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A path towards citizenship: The effects of early college high schools on criminal convictions and voting

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ABSTRACT

Building on a growing literature showing that early college high schools substantially improve educational outcomes, we investigate possible spillover impacts of this intervention on civic outcomes in North Carolina, which has early colleges in most of its 100 counties. We present both lottery and observational impacts on voting and criminal convictions. Our results suggest a modest increase in voting during early adulthood of about 4–5 percent, though lottery estimates do not rule out a null effect. For criminal convictions, lottery estimates are imprecise due to very low conviction rates, but observational evidence suggests a moderate decrease in convictions. We additionally identify stronger impacts on voting and conviction outcomes for key student subgroups, particularly black males and economically-disadvantaged white students. These results suggest that scaling up the early college program can improve youth civic outcomes and help to close key civic and political participation gaps.

1. Introduction

Although education is increasingly viewed through the lens of its impacts on social and economic mobility, public education was also historically promoted for its potential to teach students to be responsible democratic citizens who positively contribute to civic life (Labaree, 1997). Though the focus on this function of schools has diminished, empirical literature continues to show strong associations between schooling and prosocial civic behavior, including reduced criminal activity and increased political and community involvement (e.g., Hout, 2012; Lochner, 2010; Oreopoulos and Salvanes, 2009; Schofer et al., 2021). This can result in substantial social benefits, such as cost savings from the decreased use of public services and stronger and more representative civic institutions (Anderson, 2007; Hout, 2012; Schofer et al., 2021).

However, young people tend to demonstrate less prosocial civic behavior than older adults, which can have far-reaching impacts on individual and social well-being. For example, criminal activity is greatest among late adolescents, but many who commit crime do so only during this period of their life (Farrington et al., 2013). Because criminal convictions may diminish long-run educational, economic, and political opportunities (Flanagan and Levine, 2010; Wakefield and Uggen, 2010), preventing youth crime may affect overall rates of crime as well as other social problems such as poverty and unemployment. Similarly, youth are consistently less likely

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to vote than older adults (Flanagan and Levine, 2010). Because voting is often viewed as a habitual activity, increasing rates of young adult voting may help to create lifelong political participants (Highton and Wolfinger, 2001; Plutzer, 2002) and lead to better political representation (Canes-Wrone, 2015). Finally, improving civic outcomes for youth from disadvantaged backgrounds in particular, who tend to be overrepresented in crime (Wakefield and Uggen, 2010) and underrepresented in voting (Flanagan and Levine, 2010; Plutzer, 2002), may help to decrease economic and political inequalities (Griffin, 2014; Lindh and McCall, 2020; Wakefield and Uggen, 2010).

Because states have limited resources with which to address social problems, it is important for policymakers to know whether an intervention focused in one domain (e.g., education) produces positive spillovers into another (e.g., civic outcomes). In the present study, we address this research need by examining the effects of a proven educational intervention implemented at scale in many states across the U.S. – the early college high school (ECHS) – on youth and young adult voting, voter registration, and criminal convictions. ECHSs are standalone high schools or programs that partner with local colleges to provide students with opportunities to enroll in college-level coursework while in high school. ECHSs intend for students to earn up to two years of college credit or an associate degree concurrently with their high school diploma, in either a four-of five-year program of study (Berger et al., 2010; Walk, 2020). Research supports the model's effectiveness on educational outcomes, especially college degree attainment (Crittenden Fuller, Lauen and Unlu, 2020; Edmunds et al., 2020; Edmunds et al., 2017; Haxton et al., 2016; Lauen et al., 2017; Song and Zeiser, 2019), as well as its cost-effectiveness based on these impacts (Atchinson et al., 2019).

However, no study has yet examined the effects of ECHSs on civic outcomes. The ECHS might improve these outcomes for several reasons. Most notably, ECHSs greatly increase educational attainment, which is a substantial predictor of both voting (Smets and Van Ham, 2013) and crime (Lochner, 2010). Additionally, ECHSs increase the time that young people spend in school and may help students to develop more relationships with prosocial peers and adults, which may also encourage civic behavior (Abrams et al., 2011; Campbell, 2013; Hoeben et al., 2016; Jacob and Lefgren, 2003; Luallen, 2006). Indeed, many other educational interventions, including some that have no effect on academic outcomes, have been shown to improve civic outcomes (e.g., Cullen et al., 2006; Holbein, 2017; Sondheimer and Green, 2010).

To identify the effects of the ECHS on civic outcomes, we conduct two primary analyses using data on all North Carolina public school students who entered ninth grade between 2005–06 and 2011–12. First, we utilize a sample of early colleges that conducted randomized admissions lotteries to estimate experimental impacts on voting, voter registration, and criminal convictions through young adulthood. Second, due to the limited number of ECHSs that held admissions lotteries, we supplement these experimental impacts with effect estimates calculated using propensity scores on the full population of all students in the state.

To preview, we find consistent evidence that students that attend early colleges are about 2–3.5 percent (1.5–2.5 percentage points) more likely to register to vote and 4 to 5 percent (2 percentage points) more likely to vote as young adults than their peers. However, the lottery study is not sufficiently powered to detect an effect of this size, so these results are only statistically significant in the higher-powered observational study. Rates of criminal conviction are low, making lottery study estimates for these outcomes imprecise, but point estimates are suggestive of declines in convictions in this sample. Observational estimates suggest statistically significant declines of about 35 percent in both misdemeanor and felony convictions (1.2 and 0.6 percentage points, respectively), and a sensitivity analysis shows that the direction of these results is robust to relatively large confounding.

Finally, we produce two additional analyses using the observational sample. First, in a subgroup analysis, we find that estimated effects are largest for students most at risk of experiencing adverse civic outcomes, particularly black males and economically-disadvantaged white students. Second, examining the timing of impacts, we find that effects are apparent at all points from year four through year seven after high school entry (i.e., approximately grade 12 through grade 15). This demonstrates that impacts begin while students are still in high school and are sustained through the immediate following years. For voting, we additionally identify a spike in the effect in year five (i.e., grade 13), a year in which many students become eligible to vote for the first time and a year in which many more ECHS students remain in school than their peers. This aligns with prior research that suggests that actively enrolled students are more likely to vote than those who have exited school (e.g., Highton and Wolfinger, 2001; Tenn, 2007; Zeglovits and Aichholzer, 2014).

This study thus provides evidence that an educational intervention designed to accelerate high-achieving but traditionally-underrepresented students through high school and college may also increase prosocial civic behavior among young people. In comparison to the large impact of the ECHS on college degree attainment, the overall impact on civic outcomes is modest, though effects appear to be stronger for students of more disadvantaged subgroups. This suggests that scaling up the ECHS program, with a continued focus on reaching traditionally-underrepresented students, may have long-term impacts not only on educational and economic opportunity, but also civic behavior and social and political equity.

2. Empirical and theoretical background

2.1. Early college high schools

Early colleges are designed with the goal of improving educational attainment for traditionally underrepresented students (Berger et al., 2010; Edmunds et al., 2017; Haxton et al., 2016; Lauen et al., 2017; Walk, 2020). Nearly all ECHSs are located on the campus of community colleges or universities to give students early exposure to college life and coursework. Students typically enroll in ECHSs in ninth grade, remain for four or five years, and engage in a rigorous college-preparatory curriculum. A key feature of the ECHS is that students are expected to dually-enroll in the partner college's courses, with the potential of earning up to two years of college credit or an associate degree for free by the time they graduate from high school. This combination of exposure, support, and early credit accumulation should improve educational outcomes for students, particularly those from backgrounds that lack strong connections to higher education.

Results from both observational and randomized lottery studies of early colleges have found positive effects of attendance on several academic outcomes. ECHSs have been shown to produce fairly small positive effects on high school achievement and attainment, with studies typically finding increases in high school test scores of no more than about 0.1 standard deviations (Berger et al., 2013; Crittenden Fuller et al., 2020; Lauen et al., 2017; Miller and Corritore, 2013) and less than 5 percentage point increases in high school graduation (Edmunds et al., 2017; Haxton et al., 2016; Lauen et al., 2017). However, the program produces large increases in college degree attainment, driven especially by associate degree completion. ECHS students are around 20–25 percentage points more likely to obtain associate degrees within 6 years of high school graduation (over baselines of about 5–10 percent) and are about 5–10 percentage points more likely to earn Bachelor's degrees on-time (over baselines of about 10–15 percent), though impacts on Bachelor's degrees diminish over time (Crittenden Fuller et al., 2020; Edmunds et al., 2020; Song and Zeiser, 2019). This literature also identifies positive effects of ECHSs on measures of student behavior and engagement in high school, such as fewer absences (Edmunds et al., 2013; Lauen et al., 2017), fewer suspensions (Edmunds et al., 2013), and self-reports of a stronger academic climate (e.g., more rigorous and relevant instruction) and better relationships with teachers (Edmunds et al., 2013; Haxton et al., 2016). However, no study has yet explored the effect of ECHSs on civic outcomes.

2.2. *Youth civic behavior*

We hypothesize that ECHSs additionally help students to develop a stronger capacity and willingness to engage with society as responsible citizens (Brighouse et al., 2017; Levine, 2007; Westheimer and Kahne, 2004). We observe this through two sets of behaviors: participation in the voting process and avoidance of crime (i.e., following the law).[1] Each behavior is necessary to the functioning of a democratic society, which depends on individuals to work collectively to construct, enforce, and adhere to social rules and institutions that serve the public interest and address public problems (Callan, 2016; Levine, 2007; Westheimer and Kahne, 2004). Each behavior also contributes to both individual and social flourishing by allowing individuals to more fully participate and be represented in social, economic, and political life (Brighouse et al., 2017; Flanagan and Levine, 2010; Levine, 2007).

However, although both behaviors represent characteristics of responsible citizenship, they also represent different levels of engagement that may require different skills and resources (Westheimer and Kahne, 2004). For example, access to basic economic opportunities and connections to prosocial influences may be sufficient to create relatively strong financial and social disincentives against criminal behavior (Agnew and Messner, 2015). On the other hand, political engagement may require more developed knowledge of history, civics, and economics; stronger reading and critical thinking skills; and time and financial resources to facilitate active and meaningful participation (Callan, 2016; Persson, 2015; Unlu, 2014). The study of each therefore offers more complete insight into the kind and degree of prosocial civic behavior that ECHSs may facilitate. Effects on the two domains may differ depending on the baseline skills and resources of ECHS students and the effectiveness of the ECHS in helping students to develop the tools and resources that encourage civic behavior.

The study of each is additionally important because young people tend to experience relatively poor outcomes in both domains, each of which carries potentially long-lasting harms to both individuals and society. Crime most often occurs during adolescence and young adulthood, with the peak of criminal activity occurring around the ages of 16–19 (DeLisi and Vaughn, 2016; Farrington et al., 2013; Shulman et al., 2013). Rates of convictions are especially high for those who are black, male, and from lower-income backgrounds (Pettit and Western, 2004; Wakefield and Uggen, 2010). While a small proportion of the population will become chronically involved in the justice system, many individuals commit crime only during these adolescent years, and may do so only once or a small number of times (Farrington et al., 2013). However, being involved with the justice system can leave a lasting mark on an individual's long-run opportunities. For example, those arrested during high school are much less likely to complete high school or higher education (Kirk and Sampson, 2013), and those who are incarcerated at any point may experience a direct decrease in employment opportunities due to employers screening out formerly-incarcerated individuals (Pager, 2003). There are also substantial social costs to crime and incarceration – in addition to psychological costs of victimization (Cohen and Farrington, 2021), the direct costs of incarceration amount to about $100 per day per individual in North Carolina, for example (North Carolina Department of Public Safety, 2017). Together, these statistics suggest that preventing crime during adolescence and early adulthood, especially among youth from marginalized subgroups, can have important effects on long-run individual and social well-being and equity.

