
		(3.862)	(3.683)	(3.846)		(0.076)	(0.087)	(0.259)
English	0.019		3.280	2.697	2.629		-0.179	-0.272
			(2.602)	(2.545)			(0.143)	(0.401)
Calculus AB	0.021		5.973	5.289	2.060		-0.181+	-0.322
			(4.553)	(4.522)			(0.099)	(0.224)
English Literature	0.018		-1.171	-1.810	2.502		0.402*	0.015
			(2.947)	(2.897)			(0.190)	(0.383)
Biology	0.011			5.057	2.207			0.109
				(3.738)				(0.247)
Chemistry	0.009			5.056	1.988			0.173
				(4.035)				(0.230)
Psychology	0.005			-1.894	2.618			-0.259
				(3.952)				(0.183)
World History	0.007			4.139	2.487			0.136
				(2.938)				(0.192)
Statistics	0.005			0.743	2.382			-0.053
				(4.483)				(0.153)
Ln(AP Test Count)		X	X	X		X	X	X
School/County Controls		X	X	X		X	X	X
Observations		335	335	335		167	148	60

Regression output. Outcome is the school average civic effect estimate. In columns 2-4, 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 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 6-7, 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 average scores in each of these subjects. The school and county-level controls are the same as for the controlled specification (column 6) of Table 8 (see notes), adding the log of AP test count (plus one). 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	0.152	0.185	0.209	0.216*	0.071	-0.024
	(0.146)	(0.126)	(0.128)	(0.094)	(0.073)	(0.064)
Music			0.007			-0.139***
			(0.112)			(0.041)
Science			0.071			-0.099
			(0.111)			(0.110)
Math			0.212			-0.025
			(0.142)			(0.085)
Quiz			0.118			0.061
			(0.112)			(0.073)
Best Buddies						-0.311+
						(0.169)
Speech						-0.178+
						(0.100)
N Extracurriculars (all)	-0.108***	-0.084***	-0.107***	-0.141***	-0.084***	
	(0.019)	(0.022)	(0.029)	(0.023)	(0.025)	
School/County Controls		X	X		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 1-3, 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 4-6, the extracurricular predictors are the number of activities of each type found at the school. See Appendix Table A11 and A12 for details on the activity types and activity measures. The school/county controls are the same as in Table 8 (see notes). Robust standard errors in parentheses. +p<0.10, *p<0.05, ** p<0.01, ***p<0.001

