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The Stubborn Unresponsiveness of Youth Voter Turnout to Civic Education:

Quasi-experimental Evidence from State-Mandated Civics Tests

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Abstract: Youth voter turnout remains stubbornly low and unresponsive to civic education. Rigorous evaluations of the adoption of civic tests for high school graduation by some states on youth voter turnout remain limited. We estimate the impact of a recent, state-mandated civics test policy—the Civics Education Initiative (CEI)—on youth voter turnout by exploiting spatial and temporal variation in the adoption of CEI across states. Using nationally-representative data from the 1996-2020 Current Population Survey and a Difference-in-Differences analysis, we find that CEI does not significantly affect youth voter turnout. Our null results, largely insensitive to a variety of alternative specifications and robustness checks, provide evidence regarding the lack of efficacy of civic test policies when it comes to youth voter participation.

Keywords: youth voter turnout, civic education, quasi-experiment, difference-in-differences, fixed effects, civic engagement

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Introduction

Among advanced democracies, the United States has the largest age gap in voter turnout (Holbein & Hillygus, 2020, pp. 3–4). For example, in the 2020 presidential election, voter turnout among young people, aged 18-24, was approximately 25 percentage points lower than those aged 65-74 (U.S. Census Bureau, 2021). Despite reaching one of its highest levels ever since the legalization of voting at 18 years old in 2020 (Center for Information & Research on Civic Learning and Engagement (CIRCLE), 2021), youth voter turnout remains low at 48 percent. Such low youth turnout is particularly concerning as several studies show that early voting experiences predict future voting participation (Coppock & Green, 2016; Dinas, 2012; Fujiwara et al., 2016).

Scholars, educators, and policymakers often recommend civic education as a solution to low youth voter turnout (Battistoni, 2013; D. Campbell et al., 2012; National Council for the Social Studies, 2005). They often argue that civic education prepares students to become responsible and participatory citizens by teaching them civic knowledge (e.g., voting processes and electoral systems), values, and skills (National Council for the Social Studies, 2013). A growing body of research has evaluated that claim by examining the effect (or lack thereof) of civic education (operationalized as enrollment in AP History/Government courses or high school civic coursework) in schools on voter turnout (Holbein & Hillygus, 2020; Weinschenk & Dawes, 2022). Uneven access to and enrollment in AP courses and advanced civic coursework across the country (Xu et al., 2021), along with significant selection effects into who enrolls in such courses has precluded a causal evaluation of civic education on youth voter turnout. Furthermore, little attention has been paid to understanding the effects of state-mandated, civic test policies required for high school graduation in some states on voter turnout. Indeed, we are aware of only one study that has evaluated the effect of varied state-level civic test policies; however, even that study evaluated the effect primarily on students' civic knowledge (D. E. Campbell & Niemi, 2016) before some states adopted a revised civic-test policy requirement, which we evaluate in this study. In all, most prior work to date finds the lack of significant effects of civic education on objective civic engagement outcomes such as voter turnout (D. E. Campbell & Niemi, 2016; Holbein & Hillygus, 2020; Weinschenk & Dawes, 2022).

In this article, we estimate the causal effect of a state-mandated, civics test policy requirement for high school graduation on American young voter turnout leveraging variation in the adoption of the Civics Education Initiative (CEI) policy across states and time. CEI is the most standardized civics test policy, requiring high school students to take and/or pass a civics test as a condition for graduation (Civics Education Initiative, 2017). As of 2022, 18 states have implemented a version of CEI. Essentially, CEI aims to ensure that all high school students have the "bare minimum of [political] knowledge" necessary to become active and engaged citizens (Hess et al., 2015, p. 174). Since CEI was introduced, scholars have criticized its narrow focus on political knowledge and questioned its effectiveness (Brezicha & Mitra, 2019; Hess et al., 2015; Kahne, 2015; Levine, 2015). CEI policy, however, has not been empirically evaluated if at all, let alone using rigorous quasi-experimental techniques. Consequently, we have limited knowledge of whether and how such state-mandated, civics test policies affect political outcomes, including voter participation.

To fill such knowledge gap, this study estimates the causal effect of CEI on young voter turnout, using national, repeated cross-sectional data on self-reported voting behaviors of 18-24 years old US citizens from the 1996-2020 Current Population Survey (CPS) and Difference-in-Differences (DD) and event study approaches. We also examine the heterogeneous effects of

CEI on voter turnout by age, sex, race/ethnicity, and immigrant status. Consistent with prior findings (Holbein & Hillygus, 2020; Weinschenk & Dawes, 2022), we find no significant impact of state-mandated CEI on youth voter turnout. Concerningly, we found a marginal, negative impact of CEI policy on Black youth voter turnout ($b=-0.097$, $p<.10$); although we interpret this result with caution given the low sample size. Our null results are robust to several alternative specifications and robustness checks—including placebo checks, triple-difference (DDD) analysis, and alternative comparison groups—which continue to raise skepticism regarding the efficacy of state-mandated, civic education policies.

It may be unsurprising that mandating the civics test required for high school graduation failed to move the needle on the voter participation of young citizens in the presidential election at a time of historic uncertainty. The US and the world were actively grappling with the pandemic, the far-reaching consequences of persistent structural racism, and high political polarization. Future research should continue to examine the effect of CEI on civic knowledge and other forms of civic engagement. However, if the goal was to improve the voting participation of youth in a consequential election—a distal but also arguably the most important civic engagement outcome—CEI did not seem to have succeeded, at least in the short-term.

When it comes to improving youth voter turnout then, more direct interventions, including voting information interventions that aim to increase voters' understanding of the voting process (Bennion & Nickerson, 2016; Bergan et al., 2021; Gill et al., 2018) and/or electoral-level policy interventions—such as preregistration (Holbein & Hillygus, 2016), same day registration (Grumbach & Hill, 2022), and online registration (Yu, 2019)—might be far more effective. On the other hand, a broader curriculum targeting adolescents' non-cognitive skills—such as grit and task perseverance, which helps youth convert political motivation into actual participation (Holbein & Hillygus, 2020)—or other key psychosocial skills promoted by some high-achieving charter schools (Cohodes & Feigenbaum, 2021; McEachin et al., 2020) are also beginning to show promise. Future research should focus on identifying the mechanisms and components of such broad-based curricular components to promote civic engagement holistically among young citizens instead of narrowing the focus of civic education to basic rote memorization of political and/or civic knowledge.

Policy Background: The Civics Education Initiative

One recent state-level, civic education policy effort to address the low youth voter turnout is the Civics Education Initiative (CEI). CEI, an advocacy campaign lead by the Joe Foss Institute between September 2015 and September 2017, lobbied for state adoption of mandatory civics test requirement for high school graduation (Civics Education Initiative, 2017). In terms of the civics test format, CEI recommended that states choose 100 questions about basic federal historical and civic facts, drawn from the United States Citizenship and Immigration Services (USCIS) naturalization civics test (U.S. Citizenship and Immigration Service, 2022). This policy aimed to ensure that high school students graduate with the foundational civic knowledge necessary to become informed and engaged citizens. As a consequence of the advocacy campaign, Arizona adopted CEI in 2015, followed by 18 states, all of which adopted a form of CEI between 2015 and 2018 (Brennan & Railey, 2017; Brezicha & Mitra, 2019).

Leveraging the variation in CEI adoption across states over time, we classified states into two groups: states which have implemented CEI policy and states which have not implemented CEI policy at the time of the presidential election of 2020. Even though CEI aimed at making the civics test a high school graduation requirement, several states relaxed the graduation requirement. In other words, these states did not make high school graduation conditional on passing the civics test (Brennan & Railey, 2017). Based on this variation in policy implementation, we classified "treatment" states with CEI policy into two sub-categories: states with a *strong* civics test requirement and states with a *weak* civics test requirement. Table 1 shows the list of states in the treatment and comparison groups, respectively along with the exact dates of policy adoption and implementation. As can be seen from the Table 1, all treatment states, except Pennsylvania, passed the law between 2015 and 2017 despite some variation in the exact timing (Brennan & Railey, 2017). In all, we have 15 treatment states (referred to as "Omnibus" treatment hereafter) and 34 comparison states in our analytical sample.

[Table 1]

It is also important to place CEI within the context of other civic education policies in schools in the US. Prior to the widespread adoption of CEI by some states, several states had civic coursework completion and assessment requirements (D. E. Campbell & Niemi, 2016). Similarly, as described earlier, the rates of AP History and AP Government (the most relevant civic coursework analyzed by past studies) course availability, enrollments, and passing vary considerably across states (Holbein & Hillygus, 2020). Though several states that adopted CEI between 2015 and 2017 had historically lower rates of civic education prior to CEI, our treatment effects might be lower bound if comparison states had a historically higher civic course participation even though they did not adopt the new civics test requirement. For example, Virginia showed the highest rates of advanced civic coursework participation, with close to 9 percent of high school students enrolling in AP US History or Government courses in 2015. For our core analysis, we focus singularly on the variation in CEI adoption across states. However, in robustness checks, we try to disentangle these confounding effects by using alternative comparison groups. In other words, we compare states that adopted CEI with states that did not have high civic course participation. Our baseline results are robust to these alternative comparison groups. We return to this point in our discussion.