As with crime, youth voting rates are historically and consistently much worse than the rates in the rest of the population, though the propensity to vote increases as individuals age and as their lives stabilize (Highton and Wolfinger, 2001; Flanagan and Levine,

[1] Although avoidance of criminal activity is not always considered a form of civic "engagement," criminal activity can prevent individuals from participating in civic and economic life (Flanagan and Levine, 2010). However, we note that certain criminal acts can function as acts of political resistance against unjustified states (Levine, 2007), and, more generally, that arrests and criminal convictions do not simply reflect a decision to rebel against social rules, but also reflect structural disadvantages and inequalities (Wakefield and Uggen, 2010). For this study, we assume that a reduction in criminal convictions would primarily be reflective of the ECHS providing students with skills, resources, and opportunities that allow and encourage them to pursue legal employment and avoid crimes that may harm others; and that, by avoiding criminal convictions, youth have more opportunity to fully participate in civic, social, and economic life, thus constituting both an individual and a public good. To acknowledge that crime avoidance does not represent the more active form of "engagement" that is often meant by "civic engagement" (e.g., Levine, 2007), but also that crime avoidance represents an important dimension of citizenship behavior (Westheimer and Kahne, 2004), throughout the text we primarily refer to our outcomes as "civic outcomes" or "civic behaviors."

2010; Plutzer, 2002). However, voting is also often viewed as habitual – those who vote once are much more likely to continue voting in subsequent elections, and those who do not are more likely to continue being non-voters (Plutzer, 2002). Because politicians are responsive to voters' interests (Canes-Wrone, 2015), lack of political participation could lead to young peoples' interests being systematically underrepresented. This may be especially true for non-white and lower-income individuals, who may have fewer resources to draw on to help them engage in voting (Flanagan and Levine, 2010; Plutzer, 2002), and who may have unique political issues of interest that higher-income and white voters and politicians might otherwise fail to address (Griffin, 2014; Lindh and McCall, 2020). Therefore, helping young people – especially of lower-income and non-white backgrounds – to begin voting may have long-lasting impacts on political representation.

Although ECHSs are not designed to improve youth civic outcomes specifically, we posit that the program may produce spillovers into this domain. In the next sections, we discuss two common factors that encourage civic behavior and that are also related to impacts the ECHS may have on its students: 1) education and associated economic opportunities; and 2) connections to prosocial peers and institutions.

2.3. Effects of education on civic behavior

Education is associated with the development of skills and knowledge that individuals can use to engage as active members of society. These may include trade-specific skills that allow individuals to specialize and participate in economic life; domain-specific knowledge in subjects such as civics, history, and economics, which allow individuals to more fully understand and engage with political debates; and general skills such as reading comprehension and critical thinking that can help individuals to follow political events and navigate participation in civic institutions (Callan, 2016; Lochner, 2010; Persson, 2015; Satz, 2007; Unlu, 2014).

Education is also associated with stronger socioeconomic opportunities and resources, which can further encourage civic engagement. Economic opportunity may particularly disincentivize delinquency by raising the financial opportunity costs of engaging in such behavior (Agnew and Messner, 2015; Lochner, 2010). Further, those with greater education are more likely to have access to well-paying and high-prestige employment, which can provide time, financial, and social or cultural resources that can be used to support more active participation in civic life (Campbell, 2013; Persson, 2015).

These theories are supported empirically by a wealth of observational, quasi-experimental, and experimental research that documents a link between both education and voting (e.g., Dee, 2004; Milligan et al., 2004; Smets and van Ham, 2013; Sondheimer and Green, 2010) and education and (avoidance of) crime (Amin et al., 2016; Bell et al., 2016; Lochner and Moretti, 2004; Oreopoulos and Salvanes, 2009; cf. Stephens Jr. and Yang, 2014). As such, the very large impacts of the ECHS on educational attainment might also result in large effects on civic behavior.

However, we identify two key caveats in this literature that raise uncertainty about this hypothesis. First, with respect to crime, most research has focused on the effects of additional years of secondary education; however, the ECHS has relatively little impact on high school achievement or attainment, but instead primarily increases college degree attainment. Second, with respect to voting, several recent studies suggest that the link between education and voting is spurious (Berinsky and Lenz, 2011; Denny and Doyle, 2008; Persson, 2015; Tenn, 2007) or that educational expansion primarily benefits students of disadvantaged backgrounds (Lindgren et al., 2019), suggesting that the perceived link between educational attainment and political behavior may be due to common causes such as cognitive skills or other social background factors. Thus, if the relationship between education and civic behavior is driven primarily by secondary educational attainment and achievement, the ECHS's large impacts on college degree attainment may not translate into large improvements in civic outcomes.

2.4. Effects of prosocial connections on civic behavior

Social networks can also provide incentives or disincentives to civic behavior. A prosocial and civically engaged social network can provide information about civic opportunities and current events, which can stimulate political interest and recruit individuals into participation (Campbell, 2013; Persson, 2015). A prosocial network may also offer general social rewards for engaging in civic processes and disapproval for disengagement or antisocial behavior, thus altering the social costs and benefits of engaging in civic behaviors (Abrams et al., 2011; Agnew and Messner, 2015; Campbell, 2013; Loughran et al., 2016).

As with education, a large body of empirical research documents a relationship between one's social network and their political engagement (Bond et al., 2012; Klofstad, 2007, 2010; Nickerson, 2008; Sinclair et al., 2012) or criminal activity, especially among youth (see Hoeben et al., 2016, for a comprehensive review). Research also shows that remaining connected to prosocial institutions, such as schools, can have a positive effect on civic behavior. For example, several studies show that students are more likely to engage in crime on atypical non-school days as compared to regular school days (Cuellar and Markowitz, 2015; Fischer and Argyle, 2018; Jacob and Lefgren, 2003; Luallen, 2006; Monahan et al., 2014), and many studies of young adult voting have found that actively enrolled students are more likely to vote than similar non-enrolled peers (Highton and Wolfinger, 2001; Smets and van Ham, 2013; Tenn, 2007; Zeglovitz and Aichholzer, 2014).

As reviewed above, the ECHS places students into small school environments with other academically-inclined peers, supports relatively strong student-teacher relationships, and increases time spent in school (both by reducing absences and suspensions and by increasing high school to college transition rates), suggesting that the ECHS may additionally improve civic outcomes by increasing students' prosocial connections. However, we do not know the degree to which the ECHS substantively affects the composition of students' closer personal relationships (e.g., classmates and friendships). If ECHS students would have primarily chosen to interact with other high-achieving and civically-engaged peers in their traditional schools, then the impact of this change in their broader school environment may be limited.

2.5. Subgroup effects

Finally, we hypothesize that the ECHS may have stronger effects on students from more disadvantaged backgrounds. Due to many historical factors (Rothstein, 2017), students from lower-income and non-white backgrounds are more likely to grow up in environments that provide them less access to educational and economic opportunities (Orfield et al., 2012; Owens 2010) and more exposure to crime (Hirschfield, 2008; Wakefield and Uggen, 2010). These students may therefore have less opportunity than their more advantaged peers to develop and acquire the kinds of skills and resources that support civic engagement. Further, negative experiences with public institutions – such as experiences with under-resourced public schools – can actively discourage engagement by leading students to feel that they hold a low political status (Bruch et al., 2010; Fine et al., 2004). The ECHS may therefore be more impactful for students from relatively disadvantaged backgrounds because it may provide them with skills, resources, and opportunities that more advantaged students more often obtain from their home or traditional school environments – that is, the ECHS may compensate for inequalities in students' backgrounds, including the quality of their prior experiences with public institutions (Campbell, 2008; Lochner, 2010; Neundorf et al., 2016; Raudenbush and Eschmann, 2015). Indeed, empirically, many studies of educational interventions show relatively stronger civic effects for students from lower-income and non-white backgrounds (Campbell, 2008; Deming, 2011; Holbein, 2017; Lindgren et al., 2019; Lochner and Moretti, 2004; Neundorf et al., 2016).

2.6. Overview of the current study

The aim of the present study is to estimate the total effect of being enrolled in an early college on civic outcomes. Our review of literature suggests several possible ways in which ECHS attendance may encourage prosocial civic behavior, including improving educational attainment and economic opportunity as well as connections to prosocial peers, adults, and institutions. Impacts may be stronger for students of more disadvantaged backgrounds, who may otherwise accumulate fewer supportive resources and experiences that help to overcome barriers to engagement. We also identify that preventing crime and encouraging voting at early ages is likely to have a sustaining effect: young people are especially unlikely to vote and therefore to be underrepresented, but those who vote once are likely to continue voting; similarly, those who engage in crime often do so only during adolescence, but this may have lifelong impacts on their educational, economic, and political opportunities.

As such, we specifically explore the following three research questions:

1. What are the effects of attending an ECHS on the likelihood of voting, registering to vote, and being convicted of a crime (felony or misdemeanor) through young adulthood?
2. How do effects vary across key student subgroups?
3. How do effects vary across key time points during adolescence and young adulthood?

3. Data and sample

The data for this study come from three main sources. First, the North Carolina Department of Public Instruction (NCDPI) provided individual-level administrative data on all public school students in North Carolina between 2005–06 and 2015–16. Second, we gathered voter registration and voting records for all federal, state, county, and municipal elections in North Carolina from the North Carolina Board of Elections (NCBOE) through November 2016.[2] Finally, we obtained criminal conviction records from the North Carolina Department of Public Safety (NCDPS) through August 2019. North Carolina is one of only two US states that tried all 16- and 17-year-olds in adult court for criminal offenses during our study period.[3] We merged data from these three sources using first name, last name, and birth date to create a unique longitudinal database that tracks individual students from middle school into early adulthood. The main limitation is that we are not able to track voting and crime outcomes outside of North Carolina or for individuals

[2] We collected NCBOE records using a Python programming script querying the website with first name, last name, and birth date of each student who appeared in the NCDPI data and was over the age of eighteen. Due to a change in the structure of the publicly available data, we obtained only a 40% random sample of voting data for the November 2016 general election. However, because this sample is random, and therefore representative, we include it.

[3] North Carolina was the last state to end this practice in 2019; see https://www.newsobserver.com/news/politics-government/article157219234.html.

who changed their first or last name after ninth grade.[4]

Our full sample includes seven cohorts of first-time ninth graders in North Carolina public schools from 2006 to 2012 (referred to by the spring of the academic year). To be included in the sample, a student must have appeared in public school in North Carolina in eighth and ninth grade in consecutive years. The final dataset consists of over 700,000 students, about 20,000 of whom attended one of 79 unique early college high schools.[5]

The lottery sample includes 3758 students, 2174 assigned to the treatment group and 1584 to control. The lotteries were conducted by the research team for a given school and year using the list of eighth grade applicants deemed eligible by the school administrators. The research team conducted stratified lotteries for some schools to accommodate their admission priorities (e.g., to admit a higher proportion of first-generation college-goers). The analyses adjust for all relevant aspects of the randomization design that led to unequal probabilities of being assigned to the treatment group by using analytic weights (see Edmunds and colleagues [2017, 2013] for more details on the design and implementation of the lotteries).