Last updated: June 8, 2026

Appendix Figures

Figure A1: Distribution of Index Outcome Measures

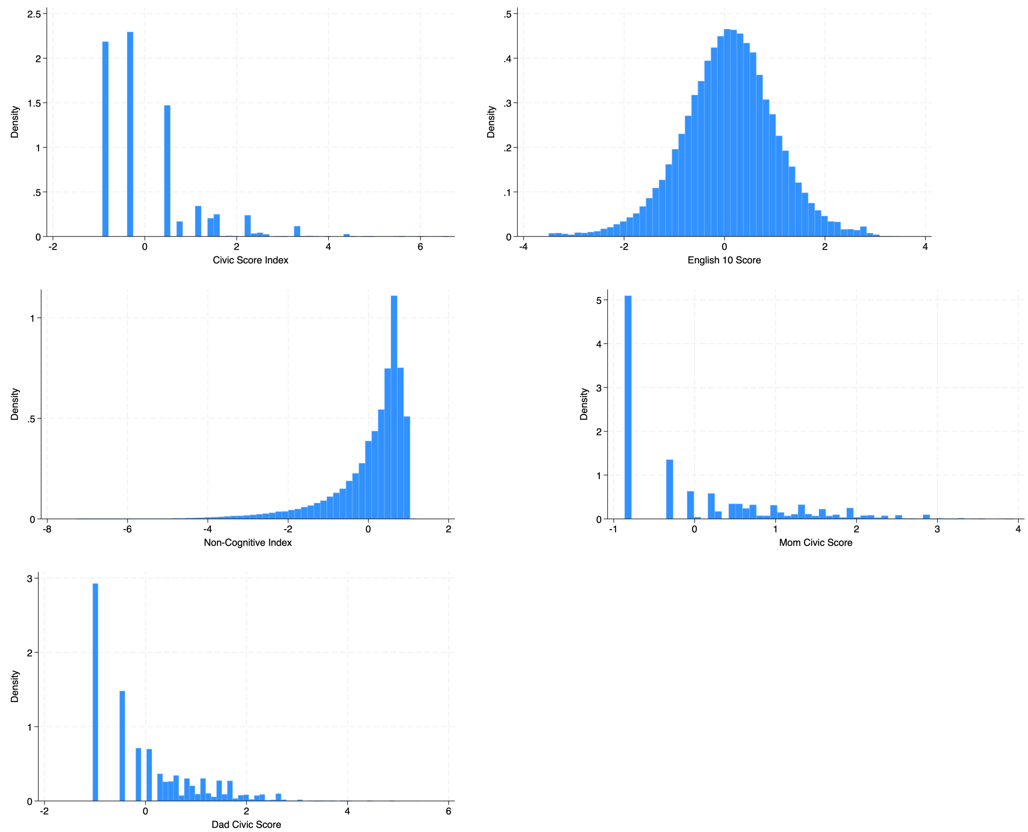


Figure A2: Distribution of School Effect Estimates

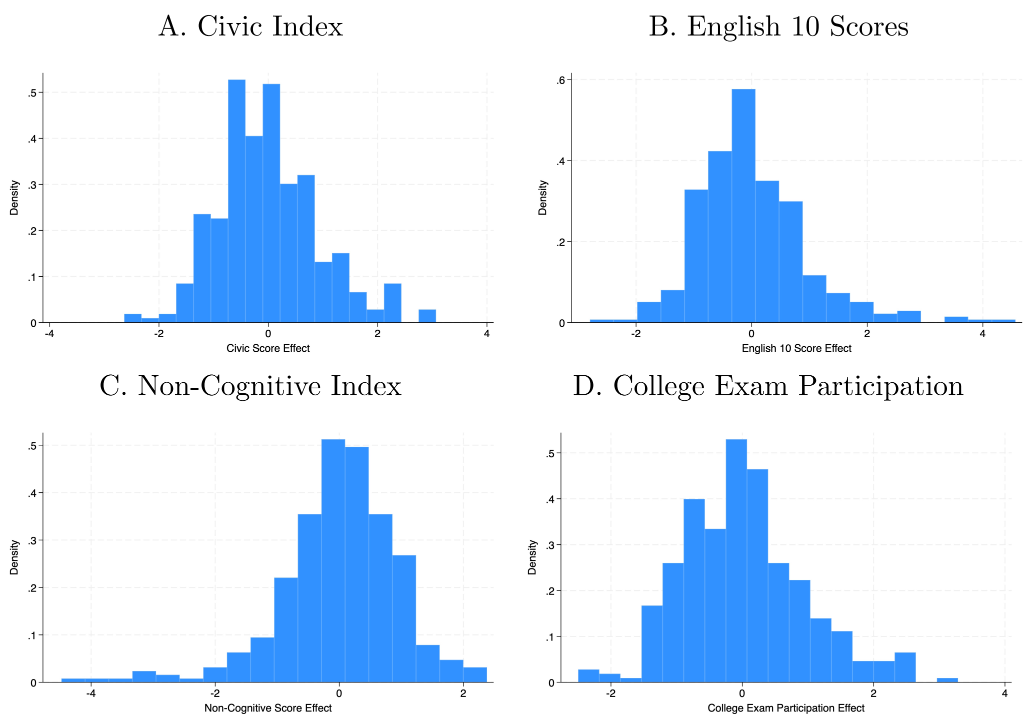
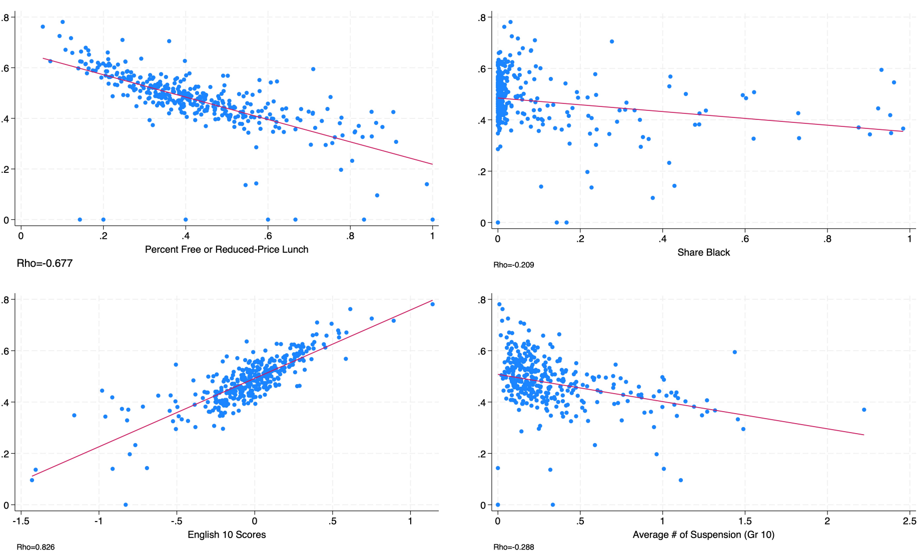


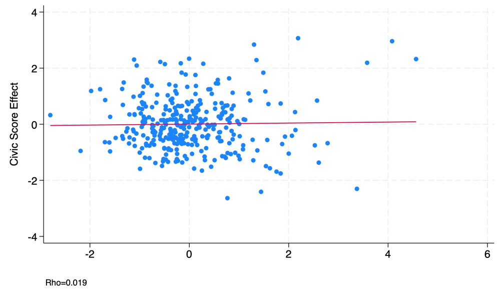
Figure A3: Share of Students who Voted and School Characteristics



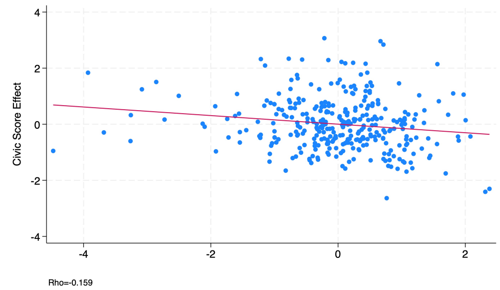
Y-axis is share of students who ever were ever recorded voting.

Figure A4: Civic School Effect and Effects on Other Outcomes

A. Test Scores



B. Non-Cognitive Outcomes



C. Taking the ACT/SAT

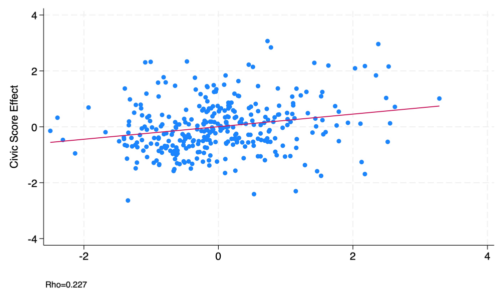
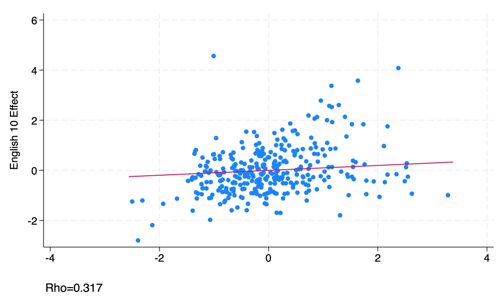
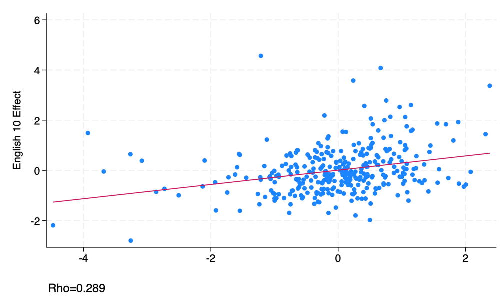


Figure A5: Non-Civic Effect Correlations

A. College Exam vs. Test Scores



B. Test Scores vs. Non-Cognitive



C. College Exam vs. Non-Cognitive

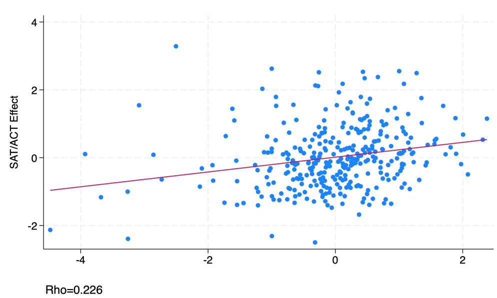
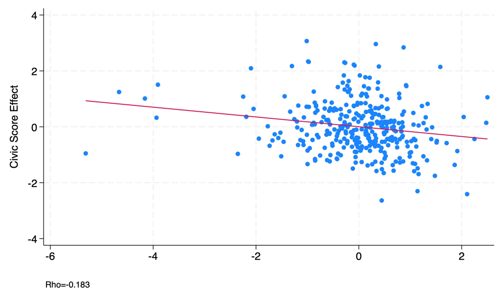
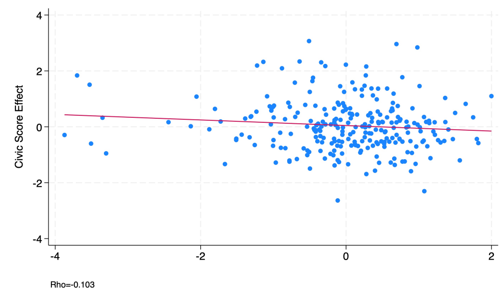