Civic education and voter turnout

Civic education is often viewed as an effective remedy for low youth voter turnout by scholars, educators, and policymakers alike (Battistoni, 2013; D. Campbell et al., 2012; National Council for the Social Studies, 2005). Specifically, civic education is expected to prepare students to be responsible and participatory citizens by teaching them political knowledge, values, and skills (National Council for the Social Studies, 2013). Despite this expectation, there are few studies that empirically evaluate the effect of civic education on voter participation, especially in comparison with studies on other civic engagement outcomes—such as, political knowledge and interest (see Holbein & Hillygus, 2020 for a good overview of the efficacy of civic education in schools). In addition, most observational studies suffer from selection bias (i.e., people who are more civically engaged and thus more likely to vote regardless are also more likely to enroll in more civics courses—such as AP US History or AP Government). To overcome this key limitation, recently, scholars have begun employing more rigorous quasi-experimental research designs, including family fixed effects and difference-in-differences. For example, Weinschenk and Dawes (2022) compared voter turnout among siblings who were differentially exposed to civic education but had common, shared family backgrounds using family fixed-effects. Furthermore, Holbein and Hillygus (2020, Chapter 5) estimated the causal effect of civic education on youth turnout using state-level variation in AP civic-related courses enrollments (e.g., course enrollments in AP US History or AP Government) and a difference-in-differences design. Concerningly, all of these rigorous studies found no effect of civic education on youth voter turnout.

While there has been a growing body of research on the effect of taking civic courses (Bell et al., 2022; Holbein & Hillygus, 2020; Weinschenk & Dawes, 2022), the effect of civics test policy on youth voter turnout remains largely unexplored. Campbell and Niemi (2014; 2016), a notable exception, explored the effect of state-level, high-stakes civics exams on civic knowledge and voter turnout. Using National Assessment of Educational Progress (NAEP) data and ordinal least squares (OLS) regression, Campbell and Niemi (2016) found that students who had taken civics tests showed higher political knowledge than those who did not. They also showed that this association was especially prominent among Latinx students. However, Campbell (2014) did not find any significant relationship between consequential civic assessment policies at the state-level and voter turnout.[1]

Despite a substantive contribution of prior work on the efficacy (or lack thereof) of civic education on youth voter turnout, the debate continues both in academic circles and popular culture (Vara, 2015). This study aims to fill this gap by leveraging variation in a recent state-level civics test policy, the Civics Education Initiative.

How would CEI affect young voter turnout?

In the resource model framework, which is one of the most prominent theories of voter participation, individuals' political participation is influenced by their political resources, including time, money, and cognitive abilities—verbal skills and political knowledge (Brady et al., 1995; Burns et al., 2001; Verba et al., 1993, 1995). These resources are hypothesized to promote political participation by lowering the costs of participation. In terms of youth voter participation, verbal skills and political knowledge, which are mostly acquired in school, have been considered to be the most crucial political resources (Brady et al., 1995). This framework

[1] We also replicate those null findings of Campbell & Niemi (2014) using voter turnout data from presidential elections and an alternative DD research design (see Appendix Table A5).

suggests that political knowledge gained through preparation for and participation in civics tests may boost voter participation by lessening the informational cost of voting. As a result of CEI, which requires students to pass the 100 basic civics-related questions, students may gain political knowledge of how the US government and politics work (Hess et al., 2015). And this political knowledge, in turn, may translate to higher voter participation, as envisaged by the CEI advocates (Civics Education Initiative, 2017).

On the other hand, "political knowledge" is a fairly broad construct. Some types of political knowledge—such as factual knowledge about political institutions, structures, and history—may not be relevant to voting at all. Indeed, some scholars have argued that the knowledge needed to vote might be more specific than general political knowledge (Boudreau, 2009; Cramer & Toff, 2017; Lupia, 2016). Empirical research also shows that an understanding of the current political and social debates as well as the mechanics of voting could more effectively reduce the cost of voting. Indeed, knowledge for the civics test, including "the name of the territory the United States purchased in 1803" and "the name of the longest rivers in the United States", may not be necessary for voting (Hess et al., 2015; Kahne, 2015; Levine, 2015; U.S. Citizenship and Immigration Service, 2022). In other words, the political knowledge gained in the likely preparation and performance of state-mandated civics tests by CEI may not necessarily reduce the informational cost of voting because such knowledge is not useful in voting contexts. In addition, considering the failure of traditional civic education, which has also often highlighted mastering facts about government and politics, in increasing voter turnout (Holbein & Hillygus, 2020; Weinschenk & Dawes, 2022), this knowledge-focused policy (i.e., CEI) may not be sufficient to improve youth turnout. Accordingly, there might be a null relationship between CEI and voter turnout.

Lastly, the implementation of CEI may negatively impact young voter turnout by limiting students' opportunities to develop political skills and attitudes that assist them in overcoming barriers to voting. In illustrating the limitation of memorizing facts, recent scholarship underscores the importance of practical experience and skills in becoming an empowered, active, and engaged citizen (Brezicha & Mitra, 2019; Holbein & Hillygus, 2020; Mitra & Serriere, 2012; Westheimer & Kahne, 2004). For example, Holbein and Hillygus (2020) showed that noncognitive skills, including perseverance, determination, and self-control, are crucial for following through on their intention to vote. Similarly, Cohodes and Feigenbaum (2021) showed that the gains in voter turnout among girls who attended high-performing charter schools in Boston were largely through the development of psychosocial skills. In light of the prior research emphasizing political skills, scholars and educators have cautioned that civic tests may narrow the content of civic education and turn it into a memory exercise (Brezicha & Mitra, 2019; Levine, 2015; Mitra & Serriere, 2012). Indeed, Kahne, Rodriguez, Smith, and Thiede (2000) found that when schools administer civic assessments, civic learning becomes confined to measured content. And teachers expressed that it is difficult to conduct engaging activities such as a mock election when they have standardized tests (Holbein & Hillygus, 2020). In sum, the civics test might encroach upon students' time for practical skills which meaningfully increase voter turnout.

Considering the debate surrounding the civics test policy, one would expect vibrant literature evaluating its effectiveness. Despite this, research on the policy is surprisingly scarce, and as a result, we know little about the impact of the civics test policy on youth turnout. This study makes a significant contribution to the literature on civic education policy by estimating the causal effect of state-level civics test policy on youth voter turnout for the first time.

Data and Method

Data

We use pooled cross-section data from the 1996-2020 Current Population Survey (CPS) – Voting and Registration Supplement (The Bureau of the Census for the Bureau of Labor Statistics, 2020). The CPS is a large nationally-representative survey interviewing approximately 54,000 households monthly and providing extensive information on the employment situation and demographic characteristics. The CPS's Voting and Registration Supplement is a supplement to the monthly CPS which is conducted every two years in November after elections. This survey covers both registered and non-registered individuals who are eligible to vote (i.e., U.S. citizens who are 18 years old or older) and is a key source of national information on civic engagement, including self-reported voter turnout, data in the US.

The CPS and Voting and Registration Supplement together contain information on voting participation, age, and residence. More importantly, it includes data on youth voter turnout before and after the adoption of CEI in several states. Specifically, we use age-specific, voter turnout data from the 2020 presidential election (survey conducted after the adoption of CEI in several states) as well as 6 other prior presidential elections from 1996-2016 to evaluate the efficacy of state-mandated CEI. In all, we use seven waves of the CPS data, each conducted after the presidential elections, to construct our analytical sample; 1996, 2000, 2004, 2008, 2012, 2016, and 2020.[2] Since some demographic variables (e.g., nativity and citizenship variables) are

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[2] We use voter turnout in presidential elections rather than counterpart in midterm elections. As some states implemented CEI at the beginning of the 2018-2019 school year, the 2018 midterm election was in the middle of the period of treatment. Therefore, midterm election cycles are less useful for distinguishing a post-implementation period from pre-implementation periods. By contrast, the 2016 and 2020 presidential elections barely overlapped with the period of treatment. For example, Arizona State, which first passed CEI in 2015, implemented it in the 2016-2017 school year. Since some senior students were 18 years old, they had the right to vote in the 2016 election. Considering that the 2016 presidential election was in November 2016, however, it is less likely that students who voted for the election took the civics test for graduation. Because of such complexity, we consider all 18-22 year old

not available in the years before 1994, waves before 1994 are excluded from the analysis. In addition, we limit the analytical sample to young US citizens (18-22 years old) to capture the population who were most likely to be exposed to CEI. There is not a clear age threshold that is appropriate for the analysis since the age of voters exposed to the policy varies by state. For example, in Arizona, where the policy was implemented in the 2016-2017 school year, voters aged 18-22 years in 2020 were exposed to treatment. However, in Arkansas, where the policy was implemented in the 2018-2019 school year, only people aged 18-20 in 2020 were likely treated. To maximize statistical power, we restricted our analytical sample to include 18-22 year olds for our core results.[3]

Measures

Voter turnout. The outcome variable is self-reported voting of young people aged 18-22 in presidential elections from 1996 to 2020 (voted = 1, not voted = 0).[4]

Treatment. As described earlier, our key treatment variable indicates whether the respondent's state had adopted CEI or not in each wave between 1996-2020. We also distinguish between states that adopted a *strong* civics test requirement—high school graduation is conditional on passing the civics test—and states that adopted a *weak* civics test requirement—graduation is not conditional on it—with separate indicators.