We explore four primary outcomes of interest, each measured as a binary indicator: whether students were ever convicted of a felony or of a misdemeanor as an adult by August 2019, and whether students had ever voted or registered to vote by the November 2016 general elections. All percentages for these outcomes are naturally higher among our earliest cohorts, who are observed for longer time frames. We take advantage of the opportunity to observe long-term outcomes for some cohorts (e.g., up to 13 years after the initial ninth grade year – approximately age 27 – for our earliest cohort in the case of criminal convictions), absorbing these panel-length differences with cohort fixed effects.

The treatment variable is an indicator equal to one if the student attended an early college high school during their initial ninth grade year. This specification of the treatment variable means that students who transfer out of an ECHS after ninth grade are included in the treatment group. If anything, this likely produces a more conservative estimate than would an estimate of only students who obtained a "full dose" of the early college – i.e., those who remained enrolled until they exited high school altogether.

To reduce confounding bias in our propensity score estimates, we controlled for the following pre-treatment covariates measured in middle school:

Demographics – We included indicators for gender, student race/ethnicity, economic disadvantage (ED; defined as certification for free or reduced-price meals), and being old for grade (defined as being at least 15 years old on September 1 of their ninth grade year). We also included an interaction of these latter two variables.

Achievement – We measured pretreatment achievement as the average of students' sixth through eighth grade math and reading standardized test scores, eighth grade science standardized test score, and an indicator for having passed Algebra I in middle school. We standardized all test score variables to have a mean of zero and standard deviation of one. For students who did not have data available for a particular grade, math and reading test scores were averaged across the grades for which data were available.

Academic classifications – We included indicators for school-designated classifications of limited English proficiency (LEP), disability, and gifted status (AIG).

Absences and mobility – We included a continuous measure of the students' average number of days absent from sixth through eighth grade as well as an indicator for whether the student ever changed schools during middle school ("mobility").

County characteristics – Finally, to account for the fact that early colleges may not appear representatively across the state, we included annual measures of county unemployment and median income, obtained from the American Community Survey (ACS), and crime rates, obtained from North Carolina's Office of State Budget and Management "Log into North Carolina" (LINC) database. These statistics were assigned to students based on the county in which their eighth grade school was located.[6]

[4] Within our data, we find little descriptive difference in the rates of exiting the state during high school or of enrolling in out-of-state colleges conditional on college enrollment. Additionally, rates of each are low – less than 4% of high school students receive an exit code indicating that they exited the state and less than 20% of college students enroll out-of-state. Though we cannot be certain of the geographic locations of former students, we acknowledge two competing possibilities: first, ECHS students are more likely to remain in school during their late teens and early 20s; this may result in us over-observing ECHS students in the short-run as comparison students finish school and may thus begin to exit the state for work or other reasons. However, second, ECHSs increase educational attainment, and higher educational attainment is associated with greater mobility (Ihrke et al., 2009); this may therefore result in us under-observing ECHS students in the long-run due to their higher education facilitating more mobility. We note that our primary estimates average across multiple cohorts observed through different stages of young adulthood and thus average across these potentially different attrition biases.

[5] Our sample excludes students who attended private school, were homeschooled, or attended school out-of-state in either grade, as well as those who could not be matched longitudinally due to data inconsistencies. By cohort, the percentages of students who were matched in both 8th and 9th grade were: 2006–81.7%; 2007–71.5%; 2008–77.8%; 2009–84.4%; 2010–87.4%; 2011–87.5%; 2012–87.7%. Match rates were higher in the later years of data due to improvements in key identification variables, though some percentage of the sample should not be expected to appear in both years for the reasons described.

[6] For ACS data, we used five-year estimates when available, and three- or one-year estimates otherwise. Approximately 65 percent of counties had no ACS data for 2006, and about 20 percent had no ACS data in 2007 or 2008. We imputed these missing values using a regression of the missing variable on county and year indicators. For crime rates, three counties – Gates, Graham, and Hyde – did not have crime data available. These counties account for 73 ECHS students and 1993 non-ECHS students in our sample and were excluded. One remaining county, Pamlico, was missing data for 2006 only. This data was imputed using the mean of the county's 2005 and 2007 crime indexes. Finally, we note that 541 students attended a school assigned to the Departments of Juvenile Justice, Health and Human Services, or Prisons. We were able to assign counties based on a secondary eighth grade school, prior middle school, or ninth grade school for 364 of these students, with the remaining 177 being unable to be matched and dropped from the sample.

Finally, we note that because we use administrative records, data are missing at low rates (usually less than five percent); however, two variables have larger missing shares – Algebra I course-taking (about 33 percent missing in the earliest cohort, but decreasing to about 6 percent missing for later cohorts) and science test scores (unavailable prior to the 2009 cohort). In our main specifications, we address missing data through a dummy variable adjustment, setting missing values to zero and including an indicator for whether that variable is missing. We assess sensitivity to this decision by running a complete case analysis, as well as by running both a complete case analysis and dummy variable adjustment on the 2009 to 2012 cohorts, where data are mostly complete. Results do not substantively vary across these specifications (available on request).

4. Methods

We rely on two methods to estimate the causal effect of attending an early college on civic outcomes. First, we report experimental impacts from a lottery sample using data from nineteen early colleges (44 unique school-year cohorts) that conducted admissions lotteries between 2006 and 2011. Second, we report propensity score weighted impacts on the full sample of students in all early college sites (389 unique school-year cohorts) between 2006 and 2012.

These two methods produce complementary tradeoffs. In particular, the propensity score analysis is more precise and able to detect smaller effect sizes, but also relies on stronger assumptions to interpret its estimates as unbiased; the RCT is less likely to produce biased estimates, but cannot detect small or very precise effects. Ongoing research shows that these lottery and non-lottery ECHSs have produced similar impacts on educational outcomes (Crittenden Fuller et al., 2020; Unlu et al., 2021), suggesting that each method in this study, if unbiased, might also be expected to estimate similar effects on civic outcomes. Thus, the presence of consistent estimation across both samples would allow for stronger inferences about the direction and magnitude of the effects.

4.1. Lottery study (RCT)

Our first method uses available data on lottery winners and losers among 19 early colleges that held admissions lotteries between 2006 and 2011. Descriptive statistics of this sample, which are similar to the full sample of ECHS students (discussed in more detail below), are provided in Table 1. We calculate balance between the treatment and control groups for each covariate using the standardized difference, measured as the difference between the weighted treatment and control sample means divided by the square root of the average of the two variances: $(\widehat{x}_T - \widehat{x}_C)/\left(\sqrt{\frac{s_T^2+s_C^2}{2}}\right)$ (Austin, 2009; Rosenbaum and Rubin, 1985). We multiply this result by 100 to report the difference as a percent of the pooled standard deviation. We would characterize a covariate as unbalanced if its standardized difference was greater than 10 percent. However, we find no standardized difference greater than 7 percent.[7]

We estimated RCT impacts with logistic regression models, with standard errors clustered at the ninth grade school level. The analyses were conducted within the intent-to-treat (ITT) framework using the initial random assignment indicator as the primary predictor given the high compliance rate with random assignment (85% in the treatment group, 97% in the control group).[8] We report all effects in terms of adjusted risk ratios, which are equal to the likelihood of the outcome in the treatment group divided by the likelihood of the outcome in the control group (Norton et al., 2013).[9] A risk ratio of 1.0 indicates no effect. A risk ratio of 0.9 indicates that the outcome occurs with about 10 percent lower probability in the treatment group than in the control group (i.e., it is 90 percent as likely); a risk ratio of 1.1 indicates that the outcome occurs with about 10 percent higher probability in the treatment group (i.e., it is 110 percent as likely).

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[7] The four variables with the highest standardized differences (white, multi-racial, economic disadvantage, and disability status) show significant differences at $p < .05$ when measured by a *t*-test. However, these differences in means are small in absolute value. Following Austin (2009) and Imai et al. (2008), we prefer the standardized difference metric as a measure of balance because the standardized difference is a characteristic of the sample that is not dependent on sample size, whereas the *t*-test is an inferential statistic that is influenced by sample size – that is, the *t*-test becomes more likely to reject the null (and thus indicate "imbalance") as sample size grows. However, we note that results are essentially unchanged when running RCT models with or without covariates (see footnote 8).

[8] For simplicity of interpretation and because we observe baseline balance, we report estimates based on models without covariates (models still include design weights accounting for the probability of being selected into treatment from the lottery). Logit estimation with rare outcomes can be biased, especially when including many covariates relative to the number of positive events (Leitgob, 2020). With 26 covariates in our full model and less than 2 percent of the sample experiencing criminal convictions, our number of positive events per variable in the crime models falls well below minimum recommended thresholds of about 5–10 (Leitgob, 2020). Therefore, we prefer the simple model. However, when we include student-level covariates, results change very little in magnitude or precision and are completely unchanged in terms of statistical significance. We note that estimates for misdemeanors move in direction from slightly negative when unadjusted to essentially zero with covariate adjustment, though non-significant in either case.

[9] Specifically, with covariates, the predicted probability of experiencing the outcome is defined as $P_1 = \frac{1}{N}\sum_{i=1}^{N}\Pr(y_i = 1|X, x = 1)$ and the predicted probability of not experiencing the outcome is defined as $P_0 = \frac{1}{N}\sum_{i=1}^{N}\Pr(y_i = 1|X, x = 0)$. Each is computed over the full sample with the treatment variable (x) set to 1 or 0, respectively, and all other covariates held at their original values, X. Because we use logit, $\Pr(y_i = 1) = 1/(1 + e^{-x\beta})$. The ARR is equal to P_1/P_0 (Norton et al., 2013).

Table 1
Descriptive statistics of ECHS lottery winners and losers.

	ECHS		
	Lottery winners	Lottery losers	Std Diff (%)
Male	0.40	0.39	0.7
White	0.58	0.62	7.0
Black	0.28	0.26	5.3
Hispanic	0.08	0.08	0.2
Asian	0.01	0.01	1.5
American Indian	0.00	0.01	5.5
Multi-racial	0.04	0.03	6.6
Econ. Disadvantaged	0.51	0.47	7.8
Limited English proficiency	0.03	0.03	0.9
Disability	0.04	0.06	6.5
AIG	0.21	0.21	1.3
MS Mobility	0.23	0.24	3.3
Old for grade	0.12	0.13	3.7
Old * Econ. Disadvantaged	0.08	0.08	0.6
MS Reading score average	0.31	0.33	2.0
	(0.75)	(0.74)	
MS Math score average	0.25	0.28	4.2
	(0.76)	(0.75)	
8th grade science score	0.18	0.21	4.0
	(0.78)	(0.79)	
Passed Algebra	0.22	0.24	5.8
MS Absences average	6.53	6.84	5.7
	(5.25)	(5.67)	
Observations	2174	1584	

Note. Means (standard deviations) of control variables, excluding cohort and missing indicators. Sample includes students in 9th grade in North Carolina public high schools between 2005–06 and 2010–11 who were also observed in a North Carolina public school in 8th grade in the previous year, and who entered or applied to a North Carolina early college high school via an admissions lottery. Standardized difference calculated as the difference between ECHS and non-ECHS means divided by the square root of the average of the variances (times 100).