Figure A6: Civic Effects and Alternative Non-Cognitive Effects Estimates

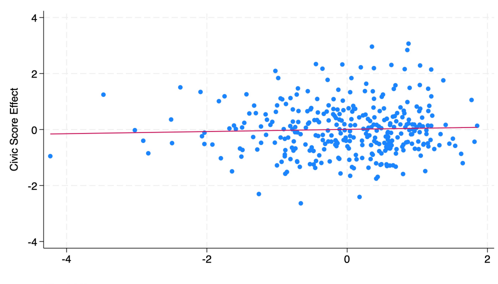
A. Civic Effects vs. Non-Cognitive (no GPA)



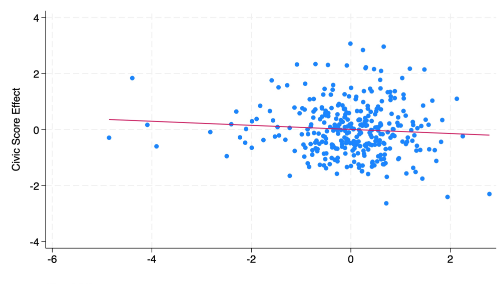
B. Civic Effects vs. Non-Cognitive (no imputed GPA)



C. Civic Effects vs. Behavior/Disciplinary Effects

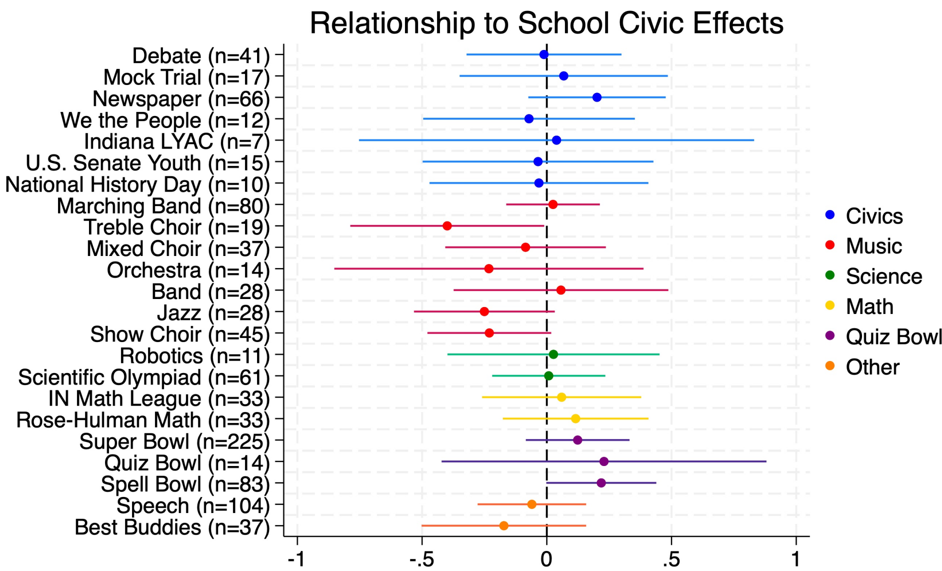


D. Civic Effects vs. Grade Progression Effects



The index measure in Panel A includes all measures used in the main specification except imputed GPA. The index measure in Panel B includes all measures used in the main specification but only uses actual GPA. The index measure in Panel C is created using disciplinary/behavioral outcomes (log suspensions, log unexcused absences, an indicator for expulsion). This is reverse coded so that a larger value reflects fewer undesirable disciplinary/behavioral issues. The index measure in Panel D is created using the non-disciplinary/behavioral outcomes (i.e., grade progression measures – imputed GPA, on time progression to grade 10, course pass rate). All indices are created with PCA and standardized, as described in the text.

Figure A7: Extracurricular Activities and School Civic Effects



Plots point estimates and 95% confidence intervals for indicators for each activity from separate regressions predicting civic school effects with an indicator for the presence of each activity with the same controls as used in Table 10, adding a control for the total number of activities in the school. Robust standard errors.

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)
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
2001	35075	0.74	0.46	0.75	0.45

Merging sample. Only includes birth years included in estimation sample. Birth years below 1990 and 2002 or above repressed because of small sample size.

Table A2: Outcome/Index Measure Summary Statistics

	Source	Type	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 end year	L2	Binary	0.71	0.46	456,699	2008	2016	0.000
Voted in first age-eligible general election	L2	Binary	0.23	0.42	456,699	2008	2016	0.000
Voted in second age-eligible general election	L2	Binary	0.25	0.43	456,699	2008	2016	0.000
Voted in first age-eligible primary election	L2	Binary	0.08	0.27	456,699	2008	2016	0.000
Voted in second age-eligible primary election	L2	Binary	0.08	0.27	456,699	2008	2016	0.000
Number of years voted outside of general/primary elections in first 4 age-eligible election cycles (odd and even years)*	L2	Integer	0.04	0.19	456,699	2008	2016	0.000
B. Test Score								
English 10 Score	IN DOE	Continuous	0.09	0.94	280,311	2009	2014	0.058
C. Non-Cognitive Measures								
Log: suspensions in grade 9 (+1)	IN DOE	Continuous	0.16	0.45	456,699	2008	2016	0.000
Log: days unexcused absences in grade 9 (+1)	IN DOE	Continuous	0.57	0.83	456,699	2008	2016	0.000
Indicator: ever expelled in grade 9	IN DOE	Binary	0.01	0.09	456,699	2008	2016	0.000
Grade 9 course passing rate	IN DOE	Binary	0.92	0.17	247,063	2012	2016	0.046
Indicator: progressed to grade 10 on time (observed in following year in grade 10 in data)	IN DOE	Binary	0.97	0.18	456,699	2008	2016	0.000
Grade 9 GPA	IN DOE	Continuous	2.58	0.94	247,063	2012	2016	0.046
D. College-Going Measure								
Indicator: Took ACT or SAT	IN DOE	Binary	0.59	0.49	456,699	2008	2016	0.000
E. Mom Civic Score								
Mom registered to vote prior to child's first 9th grade year	L2	Binary	0.56	0.50	456,699	2008	2016	0.000
Number of primary elections mom participated in; 8 years prior to child's 9th grade	L2	Integer	0.55	1.05	456,699	2008	2016	0.000
Number of general elections mom participated in; 8 years prior to child's 9th grade	L2	Integer	1.21	1.55	456,699	2008	2016	0.000
Number of other elections mom participated in; 8 years prior to child's 9th grade	L2	Integer	0.20	0.56	456,699	2008	2016	0.000
F. Dad Civic Score								
Dad registered to vote prior to child's first 9th grade year	L2	Binary	0.69	0.46	420,481	2008	2016	0.079
Number of primary elections dad participated in; 8 years prior to child's 9th grade	L2	Integer	0.63	1.11	420,481	2008	2016	0.079
Number of general elections dad participated in; 8 years prior to child's 9th grade	L2	Integer	1.44	1.61	420,481	2008	2016	0.079
Number of other elections dad participated in; 8 years prior to child's 9th grade	L2	Integer	0.23	0.60	420,481	2008	2016	0.079