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voters in Arizona as "treated" only in 2020 in our analysis. We include our exact coding schema, legislation names, and dates for treatment and comparison states in Table 1.

[3] Results are robust to alternative age thresholds such as 18-20 (See Table 4). We also carry out additional placebo checks with alternative samples restricted to citizens aged 28-32 years (See Table 5) and a more rigorous triple-difference models estimating changes in voter turnout between young adults (18-22) and older adults (28-32 years) pre- and post-CEI (See Table A1). We return to these results in the next section.

[4] Due to social desirability response bias, self-reports in surveys have often overestimated voter turnout (Highton, 2005; Holbrook & Krosnick, 2010). To be specific, people who did not vote tend to answer that they voted because they want to appear to be responsible citizens. To prevent this social desirability response bias, the CPS mentioned the following in the questionnaire: "People are not able to vote because they are sick or busy or have some other reason." However, self-reported turnout still overestimates true turnout rates. Despite the limitation of a self-reported measurement, using a self-reported measure to explore changes in turnout levels over time is relatively reliable (Highton, 2005; Katosh & Traugott, 1981; Sigelman, 1982). Since this study aims to explore the change in voter turnouts rather than voter turnout itself, using self-reported measurement is less problematic.

Demographics. We include demographic characteristics such as sex, race/ethnicity, educational attainment, family income, immigrant background, marital status, metropolis residence, and employment status.

Time-varying State Variables. Finally, we also include state-level time-varying covariates that may have differentially impacted voter turnout across states. These covariates include the proportion of people who are White, Black, Hispanic, married, separated/divorced/widowed, unemployed, graduated high school, registered for the election, metropolis residence, and have immigrant backgrounds, as well as the median household income and Democratic-to-Republican vote share ratio.[5]

The final analytic sample size is approximately 5,000 respondents across seven periods and a total of 36,627 respondent-wave observations. To be specific, the final sample size is 36,627 for the *omnibus* treatment group, 32,452 for the *strong* civics test treatment group, and 31,897 for the *weak* civics test treatment group.

Analytic plan

We implement difference-in-differences (DD) and event study analyses leveraging the variation in state-mandated adoption of CEI across states in the country.

First, in our core analysis, we use a two-way fixed effects model with the following specification:

$$Y_{ist} = \beta_0 + \beta_1 civics\_test\_policy\_implement_{st} + \gamma_s + \lambda_t + \eta X_{ist} + \delta Z_{st} + \varepsilon_{ist} \quad (1)$$

Where Y_{ist} is whether the individual i in a State s reported voting (*1 = Yes, 0 = No*) at each period t. The $civics\_test\_policy\_implement_{st}$ is a binary variable equal to 1 if an individual i lives in a

[5] Democratic-to-Republican vote share information is from the Federal Election Commission (2022), and others are from CPS.

State s that adopted the policy during period t, and zero otherwise. Therefore, β_1 represents the effect of civics test policy implementation. γ_s is a set of state fixed effects, and λ_t is a set of fixed effects for the period. X_{ist} is a vector of individual-level covariates and Z_{st} is a vector of state-level time-varying covariates that may have differentially impacted the voter turnout across states described above.

In alternative specifications, we also add φ_t, a linear time index (to control for a general linear trend in voter turnout across both treatment and comparison states), and $\gamma_s\varphi_t$, a set of interactions between states and the linear time index to capture the state-specific, linear trends. In other words, each state is allowed to have a unique trajectory in voter turnout, which relaxes the parallel trends assumption (Wing et al., 2018).

Even though there is temporal variation in CEI adoption, we do not have the staggered treatment adoption issues highlighted in the recent DD literature (Goodman-Bacon, 2021; Roth et al., 2022) because of the way we have constructed our analytical sample. Our analytical sample includes individuals clustered in states, followed up every 4 years (our panel spans 4 consecutive years within each time period). Essentially, we have 1 post-treatment time period and 6 pre-treatment time periods. All treatment adoptions are thus collapsed to the 4-year level.

Second, we conduct an event study analysis, that has now become standard (Angrist & Pischke, 2009), to identify the effect of CEI and examine the pre-treatment parallel tredns assumption—in the absence of the civics test policy, the voter turnout rates in treatment states and comparison states would have evolved in the same manner as it did in the pre-policy period. Specifically, we include a set of indicator variables (leads and lags) h_{ist}^{k} to represent whether CEI policy adoption occurred in state s at time $t-k$, for all integers k from $-k_0$ to k_0 representing the number of time periods relative to CEI policy adoption. Let the set K include all integers from k to $-k_0$except for -1 (that is, we normalize the coefficient to the time period before the CEI policy adoption), the regression model is:

$$Y_{ist} = \sum_{k \in K} \theta_k h_{ist}^k + \gamma_s + \lambda_t + \eta X_{ist} + \delta Z_{st} + \epsilon_{ist} \ (2)$$

Essentially, k represents a period of four consecutive years as opposed to a single year to preserve statistical power in the analysis. This study examines the trends in voter turnout four, eight, 12, 16, 20 and 24 years before CEI policy adoption as well as, four years after policy adoption. We later present event study graphs that visualize estimates of θ_k, with coefficients normalized to the time period prior to the CEI policy adoption. We also report results from joint F-tests where the null hypothesis is that coefficient estimates of all corresponding pre-CEI implementation periods are jointly equal to zero.

For all models, we cluster the standard errors at the state level to account for the nested characteristics of data and the level of treatment (i.e., citizens within states) and account for heteroscedasticity. Also, to generalize the result from the sample to the target population, we use the basic CPS weight variable which was created by the Census survey team to adjust the potential bias from unequal selection probability and nonresponse (The Bureau of the Census for the Bureau of Labor Statistics, 2020).

Results

First, we present descriptive statistics of the key measures from our analytical sample (separated by treatment status) in Table 2.

[Table 2]

We find noticeable differences in demographic and socioeconomic characteristics, including race/ethnicity, immigrant background, and family income. However, voter participation is similar across four group categories. We explore these trends further below.

[Figure 1]

Figure 1A-C show the trends of turnout among young voters (18-22 year olds) in treatment states relative to comparison states from 1996 to 2020. First, we compared all states that implemented any type of CEI (i.e., either *strong* or *weak* requirement states) to comparison states (Figure 1A). Next, we split treatment states into two groups (i.e., states with a strong requirement and states with a weak requirement). Figures 1B and 1C show trajectories of turnout in each group, respectively. The vertical line represents mid-2016 when Arizona, the first state to adopt CEI, is shown. Soon several states followed (See Table 1 for detailed information). Visually, all three figures show parallel trends in voter turnout among 18-22 year old voters prior to CEI (August 2016) giving us confidence in our identification strategy. We include the event study graphs and more rigorous tests of parallel pre-treatment trends in the "robustness" checks section.

Difference-in-Differences Results

[Table 3]

Table 3 reports the DD estimates of the effect of CEI on young voter turnout. First, we estimated the effect of the omnibus treatment (models 1-5 in Table 3). Model 1 only includes state- and year- fixed effects, model 2 includes individual covariates, and model 3 includes both individual and state-level time-varying covariates. In models 4 and 5, we added the general linear trend control and state-specific linear trend control, respectively. We found that the omnibus CEI treatment did not have a significant detectable effect on young voter turnout across all models with different sets of covariates. While the DD coefficient is 0.004 in the first model without any covariates, it became negative once state-specific linear trend control was entered in model 5 (b = -0.012). However, neither was statistically significant at $p < .05$.

The results separated by two treatment arms (states with a strong and weak civics test requirements in comparison with no CEI states) are presented in models 6-10 and 11-15, respectively. Regardless of an intensity of requirement, CEI implementation does not significantly impact young voter turnout.

Event Study Results

We present the results of event studies for each treatment in Figure 2.

[Figure 2]

Figure 2-A shows the effect of CEI (the omnibus treatment) on young voter turnout. Consistent with our DD results, the 95% confidence intervals include zero in the post- period, which implies that there were no statistically significant effects of CEI on voter turnout. Similarly, Figure 2-B and 2-C, which show the effects of strong and weak civics test requirement respectively, document the null effect of CEI regardless of an intensity of requirement.

In addition, in support of the parallel trends assumption, 95% confidence intervals of coefficients of all corresponding pre-CEI implementation periods except the 2008 period for the strong civics test treatment (Figure 2-B) include zero. Furthermore, we failed to reject the null hypothesis that there are no significant differences between the treatments and comparison states before CEI implementation (F=1.20, P-value=0.32 for the omnibus treatment; F=1.87, P-value=0.13 for the strong civics test treatment; and F=2.07, P-value=0.10 for the weak civics test treatment). In addition,

Heterogenous effects of CEI

[Table 4]

Since Campbell and Niemi (2016) found heterogeneous effects of civics test policy on civic knowledge, particularly among Hispanic students, we further examined whether the effect of CEI differs across demographic characteristics, including race/ethnicity, gender, and immigrant background. In addition, to examine whether results are robust to alternative age thresholds such as 18-20, we also explored heterogeneous effects across age. To economize on space, we only present heterogeneous effects of the omnibus treatment (all other results are fairly consistent and available on request).