4.2. *Propensity score weighting*

Our second method utilizes a propensity score analysis to estimate treatment impacts on the full sample of ECHS students. The propensity score, $\widehat{p}_i$, is generated by running a logistic regression of the treatment on our full sample of pre-treatment covariates. We use the propensity score to generate an average treatment on the treated (ATT) weight, which is the most theoretically relevant given that ECHSs target a specific population of students, and is also most comparable to RCT estimates given the high level of compliance in the RCT. Specifically, all treated units receive a weight of one, while comparison units are assigned a weight equal to $\widehat{p}_i / (1 - \widehat{p}_i)$ (Stuart, 2010). We specify our outcome model as a logistic regression of our outcome on the treatment indicator and all pretreatment covariates (i.e., doubly-robust), weighted by the ATT weight, with standard errors clustered by the ninth grade school (Stuart, 2010).

Table 2 presents descriptive statistics for ECHS attenders and non-attenders in our sample. Prior to weighting, ECHS students are more likely to be female (61 percent versus 49 percent) and economically-disadvantaged (51 percent versus 46 percent) and are generally higher-achieving than their non-ECHS peers. We also notice imbalance on several other covariates – in all, 13 of the 22 unweighted covariates in Table 2 display imbalance as indicated by a standardized difference greater than 10 percent. Table 2 also presents weighted descriptive statistics and standardized differences for the propensity weighted samples. After adjustment, we find no standardized difference greater than 1.4 percent. Thus, the weighted comparison group resembles the treatment group on all observable characteristics.

4.3. *Sensitivity analysis*

Although propensity score weighting creates balance on observed covariates, it remains susceptible to bias from omitted confounds. The propensity score analysis assumes selection-on-observables, that is, that a student's potential outcomes are independent of treatment status after conditioning on the covariates that we observe (Imbens and Wooldridge, 2009). This would be violated if, after weighting and controlling for observed covariates, students in the treatment group still have a better potential outcome under control than the non-treated group – that is, if students who attended an ECHS would have been less likely to be convicted of a crime or more likely to vote even if they had attended the same school as comparison students.

In addition to comparing the propensity score results to RCT estimates, we assess the sensitivity of our propensity score estimates to possible confounding through a sensitivity analysis that examines how these estimates would change in response to omitted confounds of varying strength (Rosenbaum and Rubin, 1985). Specifically, we indicate what our point estimates would be after assuming varying risk ratio relationships between the treatment and a hypothetical confounder set and the outcome and the same confounder set. Because we interpret our results in risk ratios, we calculate bias according to the formula provided by VanderWeele and Ding (2017): $Bias = RR_{ud} * RR_{eu} / (RR_{ud} + RR_{eu} - 1)$, where RR_{ud} refers to the risk ratio between the confounder set and treatment and RR_{eu} refers to the risk ratio between the confounder set and the outcome. We then adjust our point estimates by this bias term by dividing our observed risk ratios by the calculated bias (or multiplying in the case that the observed risk ratio is below 1; VanderWeele & Ding [2017]). Additionally, we compute e-values, which represent the values that treatment- and outcome-confounder risk ratios would each need to take on to reduce the true effect to a risk ratio of 1.0, calculated as: $E = RR + \sqrt{RR^{*}(RR-1)}$ (Mathur et al., 2018; VanderWeele and Ding, 2017).

5. Results

5.1. Main results

The main results of the RCT lottery model and the full sample propensity model are displayed in Table 3 (for the propensity score models, results without covariate adjustment are shown in Appendix Table A1, and complete results with all covariates are provided in Appendix Table A2). We begin with results on voting and registration (the top two sets of estimates). Column 1 displays results in our RCT sample. RCT point estimates indicate a 4.2 percent increase for voting (rising from a 49.2 to 51.2 percent likelihood) and a 3.5 percent increase for registration (rising from 74.0 to 76.5 percent) for those who won an early college lottery. However, these estimates are not significantly different from zero. Column 2 displays the full sample propensity-weighted results, which produce similar but more precise results due to the much larger sample size. Net of controls, attending an ECHS is associated with a statistically significant 5.4 percent increase in the likelihood of having ever voted (rising from 48.6 to 51.2 percent) and a statistically significant 1.9 percent increase in the likelihood of having registered to vote (rising from 72.0 to 73.4 percent).

We next examine results on felony and misdemeanor convictions (the bottom two sets of estimates in Table 3). In column 1, the RCT results are again non-significant, with point estimates showing a 23 percent decline in the likelihood of felony convictions (from a 1.4 to a 1.1 percent probability) and a 5 percent decrease in misdemeanor convictions (from a 1.9 to a 1.8 percent probability). However, because so few students experience a conviction, these estimates are very imprecise. For example, in the case of felonies, the 95% confidence interval ranges from a 67 percent decrease to a 79 percent increase. In column 2, the higher-powered propensity-weighted results show more precise estimates indicating statistically significant declines in each conviction outcome – a 34 percent decrease in the likelihood of felony convictions (from a 1.7 percent to 1.1 percent probability) and a 35 percent decline for misdemeanor convictions (from a 3.4 percent to 2.2 percent probability).

As discussed above, the lottery and observational studies provide complementary tradeoffs – the lottery study is less likely to be biased, but is imprecise; whereas the observational study is more likely to contain bias, but is more precise. The consistency of the point estimates for voting and registration across the two methods is informative, providing evidence that there may be a causal impact of ECHS attendance on these outcomes, but one that is too small to be distinguished from the null in the moderately-sized lottery study. By contrast, the lottery study produces imprecise estimates of criminal conviction outcomes, such that for these outcomes we must rely more on the observational study and its assumptions. While we cannot improve the efficiency of the lottery estimates, in the next sections we assess susceptibility to bias in the observational estimates by conducting a set of robustness checks and a sensitivity analysis of these results.

5.2. Robustness checks

In this section, we perform two robustness checks that utilize additional covariates available in a limited number of cohorts to examine how our observational estimates may be affected by the omission of these covariates in the full sample. Specifically, in our first two cohorts (2006 and 2007), we are able to incorporate information on parent education (coded as less than high school; high school; some college; and BA or more), which provides a more precise measure of socioeconomic status than does ED status alone. In our last two cohorts (2011 and 2012), we are able to incorporate information on eighth grade suspensions (coded as an indicator of whether the student was suspended), which provides information related to students' in-school behavior. To determine how much the omission of these covariates in the full sample might be biasing our results, we produce estimates on these sub-samples that first exclude and then include the additional covariate.

Table 4 provides these results. The first two columns show adjusted risk ratios for the 2006 and 2007 cohorts, first without and then with the parent education variable. Point estimates in these cohorts are similar to, but slightly weaker than, the full sample. More importantly, we find that the inclusion of the parent education variable produces almost no change in the estimated adjusted risk ratios for any outcome in these cohorts (i.e., no risk ratio changes by more than about 6 percent of its original value). Thus, the omission of a more precise measure of socioeconomic status does not appear to be a likely source of bias in our full sample propensity-weighted results.

Columns 3 and 4 show the results for the 2011 and 2012 cohorts, first without and then with the indicator of eighth grade suspensions. Estimates in these samples are slightly larger to begin with than in the full sample when using only the baseline set of covariates, and all results are statistically significant. More importantly, including the suspension variable produces a substantive change to the estimated effects in these cohorts, though all results remain statistically significant. Specifically, the estimate on felony convictions is reduced to about 78% of its original size (from a 42% decrease to a 33% decrease); the estimate on misdemeanor convictions is reduced to about 85% of its original size (from a 44% decrease to a 37% decrease); the estimate on voting is reduced to about 91% of its original size (from a 7.7% increase to a 7% increase); and the estimate on registration is reduced to about 90% of its original size (from a 2.1% increase to a 1.9% increase).

Table 2
Descriptive statistics of full sample ECHS and non-ECHS students.

	Unadjusted			PS ATT Weighted		
	ECHS	Non-ECHS	Std diff	ECHS	Non-ECHS	Std diff
Male	0.39	0.51	23.7	0.39	0.39	0.0
White	0.56	0.56	0.1	0.56	0.56	0.2
Black	0.26	0.29	7.5	0.26	0.26	0.5
Hispanic	0.10	0.09	6.5	0.10	0.10	1.0
Asian	0.03	0.02	6.6	0.03	0.03	0.2
American Indian	0.02	0.02	0.2	0.02	0.02	0.1
Multi-racial	0.03	0.03	2.4	0.03	0.03	0.1
Econ. Disadvantaged	0.51	0.46	10.5	0.51	0.50	1.4
Limited English proficiency	0.04	0.05	5.6	0.04	0.04	0.8
Disability	0.05	0.13	29.4	0.05	0.05	0.3
AIG	0.21	0.16	12.6	0.21	0.21	0.6
MS Mobility	0.23	0.21	5.0	0.23	0.22	0.8
Old for grade	0.12	0.20	22.2	0.12	0.12	0.6
Old * Econ. Disadvantaged	0.08	0.13	16.5	0.08	0.08	0.8
MS Reading score average	0.38	−0.01	45.1	0.38	0.38	0.9
	(0.76)	(0.95)		(0.76)	(0.83)	
MS Math score average	0.33	−0.01	39.2	0.33	0.34	1.1
	(0.79)	(0.95)		(0.79)	(0.87)	
8th grade science score	0.24	−0.04	37.6	0.24	0.24	0.8
	(0.74)	(0.74)		(0.74)	(0.79)	
Passed Algebra	0.28	0.19	21.4	0.30	0.30	0.9
MS Absences average	6.78	7.77	15.2	6.78	6.76	0.3
	(5.65)	(7.30)		(5.65)	(5.82)	
County crime rate/100 people	4.01	4.15	8.5	4.01	4.00	0.7
	(1.68)	(1.60)		(1.68)	(1.58)	
County unemployment rate	0.09	0.08	42.0	0.09	0.09	1.0
	(0.03)	(0.02)		(0.03)	(0.02)	
County median income	4.23	4.61	44.2	4.23	4.23	0.0
	(0.76)	(0.92)		(0.76)	(0.80)	
Observations	19,026	717,775		19,026	717,775	

Note. Means (standard deviations for continuous variables) of control variables with dummy variable adjustment, excluding cohort and missing indicators. Sample includes all students in 9th grade in North Carolina public high schools between 2005–06 and 2011–12 who were also observed in a North Carolina public school in 8th grade in the previous year. Standardized difference calculated as the difference between ECHS and non-ECHS means divided by the square root of the average of the variances (times 100).

Thus, the lack of information on prior behavior, such as suspensions, is likely causing some bias to our full sample estimates, especially for crime. If we assume that the amount of bias from the omission of this variable in the full sample is the same as the amount of bias its omission produced in the 2011/2012 sample, then we could adjust our full-sample estimates as follows: the full-sample felony estimate would be adjusted from a 34 percent decline to a 27 percent decline (i.e., 78% of its original estimated strength); the misdemeanor estimate would adjust from a 35 percent decline to a 30 percent decline (i.e., 85% of its original estimated strength); the voting estimate would adjust from a 5.4 percent increase to a 4.9 percent increase (i.e., 91% of its original estimated strength); and the registration estimate would adjust from a 1.9 percent increase to a 1.7 percent increase (i.e., 90% of its original estimated strength).

5.3. Sensitivity analysis

While the robustness checks above provide some indication of potential bias in our propensity score estimates, they fail to provide information as to the extent to which other unobservable confounders could be biasing our estimates. As such, in this section we conduct a traditional sensitivity analysis that identifies the impact that different levels of confounding would have on our estimates.

We first note that we have controlled some of the largest predictors of both voting and crime, which include race, sex, socioeconomic status, and cognitive skill or academic achievement (DeLisi and Vaughn, 2016; Smets and Van Ham, 2013). However, for voting and registration, key unobservable predictors may include factors such as political interest (Smets and Van Ham, 2013); while for crime, key unobservable predictors may include characteristics such as the individual's personality, family composition, and prior measures of delinquency (DeLisi and Vaughn, 2016; for additional context, see Appendix B for a discussion of recent studies that generate regression-based estimates of voting and crime that control for socio-demographics, achievement, and additional predictors that we cannot observe in our setting).