Table A3: Correlations: Civic Index Measures (Student)

	Civic Index	Registered to Vote in First 4 Election Cycles	Voted: 1st General Election	Voted: 2nd General Election	Voted: 1st Primary Election	Years Voted: Odd Year Election	Number of Cycles Voted (Non-Primary, Non-General)
Civic Index	1.00	0.63	0.66	0.69	0.56	0.59	0.45
	[456699]	[456699]	[456699]	[456699]	[456699]	[456699]	[456699]
Registered to Vote in First 4 Election Cycles	0.63	1.00	0.35	0.37	0.19	0.19	0.12
	[456699]	[456699]	[456699]	[456699]	[456699]	[456699]	[456699]
Voted: 1st General Election	0.66	0.35	1.00	0.26	0.35	0.18	0.18
	[456699]	[456699]	[456699]	[456699]	[456699]	[456699]	[456699]
Voted: 2nd General Election	0.69	0.37	0.26	1.00	0.19	0.38	0.17
	[456699]	[456699]	[456699]	[456699]	[456699]	[456699]	[456699]
Voted: 1st Primary Election	0.56	0.19	0.35	0.19	1.00	0.17	0.16
	[456699]	[456699]	[456699]	[456699]	[456699]	[456699]	[456699]
Voted: 2nd Primary Election	0.59	0.19	0.18	0.38	0.17	1.00	0.19
	[456699]	[456699]	[456699]	[456699]	[456699]	[456699]	[456699]
Number of Cycles Voted (Non-Primary, Non-General)	0.45	0.12	0.18	0.17	0.16	0.19	1.00
	[456699]	[456699]	[456699]	[456699]	[456699]	[456699]	[456699]

All variables over the first four age-eligible vote cycles (two even years and two odd years). Observations for pairwise correlations in brackets.

Table A4: Correlations: Maternal Civic Index Measures

	Mom Civic Index	Mom Registered to Vote	Number of Primary Elections Voted	Number of General Elections Voted	Years Voted in Other (Non-General, Non-Primary)
Mom Civic Index	1.00	0.77	0.84	0.92	0.67
	[456699]	[456699]	[456699]	[456699]	[456699]
Mom Registered to Vote	0.77	1.00	0.46	0.69	0.31
	[456699]	[456699]	[456699]	[456699]	[456699]
Number of Primary Elections Voted	0.84	0.46	1.00	0.73	0.46
	[456699]	[456699]	[456699]	[456699]	[456699]
Number of General Elections Voted	0.92	0.69	0.73	1.00	0.47
	[456699]	[456699]	[456699]	[456699]	[456699]
Years Voted in Other (Non-General, Non-Primary)	0.67	0.31	0.46	0.47	1.00
	[456699]	[456699]	[456699]	[456699]	[456699]

All variables are defined over the 8 years before the child's 9th grade year. Observations for pairwise correlations in brackets.

Table A5: Correlations: Paternal Civic Index Measures

	DadCivic Index	Dad Registered to Vote	Number of Primary Elections Voted	Number of General Elections Voted	Years Voted in Other (Non-General, Non-Primary)
Dad Civic Index	1.00	0.71	0.83	0.90	0.67
	[420481]	[420481]	[420481]	[420481]	[420481]
Dad Registered to Vote	0.71	1.00	0.38	0.60	0.26
	[420481]	[420481]	[420481]	[420481]	[420481]
Number of Primary Elections Voted	0.83	0.38	1.00	0.70	0.45
	[420481]	[420481]	[420481]	[420481]	[420481]
Number of General Elections Voted	0.90	0.60	0.70	1.00	0.45
	[420481]	[420481]	[420481]	[420481]	[420481]
Years Voted in Other (Non-General, Non-Primary)	0.67	0.26	0.45	0.45	1.00
	[420481]	[420481]	[420481]	[420481]	[420481]

All variables are defined over the 8 years before the child's 9th grade year. Observations for pairwise correlations in brackets.

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	GPA (with imputation)
Noncognitive Index	1.00	-0.66	-0.63	-0.26	0.86	0.35	0.85
	[247063]	[247063]	[247063]	[247063]	[247063]	[247063]	[247063]
Log Gr9 Suspensions	-0.66	1.00	0.31	0.18	-0.45	-0.17	-0.41
	[247063]	[456699]	[456699]	[456699]	[247063]	[456699]	[247063]
Log Gr9 Unexcused Absences	-0.63	0.31	1.00	0.08	-0.38	-0.17	-0.40
	[247063]	[456699]	[456699]	[456699]	[247063]	[456699]	[247063]
Indicator: Expelled (Gr 9)	-0.26	0.18	0.08	1.00	-0.15	-0.13	-0.11
	[247063]	[456699]	[456699]	[456699]	[247063]	[456699]	[247063]
Gr 9 Passing Rate	0.86	-0.45	-0.38	-0.15	1.00	0.23	0.74
	[247063]	[247063]	[247063]	[247063]	[247063]	[247063]	[247063]
Entered Grade 10 On Time	0.35	-0.17	-0.17	-0.13	0.23	1.00	0.18
	[247063]	[456699]	[456699]	[456699]	[247063]	[456699]	[247063]
GPA (with imputation)	0.85	-0.41	-0.40	-0.11	0.74	0.18	1.00
	[247063]	[247063]	[247063]	[247063]	[247063]	[247063]	[247063]

Measures used to construct non-cognitive index measure. Observations in brackets. Course data is not available for all years as described in text.

Table A7: Voting Outcomes and Alternative Non-Cognitive Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Non-Cognitive Effect (Main)	0.002	0.004*								
	(0.002)	(0.002)								
No Imputed GPA			0.001	0.001						
			(0.002)	(0.002)						
Drop GPA					-0.001	0.001				
					(0.003)	(0.002)				
Behavior/Disciplinary							-0.004+	-0.004**		
							(0.002)	(0.001)		
Grade Progression									0.004**	0.006***
									(0.002)	(0.002)
English 10 Effect		0.009***		0.010***		0.010***		0.010***		0.009***
		(0.002)		(0.002)		(0.002)		(0.002)		(0.002)
College Exam Effect		0.002		0.001		0.003		0.002		0.001
		(0.002)		(0.002)		(0.002)		(0.002)		(0.002)
Civic Effect		0.025***		0.022***		0.024***		0.024***		0.025***
		(0.002)		(0.002)		(0.002)		(0.002)		(0.002)
Student, Cohort, First Election Controls	X	X	X	X	X	X	X	X	X	X
Observations	253,171	146,014	171,717	100,451	253,171	146,014	456,699	297,672	253,171	146,014

See notes for Table 7. Index that does not use GPA only uses actual GPA. Index that drops GPA drops GPA from the measures used to create the index. Behavior/disciplinary index measure is created using log suspensions, log unexcused absences, and an indicator for being expelled (grade 9). The behavior/disciplinary index is reverse coded such that a higher value indicates a school that reduces behavioral/disciplinary issues. Grade progression index is created using all other (non-disciplinary/behavioral) measures in the main non-cognitive index (progression to grade 10 on tiem, GPA, course passing rate). See text for detail. +p<0.10, *p<0.05, ** p<0.01, ***p<0.001. Number of observations differs because not all cohorts have data available for all measures and some individuals are missing data for certain measures.