Across the board, we continue to find statistically insignificant null results. Concerningly, we found that Black young voters in states with CEI showed a 9.7 percentage point lower predicted probability of voting compared to Black young voters in comparison states, holding other covariates constant. Given the marginal significance level ($p<.10$), we interpret this result with caution; but going forward, research must continue to monitor heterogenous effects by race/ethnicity given mixed theoretical and empirical evidence from prior literature.

Robustness Checks

In this section, we conduct four additional analyses to examine the robustness of our results—(1) placebo group checks; (2) triple-differences or DDD analysis; (3) alternative comparison group analyses to account for pre-existing civic education/assessment policy differences across states; and (4) analyses with additional state-level time-varying covariate—a cost of voting index—to account for election law and policy differences across states and periods.

First, we conduct similar DD and event study analyses (following the same model specification (1) and (2)) for a placebo group sample (28-32 years old). The DD estimates of CEI effects on the placebo group are presented in Table 5. Models 1-5 show that there was no statistically significant effect of the omnibus CEI treatment on the voter turnout of the placebo group.[6]

[Table 5]

Second, in addition to placebo tests on 28-32 years old citizens (a group that is unlikely to have been exposed to the CEI treatment in schools), we also conducted a triple differences (i.e., difference-in-differences-in-differences [DDD]) analysis. The DDD design helps to remove the potential bias from state-level time-varying confounders that change differentially across states (Wing et al., 2018).[7] We report these results in Table A1 in the Appendix. Similar to the DD analysis, we added individual-level covariates, state-level time-varying covariates, the general linear trend control, and state-specific linear trend control, one at a time in successive models. Consistent with the findings from the DD design, our DDD estimates show the null effect of CEI on young voter turnout (P<.05).

Third, we included two alternative comparison groups in our analysis to test the sensitivity of our core results. The canonical DD research designs compare treated units with not-yet-treated or control units before and after treatment implementation to estimate the causal

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[6] Models 6-10 also show that the placebo group in the states with a strong civics test policy did not show significantly different voter turnout from the placebo group in the comparison states. For states with a weak civics test policy, we found a negative effect on voter turnout of the placebo group. Model 11 shows that 28-32 years old citizens in the states with a weak civics test policy showed lower voter turnout than counterparts in the comparison states. However, once state-specific linear trend control was included in model 15, the coefficient became smaller and statistically not significant. The results of event studies for the placebo group are presented in Figure A1. In support of the parallel trends assumption, we failed to reject the null hypothesis that there are no significant differences between the treatments and comparison states before CEI implementation (F=1.49, P-value=0.22 for the omnibus treatment; F=1.49, P-value=0.22 for the pass treatment; and F=1.62, P-value=0.18 for the test-only treatment).

[7] For the DDD analysis, we introduce 28-32 years old citizens to our model as a new within-state comparison group. Instead of the $civics\_test\_policy\_implement_{st}$ variable in equation (1), the DDD equation includes the group dummy (age $18\text{-}22_{ij}$), the treatment state dummy (state $treatment_{sj}$), the post-treatment dummy ($post_{tj}$), and all possible interactions across them. The parameter of interest is β_7 which estimates the effect of the civics test policy.

$$Y_{istj} = \beta_0 + \beta_1 age\ 18-22_{ij} + \beta_2 state\ treatment_{sj} + \beta_3 post_{tj} + \beta_4(age\ 18-22 * state\ treatment)_{isj} + \beta_5(age\ 18-22 * post)_{itj} + \beta_6(state\ treatment * post)_{stj} + \beta_7(age\ 18-22 * state\ treatment * post)_{istj} + \gamma_s + \lambda_t + \eta X_{ist} + \delta Z_{st} + \varphi_t + \gamma_s\varphi_t + \varepsilon_{istj} \quad (3)$$

effects of the treatment. In other words, DD models rely on making "clean" comparisons between treated and control units. While all the states in our baseline comparison group did not technically adopt CEI prior to 2020 and are thus "clean" comparison groups per se, some of them had other strong civic course curricula/assessments in high schools even in the absence of CEI. To disentangle that likely confound, we define two alternative comparison groups. First, using state-level data on enrollment in AP US History and AP Government (a proxy for other civic education initiatives), we exclude states, which had AP US History or AP Government enrollment in the upper quartile in 2015, from our comparison group.

Table 6 shows a positive and marginally significant effect of CEI (P<.10). To be specific, 18-22 years old citizens in states that implemented CEI show an average 36 percentage point increase in the probability of voting compared to those in states without CEI and relatively high enrollment in AP US History/Government. However, this effect dissipates once state-specific linear trend control was included in the model. Second, we excluded states, which had a state-mandated civics assessment and course policy in 2012, from our comparison group, using information from Campbell and Niemi (2016). The results, presented in Table 7, show that there was a negative but not statistically significant effect of CEI on young voter turnout. Taken together, the findings show that requiring a mandatory, civics test exam for high school graduation—when compared to states with no strong civic test requirements—does not affect voter turnout rates.

[Tables 6 and 7]

Lastly, we included a cost of voting index (Schraufnagel et al., 2022) in our model as a state-level time-varying covariate to account for state voting laws. Voting laws are known as an important factor in voter turnout—ease of voting policies increases voter turnout (Grumbach &

Hill, 2022; Holbein & Hillygus, 2016; Yu, 2019). Therefore, our DD estimates could be biased if state voting laws changed between 2016 and 2020, or if CEI adoption was endogenous to state laws. We report results of DD models with the voting index in Table A6 in the Appendix. Table A6 shows a positive and marginally significant effect of CEI ($P<.10$). To be specific, 18-22 years old citizens in states that implemented strong CEI show an average 46 percentage point increase in the probability of voting compared to those in states without CEI. However, this effect dissipates once state-specific linear trend control was included in the model. In sum, consistent with the findings from the main DD in Table 3, the results show the null effect of CEI on young voter turnout ($P<.05$).

Limitations

It is important to note that our estimates are "intent-to-treat" (ITT) estimates. CEI effect could be underestimated because young voters who were likely not exposed to the CEI policy could have been included in the treatment group due to data limitations. First, the age-based inclusion criteria (18-22 years old) used to create the analytical sample likely includes untreated participants due to diversity in the timing of the policy implementation across states. For example, citizens aged 21-22 in 2020 in Arkansas, who are classified as belonging to a treatment group, graduated high school before the policy was implemented, which indicates that they were likely not exposed to treatment. This may dilute the effect of the policy. Second, due to exemption rules for student groups, treatment groups might include students who were not exposed to the civics test even in states that adopted the policy. For example, Nevada exempts some English learners and students doing Individualized Education Programs (IEP) from the civics test requirement (Brennan & Railey, 2017). Therefore, those students should indeed be categorized into the untreated group. Even though English learners and students with IEPs do not account for a large proportion of the total population, including them in the treatment group may result in an underestimation of the policy effect. In sum, owing to this noise in the treatment group, the estimated effect could be smaller than the "true" policy effect. Therefore, this should be considered a conservative estimate (i.e., ITT) of the civics test policy's effect on voter turnout.

Second, the CPS does not have information on people's residences during high school; thus migration across states from high school days to adulthood cannot be estimated very clearly. Given that some high school students move to other states where their colleges are located, it is likely that distinguishing between the treatment group and comparison group based on residence at the time of survey interviews is somewhat imperfect. That said, past research shows that the vast majority of college students who were registered to vote did so in their hometowns (Niemi & Hanmer, 2010). Given the unprecedented shutdown of college campuses amidst COVID-19 in 2020, it is likely that college students voted in their hometowns (or used mail-in-ballots), which further bolsters confidence in our decision to use state of residence at the time of surveys as a proxy for treatment/control exposure as well as outcome analysis.[8] We, therefore, do not believe that this data limitation biases our results substantively.

Third, this study does not account for differences in treatment “dosage” (beyond simplistic categorizations of strong vs. weak policy adoptions) or fidelity of implementation. To be specific, since several states customized CEI, details of policy differ by state. For example, each state had slightly different passage standards (e.g., 60 percent in Wisconsin vs. 70 percent in Tennessee). In addition, some states, such as Arizona and Arkansas, allow students to retake

[8] Nevertheless, we carried out another robustness check to examine if residential moves bias our results given our data limitation. Essentially, we replicated the same DD model specification (1) with a restricted sample where respondents who have changed their address within 12 months as of the time of survey interviews were dropped in order to minimize the treatment-comparison status confounds due to migration across states. The DD estimates of CEI effects on this restricted sample are presented in Table A2. Models 1-12 show that there was no statistically significant effect of the CEI treatment on voter turnout of 18-22 years old citizens who had not changed addresses within 12 months as of the time of survey interviews, which is consistent with our main DD results.

the test to pass it. While our study provides an average ITT effect of policy adoption, additional nuance on policy implementation might reveal heterogeneity that future research could tackle.