We begin the sensitivity analysis with the original point estimates as observed in the full propensity-weighted sample (from Table 3). The analysis for voting is displayed in Table 5A (results for registration would be similar, though somewhat more sensitive to lower levels of confounding). The columns display treatment-confounder risk ratios ranging from 1.0 to 1.8, where a risk ratio of 1.8 means that, net of all variables already included in the model, the confounder set is 80 percent more likely to be present in the treatment group than the control group. Similarly, the rows display outcome-confounder risk ratios from 1.0 to 1.7, where a risk ratio of 1.7 means that, net of all variables already included in the model, the confounder set increases the likelihood of voting by about 70

Table 3
RCT and propensity score results.

		Lottery RCT (1)	Full Sample PSW (2)
Voted			
	ARR	1.042	1.054***
	95% CI	(0.947, 1.136)	(1.030, 1.079)
	Treatment margin	0.512	0.512
	Comparison margin	0.492	0.486
	N	3758	736,801
Registered to vote			
	ARR	1.035	1.019*
	95% CI	(0.983, 1.081)	(1.005, 1.033)
	Treatment margin	0.765	0.734
	Comparison margin	0.740	0.720
Felony			
	ARR	0.767	0.656***
	95% CI	(0.326, 1.790)	(0.562, 0.766)
	Treatment margin	0.011	0.0113
	Comparison margin	0.014	0.0173
	N	3758	736,462
Misdemeanor			
	ARR	0.950	0.649***
	95% CI	(0.541, 1.657)	(0.569, 0.741)
	Treatment margin	0.018	0.0222
	Comparison margin	0.019	0.0341
	N	3758	736,462

Note. Propensity-weighted models were run as doubly-robust, ATT-weighted logistic regressions controlling for individual-level covariates of gender, race/ethnicity, economic disadvantage, LEP status, disability status, AIG status, middle school test scores, passing Algebra in middle school, and 8th grade county characteristics of crime rate, unemployment rate, and median income, as well as cohort and missing variable indicators. Standard errors clustered by 9th grade school. ARR = adjusted risk ratio, equal to the covariate-adjusted likelihood of the outcome in the treatment group divided by the covariate-adjusted likelihood in the control group.
$^{*}p < .05$. $^{**}p < .01$. $^{***}p < .001$.

percent. The e-value for our observed risk ratio is 1.29, meaning that the true effect would be a risk ratio of 1.0 if an outcome-confounder and treatment-confounder risk ratio were each equal to 1.29. We can further see that a confounder set that increased the likelihood of voting by about 40 percent, net of our current controls, would need to be about 20 percent more prevalent in the treatment than the control group, net of weighting, to nullify our estimates; and a confounder set that increased the likelihood of voting by about 50–70 percent would still need to be slightly more than 10 percent more prevalent in the treatment than control group to nullify our estimates.

We next turn to the estimates on criminal convictions. The sensitivity analysis in Table 5B is conducted with respect to the full sample propensity score estimates on felony convictions (misdemeanors would be nearly identical).[10] The e-value for our risk ratio on felonies is 2.42. The results further show that a confounder set that increased the likelihood of criminal convictions by 300 percent, net of current controls, would still need to be nearly twice as prevalent in the comparison group, net of weighting, to nullify our estimates on convictions.

Thus, although a sensitivity analysis cannot provide definitive evidence as to the extent of bias in observational estimates, we determine that net treatment-outcome confounding factors need to be of at least moderate size and paired with at least moderate net treatment-confound prevalence differences to nullify our estimates. This suggests that the general directional conclusion – that ECHSs produce positive spillovers into the domain of civic behavior – is robust to at least moderate selection bias, though we cannot be certain that such bias does not exist.

5.4. Subgroup analysis

Students of disadvantaged subgroups may have fewer resources to draw on to help them overcome barriers to engaging in civic activity and may therefore be more greatly aided by the supports provided by the ECHS. As such, we next examine differential effects across key subgroups using the observational sample, presented graphically in Fig. 1 and in Table 6. Given the results of the robustness checks above, we note that caution should be used especially in interpreting the point estimates of the criminal conviction results. We

[10] Because our estimates are less than one, we would require either the treatment-confounder or outcome-confounder relationship to be below a risk ratio of 1, and the other to be above a risk ratio of 1. For simplicity, and following the convention and formulas set forth VanderWeele and Ding (2017), we report all hypothetical relationships as risk ratios greater than 1. Because we are primarily interested in confounders that increase the likelihood of crime but are less prevalent in the treatment group, the treatment-confounder risk ratios would be inverted.

Table 4
Alternative subsample estimates.

–		2006/2007	2006/2007	2011/2012	2011/2012
		(1)	(2)	(3)	(4)
Voted					
	ARR	1.035	1.037	1.077***	1.070***
	95% CI	(0.994, 1.078)	(0.995, 1.081)	(1.042, 1.114)	(1.035, 1.106)
	Treatment margin	0.521	0.521	0.482	0.482
	Comparison margin	0.504	0.503	0.447	0.450
	Control parent ed?		X		
	Control suspensions?				X
	N	208,300	208,300	211,470	211,470
Registered to vote					
	ARR	1.029*	1.030*	1.021*	1.019
	95% CI	(1.005, 1.055)	(1.005, 1.056)	(1.001, 1.042)	(0.999, 1.040)
	Treatment margin	0.700	0.700	0.719	0.719
	Comparison margin	0.680	0.680	0.704	0.705
	Control parent ed?		X		
	Control suspensions?				X
	N	208,300	208,300	211,470	211,470
Felony					
	ARR	0.711**	0.714**	0.579**	0.671*
	95% CI	(0.543, 0.931)	(0.546, 0.935)	(0.402, 0.833)	(0.470, 0.958)
	Treatment margin	0.020	0.020	0.006	0.006
	Comparison margin	0.028	0.028	0.011	0.009
	Control parent ed?		X		
	Control suspensions?				X
	N	208,205	208,205	211,379	211,379
Misdemeanor					
	ARR	0.759**	0.758**	0.562***	0.626***
	95% CI	(0.635, 0.907)	(0.634, 0.905)	(0.449, 0.705)	(0.497, 0.788)
	Treatment margin	0.043	0.043	0.011	0.011
	Comparison margin	0.057	0.057	0.020	0.018
	Control parent ed?		X		
	Control suspensions?				X
	N	208,205	208,205	211,379	211,379

Note. Propensity-weighted models were run as doubly-robust, ATT-weighted logistic regressions controlling for individual-level covariates of gender, race/ethnicity, economic disadvantage, LEP status, disability status, AIG status, middle school test scores, passing Algebra in middle school, and 8th grade county characteristics of crime rate, unemployment rate, and median income, as well as cohort and missing variable indicators. Standard errors clustered by 9th grade school. ARR = adjusted risk ratio, equal to the covariate-adjusted likelihood of the outcome in the treatment group divided by the covariate-adjusted likelihood in the control group.
$^*p < .05$. $^{**}p < .01$. $^{***}p < .001$.

Table 5A
Sensitivity analysis for voting.

Treat-confound RR	1.00	1.10	1.20	1.30	1.40	1.50	1.60	1.70	1.80
Voted-confound RR									
1.00	1.054	1.054	1.054	1.054	1.054	1.054	1.054	1.054	1.054
1.10	1.054	1.045	1.038	1.032	1.027	1.022	1.018	1.015	1.011
1.20	1.054	1.038	1.025	1.013	1.004	0.995	0.988	0.982	0.976
1.30	1.054	1.032	1.013	0.998	0.985	0.973	0.963	0.954	0.946
1.40	1.054	1.027	1.004	0.985	0.968	0.954	0.941	0.930	0.920
1.50	1.054	1.022	0.995	0.973	0.954	0.937	0.922	0.909	0.898
1.60	1.054	1.018	0.988	0.963	0.941	0.922	0.906	0.891	0.878
1.70	1.054	1.015	0.982	0.954	0.930	0.909	0.891	0.875	0.861

Note. Columns indicate risk ratio associations between treatment take-up and unobserved confounding, and rows indicate risk ratio associations between outcome and unobserved confounding. Cells indicate what adjusted risk ratios of ECHS treatment on each outcome would be after accounting for confounding denoted by the corresponding row and column.

Table 5B
Sensitivity analysis for felony convictions.

Treat-confound RR	1.00	1.25	1.50	1.75	2.00	2.25	2.50	2.75	3.00
Felony-confound RR									
1.00	0.656	0.656	0.656	0.656	0.656	0.656	0.656	0.656	0.656
1.50	0.656	0.703	0.738	0.765	0.787	0.805	0.820	0.833	0.843
2.00	0.656	0.729	0.787	0.835	0.875	0.908	0.937	0.962	0.984
2.50	0.656	0.745	0.820	0.883	0.937	0.984	1.025	1.061	1.093
3.00	0.656	0.757	0.843	0.918	0.984	1.042	1.093	1.139	1.181
3.50	0.656	0.765	0.861	0.945	1.020	1.088	1.148	1.203	1.252
3.75	0.656	0.769	0.868	0.957	1.036	1.107	1.171	1.230	1.283
4.00	0.656	0.772	0.875	0.967	1.050	1.125	1.193	1.255	1.312

Note. Columns indicate risk ratio associations between treatment take-up and unobserved confounding, and rows indicate risk ratio associations between outcome and unobserved confounding. Cells indicate what adjusted risk ratios of ECHS treatment on each outcome would be after accounting for confounding denoted by the corresponding row and column.

produce estimates for several intersectional subgroups (e.g., black males) as well as a subsample of "high risk" students.[11] Following the same process as Deming (2011), we define a high-risk sample by running a propensity model of criminal convictions on our pre-treatment covariates, by cohort year, and categorizing those in the highest propensity score quintile as "high risk." Descriptive statistics of this sample can briefly be described as almost entirely male (90%) and economically disadvantaged (90%), as well as disproportionately black (55%; 36% white), low-achieving (about 1 SD below average on each test, on average), and with high middle school absences (mean = 13.31). These students constitute about 8 percent of early college students.

In Fig. 1, we highlight the high-risk, black male, and white ED subsamples, as these subgroups are the ones most consistently estimated to be sizably affected by the treatment. High-risk students experienced the largest increase in voting likelihood of any subgroup examined (29 percent), the largest increase in voter registration (10 percent), the second largest decrease in misdemeanor convictions (41 percent), and large decreases in felony convictions (31 percent). In most cases, estimates for black males were very similar to the estimates for the high-risk group – 22 percent for voting, 8 percent for registering, 35 percent for felonies, and 44 percent for misdemeanors. Finally, behind the high-risk and black male samples, the largest effects on voting and voter registration were on white ED students (11 percent and 4 percent, respectively), who also experienced the greatest reduction in felonies (41 percent) and a sizable reduction in misdemeanors (34 percent). Thus, the subgroup analysis suggests that at-risk students generally experience the largest civic benefits from attending early colleges.