Table A8: Summary of Control Variables

Covariate	Mean	SD	N non-missing	Min Grade Cohort	Max Grade Cohort
Black	0.07	0.261	456699	2008	2016
White	0.84	0.367	456699	2008	2016
Hispanic	0.05	0.209	456699	2008	2016
Asian/Pacific Islander/Hawaiian	0.01	0.075	456699	2008	2016
Free or Reduced-Price Lunch	0.37	0.482	456699	2008	2016
Male	0.50	0.500	456699	2008	2016
English Leaner	0.01	0.090	456699	2008	2016
Special Education	0.12	0.324	456699	2008	2016
Age in Gr 9	14.81	0.440	456699	2008	2016
Gr 8 Math (z-score)	0.07	0.964	456699	2008	2016
Gr 8 ELA (z-score)	0.05	0.972	456699	2008	2016
Misisng Either Gr 8 Test	0.01	0.096	456699	2008	2016
(Ln) Lag1 Unexcused Absences	0.58	0.824	456699	2008	2016
(Ln) Lag1 Suspensions	0.16	0.443	456699	2008	2016
Missing Lag 1 Unexcused Absences	0.02	0.127	456699	2008	2016
Mom Civic Score	0.01	1.004	456699	2008	2016
Dad Civic Score	0.01	0.961	456699	2008	2016
Matched to Dad	0.92	0.270	456699	2008	2016
Missing County of Birth	0.01	0.101	456699	2008	2016
Ln(County Population)	11.98	1.667	456699	2008	2016
County % BA or Higher	19.32	7.111	456699	2008	2016
County Poverty Rate	10.40	3.249	456699	2008	2016
County Turnout	0.54	0.069	456699	2008	2016
County Rurality	2.31	1.958	456699	2008	2016
County Political Competitiveness	0.54	0.063	456699	2008	2016
County Republican Partisanship	11.36	15.807	456699	2008	2016
Cohort Size	367.88	239.354	456699	2008	2016
Cohort Share Has Mom	0.72	0.103	456699	2008	2016
Cohort Share Has Dad	0.66	0.113	456699	2008	2016
Cohort Mean Mom Civic Score	0.00	0.211	456699	2008	2016
Cohort Mean Dad Civic Score	0.00	0.212	456699	2008	2016
Cohort Mean Gr 8 ELA Score	0.02	0.278	456699	2008	2016
Cohort Mean Gr 8 Math Score	0.03	0.323	456699	2008	2016
Cohort Share Missing Either Gr8 Test Score	0.07	0.037	456699	2008	2016

Measures are as described in text. County-level measures are from around the time of the child's birth, as described.

Table A9: Correlations with Alternative Civic School Effect Estimates

Model	Description	Preferred Specification
1	Preferred (esimated with vam)	1.0000
2	Drift Limit=3	0.9704
3	Lagged Fall Test Interactions	0.9629
4	No Residualization on School Effects	0.7650
5	No Cohort FE	0.9928
6	No First Election FE	0.9995
7	No First Election FE, No Cohort FE	0.6381
8	SchoolxCohort Residuals	0.7146
	Observations	456,699

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 with interactions for fall test score dates. Model 4 shows results estimated without residualizing on school effects. Model 5 does not include cohort fixed effects. Model 6 does not include first age-eligible election fixed effects. Model 7 drops both sets of fixed effects. Model 8 shows average school-by-cohort residuals from equation (3).

Table A10: Distribution of Other Effects by Quartile Civic Effects

	Q1 (smallest)	Q2	Q3	Q4 (largest)	Beta (uncontrolled)	Beta (controlled)
	(1)	(2)	(3)	(4)	(5)	(6)
N Schools	84	84	84	83		
Effects on Other Outcomes						
English 10 Test Effect	0.162	-0.143	-0.111	0.092	0.018	0.019
	(1.054)	(0.754)	(0.722)	(1.191)	(0.083)	(0.080)
Non-Cognitive Effect	0.144	0.016	0.022	-0.191	-0.153**	-0.013
	(1.032)	(0.901)	(0.918)	(0.997)	(0.058)	(0.054)
College Exam Effect	-0.265	-0.160	0.150	0.312	0.225***	0.071
	(0.843)	(0.872)	(0.883)	(1.036)	(0.060)	(0.058)

Plots average school-level effects on non-civic outcomes by quartile of (average) effects on civic outcomes. See notes for Table 8 for detail. + p<0.10, *p<0.05, ** p<0.01, ***p<0.001. Two schools are missing data for non-cognitive score effects.

Table A11: AP/Extracurricular Measure Summary Statistics

Measure	Mean	SD	N non-missing
(Log) Average # AP Tests	4.27	1.331	335
Participation rate (exams/HS student)			
US History	0.02	0.021	335
US Government	0.01	0.015	335
English	0.02	0.022	335
Calculus AB	0.02	0.012	335
English Literature	0.02	0.020	335
Biology	0.01	0.013	335
Chemistry	0.01	0.011	335
Psychology	0.01	0.013	335
World History	0.01	0.018	335
Statistics	0.00	0.010	335
Average scores			
US History	2.15	0.681	264
US Government	2.30	0.844	184
English	2.63	0.667	270
Calculus AB	2.06	0.781	325
English Literature	2.50	0.563	258
Biology	2.21	0.672	259
Chemistry	1.99	0.782	259
Psychology	2.62	1.047	157
World History	2.49	0.839	144
Statistics	2.38	0.960	177
Any Extracurricular			
Civic	0.31	0.462	335
Music	0.37	0.483	335
Science	0.21	0.407	335
Math	0.15	0.360	335
Quiz	0.70	0.458	335
Speech	0.31	0.463	335
Best Buddies	0.11	0.314	335
Count of Extracurriculars			
All Extracurriculars	3.47	3.524	335
Civic	0.50	0.935	335
Music	0.75	1.296	335
Science	0.21	0.426	335
Math	0.20	0.498	335
Quiz	0.96	0.775	335

Table A12: 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 insurmental 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 A13: Mean Civic Score Index Measures by Student Cohort

Grade 9 Cohort (Spring)	Student Civic Score (mean)	Mom Civic Score (mean)	Dad Civic Score (mean)
2008	-0.24	-0.12	-0.11
2009	-0.20	-0.06	-0.06
2010	-0.07	0.02	0.02
2011	0.01	0.02	0.03
2012	0.12	0.06	0.06
2013	0.29	0.05	0.05
2014	0.24	0.06	0.07
2015	0.27	0.05	0.05
2016	0.18	0.03	0.02
Overall Mean	0.07	0.01	0.01

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[32] 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.[33] 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

[32]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.

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

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% on average over all years.