Lastly, even though we used a difference-in-differences design to isolate the effect of CEI, we acknowledge the potential bias in estimations likely to occur due to time-varying omitted variables at multiple levels. Specifically, if there were omitted, time-varying variables that are correlated with youth voter turnout and whether a state adopted CEI, our estimates could be biased.

Discussion and Conclusion

This study is one of the first studies to examine the causal effect of a state-mandated civics test policy adoption on youth voter turnout. Using nationally representative data of young voters from 1996 to 2020 and difference-in-differences and event study designs, we found no detectable effect of CEI on young voter turnout. Indeed, in terms of magnitude of effects, the difference in the probability of voting between young citizens in states which implemented CEI and those that did not was a statistically insignificant 1.5 percentage points at most ($p<0.05$). This effect is smaller than the effect of interventions providing specific and detailed information on the voting process. Recent experimental studies, which examined the effect of voting information on youth voter turnout (e.g., how to register to vote), found at least three times larger effects (Bennion & Nickerson, 2016; Bergan et al., 2021). Furthermore, students who participated in voting-related activities at a high school, including a get out the vote (GOTV) campaign and visiting elected officials, showed about 7.2 percentage points higher rates of voter turnout than comparison students (Gill et al., 2018). Similarly, Cohodes and Feigenbaum (2021) show that broad-based curricula that promotes non-cognitive skills in high-performing charter schools improved not just young voter turnout among girls by 6 percentage points but also spilled over to their parents. In sum, as Holbein and Hillygus (2020, Chapter 5) point out, applied political learning seems more effective than memorizing general facts about government in terms of increasing youth voter turnouts.

Even when comparing the turnout among young citizens in states that newly adopted CEI with states that had historically low rates of civic coursework participation or low state civic education requirements prior to CEI, we find fairly insignificant null effects across the board. Our results provide further suggestive evidence regarding the persistent gaps between civic knowledge and engagement—at least when operationalized as voter turnout. Even though prior research suggests that a civics test policy might increase youth's political knowledge (D. E. Campbell & Niemi, 2016), it does not seem to increase voter turnout. Unsurprisingly, this finding is consistent with prior rigorous research that found no significant relationship between taking civic courses and voter turnout (Holbein & Hillygus, 2020; Weinschenk & Dawes, 2022).

In addition, we examined heterogeneous effects of CEI based on demographic characteristics, including race, ethnicity, gender, and immigrant background. Although heterogeneous effects were not observed in most subgroups, we found a marginally significant, 9.7 percentage point decrease in the probability of voting among Black youth in states which implemented CEI although the impact was only marginally significant ($p < 0.1$). While prior studies have documented *stronger* associations between civic education and political outcomes for students from historically marginalized groups, including people of color and low socioeconomic status (D. E. Campbell, 2008; Langton & Jennings, 1968), we find a concerning pattern in the opposite direction here that future research must continue to dig deeper into. As discussed in a literature review section, some scholars have warned of a narrowing of civic education—spending more time memorizing facts and less time engaging in class discussions and activities—caused by the civics test policy. This loss might be particularly detrimental to some students who might not have many opportunities outside of school to experience political discussions or activities. In light of prior research, our finding implies that the negative impact of civics test policies may be particularly prominent among some groups, such as Black people, who have historically been excluded from politics (Conway, 2000; Holbrook et al., 2016; Leighley & Vedlitz, 1999; Verba et al., 1993). Although we interpret this result with caution given limited statistical power, future research should continue to examine ways to broaden civic participation in the US.

Civic education efforts in schools, while often well-intentioned, struggle to move the needle when it comes to consequential civic participation among youth. It is particularly true when it is traditional civic education that emphasizes increasing students' political knowledge, such as CEI. A change of focus of civic education from civic knowledge testing/fact-based assessments to more practical information provision interventions that rely on providing information on the voting process and the development of noncognitive skills (Holbein & Hillygus, 2020), might directly help reduce the cost of voting. That said, we are glad to note that the CEI policy also did not adversely impact high school graduation rates (see Appendix Tables A3 and A4).[9]

Despite several limitations, our analysis which uses national data, a rigorous quasi-experimental research design, and several robustness checks—including the use of state- and time-fixed effects, state-specific linear trends, event study analyses, triple differences, placebo

[9] To examine whether CEI policy impacts high school students' graduation, we compared students in states with CEI and without CEI before and after CEI using two-way fixed effects difference-in-difference (TWFE). For this purpose, we used the sample of 16-19 years old in the US from the Current Population Survey (CPS) March survey from 1996-2020. For covariates, we included sex, race, ethnicity (individual-level), percentage of Black high school students, Hispanic high school students, state median income, and state poverty rate (state-level time-varying). State poverty rate information is from the UKCPR National Welfare Data (2022), and others are from CPS.

checks, comparisons with other civic test coursework variation across states, and accounting for state voting laws—documents the stubborn unresponsiveness of youth voter turnout to state-mandated CEI in the US. If states hope to improve civic participation among successive generations of citizen leaders, they need to do a lot more (or a lot different) than just mandate a civic test policy aimed at testing civic and political knowledge for high school graduation.

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Table 1 Timing of the Civics Education Initiative by group

Omnibus Treatment: Civics Test								Comparison group			
Treatment 1: Strong Civics Test				Treatment 2: Weak Civics Test							
State	Legislation Name	Passed	Effective	State	Legislation Name	Passed	Effective	Alaska	Illinois	Nebraska	Rhode Island
Arizona	HB 2064	2015	2016-2017 School year	Alabama	SB 32	2017	2018-2019 School year	California	Indiana	New Jersey	South Dakota
Arkansas	HB 1539	2017	2018-2019 School year	Louisiana	SB 283	2015	2016-2017 School year	Colorado	Iowa	New Mexico	Texas
Idaho	SB 1071	2015	2016-2017 School year	Minnesota	HF 1497	2016	2017-2018 School year	Connecticut	Kansas	New York	Vermont
Kentucky	SB 159	2017	July, 2018	South Carolina	S 437	2015	2016-2017 school year (ninth grade)	Delaware	Maine	North Carolina	Virginia
Missouri	HB 1646	2016	July 1st, 2017 (ninth grade)	Tennessee	HB 0010	2015	January 1st, 2017	District of Columbia	Maryland	Ohio	Washington
Nevada	SB 322	2017	July 1st, 2019	West Virginia	HB 3080	2017	2018-2019 School year	Florida	Massachusetts	Oklahoma	Wyoming
North Dakota	HB 1987	2015	2016-2017 School year	Wisconsin	SB 21	2015	2016-2017 School year	Georgia	Michigan	Oregon	
Utah	SB 0060	2015	January 1st, 2016					Hawaii	Mississippi	Pennsylvania	

Note. We exclude Montana (legislation name: SB 242) from the analyses because it adopted the Civics Education Initiative but does not require a state-level test. We also exclude New Hampshire (legislation name: SB 157) from the analyses because it uses a locally developed competency assessment, which could be different from the USCIS naturalization civics test. We classify Pennsylvania (HB 564) as a comparison group because the bill was implemented after the analyzed periods (i.e., effective for the 2020-2021 school year). In Utah, the policy was implemented in January 2016, shortly before the 2016 presidential election. Despite this, we include Utah in our analysis. Results are robust to the exclusion of Utah. To make our coding scheme accurate, we read the legislation of CEI in each state. We then contacted each state's department of education to obtain further information on whether and how CEI-related civics test policies are implemented in each state, and whether CEI has replaced existing civics requirement policies or been added. Source: Brennan & Railey (2017) and Brezicha & Mitra (2019)

Table 2 Descriptive Statistics (age 18-22)