5.5. *Timing of effects*

Finally, while our primary analysis averages across impacts through young adulthood for students who are of different ages by the end of the panel, we have identified that impacts at early ages may be especially important due to the fact that most crime is committed during late adolescence and that voting behavior may be habitually sustained over time. We have also identified that the ECHS may produce part of its impact by creating a supportive school climate and by keeping students enrolled in school longer, suggesting that there may be particularly large impacts while students are still enrolled in school and in the ECHS in particular. As such, in this section we explore whether there is variation in the timing of effects on voting and criminal convictions through adolescence and early adulthood. Specifically, we use the full sample propensity weighted models to explore three additional outcomes measured in consecutive time points: 1) whether a student was enrolled in school[12]; 2) whether a student who was 18 voted[13]; and 3) whether a student was convicted of a crime during school months[14]; measuring each outcome at year 4, 5, 6, and 7 following the student's initial ninth grade cohort year (i.e., grades 12, 13, 14, and 15 for students who progress typically). We end at year 7 because this is the last year that can be observed across outcomes for almost all cohorts.

Table 7 and Fig. 2 present these results, though for clarity of presentation we have omitted the effects on being enrolled in school from the figure. We first find that early college students are much more likely to be in school in year 5 relative to their peers and relative to other years. Early college students are about 3 percent more likely to still be in school in year 4, 16 percent more likely to be in school in year 5, and not significantly more likely to be in school in years 6 or 7. Second, we find that students who were 18 were about 7–9 percent more likely to vote in years 4, 6, and 7, and 16 percent more likely to vote in year 5. Though these between-year effects are not significantly different from each other, these results suggest that impacts on voting appear while students are still in high school, are sustained through the immediate following years, and may be particularly concentrated in the fifth year, a year in which many students

[11] Some intersectional subgroups are omitted due to their small sample sizes. Results for traditional subgroups such as black, Hispanic, male, and female, are also available on request.

[12] "In school" includes students in high school in year 4, in high school or college in year 5, and in college in year 6 or 7 after entering 9th grade. College entry data come from the University of North Carolina system, the North Carolina Community College system, and the National Student Clearinghouse.

[13] Voting data for the final cohort is only available through the sixth year after 9th grade, so this cohort is excluded from Year 7 estimates.

[14] We pool felony and misdemeanor convictions due to the very small cell sizes. The outcome is defined as convictions that resulted from an offense committed in August through May of a given school year.

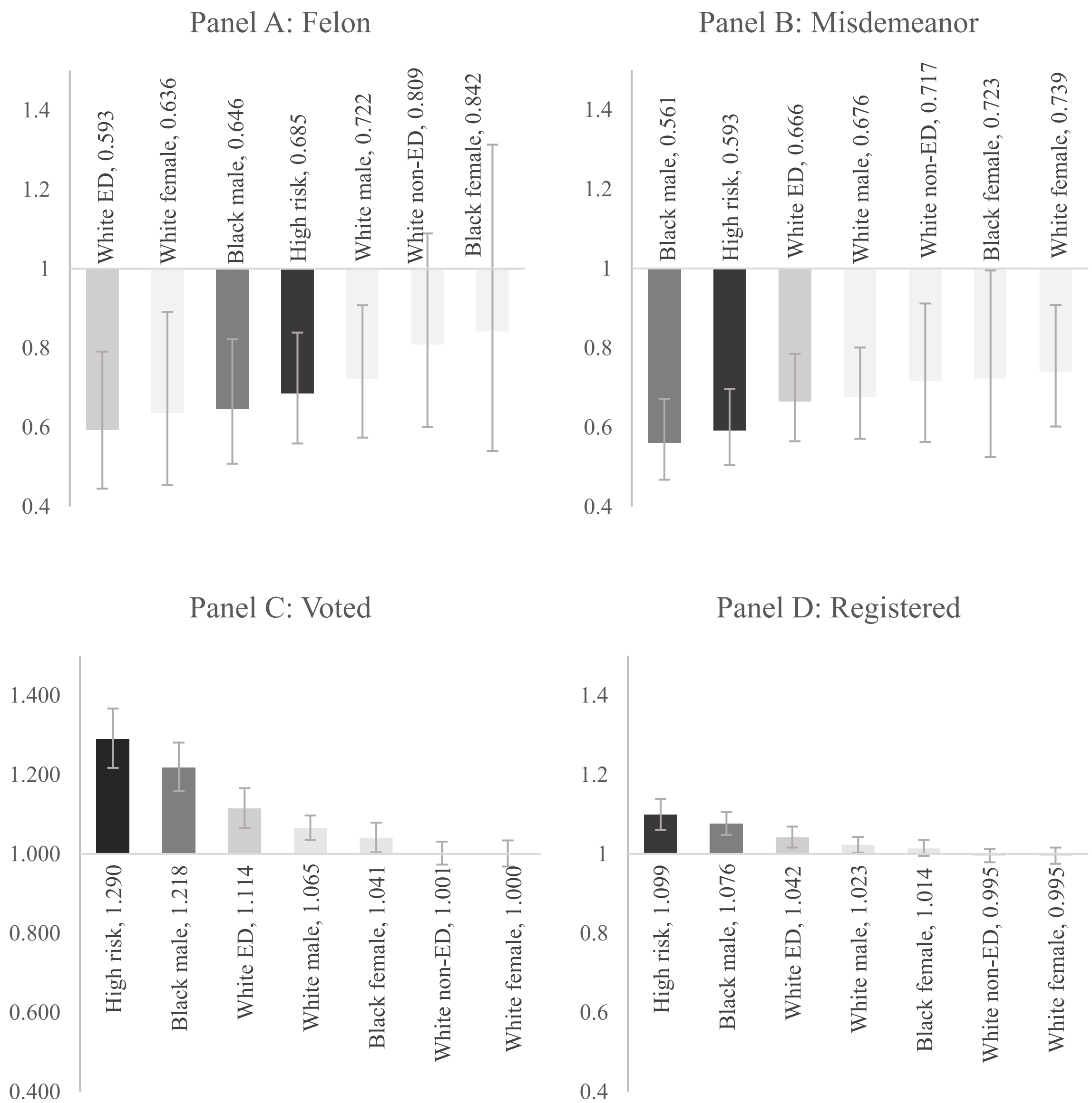

Fig. 1. Adjusted risk ratios of propensity score weighted estimates by subgroups from the full sample. The height of the bars represents the adjusted risk ratio point estimate, while error bars represent 95% confidence intervals around these estimates. ED = economic disadvantage as measured by free/reduced priced lunch. High risk defined as being in the highest quintile propensity score, within cohort, for being convicted of a crime based on pre-treatment covariates. Estimates obtained from propensity weighted logistic regressions controlling for individual-level covariates of gender, race/ethnicity, economic disadvantage, LEP status, disability status, AIG status, middle school test scores, passing Algebra in middle school, and 8th grade county characteristics of crime rate, unemployment rate, and median income, as well as missing variable indicators and cohort fixed effects, within each subgroup. Standard errors clustered by 9th grade school.

become eligible to vote for the first time and in which ECHS students are much more likely to still be enrolled in school than their peers.[15] With respect to criminal convictions, we again suggest caution in interpreting the point estimates due to the greater possibility of bias in these estimates; however, we see estimated effects range from about 28 to 44 percent decreases across all of these key late adolescent and early adulthood years, thus suggesting that impacts of the ECHS on criminal behavior also begin during the high school years and are sustained through the immediate following years.

[15] As discussed in footnote 4, a second possible explanation for the spike at this time point is that ECHS students are less likely to have left the state as a result of having been induced to stay in school longer (i.e., differential attrition from the data), thus increasing our likelihood of identifying ECHS students who voted in this year relative to non-ECHS (former) students.

Table 6
Subgroup analysis.

		Felon	Misdemeanor	Voting	Registration
High Risk	ARR	0.685***	0.593***	1.290***	1.099***
	95% CI	(0.559, 0.839)	(0.505, 0.697)	(1.217, 1.367)	(1.061, 1.139)
	Treat margin	0.0576	0.075	0.480	0.756
	Comp margin	0.0842	0.126	0.372	0.688
	Observations	146,966	146,966	147,124	147,124
Black male	ARR	0.646***	0.561***	1.218***	1.076***
	95% CI	(0.508, 0.822)	(0.468, 0.672)	(1.159, 1.281)	(1.048, 1.106)
	Treat margin	0.038	0.046	0.577	0.822
	Comp margin	0.059	0.083	0.474	0.764
	Observations	106,641	106,641	106,759	106,759
White male	ARR	0.722**	0.676***	1.065***	1.023*
	95% CI	(0.574, 0.908)	(0.571, 0.801)	(1.035, 1.097)	(1.003, 1.043)
	Treat margin	0.0171	0.0308	0.538	0.790
	Comp margin	0.0237	0.0455	0.505	0.772
	Observations	209,698	209,698	209,801	209,801
Black female	ARR	0.842	0.723*	1.041*	1.014
	95% CI	(0.540, 1.313)	(0.525, 0.995)	(1.004, 1.079)	(0.995, 1.035)
	Treat margin	0.00578	0.0200	0.637	0.833
	Comp margin	0.00687	0.0277	0.612	0.821
	Observations	104,937	104,937	104,959	104,959
White female	ARR	0.636**	0.739*	1.000	0.995
	95% CI	(0.454, 0.891)	(0.602, 0.908)	(0.968, 1.034)	(0.975, 1.016)
	Treat margin	0.00545	0.0147	0.492	0.713
	Comp margin	0.00856	0.0199	0.491	0.716
	Observations	199,377	199,377	199,425	199,425
White ED	ARR	0.593***	0.666***	1.114***	1.042**
	95% CI	(0.445, 0.791)	(0.565, 0.785)	(1.065, 1.166)	(1.016, 1.069)
	Treat margin	0.0134	0.0291	0.419	0.695
	Comp margin	0.0226	0.0438	0.376	0.667
	Observations	110,365	110,365	110,431	110,431
White non-ED	ARR	0.809	0.717*	1.001	0.995
	95% CI	(0.601, 1.089)	(0.563, 0.912)	(0.973, 1.031)	(0.979, 1.012)
	Treat margin	0.00823	0.0163	0.565	0.777
	Comp margin	0.0102	0.0228	0.564	0.780
	Observations	297,162	297,162	297,247	297,247

Note. ED = economic disadvantage as measured by free/reduced priced lunch. Propensity-weighted models were run as doubly-robust, ATT-weighted logistic regressions controlling for individual-level covariates of gender, race/ethnicity, economic disadvantage, LEP status, disability status, AIG status, middle school test scores, passing Algebra in middle school, and 8th grade county characteristics of crime rate, unemployment rate, and median income, as well as cohort and missing variable indicators. Standard errors clustered by 9th grade school.
*$p < .05$. **$p < .01$. ***$p < .001$.

6. Discussion

In this study, we have provided evidence that ECHS attendance leads to an increase in prosocial civic behaviors associated with responsible citizenship among young people. We consistently estimate about a 4 to 5 percent increase in voting and a 2 to 3 percent increase in voter registration across a variety of specifications, though an effect of this size is too small to be detected at conventional levels of significance in our lottery sample. Similarly, low rates of criminal convictions make lottery estimates for these outcomes imprecise, but we estimate moderate declines in convictions in our observational sample and find that the direction of this impact is robust to relatively large confounding bias.