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 A1 (earlier) reports summary statistics on match rates and gender breakdown of matched observations by year of birth.

Appendix Table B1 shows match rates by student birth year across voting and birth records.

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.

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	996,037	0.72
Matched to Birth	996,036	996,036	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	40,195	0.69
Born 1992	86,105	57,631	0.67
Born 1993	85,799	56,374	0.66
Born 1994	84,610	58,187	0.69
Born 1995	82,065	56,929	0.69
Born 1996	82,394	57,862	0.70
Born 1997	83,818	59,362	0.71
Born 1998	85,604	60,453	0.71
Born 1999	85,778	64,247	0.75
Born 2000	86,590	65,366	0.76
Born 2001	85,072	63,937	0.75
Born 2002	83,792	62,748	0.75
Born 2003	85,116	64,012	0.75
Born 2004	85,504	63,944	0.75
Born 2005	85,140	63,921	0.75
Born 2006	84,000	63,085	0.75
Born 2007	43,142	31,506	0.73

* Birth years above 2007 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 Birth Records: Matched to Dad	Matched to Birth Records: 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	996,036	996,036	996,036
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	996,036	996,036	996,036
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	996,036	996,036	996,036
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	996,036	996,036	996,036
Asian American/Pacific Islander (Hawaiian)				
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	996,036	996,036	996,036
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	996,036	996,036	996,036
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
Number of observations	1,383,526	996,036	996,036	996,036
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	995,977	995,977	995,977

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.[34] 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. (See Appendix Table C2 for some evidence of coverage in early elections). 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.[35] 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

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

[35] 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.

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 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 required that matches identified by fastLink exactly match on last name, birth date, and/or full name. 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 larger 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)	4,356,818	8,221,447	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	5,705,460	12,442,215	4,648,678	9,430,522	18,486,104	20,218,640	12,901,128	28,367,183	11,435,568	11,776,135	7,186,071	5,290,828	7,975,389	5,172,423	5,284,370	4,265,411	2,273,568
Unqiue Voter IDs	5,469,205	10,184,953	4,455,856	9,147,781	17,753,278	19,363,210	9,788,532	27,866,357	8,784,322	8,760,651	6,847,437	5,121,014	5,758,244	4,946,229	4,227,071	4,109,230	2,192,687
Unique First/Last/Middle Initial/DOB	5,695,701	12,336,749	4,371,750	9,373,307	18,449,324	20,151,573	12,135,128	28,227,922	11,335,214	11,643,395	7,173,919	5,255,653	7,774,106	5,166,951	5,266,538	4,262,176	2,271,535
Share duplicated at all	0.003	0.017	0.104	0.012	0.004	0.007	0.113	0.010	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	0.020	0.003	0.011	0.046	0.002	0.006	0.001	0.002
Share duplicated >1 time	<0.001	0.001	0.039	<0.001	<0.001	<0.001	0.015	<0.001	0.002	0.002	<0.001	0.002	0.003	<0.001	0.001	<0.001	<0.001
C. Student Sample																	
N records	1,437,982	3,157,963	1,149,696	2,403,884	4,371,604	5,767,882	3,066,966	8,264,306	3,346,830	3,634,184	1,952,262	1,336,294	1,853,121	1,378,377	1,293,050	968,142	611,331
Unique First/Last/Middle Initial/DOB	1,437,982	3,157,963	1,149,696	2,403,884	4,371,604	5,767,882	3,066,966	8,264,306	3,346,830	3,634,184	1,952,262	1,336,294	1,853,121	1,378,377	1,293,050	968,142	611,331
Unqiue Fullname/DOB	1,437,771	3,155,624	1,149,222	2,403,488	4,370,747	5,765,788	3,066,636	8,237,978	3,345,894	3,632,210	1,950,960	1,335,532	1,850,031	1,377,964	1,292,342	968,142	611,231
Unique Voter IDs	1,347,238	2,638,990	1,071,091	2,298,958	4,162,368	5,546,914	2,427,423	8,139,023	2,553,516	2,485,635	1,848,104	1,280,043	1,509,646	1,291,906	1,077,642	917,867	576,701
Share collapsed voters	0.001	0.007	0.035	0.004	0.001	0.003	0.060	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	<0.001	<0.001	0.021	0.002	0.001	<0.001	0.032	<0.001	0.002	<0.001	<0.001	0.009	0.012	0.013	<0.001	<0.001
Share male	0.479	0.466	0.462	0.486	0.469	0.470	0.479	0.480	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	4,981,666	10,827,001	3,832,567	8,244,467	16,232,408	17,145,041	10,671,670	24,165,111	9,688,123	9,778,557	6,234,303	4,642,118	6,869,573	4,483,129	4,638,065	3,819,704	1,959,860
Unique First/Last/Middle Initial/DOB	4,981,666	10,827,001	3,832,567	8,244,467	16,232,408	17,145,041	10,671,670	24,165,111	9,688,123	9,778,557	6,234,303	4,642,118	6,869,573	4,483,129	4,638,065	3,819,704	1,959,860
Unqiue Fullname/DOB	4,980,692	10,818,338	3,831,116	8,243,126	16,228,794	17,139,150	10,670,239	24,039,864	9,684,680	9,773,962	6,229,668	4,639,311	6,861,461	4,482,288	4,636,121	3,819,695	1,959,602
Unique Voter IDs	4,778,147	8,873,455	3,720,421	7,997,362	15,578,848	16,381,838	8,076,922	23,738,351	7,457,217	7,455,581	5,928,733	4,493,569	4,885,118	4,285,844	3,660,243	3,678,739	1,890,473
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	<0.001	0.001	<0.001	<0.001
Share duplciate names/DOB (randomly selected)	0.001	0.004	0.026	0.004	0.001	0.001	0.020	0.004	0.003	0.003	<0.001	0.002	0.010	0.001	0.002	<0.001	0.001
Share missing gender	0.001	<0.001	<0.001	0.011	0.001	0.001	<0.001	0.019	<0.001	0.001	<0.001	<0.001	0.004	0.008	0.009	<0.001	<0.001
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	0.450	0.466
Share missing middle initial	0.094	0.194	0.063	0.072	0.159	0.138	0.071	0.234	0.097	0.070	0.067	0.103	0.112	0.071	0.053	0.090	0.066