	All States		All treated states (Omnibus)		States with strong requirement		States with weak requirement		Comparison States	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Outcome										
Voter participation	0.502	0.500	0.506	0.500	0.482	0.500	0.533	0.499	0.500	0.500
Individual Characteristics										
Female	0.510	0.500	0.512	0.500	0.514	0.500	0.510	0.500	0.509	0.500
Race										
White	0.803	0.398	0.836	0.370	0.877	0.329	0.790	0.407	0.792	0.406
Black	0.121	0.326	0.114	0.317	0.063	0.243	0.171	0.376	0.124	0.329
Asian or pacific	0.034	0.182	0.018	0.131	0.018	0.134	0.017	0.128	0.039	0.195
American indian/aleut/eskimo	0.016	0.125	0.015	0.120	0.021	0.142	0.008	0.090	0.016	0.127
Multi-race	0.026	0.159	0.018	0.133	0.022	0.145	0.014	0.118	0.028	0.166
Hispanic Origin	0.135	0.342	0.075	0.264	0.112	0.316	0.033	0.179	0.155	0.362
Educational attainment										
Below high school	0.166	0.372	0.170	0.375	0.168	0.374	0.171	0.377	0.165	0.371
High school diploma	0.341	0.474	0.368	0.482	0.377	0.485	0.358	0.479	0.333	0.471
Some college/associate	0.459	0.498	0.436	0.496	0.430	0.495	0.442	0.497	0.466	0.499
Bachelor's degree	0.033	0.178	0.026	0.159	0.024	0.154	0.028	0.165	0.035	0.183
Graduate degree	0.001	0.031	0.001	0.024	0.000	0.021	0.001	0.027	0.001	0.033
Family income	424.145	259.810	399.986	256.303	391.210	253.917	409.928	258.650	431.905	260.457
Immigrant background	0.157	0.364	0.080	0.271	0.101	0.301	0.056	0.229	0.182	0.386
Marital status										
married, spouse present	0.065	0.246	0.088	0.283	0.114	0.318	0.058	0.233	0.057	0.233
married, spouse absent	0.005	0.068	0.004	0.062	0.004	0.066	0.003	0.056	0.005	0.070
separated	0.008	0.089	0.009	0.096	0.011	0.102	0.008	0.087	0.008	0.087
divorced	0.004	0.064	0.006	0.077	0.007	0.081	0.005	0.072	0.004	0.060
widowed	0.001	0.026	0.001	0.024	0.001	0.025	0.000	0.022	0.001	0.027
never married/single	0.918	0.275	0.893	0.309	0.864	0.343	0.926	0.262	0.926	0.262
Metropolis residence	0.784	0.411	0.707	0.455	0.710	0.454	0.703	0.457	0.809	0.393
Unemployed	0.078	0.269	0.076	0.266	0.075	0.264	0.078	0.268	0.079	0.270
Time-varing State Characteristics										
% of White	0.802	0.118	0.842	0.094	0.879	0.059	0.800	0.107	0.790	0.122
% of Black	0.116	0.098	0.108	0.100	0.060	0.053	0.162	0.113	0.119	0.097
% of Hispanic	0.131	0.125	0.079	0.087	0.114	0.106	0.040	0.023	0.148	0.130
% of graduated high school	0.547	0.063	0.534	0.065	0.533	0.059	0.535	0.071	0.551	0.061
% of people with immigrant backgrounds	0.207	0.136	0.115	0.081	0.145	0.097	0.082	0.037	0.236	0.137
% of married	0.528	0.042	0.543	0.040	0.553	0.041	0.531	0.035	0.523	0.042

% of separated/divorced/widowed	0.176	0.020	0.180	0.026	0.178	0.027	0.183	0.023	0.175	0.018
% of Metropolis residence	0.780	0.197	0.706	0.177	0.699	0.217	0.714	0.116	0.803	0.197
% of unemployed	0.035	0.011	0.032	0.009	0.032	0.010	0.033	0.008	0.036	0.011
% of registered for the election	0.815	0.052	0.810	0.064	0.793	0.066	0.829	0.055	0.816	0.048
Median household income	457.432	110.754	426.233	96.500	424.302	95.272	428.419	97.839	467.454	113.149
Democratic-to-Republican vote share ratio	1.077	1.273	0.773	0.236	0.726	0.231	0.826	0.230	1.174	1.444
N	36,627		8,905		4,730		4,175		27,722	

Note. Montana and New Hamphire, which were excluded from the main DD analyses, were also excluded from this descriptive statistics analysis

Fig 1-A

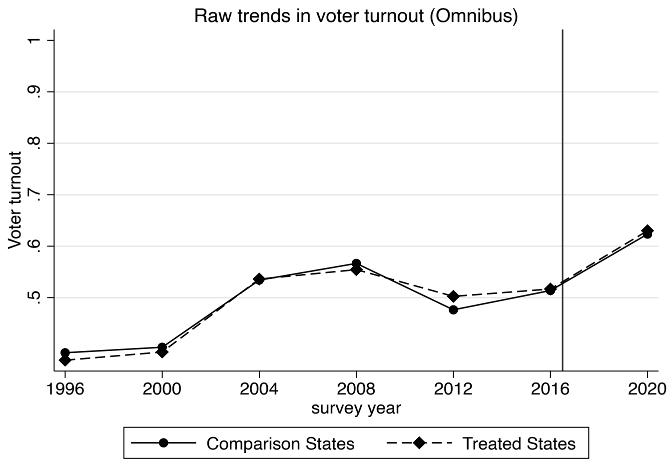

Fig 1-B

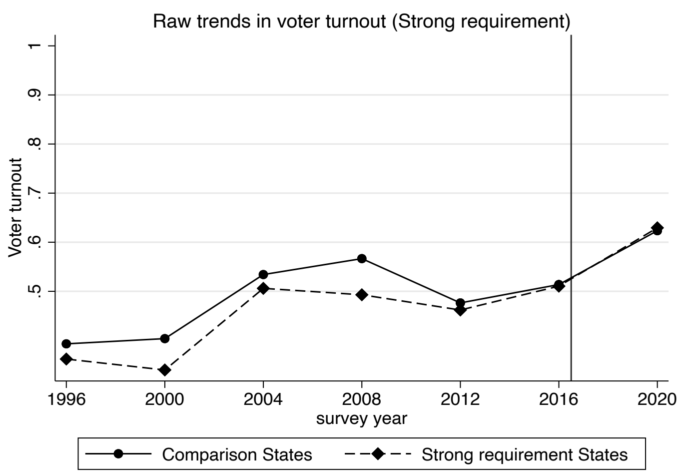

Fig 1-C

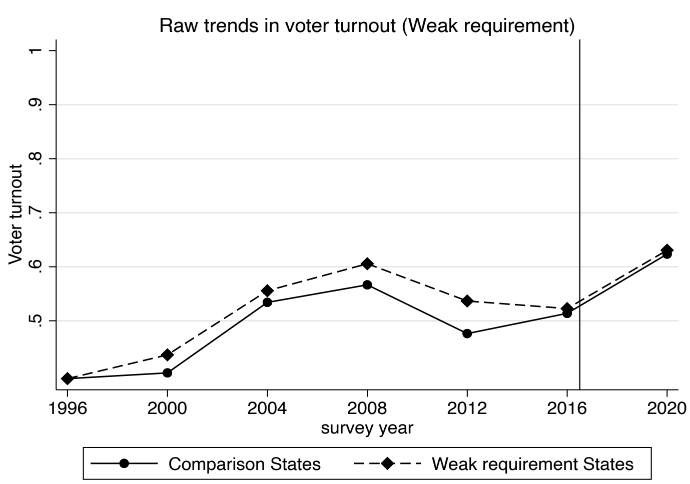

Figure 1. Raw trends in voter turnout among 18-22 years old citizens

Table 3 The Impact of the civics test policy on voter turnout among a treated group (18-22 years old citizens)

Treatment			Omnibus					Strong civics test requirement					Weak civics test requirement		
Model	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
DD estimate	0.004	-0.002	0.015	0.015	-0.012	0.04	0.024	0.018	0.018	-0.008	-0.023	-0.023	0.018	0.018	-0.009
(SE)	(0.023)	(0.022)	(0.019)	(0.019)	(0.024)	(0.025)	(0.024)	(0.032)	(0.032)	(0.033)	(0.025)	(0.024)	(0.018)	(0.018)	(0.028)
Individual Covariates		X	X	X	X		X	X	X	X		X	X	X	X
Time-varying State Covariates			X	X	X			X	X	X			X	X	X
General linear trend control				X	X				X	X				X	X
State-specific Linear trend control					X					X					X
N			36627					32452					31897		
R^2	0.038	0.128	0.132	0.132	0.134	0.035	0.125	0.129	0.129	0.132	0.037	0.128	0.132	0.132	0.134

Note: Robust standard errors, in parentheses, are clustered at the state level. State fixed effects and year fixed effects are included in all models. We report estimates of β_1 In Equation (1). ~ p < .10, * p < .05, ** p < .01,*** p < .001

Table 4 Heterogenous Impacts of the civics test policy on voter turnout among a treated group

	Panel A: Race/Ethnicity									
	White					Black				
Coef	0.018	0.013	0.031	0.031	0.011	-0.002	0.006	0.012	0.012	-0.097~
(SE)	(0.017)	(0.016)	(0.019)	(0.019)	(0.025)	(0.062)	(0.054)	(0.048)	(0.048)	(0.054)
R^2	0.041	0.132	0.135	0.135	0.137	0.06	0.158	0.168	0.168	0.179
N			29410					4435		
	Asian					Hispanic Origin				
Coef	0.047	-0.019	0.051	0.051	0.029	-0.040	-0.022	-0.038	-0.038	-0.051
(SE)	(0.102)	(0.118)	(0.101)	(0.101)	(0.133)	(0.069)	(0.067)	(0.063)	(0.063)	(0.103)
R^2	0.136	0.195	0.211	0.211	0.248	0.051	0.134	0.142	0.142	0.148
N			1251					4961		
	Panel B: Sex									
	Female					Male				
Coef	0.005	0.002	0.022	0.022	-0.002	0.006	-0.009	0.004	0.004	-0.022
(SE)	(0.028)	(0.028)	(0.024)	(0.024)	(0.032)	(0.027)	(0.025)	(0.023)	(0.023)	(0.030)
R^2	0.043	0.132	0.136	0.136	0.140	0.036	0.123	0.127	0.127	0.131
N			18667					17960		
	Panel C: Immigrant Status									
	people with immigrant background					people without immigrant background				
Coef	-0.035	-0.018	-0.011	-0.011	0.000	0.012	0.002	0.014	0.014	-0.012
(SE)	(0.048)	(0.046)	(0.047)	(0.047)	(0.084)	(0.022)	(0.022)	(0.018)	(0.018)	(0.023)
R^2	0.048	0.13	0.138	0.138	0.144	0.039	0.13	0.134	0.134	0.136
N			5745					30882		
	Panel D: Age									
	18-20 years old					21-22 years old				
Coef	-0.013	-0.019	-0.006	-0.006	-0.039	0.025	0.016	0.038	0.038	0.023
(SE)	(0.030)	(0.028)	(0.027)	(0.027)	(0.038)	(0.030)	(0.030)	(0.025)	(0.025)	(0.027)
R^2	0.037	0.12	0.123	0.123	0.127	0.042	0.143	0.148	0.148	0.153
N			22117					14510		
Individual Covariates		X	X	X	X		X	X	X	X
Time-varying State Covariates			X	X	X			X	X	X
General linear trend control				X	X				X	X
State-specific Linear trend control					X					X