We thus identify that effects on civic behaviors are likely present, but are relatively small. This result may be surprising considering the large increases in educational attainment experienced by ECHS students. However, with respect to voting, some research suggests that the relationship between educational attainment and voting may be spurious, driven by common causes such as cognitive skill (Denny and Doyle, 2008). While ECHS attendance causes a large increase in educational attainment, it has been shown to produce only more modest effects on educational achievement (Berger et al., 2013; Crittenden Fuller et al., 2020; Lauen et al., 2017; Miller and Corritore, 2013). Thus, relatively small positive effects on political participation as compared to the large effects on college degree attainment could arise because: 1) Cognitive skill rather than degree attainment is a main driver of political participation; and 2) ECHSs greatly increase the rate at which capable students earn degrees, but only modestly increase the underlying cognitive skills of the typical student. However, we also note that our sample period is limited to a time when former students remain relatively young,

Table 7
Impacts of ECHS attendance at time points relative to high school entry.

–		Year 4	Year 5	Year 6	Year 7
		(1)	(2)	(3)	(4)
Enrolled in school					
	ARR	1.026***	1.157***	1.019	1.010
	95% CI	(1.016, 1.036)	(1.134, 1.180)	(0.995, 1.044)	(0.980, 1.040)
	Treatment margin	0.887	0.739	0.589	0.524
	Comparison margin	0.864	0.639	0.579	0.519
	N	736,801	736,801	736,801	736,801
Voted (if age 18)					
	ARR	1.066	1.163***	1.088***	1.067**
	95% CI	(0.971, 1.170)	(1.107, 1.221)	(1.052, 1.126)	(1.025, 1.111)
	Treatment margin	0.141	0.131	0.178	0.192
	Comparison margin	0.132	0.113	0.163	0.180
	N	205,671	702,935	709,689	603,885
Convicted of any crime					
	ARR	0.584***	0.715**	0.560***	0.606***
	95% CI	(0.443, 0.771)	(0.574, 0.891)	(0.417, 0.750)	(0.478, 0.768)
	Treatment margin	0.0036	0.0049	0.0038	0.0041
	Comparison margin	0.0061	0.0068	0.0068	0.0068
	N	736,801	736,801	736,801	736,801

Note. Propensity-weighted models were run as doubly-robust, ATT-weighted logistic regressions controlling for individual-level covariates of gender, race/ethnicity, economic disadvantage, LEP status, disability status, AIG status, middle school test scores, passing Algebra in middle school, and 8th grade county characteristics of crime rate, unemployment rate, and median income, as well as cohort and missing variable indicators. Standard errors clustered by 9th grade school. ARR = adjusted risk ratio, equal to the covariate-adjusted likelihood of the outcome in the treatment group divided by the covariate-adjusted likelihood in the control group. Year 4, 5, 6, and 7 measured relative to 9th grade entry year. Enrolled in school defined as enrolled in high school in year 4, in high school or college in year 5, and in college in year 6 or 7. Voted samples contain those who were 18 and older; year 7 voting outcomes cannot be observed for the final cohort. Convictions restricted to incidents that occurred during school months (August through May).
*$p < .05$. **$p < .01$. ***$p < .001$.

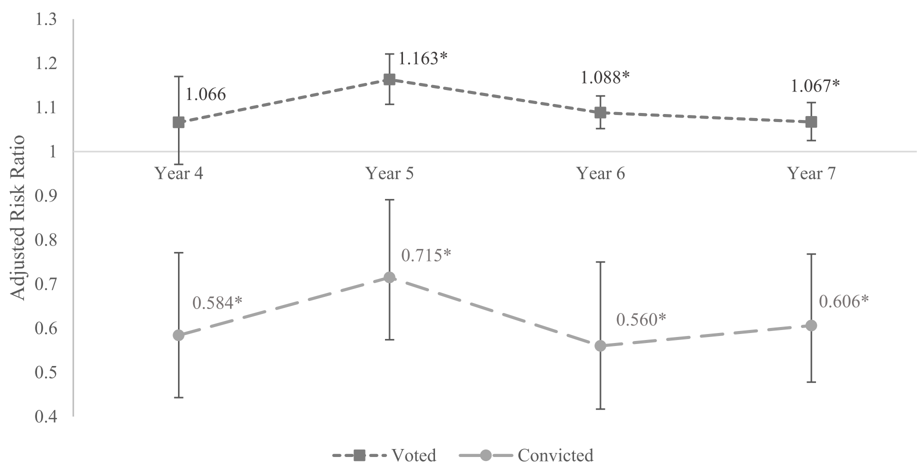

Fig. 2. Adjusted risk ratios of propensity score weighted estimates from the full sample of students for voting (if age 18) and being convicted of any crime during year 4, 5, 6, and 7 after entering 9th grade. Statistically significant results at $p < .05$ denoted with a star. Estimates obtained from propensity weighted logistic regressions controlling for individual-level covariates of gender, race/ethnicity, economic disadvantage, LEP status, disability status, AIG status, middle school test scores, passing Algebra in middle school, and 8th grade county characteristics of crime rate, unemployment rate, and median income, as well as missing variable indicators and cohort fixed effects. Standard errors clustered by 9th grade school.

and it is possible that the effects of the ECHS and associated increases in educational attainment could grow over time as students age into more stable lives in middle adulthood. While we cannot adequately test such propositions here, these plausible explanations of the impact of the ECHS are deserving of further exploration.

Similarly, while prior literature on crime suggests a large protective impact of high school completion (Lochner, 2010), ECHSs primarily improve college degree attainment, with only small impacts on high school completion (Edmunds et al., 2017; Haxton et al., 2016; Lauen et al., 2017). Thus, despite large effects on college degree attainment, more modest effects on criminal convictions might arise because 1) effects of educational attainment on crime are primarily concentrated to secondary schooling; and 2) ECHSs produce only small impacts on secondary educational outcomes. Again, these propositions cannot be fully tested here, but emerge as plausible explanations worthy of further inquiry.

However, we estimate larger impacts on higher-risk, black male, and white ED students, subgroups that have low baseline likelihoods of voting and high risks of experiencing convictions. For example, conviction rates for the weighted black male comparison sample are 5.9 percent for felonies and 8.3 percent for misdemeanors. Thus, even 5 to 10 percent reductions in convictions could affect the life course of many students (Wakefield and Uggen, 2010) while also substantively reducing state expenses on incarceration (North Carolina Department of Public Safety, 2017). Similarly, we see that students of these subgroups who attend an early college come to vote at rates that are more comparable to their peers than their non-ECHS counterparts, which could have important implications for equity in political representation (Griffin, 2014; Lindh and McCall, 2020).

We additionally find that ECHS students are more likely to vote and less likely to be convicted of crime at all analyzed time points during late adolescence and early adulthood, suggesting that effects begin while students are still enrolled in high school and are maintained through the immediate subsequent years. Because many who commit crime do so only during adolescence (Farrington et al., 2013), the ECHS may reduce long-run overall crime rates by simply preventing students from engaging in crime during their late adolescent/secondary school years, which could have important long-run impacts – for example, this could mediate some of the increase in high school and college degree attainment among ECHS students (Kirk and Sampson, 2013) and protect against later crime and poverty by improving students' long-run economic opportunities (Wakefield and Uggen, 2010). Similarly, because voting appears to be habitually sustained within individuals over time (Plutzer, 2002), helping students to begin voting when they first become eligible may have a long-lasting impact on their political participation and representation (Canes-Wrone, 2015).

Finally, with respect to voting, we find that students may be especially more likely to vote in their fifth year after entering high school. This spike occurs in a year in which many ECHS students are still in school and in which many first become eligible to vote, adding evidence to political literature showing that being enrolled in the academic environment may increase political participation (Highton and Wolfinger, 2001; Smets and van Ham, 2013; Tenn, 2007; Zeglovits and Aichholzer, 2014). Thus, the ECHS might affect short- and longer-run voting behavior by helping students to begin voting while they are still enrolled in the ECHS.

We note two key limitations in the current study that future literature can improve upon. First, we find that even a moderately-sized randomized trial may not be well-suited to estimating impacts on a very rare outcome like criminal convictions. Though our lottery sample includes thousands of students – larger than many RCTs – the outcome occurs in only 1–2 percent of the sample, which translates to only about 20–40 students in each of the treatment and control groups.[16] However, our review of key predictors of crime also makes us uncertain that basic educational administrative data is enough to remove confounding that might be present in selection-on-observables designs. Thus, future researchers might seek to collect relatively fine-grained indicators of early student behavior that may proxy for many of the psychosocial predictors of crime found in criminological literature, or else conduct prospective longitudinal surveys that collect data on psychosocial predictors more typically available in national longitudinal studies.

Second, more research is still needed to determine the relative influence of each component of the early college model on both civic and educational outcomes – specifically, whether effects are due primarily to the accelerated educational model, to staff and school practices, to peer effects, or to some combination of the three. Each possibility holds different implications for the design of future educational programs as well as traditional K-12 school practices – for example, will the expansion of academic initiatives designed to increase academic preparation and college-going in traditional schools, such as dual-enrollment and Advanced Placement, also create positive civic spillovers, or do indirect civic benefits arise in the ECHS primarily due to the creation of a unique and supportive school climate?

However, understanding the total effect of the ECHS remains important. In particular, the ECHS is a program with many strong features that might be expected to produce large positive civic spillovers. Consistent with other recent work (e.g., Lindgren et al., 2019), we find that the ECHS's impact on civic outcomes is concentrated among students from disadvantaged backgrounds, with smaller or no impacts on more advantaged (e.g., non-ED white) students. Thus, while educational expansion and acceleration may hold promise for improving civic outcomes and reducing inequalities in participation, increasing engagement beyond the levels currently typical of relatively advantaged students may require more focused interventions and institutional changes that directly address additional barriers to engagement faced by young people, such as competing pressures to engage in work and family formation, lack of residential stability, and lack of experience with community organizations (Highton and Wolfinger, 2001; Levine, 2007; Plutzer, 2002).

7. Conclusion

The results of this study suggest that early college high schools in North Carolina do not only improve the educational outcomes of their students, but also produce prosocial behavioral changes that may have broader positive impacts for society. While the early college model has already been shown to be cost-effective based on educational improvements alone (Atchinson et al., 2019), potential

[16] In a post-hoc power analysis, we estimate that we would need about 16,000 to 36,000 students in the lottery sample to achieve statistical significance given the effect sizes we have observed for felonies, voting, and registration. Given lotteries of a similar size, this would require about 180–400 school lottery-year observations. We note that prior research on ECHSs has had sufficient power to detect significant effects on post-secondary outcomes in part because the ECHS has very large effects on these outcomes. For example, in Edmunds and colleagues' (2020, Table 4) study of almost 1700 lottery ECHS students, standard errors on associate degree attainment rates imply a 95% confidence interval of about ±6 percentage points around the impact estimate; however, point estimates on associate degree attainment show greater than 20 percentage point increases (about 200% increases over baseline rates of around 10% attainment).

civic benefits outside of individual mobility should also be considered by policymakers considering implementing or expanding the early college model, as these may result in further long-run cost savings and benefits to civic functioning. Expansion may particularly have important equity effects, as we estimate that impacts are strongest for students at the highest risk of otherwise experiencing adverse civic outcomes. Such students have also been found to experience at least as strong or stronger long-term impacts in educational outcomes from attending ECHSs as other students (Edmunds et al., 2020; Song and Zeiser, 2019) and to experience larger impacts on civic outcomes from educational expansion generally (Lindgren et al., 2019; Lochner and Moretti, 2004). Thus, expanding the ECHS model is likely to carry overall benefits to social welfare and civic functioning in addition to helping to close gaps in political representation and individual life course outcomes between students of more-advantaged and less-advantaged social groups.