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)	6,127,664	3,824,119	14,416,297	4,785,050	3,438,589	4,528,434	8,170,707	2,024,695	1,608,203	1,073,592	4,941,569	1,932,177	4,170,282	692,447	2,329,404	3,268,212	2,085,813
B. Full Multiyear Sample																	
N records	7,526,661	6,165,508	16,656,714	5,475,516	4,321,328	5,665,814	10,305,109	2,653,748	2,130,433	1,470,780	6,257,236	2,217,637	4,474,667	896,636	3,093,999	4,171,232	2,743,766
Unqiue Voter IDs	7,214,891	4,815,307	16,135,640	3,950,204	4,068,510	5,515,137	9,965,863	2,541,791	2,054,696	1,420,480	6,056,341	1,790,807	4,302,532	867,284	2,997,971	3,845,660	2,657,868
Unique First/Last/Middle Initial/DOB	7,502,192	6,152,672	16,518,657	5,372,539	4,311,098	5,648,752	10,284,915	2,642,902	2,127,837	1,469,238	6,247,207	2,199,105	4,468,863	896,257	3,051,956	4,166,295	2,729,206
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	0.016	0.003	0.001	0.025	0.002	0.010
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	<0.001	<0.001	0.001	0.002	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	0.001	<0.001	<0.001	0.008	<0.001	0.001
C. Student Sample																	
N records	1,942,933	1,910,416	3,771,324	513,539	1,093,505	1,469,037	2,526,582	761,561	563,789	354,118	1,694,706	232,944	924,302	217,968	746,928	1,169,055	683,711
Unique First/Last/Middle Initial/DOB	1,942,933	1,910,416	3,771,324	513,539	1,093,505	1,469,037	2,526,582	761,561	563,789	354,118	1,694,706	232,944	924,302	217,968	746,928	1,169,055	683,711
Unqiue Fullname/DOB	1,942,900	1,909,688	3,770,552	513,512	1,092,771	1,468,494	2,526,181	760,993	563,682	354,097	1,694,199	232,940	923,936	217,964	746,854	1,168,603	683,525
Unique Voter IDs	1,867,598	1,473,349	3,622,629	449,029	1,024,979	1,425,868	2,419,982	724,208	533,182	334,312	1,627,161	203,699	875,223	207,124	721,124	1,049,683	655,903
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	<0.001	<0.001	0.006	0.001	0.005
Share duplciate names/DOB (randomly selected)	0.001	0.001	0.003	0.020	0.001	0.001	0.001	0.001	<0.001	<0.001	<0.001	0.002	0.001	<0.001	0.011	0.001	0.001
Share missing gender	0.021	<0.001	0.001	0.017	<0.001	0.010	0.006	0.022	0.016	0.002	0.001	0.031	<0.001	0.030	<0.001	0.012	0.010
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	0.500	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	0.060	0.067	0.197	0.151	0.118	0.044
D. Parent Sample																	
N records	6,499,614	5,273,004	14,684,837	5,179,357	3,773,981	4,964,379	9,080,152	2,278,911	1,835,687	1,291,344	5,464,416	2,114,273	4,135,015	794,914	2,692,415	3,592,925	2,393,097
Unique First/Last/Middle Initial/DOB	6,499,614	5,273,004	14,684,837	5,179,357	3,773,981	4,964,379	9,080,152	2,278,911	1,835,687	1,291,344	5,464,416	2,114,273	4,135,015	794,914	2,692,415	3,592,925	2,393,097
Unqiue Fullname/DOB	6,498,683	5,269,965	14,680,474	5,178,949	3,771,160	4,960,216	9,078,666	2,277,782	1,835,369	1,291,175	5,462,860	2,114,055	4,132,086	794,877	2,691,985	3,591,987	2,392,550
Unique Voter IDs	6,225,371	4,102,415	14,260,470	3,704,188	3,548,228	4,830,050	8,776,227	2,181,945	1,772,313	1,248,908	5,284,714	1,707,142	3,977,888	768,935	2,607,019	3,328,367	2,319,886
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	<0.001	<0.001	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	<0.001	<0.001	<0.001	0.006	0.001	<0.001	0.008	<0.001	0.001
Share missing gender	0.012	<0.001	<0.001	0.009	<0.001	0.004	0.004	0.015	0.009	0.001	<0.001	0.015	<0.001	0.013	0.001	0.010	0.006
Share male	0.460	0.480	0.453	0.474	0.440	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	0.050