Note: Robust standard errors, in parentheses, are clustered at the state level. State fixed effects and year fixed effects are included in all models. ~ p < .10, * p < .05, ** p < .01,*** p < .001

Fig 2-A Omnibus treatment

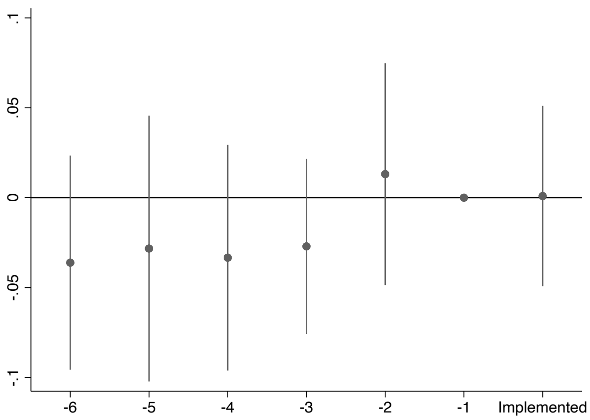

Fig 2-B Strong civics test treatment

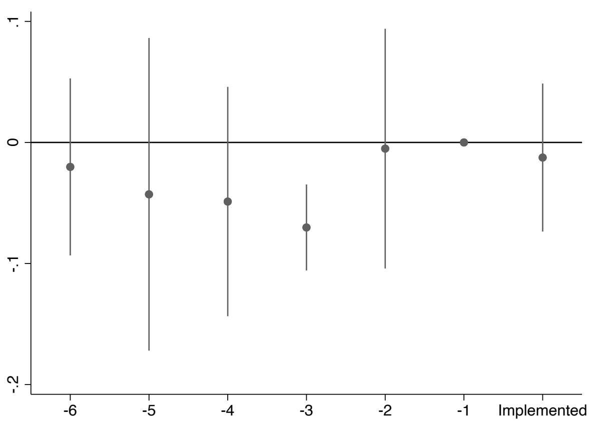

Fig 2-C Weak civics test treatment

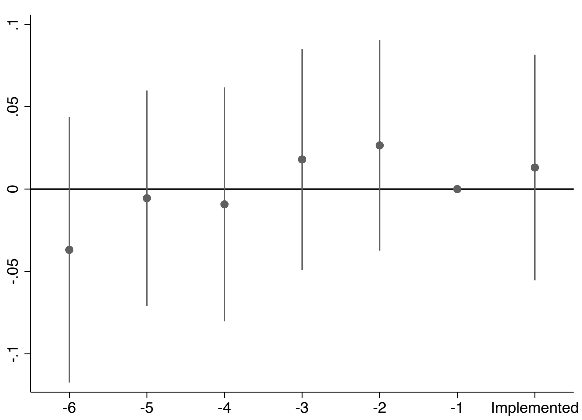

Figure 2 Event study graphs among a treated group (18-22 years old citizens). Notes: Each point represents the point estimate, and each bar represents the 95% confidence interval, calculated with standard errors clustered at the state level. The coefficient for the one time period before CEI is normalized to zero.

Table 5 The Impact of the civics test policy on voter turnout among a placebo group (28-32 years old citizens)

Treatment	Omnibus					Strong civics test requirement					Weak civics test requirement				
Model	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
DD estimate	-0.028	-0.031	-0.019	-0.019	0.005	0.021	0.014	0.015	0.015	0.022	-0.068*	-0.069***	-0.047*	-0.047*	-0.013
(SE)	(0.025)	(0.020)	(0.017)	(0.017)	(0.015)	(0.032)	(0.021)	(0.014)	(0.014)	(0.017)	(0.026)	(0.018)	(0.021)	(0.021)	(0.017)
Individual Covariates		X	X	X	X		X	X	X	X		X	X	X	X
Time-varying State Covariates			X	X	X			X	X	X			X	X	X
General linear trend control				X	X				X	X				X	X
State-specific Linear trend control					X					X					X
N			36627					32452					31897		
R^2	0.029	0.128	0.132	0.132	0.134	0.029	0.125	0.129	0.129	0.132	0.029	0.128	0.132	0.132	0.134

Note: Robust standard errors, in parentheses, are clustered at the state level. State fixed effects and year fixed effects are included in all models. ~ p < .10, * p < .05, ** p < .01,*** p < .001

Table 6 Difference-in-Differences Analyses without States with AP US History/Government Enrollment in Upper Quartile in 2015

Treatment	Omnibus				
Model	1	2	3	4	5
DD estimate	0.039	0.026	0.036~	0.036~	0.036
(SE)	(0.024)	(0.022)	(0.020)	(0.020)	(0.031)
Individual Covariates		X	X	X	X
Time-varying State Covariates			X	X	X
General linear trend control				X	X
State-specific Linear trend control					X
N			19906		
R^2	0.045	0.142	0.145	0.145	0.148

Note: Robust standard errors, in parentheses, are clustered at the state level. State fixed effects and year fixed effects are included in all models.
For this analyses, we dropped upper 25 % in either AP US history or AP government enrollment in 2015: california, connecticut, district of columbia, florida, georgia, illinois, maryland, massachusetts, michigan, new jersey, new york, north carolina, ohio, texas, virginia (total 15 states were excluded from the comparison group). ~ p < .10, * p < .05, ** p < .01,*** p < .001

Table 7 Difference-in-Differences Analyses without States with strong civic education requirement in 2012

Treatment	Omnibus				
Model	1	2	3	4	5
DD estimate	-0.033	-0.042~	-0.025	-0.025	-0.018
(SE)	(0.027)	(0.024)	(0.021)	(0.021)	(0.034)
Individual Covariates		X	X	X	X
Time-varying State Covariates			X	X	X
General linear trend control				X	X
State-specific Linear trend control					X
N			23402		
R^2	0.047	0.138	0.141	0.141	0.143

Note: Robust standard errors, in parentheses, are clustered at the state level. State fixed effects and year fixed effects are included in all models.
For this analyses, we dropped states with strong civic education policy (i.e., having state-mandated civic test and coursework) in 2012: California, Georgia, Indiana, Kansas, Michigan, Mississippi, New Mexico, New york, Ohio, Oklahoma, Texas, Virginia (total 12 states were excluded from the comparison group). ~ p < .10, * p < .05, ** p < .01,*** p < .001

Fig A1-A Omnibus treatment

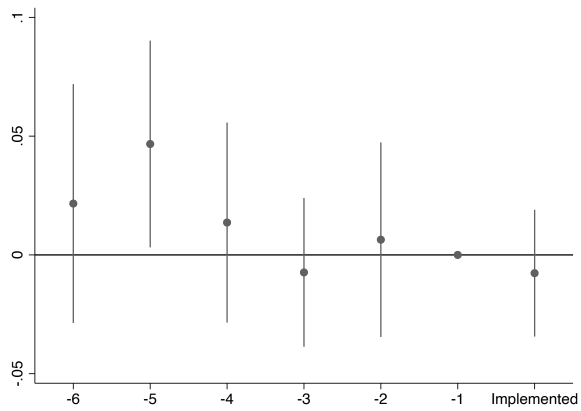

Fig A1-B Strong civics test treatment

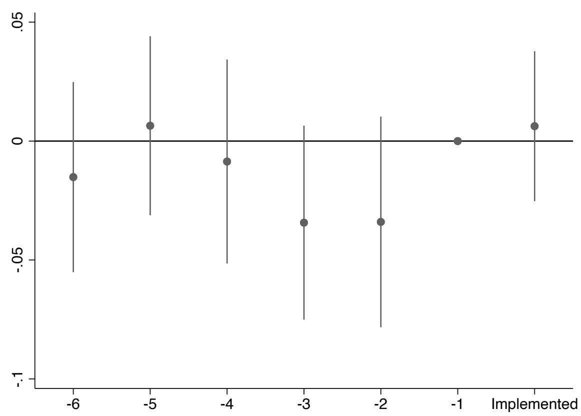

Fig A1-C Weak civics test treatment

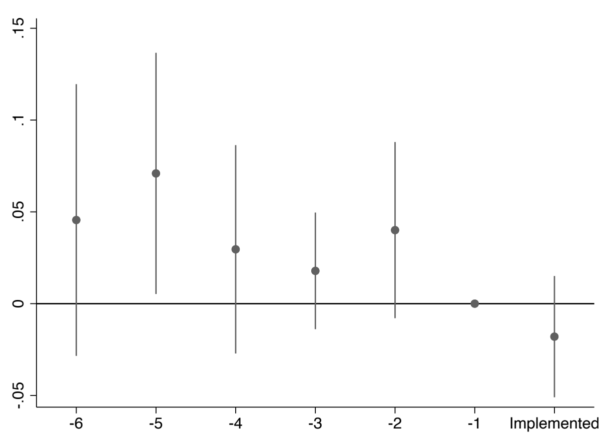

Figure A1 Event study graphs among a placebo group (28-32 years old citizens). Notes: Each point represents the point estimate, and each bar represents the 95% confidence interval, calculated with standard errors clustered at the state level. The coefficient for the one time period before CEI is normalized to zero.