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Appendix A. Tables

Table A1
Unweighted propensity score results

		Full Sample Unadjusted Logit (1)	Full Sample PSW Logit (2)
Voted			
	ARR	1.110***	1.054***
	95% CI	(1.068, 1.152)	(1.030, 1.079)
	Treatment margin	0.521	0.512
	Comparison margin	0.470	0.486
	N	736,801	736,801
Registered to vote			
	ARR	1.043***	1.019*
	95% CI	(1.020, 1.065)	(1.005, 1.033)
	Treatment margin	0.729	0.734
	Comparison margin	0.700	0.720
	N	736,801	736,801
Felony			
	ARR	0.323***	0.656***
	95% CI	(0.262, 0.399)	(0.562, 0.766)
	Treatment margin	0.0115	0.0113
	Comparison margin	0.0356	0.0173
	N	736,462	736,462
Misdemeanor			
	ARR	0.414***	0.649***
	95% CI	(0.349, 0.490)	(0.569, 0.741)
	Treatment margin	0.0235	0.0222
	Comparison margin	0.0569	0.0341
	N	736,462	736,462

Note. Model 1 estimates obtained from unconditional logits that control only for cohort effects and ECHS treatment status. Model 2 estimates are reproduced from Table 3, Column 2. Standard errors clustered by 9th grade school. ARR = adjusted risk ratio, equal to the covariate-adjusted likelihood of the outcome in the treatment group divided by the covariate-adjusted likelihood in the control group.

$^{*}p < .05$. $^{**}p < .01$. $^{***}p < .001$.

Table A2
Full sample propensity score weighted ATT estimates

	(1)	(2)	(3)	(4)
	Felon	Misd	Vote	Reg

(continued on next page)

Table A2 (*continued*)

	(1)	(2)	(3)	(4)
	Felon	Misd	Vote	Reg
ECHS	−0.449***	−0.466***	0.113***	0.0735*
	(0.0835)	(0.0718)	(0.0263)	(0.0288)
Asian	−1.272***	−0.679*	0.0570	0.146
	(0.357)	(0.333)	(0.172)	(0.212)
Black	0.548	0.911**	1.175***	1.231***
	(0.394)	(0.308)	(0.174)	(0.216)
Hispanic	−0.466	0.194	0.0958	−0.313
	(0.436)	(0.323)	(0.167)	(0.215)
American Indian	0.322	0.739*	0.324	0.503*
	(0.435)	(0.329)	(0.169)	(0.212)
White	0.348	0.746*	0.556**	0.597**
	(0.380)	(0.303)	(0.173)	(0.218)
Multi-racial	0.547	0.960**	0.417*	0.490*
	(0.388)	(0.329)	(0.174)	(0.217)
Economically disadvantaged	0.481***	0.464***	−0.445***	−0.294***
	(0.0882)	(0.0601)	(0.0193)	(0.0193)
Limited English proficiency	−0.549*	−0.466*	−0.390***	−0.456***
	(0.214)	(0.193)	(0.0753)	(0.0717)
Disability	−0.267*	−0.186*	0.232***	0.165***
	(0.113)	(0.0903)	(0.0372)	(0.0406)
Academically & intellectually gifted	−0.0659	−0.267**	0.192***	0.154***
	(0.138)	(0.0847)	(0.0278)	(0.0263)
Missing demographics	0.0660	0.0770	0.101*	−0.00932
	(0.182)	(0.0923)	(0.0426)	(0.0513)
Mobility	0.447***	0.331***	−0.130***(0.0200)	−0.0317(0.0271)
	(0.0574)	(0.0451)		
Old for grade	0.0893	0.166	−0.0669	−0.0940
	(0.132)	(0.105)	(0.0384)	(0.0516)
Old for grade * Economically disadvantaged	−0.0577	−0.0540	0.0401	0.0368
	(0.165)	(0.117)	(0.0484)	(0.0641)
Male	1.655***	1.041***	−0.131***	0.128***
	(0.0628)	(0.0441)	(0.0196)	(0.0209)
Middle school reading average	−0.175***	−0.112**	0.104***	0.0946***
	(0.0493)	(0.0421)	(0.0181)	(0.0200)
Missing middle school reading average	0.908	0.355	−0.277	−0.740***
	(0.778)	(0.631)	(0.236)	(0.172)
Middle school math average	−0.328***	−0.257***	0.0484***	0.0649***
	(0.0582)	(0.0479)	(0.0145)	(0.0169)
Missing middle school math average	−1.077	−0.548	−0.404	−0.0427
	(0.770)	(0.625)	(0.251)	(0.211)
Middle school average absences	0.0449***	0.0431***	−0.0204***	−0.0132***
	(0.00304)	(0.00227)	(0.00165)	(0.00169)
Missing middle school average absences	0.429	0.242	−0.712***	−0.736***
	(0.453)	(0.246)	(0.149)	(0.117)
8th grade science score	−0.293***	−0.239***	0.104***	0.0445**
	(0.0497)	(0.0316)	(0.0122)	(0.0151)
Missing 8th grade science score	0.185	0.505**	0.120	0.136
	(0.262)	(0.190)	(0.0990)	(0.124)
Took/passed Algebra	−0.192	−0.156*	0.123***	0.0874**
	(0.142)	(0.0781)	(0.0253)	(0.0306)
Missing took/passed Algebra	0.578***	0.295***	−0.210***	−0.274***
	(0.0774)	(0.0663)	(0.0561)	(0.0509)
County crime index per 100 k people (1000s)	−0.0251	−0.0818***	0.00626	0.00456
	(0.0193)	(0.0216)	(0.00846)	(0.0107)
County median income ($10 k)	0.123**	−0.0911*	−0.0158	0.0366
	(0.0468)	(0.0412)	(0.0189)	(0.0209)
County unemployment rate	0.339	0.339	−3.215**	−1.560
	(1.868)	(2.326)	(0.925)	(1.368)
Cohort Year = 2007	−0.0935	−0.0609	−0.0989*	−0.0715
	(0.159)	(0.102)	(0.0420)	(0.0459)
Cohort Year = 2008	−0.140	−0.269*	0.0242	0.114*
	(0.167)	(0.116)	(0.0483)	(0.0449)
Cohort Year = 2009	−0.234	−0.0774	0.270*	0.489***
	(0.307)	(0.247)	(0.107)	(0.126)
Cohort Year = 2010	−0.489	−0.175	−0.131	0.360**
	(0.324)	(0.255)	(0.110)	(0.136)
Cohort Year = 2011	−0.778*	−0.559*	−0.0800	0.335*
	(0.324)	(0.278)	(0.117)	(0.146)

(*continued on next page*)

Table A2 (*continued*)

	(1) Felon	(2) Misd	(3) Vote	(4) Reg
Cohort Year = 2012	−0.950**	−0.877**	−0.0967	0.0913
	(0.316)	(0.292)	(0.112)	(0.147)
Constant	−6.115***	−4.399***	−0.0722	0.176
	(0.543)	(0.444)	(0.229)	(0.305)
Treatment margin	0.0113	0.0222	0.512	0.733
Control margin	0.0173	0.0341	0.486	0.720
ARR	0.656	0.649	1.054	1.019
N	736,462	736,462	736,801	736,801

Note. Propensity-weighted models were run as doubly-robust, ATT-weighted logistic regressions. Standard errors clustered by 9th grade school. *$p < .05$. **$p < .01$. ***$p < .001$.

Appendix B. Sensitivity Analysis

In this appendix, we review results of recent studies that use nationally-representative samples to predict the likelihood of voting or crime using regression techniques. We do not consider this review to be exhaustive nor the estimates to translate perfectly to our setting, but we have identified recent studies that have been widely cited and produce estimates that are relevant to our study by controlling for socio-demographics, early adolescent achievement, and additional psycho-social factors that we cannot observe. We focus on identifying the approximate net effect of these additional factors to discuss the potential magnitude of unobserved outcome-confounder risk ratios in our study. We highlight one study within the voting literature and one study within the crime literature that produced estimates of these potential confounding factors that appear to be near or somewhat larger than the average estimates across all of the studies we reviewed.

We begin with voting. While some of the consistently largest predictors of voting include the basic demographics that we control, such as gender, race/ethnicity, education (or achievement), and income/family background, others include political interest and personality characteristics (Smets and Van Ham, 2013). To examine the possible magnitude of the net relationship between these variables with the outcome in our sample, we searched for prior studies that have predicted voting using cognitive and socio-demographic variables (which we control) and measures of political interest and personality (which we cannot control). A close match is Denny and Doyle (2008; see also Hillygus et al., 2016, and Weinschenk and Dawes, 2020), who estimated voting in the 1997 British general election using cognitive ability measured at age 11, childhood socio-demographics, personality at age 16, and interest in politics. They found that the largest explanatory factor for voting was interest in politics, which, net of the other controls, was associated with about a 17 percentage point percent increase in voting (about 22% over the mean of 80%). While many other variables were significant, they generally had more limited impacts. For example, a one standard deviation increase in work ethic was associated with a less than 2 percentage point increase in voting.

Although these results may not translate perfectly to our setting, they suggest that strong omitted predictors of voting may have a risk ratio relationship with the outcome of about 1.2–1.3, similar to our e-value. Unfortunately, we cannot estimate likely relationships between these omitted predictors and treatment take-up. Because ECHSs are not schools that have any particular political or civic emphasis, it is plausible that students do not select into ECHSs on the basis of political interest, but instead that students with high political interest may be overrepresented in ECHSs because of correlations between political interest and academic achievement, motivation, or work ethic, which are either directly or indirectly controlled by our baseline covariates. Our sensitivity analysis shows that, at an outcome-confound risk ratio of 1.3, the net treatment-confound risk ratio would also need to be about 1.3 or greater to nullify the results.

We next turn to criminal convictions. With respect to extant literature, we again find that race, socioeconomic status, and sex are some of the strongest predictors of crime, followed by other personality and family background variables (DeLisi and Vaughn, 2016). As such, we again searched for studies that predict criminal outcomes using measures of achievement, socio-demographics, and additional personality and background variables. A close match is Wolf and Kupchik (2017; see also Barnes and Motz, 2018), who use the US National Survey of Adolescent Health to predict experience of incarcerations lasting at least one year by early adulthood using early adolescent socio-demographics, grades, school characteristics, and psycho-social factors such as mental health, drug use, and delinquency. Beyond socio-demographics, grades, and school-level factors, the authors found significant net associations with the outcome for individual-level variables of having no father in the residence (OR = 1.22), delinquency (OR = 1.92), marijuana use (OR = 1.20), and suspensions (OR = 1.72).

To assess the combined strength of these potential confounders, we converted these odds ratios into risk ratios using the formula: $RR = \frac{OR}{(1-P)+(P^{*}OR)}$ (Zhang and Yu, 1998) and then took the product of the four estimates. From the descriptive statistics in Wolf and Kupchik, 2017, Table 1), we used an approximate baseline incarceration rate (P) of about 0.14. We find that these four factors may constitute a potential confounder-outcome effect amounting to a risk ratio of about 3.7.

This suggests that there may be large predictors of criminal convictions that are not present in our data, especially indicators of prior behavior and delinquency. However, as above, to complete the analysis we would need to know the prevalence difference of these confounders in the treatment and comparison groups. To the extent that education and crime may represent distinct life paths, students may select into ECHSs directly on an interest in education and a disinterest in (or low proclivity towards) crime. However, it is also likely that at least some of this prevalence difference is already indirectly controlled by our baseline covariates, such as academic achievement. The sensitivity analysis shows that at an outcome-confounder risk ratio of about 3.75, the net treatment-confound risk ratio would still need to be between 1.75 and 2.00 to nullify our estimates.

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