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)	2,074,150	422,604	530,462	2,854,325	534,312	1,002,660	793,102	465,359	722,662	1,174,456	1,511,677	284,937	576,989	1,043,961	941,511	723,223	1,269,553
B. Full Multiyear Sample																	
N records	2,626,605	467,429	573,139	6,345,180	583,460	1,239,075	813,370	777,785	1,203,683	1,492,126	2,522,630	332,270	838,057	1,542,682	2,093,193	988,627	2,056,977
Unqiue Voter IDs	2,524,616	382,552	461,136	3,673,147	466,332	950,420	682,982	590,845	843,122	1,433,984	1,996,854	262,568	630,442	1,230,195	1,164,533	918,223	1,563,649
Unique First/Last/Middle Initial/DOB	2,625,307	466,729	570,569	6,108,844	579,455	1,233,452	808,705	774,037	1,201,947	1,491,173	2,518,351	326,310	836,192	1,535,257	2,076,772	986,207	2,050,535
Share duplicated at all	0.001	0.003	0.009	0.070	0.013	0.009	0.011	0.010	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	<0.001	<0.001	<0.001	0.013	0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	0.001	<0.001	<0.001	<0.001
C. Student Sample																	
N records	737,357	47,339	73,273	1,439,071	95,841	130,632	65,366	165,939	308,410	396,416	811,692	24,161	145,140	295,802	477,454	267,112	459,056
Unique First/Last/Middle Initial/DOB	737,357	47,339	73,273	1,439,071	95,841	130,632	65,366	165,939	308,410	396,416	811,692	24,161	145,140	295,802	477,454	267,112	459,056
Unqiue Fullname/DOB	737,231	47,328	73,174	1,438,844	95,840	130,628	62,603	165,925	308,319	396,407	810,105	24,160	145,128	295,782	477,409	267,095	458,904
Unique Voter IDs	690,220	41,450	61,736	904,944	87,602	110,542	60,524	142,321	213,351	369,809	628,462	22,124	116,856	247,409	279,513	236,649	371,619
Share collapsed voters	<0.001	<0.001	<0.001	0.020	0.001	0.001	0.003	0.004	0.001	<0.001	0.001	0.002	0.001	0.005	0.004	0.002	0.003
Share duplciate names/DOB (randomly selected)	<0.001	<0.001	<0.001	0.039	0.002	0.001	0.002	0.001	0.001	<0.001	0.001	0.054	0.001	0.002	0.009	<0.001	0.001
Share missing gender	<0.001	0.007	<0.001	<0.001	0.020	0.005	0.024	0.034	0.015	0.015	0.011	0.020	0.014	0.016	<0.001	0.009	<0.001
Share male	0.474	0.466	0.432	0.441	0.432	0.432	0.406	0.489	0.489	0.480	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	0.020	0.149	0.115	0.117	0.106
D. Parent Sample																	
N records	2,238,683	447,267	548,207	5,440,847	557,106	1,186,838	774,775	695,867	1,055,328	1,289,982	2,110,564	316,287	778,330	1,400,007	1,849,834	855,874	1,825,648
Unique First/Last/Middle Initial/DOB	2,238,683	447,267	548,207	5,440,847	557,106	1,186,838	774,775	695,867	1,055,328	1,289,982	2,110,564	316,287	778,330	1,400,007	1,849,834	855,874	1,825,648
Unqiue Fullname/DOB	2,238,295	447,164	547,715	5,439,465	557,078	1,186,795	751,010	695,660	1,055,146	1,289,889	2,107,926	316,266	778,289	1,399,881	1,849,705	855,704	1,825,358
Unique Voter IDs	2,152,225	365,130	440,977	3,130,170	443,805	908,842	650,852	520,493	731,651	1,240,326	1,693,605	248,391	583,775	1,105,773	1,028,464	804,840	1,374,124
Share collapsed voters	<0.001	<0.001	0.002	0.008	0.002	0.001	0.003	0.003	<0.001	<0.001	0.001	0.001	0.001	0.003	0.002	0.002	0.002
Share duplciate names/DOB (randomly selected)	<0.001	0.001	0.003	0.024	0.005	0.003	0.003	0.002	0.001	<0.001	0.001	0.016	0.001	0.002	0.006	0.001	0.001
Share missing gender	<0.001	0.003	<0.001	<0.001	0.016	0.005	0.004	0.013	0.010	0.005	0.009	0.008	0.008	0.007	<0.001	0.005	<0.001
Share male	0.468	0.486	0.493	0.445	0.457	0.467	0.476	0.473	0.465	0.470	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	0.080	0.163	0.099	0.049	0.097	0.080	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	<0.01	0.25	0.19	0.32
FL	0.036	2	18,449,324	61,356	<0.01	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	<0.01	0.14	0.12	0.25
OH	0.021	5	9,373,307	42,863	<0.01	0.32	0.24	0.45
KY	0.021	6	4,371,750	991,854	0.23	<0.01	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	<0.01	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	<0.01	<0.01	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	<0.01	0.32	0.49
SC	0.006	17	4,262,176	14,946	<0.01	0.19	0.17	0.28
WA	0.006	18	6,247,207	26,728	<0.01	0.22	0.19	0.31
AL	0.005	19	4,311,098	15,189	<0.01	0.22	0.21	0.33
PA	0.005	20	10,284,915	44,758	<0.01	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	<0.01	0.24	0.23	0.33
OK	0.003	26	2,729,206	9,225	<0.01	0.24	0.23	0.34
NV	0.003	27	2,642,902	7,781	<0.01	0.11	0.11	0.19
KS	0.003	28	2,271,535	8,062	<0.01	0.18	0.17	0.35
MA	0.002	29	5,648,752	19,835	<0.01	0.30	0.27	0.37
IA	0.002	30	2,625,307	11,801	<0.01	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	<0.01	0.20	0.30
MT	0.001	37	896,257	2,461	<0.01	0.21	0.20	0.28
WV	0.001	38	1,469,238	4,823	<0.01	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	<0.01	<0.01	0.43
ME	0.001	43	1,535,257	617,048	0.40	<0.01	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	<0.01	<0.01	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	<0.01	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	N has Mom	N Mom Matched	N Has Dad	N Dad Matched
IN	503,020	483,805	287,492	444,529	331,043
FL	9,968	483,805	4,278	444,529	7,354
IL	5,123	483,805	1,109	444,529	2,506
TX	6,440	483,805	1,299	444,529	2,315
OH	7,896	483,805	1,179	444,529	2,297
KY	3,508	483,805	1,540	444,529	2,886
MI	1,432	483,805	861	444,529	1,358
CA	5,721	483,805	617	444,529	1,116
TN	3,722	483,805	1,042	444,529	1,885
GA	760	483,805	390	444,529	592
AZ	704	483,805	415	444,529	566
NC	765	483,805	465	444,529	648
CO	1,085	483,805	286	444,529	360
VA	1,943	483,805	275	444,529	497
MO	1,839	483,805	319	444,529	610
WI	287	483,805	157	444,529	221
SC	1,412	483,805	573	444,529	967
WA	2,173	483,805	181	444,529	334
AL	1,341	483,805	453	444,529	778
PA	1,518	483,805	240	444,529	421
NY	1,777	483,805	271	444,529	468
MN	185	483,805	<100	444,529	<100
OR	434	483,805	<100	444,529	173
MD	291	483,805	<100	444,529	148
AR	539	483,805	129	444,529	280
OK	592	483,805	137	444,529	245
NV	1,187	483,805	227	444,529	471
KS	562	483,805	<100	444,529	210
MA	748	483,805	<100	444,529	108
IA	1,095	483,805	140	444,529	275
NJ	457	483,805	<100	444,529	116
MS	<100	483,805	<100	444,529	106
UT	262	483,805	<100	444,529	<100
NM	<100	483,805	<100	444,529	<100
LA	<100	483,805	<100	444,529	110
ID	<100	483,805	<100	444,529	<100
MT	290	483,805	<100	444,529	127
WV	292	483,805	<100	444,529	162
NE	297	483,805	<100	444,529	103
CT	216	483,805	<100	444,529	<100
HI	<100	483,805	<100	444,529	<100
AK	<100	483,805	<100	444,529	<100
ME	<100	483,805	<100	444,529	<100
ND	<100	483,805	<100	444,529	<100
DC	<100	483,805	<100	444,529	<100
WY	<100	483,805	<100	444,529	<100
NH	<100	483,805	<100	444,529	<100
SD	<100	483,805	<100	444,529	<100
RI	<100	483,805	<100	444,529	<100
DE	<100	483,805	<100	444,529	<100
VT	<100	483,805	<100	444,529	<100

D. Data Sources

- Clary, W., Gomez-Lopez, I. N., Chenoweth, M., Gypin, L., Clarke, P., Noppert, G., Li, M., & Kollman, K. (2024). National Neighborhood Data Archive (NaNDA): Voter registration, turnout, and partisanship by county, United States, 2004–2022 (Version V2) [Data set]. Inter-university Consortium for Political and Social Research. https://doi.org/10.3886/ICPSR38506.v2

- Leip, Dave. (2026). David Leip's Atlas of U.S. Presidential Elections Accessed via Columbia University Research Data Services). https://www.columbia.edu/acis/eds/holdings/4085/www1-data.pl@C4085.html.

- Indiana Bar Foundation. Data on school-level participation in We the People and Mock Trial. 2016-2020. [This data was shared with me by the Indiana Bar Foundation. Individuals interested in accessing this data should contact the Indiana Bar Foundation directly and request it.]

  All data from the Indiana Department of Education and Indiana Department of Health are restricted-use. I list all dates I have access to, whether or not all years were used in the final analysis.

- Indiana Department of Education. ACT Records. 2007-2021. [Data set].

- Indiana Department of Education. Advanced Placement Test Records. 2007-2021. [Data set].

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- Indiana Department of Education. Discipline Reports. 2007-2021. [Data set].
- Indiana Department of Education. End of Course Assessments. 2011-2015. [Data set].
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- U.S. Census Bureau. (2022). Table 1. State of residence by state of birth: 2022. [Data set]. U.S. Census Bureau. https://www.census.gov/data/tables/time-series/demo/geographic-mobility/state-of-residence-place-of-birth-acs.html