Table A1 Difference-in-Difference-in-Differences (DDD) Estimates of the Impact of the civics test policy on voter turnout

Treatment	Omnibus					Strong civics test requirement					Weak civics test requirement				
Model	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
DDD estimate	0.035	0.036	0.035	0.035	0.037	0.021	0.017	0.015	0.015	0.015	0.049	0.054~	0.052~	0.052~	0.055~
(SE)	(0.029)	(0.023)	(0.023)	(0.023)	(0.024)	(0.035)	(0.029)	(0.029)	(0.029)	(0.030)	(0.040)	(0.030)	(0.030)	(0.030)	(0.031)
Individual Covariates		X	X	X	X		X	X	X	X		X	X	X	X
Time-varying State Covariates			X	X	X			X	X	X			X	X	X
General linear trend control				X	X				X	X				X	X
State-specific Linear trend control					X					X					X
N			76742					68170					66911		
R^2	0.048	0.157	0.159	0.159	0.16	0.047	0.155	0.157	0.157	0.158	0.049	0.156	0.158	0.158	0.16

Note: Robust standard errors, in parentheses, are clustered at the state level. State fixed effects and year fixed effects are included in all models. We report estimates of β7 in Equation (3). ~ $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$

Table A2 The Impact of the civics test policy on voter turnout among a treated group (18-22 years old citizens) after dropping cases that changed address within a year

Treatment	Omnibus					Strong civics test requirement					Weak civics test requirement				
Model	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
DD estimate	0.016	0.012	0.028	0.028	0.003	0.057*	0.045	0.034	0.034	0.004	-0.014	-0.013	0.030	0.03	0.008
(SE)	(0.029)	(0.028)	(0.023)	(0.023)	(0.027)	(0.028)	(0.027)	(0.034)	(0.034)	(0.033)	(0.037)	(0.034)	(0.030)	(0.030)	(0.038)
Individual Covariates		X	X	X	X		X	X	X	X		X	X	X	X
Time-varying State Covariates			X	X	X			X	X	X			X	X	X
General linear trend control				X	X				X	X				X	X
State-specific Linear trend control					X					X					X
N			26440					23379					23375		
R^2	0.033	0.117	0.121	0.121	0.124	0.03	0.115	0.118	0.118	0.121	0.032	0.117	0.121	0.121	0.123

Note: Robust standard errors, in parentheses, are clustered at the state level. State fixed effects and year fixed effects are included in all models. ~ p < .10, * p < .05, ** p < .01,*** p < .001

Table A3 The Impact of the civics test policy on High School Graduation among 18-19 years old

Treatment	Omnibus					Strong civics test requirement					Weak civics test requirement				
Model	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
DD estimate	-0.001	0.005	0.000	0.000	0.030	0.021	0.023	0.009	0.009	0.031	-0.014	-0.007	-0.004	-0.004	0.031
(SE)	(0.025)	(0.024)	(0.020)	(0.020)	(0.018)	(0.043)	(0.043)	(0.036)	(0.036)	(0.022)	(0.023)	(0.021)	(0.019)	(0.019)	(0.023)
Individual Covariates		X	X	X	X		X	X	X	X		X	X	X	X
Time-varying State Covariates			X	X	X			X	X	X			X	X	X
General linear trend control				X	X				X	X				X	X
State-specific Linear trend control					X					X					X
N			77100					68816					68004		
R^2	0.01	0.025	0.025	0.025	0.027	0.011	0.026	0.027	0.027	0.028	0.01	0.026	0.026	0.026	0.027

Note: Robust standard errors, in parentheses, are clustered at the state level. State fixed effects and year fixed effects are included in all models. The outcome (high school graduation) equals one if the individual have high school diploma and zero otherwise. We followed Urban's (2022) operationalization of the high school graduation.~ p < .10, * p < .05, ** p < .01,*** p < .001

Table A4 The Impact of the civics test policy on Being "On-Track" for High School Graduation among 18-19 years old

Treatment	Omnibus					Strong civics test requirement						Weak civics test requirement			
Model	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
DD estimate	0.005	0.004	0.004	0.004	0.005	0.006	0.006	0.006	0.006	0.002	0.006	0.004	0.003	0.003	0.010
(SE)	(0.014)	(0.013)	(0.013)	(0.013)	(0.013)	(0.015)	(0.014)	(0.014)	(0.014)	(0.006)	(0.021)	(0.020)	(0.020)	(0.020)	(0.021)
Individual Covariates		X	X	X	X		X	X	X	X		X	X	X	X
Time-varying State Covariates			X	X	X			X	X	X			X	X	X
General linear trend control				X	X				X	X				X	X
State-specific Linear trend control					X					X					X
N	128501					114836						113120			
R^2	0.013	0.026	0.026	0.026	0.027	0.012	0.025	0.025	0.025	0.026	0.014	0.026	0.026	0.026	0.027

Note: Robust standard errors, in parentheses, are clustered at the state level. State fixed effects and year fixed effects are included in all models. The outcome (On-track) equals one if the individual was in an on-track grade (e.g., at least 12th grade for 18 years old) for high school graduation and zero otherwise. We followed Urban's (2022) operationalization of the "On-Track." ~ $p < .10$, * $p < .05$, ** $p < .01$,*** $p < .001$

Table A5 The Impact of state-mandated civic education requirement policy on voter turnout among a treated group (18-22 years old citizens)

Treatment	Omnibus				
Model	1	2	3	4	5
DD estimate	-0.034	-0.032	-0.022	-0.022	-0.022
(SE)	(0.021)	(0.022)	(0.017)	(0.017)	(0.017)
Individual Covariates		X	X	X	X
Time-varying State Covariates			X	X	X
General linear trend control				X	X
State-specific Linear trend control					X
N			37830		
R^2	0.038	0.128	0.132	0.132	0.134

Note: Robust standard errors, in parentheses, are clustered at the state level. State fixed effects and year fixed effects are included in all models. ~ p < .10, * p < .05, ** p < .01,*** p < .001

Table A6 DD model accounting for state voting laws using a cost of voting index (COVI)

Treatment			Omnibus					Strong civics test requirement					Weak civics test requirement		
Panel A: Regular COVI															
DD estimate	0.004	0.004	0.015	0.015	-0.009	0.040	0.040	0.046~	0.046~	0.018	-0.023	-0.023	-0.008	-0.008	-0.031
(SE)	(0.023)	(0.023)	(0.020)	(0.020)	(0.030)	(0.025)	(0.025)	(0.025)	(0.025)	(0.036)	(0.025)	(0.025)	(0.021)	(0.021)	(0.034)
R^2	0.037	0.037	0.038	0.038	0.041	0.034	0.034	0.034	0.034	0.038	0.037	0.037	0.037	0.037	0.041
Panel B: Covid-19 COVI in 2020															
DD estimate	0.004	0.004	0.016	0.016	-0.008	0.040	0.040	0.047~	0.047~	0.018	-0.023	-0.023	-0.006	-0.006	-0.029
(SE)	(0.023)	(0.023)	(0.020)	(0.020)	(0.030)	(0.025)	(0.025)	(0.024)	(0.024)	(0.037)	(0.025)	(0.025)	(0.020)	(0.020)	(0.034)
R^2	0.037	0.037	0.038	0.038	0.041	0.034	0.034	0.034	0.034	0.038	0.037	0.037	0.037	0.037	0.041
N			36238					32063					31508		
Individual Covariates		X	X	X	X		X	X	X	X		X	X	X	X
Time-varying State Covariates			X	X	X			X	X	X			X	X	X
General linear trend control				X	X				X	X				X	X
State-specific Linear trend control					X					X					X

Note: Robust standard errors, in parentheses, are clustered at the state level. State fixed effects and year fixed effects are included in all models. A cost of voting index (Schraufnagel et al., 2022; COVI) from 1996 to 2022 is an index that measures the overall electoral climate in each state in each presidential election year, reflecting state election laws, such as preregistration law, voter ID laws, etc. In addition to regular COVI, Schraufnagel et al (2022) calculated covid-19 COVI for 2020. Therefore, we used both indexes. For the detailed information about COVI, please see Schraufnagel et al. (2022). As COVI does not have a voting index for the District of Columbia, samples from the District of Columbia are excluded from the analysis. Therefore, analytic sample sizes are different from other models. Panel A shows the results of models with regular COVI, and Panel B shows the results of models with covid-19 COVI in 2020.

~ p < .10, * p < .05, ** p < .01,*** p < .001